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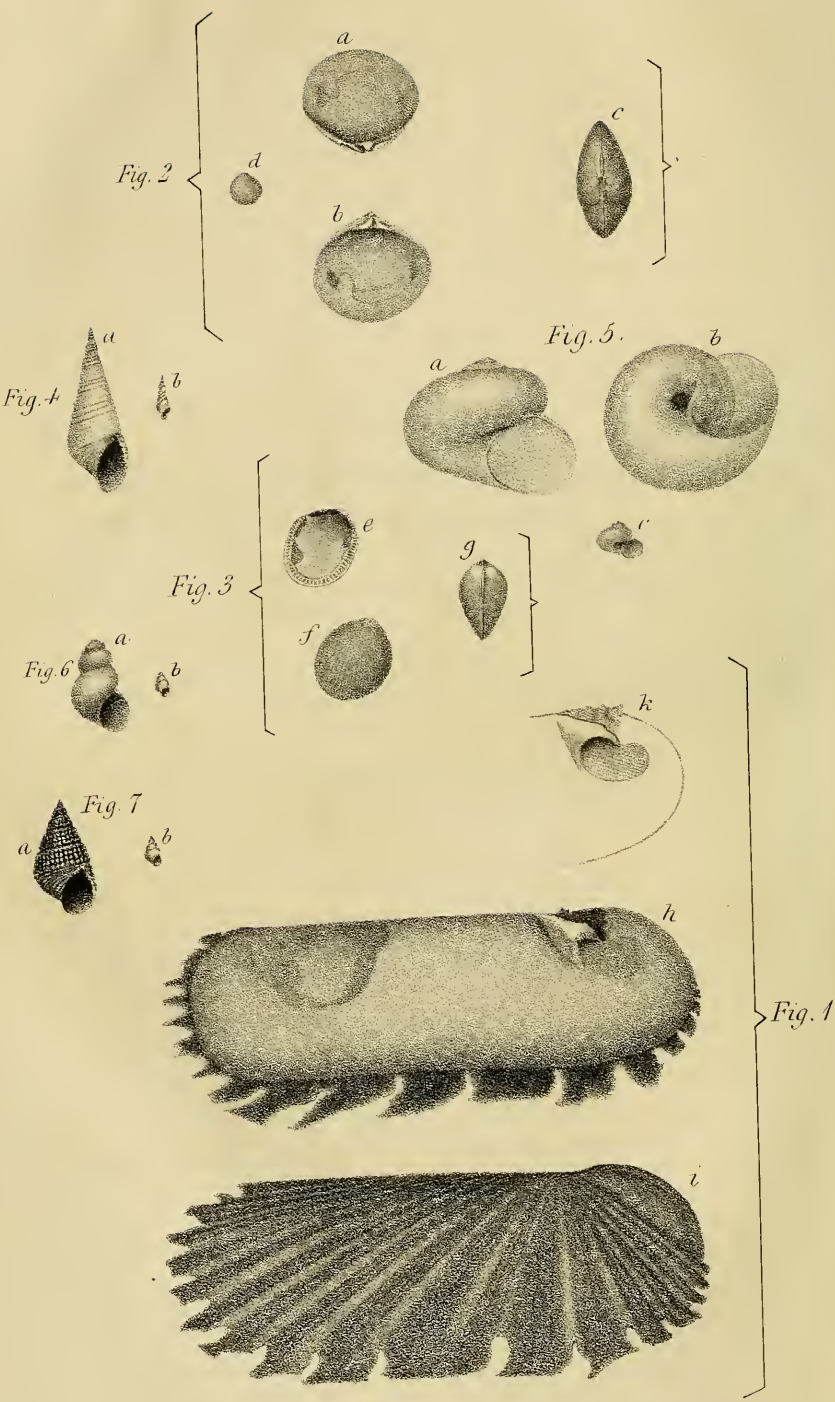
ERRATA.

Page 32, l. 5 fr. bot., after rotation, add, *If we suppose*.—p. 223, l. 11 fr. top, for *probably*, read *popularly*; l. 14 fr. top, for *was*, read *runs*.—p. 226, l 17 fr. top, for *moving*, read *running*.—p. 227, l. 4 fr. bot., for *Laudisburg*, read *Landisburg*.—p. 228, l. 6 fr. top, for *Laudisburg*, read *Landisburg*.—p. 236, l. 28 fr. top, for *sestertium*, read *sestertia*.—p. 239, l. 23 fr. top, for *to*, read *at*.—p. 261, l. 13 fr. bot., for *Whöler*, read *Wöhler*.—p. 293, l. 15 fr. top, for *Masses*, read *Mosses*.

The plate which accompanies the article on the meteors of Nov. 13th, on account of inaccuracies in the engraving, does not correspond as *exactly* as it should do with the results given in the paper. It is however, perfect, for the purposes of illustration.

Vol. 25.—Page 129, l. 28, for *Edinburgh*, read *high latitudes*.



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THE WARWICK VASE.

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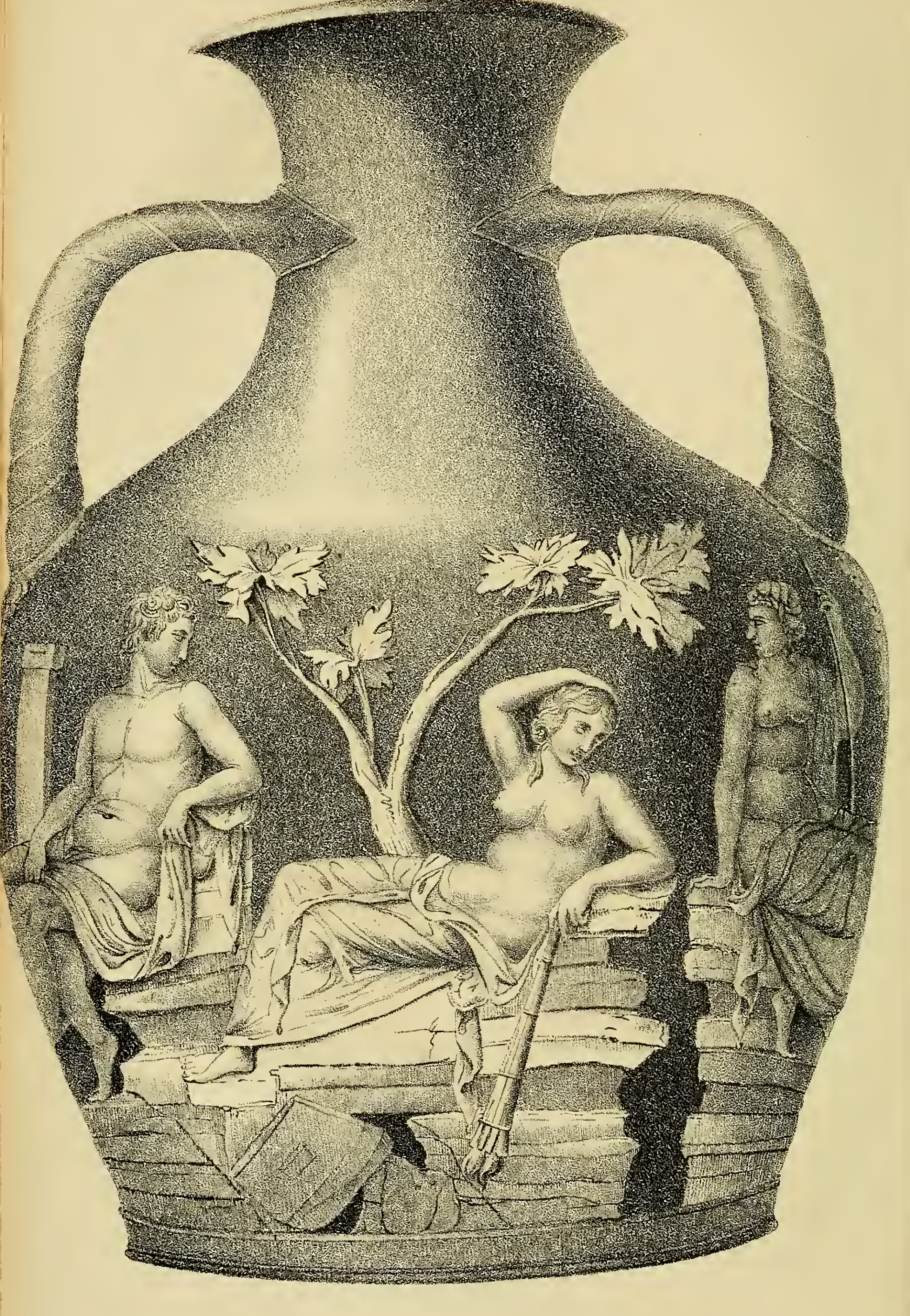

THE BARBERINI OR PORTLAND VASE.

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TERRA COTTA VESSELS OF POMPEII.

TERRA COTTA VASE, *FOUND IN POMPEII.*

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THE

AMERICAN JOURNAL OF SCIENCE, &c.

Art. I.—*Ascent of Mount Etna, February*, 1832; by Sidney L. Johnson, late tutor in Yale College, and teacher in the U. States squadron, in the Mediterranean.

A wish to ascend Mt. Etna, was at first, the chief motive of our visit to Catania, but before departure, our hopes of reaching the summit were somewhat diminished. Since the snow fell, several parties had attempted it, but all without success. We often gazed upon it from our ships in the harbor of Syracuse, where it presented the singular appearance of a perfect cone of snow of astounding size, to whose dazzling whiteness the vertex tipped with black and tufted with a graceful plume of smoke, afforded the only relief. From the more commanding heights of Epipolæ, we could trace the sides lower down; the skirts of the snow were dappled with the naked patches of dark rock, then disappeared and the broad green base presented a cheerful contrast to the cold and glittering summit.

On our approach and entrance to Catania, the mountain was entirely veiled from view by clouds and the rain descended in torrents. Had this weather continued a little longer than it did, we might have departed without ocular proof of the existence of elevated ground in that vicinity. But after two or three days, a delightful change inspired us with strong hopes of accomplishing our desires, and we determined upon an immediate attempt.

Our arrangements were made for riding up as far as Nicolosi, on the 22d. of February. Abbate, our landlord had provided every necessary refreshment; and with a due supply of extra clothing, we mounted and were in motion by 4 P. M. Our party consisted of four, and was guided to the resting place for the night by our humorous and obliging host. A few steps brought us from the hotel in the Corso, to the Strada Etnea: these are the two finest streets of Catania, the former stretching from the sea, to the west quite through the city; the latter cutting it at right angles and running towards the mountain from which it is named. As we turned the corner into this street, it seemed to extend nearly the whole of the route which we were to take, that is, to a distance of thirty miles and with a continuous ascent, to the elevation of 10,000 feet. Its line of direction cuts the mountain high up, but unfortunately a little to the south of its apex. A slight deviation westerly would have presented the whole rise of Etna, from its commencement midway through Etna Street, up to the smoky crest of the crater, and terminated a long vista of palaces with the sublimest object in the world.

Sallying from the city by a cottage delightfully situated at the extremity of the street, we followed, for the first six miles, the new and excellent carriage road, leading to Messina. We passed through a toll-gate, and it struck me as the first I had seen out of my own country. Two or three villages skirted the first part of the way with houses, and these with the fields and vineyards evinced a more thriving and happy population than we had noticed elsewhere in Sicily. Shortly after leaving the city, Abbate told us, we were passing over the port of Ulysses. It had been completely filled up by lava at an unknown period; that of Catania, on the other hand, owes its formation to the eruption of 1669. We dismounted and went a short distance from the road, to see an extinct crater. It must be a very ancient one; it presented the appearance of an irregular bowl, not more than two rods in diameter at the brim and with a small jagged orifice at the bottom: stones were dropped into this, and the sounds indicated frequent collision with the sides of the cavity, and but a trifling perpendicular descent.

About 6 o'clock, we reached Nicolosi, after an up-hill ride of twelve miles. The elevation, by the observations of Schow and Gemmellaro, is 2128 Paris feet, or about 2360 English feet. The evening air was rather keener than usual, but the fig, the orange, and the pomegranate, were evidences of a general security from frost. On the left of the village, towered to the height of 1000 feet, the scorched and menacing Monti Rossi, or Red Mounts. The course of the lava of 1669, can be distinctly seen, through the whole distance of twelve miles, from these two mountains, which it reared as landmarks of its source, to the mole of Catania, where it drove back the sea and forever bids it defiance. Its dark track, contrasting with the smiling beauty and luxuriance every where close upon its sides, brings fearfully to the imagination the horror and helpless dismay of the inhabitants, when beholding so tremendous a deluge of fire advancing upon their fair possessions, burying every trace of cultivated fields, of houses, churches and spires, climbing the walls of their city and finally marching over its ruins to invade the sea itself. The sea yields to this novel attack of her enemy under her own fluid form, and volumes of flames and clouds of vapor arise from this new war of elements. Who can figure to himself the sounds and sights and other terrific accompaniments of such an event; the constant detonations of the lava, drowned from time to time, by the louder thunder of the mountain, the lurid canopy of clouds glowing with the fires below, and the most vivid lightnings of heaven, paled by the intenser glare of earth? Surely, the ignorant peasantry of Etna, may be forgiven the superstition which ascribes calamities so dreadful, to the immediate agency of the most powerful and terrible beings.

We almost stumbled upon Nicolosi before we saw it. The houses of the village are low, as if crouching to avoid some impending danger, and it was easy to confound their tiled roofs, with the ground which had been burnt to a similar color in the hotter furnace of the volcano. They are built thus low from fear of earthquakes. Abbate soon guided us into the courtyard of one of these humble tenements. Passing through the kitchen we found one large room furnished with just enough beds for our party, and such beds too as we could leave without regret, at any hour however early. Mr. Mario Gemmellaro occupied the house adjoining, and we repaired to him for the purpose of getting the keys of the English house and of purchasing some of his charts and views of Etna.

He was a bluff, hearty man, whose broad face and florid complexion were the more striking, from their contrast with the pallid features of most of his countrymen. For many years he has been a fearless observer of the terrific phenomena of Etna, and has made them the subject of several published pamphlets. We sat awhile, and conversation turning upon the numerous eruptions from the sides of the mountain, he said he had incontestable evidence, that they do not proceed directly from the center of the earth or of the volcanic force by separate tubes, but that the lava arises in the grand central and original funnel, and that by the pressure of the immense column of fluid, a passage is forced in between the conical caps, of which the mountain by repeated eruptions has been gradually formed. By this passage the lava flows down *underneath* the crust, until it makes or finds openings *through* it, and by these, discharges itself into the air.

One proof was derived from observation of the times and distances of the successive explosions from the sides of Etna during an eruption. Those nearer the top precede those farther down, by a space of time proportioned to the distances of the discharges from the summit. He argued to the same conclusion from the heights of the jets. The lower down they are, the greater the force of projection, according to the laws of gravity respecting a fluid descending an inclined plane. Taking some of the charts and pamphlets we bid adieu to Sign. Gemmellaro.

We found our room cold and our beds hard, but soon forgot every thing in sleep. Owing to some cause I awoke during the night and heard a sound like a faint but deep murmur, as if from the struggling elements beneath. Whether real or imaginary, it unfortunately sufficed to dispel slumber, and to excite in my mind a feverish and ill-timed activity. Every image that memory had preserved of the old mythology, was passed in distinct review. The giants groaning and heaving under their uncomfortable loads, and the din of Vulcan and his huge one-eyed smiths, seemed no more fabulous than this vast smoking furnace, and the now audible roar of its fires: well might the Trojans tremble as they neared even the shore. Fancy then followed Empedocles into the crater, down which he had leapt, that men might believe him a god translated to heaven; but his brazen sandal, either vomited up by the mountain, or tossed out by some malicious Cyclops in scorn of its human workmanship, revealed the fate of the philosopher to mankind and changed their worship into laughter. The phantoms of antiquity finally vanished before the images of real life; I saw priestly processions alternating with motley troops of masquers, and mummery and antics absorbing by turns the pleasure-loving populace of Sicily: now they were on their knees in silent adoration of the passing host, now throwing their caps in air, and shouting on one leg, as if convulsed with delight at some inconceivable oddity of a harlequin out-monkeying Jacko. The rumbling of the mountain itself seemed drowned by the sound of the song and the dance, from the thousand villages which hang on the skirts of Mongibello.*

At length I sprung from my bed impatient of the ceaseless train of associations, which at any other time I should have enjoyed but now

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* Among the Sicilians, the popular name for Etna.

dreaded, as it bid fair to deprive me of that sleep which was so necessary a preparative to the labors of the ensuing day. The damp stone floor and keen mountain air which entered our room without much obstruction, soon composed me and I enjoyed an hour more of repose. Between two and three o'clock, the faithful Abbate aroused us with the news that every thing was favorable, that the night was clear and calm, and that a bright moon would aid us in riding over the broken lava. In midwinter it is all important to regard the state of the weather in ascending Etna. A high wind drifting clouds of snow renders the attempt always futile, and often dangerous. Having partaken of an excellent cup of hot coffee and bundled ourselves well with coats and cloaks, caps and moccasins, we mounted, and by half past three, our mules were moving slowly to the hearty thwacks upon their hides from the muleteers' cudgels.

Two guides accompanied us to enable any of the party to return, if necessary, without frustrating the rest. By the light of the moon we could see that our road was over dark scoria, or fragments of lava. On entering the Bosco or wooded region, small patches of snow began to appear, which rapidly increased in number and extent until they formed one continuous sheet. Our mules were soon floundering in it, and at 6 o'clock we were forced to dismount. The thermometer stood at 28°. A half an hour's walk on the crust of the snow brought us to the "casa della neve." The smoke was issuing in volumes through the door aud numerous apertures in the roof. A peasant from Nicolosi, had kindled a fire before our arrival.

We stopped but a few moments outside the "casa della neve" for the smoke precluded our entering it and we did not wish to breakfast, so throwing off our cloaks with a roll of bread in our pockets and more substantial fare in the knapsacks of our guides, we advanced, and sallying from the Bosco, saw the sun, then apparently about half an hour high. The thermometer at the "casa della neve" was at 27° but it rose from the effect of the sun as we ascended to above 32°.

Between nine and ten o'clock, Dr. H., was obliged to return with one of our guides: with the other we proceeded until we reached a stone pile of a pyramidical form distant one hour and a quarter from the English house, which the guide now descried for the first time. The ascent was here peculiarly laborious. A hard and slippery crust on the snow, together with the acclivity of the mountain, obliged us to turn our feet outward and stamp firmly with the inner edge of the soles of our boots, in order to make some footing; this was excessively painful, particularly to the ancle joints: in some places on the other hand the snow was soft, and lifting the foot from its deep bed to take another step was the most trying part of the labor; it was a pain caused by this which had exhausted the Doctor. We halted to rest our limbs and to enjoy the prospect which was increasing in grandeur with every step. Several times, we threw ourselves at full length on the snow and felt in all its luxury, that exquisite sensation of pleasure which attends the rapid recovery of the body from the fatigue of intense exertion. We rose above the level of Mt. Agnola, which we left to the right, and at ten minutes before noon reached the English house ("Casa degli Inglesi") which was so buried in snow that we could not enter it, although we had obtained the keys for that purpose from Sign. Gemmellaro. Travelers usually ride up to this place, and sleep and take refreshment before mounting the cone, which occupies but an hour from the English house. We were already worn out by six hours of most exhausting exertion: as there was no time to lose, we proceeded to make our first repast as well as we could, by taking our seats in the sun, under the lee of the building and tearing to pieces with our fingers a cold roast chicken. I had no appetite, ate very little and took no drink except snow melted in my mouth.

We here saw ourselves far above points, which, when we issued from the Bosco, appeared but little below the summit. The side of the mountain is covered with conical protuberances, whose hollow tops prove them to be the craters or vents of some previous eruptions. The snow was broken, in some few places, by black jutting rocks of lava. Our guides pointed out several wolf tracks and one of a hare. At a quarter past twelve, we started to ascend the cone, between which and the English house, was a space nearly level; on the other side of it, the snow which we had seen sprinkled with ashes sometime before, now became dirty, soon black, and after ascending the cone a little way, was succeeded by loose stones and cinders; from these a hot, sulphureous, suffocating vapor was steaming, our feet soon felt the change, and from being very cold became very warm. The ascent was steep and peculiarly difficult from the loose stones and cinders yielding under the feet: the vapor moreover was so dense that we could see but a short distance. Lieut. S. falling behind about three yards, we lost sight of him entirely, and knowing him to be much exhausted, we were afraid he had halted some way below; on calling to him however he proved to be very near. The wind was from the N. E. and by moving a little in that direction we were partially relieved from the fumes; we were infinitely more relieved soon after by seeing the desired point, but a short distance above us; another struggle and we were on the summit of Mt. Etna, at half past 1 o'clock, on the 23d. of February. My fatigue vanished, I felt a glow of satisfaction from the simple attainment of my object, before I had time to look around for any other reward.

The crater first attracted my attention; we stood on a point to the north and east of it in the best situation to view it, as the wind was northerly and carried away from us the clouds of vapor. Its form is very much altered within a few years by the ejection of scoriæ and other matter and the highest point of the mountain, where we then stood occupies the center of the old crater. Volumes of steam, smoke and ashes, were constantly pouring forth from the chasm, the eye sought in vain to fathom its depth, and the last sound of the fragments of lava thrown down indicated that they were still in motion towards their former bed of fire. There was no flame visible, but the vapor and the ground on which we stood were very hot, although the air was so cold that the thermometer held in it breast high, sunk to a little below 22° Fah. It was a pocket instrument and the capacity of the tube only 120°. We directed the guide to hold it in a cavity in the ashes and scoriæ made by our feet in standing there and barely sufficient to screen it from the air. The heat made him drop it, and on withdrawing it very soon, the tube was full and the ball burst. The vapor was strongly impregnated with sulphur, and fine crystals of the same coated the fragments of lava and other volcanic substances where we stood. The whole surface of the cone consisted of these loose and crumbling materials, and gases seemed to issue from every part as if the whole were porous. We picked up several specimens for our guide to bring down.

But our eyes were wandering from these more immediate observations to the magnificent panorama which the isolated situation of the peak renders peculiarly grand and entire. On every side, except in the direction of Italy, the view was bounded by sea and sky, and the former seemed to rise to meet the latter, so as to make the concave of waters correspond in some degree to the vault of ether above. The base of Etna floated in the lower hemisphere, but its apex soared far into the regions of the upper, and on it one might almost fancy the heavens nearer than the earth, and wish to start from such vantage ground, on his flight to another world. Sicily was reduced to a map which we could study far beneath us. Almost under our feet, lay Catania and the villages which sprinkled the mountain's base. Farther off to the south, Augusta and Magnesia jutted out into the sea, and beyond were distinctly seen Ortygia and Plemmyrium, and the black specks in the beautiful round basin of Syracuse, we knew to be the ships of our squadron. The eye wandered on to Cape Passaro, and following the course of Eneas fleet by the Geloan fields and Agrigentum, rested on the blue sea beyond Lilybeum and Mt. Eryx. A prominent hill indicated the site of Palermo, and the castellated rocks embosoming the beautiful vale of Enna, were conspicuous near the center of the island, and are now known by the name of Castro Giovanni. From there to the fountain Cyane it looked like a short distance, and must have seemed so to Proserpine, as the last flower fell from her bosom and she sunk from so bright a world to the dark realms of her uncourteous lover.

The mountains of Sicily are high and many of them were covered with snow; yet seen from Etna, they dwindle into hillocks, and with their intervening vallies give the country the appearance of gentle undulations and picturesque beauty, rather than the grandeur which characterizes most of its scenery from below. Two rivers wind sluggishly through the low meadows around the base of Mongibello, and it rises as if from the sea, prominent and well defined in its whole magnitude, and therefore more conspicuous and imposing than mountains of much greater elevation. To the north lay a lake, which with the village near it, our guide named Randazzo. We looked in vain for the Lipari islands, the only place in which our view was intercepted by clouds. Messina lies behind and at the base of an amphitheatre of hills, among which Mt. Chalcidice is between three and four thousand feet high, so that from Etna, Sicily *appears*, as tradition represents it to have *been*, joined on to Italy, and the snow-capped mountains of Calabria, seemed near and distinct enough to acknowledge the sway of the monarch of Trinacria, at least to tremble at the fearful demonstrations of his power.

Unfortunately we had left behind, our ship telescope, and the small one which was politely loaned us by Signor Gemmellaro, would hardly compensate for longer stay in the freezing air and burning cinders of the "Sommo Cratere." Our guide had animated us in our toilsome ascent, by speeches, high sounding enough for a Hannibal or a

Napoleon charging nature's battlements at the head of armies; but whether it was owing to our fatigue or to the aërial height at which they were delivered, they did not seem sufficiently misplaced to excite our laughter. On the summit he gave us the whole again, with an improvement of the subject. After a flourish on his own "invincible courage" and "consummate skill," wound off with some most flattering compliments to our fortitude and resolution, he informed us that a gentleman had once rewarded a similar exhibition of these heroic qualities, by the unreserved donation of all his wet clothes. Such an act of generosity on our part would have sent us to Catania à la Highlander.

A few minutes before two, we began our descent. The philosopher's tower was pointed out on the left of the English house; tradidition says that it was built by Empedocles, and thence received its present name. At a quarter past two, P. M. we were at the English house. An immense, rich looking cloud of a whitish color lay, far below us, floating like a canopy over Catania and its plain: it seemed to have gathered while we were busy in our observations on the crater or more distant objects, or rather to have become developed in the atmosphere almost instantaneously. Stopping a few minutes to enjoy this novel and magnificent sight, we refreshed ourselves with a swallow of wine, and descended to the "Casa della neve," in less than an hour over what had cost us six of the most painful exertion in the ascent.

A motion so rapid and yet so easy, I never achieved on my own legs before, for so great a distance; we rather bounded than ran down, as the stone of Sisyphus πέδονδε κυλίνδετο. The snow had become so softened by the sun that we sunk at every step, but only enough in most cases to enable us to check and regulate the speed which gravity created. If our feet were plunged too deeply, head and shoulders were equally so, with a jerk which threatened to snap the knee joints, and we stuck like a raspberry vine planted at both ends. A slip was less dangerous as it did not stop our momentum all at once, nor until we had first ploughed a handsome furrow in the snow. Notwithstanding these mishaps, nothing could be more exhilarating than the leaps by which we descended to the common level of mankind.

We found the Doctor, philosophically consoling himself for the unseen wonders of the crater, over a bright fire in the snow house, which was kept blazing and crackling by the trees of the bosco. Our horses being found farther on, we lost no time in regaining our inn at Nicolosi. Here although fatigue and hunger counselled us to stop, yet we chose rather to bear them two or three hours longer, than to try again the miserable pallets of the night before. We therefore with as little delay as possible, resumed our route to Catania, and arrived there at 9 o'clock. Our fatigue was almost insupportable, but Abbate led us on at a mercilés pace. For though not sharing the toils, he felt his full quota of the glory of heading an expedition which had overcome the rigors of a midwinter ascent "fino alla cima dell' Etna." The streets resounded with the crack of his whip and the tramping of our steeds over the pavements, and the fire from their hoofs marked the progress of our little cavalcade to the Corona D'oro; where we alighted at nine o'clock, with a sensation of pleasure soundly paid for, by eighteen hours of toil. Though we had eaten nothing during the day but a spare breakfast, yet repose was demanded more imperiously than food; a generous supper awaited our return, but swallowing only some warm broth, en passant, we left every thing to throw ourselves into that sweet oblivion, which could alone restore us.

Art. II.—*Considerations upon the temperature of the terrestrial Globe;* by M. Parrot:—read the 5th of May, 1830, at the Academy of Sciences of St. Petersburg.—*Memoirs of this Academy.*

Translated by Prof. J. Griscom, for this Journal.

If it is useful to make discoveries in natural science, it is not less so, to correct as many as possible of the errors which arise in this domain of human knowledge, and which are sustained by the authority or the assent of respectable savans. The history of science displays to us, upon numbers of its pages, important errors which having been thus accredited, have produced new errors and retarded fresh discoveries. The subject upon which I am about to treat, belongs to this class of intellectual phenomena. Aside from the mass of physical and geognostic knowledge, which has been accumulating for thirty years, we see the system of Leibnitz and of Buffon, upon the temperature of our globe, the system of central fire, revived, to the letter, from its ashes, gaining new partisans, and strengthening itself in appearance by a display of profound calculation, which the more easily imposes upon the majority of learned men, as they are founded upon a great number of experiments, which have no other fault than that of being badly comprehended.

I have already descanted at large upon this subject, in the *Bulletin Universel* of M. de Ferussac. But as we nevertheless see men distinguished in physical science, still adhering to a system which has been once refuted, I have for more than forty years,* thought it would be right to offer an exact analysis of the facts upon which the attempts to reconstruct it are founded. These facts consist of experiments made upon the temperature of the earth, at different depths and upon different points of the continent. I shall therefore examine these facts in order to assign to them exactly the value which they hold in the problem of the temperature of the terrestrial globe. Afterwards I shall produce the experiments made at great depths, and upon different points of the earth, in the ocean, and in lakes, to compare them with the terrestrial experiments. I shall then solve the apparent contradiction between the marine and terrestrial experiments. Finally I shall consider the hypothesis of central fire as a geological system, in order to examine whether it is not in contradiction to the better known geognostical facts, and whether it is capable of throwing any light upon the formation of the crust of our globe. This will furnish materials for four chapters."

Such is the distribution of the great work of M. Parrot, upon a subject which merits the attention of natural philosophers, and upon which they are still far from being agreed. It is impossible for us to follow him in the developement of the first of the four great divisions of his memoir, in which he discusses the observations upon which has been established the belief of an increasing progression of the terrestrial temperature from the surface to the centre, and in which he endeavors to demonstrate that these observations do not present the accordance and regularity necessary to the base of a law, more especially, he observes, if we consider that the depths to which we have penetrated, with the thermometer, have been only to seven hundred and two metres, and that the attempt is made to extend the result of these observations to the depth of twenty five or thirty leagues, that is

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* Those who would infer from these remarks, that I am unjust towards Buffon, are desired to read what I have said of his system in my *Discourse upon Physics*, tome vi, p. 681 and 684. No Frenchman even has ever spoken more honorably of this great savan.

to say, to a depth, two hundred and sixty or three hundred and twelve times greater. Neither shall we undertake to bring forward the dissertation contained in the fourth chapter, where he treats the problem under a geological point of view. We shall satisfy ourselves with offering to our readers the second and third chapters, in which the author is occupied with the temperature of seas and lakes, and with the manner in which he reconciles the facts obtained, with those which relate to the places where the earth has been penetrated.

"It has been proved above," says M. Parrot, "that the observations made upon the temperature of the earth, at different depths, are in no wise capable of founding the hypothesis of a central fire. But these proofs are merely negative. Nature furnishes others of a positive character, in observations made upon the temperature of the sea at different depths. The experiments of Irwine, Forster, Peron, Horner, and Lenz, made at so many points of the ocean, attest that this temperature diminishes with the depth, entirely contrary to what the experiments on the continent furnish.* Those of M. Lenz, have doubtless attained to the greatest depths, and at the same time produce the most exact results. As I have already exhibited them in the Bulletin Universel, and as this labor of M. Lenz, has appeared a short time since in the Memoirs of the Academy of Sciences, of Petersburg,† I shall abstain from recapitulating them. These results are principally :—

1st. That the temperature diminishes as the depth increases.

2nd. That it diminishes at first rapidly, then very slowly. From the surface to 413 fathoms depth, this diminution exceeds 23° C. and from that to 915 fathoms, it does not diminish 1° C.

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* I need not dissemble that two contrary experiments, have come to my knowledge. One is of M. Irwine, who in lat. 80° 31′ N. found in December, the temperature of the surface of the sea + 2°.2 F. = — 16°.6 C. and at 60 fathoms depth + 3°.9 F. = — 15°.6 C. which makes an augmentation of temperature of a degree in 60 fathoms, or about 120 metres in depth. The second is by M. Scoresby, in the 80th degree of N. lat. and 5° lon. from Greenwich, between Greenland and Spitzbergen. He found that the temperature increases to 7° F. or 3°.9 C., at a depth of 758 fathoms or about 1516 metres. (I do not exactly know how much the fathom of the English seamen is; but it is, if I am not mistaken, nearly equal to two metres.) If we consider that one of these augmentations of heat only amounts to 1° C. for 126 metres, and the other for 388 metres, we might infer that these two anomalies may be explained by the severity of the climate and the season, and do not weigh against so many other experiments made in all other climates and in so many different longitudes.

† See Bib. Univ., 1831, tome 1, (XLVI.) p. 275.

These experiments were made at different points of the ocean, from lat. N. 7° 20′ to 45° 35′ lat. S. and in an extent of lon. from 15° 17′ to 196° 1′. If we compare analogous experiments made in deep lakes, by de Saussure the elder, and de la Bêche in the Alpine lakes, by Georgi, Pallas and Gmelin in those of Siberia, by Schaw and Makenzie in western America, we shall find the same results, but upon a smaller scale; the temperatures of the bottom, have been constantly found lower than those of the surface. The experiments of M. de la Bêche, in the lake of Geneva, at different depths,* prove besides, that as in the ocean, the temperatures diminish at first rapidly and afterwards slowly.

If we unite all the experiments made in so many lakes, and in countries so distant from each other, and if we compare them with the experiments no less numerous made in the sea, in so many latitudes and longitudes, we shall be justified in concluding that we have discovered a natural law, which is that, the temperature, in the great masses of water, diminishes from above downwards, at first rapidly, then very slowly.† Now, is not this well established theorem, in direct contradiction to the hypothesis of an interior globe of matter, whose incandescent heat is the source of the mean heat of the earth at its surface? This is what we are about to examine.

Let C, be the centre of the earth, *ac* the level of the ocean, *bd* the mean level of the continents, *ef* the mean level of the bottom of the sea, *gh* the level of incandescent heat. It is certain that at the contiguous surfaces of land and water the temperature will be the same. Let us endeavor to find, in the hypothesis of a central incandescent globe, what this temperature would be. The mean increase of heat has been admitted, in this hypothesis to be one degree C. for each depth of thirty meters, La Place has estimated the depth of the ocean to be at least six thousand fathoms. My geological system, founded on my theory of volcanoes, presents the same data as a min-

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* See Bib. Univ. tome xii. (1819,) p. 119.

† If the temperature of the interior, arose from any warm medium situated at tho exterior, and if the diminution towards the bottom, was solely the chemical progress of caloric in this exterior medium, the progression of diminution would be much more rapid, and M. Lenz and the other navigators, would have found perhaps even at 100 fathoms depth, the temperature of zero of our thermometer. This heat of the ocean and lakes, proceed from the action of the solar rays, which penetrate the water and thus produce heat as far as the lowest depths to which these rays penetrate; thence the decrease of temperature slackens.

imum, we have then twelve thousand meters for the depth *ac*. Thus, in the interior of terra firma, we shall have at this depth a temperature of 400° C. admitting that the heat augments from above downwards at a degree for thirty meters depth. But, as we have just proved, observation gives us at this depth of the sea a temperature of about 0°. What becomes of the 400 degrees which should be found there? In order satisfactorily to answer this question in the hypothesis, it will be necessary here to quote certain principles of M. Fourier. The entire surface of the globe loses the temperature, accruing from the incandescent interior, by radiation into the infinite space which surrounds it. This loss which when the whole globe was still in a state of fusion was very considerable, is at present, and has been for a long time insensible; we may then regard the actual temperature of the surface of the globe as constant, and the calculation goes back thirty thousand years* the time when this temperature was diminished one half.—The water of the ocean is, as well as the body of the earth, a cooling medium, and carries off the interior heat by virtue of movements produced by the difference of specific gravity, more rapidly than this indefinite space, which occasions the temperature at the bottom of the ocean to be so low. Let us examine these principles and the consequence to be deduced from them. We proceed to radiation, which can only be, as I believe, the chemical progression, of caloric in a material medium, as I conceive the space of our planetary system to be, since new observations upon the comet of Encke have proved that there exists a material substance, although imponderable, which occasions a mechanical resistance to motion in it,—a substance which I have long since hypothetically admitted as a chemical medium (the radiation) of the light of the stars.

Thus we admit with M. Fourier that the terrestrial globe is continually parting with its heat and that this loss may equal the augmentation caused by the action of the sun and other luminaries, if the latter be considered as, of any amount.—Let us proceed to the examination of the cooling of the inferior beds of the sea.

The cooling of a liquid is produced in two ways, one of which is the chemical motion of caloric, the other, the circulation established

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* As M. Fourier bases his calculations upon the cooling of a homogeneous globe of iron, we may be assured after what has been said upon the heterogeneous nature of the beds in the crust of the earth, that this epoch extends back to at least three hundred thousand years.

by the difference of specific gravity, produced by the difference of temperature of the superposed strata. The experiments of Count Rumford induced this excellent experimenter to conclude that all fluids are perfect non-conductors of heat. Mine have proved that air and water transmit heat, but with extreme slowness, analogous to that with which chemical substances spontaneously mingle with each other, by their physical attractions. This principle of cooling must then be considered as of very small efficiency, even when the temperature of the warm medium is continually renewed; and the numerical law of heat at different distances from the surface of this medium, would become near this surface a very divergent progression, whose difference at some distances would amount to nothing. Now maritime experiments present a series absolutely opposed to this. This element of the diminution of temperature, therefore does not explain the phenomenon of the diminution observed in the contrary direction.

This marine phenomenon, must then, according to the hypothesis, be attributed to the second principle of refrigeration, and Mr. Fourier admits it as such, attributing to it even the vast and rapid currents which pervade the whole mass of ocean. But these currents are observed only at the surface of deep seas, and not at great depths. The experiments of M. Lenz indicate no current from the depth of a few hundred to one thousand fathoms. His colossal pendulum, it is true did not always maintain a perpendicular direction. But as M. Lenz had only a few hours of calm, the surface of the sea had not yet come to a state of perfect repose, and the vessel moved more or less rapidly in the direction of the waves. Thus the chord of his *bathometer* was rarely found in a vertical position; but as this angle was always found in a plane with the vessel's line of progress, and as the mean of all these observations (very much diversified by her rolling and pitching,) was only 9 degrees, there is no reason for admitting that these angles with the vertical were the product of currents in the interior of the sea.

In theory we must admit that at the bottom of the Ocean the upper surface of the earth, and the inferior surface of the envelope of water with which it is covered are of the same temperature, that the temperature of the earth (in the hypothesis which we are examining), goes on increasing towards the interior and as this has been the case for ages, or thousands of ages, the temperature of these contiguous surfaces, as well as that of dry continents, may be regarded as absolutely stationary. But if this be true, it is impossible that there should be found at the bottom of the ocean, beds of water lighter than the superior beds, and consequently currents could never be produced.

If on the contrary the 400 degrees of temperature which would be found at the bottom of the sea, in case the surface at the bottom was covered with terra firma, are still freely discharged into the ocean, then, this heat, especially about the protuberances which are more or less elevated above the mean level of the bottom, would form, indeed, currents which would mingle the inferior and superior temperatures as M. Fourier admits. But then it is indubitable that the mean inferior temperatures are always a little higher than the superior, as takes place in a boiler full of water when heated from the bottom; experiment should shew us the same effect at the bottom of the sea, at least from the depth where the temperature approaches to Zero. Further, the theory of M. Fourier tends to prove that even the 400 degrees above cited can only give out very low degrees of heat in the ocean, because the 400 degrees have not, for ages existed at the surface of the earth which supports the ocean but that it would be necessary to go to a much greater depth to find them. Now these low degrees of temperature communicated to the inferior beds of oceans, are not capable of producing any sensible currents, still less those rapid currents which M. Fourier appears to admit. Thus the internal heat of the earth admitted in the hypothesis, can in no wise cause the low temperature of the bottom of the waters.

But the illustrious geometrician has recourse to the polar regions. Let us see if the solution of the enigma can be found there, and first what are the means, and what ought they to produce. At the latitude of 70° to 71° the mean temperature is equal to Zero, and it is from the water between that point and the poles, that we should expect to find the desired refrigeration. We cannot suppose the mean temperature of this to be less than – 5° C.; in as much as the poles are actually covered if not with earth, at least with ice of an enormous thickness, which weakens the radiation of the heat of the water found below. Such are then the means whose effect is to cool the bottom of the sea to the temperature of Zero of the thermometer, what mean temperature should we admit the entire ocean to have, as the proper heat, given to it by the incandescent globe, if the refrigeration by the polar waters had not taken place? we have found this temperature, in the latitude of Paris, $=11^{\circ}.7$ C., and we shall not err much if we admit it for the whole ocean to be 12 degrees. Thus, in supposing that all the cold water which goes from the poles towards the equator may be equal to all the rest of the mass of the ocean, the temperature of the bottom should be at $+7^{\circ}$ C. But this portion of the polar water is not perhaps $\frac{1}{1000}$ of the rest of the ocean. Soundings that have been made along the whole coast of the North Sea which borders Siberia indicate only a very small depth. This water must, in order to get to the equator, proceed the distance of one thousand seven hundred and fifty leagues, with a prodigious slowness, and consequently lose a large share of its cold on the route. The cooling then in one year would hardly amount to $\frac{1}{2400}$ of a degree, and sixteen thousand eight hundred years would be required to absorb the 7 degrees in question. But during this lapse of time, the incandescent globe would have repaired this loss, and the more certainly as the limits whence the heat departs are found to be six thousand fathoms nearer the incandescent surface, and the transmission of heat would be the more rapid, as the water arriving at the poles would be the colder. Thus, although we cannot deny that the water at the surface of the polar regions, being colder than that at the bottom of the great ocean, would sink and move along the bottom of the sea towards the equator, yet it is not less true that this water cannot there produce much refrigeration, still less reduce, in the course of ages, the original temperature of the bottom of the sea to zero, and consequently cannot resolve the problem of the low temperature of the bottom of the ocean.

M. Fourier has again recourse to the temperature of the highest point of water without my being able to conceive how this consideration can be favorable to the hypothesis which he has adopted. Let this temperature be equal to $+3^{\circ}.75$ C., as a medium between the experiments of MM. Hällströne and Muncke, which appear to be the latest and most exact. But M. Lenz has already found this temperature at four hundred and fifty fathoms depth; whence it would follow that the rest of the depth of the ocean (five thousand five hundred and fifty fathoms) would have a less density, and that, in consequence, the superior warmer beds would fall to the bottom and warm it, not cool it. But there is still another consideration, this maximum of density does not exist in sea water; which M. Fourier appears to have been ignorant of. The temperature at which this maximum takes place, is, as I have shown in my *Grundriss der* *theoretischen Physik*, printed in 1811, properly the true point of congelation, that at which the water begins to congeal in infinitely delicate and invisible crystals and the augmentation of these crystals produces the successive augmentation of volume which attains its maximum at the precise congelation which furnishes the zero of our thermometers. But we have seen above, that congelation precipitates the greater part of the salt contained in salt water, by which its specific gravity is diminished. An approximative calculation easily proves that these two opposite effects cannot compensate each other, and still less produce an augmentation of volume on the approach of perfect congelation, the augmentation of density by commencing congelation being inferior to the diminution by the removal of the salt. This theoretic proof has been confirmed by the very exact experiments of M. Erman the younger, which have proved that salt water increases in density up to the moment of complete congelation.

Other partisans of central fire, again have recourse to the waters which flow from the polar regions by the melting of the ice, and which being heavier than the warmer water, descend towards the bottom, and passes to the tropical regions. After what has been said of the water of the polar seas, we shall not be disposed to allow much stress to this argument. We proceed to demonstrate that it is of no value whatever, however specious and palpable it may appear. First, we only object that to think of cooling the whole ocean in this manner, would be like attempting to cool the Lake of Geneva with a cubic fathom of water of the temperature of melting ice.* Let us examine the matter a little closer.

Let us take for a basis the temperature of 30° C., for that of the water of the sea under the equator and at the surface. The difference of specific gravity between this water and the same water there where the temperature is $=0$, will be $\frac{1}{185}$, if we admit according to MM. Dulong and Petit, a variation of density equal to $\frac{1}{5550}$ for 1° C. abstraction being made for the dilatation of the glass. On the contrary the superior degree of saltness of the tropical water produces, according to M. Horner, a difference of $\frac{1}{227}$ in favor of this water over the water at the latitude of 60°; and we can without risking any sensible error, shew this difference to be $\frac{1}{200}$ between the tropical

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* An attempt was thus made some years ago, to explain the general fall of the mean temperature of our whole atmosphere, by the melting of ice detached from the shores of Iceland.

and the polar water. Whence it follows that these two differences nearly compensate each other, and that to produce equilibrium, the sea ought to be, under the equator, a little lower than under the poles, and that if this difference of level establishes a current, it should take place at the surface, from the poles to the equator, and in the interior in a contrary direction. Let us now see the effect of a thaw of the polar ice.

We have two kinds of polar ice, that which is formed upon the continent, the continent being land covered with snow and ice, or simply ice and that which is formed by the sea itself. The first are evidently only glaciers, like those which are formed in the alpine regions of every climate. They contain absolutely no salt, this is confirmed by the observations of MM. Egédé, Sabie and Wrangle. The second are the waters of the sea frozen, and M. Wrangle informs us that between the 70° and 71° of N. Latitude this congelation does not exceed the depth of nine or ten feet. Thus these enormous masses of floating ice, which rise even four hundred and five hundred feet above the sea, and have at least eight to nine times more thickness under the sea, are glaciers of the former kind, formed upon a base of frozen sea water, which cannot be twenty feet thick; and this base itself contains so little salt, that it was believed for a long time that it did not contain any.* Thus we may consider the polar ices and the water which runs from them as containing only a minimum of salt, perhaps less than the water of most rivers. As moreover it is detached from only a few sides of the icy platform in comparison with the mass of waters which melt every summer from the surface of these great platforms, and which contain no salt, we may without sensible error, consider the entire mass of the waters which flow each summer from the polar regions as a water without salt, and we can pronounce, without uncertainty, upon the direction of these waters.

The sea water of these latitudes being like the water of the glaciers, at the temperature of 0, these two waters act, with regard to each other, as their specific gravity impels them, that is to say, the water from the glaciers will glide upon the surface of the sea towards the equator, without sinking at all; for although, during this move-

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* I have proved that salt water in freezing retains a part of its salt. See my *Grundiss der theoretischen Physik*, T. II. and the *Annalen der Physik*, T. LVII. p 144. It follows from my experiments that the inferior parts of the ice of sea water must contain a little more salt than the superior.

ment, chemical action may cause salt to be taken up by this soft water, and the winds produce a mechanical mixture with the inferior adjacent beds, still the specific gravity of the chemical and mechanical mixture will be always less than that of the inferior water. Farther let us follow in imagination the pure water of the glaciers with its temperature of 0 to the equator, where the highest temperature of the sea water is 30° C., the pure water will not sink; for the specific difference between one temperature and the other is $\frac{1}{185}$ and the difference between the sea-water under the equator and pure water, both at the temperature 0, is $\frac{1}{34}$. Thus it is in no case possible, that the water from the polar icebergs, can get to the bottom of the ocean, to lower its temperature.

We conclude with certainty from all that has been said relative to the temperature of the sea, in its depths, that the bottom of the ocean is about the temperature of 0, of our thermometers, and that this temperature increases with the elevations above the bottom. We conclude that this phenomenon is diametrically contrary to what ought to take place if there existed below the bottom of the ocean a source of heat, which communicated to the ocean its actual temperature and that the ocean proves in this respect, in relation both to cause and effect, the contrary of what observation points out on the continents.

We have shown in the preceding chapters, so complete a difference between the observations made in the interior of the continent, and in the depths of the sea in the range of temperature, that it may be called a contradiction. But there is no real contradiction in nature, and when it is supposed that we have found one, it is our ignorance which makes it so by attributing to one and the same cause, two phenomena which the ocean offers us.

The temperature of the sea diminishes from the surface towards the bottom, at first rapidly, then slowly, and finally during the greatter part of the depth with extreme slowness; whence we conclude agreeably to the experiments of M. Lenz, that at the bottom of the ocean the temperature must be about the zero of our thermometers. We are further obliged to conclude from this, that temperatures are owing to the action of the solar rays, and that the proper temperature of the globe at this depth cannot exceed that indicated by the zero of the thermometer.

But as it has been demonstrated that the hypothesis of a globe ignited internally cannot resolve the marine problem, it is also certain that the temperatures on land which have been observed to increase with the depth cannot be explained by the action of the solar rays. We are obliged to seek for another cause applicable to continents alone, and explanatory of these results.

This cause is stated in my *Physique de la Terre,* printed in 1815, namely the volcanic action which took place at the time of the formation of the crust of the globe, and with a much greater energy than is now developed. This ancient volcanic activity, is attested by the tearing and overturning of rocks, by the great number of ancient volcanoes which are now extinct, and which are scattered over almost all latitudes and longitudes, by volcanic productions in large and small masses, which are met with so frequently in places where volcanoes are not to be found, among the number of which may be mentioned basalt and its varieties; which is so scattered that the celebrated Werner was led to believe that basalts the last product of the general precipitation which formed continents that they had formerly entirely covered and were wanting only in places from which the breaking up and mechanical force of the revolutions had removed them.

The great force of volcanic action is demonstrated not only by ancient volcanoes which are still active, but by new volcanoes which are still forming upon continents and islands, in proximity to the shores, but this force is manifested particularly by earthquakes which so often indicate new theatres of action.

Volcanic action has then, ever since the formation of our globe, produced a very elevated temperature, capable of melting the rocks upon which it has exercised its immediate power in all parts of the present continents. One part of this temperature is spread in the interior of the globe in decreasing progression in the direction of the center, without our being able to know whether it has already reached the center, in any sensible quantity. The other part is spread towards the circumference, also in decreasing progression, and is dissipated more or less in the immensity of space. Volcanic operations are at present a remnant, a weak continuation of this great work which still produces unequally disseminated heat, the irregularity of which we might more clearly perceive if it did not take place at so great a depth.

We may add to this that volcanic explosions frequently eject pyrites which have often formed beds upon layers of existing rocks and which have then been covered with new rocks. These beds of pyrites, continually acted on by the water of the atmosphere percolating through crevices of the rocks, become new sources of partial heat, which being found incomparably nearer the surface than the centers of volcanoes, may sensibly increase, in certain places, the interior temperature at depths which have been reached with the thermometer. These are the waters which furnish us with mineral springs, warm if the water has only a short distance to flow before it comes to light, and if the action of the water upon the pyrites is energetic and the quantity of water small,—cold if the intensity of the action is weak, the course long and the quantity of flowing water considerable. I do not rank in this class the Geyser, the Ricum and other spouting fountains of Iceland, which I regard as immediate products of the volcanoes of that island.

We must also reckon among the number of causes of continental heat, fossil coal, mineral coal of all kinds, the antique remains of an immense vegetation which had existed in the latter periods of the formation of the crust, inflammable substances which are not (even at a moderate temperature) indifferent to the oxygen, either of the atmosphere, or of the water which finds access to them, and whose action may increase even to a kind of volcanic activity, such as are offered us by the sacred fires of Bakou, and other analogous appearances along the shore of the Caspian and Dead Seas; to which we may add those parts of the earth where petroleum and bitumen are formed, as in France, Italy, Germany, Bohemia, China, and North America.

I do not instance the heat produced by the great work of general precipitation, for the first idea of which we are indebted to M. de Humboldt, because this temperature, occurring over the whole surface of the globe, cannot serve to explain the local anomalies, which observation proves to exist.

These considerations explain the great anomalies which are observed in mines relative to the progression of temperature in the interior of the earth. We may add to the principal causes now cited, the slow oxidation of metals in the metallic state, and the chemical changes which some oxides, and even rocks, may undergo, by air, water, and carbonic acid. From these it will necessarily result that the temperature of the exterior strata of continents should be a little higher than the corresponding beds of the ocean, even independently of the difference of action of the solar rays upon liquids and solids, and of the greater evaporation from the surface of the sea than from the surface of the earth, and that the difference of temperature should be much greater as we advance further into the interior. These considerations would explain to us (even if the exterior refrigeration during winter does not already do it) why M. Scoresby found a slight increase of temperature at increasing depths in the sea between Greenland and Spitzbergen, the ground of these two masses of land, and indeed the bottom of the sea between them, being entirely volcanic.

These same considerations discover to us the possibility of a difference of proper heat between one place and another, at the depth where the thermometer is stationary upon the continents; thus the thermometric observations given in the tables at the commencement of our first chapter, show very palpable differences, at depths where all influence of climate ceases, and which can be attributed only to local circumstances. But as we know no other causes than those just expressed, and as these suffice to account for the phenomena, we think we have solved in all its points, the problem which constitutes the subject of this chapter.*

Art. III.—*An Enquiry into the Cause of the Voltaic Currents produced by the action of magnets and electro-dynamic cylinders upon coils and revolving plates;* by John P. Emmet, M. D., Prof. of Chemistry in the University of Virginia.—Jan. 1834.

Every person, conversant with the history of electro-magnetism, knows how long it was before it became satisfactorily proved that magnetism constantly accompanies the voltaic current; and, that after Oersted furnished, by a highly important discovery, the most conclusive evidence, it was nearly six years before he arrived at satis-

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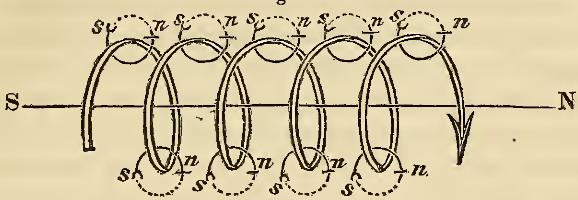
* The partisans of the system of an incandescent globe would perhaps attempt to explain these irregularities of temperature by the difference of conducting power of the different rocks, and of their appendages. But it will be immediately perceived, first, that this explanation will not apply to observations in places near each other, as two pits in one and the same mine, and it will not agree with the results which M. Fourier has drawn from his calculations, relative to the extreme slowness with which heat advances at present from the depth of 30 leagues to the surface. In order to obtain, at the depth of some hundred metres, the remarkable differences of temperature, and the increase of temperature at the same level, it is necessary that the cause of heat should be incomparably nearer these points than would be the incandescent globe; and it is on these accounts that we present the causes of heat that have just been enumerated.

factory results upon the subject. The difficulty of investigation arose, chiefly, from the peculiar manner in which the magnetic forces exhibit themselves; for, instead of acting in right lines, across or parallel to the current, as might have been anticipated from a knowledge of other forces, they invariably *revolve* in planes at right angles to the current, so as to act *tangentially* upon bodies placed within their influence. The opposite forces move against each other, and exhibit no tendency to interfere with each other's movement as long as the voltaic circulation continues. It is well known, also, that this singular manifestation, so different from that of any other kind of attraction and repulsion, soon became, in the hands of Ampère, the means of illustrating the construction and action of artificial magnets. The merit of discovery has, indeed, become almost obliterated by the brilliant results of subsequent research; although it is apparent that most of them must be regarded as merely illustrations of Oersted's first observations upon the five positions which a magnetized needle assumes, when placed near a voltaic current. The position of equilibrium to which the needle, in all cases, tends, lies exactly in the plane of magnetic revolution, and which itself is at right angles to the voltaic current. All the peculiar movements of the needle result from a disposition to take this direction, and when once in it, the current has no influence or power to invert the needle. Thus, when the needle lies north and south, the pos. current may move towards the east or towards the west, without changing its position. Hence both the magnetic forces revolve in the same plane, accurately, and at right angles to the current. Whether they are generated simultaneously in every part of the circuit, or rapidly propelled from one of the voltaic elements towards the other, cannot be determined; but there is some reason for believing that their distribution is unequal as to intensity, for very fine iron dust, as Mr. Watkins has shown, when sifted upon the circuit wire, arranges itself in *distinct bands* across it.

Magnetic forces revolving so independently of each other, must be under the influence of a common central attraction, either existing in the voltaic current, or the matter through which it passes. It is probable, therefore, that the curve they describe is an ellipse, and not a circle. Incessant rotation, while the forces are free to move, is eminently characteristic of magnetism, and such must be its existence in magnetised steel, according to the theory of Amperè, founded upon the electro-dynamic cylinder. Whether we regard their developement as depending upon a pre-existing voltaic current, or not, it is probable that they are in a perpetual state of rotation in the magnet, since they exhibit no tendency to neutralize each other, and yet possess so strong an attraction towards the steel as never to depart from it.

Fig. 1.



When a voltaic current passes through a helix, it may be represented as moving in planes at right angles to the axis; and, as the magnetic forces are known to revolve at right angles to the current, by this arrangement, their planes of revolution pass through that axis. Hence their general direction will be towards the extremities of the helix, the north polar forces all revolving towards one extremity, and the south ones towards the other, both being viewed from the same line, as the axis. Fig. 1 represents the magnetic action of a helix. The positive voltaic current moves from S. to N., and the small arches, *n s*, around every part of this heliacal current, mark the revolving magnetic forces; the cross representing the movement of the north pole, and the hook that of the south. It will be seen that similar poles have the same general direction, throughout the instrument, and that a south pole turns from the inside of the helix at the end S., while a north one passes out from the opposite end N. As these poles, in consequence of their issuing more directly from the interior of the extremity, obtain a preponderance of action, they represent the magnetic power of the helix; N. being its north end, and S. its south one. Along the sides of the helix, these poles have an equally direct action upon bodies in front of them, and hence they neutralize each other. When the extremities S. and N. are bent inwards along the axis of the helix as far as the middle, the voltaic current which each thus conveys backwards, compensates nearly for the obliquity of the coil, and makes the resemblance with the magnet more complete. The helix then becomes what Ampère has called the electro-dynamic cylinder and upon the action of which he has founded his hypothesis of magnetism. According to this philosopher's views, every substance, even the earth, obtains magnetic properties in consequence of the circulation of voltaic currents, passing through or around it; and it is obvious, that had he regarded the magnetism as independent of, or even prior, in its origin, to the other, there would have been no occasion to consider voltaic currents as contributing to the distribution of the magnetic forces. It is the object of this communication to show, that the currents actually *depend* upon the magnetism; and if I shall succeed in doing so, it will be necessary to modify that portion of Ampère's theory which involves the highly important philosophic error of substituting an effect for its cause. It is by no means intended to deny the existence of voltaic currents in the magnet; although I confess my great hesitation in admitting that a current, which Faraday* has so ingeniously attempted to show is the same as ordinary electricity, can perpetually revolve around the particles of so good a conductor as steel, without yielding some portion to the galvanometer. We can suppose the possibility of its regeneration by each particle, but the very fact of a *circulation* seems to prove that it should admit of being led off, by good conductors, as always happens in other instances. All experiments instituted since Ampère's theory was made known, but more particularly since the important discoveries of M. Faraday in relation to what he styles voltaic induction, show, abundantly, the confidence which philosophers feel as to the relation between magnetism and the voltaic currents; nor do I remember an instance where the attempt has been made to account for the phenomena by considering these currents to result from revolving magnetic forces, themselves arising from the ordinary process of induction. Upon reading M. Faraday's very able and interesting memoir,† the idea occurred to me that such a view might be taken; and subsequent reflection upon the subject has, I think, enabled me to explain all cases of voltaic induction by means of the magnetic forces alone.

When common electricity or magnetism effects induction, it is always upon surfaces placed nearly in *opposition* to the force; whereas the voltaic current, supposing it to act independently of its magnetism, produces the same effect upon surfaces *parallel* to its own position, and the induced currents, so far from exhibiting a constant relation to the creative force, are sometimes in accordance with its motion, and at others, in opposition. On the other hand, it is always true, that the rotating magnetic forces of these currents, at

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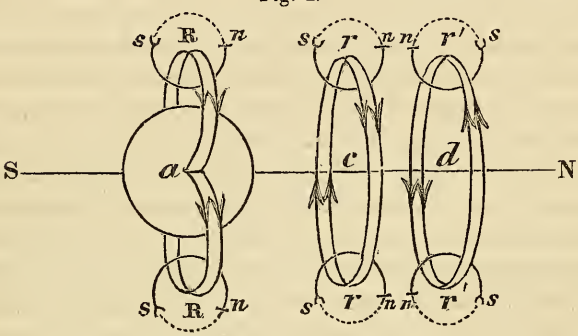
* London and Edinburgh Philosophical Magazine, September, 1832.

† Annales de Chimie, &c. May and June, 1832.

their origin at least, lie accurately within the line of magnetic induction. These circumstances, (independently of the consideration that a voltaic current, which theory supposes to have an atomic circulation, exclusively, cannot be supposed to operate beyond such limits,) are sufficient, in my opinion, to prove that we should regard magnetic *induction* as the remote, and magnetic *rotation* as the immediate cause of the existence and direction of all such voltaic currents. In order that these views may be perceived more clearly, I shall explain them by references to fig. 2.

Fig. 2.



Let *a* denote a particle taken from a magnet, around which circulates a voltaic current generated at *a*, and represented by the elliptical arrows; at right angles to this current revolve the magnetic forces in circles, R, R. *From the inside* of this current a north polar force revolves towards N, and hence that side of the particle, *a*, will act like the north pole of a magnet; whereas the other, since a south polar force revolves *from the inside* of the current towards S, will have the properties of a south pole, and the line S, N, will correspond with the magnetic axis of the particle. The action of one such particle must represent that of a magnet, which Ampère's hypothesis supposes to consist of an assemblage of them, arranged symmetrically; therefore let us suppose voltaic induction to take place. The elliptical arrows *c* and *d* represent the only directions in which it can occur; one of them corresponds with the current of *a*, but the other is in opposition, and neither is in the plane of its action, since both are parallel to it. Therefore, admitting the possibility that the current of *a* can act beyond its own limits, the positions of *c* and *d* necessarily place them out of the directions towards which the positive or negative fluids can possibly tend. But the planes of magnetic rotation $r\ r$, $r'\ r'$, of these induced voltaic currents, lie accurately within those of R R, which give the particle, a, its magnetic power; and hence, from position alone, they are capable of arising from magnetic induction, and are more probable to act as a cause of the voltaic circulation. I shall now proceed to show in what manner this may be accomplished.

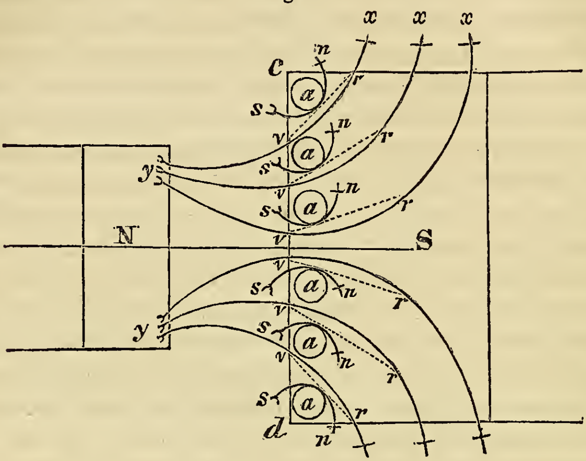
In doing so, I deem it unnecessary to discuss the question which may arise, whether copper and other unmagnetic metals are capable of receiving magnetism by induction. All these bodies, when acting as voltaic elements, not only generate, but transmit with ease both the magnetic forces; and we possess the best evidence, derived from the phenomena of thermo electricity, that circumstances connected with a mere change of temperature, and unaided by any thing like chemical action, are fully sufficient for the developement. If, moreover, as I think will appear probable from the view which I propose to take, magnetic induction be the cause of unmagnetic metals rotating under the influence of strong magnets, we shall be disposed to admit the possibility of effecting the same operation upon other bodies than the metals, since Arago detected voltaic currents under like circumstances, in revolving glass, resin and gaseous matter. One circumstance must be remembered, whatever value be attached to the conclusions of this enquiry. No magnetic rotation ever occurs, if the substance operated upon be a bad conductor of electricity; and, accordingly, though magnetic induction should take place, we cannot expect a voltaic current to result when the peculiar cohesion or other physical condition of the matter, opposes its circulation. Arago's results, just noticed, have been questioned by others; but their complete rejection would not affect the question under consideration.

I shall proceed, therefore, upon the supposition, that all bodies possess an unlimited amount of neutralized magnetic forces, capable of being liberated by the ordinary process of induction; and that, when the substance furnishing them is a good conductor of electricity, they not only revolve in circles around the component particles, but distribute themselves, *laterally*, so as to create a voltaic current. These magnetic circles become firmly established in a few bodies only, such as steel, loadstone, &c. The cause of this perpetual rotation is not apparent, but however we may explain the phenomenon, the difficulty will certainly not be greater than that which arises from the supposition of a *perpetual voltaic current* around the same particles. In unmagnetic matter, their developement is but temporary, and can be sustained only by a constant repetition of the inductive process.

One of the most remarkable circumstances attending the generation of voltaic currents, whether caused by an artificial magnet or a galvanic battery, is that *motion* is absolutely necessary for the effect. The motion proceeding from the rotation is quite insufficient, and that connected with the lateral distribution of the magnetic circles, along the circuit wire, is calculated to generate counter currents. What fact could be brought forward more opposed to the supposition that the voltaic fluid, considering it as common electricity, produces the effect by *induction?* No analogy sustains it, and the little probability attending it must disappear, if it can be shown that the result arises from magnetic induction singly. Although the rotating magnetic poles of a magnet or battery, are not capable without the aid of motion, of generating *voltaic* currents in steel, they readily produce the *magnetic* forces; and hence, it would follow that either the latter do not depend upon the same conditions as the former, and are, therefore, capable of a separate existence, or that the voltaic currents thus generated without the motion of the steel, at once pass to the particles and confine themselves exclusively to their limits. The latter supposition is the less probable because there is not the slightest evidence that voltaic currents can be produced under like circumstances, in other metals, for all of which motion is absolutely necessary, and when generated the circulation can be made to pass into the galvanometer. There is, obviously some great peculiarity, characteristic of iron and its magnetic compounds, which thus enables it to undergo the process of induction by the unaided revolutions of the magnetic forces, in a magnet placed close to it, and which cannot be accomplished with other metals unless they are moved during the exposure. Without wishing to offer any positive opinion of the cause, it may be suggested that perhaps the forces, proceeding from a magnet, upon passing into the steel, suffer a *deflection*, as the rays of light do when they enter transparent media, and which enables them to act more powerfully upon one side of every particle composing a layer of metal, than upon the opposite one. The result might be, that free polar forces would be put in motion, at the points of all the particles most directly opposing this deflection.

Thus, let N, fig. 3, represent the north pole of a magnet and $y\,x$, $y\,x$, &c. its polar forces acting in curves, like those of the electro-dynamic cylinder, or in right lines diverging from the magnetic axis N S. Let S denote a piece of steel placed within their influence. Now it is evident that were they to pass uninterruptedly, i. e. by the

Fig. 3.



lines *y x*, they would act with equal intensity upon the opposite sides of the line of particles *a*, *a*, *a*, *a*, &c., through which they pass, and thus destroy each others influence. But if we suppose that, upon entering the surface *c*, *d*, of the steel, they suffer a deflection from the magnetic axis N S, their action would be thrown into the dotted lines, *v*, *r*, and this would bring them in contact with only one side of every particle. Induction, therefore would commence upon the under sides of all the particles, *a*, above the magnetic axis N S, and upon the upper surfaces of those below it; the north poles of all the superficial particles, here only represented, would be repelled and revolve from N, while corresponding south polar forces would, in consequence of attraction, revolve around the particles, towards N; and as the latter are those that *issue from the surface c d*, they would give to the steel, S a south polarity, the opposite of that possessed by N. The duration of this rotation, after the magnet is withdrawn, most probably depends upon the molecular condition of the steel, since we find that caloric, which expands the particles, enables the forces to return to a state of neutrality.*

* This mode of explaining magnetic induction, is offered as mere conjecture. It does not, indeed, account for the absence of a voltaic current; but is not opposed to the supposition that the circulation is confined to the particles, individually.

The deflection may be supposed to be equally good in soft iron, but that the coercive force is feeble, so that the rotation soon ceases; whereas, in unmagnetic metals, it is probable that there is neither deflection nor coercion, and that in consequence of this difference, they can receive magnetic induction only by an extraneous motion, either of the magnet or the metallic body, so as to imitate the deflection.

Upon this inequality of the inductive process at the opposite sides of every particle, rests my hypothesis; I shall, therefore proceed to notice its application to unmagnetic metals, more particularly.

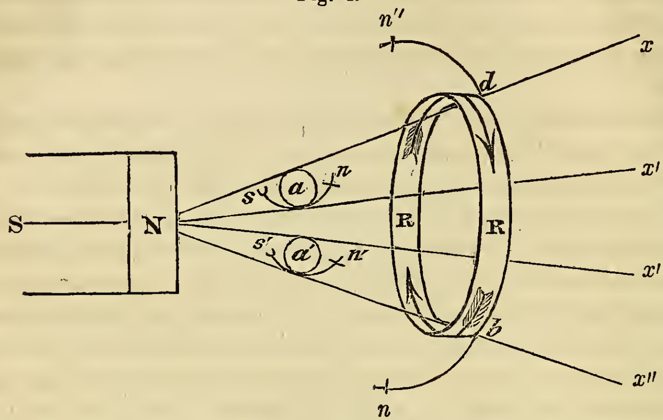
That motion is necessary for voltaic induction is now demonstrated beyond a doubt. Ampère* has satisfactorily shown that when one helix, connected with a voltaic battery (and therefore highly charged with magnetism in an active state of motion both around and along the circuit wire) is introduced within another helix, connected with the galvanometer, no currents are produced in the latter, while at rest; but, upon the slightest movement along the axis either to or from the galvanometer helix, they become at once apparent and continue during the motion. Upon entering the helix and upon leaving it, *opposite currents* are produced; a fact that cannot well be explained by reference to the current of the battery, which never changed. M. Faraday, as far as I understand his views, supposes that during the approach, the galvanometer helix gains what he calls *electric tension,* producing a current of one denomination, and that upon reversing the motion, this tension is either destroyed by re-action or reversed, so as to occasion a current of an opposite character. Observations from so sagacious an inquirer are not to be viewed negligently, but I hope to make it apparent that such opposite currents admit of explanation without the necessity of adopting so purely hypothetical a condition of matter. Considering the magnet and electro-dynamic cylinder to effect magnetic induction upon similar principles, I shall confine my illustrations to the former.

Let N, fig. 4, represent the north pole of a magnet and Nx, Nx', Nx'' lines of action. Now if we imagine two particles of copper a a' to be placed between them, but at rest, these forces must act with equal energy upon all the opposite surfaces, supposing no deflection to take place; and, as all of them operate from the same point N, no magnetic induction sufficient to occasion rotation, can ensue.

* Annales de Chimie, &c. December, 1831.

If, however, we make these particles move downwards, in front of the magnet, one surface of each, the under one, will enter upon the line of induction at the very instant when the opposite one is depart-

Fig. 4.



ing from it. At the under sides, therefore, it is supposed that the process commences and continues during this kind of motion. From the under side of a, the force Nx' will repel a north polar force towards x' and from that of a', a similar one will revolve towards x'', each being accompanied by equal forces of an opposite character, moving in a contrary direction in consequence of its attraction towards N. The arches $s\ n$ and $s'n'$ represent the respective directions of these poles, which are supposed to revolve in curves around the particles, in consequence of their mutual attraction for the matter. When the motion is discontinued, so will also the induction cease, and the polar forces, after revolving for a time in proportion to the impulse already received and the corpuscular attraction, pass into the particles and neutralize each other. But when the motion is continued in the same direction, fresh magnetic forces act in quick succession upon the same surface, so as not only to augment the force and amplitude of the rotation produced by preceding ones, but to lead to their extension *laterally*, by the rapid generation of similar circles. The result is the circulation of a voltaic current, passing through the particles at right angles to the magnetic rotation. the same particles a, a', to move *upwards* before the magnet, at their upper surfaces will commence the induction, for the reason already assigned; and this, leading to a rotation the reverse of the former, will be accompanied by a current, likewise of an opposite nature. It is also well known that when the metal in which the induction takes place, is made to move to and from the magnet, opposite voltaic currents, result; the cause of which, admitting the hypothesis, will be apparent. For it is obvious that the magnetic forces Nx, Nx', Nx'' do not act in parallel lines, but diverge from the point N, of the magnet; and whether we attribute this want of parallelism, to a repulsion among themselves, to an attraction towards the opposite poles of the instrument, or, which is most probable, to the curvature that characterizes their action as they issue from the polar surface, and which is exhibited more intelligibly by the electro-dynamic cylinder, still it will follow that the particles, placed between them, must be more influenced by those forces intercepted than by those upon the opposite sides, and from which they are receding. Accordingly, the *outer* forces Nx, Nx'' will effect induction during the motion towards the magnet and the inner ones during the retreat. The difference between the four movements will be apparent from an inspection of the figure. All particles in their motion downwards will have a similarity of magnetic rotation, which also will happen when they are moved upwards; but the direction in the last case will be the reverse of the former. When, however, we suppose the motion to be to or from the magnet, counter currents will arise on opposite sides of the magnetic axis S N, all the particles, above it, acquiring one kind of rotation, and all those below it, the opposite.

Viewed in relation to single particles, the difference of induction, here supposed to result from motion, upon opposite sides, may appear to be very inconsiderable; but it must be remembered that the portions of matter, simultaneously influenced, are infinitely numerous and are exposed, almost in the same instant of time, to an infinity of forces acting in uniformity with each other. The disproportion is, perhaps, not much greater, (for infinity limits both,) than that which exists between an atom and the universe. But it is freely admitted that all this reasoning should have no weight attached to it, unless fully supported by experiment; I shall, therefore, proceed to determine the direction of the voltaic currents corresponding with these circles of magnetic rotation and show that the hypothetical deduction is in full accordance with their actual existence. This point being satisfactorily established, the conclusion urges itself irresistibly, that all voltaic currents, arising from the influence of a magnet or electro-dynamic cylinder, are caused by the ordinary process of magnetic induction, *remotely* and *immediately* by the order of rotation.

If we place a metallic ring R R, fig. 4, near and parallel to the north polar surface N, and then suddenly withdraw it, the outer edge will undergo magnetic induction inasmuch as it intercepts the polar forces Nx Nx'', while the opposite or inner edge, is passing out of similar ones; hence north poles will be repelled from within the ring towards x, x'', as shown by the curved crosses n'', n'', at the points d and b. Now it is an established fact in electro magnetism, that whenever a north polar force revolves from right to left *over* the current, a positive current moves towards the observer; the arrow on the ring at d, marks its advance. When, on the contrary, the north polar force revolves from right to left *under* the current, the latter if *positive*, moves from the observer. This is the case at the lower part of the ring, as denoted by the arrow. The effect of withdrawing the ring from the magnet, is therefore, to create a homogeneous current through the ring, the direction of which corresponds fully with the calculated results. If we now move the ring towards the magnet, the *inner* edge will receive the magnetic induction and thus lead to the developement of a rotation and voltaic current the opposites of the former.

When the same ring is moved before the magnet in any direction *laterally*, but still parallel to this position, fig. 4, there are opposite currents produced in each half of it; a fact in full accordance with the explanation given for a similar motion of the particles a a', of the same figure. In this instance all the forces upon one side of the magnetic axis S N, will produce induction upon the *inside* of the ring, while those upon the opposite side of this axis, produce the same effect upon the *outside;* the action being in every case exerted from the point N.

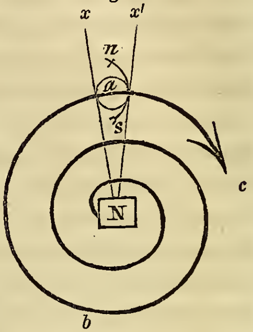
Theoretically considered, therefore, no voltaic currents should appear in the ring, when moved upwards, downwards, sidewise or in any direction perpendicular to the magnetic axis S N; but it is equally obvious that such a movement cannot be sustained without producing a preponderance of magnetic forces upon one side of the axis, and these prevailing, will occasion weak currents. Thus, if we move the ring downwards, the upper half circle, by getting more in front of the magnet, receives its induction from very powerful forces while the under portion is influenced by the more feeble ones. The current is found, by appeal to experiment, to be very feeble, and this makes the hypothetical indication the more probable. When we place the ring so that only its upper margin stands opposite to the polar surface, then, upon moving it upwards, the outer surface of that margin will receive induction and establish a positive current moving from the observer; but upon pulling it back again to its first position, it is the inner surface that generates the magnetic circles and the rotation, being obviously the reverse of the former, the positive current will move towards the observer. All these results are fully confirmed by the currents actually formed.

If, instead of a ring, we employ a flat coil, covered throughout its length with silk or other non-conductor of electricity, the current may be conducted either to the circumference or the centre, by simply subjecting the opposite sides to the same kind of motion before the magnet; but the inversion of the current is still only apparent and the result always conforms with the magnetic rotation, as indicated by theory. The mechanical construction of a flat coil is such, that, when we look at the winding upon one side, which turns from left to right, over, that, upon the other, becomes from right to left, the opposite of the former. Hence it happens that a current, always having the same direction, will pass to the centre of the coil, if one side be exposed to the magnet, and to the circumference, if we substitute the other. The current necessarily follows the curvature of the wire in consequence of its insulation. Thus, if we take a perpendicular wire conducting downwards the positive current, and bend it either to the right or left, we give a corresponding direction to the current, so that the rotating magnetic pole which revolves horizontally from right to left across the near side, may be made to turn over vertically, from above downwards or from below upwards. The natural position of the transverse magnetic forces is in planes parallel to each other, but, as the curvature of a coil or helix necessarily destroys this condition, without any obvious diminution of intensity, we may infer that these magnetic circles have but little action upon each other, laterally.

Perhaps we could not have a better proof of the truth of the hypothesis offered in this communication, than that furnished by examining the influence which a magnet exerts upon a flat coil, in its different positions. Thus, when the axis of motion is common to the magnet and coil, so that both may be made to revolve singly or conjointly, the theory indicates that the *magnetic* circles, instead of distributing themselves laterally either to the centre or circumference, so as to form a continuous current, actually revolve in planes parallel to the coil, and consequently the voltaic currents must circulate *transversely* around the wires. I shall illustrate this by supposing the magnet to be stationary, and the coil to revolve, at its center, around the magnetic axis.

Let N, fig. 5, represent a vertical section of the axis, at the north polar surface, and *a*, *b*, *c*, a flat coil, revolving, vertically, at its centre, around the pole N, and having its direction indicated by the terminal arrow at *c*. Any particle, as *a*, included within two lines of polar action, will, in consequence of the supposed motion, receive induction from the force Nx', and from the point of interception, a north pole will move around the particle towards x, and a south one towards N. But as the force Nx' lies in the same plane as the coil, so will the induced magnetic circle *n*, *s*; and the voltaic currents, which result from this action, must of necessity move across the wire, at *a*. Hence it cannot be detected at other parts, as *b* and *c* of the coil. If we employ a copper plate, the result would be different, but only in consequence of an *extension* of this transverse current; for the plate forming a continuous conductor from circumference to centre, the positive and negative elements would distribute themselves in this direction, and even enter the wires of a galvanometer when they are placed upon the radius *a*, N. By the motion, as here represented, (fig. 5,) a positive current would issue from this side of a particle, at *a*, and descending over the near surface of the plate, proceed towards its centre. Reversing the motion, will obviously invert the direction of the current.

Fig. 5.



For the full effect of induction under such circumstances, it is not necessary that there should be a plate at all; for, if a magnet, supported at its extremities by pieces of metal, be made to revolve upon its axis, these currents will be generated in the supports, and may be drawn off by wires placed in contact with either the magnet or the metallic fixtures. Nobili is, therefore mistaken, when he says that currents cannot be generated by a concentric rotation. The highly interesting researches of Faraday, published in the second part of his memoir,* place this question beyond a doubt; for he detected

* Annales de Chimie, June, 1832.

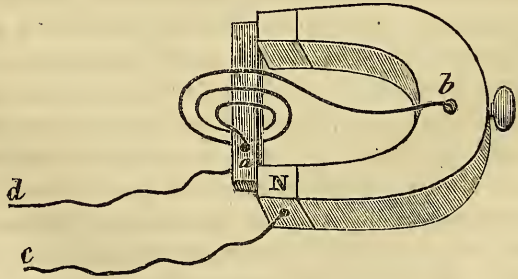
currents along the magnet, when the latter, without a plate or coil, was made to revolve upon its axis either in the open air or while immersed up to the middle in mercury. Although he does not give the particulars as to construction, I am inclined to suppose, that in the case where the magnet alone revolved on its axis, a metallic communication existed between the magnet and its fixed supports, also probably metallic. If such was his arrangement, there is no doubt that one kind of current was generated in the north support, and the opposite in the south one, and that both these currents passed into the magnet to neutralize each other. It does not seem even necessary to suppose that the circuit became complete by the union of these forces at their opposite extremities outside of the magnet, but it is probable that a metallic communication enabled them to do so. It is equally apparent, that when the magnet was made to revolve in mercury, the currents originated in this metal as well as in the support at the other pole, and that the magnet became the line of communication for the opposite currents. Viewed in any other light than that furnished by the hypothesis proposed, I am inclined to believe that these peculiar cases of concentric rotation would appear paradoxical. The deduction seems to establish the rule, that when a flat coil or plate is made to revolve, centrally, upon any point around a magnetic pole, the tendency will be to form currents, moving to or from the centre of motion, in the line of radius.

It will be intelligible, from what has already been said, why currents cannot be generated, by any kind of motion, when the same side of a flat coil or plate is exposed to the simultaneous action of opposite magnetic poles, for counter currents must ensue in the same portion of metal. Directly the reverse of this will follow, however, if we expose opposite sides to the opposite forces. When a coil or plate is introduced edgewise between the poles of a horse-shoe magnet, it obtains this position, and must necessarily be exposed to forces, which, from their proximity, gain a power of action, by mutual induction, far superior to that of any others situated elsewhere. Accordingly this interpolar position is the most favorable for generating voltaic currents. It has its disadvantages, however, for only a portion of the coil can be operated upon at once. If we exceed this, the motion being the same, counter currents will arise. When we introduce the coil between the poles, so that its centre lies in the plane of the magnetic axis, about one half will be in action; and the removal of it, in the same plane, will produce an opposite current to the same extent. If, however, after having placed it as far as its centre, between the poles, we raise or lower it until it passes quite through, counter currents at first arise, because the magnetic forces on the upper side of the magnet, act upon one surface of the coil-wire, while those upon the lower side affect the opposite surface; and a uniformity of rotation does not commence until one quarter of the coil is more influenced than the other. The maximum effect resulting from this arrangement therefore, never exceeds that which can be produced by a uniformity of action upon one quarter of the coil.

When, in addition to this interpolar position, a piece of soft iron is made to pass through the centre of the coil, but without touching it, the iron, by its approach to or contact with the magnet, acquires powerful but temporary polar forces, which become highly developed when we make it slide upwards or downwards from the magnet. The latter motion adds so much to the promptitude of action, that we obtain as strong a current with only one quarter of the coil, when we make the armature slide quickly from the polar surface, as we do with one half, when the armature is pulled off.

Shortly after having become acquainted with Nobili's process for obtaining sparks from the horse-shoe magnet, and long before Pixii's improvement, or even Faraday's memoir reached me, I succeeded in obtaining brilliant scintillations, and most unpleasant shocks, by a new arrangement of the apparatus, and which I am here induced to notice in consequence of a remarkable circumstance attending its action. An account of it was published in vol. xxiv. No. 1. of this Journal; but, for present purposes, a reference to the accompanying sketch (fig. 6) will be sufficient.

Fig. 6.



The coil has the armature of soft iron passing through the centre, and connected at one end, *a*, with it. The magnet is united to the other end, *b*, so as to form a perfect metallic communication, through the coil, between the magnet and armature. From each of these portions of the instrument proceeds a conducting wire, *c*, *d*. The connections at *a* and *b* being preserved, if the armature be made to slide from the magnet, very brilliant sparks appear between the latter, at the last point of contact; and the wires *c*, *d*, if taken into the mouth at the time of separation, will communicate very unpleasant shocks, or if brought close together, furnish a small but vivid spark. Yet the current actually passing through these wires, scarcely admits of detection, even by the galvanic multiplier. As, however, the effect varies with the magnetic intensity, length of coil, &c., the observation will be best sustained by an appeal to experiment. The magnet, coil, armature, and galvanic multiplier being the same in all cases, the following results were obtained.

1. One half the coil, moved downwards between the poles, gave, alone, a declination of 12°.

2. One half the coil, moved downwards between the poles, with the armature, gave, alone, a declination of 70°.

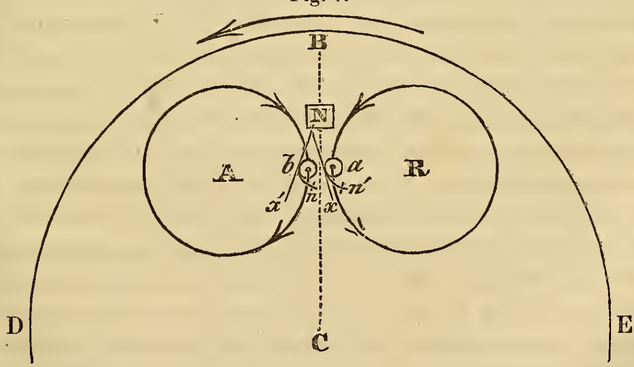
3. One half the coil, moved downwards between the poles, and arranged as in fig. 6, gave, alone, a declination of 5°.

Here it will be seen that a current, equal to 70° when the armature merely passes through the coil, becomes diminished to $\frac{1}{14}$ by merely making the magnet form a part of the circuit, as shown in fig. 6. That the armature contributes nothing to this effect is shown by the fact, that an equal reduction takes place when the end of the coil, *a*, is separated from the armature and made to touch the magnet, so that upon breaking off the contact of the armature, this end also becomes removed. The result, therefore, clearly depends upon breaking off the circulation in the magnet and throwing it suddenly into the wires *c*, *d*.—It is easy to perceive why this arrangement should furnish a smaller current than even the coil alone; since, up to the period when the armature leaves the magnet, a *close* circuit exists and the wires *c*, *d*, cannot possibly receive any portion. After this separation the coil alone acts, but as its position is then necessarily *lower down* than when it is used alone, so the resultant current must be less. What then produces the shock, by this arrangement, when the wires *c d* are taken into the mouth, or the spark, when they are close to each other? The galvanometer shows that there is scarcely any fluid circulating through them at the time. The result of my observation upon the three positions, establishes the fact, that the shock is not at all in proportion to the intensity of the current; for the second arrangement which affected the galvanometer to the extent of 70°, gave scarcely any sensation to the tongue; whereas that in, fig. 6, hardly moved the needle of the galvanic multiplier and yet occasioned shocks as disagreeable as those produced by an active galvanic battery of fifty plates, four inches square. I do not think that the distinctions of intensity and quantity will solve the difficulty; for, if the want of action upon the galvanometer and the power of giving shocks, be owing to the passage of a fluid, great in quantity but of weak intensity, then we should expect to find common electricity in circulation, and this was my own opinion when I first noticed the phenomenon, but the gold leaf electrometer was not in the slightest degree affected by the arrangement of fig. 6. It is nothing unusual to have shocks following the sudden interruption and renewal of common or voltaic electricity, but, in all such cases, the preexisting forces are powerful and proportionate to the effect. Upon a late occasion, while performing the usual galvanic experiments upon an executed criminal, I had an excellent opportunity of proving this fact in relation to muscular action. The prostrate arm, by the first impulse, suddenly became elevated, but fell down as rapidly although still under the full influence of the uninterrupted current; yet, when, by quickly tapping the circuit wire against an extreme plate of the battery, a succession of impulses was created, the lifeless arm preserved, vigorously, its upright posture. It is probable, therefore, that the nervous fluid, supposing it the same as the voltaic, occasions muscular action less by quantity or intensity than by the distinct repetition of its impulses, the rapidity of which must be inconceivably great even during its ordinary action, but infinitely more so when it sustains those tremendous convulsive efforts which characterize Opisthotonos and other tetanic diseases. Another physiological conclusion suggests itself, as more akin to our subject, and which might almost induce one to become a convert to the doctrine of animal magnetism. It has been fully proved by Dr. Davy and others, that the voltaic current, generated by those animals which possess the power of giving shocks, is constantly accompanied by the transverse magnetic forces. If, therefore, the nervous power which occasions ordinary muscular motion and the different operations of secretion, be considered the same as the voltaic, it would follow, that every nerve possess a tangential magnetic force; and, further, that, upon the supposition that an uniform current proceeds from the brain down through the spinal column, there should be a tendency to produce north and south poles upon opposite sides of the body. Let those, therefore, who indulge in such general speculations, seriously reflect in what a condition the *brain* must be, with its whirling magnets and electric conflicts!

I shall now proceed to the conclusion of this enquiry, by making an application of the hypothesis to those cases of rotation first announced by M. Arago in 1824. The most important facts are as follows—when a copper plate is made to revolve on its centre, under the influence of a powerful magnet placed over its marginal surface, or, indeed, in any other eccentric position, compared with the plate, the magnet receives four distinct impulses, two of which, are the result of suddenly changing its position, so as to place it nearer the circumference or the centre of the revolving plate.

If, however, the magnet obeys the influence of the plate, alone, and has not its position otherwise altered, it receives the other two impulses—one tending to make it follow the plate in its revolutions, and the other to repel it perpendicularly. Any unmagnetic metal may be substituted for the copper with similar, although, generally, diminished effects; and as the action and reaction are equal to each other, all the movements may be obtained by giving the rotation to the magnet and the freedom of motion to the plate. In my illustrations, I shall adopt the former and suppose the plate to revolve.

Fig. 7.



Let D B E, fig. 7, represent a portion of the copper plate, revolving horizontally at its centre C, and in the direction of the arrow at B. Let N represent the position of the magnet, suspended over the plate, and Nx Nx' two of its north polar forces, projected upon the plate so as to cut it at the points a and b; these letters also denoting two particles composing the plate. In consequence of the rotation in the direction indicated by the figure, the induction of the particle *a*, will commence upon the surface cut by N*x*, and next to the line BC, whereas the same process will commence, for *b*, upon its surface most remote from the line B C. As the magnetic forces of the magnet, act in planes perpendicular to the revolving plate, so will the induced magnetic circles *n n'*; and the voltaic circuits resulting from this rotation, must necessarily coincide with the plane of revolution. Both the particles *a* and *b*, will have a north polar force repelled around them, from left to right, *under*, and must accordingly be penetrated, at right angles to the rotation, by a *positive* current moving towards the centre C, of the plate. Now experiment fully indicates the existence of such a current, as will be shown presently. These positive currents, issuing from the particles, are represented by the circular arrows upon each side of the magnet, and it is supposed, that being accompanied by a negative current, moving in an opposite direction, each one, instead of proceeding in straight lines to the centre of the plate, turns outwards, as it advances, in order to meet its negative current; and thus the two voltaic circles A, R are formed, upon either side of the magnet. As all the magnetic circles arise within the limits of the forces projected from the magnet upon the plate, it necessarily follows that the strongest currents will be found passing under the magnet, in lines more or less parallel to B C, or the radius which bisects these projections. The galvanometer abundantly proves this to be the case, and a few positions of the instrument, fully confirm the hypothetical direction, here given, to these currents. Thus, when one wire of the galvanometer is placed at C, and the other at *a* or *b*, under the magnet, the latter will be found to convey a negative current; but when the wire at C is removed to B, near the circumference, that one at *b* or *a* remaining stationary, the latter will convey a positive current; consequently, opposite currents must issue from beneath the magnet, a negative one moving towards B and a positive one towards C. When the same wires were placed, one on each side of the radius B C, I could obtain no evidence of a circulation, which circumstance seems to indicate, that, in these positions, they rest upon two distinct systems of voltaic circulation, A and R; and this opinion is confirmed by the additional fact that currents enter the galvanometer wires, when both are placed upon the same side of the line B C, even though they are removed from each other as far as the limits of the plate will permit. Were it not for these circumstances, I should be led to conclude, that the mass of positive and negative currents, generated under the magnet, proceeded directly towards the opposite points C B, and completed the circulation, along the same line, upon the opposite side of the plate.

The supposition, that they form two distinct circles upon the upper surface of the plate, one on either side of the magnet, seems, however, to be admitted both by Faraday and Nobili. I shall, accordingly adopt it, and proceed to illustrate the movements of the magnet.

1st. *Rotation with the plate.*—It will be seen, by reference to the 2nd fig. at the commencement of this enquiry, that the magnetic force which issues from the *interior* of a voltaic circuit, gives to that side of the circle, its active polarity. This is a general rule; and if we examine the voltaic currents upon the revolving plate, (fig. 7.) we shall perceive that the one, generated by the particle *a*, has a *north* pole issuing from the interior, upwards. Hence this voltaic circle, denoted by R, must tend to repel the magnet, whose north pole is down; whereas the voltaic circle A, generated in the particle *b*, having a *south* pole issuing from its interior, must attract it. As both these circles act obliquely upon N, above them, their tendency will be to move the magnet horizontally, and in the direction of the revolving plate.

2nd. *Repulsion of the magnet perpendicularly from the plate.*—In order to explain this fact, I do not consider it necessary to suppose, with Faraday, that the currents require a certain portion of time for generation; but simply, that they exist for a short period after their creation, which, indeed, seems to follow as a matter of course. Presuming that such is the fact, it is obvious that, in consequence of the motion of the plate being necessarily much greater than that of the magnet, the circle of *repulsion* R, gets under the magnet and forces it directly upwards.

3rd. and 4th. *Motion of the magnet towards the circumference and centre of the plate.*—If, during the revolution of the plate, the magnet be moved along the line C B towards B, the circle of repulsion R, by the motion of the plate, gets more or less between the magnet and the centre C, and hence repels it towards the circumference of the plate:—but, when the magnet is made to approach C, the same circle advances so as to pass between it and the circumference, and thus compels the magnet to incline towards the centre.

All these movements will, it is hoped, be sufficiently intelligible, by a reference to the figure, and the hypothesis is equally applicable, whether we invert the motion of the plate, or make the magnet revolve in opposite directions.

When the magnet is suspended directly over the centre of the revolving plate, (and which is called its concentric position,) it receives no impulse; because the voltaic currents thus generated, lie in planes passing through the magnetic axis of N, and those of the *same* denomination meet at the centre. The counter currents that thus arise upon opposite sides of the magnetic pole, exactly neutralize each other.

Art. IV.—*Motion of a System of Bodies;* by Prof. Theodore Strong.

Continued from Vol. xxv, p. 289.

Again, supposing T, T′, &c. to denote the same things as before, we have $Qx - Py = \left(\frac{x}{r} \times Q - \frac{y}{r} \times P\right) r = Tr$, for $\frac{x}{r} \times Q =$ the force Q when resolved at right angles to r, and $\frac{y}{r} \times P =$ the force P resolved at right angles to r, and $\frac{x}{r} \times Q - \frac{y}{r} \times P = \frac{Qx - Py}{r} =$ the resultant of the forces $\frac{x}{r} \times Q, \frac{y}{r} \times P$, since they act in contrary directions; in the same way $Q'x' - P'y' = T'r'$, and so on; hence we have $\frac{xd^2y - yd^2x}{dt^2} = Tr, \frac{x'd^2y' - y'd^2x'}{dt^2} = T'r'$, &c.; ∴ it may be shown in the very same way as at p. 43, that if we multiply these equations by m, m', &c. respectively, we shall have, (by adding the products,) the equation $Sm\left(\frac{xd^2y - yd^2x}{dt^2}\right) = SmTr$, which is independent of the reciprocal actions of the bodies m, m', &c.; by restoring the values of Tr, $T'r'$, &c. we have the first of (18); and in a similar way may the second and third be found. Let h denote the distance of m from the origin of the coördinates, then if m is acted upon by any force mF in the direction of the straight line h, we shall have F for the force which acts on a unit of m in that direction; ∴ by resolving F in the directions of the axes of x and y, we have $\frac{x}{h} \times F, \frac{y}{h} \times F$ for the parts of P and Q respectively which arise from F, ∴ by considering these forces only, we have $Qx-Py=\left(\frac{xy-yx}{h}\right)F=0$; hence it is evident that (18) are independent of any forces which act upon m, m', &c. in the directions of straight lines drawn to the origin of the coördinates; the same thing is also evident from the expressions Tr, $T'r'$, &c., which require F to be resolved in the direction of a straight line which is at right angles to the extremity of r in the plane x, y; ∴ resolving F into two, one of which is perpendicular to the plane x, y, and the other in the direction of r; the first of these will not affect T, and the second $=\frac{r}{h}\times F$, but as this acts in the direction of r, it will give nothing when resolved in a direction at right angles to r, indeed F will not affect T, since their directions are perpendicular to each other; hence Tr, $T'r'$, &c. are independent of any forces which act on m, m', &c. in the directions of straight lines drawn to the origin of the coördinates; ∴ as before (18) are independent of such forces. Let X, Y, Z denote the coördinates of the centre of gravity of the system, then put $x=X+{}_{,}x$, $y=Y+{}_{,}y$, $z=Z+{}_{,}z$, $x'=X+{}_{,}x'$, &c.; by substituting these values of x, y, z, &c. in (18), (since by the nature of the centre of gravity $Sm{}_{,}x=0$, $Sm{}_{,}y=0$, $Sm{}_{,}z=0$, $Smd^2{}_{,}x=0$, &c. also by (4) $SmP=\frac{d^2X}{dt^2}Sm$, $SmQ=\frac{d^2Y}{dt^2}Sm$, $SmR=\frac{d^2Z}{dt^2}Sm$;) they will be changed to $Sm\left(\frac{{}_{,}xd^2{}_{,}y-{}_{,}yd^2{}_{,}x}{dt^2}\right)=Sm(Q{}_{,}x-P{}_{,}y)$, $Sm\left(\frac{{}_{,}xd^2{}_{,}z-{}_{,}zd^2{}_{,}x}{dt^2}\right)=Sm(R{}_{,}x-P{}_{,}z)$, $Sm\left(\frac{{}_{,}yd^2{}_{,}z-{}_{,}zd^2{}_{,}y}{dt^2}\right)=Sm(R{}_{,}y-Q{}_{,}z)$, (19). Hence since (19) are independent of the coördinates of the centre of gravity, the motion of that centre is found in the same way as it would be if all the bodies of the system were united at the centre, and the motion of the system about the centre is found by (19) in the same way that it would be if the centre was at rest, and the same forces were applied, and in the same manner, as when the centre is in motion; that is, the motion of the system is resolved into two, viz. the motion of the centre of gravity, and the motion of the system about the centre, which are independent of each other. Again, it is evident that (1), (2), (3) will remain the same if the origin of the coördinates has a uniform rectilineal motion in space: ∴ (4), and (18) or (7), which are merely transformations of (1), (2), (3) will exist relative to the moveable origin; supposing the axes of x, y, z to be reckoned from the moveable origin, and each to move parallel to itself. If the system is not subjected to the actions of any foreign forces, (11) will exist relative to the moveable origin, whether the changes of the motions of the bodies are finite in an instant or not, and A, $_{,}$A, $_{,,}$A, will each be invariable during the motion of the system; ∴ (16) will each be invariable, hence the invariable plane always passes through the moveable origin and moves parallel to itself. It has been proved when the system is not subjected to the actions of any foreign forces, that the centre of gravity is either at rest or moves uniformly forward in a straight line; ∴ by fixing the origin at the centre of gravity, the invariable plane is either at rest or moves parallel to itself. Let the origin be at the centre of gravity, and suppose that the system is not subjected to the actions of any foreign forces, then by (11) $\text{S}m\left(\frac{xdy-ydx}{dt}\right)=\text{A}$, $\text{S}m\left(\frac{xdz-zdx}{dt}\right)=$ $_{,}\text{A}$, $\text{S}m\left(\frac{ydz-zdy}{dt}\right)=_{,,}\text{A}$, (20). Now we have $m'((x'-x)dy'-(y'-y)dx')=m'(x'dy'-y'dx')+m'(ydx'-xdy'), m''((x''-x)dy''-(y''-y)dx'')=m''(x''dy''-y''dx'')+m''(ydx''-xdy'')$, and so on for all the bodies except m: hence $\text{S}m'((x'-x)dy'-(y'-y)dx')=\text{S}m'(x'dy'-y'dx')+\text{S}m'(ydx'-xdy')$, but $\text{S}m'ydx'=y\text{S}m'dx'$, $\text{S}m'xdy'=x\text{S}mdy'$, and by the nature of the centre of gravity $\text{S}m'dx'=-mdx$, $\text{S}m'dy'=-mdy$; hence $m\text{S}m'\left(\frac{(x'-x)dy'-(y'-y)dx'}{dt}\right)$ $=m\text{A}$, in the same way $m'\text{S}m\left(\frac{(x-x')dy-(y-y')dx}{dt}\right)=m'\text{A}$, and so on for all the bodies; ∴ $m\text{S}m'\left(\frac{(x'-x)dy'-(y'-y)dx'}{dt}\right)+$ $m'\text{S}m\left(\frac{(x-x')dy-(y-y')dx}{dt}\right)+\&c.=m\text{A}+m'\text{A}+\&c.$ or

$$\text{S}mm'\left(\frac{(x'-x).(dy'-dy)-(y'-y).(dx'-dx)}{dt}\right)=\text{AS}m, \quad (21),$$

in the same way

$$\text{S}mm'\left(\frac{(x'-x).(dz'-dz)-(z'-z).(dx'-dx)}{dt}\right)={}_{,}\text{AS}m,$$

$$\text{S}mm'\left(\frac{(y'-y).(dz'-dz)-(z'-z).(dy'-dy)}{dt}\right)={}_{,,}\text{AS}m, \quad (22).$$

Let the plane x, y be the invariable plane, then A is a maximum, and $_{,}$A, $_{,,}$A are each $=0$; ∴ the first member of (21) is a maximum, and those of (22) are each $=0$. Let a plane be drawn through any body of the system parallel to the plane x, y, then the first member of (21) is a maximum relative to the parallel plane; for its value is the same for the parallel plane as for that of x, y, as is evident by supposing the system to be reduced orthographically to the planes. Now since the plane x, y is either at rest or moves parallel to itself, its parallel plane will always be parallel to itself as the system moves: it is also manifest that the first members of (22) will each $=0$, relative to any plane drawn through the same body (as before,) perpendicular to the parallel plane; Mec. Cel. Vol. I, p. 63.

Again, let, (for brevity,) any body of the system be indefinitely denoted by m, x, y, z, being its rectangular coördinates; also suppose (as in (d), (e), &c.) that x', y', z', are the rectangular coördinates of m, when referred to any other system of rectangular axes, which have the same origin as the axes of x, y, z; then denoting by a, b, &c. the same things as in (d,) (e,) &c., we have by (d), $x=ax'+by'+cz'$, $y=a'x'+b'y'+c'z'$, $z=a''x'+b''y'+c''z'$, (a'): supposing the quantities in (a') to be functions of the time, we have $\frac{dx}{dt}=\frac{x'da+y'db+z'dc}{dt}+\frac{adx'+bdy'+cdz'}{dt}$, $\frac{dy}{dt}=\frac{x'da'+y'db'+z'dc'}{dt}+\frac{a'dx'+b'dy'+c'dz'}{dt}$, $\frac{dz}{dt}=\frac{x'da''+y'db''+z'dc''}{dt}+\frac{a''dx'+b''dy'+c''dz'}{dt}$, ($b'$). Put $cdb+c'db'+c''db''=pdt$, $adc+a'dc'+a''dc''=qdt$, $bda+b'da'+b''da''=rdt$; then by (f) $cdb+c'db'+c''db''=-bdc-b'dc'-b''dc''=pdt$, $adc+a'dc'+a''dc''=-cda-c'da'-c''da''=qdt$, $bda+b'da'+b''da''=-adb-a'db'-a''db''=rdt$, ($d'$); by substituting the values of a, b, &c. from (o) in (d'), we have $\sin.\varphi \sin.\theta d\psi-\cos.\varphi d\theta=pdt$, $\cos.\varphi \sin.\theta d\psi+\sin.\varphi d\theta=qdt$, $d\varphi-\cos.\theta d\psi=rdt$, ($e'$). Put $qz'-ry'+\frac{dx'}{dt}=\text{L}$, $rx'-pz'+\frac{dy'}{dt}=\text{M}$, $py'-qx'+\frac{dz'}{dt}=\text{N}$, ($f'$); then multiply ($b'$) by a, a', a'', respectively, add the products, and we have by (f), (d'), (f'), $\frac{adx+a'dy+a''dz}{dt}=\text{L}$, in like manner $\frac{bdx+b'dy+b''dz}{dt}=\text{M}$, $\frac{cdx+c'dy+c''dz}{dt}=\text{N}$, ($g'$); multiply ($g'$) by a, b, c severally, add the products, and we have $\frac{dx}{dt}=a\text{L}+b\text{M}+c\text{N}$, and in like manner $\frac{dy}{dt}=a'\text{L}+b'\text{M}+c'\text{N}$, $\frac{dz}{dt}=a''\text{L}+b''\text{M}+c''\text{N}$, ($h'$). By adding the squares of (h'), we have by (f) $\frac{dx^2+dy^2+dz^2}{dt^2}=\text{L}^2+\text{M}^2+\text{N}^2$; hence by ($f'$), $\frac{dx^2+dy^2+dz^2}{dt^2}=(y'^2+z'^2)p^2+(x'^2$

$+z'^2)q^2+(x'^2+y'^2)r^2-2y'z'.qr-2x'z'.pr-2x'y'.pq+2\left(\frac{y'dz'-z'dy'}{dt}\right)$
$p+2\left(\frac{z'dx'-x'dz'}{dt}\right)q+2\left(\frac{x'dy'-y'dx'}{dt}\right)r+\frac{dx'^2+dy'^2+dz^2}{dt^2}$, (i').

Now (18) are easily changed to $d.Sm\left(\frac{xdy-ydx}{dt}\right)=dt.\ Sm(Qx-Py)$, $d.Sm\left(\frac{zdx-xdz}{dt}\right)=dt.\ Sm(Pz-Rx)$, $d.Sm\left(\frac{ydz-zdy}{dt}\right)=dt.\ Sm(Ry-Qz)$, (23); put $S(y'^2+z'^2)m=A$, $S(x'^2+z'^2)m=B$, $S(x'^2+y'^2)m=C$, $Sy'z'm=D$, $Sx'z'm=E$, $Sx'y'm=F$, (k'), also put $Ap-Er-Fq=p'$, $Bq-Dr-Fp=q'$, $Cr-Dq-Ep=r'$, $Sm\left(\frac{x'dy'-y'dx'}{dt}\right)={}_{,}A$, $Sm\left(\frac{z'dx'-x'dz'}{dt}\right)={}_{,}B$, $Sm\left(\frac{y'dz'-z'dy'}{dt}\right)={}_{,}C$, (l'). By substituting the values of x and y from (a') and those of $\frac{dx}{dt}$, $\frac{dy}{dt}$, from (h'), in the first of (23), we shall have (since a, b, &c., are the same for all the bodies;) $d.\ [(bc'-cb').Sm(Ny'-Mz')+(a'c-ac').Sm(Lz'-Nx')+(ab'-ba').Sm(Mx'-Ly')]=dt.\ Sm(Qx-Py,)$ (24).

Put $dt.Sm(Qx-Py)=dN'''$, $dt.Sm(Pz-Rx)=dN''$, $dt.Sm(Ry-Qz)=dN'$, (m'); by substituting the values of L, M, N, from (f'), we have $Sm(Ny'-Mz')=p'+{}_{,}C$, $Sm(Lz'-Nx')=q'+{}_{,}B$, $Sm(Mx'-Ly')=r'+{}_{,}A$, (n'), by substituting these values, and those of $bc'-cb'$, &c., from (g), and using dN''', (24) becomes $d.[a''(p'+{}_{,}C)+b''(q'+{}_{,}B)+c''(r'+{}_{,}A)]=dN'''$, in like manner the second and third of (23) will give, $d.[a'(p'+{}_{,}C)+b'(q'+{}_{,}B)+c'(r'+{}_{,}A)]=dN''$, $d.[a(p'+{}_{,}C)+b(q'+{}_{,}B)+c(r'+{}_{,}A)]=dN'$, (25); the two last of these are easily derived from the first by making some very obvious changes in (24).

By taking the differentials indicated in (25), then multiplying the resulting equations by a'', a', a respectively, and adding the products, we shall have by (f) and (d'), after dividing by dt, $\frac{d.(p'+{}_{,}C)}{dt}+q(r'+{}_{,}A)-r(q'+{}_{,}B)=\frac{a''dN'''+a'dN''+adN'}{dt}$, and in a similar way
$$\frac{d.(q'+{}_{,}B)}{dt}+r(p'+{}_{,}C)-p(r'+{}_{,}A)=\frac{b''dN'''+b'dN''+bdN'}{dt},$$
$$\frac{d.(r'+{}_{,}A)}{dt}+p(q'+{}_{,}B)-q(p'+{}_{,}C)=\frac{c''dN'''+c'dN''+cdN'}{dt},\ (26).$$

Put the right hand members of (26) equal to $N_{,}$, $N_{,,}$, $N_{,,,}$ respectively and we have by (m'), $N_{,}=Sm((a'z-a''y)P+(a''x-az)Q+(ay-a'x)R,)$ $(0')$; by (a'), and (g), $a'z-a''y=cy'-bz'$, $a''x-az=c'y'-b'z'$, $ay-a'x=c''y'-b''z'$; $\therefore$ put $cP+c'Q+c''R=R'$, $bP+b'Q+b''R=Q'$ also, $aP+a'Q+a''R=P'$, (p'), and we have $N_{,}=Sm(R'y'-Q'z')$, in the same way (by making some very obvious changes in $(0')$,) we have $N_{,,}=Sm(P'z'-R'x')$, $N_{,,,}=Sm(Q'x'-P'y')$, (q'),.

Multiply (i') by m, take the finite integrals relative to all the bodies of the system, put $Sm\left(\frac{dx^2+dy^2+dz^2}{dt^2}\right)=2T=$ the living force of the system, and we have by (l'), (since p, q, r, are the same throughout the system;) $T=\left(\frac{Ap^2+Bq^2+Cr^2}{2}\right)-Dqr-Epr-Fpq+{}_{,}Cp+{}_{,}Bq+{}_{,}Ar+Sm\left(\frac{dx'^2+dy'^2+dz'^2}{dt^2}\right)$, (r'); by taking the partial differential co-efficients of (r') relative to p, q, r, we have by (l'), $\left(\frac{dT}{dp}\right)=p'+{}_{,}C$, $\left(\frac{dT}{dq}\right)=q'+{}_{,}B$, $\left(\frac{dT}{dr}\right)=r'+{}_{,}A$, (s'); by substituting from (s') and (q') in (26), they will be changed to $\frac{d\left(\frac{dT}{dp}\right)}{dt}+q\left(\frac{dT}{dr}\right)-r\left(\frac{dT}{dq}\right)=Sm(R'y'-Q'z')$, $\frac{d\left(\frac{dT}{dq}\right)}{dt}+r\left(\frac{dT}{dp}\right)-p\left(\frac{dT}{dr}\right)=Sm(P'z'-R'x')$, $\frac{d\left(\frac{dT}{dr}\right)}{dt}+p\left(\frac{dT}{dq}\right)-q\left(\frac{dT}{dp,}\right)=Sm(Q'x'-P'y')$, (27): also substituting from (s') in (25), they will be changed to $d\left(a''\left(\frac{dT}{dp}\right)+b''\left(\frac{dT}{dq}\right)+c''\left(\frac{dT}{dr}\right)\right)=dN'''$, $d\left(a'\left(\frac{dT}{dp}\right)+b'\left(\frac{dT}{dq}\right)+c'\left(\frac{dT}{dr}\right)\right)=dN''$ $d\left(a\left(\frac{dT}{dp}\right)+b\left(\frac{dT}{dq}\right)+c\left(\frac{dT}{dr}\right)\right)=dN'$, (28).

By (e), $x'=ax+a'y+a''z$, $y'=bx+b'y+b''z$, $z'=cx+c'y+c''z$, (t'); $\therefore$ supposing a, b, c, &c. to be momentarily constant, we have $\frac{dx'}{dt}=\frac{adx+a'dy+a''dz}{dt}$, $\frac{dy'}{dt}=\frac{bdx+b'dy+b''dz}{dt}$, $\frac{dz'}{dt}=\frac{cdx+c'dy+c''dz}{dt}$ (u'); hence, and by (g'), $L=\frac{dx'}{dt}$, $M=\frac{dy'}{dt}$, $N=\frac{dz'}{dt}$, (v'); substituting from (v') in (n') we have $Sm\left(\frac{y'dz'-z'dy'}{dt}\right)=p'+{}_{,}C=$ (by (s'),) $\left(\frac{dT}{dp}\right)$, $Sm\left(\frac{z'dx'-x'dz'}{dt}\right)=q'+B_{,}=\left(\frac{dT}{dq}\right)$, $Sm\left(\frac{x'dy'-y'dx'}{dt}\right)=r'$ $+{}_{,}A=\left(\frac{dT}{dr}\right)$, (w'), see Mec. Anal. Vol. 2. p. 364, &c. If the system is not affected by any forces except the mutual actions of the bodies which compose it, (whether the bodies are acted on by forces in the directions of straight lines drawn to the origin of the coördinates or not,) then, by what has been before shown, the second members of (23) will each$=0$, $\therefore$ by taking the integrals of their first members we have $\frac{xdy-ydx}{dt}=A'$, $\frac{zdx-xdz}{dt}=B'$, $\frac{ydz-zdy}{dt}=C'$ (29), A' B' C' being the arbitrary constants; but it is evident from the method of obtaining (25), that $\frac{xdy-ydx}{dt}=a''(p'+{}_{,}C)+b''(q'+{}_{,}B)+$ $c''(r'+{}_{,}A)=$ (by (w')) $a''\left(\frac{dT}{dp}\right)+b''\left(\frac{dT}{dq}\right)+c''\left(\frac{dT}{dr}\right)$, $\therefore a''\left(\frac{dT}{dp}\right)+$ $b''\left(\frac{dT}{dq}\right)+c''\left(\frac{dT}{dr}\right)=A'$, in the same manner $a'\left(\frac{dT}{dp}\right)+b'\left(\frac{dT}{dq}\right)+c'$ $\left(\frac{dT}{dr}\right)=B'$, $a\left(\frac{dT}{dp}\right)+b\left(\frac{dT}{dq}\right)+c\left(\frac{dT}{dr}\right)=C'$, (30); by adding the squares of (30), we have by (f), $\left(\frac{dT}{dp}\right)^2+\left(\frac{dT}{dq}\right)^2+\left(\frac{dT}{dr}\right)^2=A'^2+B'^2+$ $C'^2=$ const. (31).

If we suppose the forces to be as before, and besides that a, b, c, &c. are invariable, then we shall have p', q', r', each$=0$; $\therefore Sm$ $\left(\frac{x'dy'-y'dx'}{dt}\right)={}_{,}A$, $Sm\left(\frac{z'dx'-x'dz'}{dt}\right)={}_{,}B$, $Sm\left(\frac{y'dz'-z'dy'}{dt}\right)=$ ${}_{,}C$, and ${}_{,}A$, ${}_{,}B$, ${}_{,}C$, will each be constant; and we have $a''{}_{,}C+b''{}_{,}B$ $+c''{}_{,}A=A'$, $a'{}_{,}C+b'{}_{,}B+c'{}_{,}A=B'$, $a{}_{,}C+b{}_{,}B+c{}_{,}A=C'$, (32), hence ${}_{,}A^2+{}_{,}B^2+{}_{,}C^2=A'^2+B'^2+C'^2$, (33); multiply (32) by a'', a', a, respectively, add the products, and we have ${}_{,}C=a''A'+a'B'+aC'$, in like manner ${}_{,}B=b''A'+b'B'+bC'$, ${}_{,}A=c''A'+c'B'+cC'$ (34).

Now since the position of the plane x', y' is arbitrary, let it be so assumed that $c''=\frac{A'}{\sqrt{A'^2+B'^2+C'^2}}$, $c'=\frac{B'}{\sqrt{B'^2+B'^2+C'^2}}$, $c=$ $\frac{C'}{\sqrt{A'^2+B'^2+C'^2}}$, (35); hence $A'^2+B'^2+C'^2={}_{,}A^2$, $\therefore$ by (33),

$_{,}$B, $_{,}$C are each$=0$: by substituting the values of c'', c' c from (o) in (35), they become $\cos.\ \theta = \frac{A'}{\sqrt{A'^2+B'^2+C'^2}}$, $\sin.\ \theta \cos.\ \psi = \frac{B'}{\sqrt{A'^2+B'^2+C'^2}}$, $\sin.\ \theta \sin.\ \psi = \frac{C'}{\sqrt{A'^2+B'^2+C'^2}}$, (36). (35) agree with the formulæ given at p. 269, Vol. 1. Mec. Anal. for the determination of the invariable plane, and (36) are given for the same purpose, at p. 60, Vol. 1. Mec. Cel. and it is evident that they agree with (16).

Again, suppose the system to be rigid, or that the bodies which compose it are invariably connected with each other; also that x', y', z', are invariably connected with the system, so that they do not vary with the time, and change their values only in passing from one body of the system to another.

In this case $\frac{dx'}{dt}=0$, $\frac{dy'}{dt}=0$, $\frac{dz'}{dt}=0$, $\therefore {}_{,}A=0$, $_{,}B=0$, $_{,}C=0$, and A, B, &c. are each constant, hence (26) will be changed to $\frac{dp'}{dt}+qr'-rq' = \frac{a''dN'''+a'dN''+adN'}{dt}$, $\frac{dq'}{dt}+rp'-pr' = \frac{b''dN'''+b'dN''+bdN'}{dt}$, $\frac{dr'}{dt}+pq'-qp' = \frac{c''dN'''+c'dN''+cdN'}{dt}$, (38).

It is evident by (p), that the axes of x', y', z' can be found so as to satisfy the equations $D=Sy'z'm=0$, $E=Sx'z'm=0$, $F=Sx'y'm=0$; then will the axes of x', y', z', be principal axes. Hence put $D=0$, $E=0$, $F=0$; then by (l'), $p'=Ap$, $q'=Bq$, $r'=Cr$; hence (38), become $\frac{Adp}{dt}+(C-B)qr = \frac{a''dN'''+a'dN''+adN'}{dt}$, $\frac{Bdq}{dt}+(A-C)pr = \frac{b''dN'''+b'dN''+bdN'}{dt}$, $\frac{Cdr}{dt}+(B-A)pq = \frac{c''dN'''+c'dN''+cdN'}{dt}$, (39).

Since the position of the axis of x in the plane x, y, is arbitrary, we willl now suppose that it makes an infinitely small angle with the line of intersection of the planes x, y, and x', y', hence neglecting infinitely small quantities of the second, &c. orders, we have $\sin.\ \psi = \psi$, $\cos.\ \psi = 1 \therefore$ substituting these values of $\sin.\ \psi$, $\cos.\ \psi$, in (0), we have by neglecting quantities of the order ψ, $a''=-\sin.\ \theta \sin.\ \varphi$, $a'=\cos.\ \theta \sin.\ \varphi$, $a=\cos.\ \varphi$, $b''=-\sin.\ \theta \cos.\ \varphi$, $b'=\cos.\ \theta \cos.\ \varphi$, $b=-\sin.\ \varphi$, $c''=\cos.\ \theta$, $c'=\sin.\ \theta$, $c=0$, (x').

Multiply (39) by dt, substitute the values of a'', a', &c. from (x'), and we have $\mathrm{A}dp+(\mathrm{C}-\mathrm{B})qr\,dt=-(\sin.\theta\, d\mathrm{N}'''-\cos.\theta\, d\mathrm{N}'')\sin.\varphi+\cos.\varphi\ d\mathrm{N}'$, $\mathrm{B}dq+(\mathrm{A}-\mathrm{C})\,prdt=-(\sin.\theta\, d\mathrm{N}'''-\cos.\theta\, d\mathrm{N}'')\cos.\varphi-\sin.\varphi\ d\mathrm{N}'$, $\mathrm{C}dr+(\mathrm{B}-\mathrm{A})\ pqdt=\cos.\theta\, d\mathrm{N}'''+\sin.\theta\ d\mathrm{N}''$, (40); which agree with (D) given at p. 74, Vol. I. of the Mecanique Celeste, for the motion of a solid about a fixed point; as they evidently ought to do: for the above formulæ are equally applicable, whether we consider the motion of a rigid system, or solid, about a fixed point.

Again, x', y', z', being principal axes, (r') will be changed to $\mathrm{T}=\frac{\mathrm{A}p^2+\mathrm{B}q^2+\mathrm{C}r^2}{2}$, (y'); hence $\left(\frac{d\mathrm{T}}{dp}\right)=\mathrm{A}p, \left(\frac{d\mathrm{T}}{dq}\right)=\mathrm{B}q, \left(\frac{d\mathrm{T}}{dr}\right)=\mathrm{C}r$; ∴ in the case of (30) and (31), (that is when the system is not affected by any disturbing forces,) we shall have $a''\mathrm{A}p+b''\mathrm{B}q+c''\mathrm{C}r=\mathrm{A}'$, $a'\mathrm{A}p+b'\mathrm{B}q+c'\mathrm{C}r=\mathrm{B}'$, $a\mathrm{A}p+b\mathrm{B}q+c\mathrm{C}r=\mathrm{C}'$, (41), $\mathrm{A}^2p^2+\mathrm{B}^2q^2+\mathrm{C}^2r^2=\mathrm{A}'^2+\mathrm{B}'^2+\mathrm{C}'^2$, (42); also (39) become $\frac{\mathrm{A}dp}{dt}+(\mathrm{C}-\mathrm{B})qr=0$, $\frac{\mathrm{B}dq}{dt}+(\mathrm{A}-\mathrm{C})pr=0$, $\frac{\mathrm{C}dr}{dt}+(\mathrm{B}-\mathrm{A})pq=0$, (43); multiply (43) by p, q, r, severally, add the products, take the integral, and we have $\mathrm{A}p^2+\mathrm{B}q^2+\mathrm{C}r^2=\mathrm{D}'=$const. (44), which agrees with the well known principle of the preservation of living forces, multiply (43), by $\mathrm{A}p$, $\mathrm{B}q$, $\mathrm{C}r$, then proceed as before, and we have $\mathrm{A}^2p^2+\mathrm{B}^2q^2+\mathrm{C}^2r^2=\mathrm{E}'^2=$const. (45); this compared with (42) gives $\mathrm{A}'^2+\mathrm{B}'^2+\mathrm{C}'^2=\mathrm{E}'^2$, (z').

It is evident that the system may be considered as having a momentary axis of rotation: to find the momentary axis we observe that relative to it, we shall have $\frac{dx}{dt}=0$, $\frac{dy}{dt}=0$, $\frac{dz}{dt}=0$, ∴ by (g'), $\mathrm{L}=0, \mathrm{M}=0, \mathrm{N}=0$, or by (f'), (and because $\frac{dx'}{dt}=0$, $\frac{dy'}{dt}=0$, $\frac{dz'}{dt}=0$,) we have $qz'-ry'=0$, $rx'-pz'=0$, $py'-qx'=0$, (46); which indicate that the momentary axis is a right line which passes through the origin of the coördinates. Let $a_{,,}$ $b_{,,}$ $c_{,}$ respectively, denote the cosines of the angles which the momentary axis makes with the axes of x', y', z' respectively, then by (46), $\frac{p}{\sqrt{p^2+q^2+r^2}}=\frac{x'}{\sqrt{x'^2+y'^2+z'^2}}=a_{,,}$ $\frac{q}{\sqrt{p^2+q^2+r^2}}=\frac{y'}{\sqrt{x'^2+y'^2+z'^2}}=b_{,,}$ $\frac{r}{\sqrt{p^2+q^2+r^2}}=\frac{z'}{\sqrt{x'^2+y'^2+z'^2}}=c_{,,}$ (47).

By (i'), (since $\frac{dx'}{dt}$, &c. are each $=0$,) if we suppose $x'=0$, $y'=0$, we shall have $z'\sqrt{p^2+q^2}=\frac{\sqrt{dx^2+dy^2+dz^2}}{dt}=$ the velocity of a point which is on the axis of z', at the distance z' from the origin, also by (47), $\frac{\sqrt{p^2+q^2}}{\sqrt{p^2+q^2+r^2}}=\sqrt{1-c_{,}^2}=$ the sine of the angle made by the axis of z' with the momentary axis, $\therefore \frac{z'\sqrt{p^2+q^2}}{\sqrt{p^2+q^2+r^2}}=$ the perpendicular from the extremity of z' to the instantaneous axis; put $w=$ the angular velocity around the instantaneous axis, and we have $z'\sqrt{p^2+q^2} \div \frac{z'\sqrt{p^2+q^2}}{\sqrt{p^2+q^2+r^2}}=w$, or $w=\sqrt{p^2+q^2+r^2}$, (48).

By (47) and (48), $p=a_{,}w$, $q=b_{,}w$, $r=c_{,}w$, (49), where p, q, r are evidently the momentary rotations around the axes of x', y', z' respectively; hence it is evident that rotary velocities are compounded and resolved by the same rules as rectilineal velocities.

Remarks.—It is evident that if the origin of the coördinates is at the centre of gravity of the system, all the formulæ which we have found will apply, whether the centre is at rest or in motion; for (19) which have the same forms as (18), are applicable whether the centre is at rest or in motion; hence by proceeding with (19), as we have done with (18), we shall obtain the same results as before; $\therefore$ by placing the origin at the center of gravity, all the above formulæ will apply when the system is free; and the motion of the centre will be found by (4).

Again, it is supposed in (k') that the bodies are so small, that x', y', z' may be considered as having the same values for all the points of each, but should not this be the case, we must change m into dm, then find the value of A for each body, by taking the integral relative to its mass; then the sum of all the values thus found, will be the complete value of A; and in the same way we must find the complete values of B, &c.; but should the system be a continuous solid, we must find the values of A, B, &c. by integrating relative to its mass.

Art. V.—*On the Navigation of Cape Horn;* by M. F. Maury, Passed Midshipman, U. S. Navy.

A variety of causes combine to render the navigation, from the Atlantic around Cape Horn to the Pacific, dangerous.

From the time Sir Francis Drake was driven off Cape Horn, till the present day, the boldest navigators have approached it with caution. They never venture in the latitude of it, until each has prepared his vessel for the rough weather to be expected in rounding it; for this, no precaution is omitted. Men of war strike part of their armament into the hold; get their anchors between decks; send up stump masts; bend the storm sails; and secure their spars with preventer rigging, as they get near the tempestuous regions. In the roughness of the passage, the crew is liable to much exposure.

There the tempest, the sea, and the iceberg assume their most terrible character, each presenting dangers almost new in their kind and peculiar to the region.

The ice, from its beds of a thousand years, is detached in islands like masses by the gale and the shock of the sea; it is swept to the north by the winds and currents, and carries in its silent course, all the dangers of the hidden rock, until it gradually melts away under the influence of more genial climates.

The gales, frequently accompanied with hail and sleet, are proverbial among seamen for their unremitting severity, and the length of their duration. Occurrences of vessels "lying to" in gales of wind, for many days, off Cape Horn, are frequent. I have seen them arrive in Valparaiso and Callao, after having been detained eighty and even one hundred and twenty days in gales and head winds off the Cape. The case of a ship's "lying to" there, in one continued gale, for seventy days, is of recent occurrence. It is not unfrequent that vessels even of war, put into the ports of Chili crippled in the rough weather at the South. The most robust constitutions overcome by long exposure to it, succumb to its severity; they may bear up against it for many days, but the hardiest crew, exhausted at last by incessant toil, are forced in despair to give up the ship, clogged with ice and snow, to the mercies of the contending climates.

The waves run to a height, which, in other seas, they seldom attain. In the calm they cause no less damage than in the gale, by distressing the ship with labor. In that succeeding a storm, vessels sometimes roll their masts away.

To determine upon the best route for doubling Cape Horn, has been a desideratum of the first importance to South Sea navigators. Many opinions have been advanced on the subject, but down to the present time, no route has been proposed, nor directions given, which have received general approbation, or have met with the concurrence of those, whose experience in Cape Horn navigation, gives value to their opinions.

The routes, which have been most recommended, and which have been followed with most success, have resolved themselves into two—the "inshore" and the "southern." The former is peferable and more expeditious, when the winds are favorable for sailing westwardly. The latter should be taken, when gales from the westward are encountered, while doubling the Cape. By standing to the southward in such cases, the track of the violent winds, that come sweeping around the extremity of the land, from the west and northwest will be crossed; sometimes it does not reach further to the south than 57° 30′ lat., it seldom extends beyond 63° south lat.

The absence of regular periodical winds in the vicinity of the Cape, contributes to the embarrassment of opinion with regard to the most expeditious route for doubling it.

No general directions can be given, which will invariably point out the best course for a vessel to steer, while passing the boisterous region. This is prevented by the uncertainty of the winds, in regard both to their strength and the direction in which they may blow. But under the guidance of certain circumstances to be pointed out, the navigator may be greatly assisted in conducting his vessel in safety through the tempestuous sea connecting the Pacific with the Atlantic.

From peculiar circumstances connected with the western gales that blow around the Cape, there is reason to believe, that they do not extend far beyond it, with equal violence, and that they are strongest in its vicinity. It is a phenomenon occurring not unfrequently under the observations of sailors, that the same gale does not always blow over extensive tracts of the ocean. Ships, a few leagues apart, are sailing sometimes at the same moment, with winds of unequal strength and even from different directions; of this a case which occurred in 1829 can be instanced; one vessel was dismasted in a gale, when another only a few leagues from her, was sailing in fine weather with a moderate breeze from a different direction. This gale continued for several days nearly within the same limits.

Winds from every point of the compass are met with off Cape Horn. They blow with great violence from every quarter. The secondary causes which govern them seem to follow no laws, save those concealed in their own mysterious effects. The fact that winds with *westing*, are more prevalent than those with *easting*, in them, is established from the circumstance, that the return is less dreaded and shorter, than the outward bound passage. The ratio of winds with westing in them to those with easting is as *three* to *one*.

During the month of our vernal equinox they appear to assume something of the character of periodicals, prevailing from the eastward; hence March is considered the most favorable season for passing from the Atlantic around Cape Horn, into the Pacific. In November they are more prevalent from the opposite direction. This is the most favorable month for returning from the Pacific.

I have before me extracts from the logbooks of a number of vessels, that have doubled Cape Horn at different seasons of the year. Of those which have passed the Cape in March, all have had fine weather with eastwardly winds. One of them, in March, performed the passage from Bordeaux, around the Cape, to Callao, without having reefed a sail.

The recent observations of sealers, engaged in taking skins, for several years, on the South Shetland Islands, go to establish the fact, that the winds there and along the icy continent to the southward, blow from the eastward two thirds of the year, the reverse of what has long been known to be the case in the vicinity of Cape Horn.

I am informed by some masters of vessels who have been in the habit of coming to the Pacific by the southern route that by going as far south as 63°, they have not only a smoother sea, but a climate less boisterous and rigid. The fact of this comparative mildness of climate is not attested sufficiently to be admitted as a truth. It is near the region of perpetual ice. The eastwardly winds that prevail near the South Shetlands and along the icy continent, are eddies to the gales from the westward, sweeping over regions a little to the north. They are confined to certain parallels by the same peculiarity of causes, by which they are put in motion.

The icebergs common in the lat. 63° are serious objections to some, why the southern rout should never be attempted, but the probability of falling in with them, is less to be dreaded, than are the injuries and delays incidental to the westerly gales, by attempting to ride them out in the vicinity of the Cape, where they are always most violent. The range of these gales, is frequently passed, by standing two or three degrees to the southward of St. John's.

The early navigators followed the "inshore" passage. Those who came after them, in more modern times, steered more to the south, and were sometimes favored with fair winds and speedy passages. Those who were fortunate, approved of the plan, and in the pride of success, they recommended others to pursue the same route, arguing that although the distance was greater, yet the passage was shortened, by having favorable breezes and a smooth sea. In the present day, there are those who sail by both routes, and make short passages, showing that the preference should sometimes be given to the one, and at other times, and under other circumstances, to the other.

Those who go the "inshore" passage, keep close in with the land. When the wind is fair they go to the north of Diego Ramirez; never to the south of it, further than tén or twelve leagues, if they can avoid it. Supposing this cleared, they continue on due west, upon the same parallel, as far as 85° of longitude; thence upon that meridian due north, to lat. 40° S. whence they shape their course directly for the port of destination. When the wind is favorable, they pass through the straits of Le Maire; but this should be done only when they are likely to be embayed, or when they are swept under the land so that they cannot pass to the east of Staten Land, without loss of time, and probably of a fair breeze.

A vessel may enter the straits, with a favorable breeze, and under every appearance of good weather, and in coming through, be met by a gale from the south east, which would place her on a lee shore, and in a very critical situation. The possibility of taking this gale, is a good reason why vessels should go around St. Johns, in preference to passing through the straits of Le Maire, when they are free to choose either.

If a gale from the westward, be encountered off Staten Land, they seek refuge from its violence, under the lee of the island, and "heave" or "lay to" in smooth water, until the gale abates. If they be further to the westward, before they meet it, they "lay to" on either tack, preserving the latitude in which they may be at the time of taking it, as near as practicable. After the gale has passed over, they stand again to the westward. On nearing the Cape the second time, they run the same risk of meeting an adverse gale, that they did when it was first approached. Frequently they do not clear the Cape, until the third or fourth attempt after having been set to the eastward by gales from the westward.

During the north west gales, vessels have been driven several hundred miles to the south east. In 1819—20, an English brig was set in a north wester, from the vicinity of Hermit's Island, down to the south Shetland's, which had been discovered by a Dutchman, about two hundred years previously: during this lapse of time, their existence had never been confirmed to the world, by a concurrent report from other navigators, and the reported discovery of the Dutchman, had sunk into disbelief, and finally into oblivion. The brig, after a tedious passage, arrived at Valparaiso, and her master, (one Smith,) reported the discovery he had made to Capt. Sherif, R. N. who was in the bay of Valparaiso, in command of one of his Majesty's men of war. Capt. Sherif chartered the brig, sent officers on board, and despatched her, to ascertain the reality of the reported discovery, and the position of the Islands. They were found without any difficulty, and after sailing among them for a day or two, the brig put into a harbor, where were several American vessels, lying quietly at anchor, some of which had been in the habit for five years, of visiting that place.

When the westerly gales, become so violent as to strip the canvass from the yards, the ship is liable to much injury, if they blow for many days, which they frequently do. By persisting in the attempt to weather out the storm, and to secure the "inshore" passage, vessels have been reduced almost to the last extremity before they succeeded. In waiting to catch a favorable moment for passing the land, some are even less fortunate. After riding out gale after gale, and being driven from the land as often as they made it, they are at last, forced in distress to put back into some port on the Atlantic side. They are seen coming into Rio Janeiro or the La Plata, their hulls so completely shattered, that they scarcely keep afloat, and the crew unable to manage them, being exhausted by long exposure to the freezing winds. The delay necessarily incurred by refitting, and from the difficulty of shipping another crew, amounts to several months. Probability favors the supposition, that these misfortunes would have been avoided by lying to, on the starboard tack, and forging to the southward, out of the strength of the gale, with the expectation of catching an easterly wind in the icy regions.

Those who follow the "southern" route, take their departure from St John's, (Staten Land,) and steer to the southward to lat. 63°, where they expect to find the wind from the eastward, which will carry then as far as 85° or 87° of W. lon. They make this longitude, before they cross the parallel of 61°, whence, as those who go the other route, they steer directly north to lat. 40°.

Independently of personal observation, other means of acquiring information, relative to the navigation around Cape Horn, have been resorted to. Besides access to numerous log books and notes, information has been obtained on the subject, from masters of vessels, who have been sailing to and from the west coast of South America, for many years. The opinions of some, derived from an attentive observation, and strengthened by the experience of twenty voyages, have the highest claims to respect. The advice of these men urges the propriety of yielding to circumstances in doubling Cape Horn, and of being guided by the winds, in giving preference to either the "inshore" or the "southern" passage.

The former is to be pursued always, when the winds are favorable, keeping close into the land, never passing to the southward of Diego Ramirez, more than ten or twelve leagues. It is better to go to the northward of it, when it can be done without loss of time. The "inshore" passage being nearer in point of distance, when the winds are ahead, if the sea be smooth enough to allow a vessel to beat to windward without losing by leeway; but when this can no longer be done in a breeze that is freshening, the route should immediately be abandoned by standing to the south, until the wind shall be found to be more favorable for getting to the west, which frequently happens by running a few leagues to the southward of Diego Ramirez. The westerly winds, for the most part, come in a sweep around the land, without stretching many degrees in breadth, towards the south.

The longitude of 85° should be gained, without going to the northward of the parallel on which the land is cleared. The latitude of 40° S. as in the other route, is made on the meridian of 85°. This is always done to clear the gales and currents, which blow and set on shore in the vicinity of the Island of Mocha, and the outlet to the straits of Magellan.

Vessels bound from the U. States around Cape Horn, are recommended to cross the line, between lon. 23° and 26°, so that with the south east trades, they can fetch Cape Frio, which should always be done; then to run the coast down on soundings and to pass between the Falklands and the Main.

If driven off the coast before reaching the Islands, it is better to beat up to it, to the northward, than to pass down south, to the eastward of them, after the gale abates. There are circumstances under which the outside passage would prove the more expeditious, but their presence cannot be known by description; the situation of the vessel, the direction of the winds, the appearance of the weather, etc. are the guides for pointing out the proper time for the outside passage, and they frequently deceive seamen, who have never made a voyage around Cape Horn.

The probability of meeting westerly gales to the south, after having passed to the east of the Islands, and the sufferings to which the ship's company is liable in them, are sufficient reasons why preference should be given to the passage between the Islands and the Main. The coast and the soundings along it, are clear and regular.

When the wind is fair, Cape St. John's should be doubled close around, and all canvass crowded on the ship, to carry her to the west as fast as possible. The difficulty of the passage consists in getting from Staten Land to 85° west.

If on clearing St. John's, or making Hermit's Island, a gale be met from the westward, the vessel, unless she could clear all danger by standing to the northward and westward, should be kept constantly on the starboard tack, until she either forges out of the range of the gale, or arrives in lat. 63°. With the easterly winds to the south, she can run to 85° west, whence she can steer north to 40° as previously directed.

If it be necessary to go to 63° south, before the winds will allow the vessel to stand to the westward, she should make her westing to the southward of 60°; if she gets out of the strength of the gale, before she reaches 63°, she can run up her westing on the parallel upon which she may be, or as near it, as the breeze will allow. It is always advisable to be in lon. 85° before attempting to pass to the northward of Cape Horn.

The U. S. S. Falmouth, and H. B. M. S. Volage, doubled Cape Horn in Oct. 1831; the latter had thirty eight the former twenty four days from the Cape to the lat. of Talcahuana. Both of them took a westerly gale off the pitch of the Cape. The Falmouth stood down on the starboard tack to 62° 5′ S. and found the winds more favorable. The Volage, persisting in the attempt to gain the "in-shore" passage, lay to on either tack, to preserve her relative position with regard to the lat. of the Cape, and was drifted off to the eastward. When this gale abated, she stood up to the Cape again, and took another, in which she was also driven to the eastward. In the third attempt she succeeded in doubling the Cape. She put into Talcahuana, to repair the damages which she had sustained while riding out the gales from the westward. The Falmouth arrived in Valparaiso in excellent order.

In May 1829, the U. S. S. Guerriere, sailed around Cape Horn into the Pacific; she encountered a gale from the northward and westward, before she passed Diego Ramirez, she received it in the starboard tack, and forged to the southward: she got clear of it in lat. 58° 37′ near which parallel she stood to the west, she was twenty one days from the Cape to the latitude of Talcahuana.

The U. S. S. Brandywine, made the same passage in seventeen days, she passed the Cape in December, 1826; she found the winds varying from N. W. to S. W.; she ran up the usual *westing* without crossing the parallel of 57° 30′. When the winds freshened so that she could not beat to windward, she lay to with her head to the south, giving the land a wider berth.

The American whale ship Enterprise, and the English whaler Sussex, encountered a gale off the Cape, near the same time. The former, by forging to the southward cleared the gale in lat. 58° and in fifteen days after first crossing the parallel of the Cape, she was in the latitude of Talcahuana. The Englishman had thirty six days to the same parallel; she lay to, close to the cape in order that when the gale should abate, she might hug the land around. Before she cleared the Cape, she was twice driven by gales off to the eastward. Short passages are made by hugging the land when the wind is fair or moderate from the westward, but seldom by waiting first to ride out a gale from that quarter. Many instances could be cited shewing the advantage of steering to the southward under such circumstances. But to prove what is here recommended is not pertinent to the object in view, reasons must suffice. Common practice teaches that good passages are more frequently made by those vessels, which finding contrary gales off the Cape, stand boldly to the south, than by those, that lie to in them, keeping near the parallel of the Cape. The barometer has not been found to be of much practical utility off Cape Horn, how useful soever it may be in middle latitudes, by indicating the approach of hurricanes; it is no index to the winds in the high latitudes to the south of Cape Horn.

He, who in the China seas, is warned by the barometer, of the approaching Typhoon, and can foretell the coming of a gale by the height of the mercury in it, finds that off Cape Horn, the same indications are frequently followed by moderate breezes and even by calms. Here the mercury, below the mean height of lower latitudes, becomes very unsteady, falling and rising several inches in a few hours. During the strength of a gale, sometimes it is observed to rise, at other times it falls or remains in statu quo. Its mean height south of the latitude of Cape Horn is 29·03 *in.*

As the Pacific coast of Tierra del Fuego and Patagonia is approached with the wind from the westward, the mercury in the barometer ascends. When the wind is strong, it rises above thirty inches, and close under the land with fresh westerly gales it frequently stands above 30·50 *in.*

From lat. 45°, embracing a region towards the south of twelve or thirteen degrees in breadth, the most prevalent winds are from the westward. Vessels entering this region from the south have a rise in the barometer, when the wind is on the land. The rise is generally observed to commence about the latitude of the Cape, continuing to increase as the land is neared; and when the winds are fresh, a greater accumulation of atmosphere is shown by a higher range of the mercury.

The result of my own barometrical observations compared with others to which I have had access, shews that within this region, the barometer stands higher when the winds are from the westward, than it does, cæteris paribus, between the same parallels in the Atlantic. The difference is nearly as twenty nine to thirty, and increases as the land is approached. This accumulation of atmosphere is caused from the obstruction which the mountains of Patagonia, and the highlands cf Tierra del Fuego affix to the winds in their passage across the continent towards the Atlantic. The air thus obstructed is compressed by that coming after it, and according to the force of the wind, and the distance from the land, the barometer indicates a greater or less superincumbent pressure. The disturbing cause which first destroyed the atmospheric equilibrium towards the East, continuing to act, the density of the obstructed air is increased by the increased tendency to restore the equilibrium from the west. The air thus forced, rushes around the southern extremity of the land, with an impetuosity that is known only to those, who experience the effects of its violence. This current of air, as it sweeps around

Cape Horn is confined to a channel, which is widened towards the south in proportion to the latitudinal breadth of the column, that is rushing to the east. Near the southern borders of this channel, the easterly winds commence, returning in eddies towards the west whence they are again carried eastwardly, in the current that rushes around Cape Horn.

Art. VI.—*Plan of an Instrument for finding the true Lunar Distance;* invented by M. F. Maury, Passed Midshipman, U. S. Navy.

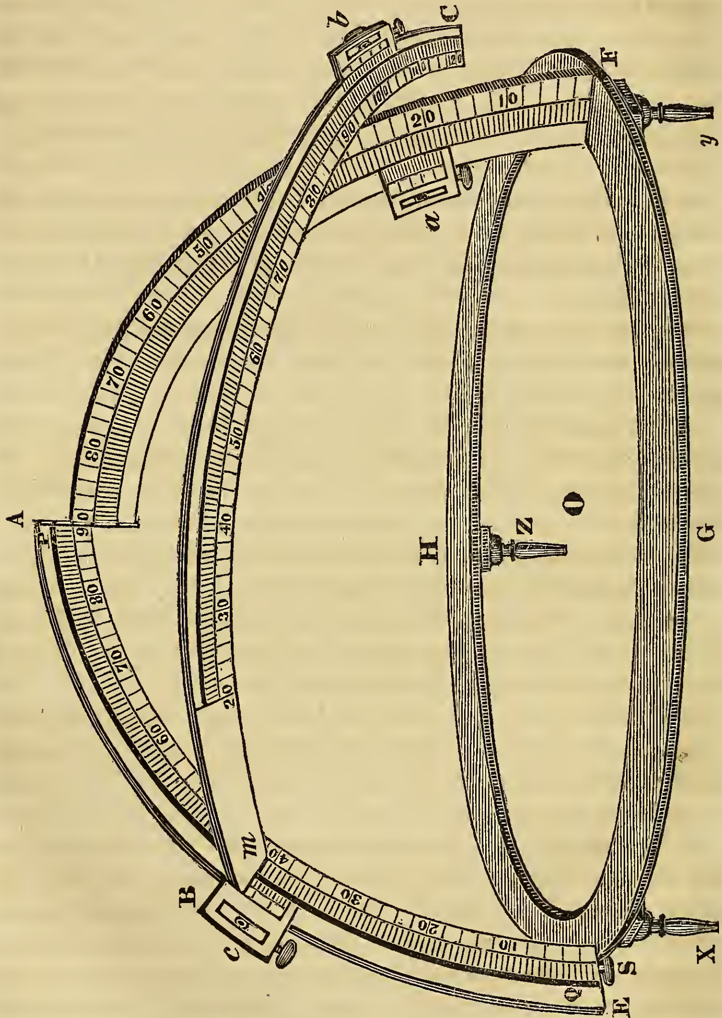
G F H E, is a great circle of brass, standing upon three legs X *y* Z; it represents the horizon. A E, A F and B C, are arcs of great circles; they also are of brass, and their planes pass through O, a common centre.

The periphery of G F H E, the middle curvature P Q, of A E, the concave circumference of B C, and the convex of A F, have equal radii centering in O.

A E, describes half of a hemisphere on the hinge at A, the axis of which, is that of the zenith and nadir, and of course passes through O. The extremity F, of the arc A F, is fixed in the plane of G F H E, and the extremity E, of A E, revolves in the plane of G F H E, and along its circumference, describing arcs of equal circles, from the centre O, until it stands in the plane of A F, the two then represent a semicircle. By means of the screw S, which presses E to G F H E, E is placed and fixed at any distance from F. The hinge at A, is of brass turning against steel, which lessens friction.

A E and A F, are arcs of the azimuth circles, in which the sun or a star and the moon are at the time of taking a lunar observation. Each is 90° and graduated to every 10′ or 15′ on a slip of finer metal, let in for that purpose.

B C, is an arc of the geocentric circle, in the plane of which, the sun and moon appear, when the observation is taken; it is graduated as the other arcs are, but from 20° to 120°. It is intended first to set off the *apparent*, and then to measure the *true* lunar distance. To assist in transferring the altitudes and distance with exactness, from the sextants with which they are taken, to the arcs of the instrument, each of them is provided with a vernier scale, *a*, *b*, *c*, having a tangent and a fixture screw respectively attached to them. The extremity *m*, of B C, is hinged to zero of the vernier *c*; the axis of this hinge is moveable along A E, around the centre O; it is the line of intersection of the plane of B C with that of A E. The inner curvature of B C, moves in the plane of A E, and along its middle curvature P Q, as the vernier *c*, travels along A E, and the plane of the arc B C, always passes through the common centre O.



With the observed altitudes and distance of the two bodies, taken in the usual manner, for finding longitude from a lunar observation, the *true* distance is determined, by first marking off on the two arcs

A E, A F, the altitudes of the centre of the sun or a star, and the moon, corrected for *dip* and *semi diameter*, and their apparent distance on B C. The verniers *a*, *b*, are fixed at the altitude and distance, by means of the fixture screw—the two legs A E, A F, are opened until zero of the verniers *a* and *b* are in the same line with regard to the centre O; the screw S, is then turned, and the two legs A E, A F, are fixed at that opening—the two altitudes are then corrected for parallax and refraction—(say the sun's altitude has been set off on A E, the moon's on A F,) move the vernier *c* to which *m* is hinged, down to the degrees, minutes and seconds, corresponding to the sun's corrected altitude, and turn the fixture screw. Move *a* up to the moon's altitude, corrected for parallax and refraction, and fix the vernier at it—then move the vernier *b* and turn the arc B C on its hinge until zero of *b* stand in a line with zero on *a*, and the degrees, minutes and seconds, upon which *b* stands, will be the *true lunar* distance from which the longitude is determined, as it is in all other methods.

Art. VII.—*On the Color of the Air and of deep waters, and on some other Analogous Fugitive Colors;* by Count Xavier De Maistre.

Translated from the Bib. Univ. by Prof. J. Griscom.

The blue color of the sky is accounted for, by supposing that the sun's light reflected by the surface of the earth, is not entirely transmitted by the atmosphere and lost in space, but that the molecules of air reflect and disperse the blue ray. Why this ray is reflected in preference to the indigo and violet which are more refrangible and appear to be more easily reflected, is a circumstance not accounted for.

The same blue reflexion is observed in deep sea water, and in lakes, and rivers, when they are limpid.

The same singular phenomenon is also witnessed in various substances of different natures which have no apparent analogy; thus opaline substances are blue by reflexion: the noble opal, (independently of the partial rays which give so high a value to this stone and which are attributed to natural fissures*) reflects a general blue color

* This was the opinion of the celebrated Haüy.

which is also observed in some other siliceous stones, and which is still more obvious in opaline glass. A weak solution of soap, is slightly blue; the jelly of ichthyocolla is more so, and an infusion of the bark of the large chesnut tree, (*maronnier*) which is perfectly opaline, still more. Newton speaks of a wood which he calls nephritic, the infusion of which is opaline. In the Sicilian sea, at the mouth of the Giaretta, (the ancient Simethus) specimens of amber are found, which are in great request on account of their highly opaline properties.

A blue reflexion is also observed in certain bodies which are opake-white when reduced to plates thin enough to transmit light. A familiar example occurs in the skin covering the veins which transmits a blue, although neither the skin nor the blood is of that color.

The mixture of white with black and with transparent colors gives, in painting, numerous examples of opaline blue.

This blue color is the only one which can be explained on the theory of thin plates, by supposing that the particles of opaline bodies have just the dimensions requisite to reflect the blue ray. This explanation derives some probability from observing that the color transmitted by these bodies is the complementary yellow of the reflected blue. This theory however presents great difficulties, and it is not intended absolutely to admit it in this essay.

The analogy between the colors of opaline substances and those observed in the air and waters, will become obvious by an examination of their action on reflected and transmitted light, proving that the phenomena are owing to the same cause.

Opaline glass is produced by mingling in the common metal of white glass, a portion of calcined bones, which gives a blue shade without much impairing the transparency. The bone powder appears to be in a state of extreme division or in a kind of demi-solution which does not disperse the transmitted light.

The color of the light transmitted by opaline bodies varies according to the volume of the mass; it is yellow if the body is thin, and becomes successively orange and red in proportion to the increase of thickness. The analogy of the air, with opaline substances is not only manifest, in the blue reflexion, but also in its action on transmitted light which becomes successively yellow, orange and red, according to the volume of air and the kind of aqueous vapors with which it is impregnated. When the sun is high and his light crosses only the purest and thinnest portions of the atmosphere to reach the clouds, they are white with a slight tinge of yellow; they become sometimes yellow and orange as the sun declines; and at length red and purple, when his light grazes the earth and is transmitted by the densest portion of the air and loaded with the vapors of the evening.

But it often happens that the colors do not appear and that the sun sets without producing them. It is not therefore to the purity of the air alone that we must attribute the opaline property of the atmosphere, but to the mixture of air and vapor mingled in a special manner, and producing an effect similar to bone dust in opaline glass, neither is it the quantity of water in the air which occasions the colors, for when the weather is very damp, it is more transparent than during a time of drought. Distant mountains are seen more distinctly, a well known prognostic of rain; the sun then sets without producing colors, it looks white through the fog and damp vapors of the morning, but when the clouds are colored red by the setting sun, the phenomenon is generally deemed the signal of fine weather, because these colors are a proof of the dryness of the air when these contain only the peculiar diffused vapors which give it its opaline quality. In this state of things, the disc of the sun appears like a red fiery globe divested of rays.

The blueness of the sky therefore varies according to the kind of vapor which is spread through the air; and what renders it unquestionable that its blue color is caused by these vapors is, that it appears black when seen from the highest points of the globe, above which there is not sufficient vapor to reflect the blue color.

Limpid waters, when they have sufficient depth, reflect, like air, a blue color from below; it is of a deeper shade because it is not mixed with white light; very often it is not perceived at all; the reflexion from the surface on which the sky and surrounding objects are painted as in a mirror, often occasions the disappearance of the internal reflexion or forms with it complex shades.

We have seen that the property which air possesses of producing colors is derived from the presence of watery vapor; analogy leads us to presume that this property in water arises from a mixture of air which it always contains to a greater or less amount.

Although the blue color of water is often masked by numerous causes it is sometimes exhibited in all its intensity; a fine example of it is witnessed in looking at the Rhone from the Bridge at Geneva. The river seems to flow from an ultramarine* source. The spectator is in the most favorable situation for observing the internal

* Having the blue color of the ultramarine paint.

reflexion, disengaged, as much as possible under an open sky, from the reflexion at the surface.

Agitation of the surface has a great effect on the color. A tranquil sea sometimes reflects the warm color of the horizon, representing all the tints of a luminous sky so exactly, that the sky and sea appear to be blended with each other; but if a gentle breeze ruffles the surface, the brilliant tints vanish, and the blue from the interior immediately predominates.

Such is also the cause which enables one to distinguish the course of the Rhone far into the waters of the Leman: the progressive motion of the river in the motionless water of the Lake produces an agitation which diminishes the brilliant reflexion of the sky and renders the color of the water more sensible.

The green tint which the sea often assumes, may seem to throw some doubt on this property of reflecting the blue ray, regarded as inherent in the nature of water; but this green color is observable only when the depth of the sea is insufficient, that is when the bottom may reflect the transmitted light.

In looking at the sea from an elevation of about fifty toises, on the shore of the Island of Capri, I observed spots which were of the finest green, much more luminous than the dark blue sea, with which they were surrounded. To ascertain the cause, I took a boat and proceeded to the place. The spots then were no longer perceptible, but I soon rediscovered them, and found that the color was occasioned by white rocks which were easily distinguished, notwithstanding their great depth, from the dark sandy bottom in which they reposed. These rocks, viewed in a vertical direction, were of a lighter green than when seen from the height, but I could not doubt that they were the cause of the phenomenon.

To settle the point by direct experiments, I prepared a square sheet of tinned iron, fourteen inches long, painted it white on one side, suspended it horizontally to a cord, and sunk it in a deep place, where the water under the boat, was blue without any mixture of green, watching the effect under the shade of an umbrella which was held over my head. At the depth of twenty five feet, it acquired a very sensible green tinge, and this color became more and more intense to the depth of forty feet when it was of a beautiful green, inclining to yellow; at sixty feet the color was the same, but of a darker shade, and the square figure of the plate was no longer distinguishable; until at eighty feet, there was apparent only an uncertain glimmering of green which soon disappeared.

We thus perceive that the light of the sun transmitted through water and reflected from a white surface, produces green. The cause may be readily conceived by admitting in deep waters, the same opaline property which we recognize in air. The light, after penetrating a mass of one hundred feet of water to reach the plate and return to the surface ought to be yellow like that which would be transmitted by an opaline fluid; this color reflected by the plate, mixed with the blue which reaches the eye from all quarters, produces the green. If the bottom of the sea were white, like ceruse, the waters near the shore would present the same green tint which the plate produced at different depths; but the bottom is generally of a dark grey which reflects less light, and therefore yields only a dark and uncertain green; hence the green color of the sea as witnessed near the shore is owing to the reflexion of light from its bottom. To leave no room for doubt in this matter, I took a boat and pushed out from the shore, under a clear July sun, at eleven in the morning to examine the changes which might be perceptible in the color of the water, viewed on the side of the boat opposite to the sun.

At fifty toises from the shore, the water was decidedly green, the shade of which remained during fifteen minutes; it then became a bluish green, and in advancing, the blue continued to increase and at length to predominate, and in an hour's time the water under the boat was of a pure blue without a mixture of green.

In returning to the shore I was attentive to the reappearance of the green and as soon as I found it clearly marked, I sounded and found the depth one hundred and fifty feet; thus the light which renders the sea of a green color, passes through three hundred feet of water. But in that part of the gulph, another cause contributed to the green color, viz. the impurity of the water as it exists to the extent of some miles from the shore. The bay of Naples receives no river that can give motion to the waters charged with all the filth of that populous city. On the shores of the Isle of Capri, the water is perfectly blue at eighty feet, while near Naples it requires one hundred and fifty feet, a difference which must be ascribed to the impurity of the water in the bay.

If the bottom be of a black or very dark color, the water may be blue at a much less depth than eighty feet. Besides, if an obstacle intercept the direct rays of the sun, so as to throw a shade over the bottom, while the water itself is illuminated, the latter will be blue, because no longer colored by yellow rays from the bottom; this effect may take place near shore in deep waters, by projecting cliffs or high shores.

It is thus ascertained that when the sun's light, transmitted through water, is not lost in its depth, but is partially reflected by the bottom, the water is of a green color.

This effect may arise in deep water, from beds of submarine plants, or by myriads of microscopic mollusca, which, covering a vast textent of sea, may act upon the light, or even exist in mass sufficient to produce a permanent color.*

The colors transmitted by deep waters cannot be directly observed like those of air which are visible among the clouds; observations agree on this point. The learned Halley, in descending in a diving bell, observed that a ray of light which reached him through a small opening closed by glass, gave to the upper part of his hand a rose color. Had his hand been white, instead of being itself more or less red, the experiment would have been more conclusive. The depth to which he descended was probably not more than thirty or forty feet, at which the transmitted light could not differ much from yellow, which, mixed with shades of white and with the natural color of his hand, would produce a rosy tint. He observed that the under part of his hand was green, which must have been occasioned by reflexion from the bottom.

The bluish green color of crevices in the glaciers is occasioned, in the same manner as that of water near shore; if the mass of ice was as great and as homogeneous as that of the sea, the interior of the crevices would be blue; but the ice contains air bubbles, particles of snow, and fissures which reflect the transmitted light, throwing it from one face to another of the crevice until it finds an escape. These opake substances in the glacier, produce the same effect as a white surface in the depths of the sea.

There is on the shore of Capri a grotto which nature seems to have constructed to exhibit in all its beauty the green color of the sea, and which on this account is called the *azure grotto;* it is situated under a cliff on the north side of the island. As it could not be entered by a common boat, it remained unknown until 1826, when two Prussian artists Kopitch and Frisi swam into it, and made

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* The theory of the author derives confirmation from the beautifully green appearance of large fish as they turn upon their backs in rising towards the surface, and sporting round a ship, during her passage through a dark blue sea.—*Trans.*

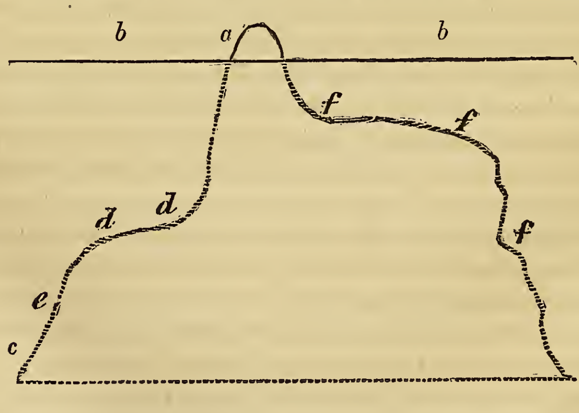
it known. Their account excited public curiosity, and boats of convenient size were made which now serve to introduce amateurs. Its entrance is triangular, having a base of four feet five inches wide and about the same height. The summit is rounded and having but little thickness, the entrance is easily effected by stooping, when the traveller finds himself in a spacious grotto, the sides and roof of which are remarkably regular. Its extent from the front to the rear, which is the only landing place, is one hundred and twenty five feet and it measures one hundred and forty five feet in a transverse direction. The depth of water at the entrance is sixty seven feet, in the middle of the grotto sixty two feet and at the landing place fifty eight feet. The rock is limestone of a clear grey fracture, and there are no indications of stratification.

On entering, every thing appears dark except the water which is luminous and of a splendid blue, contrasting with the general obscurity. In advancing from the entrance the ends of the white oars shine in the water with a splendid blue light which disappears as soon as they are raised : this is the most singular phenomenon of the azure grotto, for people are puzzled to conceive why objects are so vividly luminous in the water, and no longer so when above the surface. In dipping the hand or a cloth into the water one would think it a blue dye; the whole immersed part is luminous and colored, while the parts without are dark and uncolored.

At the bottom of the grotto there is a small space on a level with the water, where debarkation is effected, and which is the only spot which leads to any suspicion of the work of human hands in the grotto. It is a kind of bench in the rock about three feet high, on which several persons may conveniently place themselves and examine at leisure the phenomenon of the azure grotto. The light which comes in at the small opening, produces a train of white light, like the reflexion of the moon from the water when rising, and which extends half way over the sheet. The rest of the surface is blue even to the feet of the observer. This color gradually diminishes to the right where the walls of the grotto are farther from the entrance. The train of white light, illuminates also the vault and exhibits it in its natural color, but when the entrance is closed by a boat or more perfectly by a dark cloth the vault itself becomes blue, reminding one of the effect of burning spirits of wine in a dark chamber. There is then no light but that which proceeds from the water. The experiment of the cloth ought to be made by all who wish to enjoy the spectacle in its full beauty.

If when the observer is seated on the bench, a boat passes in front, it forms no reflexion, nor shade in the water. If the eyes are then covered with the hands so as to hide the boat and the water, the former appears suspended in the air like a dark silhouette crossing the sky. This spectacle is so striking when first observed, that one cannot avoid some apprehension on account of those who furnish the occasion of it. In passing to the dark side mentioned on the right, the water is no longer blue but remarkably transparent. The rock below is so illuminated as to show its fissures at a considerable depth, while above the water, it is very obscure. The water line is clearly marked and has a yellowish tint. The depth seems to increase, the longer it is observed, and at length the bottom is seen although forty feet below. The white plate which I let down was very distinguishable on the darker sand. Its color instead of being green, as when tried in the sun, was slightly yellow.

The feeble yellow light which illuminates the submarine walls in this part of the grotto proceeds by reflexion from the bottom, and from the walls opposite, which receive the exterior light; this light, which has traversed a great mass of water, should be yellow like that transmitted by opaline fluids, and thus the opaline quality of the sea affords a satisfactory explanation of the principal phenomena of the grotto. I have endeavored to give an idea of this construction by means of the subjoining figure, which represents the exterior rock or shell of the grotto as it exists both in the sea and above the surface.



The little entrance is shown at *a* above the level of the sea represented by the line *b b*. The eastern side of this entrance extends almost perpendicularly to the depth of thirty or forty feet, when it appears to be cut horizontally at *d d* and suspended on the dark blue water of the sea; *d e c* is the supposed continuation of the eastern side of the entrance, to the bottom, which as we have seen is sixty seven feet deep. The western side of the entrance *f f f* forms an angle at ten or twelve feet below the surface, and is prolonged horizontally from twenty to twenty five feet and then descends obliquely, probably to the bottom of the sea where it cannot be seen beyond thirty or forty feet.

This construction gives an immense opening for the light to enter the grotto through the water, even when the little opening above the surface is closed, and it thus occasions, over a great mass of water, that dispersion of the blue ray which always takes place in deep and limpid waters, and which is manifested in greater splendor in the azure grotto in consequence of its being mingled with no other light.

Having considered the opaline property of air and water, let us now examine the production of opaline blue in opake bodies.

The cause of the blue tint assumed by the fine skin which covers the veins has hitherto been a doubtful question. This phenomenon, which is uniformly connected with the opaline property of the skin, is mentioned by Leonard De Vinci; let us first see the conditions under which it exists.

First, the vein must be deep enough to absorb all the light transmitted by the skin; and the skin must have the thinness requisite to transmit a great portion of the light. If the vein is thin, it reflects the color of the blood and becomes red;* this color mixing with the opaline blue of the skin, forms those violaceous tints, observable on the countenances of persons of dark complexion (brouillé). If the vein is still thinner and nearer the epidermis, the transparency of the skin increases and the red color is more distinct; finally, a tissue of imperceptible veins, very near the surface of the skin, colors the cheeks and lips of young people of a fine complexion, with a uniform red; but we may observe that these beautiful colors have not the exact tint of the blood which produces them; it partakes of the opaline blue, which renders the color slightly carmine and tinges sometimes the lips of sanguine people of a purple or violet hue.

* It is thus that a wide barometer tube filled with colored wine, appears black, while a thin thermometer tube, under like circumstances, is of a beautiful purple.

Thus the difference which may exist in the size of the blood vessels and in their proximity to the surface is sufficient to produce all the shades of blue, violet, red and purple which are seen in the human face, by the mixture of the opaline blue of the skin, with the red of the blood.

The red color of the blood is not the cause of the blue tinge of the veins; it might be black or green without occasioning any change; it is enough that the coloring principle absorbs all the light transmitted by the skin. This result may be artificially produced by a very thin plate of ivory which has nearly the same effect as the skin. If a few drops of ivory black, prussian blue, cochineal, or bile, sufficiently dense to be opake, be placed on one of its surfaces, they produce alike a blue tint on the opposite surface because they equally absorb all the light transmitted by the ivory. But if instead of a coloring matter which absorbs light, we use an opake reflecting coloring substance, we have a tint compounded of opaline blue and that of the color employed.

The red oxide of lead placed on the ivory, gives on the opposite surface, a slight tinge of carmine. Some painters avail themselves of this property of ivory, in sketching the cheeks and lips of their portraits by placing a coat of minium on the opposite surface, and thus obtain indirectly the effect of a slight use of carmine.

But if instead of minium, Naples yellow be put on, there is on the opposite surface a green spot. In both these cases then, the opaline blue is mingled with the proper tint of the opake reflecting color, while the blue alone appears when the applied color absorbs the light transmitted by the ivory.

The mixing of colors in oil painting furnishes still more evidently an opaline blue. The most common case is the mixture of white with vegetable black which produces a bluish shade. Various writers have adverted to this, and as indigo and prussian blue, in mass, approximate to black, it was thought in former days, that blue was a mixture of light and shade; but the blue produced on this occasion, belongs exclusively to white and not to black as is proved by the following process: two plates are painted of a grey color, one by a mixture of ceruse and charcoal ground in oil, the other by superadding to a coat of white a glazing of charcoal, so that they may both have the same depth of shade; the first will be bluish, the second grey without a mixture of blue.

As transparent colors in oil lose almost wholly the color which they have in a pulverulent state, and thus in mass approach to black, the mixture of them with white produces also opaline blue, which modifies the natural shade of the color.

Every painter knows the striking difference there is between the color of a mixture of cochineal lacker with white, and that which the same lacker produces as a thin coating upon a white ground; the first is of a violet color, and the second has all the purity and splendor which is characteristic of this fine color. Thus artists who wish to obtain the beautiful red of cochineal or madder in their draperies, always employ these lackers in mixture (en glacis.) Opake reflecting colors, such as Naples yellow, chromate of lead, yellow ochre, produce, as well as white lead, opaline blue, by a mixture with black and the effect is still more sensible. These compounds, according to theory, ought to give only shades of yellow; and yet their tints are decidedly green, so that they are often used for painting the deepest verdure of landscapes. In these cases it is the opake reflecting color which is opaline.

I have stated the most remarkable instances of the singular property which certain colors possess of producing opaline blue by mixture, but there is an infinite number of other modifications less apparent, resulting from mixtures of compound colors, which it would be impossible to describe, but which may always be pre-ascertained by the following rule: *When white lead, or opake reflecting colors are mixed with black or with transparent colors, there is a production of blue, and a consequent modification of the primitive shade of the coloring matter.*

These modifications are often very slight, but they do not escape attentive observers. In the preceding observations, I have described effects, well known it is true, but which appear to have no analogy to each other, and which appear to me to depend wholly on the peculiar property which the blue ray possesses of being reflected, in preference to other rays more or less refrangible, by the simple mechanical resistance of the molecules of bodies which transmit light. This resistance takes place in large masses of transparent fluids, as in air mixed with watery vapor, and in water mixed with air.

It takes place also in opake bodies which are less transparent, but under smaller dimensions. Lastly, it is observed in white opake or colored bodies, as in the fine skin which covers the veins and in mixtures of colors.

ART. VII.—*The Voice and its Modifications, (more particularly Ventriloquism) briefly considered;* by ROBERT TOLEFREE, Jr.

REASON and speech are the characteristic prerogatives of man and although the latter may be acquired by some animals to limited extent, nevertheless it is merely imitative with them. The brutes may learn to pronounce the name of an individual person and may perceive, in time, that this sound raises the attention of the person designated ; or they may be taught by frequent repetition, that some other sound corresponding with the articulation of some particular word may gain something which they may desire, yet when made to combine words which they may have been instructed to repeat, these animals are destitute of a knowledge of their conjoined signification. The ideas have not assisted them in preserving an accurate recollection of the arrangement, for they learn a concatenation of even familiar sounds without any reference to the import of their component parts. Man however when taught the use of words can bring forth, combinations entirely new and appropriate, while inferior animals scarcely ever utter other significant sounds than those with which they are acquainted, and should words happen to be combined by them differently from any thing they had heard, this strange and unmeaning collocation would not occur to a mad man. All who have listened for some time to a loquacious parrot will be convinced of the truth of this statement. It may be objected to my distinction between man and brutes that many persons can learn sentences, passages and even long poems in their native tongue by simply employing the ear. This is not to be denied, and we must reply that the man whose memonics are based on vocal distinctions only, has little to rank him among the human family whose exalted criterion is *res non verba.*

From the foregoing statement it is plain, that the acquirement of language is very limited among inferior animals and this curtailed gift can be possessed by very few. Man however, from conversation to declamation from recitative to singing, enjoys this faculty to the greatest extent, and the permutations of which each of these are susceptible are very extensive. In declamation, a greater portion of air passes through the larynx ; there is more resonance not only from the increased velocity and the augmented quantity of air, but from the position of the mouth and velum palati ; a more considerable vibration of the chordæ vocales occurs; the cheeks are brought nearer the teeth; the muscles at the posterior part of the jaw are felt to act with greater force; the lips are generally more advanced and thus the depth of the oral cavity is increased. The notes are few, while the modifications and combinations are many. In conversation, the inspirations and expirations are not so forcible and irregular, nor do the thorax and vocal organs undergo such changes in position; the variations are not so numerous and their extent is more limited.

In singing, the sounds can be measured and regulated by rules and with these sounds others can be formed in unison; the notes are more numerous and there are greater variations. In the acute sounds, the opening of the glottis is smaller and the rapidity of the air is increased in its passage, while deep tones induce a position of the larynx the reverse of the preceding; the situation of the musician has some effect on the ease or labor with which he sings, the muscles of the trunk seem to vibrate in the gravest tones and he who would comprehend the more complicated movements of our best singers must observe them during their masterly efforts. Recitative partakes of both declamation and singing, and as we have spoken of both of these, it is not necessary to dwell on the intermediate state. The changes above mentioned are nearly all open to the ocular demonstration of every enquirer and it is only the more minute and complicated internal movements of the vocal organs that have been subjects for so much discussion. The lapse of every few years brings a new theory and its author expects to sweep away the last cobweb system, but perhaps a tissue of no firmer or better woven materials is substituted.

Ferrein leaned to the notion that this organ was like a stringed instrument. Dodart maintained that the larynx was a wind instrument. Richerand and Copeland compare it to a French horn and Cuvier to a flute. Mayer modified the opinions of Ferrein and Dodart. Blumenbach believes it to be like an Æolian harp. Magendie and Malgaigne liken it to a reed instrument. Geoffrey St. Hilaire to a reed and flute. Kratzenstein to a drum. Savart to a bird call. To the catalogue we might make additions, but we have presented a sufficient number to convince the reader of their contrariety and the only thread that can be furnished the student to extricate himself from this labyrinth, is to advise him to adopt exclusively none of their doctrines but bear in mind that they are all partially correct. Anatomists have, in every age, been too fond of comparisons, and they have adopted imperfect ones rather than lose their imaginary benefit.

The action of the chordæ vocales is in some respects like the Jews harp; still, who has compared the larynx to this simple instrument? We must not confine ourselves to one instrument in illustrating articulate sounds; every musical instrument has some points of resemblance with the human voice and yet on the whole all instruments are far inferior to this wonderful contrivance in man. What ingenuity could devise an instrument the chords of which might in a minute run through innumerable changes in contraction or relaxation and through wonderful alterations in diameter and length; could in an instant furnish different volumes of air and give to it surprising variations in its force; could imbed the instrument in materials by which its action was not impeded but facilitated, could lubricate the surface of the tube with a fluid that preserved its power, could furnish it with an elastic appendage, the lungs, serving the purpose of a bellows; could add such auxiliary appendages at the mouth and nose, which possess so great diversity in structure, as to form, density and component parts, and still, all these dissimilar parts are to be subservient not only to the voice but to the volition and other functions: and in short, could make this piece of workmanship so as to be altered and repaired by an action peculiar to itself, and that at length, it should be susceptible, of so many changes as not to leave a particle of its original materials and all this mutation to take place without destroying its power? If we reflect on these multifarious powers, we feel the inadequacy of comparisons and the impossibility of fully imitating by art this wonder of nature. Any musical instrument may be compared to parts of the human vocal organs, but a single instrument will no more convey an adequate idea of their vast and varied powers than a single brick, as mentioned in ancient fables, could give an idea of the entire edifice.

We have noticed the four classes of sounds which are expressed by symbols; still there are others peculiar to man which must be accounted for on principles quite different from the preceding expositions.

Coughing arises generally from some irritation in the larynx, trachea, bronchiæ or substance of the lungs, and is produced by the action of the muscles of respiration. It might be shown, if necessary, that the lungs are passive and, like the stomach in vomiting, act not by any contractile power of their own.

Yawning is caused by debility or the sluggish action of the muscles of respiration, and consequently a diminished force in the circulation; and a smaller quantity of air is admitted to the lungs and carbonic acid gas being accumulated in the bronchiæ, nature makes this effort to dispel the torpor and relieve the air cells.

Snoring is effected by the inactivity of the muscles of the extremities, in consequence of which the blood returns more sluggishly to the heart; some of the muscles of respiration are thus impeded, and the remainder have to labor more violently to overcome the deficiency, for the lungs require a certain quantity of air to preserve their function; what they fail in receiving in the regular inspirations they make up by more frequent inhalations, and what they want in the frequency of inspirations, they endeavor to compensate by the quantity admitted. In snoring, the mouth is besides but slightly extended, the palate is depressed, a larger volume of air is admitted into the nose, and hence the peculiar nasal resonance.

Sneezing results from the irritation of the membrane lining the nasal cavity and the sudden action of the muscles of respiration. Nature here strikingly shows the precautions she has taken to expel irritating bodies from the nose. The entrance from the mouth to the nares is guarded by the velum palati, and in sneezing, this part of the roof of the mouth is greatly depressed, the passage of course is more open and the exit by the mouth more hindered, and hence the peculiar nasal sound.

Hiccup arises from the spasmodic action of the diaphragm compressing the lungs with more or less violence. The length of time between two successive inspirations cannot be always fixed, for habit, circumstances, and the will, cause such alterations in this interval as to strike with surprise many unacquainted with the fact.*

When these inspirations are partial, short and quick and are modified by the position of the mouth, we have sobbing or sighing.

Groans are labored, deep and lengthened expirations.

Laughing is a continued expiration, with catches, or more properly there is, at intervals, from the action of the diaphragm and muscles of the chest, an increased velocity in the exit of the air. These inarticulate sounds are little affected by the larynx, whilst other modifications of the human voice which are used to convey ideas, are so de-

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* The pearl divers of India have accustomed themselves to exert the muscles of respiration so as to admit a larger quantity of air than usual and thus endure an absence from the open air, which is truly wonderful.

pendent on this organ that it will be proper to give it some attention. The larynx is an irregular tube somewhat resembling a truncated cone. It is composed of the two arytenoid, the thyroid and the cricoid cartilages, besides the epiglottis, making in all five cartilages with their ligaments, nerves, veins and absorbents, also nine muscles, two glands and four arteries, and their branches; all these are also separately essential, to the perfection of the human voice and generally the absence or diseased state of one of these parts has a striking influence on the tones. Hunter proved, that the music of the males of singing birds was produced partly by a greater developement of the muscles of the larynx.

Women have a smaller larynx than men and some physiologists have made the difference greater, than is actually found by examination.

Females of licentious habits lose so far their effeminate vocal distinctions, as to have little in their utterance, to distinguish them from males.

Age exerts no inconsiderable power in altering the tones, for infancy, puberty, manhood and the senile state bring with them their peculiar intonation.

Not only the use of the muscles of the larynx, but the exercise of the muscles of the chest as we see in the blacksmith, increases the power of the voice. The size of the chest, lungs and trachea all bear on the force of the utterance and to demonstrate this, we have only to allude to the lion whose capacious trachea and the muscular strength of whose chest, render his roaring an object of terror to the beasts of the forest. On the other hand when the quantity of air emitted is small and the velocity diminished, the sounds become soft and low, and make an impression on the ear like an echo, because the vibrations are not so rapid. This consideration is important in explaining the art of ventriloquism, which we shall shortly notice.

In grave tones, the velum palati is lower, leaving the entrance to the larynx more open, the larynx is depressed, and the cavity of the mouth is elongated : the contrary occurs in the acute sounds, the passage to the nose is more obstructed by the ascent of the velum palati, the larynx is raised, the opening of the glottis decreased and the rapidity of the air, passing through it is increased, and the depth of the oral cavity diminished.

Thus we see that the extent of the vocal cavities and their position have great effect on the voice, and this should not be lost sight of in treating of ventriloquism.

To produce intensity in the voice we all know that the mouth is open, the velum palati raised and the position is nearly the same as in declamation, and all who have seen ventriloquists must be convinced, that they cannot practise their art and use the mouth as those who wish to be heard at a distance, for in the latter case the stock of air is rapidly exhausted and the principal requisite for ventriloquists is to hold this collection for use. It is curious that the attention of physiologists has not been directed to the action of the muscles of the thorax and more especially of the diaphragm, in accounting for ventriloquism. The muscles called into action during inspiration are the internal and external inter-costales, the diaphragm and the subclavius which are occasionally assisted by the latissimi dorsi, the pectoralis, the scaleni, the sterno-mastoidei, the serrati magni and the serrati postici superiores.

In expiration, the muscles that act are the diaphragm (which dilates,) the recti, the obliqui externi, and the interni abdominis, the sterno-costales and the transversales, aided sometimes by the longissimi dorsi, the serrati postici inferiores, the serrati magni, the quadrati lumborum and the pyramidalis. The diaphragm is relaxed in expiration and consequently rises, which movement I may compare to the resilience of a sponge from which a contracting force has been removed and of course it expands, and thus this muscle presses on the pleura and lungs, and altering the force of the current of the air from the lungs as it acts with more or less vigor. The diaphragm or midriff contracts in inspiration and the force and extent of its contraction must regulate the quantity of air admitted into healthy lungs. I have referred to the power of pearl divers in this respect and the application is equally appropriate to ventriloquists.

Before entering on this subject, it will not be irrelevant to give the opinion of others concerning the manner in which this art is created. Formerly it was thought that ventriloquists were gifted with a double larynx and this notion may have been imbibed during the reign of comparative anatomy, but human dissection has refuted the doctrine. Richerand, at first believed, that the art resulted from the possession of the faculty of holding in the stomach, as in a store house, a larger quantity of air to be used by the performer as it was wanted. He afterwards abandoned this explanation. Mr. Gough, was less satisfactory in his doctrine of echoes; since the shape and size of the room and even the open air do not hinder the exhibiter from practising this art. The theory of Haller, Mazer De Bonn, can easily be refuted by a ventriloquist, for constant or frequent inspirations render it impossible to perform. Fournier believed that by some singular movement of the glottis, sound was carried into the lungs and this appears to be only the opinion of Haller, rendered a little more mysterious. The theory is so contrary to all the principles of philosophy and physiology, that its mere recital is enough to condemn it in the view of those acquainted with the organization of man. Despincy believed that the glottis is open and the epiglottis pressed down. If this position were unaltered during the performance or, to speak more correctly, during the effort to speak, articulation could be shown to be impossible, and much less could the voice go through the varied tones of the ventriloquist.

Baron Manger, an excellent ventriloquist, was the first to give an idea of the art, and although he did not speak as an anatomist or physiologist, yet had others followed the clue he gave them, (A. D. 1772,) they might have been spared all their hypotheses. He said that he stored in his throat a quantity of air which he used with great labor. Now all know that air must have been retained in his lungs, and the pressing it out, at will, could not have been caused by the larynx, but by the action of the diaphragm and muscles of expiration. After this statement, physiologists neglected what ventriloquists had offered, and still attempted to work out an exposition of their own, without appealing to him that practised the art. Magendie thought it consisted of certain modifications of sound or speech, produced by a larynx of the common formation, with a strict attention to the different effects of sound thrown at different distances and through different modes of conveyance.

Here we have a maze of words and no exact knowledge is communicated from such expositions. Good thought ventriloquism, to be an imitative art, founded on a close attention to the almost infinite variety of tones, articulations and inflexions, which the glottis is capable of producing in its own region alone, when long and dexterously practised upon. But the art appears not to be always gained by practice, for I have heard ventriloquists say, that chance not discipline developed to them the possession of this art. When it is asserted, that the glottis or larynx is the only agent which is called into operation, the assumption is unsupported by examination. Hooper maintains, that ventriloquists have no organization different from that of other men; this assurance is unsupported by investigation, and if true, it amounts only to negative evidence. I do not maintain, that the formation of ventriloquists is different from other men, but that the control they possess over the functions of respiration, is greater than is usually found in others. That this faculty produces a greater development of some muscles cannot be questioned, and it may be discovered that the nerves over which the will presides have some different and nicer distribution. He that attentively inspects a ventriloquist will notice the fixed position of the mouth and the action of the muscles of the throat in altering the situation, diameter and extent of the larynx; he will be struck with the different hue of the countenance, from obstructed respiration: he will feel the diaphragm acting with a vigor and under a control not common to the generality of men, and this, combined with the action of the muscles of expiration must exert a great influence in increasing the action of the bellows (to which I compare the thorax,) and from thence is derived the volume and force of the air that escapes by the larynx.

To prove the action of the diaphragm, I would refer the reader to the American ventriloquist, Mr. Nichols, in whom the midriff acts so powerfully on the ensiform cartilage to which it is attached, that it is not a little surprising to see the action that the xiphoid appendage possesses, and what a wonderful power it has in compressing the pleura and lungs, and also in protruding externally, and thereby increasing the volume of the the thorax, in the direction of the sternum. All ventriloquists complain of the pain attending their efforts, and this could not proceed from the exertions of the larynx alone for the uneasiness is not there, but in the chest and it can be accounted for by the action of the muscles of expiration, and the diaphragm, and sometimes from the pressure of the ensiform cartilage, which is attached to the breast bone, and united to the midriff; and moreover the uneasiness is enhanced by the retention of air in the bronchial cells. This last circumstance can be proved by the appearance of the face.

I think it strange, that the muscles of respiration have not hitherto been considered, in explaining ventriloquism, and that the quantity and force of the air which depends so much on the diaphragm, have not been objects of attention. Surgeons all know, that in case of a fracture of the ribs of the thorax, respiration is carried on solely by the midriff, and this proves its use in the animal economy. I deny the position of Good and others that this art can be perfected in all persons by practice; although most men possess the faculty of attaining to some progress in the art. That circumstances and not experimental efforts have in some instances convinced a man of his possession of the faculty is sufficient to establish this assertion.

ART. IX.—*Meteorological Journal, kept at Marietta, Ohio, for the year* 1833, in Lat. 39° 25′ N., Lon. 4° 28′ W. of Washington City; by S. P. HILDRETH.

Months.	THERMOMETER.				Fair days.	Cloudy days.	*Rain and melted snow.*		Prevailing winds.	BAROMETER.				
	Mean temperature.	Maximum.	Minimum.	Range.			Inches.	100ths.		*Mean height.* Inches.	100ths.	Maximum.	Minimum.	Range.
January,	36.30	68	6	62	14	17	3	13	S. S. W. & S. E.	29	47	29.84	29.25	.59
February,	35.30	60	10	50	15	13	3	42	N W. & S. W.	29	48	29.75	29.30	.45
March,	39.70	72	4	68	24	7	1	92	N. N. E.—S. W. & S. E.	29	40	29.80	29.10	.70
April,	57.70	89	25	64	24	6	0	83	S. S. E. & E. N. W. N.	29	43	29.70	29.10	.60
May,	67.17	91	50	41	16	15	5	75	S. W. & S. E. N.	29	40	29.60	29.15	.45
June,	65.30	80	51	39	19	11	4	80	W. S. W. N. & N. E.	29	37	29.60	29.10	.50
July,	90.30	95	50	45	22	9	6	84	S. W. W. N. E. & S. E.	29	50	29.70	29.30	40
August,	69.53	91	44	47	29	2	0	25	N. E. E. & S. E.	29	47	29.60	28.18	.42
Sept.,	66.87	94	41	53	19	11	3	30	S. W. N. & N. W.	29	50	29.73	29.30	.43
October,	48.87	79	16	63	14	17	3	17	S. W. W. N. & N. W.	29	46	29.90	29.05	.85
Nov.,	42.70	63	16	47	14	16	3	88	W. S. W.—N. N. W.	29	58	29.88	29.10	.78
Dec.,	37.00	54	18	36	12	19	3	08	W. N. W. & E. N. E.	29	50	29.75	28.85	.90
Mean,	54.56				222	143	40	37						

Mean temperature for the year 54.56; being two degrees greater than that of the year 1832—and is the regular mean for this climate.

The fluctuations of the Barometer, have been small this year; the greatest range being only .90 of an inch. The lowest depression was on the 17th Dec., with the wind from the E. and snowing rapidly. The greatest elevation, was on the 31st of Oct., wind at the N.

The mean height for the year is 29.46. My Barometer is new, and I believe accurate.

Total amount of rain and melted snow, 40.37 inches, being eight inches less than that of the year 1832. Fair days, 222—cloudy days, 143, varying only six from that of last year.

The mean	temperature	for the	winter months is	26.20
"	"	"	spring months is	54.85
"	"	"	summer months is	75.04
"	"	"	autumnal months is	52.81

The winter being milder by two degrees than that of 1832; and the summer six degrees warmer. October was unusually cold, the thermometer being no less than nine degrees lower than that of April, when they are commonly of near the same temperature. It was probably owing to the early fall of snow in the Arctic and Rocky

Mountain regions, as wild geese were seen flying to the south as early as the 19th of the month. The winter thus far has been mild. No ice was formed in our rivers until the 29th Dec., when a little was seen floating in the Muskingum. The spring months were earlier than those of 1832, by about a week. Cultivated fruit trees brought forth abundantly, especially the apple, the crops of which were unusually fine, many thousand barrels having been sent to the market below.

The summer was mild and pleasant, and the crops of all kinds generally good. After September, the autumnal months were cold and very windy, blowing in strong gales from the western quarters for many days in succession. "Indian Summer," commonly a delightful season in the western states, barely made its appearance, before the setting in of hard frosts, destroyed the foliage of our forests, and put a stop to that slow and gradual change of color, which gives to our woodlands, those rich and varied tints, so much admired by the painter and the poet.

In November, occurred that beautiful meteoric display, of which a notice is subjoined. In December, there fell, within a few days, two feet of snow, which soon melted gradually away, so that at this time the earth is nearly bare. The whole year has been unusually healthy in this part of Ohio. Not a single death by cholera, that terrible scourge, which has visited and made sorrowful, so many places in the west, both above and below us, has occurred in Marietta. It may perhaps, be attributed to its naturally healthy location, to the wide, airy streets and commons; the cleanly and sober habits of the people, and to the great abundance of shade trees, which every where, deck our streets and door yards. It was observed, several years since, while the disease was yet confined to the eastern continent, that regions thickly covered with woods, and towns and villages in which grass plats and trees abounded, suffered much less, and in many instances not at all from the cholera. It may be philosophically accounted for, in the known property which the dense foliage of trees possesses, of decomposing the poison which generates miasmatic fevers, and with which the cholera was closely allied, from its prevailing mostly in districts subject to these diseases. Whatever may have been the cause, the inhabitants of Marietta, have great reason for gratitude and praise to that being who ruleth the destinies of man.

Marietta, Ohio, Jan. 1, 1834.

METEORIC PHENOMENON.

On the morning of the 13th. of November, from midnight to near sunrise, a period of more than six hours, the whole horizon, and the heavens to an immense height, were filled with fiery meteors or "shooting stars," as far as the eye could reach. They appeared to take their course from a point a little south east of the zenith, to every quarter of the compass, sloping at an angle of thirty or forty degrees, generally towards the earth; some however took nearly a horizontal direction. They were of all sizes, from that of a small point to three times the diameter of the planet Venus, and leaving a train of light in their course like that of a rocket. The larger ones threw a glare of light equal to that of a smart flash of lightning across the horizon, and left a luminous train, generally of a greater width than the diameter of the meteor, continuing for several minutes after their extinction, resembling a shining serpentine cloud of the class called "cirrus;" and retaining its brightness for many minutes, from three or four to fifteen; and appearing to curve and move some degrees upward, very slowly before they vanished. No noise or report could be heard from the largest, a proof of their great elevation although numbers were observed to scintillate and fly into numerous small sparks at the moment of extinction. The sky, at the time, was perfectly clear of clouds, with a brisk breeze from the S. W. The atmosphere had a yellowish tinge and was so very luminous as greatly to obscure the fixed stars. Fahrenheit's thermometer, stood at 36° and the barometer at 29.50 inches. The latter part of the previous night, it had rained profusely, and the twelfth, or day previous was fair and windy, bar. at 29·40 inches and ther. at 50° at noon. Meteors were falling like a shower of snow for a period of nearly six hours, or from twelve or one o'clock to past six A. M. Some few were seen as early as ten o'clock P. M. of the twelfth, from which time they gradually increased in numbers and in brilliancy until 4 o'clock: at which period they were in the greatest abundance. A phenomenon so rare, so brilliant and so sublime, could not fail to strike with wonder and with awe, its numerous beholders. Many could not believe it to be a natural phenomenon depending on the regular laws of nature, but supposed it was a miraculous occurrence intended to warn the inhabitants of the earth of some great and impending calamity. The most striking and interesting part of the display was the duration and singular shapes assumed by the luminous atmosphere after the explosion of the meteors; some appeared like a half circle, others like waves, or the undulating folds of a serpent occupying the space traversed by the meteor, and generally changing their shape and position, a little, during their continuance as if moved by the wind. Some of these luminous or phosphorescent clouds, occupied six or eight degrees in length and one or two in width, the color not fiery, but silvery, like moonlight on a thin transparent cloud. A very remarkable one was seen at about twenty minutes before six. It started from a point in the S. W. at an elevation of about 75° taking a N. W. course and exploded at 55° above the horizon, near to the right shoulder of the constellation of the "wagoner." It was three or four times the diameter of Venus, and left a luminous train occupying several degrees, in the shape of the human arm, half bent. It was distinctly seen for at least fifteen minutes. The larger meteors were more common from three to four o'clock, as I am informed by eye witnesses. I observed them myself, only from five to six o'clock, and until the morning light extinguished them, or rather I think they had nearly ceased at that time, as they were much less numerous at six o'clock, than at an hour or two earlier. From their great elevation, they must have been seen all over North America, and probably much farther. A gentleman who lives at Quincy, one hundred and sixty miles above the town of St. Louis, on the Mississippi, informed me, that he saw a few scattering meteors, between nine and ten o'clock, the evening of the twelfth, in E. and N. E. and as they were seen at about the same time at Marietta, and in New York, it is probable they commenced about that period and in that quarter of the heavens. Nearly all the observing spectators with whom I have conversed agree in this point, that the meteors appeared to start from a centre a little east of the zenith, taking a diverging course to every point in the compass; I saw none fall to the earth, or nearer, than to within ten or fifteen degrees of the horizon.

In the year 1799, and at nearly the same time in the year, viz. the twelfth, instead of the thirteenth of November, the same wonderful display of meteors was observed by Humboldt and Bonpland at Cumana, in central America. The description, as given by these celebrated travelers, cannot fail to be interesting. "Towards morning, a very extraordinary display of luminous meteors, was observed in the east by M. Bonpland, who had risen to enjoy the freshness of the air in the gallery. Thousands of fire balls and falling stars succeeded each other during four hours, having a direction from north to south and filling a space of the sky extending from the true east, thirty degrees on either side. They rose above the horizon at E. N. E. and at E. described arcs of various sizes and fell towards the south, some attaining a height of forty degrees, and all exceeding twenty five or thirty. No trace of clouds was to be seen and a very light easterly wind blew, in the lower regions of the atmosphere. All the meteors left luminous trains, from five to ten degrees in length, the phosphorescence of which lasted seven or eight seconds. The fire balls seemed to explode, but the largest disappeared without scintillation, and many of the falling stars had a distinct nucleus, as large as the disk of Jupiter, from which sparks were emitted. The light occasioned by them was white; an effect which must be attributed to the absence of vapors. Stars of the first magnitude having within the tropics a much paler hue, at their rising, than in Europe. As the inhabitants of Cumana, leave their houses before four o'clock to attend the first morning mass, most of them were witnesses of this phenomenon, which gradually ceased soon after, although some were seen a quarter of an hour before sunrise." He found they had been seen by various individuals, in places very remote from each other; and on his return to Europe, was astonished to find that they had been seen there also. They were seen from a point near the equator in Brazil, and in Longitude 70°, to Latitude 64° N. in Greenland; and as far east as Longitude 9°, near Weimar in Germany. Calculating from these facts it is manifest, that the height of the meteors was at least one thousand four hundred and nineteen miles, and as near Weimar, they were seen in the S. and S. W. while at Cumana, they were seen in the E. and N. E. we must conclude that they fell into the sea, between Africa and South America, to the west of the Cape de Verd Islands. From the foregoing description, the meteoric display of 1799, was not so vast, nor so sublime and brilliant as that of 1833.

ART. X.—*Abstract of Meteorological Observations, made at Mantanzas, Cuba, lat.* 23° 2′ *N., lon.* 81° 36′ W. *from Aug.* 1, 1832, *to July* 31, 1833; by A. MALLORY.

TO THE EDITOR.

Sir—Annexed you have some meteorological observations, made by myself at this place during the past year, which might prove of interest to some of your numerous readers, should they be deemed worthy of a place in your valuable Journal. The observations upon the weather, were carefully made, and the thermometer, an excellent instrument of Dolland's, was placed in a situation where it communicated freely with the external air.

After seeing, in your July No. of last year, a description of Mr. DeWitt's nine inch Conical Rain Gage, I sent to New York, hoping to obtain the instrument, but it was not to be found; you will therefore see that my observations are incomplete, in an important point.

I remain, Sir, with high respect, your obt. servt.

A. MALLORY.

Mantanzas, Aug. 24, 1833.

YEARS.	THERMOMETER.										WEATHER.				WINDS.			
1832 and 1833. / MONTHS.	Mean temperature at sunrise.	Mean temperature at 2 P. M.	Mean temperature at sunset.	Aggregate of mean temperature each month.	Maximum. Day.	Highest deg. of temperature.	Minimum. Day.	Lowest deg. of temperature.	Range of Thermometer.	Greatest range in one day.	Fair.	Cloudy.	Rainy.	Showers.	Trade winds.	North winds.	South winds.	Gales.
August, - -	79.25	86.50	83.00	83.00	28	90	7	76	14	13	15	16	12	2	30	0	1	0
September, - -	79.50	86 00	84.50	83.30	1,11	88	6	76	12	10	25	5	2	1	30	0	0	0
October, - -	77.50	83.25	80.75	80.50	1	88	26	71	17	9	11	20	12	5	25	5	1	0
November, - -	74.75	80.25	78.25	77.75	5,6	84	12	68	16	11	13	17	3	2	23	7	0	0
December, - -	72.00	80.00	78.00	76.67	3,9	84	20	64	20	16	24	7	2	3	25	6	0	0
January, - -	70.12	78.25	76.00	74.75	16,31	84	14	62	22	16	20	11	4	5	22	5	4	1
February, - -	70.67	79.30	77.30	75.67	19	84	5	66	18	14	18	10	6	2	24	2	2	2
March, - -	71.50	81.50	78.50	77.00	26	88	11	58	30	18	24	7	4	1	13	7	11	0
April, - -	77.00	86.30	83.00	82.00	8,15,16,17	90	26	71	19	14	26	4	0	3	20	0	10	0
May, - - -	76.50	85.50	82.30	81.50	30,31	90	3	70	20	14	21	10	9	2	21	1	9	0
June, - - -	79.25	88.00	83.75	83.75	12 days.	90	27,28	76	14	13	20	10	10	0	22	0	8	0
July, - - -	79.00	86.67	83.00	82.25	10	90	4	75	15	12	13	18	14	2	25	0	9	0
Agg. temperature,	75.60	83.40	80.70	79.90		90		58	32	18	230	135	78	28	280	33	52	3

From the foregoing, it will be seen that the mean temperature of twelve months past, was 79° 9′, or within a fraction of 80°. The coldest month was January, 74° 7′; the warmest, June, 83° 7′. The coldest day was March 11, when the mercury fell to 58°; the warmest, 90°, at several periods in April, May, June, July and August, range of mercury 32°, greatest range in one day 18°. In the beginning, I kept no column for variable winds. I think about twenty days should have been marked thus; the wind rarely blows an entire day from the west, but it did on the 18th of May. The south winds in the spring, are very dry, hot, and oppressive, and generally blow with considerable violence. It is a fact that the more open your house is kept during their prevalence, the higher will the mercury rise in the thermometer. 'The trades,' as will be observed, are the prevailing winds, and under their influence, the weather is never oppressive, although the mercury may be at 90°; they range from N. E. to E. We have the 'northers' from October until March; they are always cold, generally attended with rain in the beginning, usually last three days, and blow with considerable violence. They in fact bring the little rain we have during winter, beginning generally at S. W. they change in a couple of hours to W. N. W. & N. The rain commencing at W.

Art. XI.—*Physical Discovery.*—(*Retrospective.*)—*The Magnetic Needle made to indicate the true North, and rendered more steady, by a newly invented Magnetic process.*

Extracted and translated from the Avant-Coureur, of Monday, June 10, 1771.—No. 23; by Gen. H. A. S. Dearborn, and by him communicated for this Journal.

The actual declination of the Magnetic Needle, from the true north, as indicated by this false guide, amounts to nearly twenty degrees, and thereby deprives the common Mariner's Compass of the general and daily advantages of that instrument, which should be in fact a portable horologe, and indicate with exactness the meridian line. The author of the new edition and translation of Pliny the Naturalist, announced in one of the last numbers of the Avant-Coureur, has devised and executed a very curious magnetic process, which represses the north west deviation of the magnet, or rather, which compells nature, in one of her most mysterious laws, to overcome a kind of impotency in the magnetic needle, which, for a century has not allowed it, in the compass, to indicate the true north. This process consists in magnetizing two needles, one of which should be, but about half the size of the other. They are proved seperately, by placing them successively on a pivot, and causing them to gyrate for some time, until it is ascertained, by various experiments, that they are magnetized, and that one end of each of them, is directed to the north, at a certain deviation, which, for Paris and its environs, is known to be about twenty degrees. The cap of the small needle, is then taken out, which leaves a hole in its middle, and thus admits of its being placed on the cap of the large needle and pressed down to the latter. The cap therefore should not have the exterior form of common compass needles, but that of a small vertical cylinder, whose height should not exceed that of the needles generally used. The small needle is then slipped down upon this little cylinder, like a ring upon the finger, so as nearly to touch the large needle ; as for reasons, which are too long here to state, it is necessary to prevent the immediate contact of the two needles ; and to effect this, it is expedient to place a very thin leaf of copper between them. By this means, the two needles are suspended upon one and the same cap, but imperfectly crossed, in such a manner as to represent the cross of St. Andrew, or a pair of scissors more or less opened. When satisfied how great must be the separation, it is necessary to establish, the small needle must be secured in its position by a little sealing wax, or in any other manner, so that, as the combined needles turn upon the pivot, they be not deranged, by the air, a jar, or any other cause. As to the exact extent of the separation of the points of the needles, it is impossible to determine or adjust it, by an invariable theory, because this depends upon the size and relative magnetic powers of the two associated needles ; it is therefore, necessary to ascertain, for each set of associated needles, what the extent of this separation of the needles should be, according to their relative powers, which will require an experiment of a few minutes duration. To perform this, the needles are to be arranged on the cap, more or less crossed, and the distance between their points, is to be enlarged or diminished, until it is perceived that the large needle, in moving freely, at last is directed accurately to the true north. Now this is easily accomplished, after having tried and examined, for a minute or two, various distances, until the requisite degree of separation has been discovered.

It must not be forgotten, that the essential conditions of the operation require, that the two needles be crossed in such a manner, that the north point of the small needle, be directed to the left of the north point of the large one, between the *west* and the *north*, and the other one between the east and the south ; for if the north point of the small needle, from the nature of the separation, cross the large needle in two opposite directions, this erroneous disposition, augments the actual deviation of the magnet, instead of rectifying it.

This discovery, is not a mere speculation,—a simple project, but it is an invention that has been tested by experiments, which the author has actually made. We have had the satisfaction of seeing them made in our presence; and we, moreover, have in our cabinet, the first compass of this kind, which the author has constructed and confided to us. The explanation which he has given us, of his magnetic process, is equally curious, and satifactory.

The little needle, according to his theory, endeavors as much as it is enabled, and on its own account, to approach the north like all other magnetic needles, to within about twenty degrees. It is counteracted in this direction, by the large needle, which, of right, preoccupies that position, and takes possession of it; but as the effort of the small needle cannot be inconsequential, its rival, finds itself to be pushed a little farther, this impulsion acting by reason of the quantity of inherent power in the little needle.

The author cencludes that the deviation which is observable in the common compass, results from an actual defect in the magnet, that is, from a weakness in its directing power; and that it is from this defect in its power, that the common magnetic needle does not now point to the true north, as it did in 1666, whereas by associating with it, another needle, it is rendered, by this auxiliary combination, that nineteen twentieths degree of power which it wants at the present time to attain this object.

Such a discovery as this cannot but excite the curiosity of all the friends of the arts; and we consider it as calculated to extend the sphere of acquired knowledge, on the character and properties of the magnet, a complete union of which, according to some philosophers, should result in a theory of longitude. At all events, we can, with reason, flatter ourselves, (thanks to the invention which we have made public,) that the mariner's compass, is advancing towards perfection. In fact, the common magnetic needle has only one cause of direction, which is deranged by the least extraneous impulse, whereas the new needle, combined, as has been described, tends to a point, where it is fixed by two directive causes, which form an angle, and by which it is, in a manner, confined, which necessarily, renders it less unstable, less capricious, and less subject to variation.

Art. XII.—*On the Prairies of Alabama;* by W. W. McGuire, (in a letter to the Editor.*)

From the period of the first settlement of this State to the present time, the prairies have been objects of great curiosity and have attracted much attention; still, although the field for scientific investigation is so rich and interesting, no one has, to my knowledge, attempted a minute examination of it. The striking peculiarities of soil, of geological conformation and organic productions, especially in shells and other marine substances, which are found scattered indiscriminately over the prairies, are well adapted to attract attention and to excite investigation respecting the period and causes of the formation of the prairies and their fossils. Many who have never conceived of the possibility of any great change in the surface of the earth, except that produced by the deluge recorded in the pentateuch, attribute to that event, the present position of these shells. Others, taking a still narrower view, believe them to have been removed by the agency of man, from their native beds to the place where they are now found.

My own observations, although limited, have satisfied me, that the prairies once constituted the boundary of the Atlantic Ocean. In support of this opinion there are still existing many satisfactory proofs, although ages must have elapsed since these changes took place; strong evidence also exists that this great change has been effected by the elevating power of earthquakes, volcanoes and subterranean heat. The face of the country, from the mountains to the prairies, is rough and uneven, presenting an outline differing from all other hilly or broken countries which I have yet seen. It abounds in iron pyrites, and pebbles. Beds of good iron ore, of anthracite and bituminous coal, and of limestone and sandstone, are found in several places.

The country lying between the prairies and the sea coast is generally, if not altogether, of the same character as that on the coast from the Potomac to St. Mary's, viz.; level sandy plains, some fertile, some sterile, either dry or swampy and covered with pine, oak, cypress, cane, &c.; but it generally, perhaps uniformly, shows the distinctive peculiarities of the above named coast. The changes, in all places, are sudden and abrupt, changing from the peculiar soil and character of the prairies, to that of the coast, which is sterile, in some places almost pure silica; or of alluvial formations along the rivers, swamps and marshes, differing in fertility, according to the portions in which silica and vegetable matter are

* Dated a year since.

mixed in their composition. This tract of country is from one hundred to one hundred and thirty miles wide, perhaps more.

In speaking of the prairies, the rock formation claims particular attention. It is uniformly found below the prairy soil, at various depths, ranging from ten to fifteen feet and it sometimes projects above the ground. This rock, is generally known by the name of *rotten limestone;* when removed for several feet on the top, and exposed to the action of the atmosphere for some time, it assumes a beautiful white color. In its soft state it is easily quarried, and blocks of almost any dimensions can be procured. It has been dressed by planes and other instruments, and used in building chimneys; some of which have stood twelve or fifteen years without injury or decay. A summer's seasoning is requisite to fit it for building. This rock has been penetrated by boring to depths varying from one hundred to five hundred and fifty feet; after the first six or seven feet, it is of a bluish or grey color; but still soft, except in a few instances, where flint strata of a foot thick or more, have been met with. On perforating the rock, a full supply of good water, is always obtained, which uniformly flows over the top. I have heard of no constant running stream of water above this rock, except one in Pickens County, near the lower line. The superincumbent earth, is for a few feet, composed principally of stiff clay, of a whitish color; then comes the mould or soil, which is very black—in wet weather, it is extremely miry and stiff, and in dry, very hard and compact.

Shells, such as the oyster, muscle, periwinkle, and some other kinds, are found in great quantities throughout almost all the prairies of Alabama and Mississippi; the first named being most numerous, mixed in every proportion with the others. The oyster shells are perfectly similar to those now obtained, from the oyster-banks on the shores of the Atlantic. The largest beds of shells in the open prairies, seem to occupy rather elevated, but not the highest places. They have probably been removed from the more elevated situations by torrents of rain. It may be that the lowest places never contained any shells; or if they did, as vegetable matter accumulates in greater quantities in low situations, they may have been thus covered. In some instances, I believe they have been found in such places, several feet beneath the surface. They are not found in very large quantities in the timbered prairies; and indeed, so far as I have observed, wherever the shells are numerous, vegetation is not so luxuriant as where there is a proper admixture of the decomposed or decomposing shells and vegetable matter.

These shells and other decomposing materials appear to have given a peculiar character to the prairie soil, which causes it to adhere so strongly to the legs of horses and to the wheels of carriages as to remain several days in travelling, unless washed or beaten off. Yet when well *broken up* at the proper season, and regularly ploughed, it remains quite mellow, producing corn and cotton equal to the best alluvial bottoms, with, so far as it has been tried, increased fertility; although from the compact nature of the rock beneath and the tenacity with which it retains moisture, crops are sometimes injured by excessive rains, but seldom by drought.

There being no openings or fissures, except above the rock, by which to convey the water directly to the channels of creeks and rivers, there are consequently no reservoirs to contain snpplies for fountains and springs. In the winter and spring seasons, the streams overflow and the land is literally submerged. In the summer and autumn neither springs or wells are to be found, except below the rock; yet, notwithstanding this scarcity of water, there is seldom a want of moisture for the purposes of vegetation. And at times when the drought is such as to produce fissures two or three inches wide, and as many feet deep, the earth will be found quite moist at the depth of two or three inches.

As an evidence of the general moisture of the prairie soil, below the surface, it may be remarked, that crawfishes are so numerous in some situations as to prove very destructive to young corn, cotton and other tender plants. After nightfall, they issue from their holes or dens and commence their devastations. Their holes are of considerable depth, supposed to reach to the rock formation, a distance of from ten to fifteen feet; and on the surface of the ground, regular and well built mud walls, five or six inches high, are erected. The crawfish is of the crustaceous class, perhaps differing but slightly, except in size, from the sea lobster. Their nocturnal perigrinations show that they differ, at least in their habits, from the common crawfish found in our brooks, &c.

Much of the soil is sterile, presenting low hills on which there is no timber; in other places, a small and stinted growth, such as blackjack and post-oak. In some places there are considerable hills, having a thin stratum of excellent vegetable mould, covered with timber indicating a good soil; but from the close texture of the substratum, it is liable to be washed away, which has been the case in Washington and Clarke Counties. In those counties, I am informed the rock projects more than in any other part of the prairies, and there are cliffs fifteen or twenty feet high.

There are open prairies of every size from one hundred to one thousand or twelve hundred acres, mixed and interspersed in every form and mode with timbered land of all kinds; some producing only black-jack and post-oak, not exceeding fifteen or twenty feet in height; others again covered with the most majestic oak, poplar, elm, hickory, walnut, pecaun, hackberry, grapevine and cane, equal in size and beauty, I understand, to similar kinds in the Mississippi alluvions.

The extent of this country may not be unimportant. I am informed that traces of prairie soil may be seen in Georgia, perhaps as far east as Milledgeville. It is indeed said to exist in North Carolina; but of this I have not evidence such as to warrant the assertion. That it stretches nearly five hundred miles eastward from the vicinity of the Mississippi, on the west almost to Milledgeville, there is no doubt; and if it extends, as is said, to be the fact, to North Carolina, it reaches four hundred or five hundred miles farther; being perhaps nine hundred or one thousand miles long and from forty to sixty in breadth.*

That the prairies were once the boundary of the Atlantic is evident 1. From the fact, that on both sides, they exhibit the indented and irregular appearance of a coast, uniformly stretching up the large water courses; and in general, the sandy low country stretches in a corresponding degree up the rivers into the praries; but except where it is more or less alluvial, it is unlike the prairies. 2. They are nearly or quite parallel to the present shore. 3. The great quantities of sea shells, found scattered on so large a tract of country, very little of which is within one hundred miles of the coast, support the opinion now advanced. The idea of their having been carried thither by the action of winds or tides, is precluded by the fact that in that case they must have been raised three or four hundred feet and I presume in no place less than one hundred above the level of the Gulf of Mexico.

That the change was the effect of earthquakes, is evident from the appearance of the Mississippi. The "father of rivers" bears

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* This kind of country contiues to the vicinity of New York. The author, has not employed the usual terms of modern geological science, but there can be no doubt, that the region which he describes, is upper secondary or tertiary, or both. The bituminous coal, we presume, belongs to an older formation. Mr. McGuire, has sent us large and distinct equiseta with bituminous coal adhering. We presume, therefore, that the proper coal measures are *near at hand.—Ed.*

strong marks that long before the earthquakes of 1811-12, its course had been altered by some more powerful convulsion of nature; for its mighty current runs strongly against the seven bluffs below its junction with the Ohio, (except at St. Francisville,) seeming still to contend for its ancient channel. The prairies themselves, afford strong proof of this position; for, in many places, they present the appearance of having been *lifted up*, and they are, in fact, considerably higher than the surrounding country. Much of the country of which I am speaking, besides the prairies, has that peculiar undulating appearance which corresponds with the expansive heavings of earthquakes.

To this theory one objection, at least, may be raised. Why is it that aquatic remains are not found between the prairies and the ocean? It may be replied that the marine exuviæ in the low country have long since, been decomposed, while the shells in the prairies have remained, in some instances, entire, for the want of suitable agents to act upon them; indeed the prairies themselves illustrate this observation, for in all places where vegetable matter in considerable quantities has been brought to act, the shells are rapidly decomposing, or have nearly passed through this process, and the vegetables have in consequence attained a luxuriant growth. While on the other hand, in situations where the shells are found in nearly their original state, it is readily perceived that the mass of actually decomposing materials (except a partial influence of air and water) is in small proportion to the whole accumulation.

The prairies present a more lovely and fascinating prospect in the spring and summer, than the liveliest imagination can picture. They are then clothed in the richest livery of those seasons—

> "Plains immense, and interminable meads,
> Lie stretch'd before—where the wandering eye,
> Unfix'd, is in verdant ocean lost.
> Another flora there, of bolder hues,
> And richer sweets, beyond our garden's pride,
> Plays o'er the fields, and showers with sudden hand
> Exuberant spring."

Herds of cattle and flocks of sheep, are seen in the distance, cropping the fresh grass, or wandering at pleasure over the flowery region. Yet the absence of large trees, is amply repaid, by the rich garniture of grass, flowers and shrubbery. The odors of the wild rose, hawthorn, &c. load the summer's breeze with the most delicious perfumes. During the hottest and most sultry weather, when in other places, every thing is drooping and withering from excessive heat, a cool breeze is " ever on the wing." This is owing to the elevation, of the prairies and the absence of timber.

During my last visit to the prairies, I found a substance existing in considerable quantities, resembling the coral, or some of the zoophytic families. It is nearly as hard as flint rock. I collected several specimens, but have lost them. Some months back, I saw in the possession of a gentleman, several very interesting prairie specimens. They were said to be *Shark's teeth*, from an inch to an inch and a half in length, slender and very sharp. Among them are also pieces of the vertebræ of fishes. They were procured in a section of the prairies which I have never visited; which, abounding in specimens of the kind just mentioned, is the most interesting portion of this singular country.

It is a well established fact, that the earth and sea have undergone frequent and violent revolutions; and that the change which left the prairies dry is the most recent, is evident from the perfect state in which shells, &c. are now found, and from the fact that vegetation in many places, has made but slow progress. The nature of the soil indicates some ingredient adverse to many kinds of plants. But it is evidently fast changing, and it is not unlikely, that in the course of time, it will entirely lose its distinctive character and become perfectly amalgamated with vegetable matter. The process of decomposition and reproduction is rapidly going on in most places, and at every successive crop of plants, more matter is added, for the final accomplishment of the great change. It would be an interesting subject of inquiry, whether the woodlands are not gradually encroaching on the naked places; and if so, it would show at once that the prairies are, by natural operations, slowly losing their peculiarities.

Postscript.—A gentleman of Clarke County, Alabama, states, that on his plantation, are parts of the back bone of some large animal, from eight to ten inches long, and proportionally large in circumference—some still held together by the cartilaginous ligatures. Many of the early settlers used them instead of *andirons.* There is no canal for the spinal marrow. An early settler informed him, that he had seen an entire skeleton, on the surface of the earth; it was of enormous dimensions, longer as is reported, than the largest whale.

Remark.—It is exceedingly desirable that the animal remains described in this page should be collected and examined, and we trust that our intelligent correspondent, will not permit it to be neglected.—*Ed.*

Art. XIII.—*Circulation in Vegetables;* by Prof. E. Emmons.

The remarks in the following communication, are offered with some reluctance, as most of them are in opposition to the common opinions of the day. The subject too is one attended with difficulties, and investigations relating to it cannot result in the clearness and certainty which are so desirable in one of so much interest. The opinions advanced in this paper are not newly adopted or taken up recently or, suddenly, but have been deliberately formed and have been often expressed in my lectures on vegetable physiology. I confess however that they have not been formed so much from direct experiment as could be wished; they may therefore have little weight in the estimation of the public. But as the subject is interesting, suggestions of any kind may not be lost, as they may lead others more able than myself into a course of investigation, which, will result in discovering the true course of the *circulation in vegetables.* Writers on this subject seem to have desired to establish too much by analogy. They have studied the functions of the different organs in animals and those functions they have transferred to vegetables. Thus they make the leaves perform the functions of the lungs, and they discover also in vegetables a double circulation, notwithstanding there is not the vestige of a heart. There are doubtless analogies between the two kingdoms, bnt they are of a more general nature; such for instance as the following. The different orders of animals feed on different kinds of food, and we might infer what we already know, that different plants might require different kinds of soil. As climate has a controlling influence over animals, we might infer the same of vegetables. Different orders of animals are furnished with different kinds of apparatus for the circulation of the blood; so may the different orders of vegetables be thus diverse in the mode of circulating their fluids. Such are the analogies which it is safe to admit. But we may not compare the different parts of vegetables with those of animals and say that their functions are analogous, because the two kingdoms are not formed on the same general plan. I shall now proceed with the subject by noticing, in few words, the theory of Mr. Knight of England, as given in the Phil. Trans. for 1803. His theory may be stated concisely as follows, " water taken up by the roots of vegetables ascends principally through the central wood to the leaves, in which organs some of it is exhaled; the remaining sap passes into a different sytem of vessels which commence near the surface of the leaves and pass out through the mid rib into the liber. It now receives the name of *cambium.* In its progress downwards, it adds a new layer to the alburnum and also to the liber. These deposits constitute the annual ring, so conspicuous in the oak and fir. The object of the ascent of sap to the leaves is to effect changes in it, by exposing it to light air and oxygen. The ascending sap is considered analogous to the chyme and the descending to the chyle of animals. Mr. Knight adduces the following experiments in support of his views. 1. When he removed a ring of bark from the limb of a tree, he found the deposits of new matter to be made principally on the upper edge of the ring. He explains the result of the experiment by saying, that the descending sap or cambium could not reach the lower edge of the ring, in consequence of the removal of a portion of continuous matter. Mr. Knight's statement of the result of this experiment is certainly remarkable. He does not say that the deposite is *entirely* on the upper edge but *mostly.* From an examination of a great many trees which have been girdled, or which have lost large patches of bark, I am satisfied that there is a deposit at both edges of the ring. He admits that if a leaf is left growing near the lower edge, the thickness of the deposit is augmented. 2. If two parallel rings of bark are removed, leaving between them a leaf, it dies in consequence, it is said, of cutting off the supply of cambium from above. But is not the supply of sap equally cut off from the root; the leaf it is evident, is as much insulated from the supply below as from above. 3. If a branch be stripped of its leaves it dies; "the organs," as Mr. Knight would say "which elaborate the cambium are destroyed." But may it not be said that the power which assists in the elevation of the sap is removed, and that the limb dies of starvation. These are the experiments of Mr. Knight to prove that the cambium descends, and that the office of the leaves is to effect a change in the sap, they acting as media through which the sap is exposed to the light, atmosphere, &c. similar in fact to what takes place in the lungs of animals.

In the farther discussion of the subject it will be my object to show that the descent of the fluids in vegetables is unnecessary and never takes place in ordinary circumstances. I do not undertake to prove that the sap cannot descend at all, but that the sap in a vegetable deriving its nourishment from the roots only ascends, and that it is not a function of the leaf to elaborate the fluids, or to effect those changes in them, in order that they may be fitted for the nutriment of the vegetable.

1. It is evident that changes must take place in the fluids absorbed by the roots, to fit them for the nutriment of the plant. These changes commence, as soon as they are taken into the vegetable system: the most important change however is effected as they pass upward through the neck of the vegetable. This view of the subject is supported by the fact that the neck is the most important part of the plant. It is a vital part; divide a plant here and it is destroyed. The structure of this part appears different. But what seems to favor, still more, the opinion expressed above, is the change, which the fluids visibly undergo in passing through this part. To be convinced of this the reader may examine the *Asclepias Syriaca*, or milk weed. Divide the root below the neck, and the fluid which exudes is merely watery, but when the division is made at the neck, that peculiar milky fluid appears. In this case, we must admit that the fluid is changed as it passes upward, or as Mr. Knight supposes in the leaves and then passes down to the neck and there stops. If the latter supposition be true, would not accumulations of sap take place in this part. But this is not the case. 2. No power has been pointed out which can cause a descent of the sap in the uninjured vegetable, while the roots remain in the earth. It appears to me that it is unphilosophical to maintain that gravity is a principal cause of the descent of the cambium, as Mr. Knight has stated in the following passages in the Phil. Trans. for 1803. "These causes, (that is of the descent of the sap,) appear to be *gravitation*, motion *communicated* by the winds or *other agents*, *capillary attraction*, and probably something in the conformation of the vessels themselves which renders them capable of carrying the fluids in one direction rather than another. pp. 277-8. Again, when a tree is deprived of all motion by being trained to a wall, or when a large tree has been deprived of its branches, it becomes unhealthy and not unfrequently perishes, apparently from a stagnation of the descending fluids under the rigid cincture of the lifeless external bark. p. 282. Another cause of the descent of the sap to the root, I have supposed to be capillary attraction and something in the conformation of the vessels themselves; I however consider *gravitation* as the most extensive and active cause of motion of the descending fluids of trees. p. 283." But if gravitation be the active cause, &c., how can the vegetable overcome this power so as to raise the fluids at all? As

it regards CAPILLARY ATTRACTION, motion by winds, &c. I forbear to remark on them as causes of the descent of the sap. Of gravitation I would say, that we ought not assign it as an efficient cause of action or motion, where vitality exists. 3. The growth of stems can be effected where no leaves are developed. The vine of the potatoe growing in the dark or diffuse light of a cellar is almost destitute of leaves. It is a strong case to show that they are not required for assimilation. In all cases, the elongation of the branch and growth of the leaf take place simultaneously. 4. The evaporation of a large portion of the watery part of the sap so far inspissates it, that the descent becomes difficult. Gardeners know that watering delicate plants with barn yard water clogs the vessels and they die if the practice be long continued. If it be diluted with pure water, the practice is highly beneficial, showing us that the water is not poisonous. The phenomena of vegetable nutrition and assimilation may be explained without bringing in the agency of the leaves. The spongioles takes up water, more or less loaded with foreign matter, as carbon, common air, ammonia and other alkaline salts. As soon as the fluids come in contact with the vegetable structure they are more or less changed. In their progress upward, especially in herbaceous plants, they are still more changed and while passing through the neck. When the sap has arrived at the extreme branches, its assimilation is complete and a part of it is employed in effecting the extension of the branches or expansion of the leaves; the remainder is exhaled. That the fluids are changed, in their upward progress, before they have reached the leaves is shown by the fact, that the sap of the sugar maple is sweeter in the higher parts of the trunk than in the roots. Its specific gravity is also greater, and it is to be remembered that the maple is yet leafless. The elongation of the branches is finished in about three or four weeks, and the leafing is perfected in the same time. These two processes engross all the fluids and bring into action all the energies of the plant. Afterwards the sap is differently employed, viz. in forming the annual layer and in developing the buds. I have been led to consider the leaves as organs of exhalation, and not of assimilation. The power, namely, *vegetable irritability*, which causes the ascent of sap is a vital property, and like animal irritability often acts periodically. Thus after the long repose of winter, with how much energy do vegetables carry on their functions on the opening of spring. A few weeks only are required to transform a desert waste, into a beautiful garden. It is not improbable that the leaves may assist in the elevation of the sap. After their expansion, not a particle of sap will flow from the wounded vegetable. They assist in this process by the rapid evaporation from their surface. That they may accomplish the functions assigned them, they seem to be fitted to the various situations in which they grow. In tropical climates they are largely expanded, in cold regions they are narrow, contracted and rigid. The circulation of vegetables is not entirely suspended during the winter. As we may still see the buds gradually enlarging, especially in sheltered places. As the vegetable draws its nutriment from the bosom of the earth, beneath its ice-bound surface, we find it preserving an elevation of temperature above the circumambient air. I wish to present the above remarks rather as hints, than established conclusions; they are offered for the purpose of calling the attention of philosophers anew to the subject, that something more satisfactory may be taught than we find in the existing systems of vegetable Physiology.

Williams College, Dec. 12, 1833.

Art. XIV.—*Notice of a Galvanometer*; by Dr. John Locke.

TO THE EDITOR.

Dear Sir.—The galvanometer concerning which, I wrote to you last spring has been changed, again and again, until I have finally constructed an instrument with which I am satisfied. The essential part of it is a wire wound around a flat ring of boxwood. The outside diameter of this ring is 3.75 inches, and the inside diameter three inches. Its thickness is one fourth of an inch. The outside edges of it are cut or notched like the teeth of a clock wheel, in order to cause it to hold the spiral wire which is coiled around it in a single layer, the several parallel turns as close as possible without being in contact; except at the diameter of the ring, where an opening of the tenth of an inch is left for the introduction of a magnetized needle within the coil. This flat coil is put into the bottom of a cylindrical mahogany box turned in the shape of a snuff box, four and a half inches in diameter and one and a half deep, outside dimensions; the projecting ends of the wire being passed through, near the bottom of it. Immediately above this coil is the card or divided circle, which is attached to a thin wooden ring. The needle is suspended by a silk filament. It is made of a simple steel wire and is partly neutralized by being connected with a second and more delicate needle, made of the smallest species of main spring. This second needle is parallel to the principal one, three eights of an inch above it, and with its poles reversed in the manner of M. Nobili. It is intended to act chiefly as an INDEX and swings above the card. The two needles are firmly connected together by a brass wire. The whole is closed by a glass cover in the manner of a common compass, which it nearly resembles in external appearance. The glass is however pierced in its center by a hole half an inch in diameter, for the insertion of a perpendicular brass tube, four inches high. This tube includes the suspension filament which is fastened to the center of the cap the latter being closed at the top. The projecting wires ascending in an arch, and approaching within a quarter of an inch, terminate, the one in a five cent piece of silver, and the other in a corresponding disc of zinc. Three projecting brass feet, with each a levelling screw, support the instrument.

A wine glass of water, applied once to the discs, will deflect the needle from two to five degrees; by repeated applications the needle may be made to vibrate through sixty degrees. When the conjunctive wires of a calorimotor of one and half square feet of zinc, filled with *rain water*, are laid on the poles of this galvanometer, the needle is deflected to ninety degrees which is the maximum. If it be suffered to remain thus in action, it will retain the needle at about seventy degrees for twenty four hours; how much longer I have not yet determined.

The advantages which it seems to me, my instrument possesses are, that the "multiplying" coil being brought the nearest possible to the needle must affect it the most sensibly; being entirely enclosed, it is not disturbed by the agitation of the air; it is compact and elegant in its form.

Fig. 1. Is a plan of the ring R, and coil of wire W, half size.

Fig. 2. Is a section of the whole instrument. B. The box. R. The ring. W. The wire. N. S. The needle. *n. s.* The index. D. The dial cut out in the center to show the wires and to permit the needle to be introduced or withdrawn readily, through the opening O. C. The wire connecting the index and needle. G. The glass cover. T. The brass tube including the suspension filament F.

Fig. 3. The perspective view, in which the corresponding parts are marked with the same letters as in fig. 2. P. Is an ivory pin

by which to raise or lower the needle, by winding up the filament attached to it. Z. S. Is the battery immersed in a wine glass.

Cincinnati Female Academy, Dec. 23, 1833.

Fig. 2.

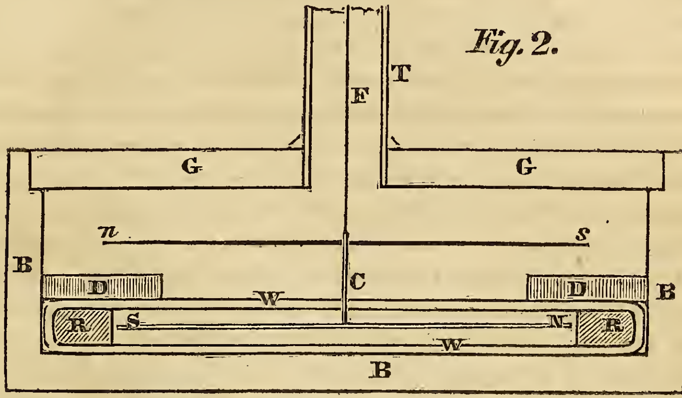


Fig. 1.

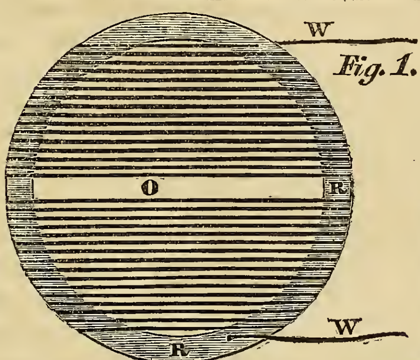
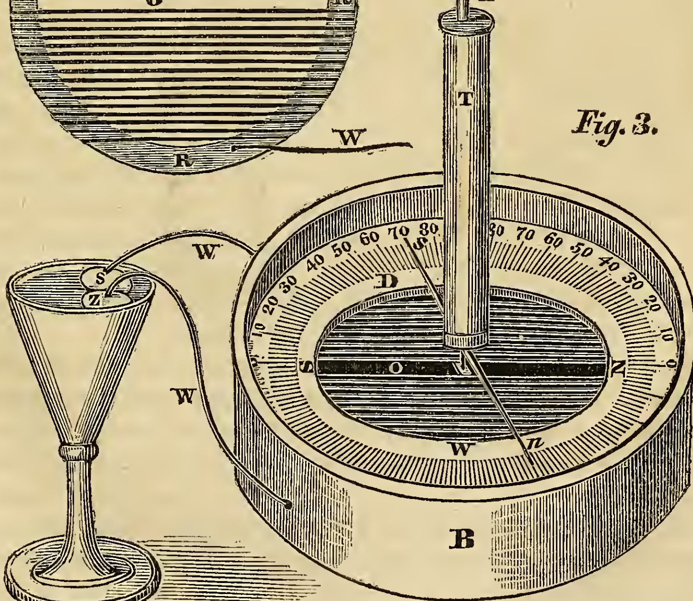


Fig. 3.

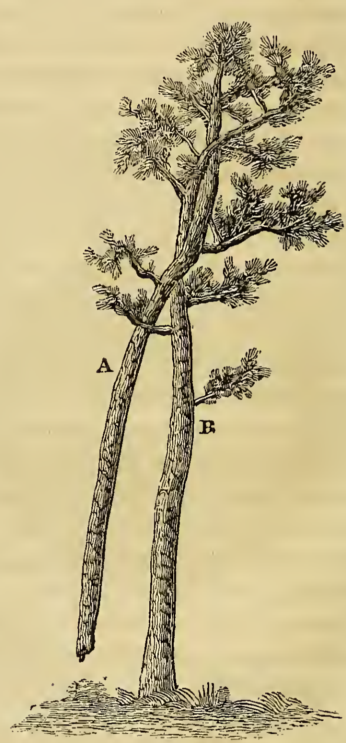


Art. XV.—*A Parasite Tree;* by Geo. W. Long, Lieut. 4th. Regt., U. S. Artillery.

TO THE EDITOR.

Sir.—I have recently, on a visit to Mr. Gee's plantation three miles south of Quincy, Gadsden county, in this territory, observed a natural curiosity, the following description of which may be interesting to you and many of the readers of the American Journal of Arts and Science.

It is a yellow pine tree bearing another in a perfectly healthful and flourishing state, like itself and those in the woods around them. The trees, as represented in this sketch, are united about thirty five feet from the ground, where they entwine around each other. The one that is borne, (marked A,) extends down, to within about two feet of the ground, and is alive and healthful to its lowest extremity.



These trees have been, in the condition in which they now are, for a period longer back than the first settlement of the country by the present population. They were pointed out by the Indians as a curiosity to the first Americans who came to Florida. The stump of the tree which is borne, has long since disappeared, and the place which it occupied, is now grown up in small bushes and grass.

Tallahasse, Florida, Oct. 26, 1833.

It would be desirable, to know the diameter of each of the trees, both near the ground, and at the point where they embrace.—*Ed.*

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

Appendix, continued from Vol. XXV. p. 146.

No. 130. Carex *Baldwinia*, Dewey.
Tab. T. fig. 61.
No. 37. Muh. Gram. C. *fulva?*

Spicis distinctis; staminifera solitaria cylindracea, bractea trinervi; spicis pistilliferis binis vel ternis tristigmaticis ovatis subrotundis, superioribus sessilibus, supernè saepe staminiferis, infima longo-exsertè pedunculata; fructibus ovatis triquetris subinflatis glabris nervosis longo-rostratis bidentatis, squama ovata acutiuscula subduplo longioribus.

Culm 1—2 feet high, leafy at the base, triquetrous, recurved; leaves linear, shorter than the culm, rough on the edge, purple at the base; staminate spike one, with a short three-nerved bract, with oblong obtuse scales; pistillate spikes two or three, upper two sessile, the third long pedunculate and projecting from the sheath, all with leafy bracts, ovate and roundish, densely flowered; stigmas three; fruit ovate, subtriquetrous, subinflated, long-beaked, nerved and two toothed, diverging or reflexed; pistillate scale ovate, obtuse or acutish, nearly half the length of the fruit.

Grows in S. Carolina, Georgia and Florida. It is No. 37 of Muh. Gram., and there called C. *fulva* with a *query*. It is not the C. *fulva* of Europe. It is not the C. *Elliottii*, as it has been thought to be, as both plants can now be compared. It differs from this, in its fruit and scale and general habit; C. *Elliottii* is a rougher plant, and has more the habit of C. *filiformis*, and is rush-like, while this species has the common habit of the Carices. It appears to be between C. *folliculata* and C. *flava*.

No. 131. C. *venusta*, Dewey.
Tab. T. fig. 62.

Spicis distinctis; staminifera solitaria cylindracea; spicis pistilliferis tristigmaticis ternis longo-cylindraceis pedunculatis sublaxifloris, infima longo-pedunculata et exsertè; fructibus ovato-conicis teretibus subtriquetris nervosis *scabro-pubescentibus* bidentatis, squama ovata obtusa paulo-duplo longioribus.

Culm 1—2 feet high, leafy at the base; leaves linear-lanceolate, much shorter than the culm; bracts leafy; staminate spike one, cylindric, an inch or two long, sometimes pistillate in the upper part, and rarely another short staminate spike near the upper pistillate spike; staminate scale oblong, *obtuse*, tawny on the edge and white on the keel; stigmas three; pistillate spikes three, long and cylindric, loose-flowered, lowest long-pedunculate, sometimes quite sparsely flowered; fruit ovate-conic, subtriquetrous, dark brown, distinctly and many-nerved, scabrous-pubescent, tapering to a two-toothed apex; pistillate scale ovate, short, *obtuse*, not half the length of the fruit.

This beautiful species is found from S. Carolina to Florida. I saw many specimens in the Herbarium collected by Dr. Baldwin, now in the possession of Dr. Schweinitz. It has some resemblance to C. *flexuosa*, but differs from that species in its fruit and scale and other particulars. It is not a variety of C. *flexuosa* growing at the south, as the southern plant of this name agrees well with the plant found in the Northern States.

Art. XVII.—*Internal Improvements of the State of Pennsylvania;* No. II. by Edward Miller, Civil Engineer.

In the last number of the Journal of Science, I gave a cursory view of the main line between the Delaware and Ohio; and it is my intention in the present to give a similar account of the Branch canals, so far as they are already finished, or far advanced towards completion. Some of these branches will probably be extended immediately, and others at a future day; but my object at present is only to show what Pennsylvania has already accomplished. Her energies and power are daily developing themselves, and it is not probable that she will stop in a career, which binds her citizens together by the strong links of mutual interest, and promises abundant increase to the general prosperity of the commonwealth.

The branches are six in number, and take their conventional names from the streams on the banks of which they are generally constructed. These are; the Delaware, Susquehannah, West Branch, North Branch, Beaver, and French Creek.

No. 1.—The Delaware division commences at Bristol, on the tide water of the River Delaware, nineteen miles above Philadelphia, crosses Pennsbury manor, the projection formed by the bend of the river at Bordentown, and reaching the western bank near Morrisville, follows it through New Hope and several less important towns, to Easton at the mouth of the River Lehigh, where it meets the canal of the Lehigh Coal and Navigation Company, having passed through Bucks and Northampton Counties.—The length of the division is 59¾ miles. The lockage counting from mid tide at Bristol 164 feet. There are 27 locks of which 23 are lift locks. They are of rubble masonry laid in cement and lined with plank; 11 feet wide and 90 long in the chambers. The canal is 40 feet wide at surface, 25 at bottom, and 5 feet deep.

No. 2.—The Susquehannah division is connected with the main line at Duncan's Island, and passes up the west side of the Susquehannah River to its forks at Northumberland, through Perry, Mifflin and Union Counties a distance of 39 miles. Lockage 86½ feet, by 11 locks of cut stone, 17 feet wide and 90 long in the chambers. At Northumberland the canal divides, and passes up both arms of the Susquehannah, forming the North Branch and West Branch divisions. The canal is 40 feet wide at surface, 28 at bottom, and 4 feet deep.

No. 3.—The North Branch division, keeps the north western bank of the river to Nanticoke Falls, where it crosses and continues its course through the valley of Wyoming, to its termination at the Lackawanna Creek in the anthracite coal region of Luzerne County; having also crossed Northumberland and Columbia Counties.—The length is 74 miles. Lockage 112 feet by 12 lift locks and one guard lock. They are built of wood, and are 17 by 90 feet in the chamber.

No. 4.—The West Branch division, lies in the Counties of Northumberland and Lycoming. It is on the east and north side of the river, and extends to the mouth of Bald Eagle Creek, a distance of 68¾ miles from Northumberland. Lockage 122 feet, by 18 lift locks, and 2 guard locks. They are built of dry rubble masonry, and lined with plank; and are 17 by 90 feet in the chamber.

No. 5.—The Beaver division, extends from the Ohio River at the mouth of Beaver Creek, to the town of Newcastle at the junction of the Mahoning and Shenango, and is wholly in Beaver County, except the northern extremity, which enters Mercer. It will form a part of the proposed connection between the main line and the Ohio Canal at Akron, and also between the main line and Lake Erie at the town of Erie, and is consequently a link of great importance. Its length is 24¾ miles. Lockage 132 feet by 17 locks, 2 of which are guard locks and 2 others guard and lift combined. They are of cut stone, 15 by 90 feet in the chamber.

No. 6.—The French Creek division, connects the Conneaut Lake with the Allegheny River at the mouth of French Creek, and passes through Crawford, Mercer and Venango. Length 45¼ miles. Lockage 128½ feet by 21 locks, of which 17 are lift locks. Dimensions 18 by 90 feet in the chamber, except the outlet lock at Franklin which is 22 by 120 feet.

The aggregate length of the Branch Canals is 311½ miles exclusive of all side cuts. The lockage is 745 feet; making, when added to the main line, 587 miles of canals, with 1923 feet of lockage.

The total length of canals and rail roads constructed by the state, is 706½ miles, overcoming 5339 feet of rise and fall.

Most of these works are completed, and it is confidently expected, that all will be finished at an early period next season. The limits which I have prescribed to myself, do not permit me at present to explain and account for the difficulties which have been encountered in their construction. They may be inferred from the fact, that the appropriations for the works which I have described have already exceeded $18,000,000.

I have now done with the dry details of compilation. When I commenced them, I expressed a belief that her sister states were hardly aware of what Pennsylvania has been doing. She has moved onward in her course, with that quiet determination which always marks extraordinary strength, and without appealing to foreign aid, has triumphed by the energy and resources of her own citizens. Nor have Pennsylvanians been content with those works alone which have been constructed by state authority. The Schuylkill, Union and Lehigh Canals, and the almost innumerable rail roads which traverse the eastern part of Pennsylvania, testify that the difficulties interposed by our rugged mountains, have only acted as incentives in the pursuit of wealth and power.

Art. XVIII.—*Notice of some New Electrical Instruments; invented by* Charles G. Page, Student in Medicine.

TO THE EDITOR.

Dear Sir.—I offer for your Journal a description of an electric syringe, which if it does not prove a useful instrument to the electrician, will perhaps afford him some amusement. A rough drawing of it will serve to explain it better than a bare description.

a, is a glass tube which may be from six to twelve inches long; from an inch to an inch and a half in diameter, and from one twelfth to one eighth of an inch thick. It is desirable to have a tube of symmetrical bore. *b*, is the piston rod of some conducting substance. *c*, is the plug which serves for the rubber. The plug is wound with silk covered with amalgam. If the bore of the tube be uneven as it is in my instrument, the rubber may be filled with springs so as to adapt itself throughout. This trouble is unnecessary where a good tube can be obtained. *d*, is a glass rod or what is better a piece of thoroughly baked cedar wood covered with shellac, at the end of which is a knob of cork, wood or metal, in which the collecting points are inserted in a radiating manner. As the piston passes up and down the tube, electricity is developed, and being collected by the points, is conducted by the chain to the hollow brass cap *e*, whence it may be received. The cap *e*, is perforated with a number of holes to allow the air which is driven before the piston to escape. The instrument when worked must be held in a vertical position to allow the chain to fall and coil itself in the brass cap. This is a very convenient substitute for the electrophorus and is indeed a portable electric machine. From its construction it is well secured from moisture and dust and its power is quite sufficient for trying a number of experiments. It is particularly convenient in the detonation of gases, fulminating compounds, &c. The one which I have affords sparks about an inch long. In this case, the electricity which is developed by the upward stroke of the piston is collected. But a constant supply of electricity might be obtained by having collectors on both sides of the plug: in which case the piston rod should be covered with shellac, and the electricity be conducted through the plug by means of a wire covered with glass or shellac until it reach the chain.*

There is another fact which I think worthy of communication. I discovered it accidentally about three years ago while I was witnessing the attraction and repulsion of a figure made of pith of cornstalk. While it was moving backwards and forwards, a spark passed from the prime conductor through it to my hand, producing a singular and

* This I contrived when at school five years ago.

beautiful illumination. After repeating it several times with this image, I prepared some pieces of a larger size by stripping off the rind carefully and drying them thoroughly. The effect of passing a spark through pieces six and eight inches long was truly beautiful. A splendid yellow light is produced, unlike any thing which I have ever seen in electrical illuminations. I have used always the same pieces which I prepared three years ago, and they still answer perfectly well; but as they absorb moisture so readily they must always be dried previous to use. I have lately prepared some pieces, and after drying them thoroughly, enclosed them in a glass tube secured at both ends by a plug of sealing wax. At each end of the pith a metallic button is fixed, connected with a wire which passes through the wax and terminating in the brass knob which is to receive the spark.

The following experiment also may be not entirely without use. I found that if a glass tube be coated with gum copal varnish its electric power is increased in a remarkable manner. When the varnish is very dry and smooth, if the tube be rubbed with amalgamated silk it throws off at each stroke long brushes and sparks. It occurred to me to varnish the cylinder of my machine. I tried the experiment several times and failed. These failures were owing probably to my impatience, in not suffering the varnish to harden sufficiently. This winter I was determined to give it a fair trial, and I have been amply repaid for a little patience. The result of the trial far exceeds my expectations. The machine works with astonishing power. In tolerable weather an eleven inch cylinder causes a ball of an inch and a half to act like a point. The varnish should be very smooth, and previous to use, it should be polished by turning the cylinder against a piece of soft silk. The rubber should be free from any projections which might scratch the varnish. The amalgam should be very soft and free from grit. There is one inconvenience attending this experiment, that is the necessity of cleaning the cylinder very often. If this could be obviated, the improvement would be complete.

Salem, Mass. March 4th, 1833.

Art. XIX.—*Communications by* David Thomas.

1. *Some account of the Chrysomela vitivora,* (with an engraving.)

In the spring of 1831, I first observed these brilliant insects creeping on my vines; and on a closer inspection, I found they were devouring the buds, eating out the central and more succulent parts. Some were advanced even half their lengths into the buds. The exigency required prompt measures; in our first attack, we probably killed a hundred; and we continued our examinations, picking up a few stragglers almost daily, till we had nearly destroyed the colony.

In the spring, they are commonly in pairs. When disturbed, they rather jump than fly, and remain where they fall for a time without motion.

On presenting this insect at that time, to a student in Entomology, he pronounced it a *Chrysomela;* and at my request furnished the following description. The specific name which he has proposed is indicative of its feeding and living on the vine *(Vitis.)*

Chrysomela. Generic character: Tarsi, with four joints; palpi six; antennæ moniliform, thickening towards the tip; thorax margined; body ovate or oblong, convex.

C. *vitivora.* Specific description: upper surface of the elytra, head, and thorax, of a deep greenish blue; the under surface of the elytra, the wings, the mouth, and the upper part of the abdomen, brown; the legs, antennæ, and the upper surface of the body, dark green; antennæ the length of the abdomen.

"Inquiries were made of several gentlemen versed in Entomology, whose impressions were that the insect had not before been described."

It was soon after observed in other parts of Cayuga County; a correspondent informed me that it had been seen near Philadelphia; and on my inquiring in a horticultural journal, if it had appeared in other places, Noyes Darling, Esquire made the following statement:

"For New Haven (Con.) and its vicinity, I answer, that this insect made its appearance last spring (1831) but *not* I believe for the first time. Its numbers however, were unusually great; and the injury which it has done to the vines, is wholly unexampled. Some vines were entirely despoiled of their fruit buds, so as to be rendered for the present season, barren. An Isabella that bore three hundred bunches the last year, is now destitute of fruit."

I had recollected that in the preceding season (1830) the vine leaves were infested by a "small chesnut colored smooth worm;" and in that year (1831) it again appeared on the leaves of my vines; and also on those which had been visited by the bug, in other places. I suspected that these were the *larvæ* of the C. *vitivora,* and we destroyed all that we could find. By pursuing this course, in the spring of 1832 and 1833, only a few bugs have been detected.

In order to settle the question, which had arisen, whether these worms were the *larvæ* of the C. *vitivora,* or not—in the summer of 1832 we put half a dozen of them into a tumbler, with moist earth, and vine leaves, covering it closely. The earth was first carefully examined to see that it harbored no other insect. Fresh leaves were supplied, from time to time; and as the worms became full grown, they passed, one by one, into the earth. After a fortnight or so, (for the time could have been exactly determined only by appropriating a glass to each worm), we found in the tumbler three perfect insects of *C. vitivora.*

This experiment, although not so complete as it might have been, seems to leave but little room to doubt the indentity of the *supposed* larva with this troublesome insect.

For the information of those who cultivate the vine, that they might be put on their guard against so dangerous an enemy,—most of the foregoing details were published from time to time, in different horticultural journals; and I have now collected and arranged them for the American Journal of Science, in order to inquire if this insect has appeared in more distant parts of the United States, and whether or not, it had been previously described and published?

2. *Remarks on the specific character of Corydalis formosa and Corydalis canadensis.*

Professor Eaton, in the last edition of his Manual of Botany, considers *C. canadensis* as "a division" of *C. formosa,* "founded on some slight difference produced by cultivation." I believe this opinion has also been adopted by some other botanists; and I therefore enclose drawings made from each plant while in a living state. I may add, that I have been acquainted with both for several years, having cultivated them in my garden, although the former is indigenous to this neighborhood; and so far, I have not had a doubt of their being *very distinct* species.

Valuable specific distinctions may sometimes be found in the roots of plants; and though such are rarely wanted, yet when doubts of the distinctness of two species arise, a resort to such characteristics may settle the question. Thus tubers are attached to the roots of *C. canadensis*, nearly globular, and so much resembling the grains of *yellow* maize which occur near the ends of the ears, that the popular name for this plant is "squirrel corn." *C. formosa*, on the contrary, has a scaly bulb; and a reference to the drawings will show them to be remarkably distinct.

In all our specimens of *C. canadensis*, there is only one leaf; but in *C. formosa* the leaves are numerous; the leaf-stalks, rising on all sides from the crown of the root, and which in this manner is gradually elongated. New shoots from the sides of the primary, also protrude, partially supported by their own roots; and in consequence, the plant is readily increased by division.

The difference in the shape of the leaf-stalks, is not less remarkable. In *C. canadensis* it is terete; in *C. formosa*, deeply channeled on the upper side, becoming greatly enlarged and even winged, near its base.

The segments of the leaves in *C. canadensis* are linear; but in *C. formosa*, these are "oblong and incisely pinnatifid."

The racemes are widely different, being compound in the latter, and simple in the former.

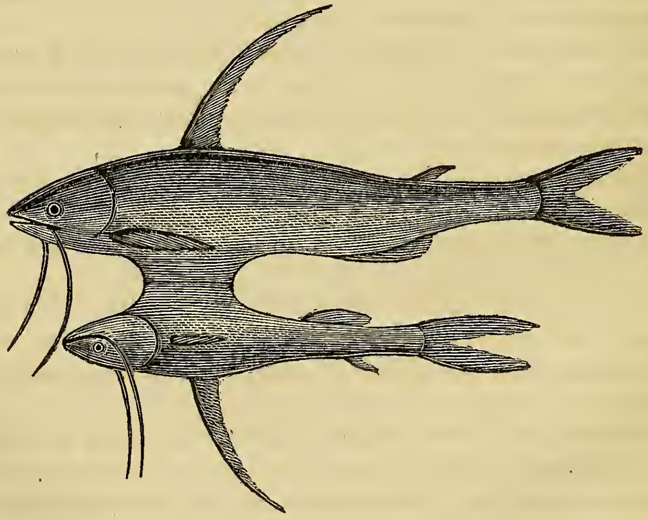
I know of no difference produced in these plants by cultivation. *C. canadensis* grows naturally in the richest soils; and instead of any enlargement of its parts, I have found it rather to deteriorate in the garden.

Another consideration might be conclusive with such botanists as have cultivated or closely observed the peculiar constitutional temperaments of plants. I am not aware that any treatment adopted by gardeners, has ever caused such as are exclusively *vernal*, to vegetate and blossom through the summer. All the showers of the finest growing season, have never been able in my garden to start a snowdrop or a crocus, a tulip or a hyacinth, from its dormant state. The law appears to be as irrevocable as the law of the seed which gives form to the future plant; and it may therefore, with great propriety, be taken as a part of the specific character. *C. canadensis* is strictly vernal. About the close of spring it disappears, leaving not a trace of verdure above ground; while *C. formosa* protrudes new leaves and flowers in continual succession till it is checked by autumnal frosts.

It appears then that these two plants are very dissimilar in their *roots*, in their *petioles*, in their *leaves*, in their *racemes*, and in their *temperaments;* and if these differences are permanent, which I have seen no reason to doubt, they must be very distinct species.

Art. XX.—*Notice of a Double fish*; by Syl. Churchill.

The annexed drawing represents a pair of cat-fish, (a species of Silurus? L.) which were taken alive in a shrimp net, at the mouth of Cape Fear river, near Fort Johnston, N. C., in August, 1833, and presented to Professor Silliman. One of them is three and a



half, the other two and a half inches long, including the tail,—the smallest, emaciated and of sickly appearance. They are connected in the manner of the Siamese twins, by the skin at the breast, which is marked by a dark streak, at the line of union. The texture and color otherwise, of this skin is the same as that of the belly. The mouth, viscera, &c., were entire and perfect in each fish, but, on withdrawing the entrails, through an incision made on one side of the abdomen, the connecting integument was found to be hollow, and nothing resisted a flexible probe in passing through from one to the other. This operation was performed with great care, with the tender and soft end of a spear of grass drawn from a green plant. But there was no appearance of the entrails of one, having come in contact with those of the other, for the integument was less than one tenth of an inch in its whole thickness, and in length from the body or trunk, of one fish to the other, it was three tenths, and in the water, when the largest fish was in its natural position, the small one could, by the length and pliancy of this skin, swim in nearly the same position. It was not ascertained whether, they were of different sexes, or of the same.

When these fish came into existence it is probable they were of almost equal size and strength, but one "born to better fortune," or exercising more ingenuity and industry, than the other, gained a trifling ascendency, which he improved to increase the disparity, and by pushing his extended mouth in advance of the other, seized the choicest and most of the food for himself. Yet though he probably hated the incumbrance of his companion, and wished the "marriage *tie* cut asunder," he afforded protection to his "weaker half," and could not *eat it* without *swallowing himself.*

Fort Johnston, N. C., Dec., 7, 1833.

Art. XXI.—*Observations on the Sexual Characters of the Animals belonging to Lamarck's family of Naiades*; by Jared P. Kirtland, M. D.

This interesting family, has of late, received an accession to its attractions, as well as numbers, by the discovery of many undescribed species, in different parts of the world, particularly in the rivers and lakes of the United States.

In consequence of the characters, which are employed for scientific arrangement, in the systems of conchology, being derived, exclusively from the shells, the animals seem in a measure, to have been disregarded. Their general anatomical structure is not well understood: much less the intricate, and minute conformation of their sexual organs. It is a disputed point, whether, they are androgynous, or whether, they possess distinct sexes.

Mr. Say, in the second No. of the American Conchology, under the article Anodonta, remarks, that "the principal naturalists and anatomists have been decidedly of the opinion that the animals of this family are hermaphrodites, but Mr. Prevost of Geneva affirms, that he observed in some individuals of the Unio pictorum, the existence of spermatic animalculæ, which he could not perceive in those, which contained eggs. He therefore, inferred, that the sexes were distinct. This led Blainville to a re-examination of the subject: he dissected about forty individuals of the genera Unio and Anodonta, without discovering any indications, that could lead him to suppose the existence of the male sex; still however he is in doubt, and we are much inclined to believe, with Ferussac, that Prevost may be right; but that more observations and more observers are required, fully to establish this disputed point, although Baer, has gone far towards even this subject.

"Treviranus, also made some interesting observations, on this subject, an account of which is published in the "Zeitsch. fur Physiol." in 1824. He was of the opinion, that the same organs produced both the ova and the fecundating fluid. He however remarks, that he found at the season of excluding their eggs, many that were entirely destitute of them."

Barnes, in the sixth Vol. of the Am. Journal of Science, p. 114, says that "They are generally supposed to be hermaphrodites *per se*." Prof. Rafinesque in his Monograph of the Fluviatile Bivalve Shells of the river Ohio, makes a concurrent statement.

In the course of the three last years, I have dissected many hundreds of them, and carefully observed their habits, under a variety of circumstances, until I am persuaded, that the sexes are distinct, and that each sex, possesses a peculiar organization of body, associated with a corresponding form of the shell, sufficiently well marked, to distinguish it, from the other.

The essential distinguishing mark of the females, discoverable in their internal structure, is the presence of ovaries, and oviducts, which may be seen, attached above to the branchiæ, and resting below, on the posterior* basal margin, at a point represented by (*a*,) in the annexed diagram. To enable these viscera, to perform their natural functions without interruption, this portion of the shell is invariably somewhat produced, and ventricose, more in some species than in others; and the posterior margin is generally truncated, and

* Posterior if we refer to the natural position of the animal.

inflated ; an arrangement calculated for their accommodation ; as at certain seasons, when teeming with ova, they not only occupy all vacant room, in the cavity of the shells, but press the valves apart, so as to render them dehiscent at that extremity.

In the males, there is of course no appearance of ovaries, and oviducts, and hence no necessity for an enlargement of the cavity, for their accommodation; accordingly, we find their shells more transverse, and less ventricose ; with the posterior basal, and posterior margins, compressed, and the latter more acutely produced.

This difference in the outlines of the shells of five distinct species can be seen, by referring to the annexed diagram. The figures A. are female, and B. male shells.

The evidence of the separate existence of the male sex, is in a degree negative at least so far as is shown by any examinations of the anatomy, that I have been able to make, being founded on the absence of ovaries, and oviducts ; but as their presence, or absence, is indicated, with certainty, by the form of the shell, is it not reasonable to conclude, that this difference in the form of the shell, indicates, with equal certainty, a distinctness in the sex?

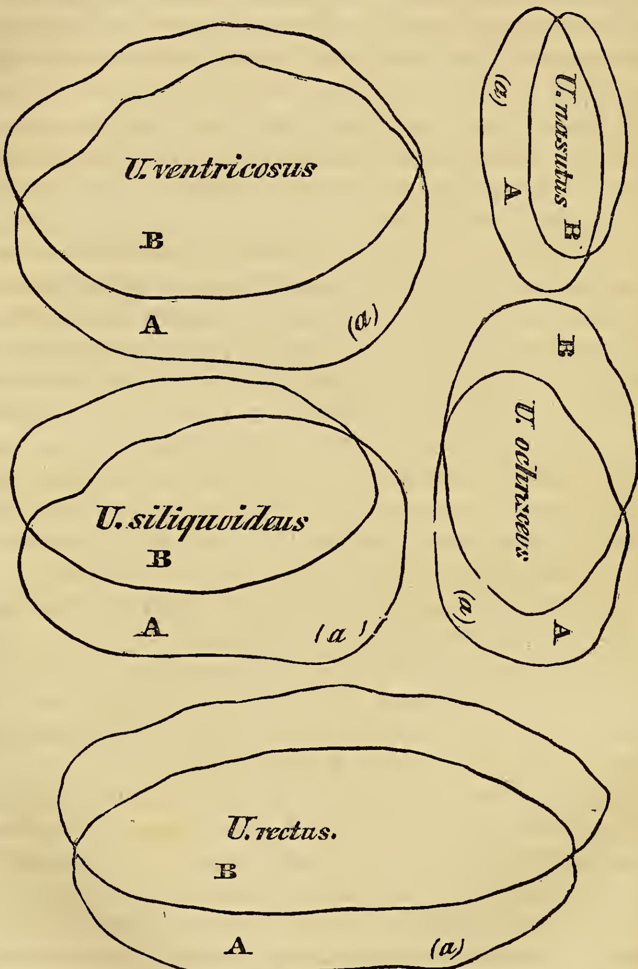
I have repeatedly tested the correctness of my conclusions with the Unio ovatus and nasutus of Say, occidens, sub-ovatus and multiradiatus of Lea, rectus of Lamarck, ventricosus and siliquoideus of Barnes, with the same satisfactory results, in every instance.

In my collection are several shells of the U. ochraceus of Say, from the Saratoga Lake, and the Schuylkill, the U. alatus of Say, from several different rivers west of the Alleghany mountains, and the U. æsopus of Green, from the Ohio, and I can distinguish with facility by their contours, to which sex they belong, notwithstanding I have never examined the animals of those species.

It is equally evident to me, that Barnes' figures *a.* and *b.* of the U. ventricosus, in the 6th. Vol. of the Am. Journal of Science, were drawn from female shells ; fig. *c.* from a male, the exterior figure of the U. prælongus from a female and the inner from a male ; that Lea's figures of the U. occidens and multiradiatus, in the Transactions of the American Philosophical society for 1830, and Say's figure of the U. ventricosus, in the fourth No. of the American Conchology, were all from female shells.

It will be found, on pursuing this subject that some which have been described, as distinct species, differ from others, only in sex. The U. formus of Lea, is probably the male, of the U. triangularis of Barnes; and the U. ridibundus of Say, the female of the U. sulcatus of Lea.

I have been essentially aided in my investigations of this matter by my friend Charles C. Cooke, M. D., of Youngstown.



Art. XXII.—*On the Sea and Land Rates of Chronometers;* by Parkinson & Frodsham.

(Republished in this Journal by request.*)

TO THE EDITOR OF THE NAUTICAL MAGAZINE.

Sir—An article in your Magazine for May, entitled "Magnetic experiments on Chronometers," by Messrs. Arnold and Dent, has appeared to us, and to several others of our distinguished contemporaries in the profession, to require some notice, as its effect may be, to lead persons unacquainted with the subject to form very erroneous notions as to the degree of perfection which the art of chronometer-making has reached. Indeed, if chronometers in general, as sent from the hands of respectable makers, were the imperfect sort of machines which those appear to have been on which your correspondents have experimented, they would ill deserve the confidence which nautical men have long seen reason to place in them.

We remark in the first place, on the striking change of rate which your correspondents represent as caused by the (we presume *careful*) removal of chronometers from London to Greenwich, amounting to as much as two seconds per diem, that it is not at all in accordance with our experience. We are constantly in the habit of taking chronometers from our house of business in town to our private resi-

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* 4 'Change Alley, London, December 20, 1833.

TO PROFESSOR SILLIMAN.

Dear Sir.—We respectfully take leave to enclose you a paper entitled Land and Sea rates of Chronometers, which we have thought it necessary to publish in answer to a paper which appeared in a periodical entitled Nautical Magazine, and contained to our ideas very erroneous opinions.

As it is a subject which we believe concerns every maritime nation and none more than your own—we have to request that you will do us the favor of inserting it in your valuable Scientific Journal. Our object in wishing to give this paper the most extended circulation, is, that we believe it might destroy the confidence of the Nautical man in one of the most important inventions for the benefit of Navigation; and we feel assured that many hundreds of your most intelligent navigators from their experience can bear witness that *our Chronometers* are not subject to the change of sea and land rates (which would render them useless for navigation) as stated to exist in the Chronometers of Messrs. Arnold & Dent.

We remain sir, your very obedient servants,
for Parkinson & Frodsham.
John Frodsham.

dence in the country, without finding their rates affected by the removal in any sensible degree—certainly not to any thing like the extent which Messrs. A. and D. represent *as common.*

After reading the letter of Messrs. A. and D., we took four chronometers, and after ascertaining their rates in 'Change Alley, we removed them to our private residence, about three miles from thence, and ascertained their rate of going there. We then brought them back to 'Change Alley, and again found their rates. After this, we had them carried around the Royal Exchange in the hand for about half an hour every morning, for several days; and after this trial we put them in an iron chest for the same period.

We subjoin the average rates during their various trials, from which it will be seen that no systematic variation took place which could be attributed to the peculiar circumstances under which the chronometers were acting.

Average Rate per Day during each of the Trials.

No. of Chron.	At 'Change Alley.	Private Residence.	'Change Alley again.	Round Royal Exchange.	Placed in Iron Chest.
	″ ‴	″ ‴	″ ‴	″ ‴	″ ‴
776	+1 0	+1 5	+1 3	+0 7	+1 0
1978	=5 0	=5 3	=5 2	=5 7	=5 2
745	=5 0	=5 1	=5 1	=5 8	=5 6
925	=1 6	=1 2	=1 4	=1 5	=1 8

We may further state on this subject, that, in August, 1833, we sent two of our chronometers, Nos. 1731, and 1751, to the Royal Observatory, Greenwich, on private trial. Their average rates for a short time previous were,

No. 1731 + 0″ 8‴ } per diem.
. . 1751 + 0″ 3‴ }

After their having been at the Observatory some time, the following were given to us as the rates they had kept there:

		″ ‴
1731	At Greenwich . .	+ 1 2
	At 'Change Alley	+ 0 8
	Difference in Rate	+ 0 4
1751	At Greenwich . .	+ 0 7
	At 'Change Alley	+ 0 3
	Difference in Rate	+ 0 4

It is evident that these chronometers shew no remarkable change of rate, as resulting from their removal from London to Greenwich.

With respect to the difference between land and ship rates, on which much has been said and written within the last few years, we have also to state, that we have no evidence of the existence of any such difference in chronometers made by ourselves; and, from the many hundreds of our making which have long been in use in all climates, we cannot doubt that we should have been aware of the fact, if it had been a general one.

On this point, we have much satisfaction in referring to the following extract from Captain Sabine's work on the figure of the earth, page 391:

"An opinion has lately prevailed, that the change in the rate of chronometers on embarkation, which used to be considered as a consequence of the motion of the ship, is principally occasioned by the magnetic influence of the iron which she contains: and it has been assumed by some of the writers who have taken part in the recent discussions on the subject, that the effect so attributed is one of general experience. I believe, on the authority of others, rather than from my own observation, that a difference does sometimes, and even frequently take place between the land and sea rates of chronometers; but from whatever cause the irregularity may arise, I must regard its occurrence as an evidence of the inferiority of the particular chronometer, to the advanced state to which the art of their construction has attained: because amongst the many with which I have at different times been furnished by Messrs. Parkinson and Frodsham and which I have frequently transferred from the ship to the shore, for two or three weeks at a time, for the purpose of trial, I have never been able to discover any systematic variation whatsoever, consequent on their removal."

On the subject of the supposed magnetic influence of iron in a ship on the rates of chronometers, we may as well take the opportunity of quoting from the same distinguished authority.

Captain S. says, "With regard to the influence of the iron, as the cause of the irregularity, a more decisive evidence can scarcely be imagined of its not being practically discovered under the most favorable circumstances for its exhibition, than took place in the four chronometers of Messrs. Parkinson and Frodsham, of which I have given an account in the appendix to Captain Parry's Voyage of discovery in 1819 and 1820, pages 7, 12, 18, 19 and 20."

Having succinctly mentioned the result, Captain S. adds, "These particulars are stated in detail in the pages referred to, but the circumstance is thus again generally noticed, because it appears to have been overlooked by many whose ingenuity has been exerted in *devi-*

sing contrivances to remedy an evil which has no practical existence where the *common* discretion of life is exercised in obtaining the better article at an equal price.

"Had the especial purpose of the Hecla's voyage been to inquire whether the iron of a ship, in its ordinary distribution, would, under extreme circumstances, exert a sensible influence on the chronometer, better adapted arrangements could scarcely have been devised for the experiment, nor could a more decisive result in the negative have been obtained.

"The Hecla was stationary and immovable, being, frozen up, for more than ten months in the vicinity of the magnetic pole, the dip being between eighty eight and eighty nine degrees: such is the situation, and such the circumstances, which are supposed to be best adapted for the developement of magnetism in the stancheons, and other vertical iron of a ship. The chronometers were kept on board the whole winter, and their rates, preparatory to the polar navigation of the following summer, were assigned from the average of the four months immediately preceding her extrication from the ice, at an equal period of four months of navigation. The Hecla arrived at Leith, having experienced much bad weather in crossing the Atlantic but on comparing the four chronometers at the Observatory at Leith, their Greenwich time, employing the winter harbor rates, proved less than two seconds in error.

"On the arrival of the Hecla in the Thames, the chronometers were returned to Messrs. P. and F.'s house in London, when, after a month's interval, they were found to be still going at the same rate as in the Hecla whilst in the harbor of Melville Island."

Attention was first, we believe, drawn formally to the supposed alteration of the rates of chronometers on their removal on ship board by the Rev. Mr. Fisher, who found that the rates of those in his charge were uniformly accelerated under such circumstances; and he assigned as the cause, the magnetic effect of the iron, to which Captain Sabine, in the above has so pointedly adverted.

Many persons well acquainted with the subject were of opinion at the time of the publication of Mr. Fisher's memoir, that from the obvious inferiority of the chronometers which he used, no authoritative inference could be drawn from any anomalies which their rates might exhibit,—an opinion in which we fully concur.

One of these chronometers had a rate on board of 3″ 4‴; but, on its removal to the Observatory, its rate was found to be 18″ 2‴; and on being taken back again to the ship, the rate was found to be 6″ 5‴; shewing a change in the ship-rate of 3″ 1‴.

Another of them, by a different maker, lost about 9″ by its removal from the vessel to the shore; and a third, by another maker, still more. The variation in the shore rates is also remarkable, that of the first appearing to have been 8″; of the second 6″; of the third, 7″ 2‴; and of a fourth, 8″. And all these errors were noted in the short period of seventeen days.

From the performance of such chronometers, it would surely be unsafe to draw positive inferences as to the effects of any generally operating causes on their rates of going.

Mr. W. C. Bond, of Boston, in America, a gentleman well qualified for the task, has taken much pains to ascertain whether there is any regular and systematic tendency in chronometers to change their rates when put on ship-board; and, in a paper published in the Transactions of the American Philosophical Society, he has given the result of experiments on a great number of chronometers.

When he had opportunity, he found the rate before the chronometer was sent to sea, and its rate after its return, and he took the mean for the shore rate; and, dividing the change in the error which had occurred while the chronometer was at sea by the number of days, he obtained the sea rate.

The following extracts are the results from eighty seven chronometers made by us, with their different numbers, and whose rates previous to the chronometers being placed on ship-board were accurately determined, and the same after their return from the voyage:

No. of Chron.	Difference of Rate.	No. of Days at Sea.	No. of Chron.	Difference of Rate.	No. of Days at Sea.	No. of Chron.	Difference of Rate.	No. of Days at Sea.	No. of Chron.	Difference of Rate.	No. of Days at Sea.
875	+0.4	96	752	+0.7	59	1100	+0.5	48	1146	−0.4	47
928	0.0	56	549	−0.1	388	832	+0.3	66	1179	0.0	55
427	−0.6	167	1035	+0.9	. . .	1047	−0.3	231	1100	0.0	55
920	+0.2	153	905	−0.6	54	1142	+0.3	58	599	−0.8	56
966	−0.3	46	871	−0.4	200	1184	−0.1	29	508	+0.1	677
873	+0.8	53	1061	−1.0	61	981	−0.5	95	981	+0.2	146
981	+0.4	55	1090	+0.1	109	1170	+0.1	36	521	+0.3	74
1030	−0.7	64	919	+0.1	47	1006	−1.0	37	947	−1.0	254
920	+0.7	157	981	+0.3	113	613	−0.5	176	599	+0 1	84
534	−0.1	63	125	−0.2	Pacif.	430	−0.1	313	873	0.0	95
871	+0.2	186			Ocen.	871	+0.7	89	1053	0.0	61
919	−0.5	. . .	. . . .	+0.0	. . .	866	−0.4	101	1246	+0.3	61
684	+0.3	. . .	727	+0.4	28	832	+0.5	114	966	−0.6	112
889	+0.1	. . .	1030	−0.7	57	1122	−0.9	95	1184	+0.2	68
1056	−0.4	52	549	−0.6	169	. . . .	0.0	53	1068	+0.2	60
669	−0.5	294	669	+0.5	45	609	+0.3	220	901	0.0	50
704	+0.3	62	. . . .	−0 7	187	966	−0.3	120	1210	+0.3	47
832	+0.7	59	. . . .	−0.3	338	1091	−0.6	77	1242	−0.7	47
1091	+0.6	50	. . . .	−0.5	294	873	0.0	27	635	+0.3	102
1079	−0.8	50	. . . .	−1.0	143	1030	+0.3	177	1100	−0.2	57
891	−0.3	161	811	+0.5	136				1215	+0 7	57
928	−0.7	65	534	−0.6	26				1030	−0.3	202*
928	−0.7	90	1144	+0.5	44						

* 70 tons of iron on board.

Summary.

	No. of Chron.	No. of Days at Sea.
Number of trials, ...	87	
Number wherein the average shore rate differed from the ship-board rate—and the difference, . .		
Number wherein the difference was one second, .	4	37 to 254
do. " " do. " $\frac{9}{10}$ of a second,	2	— to 95
do. " " do. " $\frac{8}{10}$ do. .	3	50 to 56
do. " " do. " $\frac{7}{10}$ do. .	11	47 to 157
do. " " do. " $\frac{6}{10}$ do. .	7	27 to 169
do. " " do. " $\frac{5}{10}$ do. .	10	44 to 294
do. " " do. " $\frac{4}{10}$ do. .	7	28 to 200
do. " " do. " $\frac{3}{10}$ do. .	17	46 to 338
do. " " do. " $\frac{2}{10}$ do. .	8	57 to 286
do. " " do. " $\frac{1}{10}$ do. .	9	25 to 677
Number wherein there was no difference whatever,	9	27 to 95

It is evident from these experiments, that there is no general tendency in these chronometers either to gain or lose at sea, on their land rates; as it appears from the above, that, out of eighty seven trials, thirty nine gained on their rates, and thirty nine lost on their rates: the remaining nine made no variation whatever.

Mr. Barlow, of the Royal Military Academy at Woolwich, whose attainments in science, skill as an experimenter, and discoveries on the laws of magnetism in particular, are known to all scientific men, took up the subject on the appearance of the Rev. Mr. Fisher's paper, and published the result of his inquiries in the Transactions of the Royal Society. He found, indeed, that chronometers were influenced by their *near proximity* to masses of iron; but instead of the rates of those which he tried being accelerated, five of the six which he used were retarded, and the acceleration of the sixth was doubtful.

We may be excused for stating, that the one least affected was made by us, and it was constructed on the same principle as the four mentioned above by Captain Sabine, and whose performance, under such extraordinary circumstances, was to us a subject of gratifying remark.

That a material effect on the going of a chronometer would be produced by applying a powerful magnet to it, we have no doubt, as the magnet would then operate as a disturbing force with all the advantage of proximity, but to infer from thence that the rate of a chronometer must necessarily be affected by its removal within the sphere of the ordinary magnetic influence existing in a ship, appears to us not more legitimate than to infer, that, because a chronometer will stop if put in the fire, it will necessarily go ill in the ordinary temperature of a sitting-room.

We are far from imagining, that, because so much has been done for the improvement of chronometers, there is *nothing* left to be desired; and we shall rejoice unfeignedly at any suggestion which may enable those who are engaged with the delicate task of constructing them, to arrive at this, and by more simple and certain means. Our object in writing to you on this occasion, is to convince those whom it chiefly concerns, that the errors, and causes of errors, on which your respectable correspondents have animadverted, cannot, in the present state of chronometrical science, have any appreciable effect in practice.

We are, Sir, your obedient servants,

Parkinson & Frodsham.

'Change Alley, November 14, 1833.

Art. XXIII.—*Necrology of* Count Chaptal; translated and communicated by *Dr. Lewis Feuchtwanger.*

Jean Antoine Chaptal, Count of Chantaloup, was born in the year 1756, at Nozaret, Dep. de la Lozère. After having finished his studies in the college of Rhodes, he went to Montpelier, for the purpose of learning the medical sciences under one of his uncles, at that time, professor of the school in that city; after receiving the degree of doctor, he went to Paris, to devote himself to chemistry, for which he had a particular disposition. He then obtained the newly created chair of chemistry in Montpelier, which, possessing the peculiar talents of an orator, extraordinary memory and all other requisites of a superior teacher, he filled with not uncommon success; and it was here, through his theoretical and practical pursuits, that he founded his great reputation. In the year 1790, he published his Elemens de Chimie in three volumes, which passed through three editions in French, and was translated into other languages. He established several chemical institutions, and among others that of Berard. General Washington, endeavored to induce him to emigrate to this country and three times sent him an invitation to that effect; the king of Spain also, did the same, offering a salary of three thousand six hundred francs, and an initiating donation of two hundred thousand, provided he would fix himself in his estates; the queen of Naples offered him likewise a refuge in her estates in the year 1793, when talent and fortune were dangerous acquisitions; but Chaptal preferring to serve his country, did not accept of either of these offers. He went to Paris, during the most dangerous time of the revolution and in connection with Berthollet and Monge, he conducted the laboratories for the manufacture of gunpowder from domestic materials. When a more brilliant star began to shine over France, the school of Montpellier was re-organized by Chaptal, and as teacher of chemistry at the new established Polytechnic school, he competed with Berthollet, Fourcroy, Guyton, Morveau and Vauquelin, in displaying the most important and beneficial zeal for the diffusion of science. Bonaparte, confided afterwards to Chaptal, the administration of public instruction, and then when he had discovered his intrinsic merits, he made him minister of the Interior. During the discharge of the functions of that office, he published his work "sur le perfectionnement des arts chimiques en France" and collected sufficient materials for his work "sur l'industrie francoise," and for his "Chimie appliquèe aux arts"; promoting at the same time, the most useful arts, and institutions, especially architecture and sanitary establishments. After four years, he took his leave of this station and Napoleon, as an acknowledgment for his distinguished merits, tendered him the order of the legion of honor, and a seat in the Senate, where he was several times elected President, and during the one hundred days when Napoleon was striving to recover his power, he was elected minister of State and director of commerce and manufactures. When, shortly afterwards he retired to private life, he again devoted his time to studies. In the beet manufacture, he spent a large portion of his fortune, and he fed a large number of animals, as for instance, twelve hundred merino sheep of the finest wool, with the residue of the beets. He increased, by his agronomical improvements, the value of his property, so much, that the nett proceeds which were fourteen thousand francs, amounted afterwards to sixty thousand. In 1819, Chaptal was created a Peer, he also was a member of the Academy of Sciences of Paris, and of the Royal Society of London, &c. &c. He lost in the last year of his life a very large portion of his fortune.

He died 30th of July, 1832. Besides eighty single different treatises in Chemistry, we are in possession of the following works:

Elemens de Chimie, 3 vols. in 8vo. 4 edit.

Traité sur la Salpêtre, 1 vol. in 8vo. 1796.

Essai sur le perfectionnement des arts chimiques en France, 1 vol. in 8vo. 1800.

Art de faire et gouverner les vins, l'eau de vie, vinaigre, &c. 2 vol. in 8vo. 2 edit. 1801 and 1811.

Essai sur le Blanchiment, 1 vol. in 8vo. 1801.

Chimie appliquée aux arts, 4 vol. in 8vo. 2 edit. 1803 and 1707.

Art de la teinture du coton rouge in 8vo. 1807.

Art du teinturier-degraisseur, 1 vol. 8vo. 1800.

De Industrie Françoise, 2 vol. in 8vo. 1819.

Memoire sur le sucre de betteraves, 3 edit. 1819.

Chimie appliquée à l'agrìculture, 2 vol. in 8vo. 2 edit. 1833 and 1829.

Art. XXIV.—*On the Elements of the Solar System;* by E. H. Burritt, A. M.

Upon receiving the Report of the Council of Astronomers* assembled in London, Nov., 1830, and which was first published there near the close of the last year, I set myself to re-compute the whole of the numerical values, pertaining to the solar system, so far as they depend upon, or are affected by the elements assumed and concurred in by that convention.

The pre-eminent qualifications of the men selected to draw up this report†, and the peculiar facilities they enjoyed for performing the

* In the year 1830, reference was made by the Lords Commissioners of the British Admiralty to the London Astronomical Society, to consider if any, and what, improvements could be made in the *Nautical Almanac:* Whereupon, the society appointed a council of forty members to consider and report upon the subjects involved in the reference.

† The sub-committee, appointed by the council, to draw up their report, was composed of *Sir James South, President, Sir John F. W. Herschel, Professor Airy, Professor Struve, Francis Baily, J. Pond, Rev. Dr. Robinson, C. Babbage, Capt. F. Beaufort, R. N.*, and *Lieut. W. S. Stratford, R. N.*

duties assigned to them, are the best warrant for the accuracy of their conclusions, which the case admits.

The semi-diameter of the sun, at the earth's mean distance, being settled at 16′ 0″.9, as determined by Bessel, from nearly 1700 transits, and its equatorial horizontal parallax at 8″.5776, as deduced by Professor Encke, the volume of the sun, compared with that of the earth, and the distances of all the planets from the sun, with their corresponding rates of motion, must be somewhat greater than they are usually represented. The diameters of the planets, at the mean distance of the earth from the sun, being assumed as in our table, it follows that their mass-values and volumes will be relatively affected in the proportion of their differences. The volume of Mercury, for example, will be comparatively less, while that of Saturn is considerably greater, than they were wont to be considered.

Taking the expression of these elements, as adopted by a tribunal the most competent of the age, to which the subject could be referred, I have made them the basis of the following computations. The *unit* of lineal measure employed in these calculations, of course is the equatorial diameter of the earth, estimated at 7924 English miles, according to Baily and Laplace.

The earth's mean distance from the sun *in miles*, then, will be 8″.5776 : 206264″.8 : : $\frac{7924}{2}$: 95,273,868.867748554. Having found this element, I proceeded next to construct logarithms to a great number of places, corresponding to the sidereal revolutions of the planets, and by means of these, determined their respective distances, retaining as many figures in the result, as seemed at all desirable. These results were verified in every instance, by independent modes of calculation, and the process by each was repeated, until the *terminal figures of the decimals corresponded.*

Logarithms to twelve places are appended for the convenience of testing the accuracy of the results, and to save the labor of construction when they may be wanted for other purposes.

N. B.—The diameters of the Asteroids not being given in the Report above mentioned, were derived from Biot's *Traité Elémentaire d'Astronomie Physique.*

New Britain, (Berlin,) Con., January, 1834.

Tabular Elements of the Solar System.

Names of the Planets.	Sidereal Revolutions in Mean Solar Days.	Logarithms.	True mean distances from the Sun in miles.	Logarithms of Planets' distances.	Logarithms of Planets' orbits.	Relative distances.	Logarithms.
Mercury,	87.9692580	1.944330,930294	36,880,422 34907587	7.566795,885867	8.364975,754225	0.387099031323	$\bar{1}$.587822,083445
Venus,	224.7007869	2.351604,479472	68,914,654.84245489	7.838311,585319	8.636491,453677	0.723332175563	$\bar{1}$.859337,782897
Earth,	365.2563744	2.562597,805126	95,273,868.86774855	7.978973,802422	8.777153,670780	1.000000000000	0.000000,000000
Mars,	686.9796458	2.836943,867587	145,168,094.89281471	8.161871,177396	8 960051,045754	1.523692662196	0 182897,374974
Vesta,	1325.7431000	3.122459,408208	225,016,762.14714600	8.352214,871143	9.150394,739501	2.361788860064	0.373241,068721
Juno,	1592.6608000	3.202123,345262	254,287,002.55155636	8.405324,162513	9.203504,030871	2.669010984581	0.426350,360091
Ceres,	1681.3931000	3.225669,267668	263,646,156.30912176	8.421021,444117	9.219201,312475	2.767245199994	0.442047,641695
Pallas,	1686.5388000	3.226996,342519	264,183,786.59075400	8.421906,160684	9.220086,029042	2.772888198314	0.442932,358262
Jupiter,	4332.5848212	3.636747,074052	495,533,836.87042950	8.695073,315039	9.493253,183397	5.201151614387	0.716099,512617
Saturn,	10759.2198174	4.031780,779676	908,717,975.06526816	8.958429,118755	9.756608,987113	9.537956061453	0.979455,316333
Herschel,	30686.8208296	4.486951,895561	1827,580,558.25199525	9.261876,529379	10.060056,397737	19.182390512469	1.282902,726957

Names of the Planets, &c.	Diameters at the Earth's M. Dist.	Relative Diameters.	True Diameters in miles.	Comparative Volumes.	Hourly Motion in miles.	Comparative Light and Heat.
Sun,	32′ 1″.8000	112.02434	887,681	1405844.16195		
Mercury,	6.4600	.37656	2,984	.05339	109,757	6.67363
Venus,	16.5000	.96181	7,621	.88974	80,293	1.91128
Earth,	17.1552	1.00000	7,924	1.00000	68,288	1.00 00
Mars,	8.1400	.53278	4,222	.15123	55,322	—2.32164
Vesta,	.5824	.03395	,269	.00004	44,435	—5.57805
Juno,	1.0158	.17580	1,393	.00543	41,799	—7.12362
Ceres,	3.4250	.19964	1,582	.00796	41,051	—7.65764
Pallas,	4.8740	.25429	2,025	.01669	41,009	—7.86800
Jupiter,	3 6.7400	10.88530	86,255	1289.81000	29,943	—27.05197
Saturn,	2 57.4400	10.34320	81,954	1106.54000	22.111	—90.97260
Herschel,	1 14.4000	4.33688	34,363	81.57020	15,592	—367.97400

Art. XXV.—*Observations on the Meteors of November 13th, 1833*; by Denison Olmsted, Professor of Mathematics and Natural Philosophy in Yale College.

(Continued from Vol. XXV. p. 411.)

Having in the former part of this article, recited the leading facts that have been ascertained respecting the meteors of Nov. 13th, 1833, we proceed, as was proposed, to review the principal facts respecting similar occurrences that have taken place at various other periods. In this sketch our limits compel us to use as much brevity as possible.

III. Of meteoric bodies seen at various periods.

With the hope of increasing our materials for arriving at correct conclusions on the subject before us, we have examined all the accounts within our reach of bodies that have at different times presented the appearance of fiery meteors.*

The accounts of single balls of fire, traversing the atmosphere with great velocity and projecting stony bodies to the earth, are very numerous and well attested. A synopsis of these facts may be found in various scientific works.† Although bodies of this class, or *Meteorites*, may occasionally present the same appearance as a "shooting star," yet the horizontal direction which they take, the explosions which accompany them, and the solid masses which fall from them to the earth, distinguish them from the class of bodies now under consideration. The chemical constitution of aërolites is well understood. They have generally yielded to chemical analysis a large proportion of silex and iron, with a smaller proportion of magnesia, nickel and sulphur, and occasionally, small portions of other substances, as lime, manganese, cobalt, chrome, &c. As we do not suppose the meteors of *Nov.* 13th to have been thus constituted, it will be more to our purpose to inquire what other kinds of meteors have occasionally made their appearance, and what other substances have fallen from the skies.

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* We regret not having been able to obtain access to several foreign publications which are supposed to contain more ample information on this subject; particularly, Gilbert's Annals, and the Works of Brandes and Chladni.

† See particularly, Ed. Encyclopedia, *in loc.*, Ed. Phil. Journal, III. 403.

1. *Showers of black dust* have occasionally fallen from the atmosphere. In the year 472, a great fall of black dust occurred at Constantinople, during which the heavens seemed to burn.*

A meteoric substance, resembling burnt paper, is said to have fallen in Courland, in 1686.†

2. *Showers of red dust*, likened to red snow, are repeatedly recorded as having fallen from the skies. In 1755, *November* 13, the sky was red and *red rain* fell in different countries; and in 1765, *November* 14, red rain fell in Picardy.‡

3. *Gelatinous* matter fell with a globe of fire, in the isle of Lethy in India, in 1718. A *viscous* matter fell in Lusatia, in 1796, along with a meteor. About the first of April 1826, the atmosphere being very clear and the sun shining brightly, a noise resembling rolling thunder, was heard at Saarbruck, and the environs. During the sound, a greyish object about three feet and a half in height, was seen in the air, rapidly approaching the earth, and then expanding itself like a sheet;|| there was then silence for about a minute; after which another sound resembling thunder was heard, as if it had originated at the place where the meteor fell. No residuum, or deposit was found.¶

4. Among the Chinese records of fiery meteors, mention is made of some that were consumed in the air, leaving trains or clouds of a *serpentine form*.§

5. "In *November*, 1825, about one hour after sunset, a meteor was seen in the town of Newton, Trumbull Co. Ohio, rapidly crossing the sky in a direction somewhat east of south, in size surpassing the full moon, but somewhat irregular in its form. At a certain point in its course, a portion of the meteor was seen to separate, and descend to the earth in an oblique direction, while the main body passed on, and soon disappeared. At the same time, and at the place where the meteor was seen to fall, two ladies who were walking in the road in the same direction in which the meteor was moving, found themselves suddenly enveloped in a mass of light. A ball of fire or light, several feet in diameter, seeming to come from above and behind fell upon them and the ground, breaking, as it struck into a thousand smaller balls, rolling upon the earth and breaking again

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* Ed. Phil. Jour. III. 403. † Ib. ‡ Ib.

|| See in the former part of this article, p. 391, an account of the appearance of a luminous object resembling in form, a *square table*, at Niagara Falls.

¶ *Chladni*, in London Quart. Jour. XXIV. 488. § Ib. VII. 383.

into still smaller balls, till it disappeared. Looking up, they saw the other portion of the meteor just as it disappeared in the distance before them. This phenomenon was attended with no noise or heat, and their clothing exhibited no traces of having been in contact with any foreign substance."*

6. The St. Petersburg Academical Gazette, contains the following account of an extraordinary phenomenon from a letter dated Moscow, May 2, 1832. "In March last, there fell in the fields of the village of Kourianof, thirteen versts from Wolokolamsk, a combustible substance of a yellowish color, at least two inches thick, and covering a superficies of between six and seven hundred square feet. The inhabitants, at first, thought it was snow, but on examination, it appeared to have the properties of cotton, having, on being torn, the same tenacity; but, on being put into a vessel full of water, it assumed the consistence of rosin. On being put to the fire, in its primitive state, it burnt and sent forth a flame like spirits of wine; but in its resinous state, it boiled on the fire, without becoming inflamed, probably because it was mixed with some portion of snow, from which it had been taken. After a more minute examination, the rosin had the color of amber, was elastic like indian rubber, and smelt like prepared oil, mixed with wax.†

7. "Soon after six o'clock in the morning of the 14*th November*, 1832, (says a letter from Bruneck, in the Tyrol) a broad stream of light suddenly descended from the center of the firmament nearly down to the ground, and was then drawn gradually up again to the middle of the sky, whence, for several seconds, it stretched itself out towards the north in a long train of light, which first appeared in a straight, and then changed into a wavy line; after this, it gathered into a light orb resembling a white cloud, and remained stationary in the center of the firmament for a full quarter of an hour, when it disappeared with the break of day. The appearance was accompanied with so vivid a degree of illumination that the smallest pebble in the road was readily distinguishable, and those who were abroad at the time, were completely panic struck. The sky, instead of being muddy with vapor, as is common at this season, and at this time in the morning, was clear and cloudless, and the air remarkably serene and tranquil. Between five and six o'clock, however, an unusual

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* E. N. Sill, Esq., of Cuyahoga Falls, (communicated to the writer.)

† Annual Register, 1832.

number of *falling stars* were observed in various parts of the heavens."*

The conclusion to be drawn from the foregoing facts is, that the substance of which fiery meteors are constituted is of various kinds, from the dense ferruginous matter of aërolites to that which is flocculent and of the texture of cotton, or to matter so attenuated as to be almost impalpable. The high degree of combustibility, attending some of these substances is to be particularly noted.

Meteoric phenomena, more or less resembling the one under review, have occurred at several periods before. The one most like the present, is that described by Humboldt, to which reference has already been made, in the former part of this article (p. 368.) The entire account as given in Humboldt's Personal Narrative, (vol. 3. pp. 331-346,) is worthy of being consulted not only for the interesting facts it contains, but for the opinions of so eminent a meteorologist. We shall have occasion to recur to this author hereafter.

In the month of April, 1803, a similar appearance presented itself in the United States, which is thus described in the Richmond (Virginia) Gazette of April 23d. "From one until three in the morning, those starry meteors seemed to fall from every point in the heavens, in such numbers, as to resemble a shower of sky rockets. The inhabitants happened at the same hour, to be called from their houses by the alarm bell, which was rung on account of a fire that broke out in the Armory, but which was speedily extinguished. Every one, therefore had an opportunity of witnessing a scene of nature, entirely novel, in this part of the globe, and which, probably, will never appear again. Several of these shooting meteors were accompanied with a train of fire, that illuminated the sky, for a considerable distance. One, in particular, appeared to fall from the zenith, of the apparent size of a ball eighteen inches in diameter, that lighted the whole hemisphere, for several seconds. A hissing noise, was plainly heard in the air, and several reports resembling the discharge of a pistol. Had not the city bell been ringing, the reports would probably have seemed much louder. The sky was remarkably clear and serene, and several of the largest meteors were observed to descend almost to the ground, before they exploded. Indeed, many of those, which we saw, appeared to approach within a few yards of

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* Annual Register, 1832.

the house tops, and then suddenly to vanish. Those which we particularly remarked, appeared to descend in an angle of sixty degrees with the horizon; but, as the smaller ones were so numerous, and crossed each other in different directions, it was only possible to ascertain with any degree of precision, only the paths of the largest and most brilliant."

The same exhibition was seen in the western part of Massachusetts, and probably in the intermediate country, although no records of the observations have reached us.

Remarkable exhibitions of shooting stars were seen in several parts of the earth at the corresponding time of the year 1832. On the night of *November* 19*th*, of that year, an extraordinary display of this kind occurred in England which is thus noticed in the English papers. At Portsmouth, "the heavens presented a very extraordinary appearance, shortly after midnight. Thousands of meteors were seen continually darting about in all directions, and the whole atmosphere was unusually illuminated. The driver of the night London coach, describes the effect as awful, and says it was with difficulty he could get his horses to face it. The same appearances seem to have been observed in various other places." The York Herald, speaking of the same night, says: "It was fine and moon light, when a series of fiery meteors were observed to flit across the heavens, with the rapidity and continuance of a regular discharge from a battery during a severe bombardment. They issued from the west, and in the first half hour of the phenomenon, twenty five of those balls of fire were counted, shooting along in terrific grandeur, and leaving a train of brilliant white to designate the course of their path. One of these balls had a very curious appearance, and seemed to drive a star before it."

In the Salem Register is an extract from the Journal of Capt. Hammond, giving an account of the shooting stars seen at *Mocha*, in the Red Sea, *November* 13*th*, 1832. "From one o'clock A. M. till after day light, there was a very unusual phenomenon in the heavens. It appeared like meteors bursting in every direction. The sky at the time was clear, the stars and moon bright, with streaks of light, and thin white clouds interspersed in the sky. On landing in the morning, I inquired of the Arabs if they had noticed the above; they said they had been observing it most of the night. I asked them if the like had ever appeared before: the oldest of them replied that it had not."

The following account of a "shower of fire" is taken from the New England Farmer (Boston) of May 1, 1833, in which work it is quoted from the *Medical Gazette.* "A singular phenomenon presented itself lately in some parts of France, particularly in the department of Orne, in the neighborhood of Argenton. Several times, and during two whole hours, the atmosphere, which was calm, became filled with an innumerable quantity of vivid sparks, forming a sort of shower of fire. The appearance was most striking between four and five o'clock in the morning. The same phenomenon was witnessed about Caen, where however, it excited less apprehension than at Argenton. It is said that in some places, the sparks was seen to alight upon the ground, but no traces of them have any where been found; and it is probable that the phenomenon really took place in the upper regions, the appearance of having descended being most likely an optical illusion."

A few detached facts may be added, which may prove of some importance in relation to the theory of shooting stars.

Mr. Brydone, the celebrated traveller, frequently observed shooting stars from the mountain of St. Bernard, one of the high Alps, and also several from the highest region of Mount Etna; and *they always appeared as high as when seen from the lowest grounds.**

Kirch, a German Astronomer, gives the following account of a fire ball seen at Leipsic in 1686. "On the 9th of July, O. S. at half an hour past one in the morning, a fire ball with a tail was observed in 8½ degrees of Aquarius, and 4° north, *which appeared immovable for half a quarter of an hour*, having a diameter nearly equal to half the moon's diameter.†

There are various records of meteors which left *luminous trains* that remained for some time after the bodies themselves had disappeared. One of the most remarkable of these was seen in England *November* 13*th*, 1803.‡ Even Virgil seems to have observed meteors of this kind, and alludes to them as prognostics of a high wind.

> Sæpe etiam stellas, vento impendente, videbis
> Præcipites cœlo labi, noctisque per umbram
> *Flammarum longos a tergo albescere tractus.*
> Georg. L. 1.365.

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* Nicholson's Jour. xx. 84. Brydone's Tour in Sicily.

† Quoted by Halley, Phil. trans. ab. vi. 110.

‡ Nicholson's Jour. vi. 279 and vii. 66. Phil. trans. 1742 in Dr. Young's Catalogue, p. 499. *Foster*, Nich. Jour. xxx. 132.

Sir Humphry Davy says, that falling stars are regarded in Great Britain as the fore-runner of a *westerly wind.**

Since the former part of this article was published, we have received accounts of the phenomenon as it was exhibited at Kingston (Jamaica) and in Mexico. At Kingston, the meteors are said to have radiated *from the zenith*,—an observation which would be of much value, could it be ascertained to have been made with entire accuracy; but we have before adverted to the errors which, from the difficulty of looking directly upwards, common observers are apt to commit in referring objects to the zenith.

From the description of the appearances as they were presented at Mexico, we infer that the exhibition had lost none of its magnificence and splendor when it traversed that country.

From *Professor Thomson*, formerly of the University of Nashville, Tenn., and from *Henry R. Schoolcraft, Esq.* of Michilimackinac, I have received communications, which are valuable, not only on account of the well known competency of the witnesses, but as relating to observations made at points remote from each other. Professor Thomson, remarks as follows:—

"Having been engaged in running the standard lines for the general survey of the Chickasaw Nation in Mississippi, I was at the house of Major Allen, on the night of the 'falling stars.' Major Allen is the government agent, and resides nearly in the center of the Nation. About an hour before daylight, I was called up to see the falling meteors. It was the most sublime and brilliant sight, I had ever witnessed. The largest of the falling bodies, appeared about the size of Jupiter or Venus, when brightest. Some persons present, affirmed that they heard a hissing noise on the fall of some of the largest. The sky presented the appearance of a shower of stars, which many thought were real stars, and omens of dreadful events.

"I noticed the appearance of a *radiating point*, which I conceived to be the vanishing point of straight lines as seen in perspective. *This point appeared to be stationary.* The meteors fell towards the earth at an angle of about 75°, with the horizon, moving from the east towards the west. There was not sufficient wind to account for the above inclination in the fall of the meteors.

"A surveyor's company, who were encamped in the neighborhood, stated that the fall of stars commenced about 9 o'clock, P. M. and continued all night, but that only a few were seen to fall early in the night. The position of the apparent radiating point, you will readily find on a celestial globe, by noting the latitude of this place,

* Encyc. Amer. v. 43.

34° 30′ N. and the time, viz. 5 o'clock, A. M. together with the angle of fall above mentioned."*

Mr. Schoolcraft writes thus:—

"Michilimackinac, Jan. 6, 1834.

"*To Prof. Olmsted.*—Sir—The meteoric display described in your letter of the 13th November, was observed, at the same time, on this island, and the adjacent shores of lake Huron. The appearances coincided, generally, with those you mention. The sentinels at post in the garrison, which is situated on a cliff, saw the lake illuminated, as it were, with falling stars. Indians, at point St. Ignace, describe the stars as falling into the water. Persons, at the fishing grounds, forty miles south, speak in admiration of the brilliancy of the meteoric phenomenon. Two persons encamped at Point Detour, forty five miles N. E. who passed the night in the open air, describe the scene as presenting an assemblage of dancing or shooting stars, which continued to be visible until day light. No similar scene is recollected to have occurred by white, or red men.

"It may be added, that the weather has been uncommonly mild, subsequent to this occurrence. This fact, was first noticed the latter part of November, when the existence of severe cold is expected. All December was characterized by a comparatively high range of the thermometer, with prevalent winds from the southward and westward. So striking were the effects, that maple sugar was made by the Indians, during that month. But little snow fell, until the first inst. Floating ice appeared in the lake on the 3d.; but the lake and harbor are still without any fixed body of ice.

"I am Sir, very respectfully your obedient servant,

"Henry R. Schoolcraft."

A statement made in a communication to the Boston Christian Register, of Jan. 25, 1834, deserves notice. It is as follows. "My first attention (says the writer) was to determine the centre or point from which the meteors started, which, from the place where I stood (Lat. 42° 45′ N.) appeared in the "Lion's Heart," near Regulus, but evidently within our atmosphere. There is one thing that I have not seen noticed by any that have written, and which could not have been noticed by me had I not kept my eye on the centre or point, from whence the meteors all shot forth, for a considerable time; and that was, an appearance of a star, less at first than the stars of the constellation by which it was surrounded; but it would increase un-

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* On the supposition that Prof. Thomson saw the radiating point *directly in the east*, 15° from the zenith, it would appear to have been in the southern extremity of Leo Minor, R. A. 153° 15′ Dec. 30° 40′.

til it was much larger than the stars, then totally disappear from ten to fifteen minutes, and then appear again; but the meteors shot forth in greater numbers in the interval between the appearances above mentioned."

A strong motive for making a digest of the leading facts respecting the meteors of November 13th, and of similar appearances that have occurred at other times, has been, the hope of affording facilities and materials for those who are better qualified than myself to give an explanation of these sublime but mysterious phenomena. The solution which men of science may give to the difficulties with which this subject is environed, will be looked for with impatience. In the mean time, I proceed, with much diffidence, to offer such views respecting the causes of the phenomena under consideration, as appear to me, in the present state of our knowledge, the most satisfactory.

IV. Explanation.

The principal questions involved in the present inquiry, are the following. Was the *origin* of the meteors within the atmosphere or beyond it? What was the *height* of this place above the surface of the earth? By what *force* were they drawn or impelled towards the earth? In what *directions* did they move? With what *velocity?* What was the cause of their *light and heat?* Of what *size* were the larger varieties? At what height above the earth did they disappear? What was the nature of the *luminous trains*, which sometimes remained behind? What *sort of bodies* were the meteors themselves—of *what kind of matter* constituted—and in what manner did they exist *before they fell to the earth?* Finally, what *relations* did the source from which they emanated sustain to our earth?

1. *The Meteors of November 13th, had their origin beyond the limits of our atmosphere.*

All bodies near the earth, including the atmosphere itself, have a common motion with the earth around its axis from west to east; but the *radiant point*, which indicated the position of the source from which the meteors emanated, followed the course of the stars from east to west: therefore, it was independent of the earth's rotation, and consequently at a great distance from it, and beyond the limits of the atmosphere.

It has been supposed that this westerly progress of the radiant point might be owing to the effects of *a strong current of wind*, in the upper regions of the atmosphere;* for, although the wind at the surface was at that time in the opposite direction, namely, from west to east, yet counter currents of wind are known sometimes to exist at different elevations. But it would be very remarkable that the progress of the wind westward should *exactly keep pace* with the revolution of the earth in the opposite direction; and it is, moreover, inconceivable that the wind should blow with such a velocity,—a velocity which, in our latitude, is nearly seven hundred and fifty miles, an hour, while the most violent hurricanes rarely exceed one hundred miles an hour.

It has also been supposed that the meteors appeared to radiate from a fixed point in the heavens, only because their lines of direction were parallel to the *magnetic meridian*, and their inclination in the direction of that meridian such as to make appear to converge towards the *pole of the dipping needle*.†

At the time when the attention of most of those who observed the phenomenon, was first directed to it, the position of the radiant point was in the same part of the heavens as the magnetic pole. But at half past five, the azimuth of the radiant point was, at New Haven, about 50 degrees, and at six it was 25 degrees, while that of the magnetic meridian is less than 5 degrees. But at 7 o'clock the same radiant point was nearly on the meridian: hence it could have had no fixed relation to the meridian of the place, as the magnetic pole has.

That the source of the meteors did not partake of the earth's rotation, but that it existed in space in such a manner that places lying westward of each other came successively under it by the diurnal revolution, may be inferred from the fact, that the phenomenon, at any given stage, as at the *maximum*, for example, *occurred nearly at the same hour of the night*, at places differing greatly in longitude. For, suppose that the meteors descended from the atmosphere like a shower of rain; and first, let the descent be at the same moment of absolute time. Then the occurrence would have been at an earlier hour of the night to places lying westward, by an hour for every 15 degrees of longitude. The maximum which was at 4 o'clock at New Haven, would have been at 3 o'clock in the western part of

* See Professor Caswell's observations in the Providence Journal.

† See Professor Hitchcock's remarks in the last No. of this Journal.

Ohio, and at half past 2 o'clock in Missouri and Louisiana; but in each case, it is said to have been about 4 o'clock.*

Secondly, suppose the meteoric shower to have commenced on the east, and to have had a progressive motion westward; then the simultaneous occurrence in different meridians can be accounted for, only on the supposition that such a motion was exactly equal to that of the diurnal revolution. We can hardly conceive of atmospheric bodies acquiring such a motion except in common with the wind; and we have already adverted to the improbability that the wind blew at the rate of seven hundred and fifty miles per hour.

Thirdly, suppose the meteoric shower to have commenced on the west, and to have had a progressive motion eastward; then the occurrence would have happened much later in the day to places lying eastward. For example, the shower is on the meridian in 87 degrees of west longitude, at 4 o'clock; at that moment it is 5 o'clock at New Haven; and before the shower reaches our meridian, it will be later still, by all the time it occupies in passing through 15 degrees of longitude.

We find it impossible, therefore, to account for the simultaneous occurrence of the meteoric shower at places differing so many degrees in longitude, on any other supposition than that the source or *cloud* (so to speak) was nearly stationary with respect to the earth, and beyond the influence of its rotation.

We are led by the same fact to infer that the cloud was either much larger from north to south than from east to west, or that it had a slow progressive motion in the former direction, either directly from north to south, or from north west to south east. Had it covered as great an extent of country from east to west as it did from north to south, an extent exceeding 30 degrees of latitude, the time of appearance on different meridians could not have been the same; and had the southerly progress of the cloud been otherwise than slow, the breadth being comparatively small from east to west, we cannot occount for the observed *duration* of the phenomenon, which, in its extreme limits, was about eight hours, namely, from 10 to 6 o'clock, although in most places, not more than three or four hours. Let us

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* No doubt a considerable range must be allowed for loose and indefinite statements respecting the time; but it seems hardly credible that the actual difference of time could have corresponded to the difference of longitude in places so remote, without being remarked.

then consider the consequences of a slow progressive motion in the cloud from north to south. Following the eastern limits, as the only one within our observation, we see that places lying on the same meridian, south of the place of the cloud at any moment, would pass to the eastward before the cloud would be vertical. This would cause the eastern line to extend in a direction from N. E. to S. W. Such we believe to have been the fact. In lat. 2° N. lon. 41° W. the phenomenon was not visible. In lat. 36° N. lon. 61° W. "an unusual number of meteors were seen, but comparatively few, not more than four or five in a minute." (See the last Vol. of the Journal, p. 399.) But, in this longitude, as far north as St. George's Banks, the display was as great as at New York. Capt. Parker also, in the Gulf of Mexico, actually saw the cloud advancing from N. E. to S. W.

An observation of Mr. Riddell at Worthington, Ohio, would seem, at first view, to be at variance with this fact. "It first occurred to me (says Mr. R.) to determine the location of the point from which the Meteors seemed to radiate, a little before 5 o'cleck. A gentleman present, happened to have a map of the constellation Leo, so that we had no difficulty in recording our observations pretty accurately. At 5 o'clock, the R. A. of this point, as since determined from a globe, was near 149°: its declination, 21° 45′. Twenty minutes later, the R. A. was 151°, dec. 21° 30′, nearly. From this time, until the meteoric exhibition was rendered invisible by the light of day, the center of radiation seemed to retain the same place in the heavens, moving westward with the fixed stars."*

From this observation, it appears that before it became stationary, the cloud or source of the meteors, had an absolute motion from N. W. to S. E. How then could it appear to Capt. Parker, to advance from N. E. to S. W.? Resolving the southeasterly motion into two motions, one directly south, the other directly east, had the latter portion been exactly equal to the diurnal motion, the cloud would have been seen by Capt. Parker, (who referred the motion to the terrestrial meridian,) to advance directly south; but had the easterly part of the motion been less than that of the earth, the cloud would have apparently advanced from N. E. to S. W. The latter supposition, namely, that the progress of the cloud was so slow that the easterly part of the motion was less than the diurnal, is probably the true one—

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* Letter to the writer, Dec. 21, 1833.

a fact which is indicated, indeed, by the observation itself; for, in twenty minutes, the radiant moved easterly two degrees in right ascension, while the earth in the same time must have moved five degrees. This easterly motion, moreover, must have considerably increased the duration of the phenomenon. Now had the cloud been near the earth, or within its atmosphere, its apparent as well as real progress, would have been from N. W. to S. E.; and this would have been the line of direction on the eastern limits, whereas that line appears to have been from N. E. to S. W.

2. *The height of the place whence the Meteors emanated, above the surface of the earth, was about* 2238 *miles.*

The R. Ascension and Declination of the radiant point, having been observed by Dr. Aiken, at Emmittsburg, (Md.) by Mr. Riddell, at Worthington, (Ohio,) and by myself at New Haven, we hoped to be able to obtain data for a satisfactory estimate of the height. But the observations present singular anomalies, especially in respect to the R. Ascension. As the observations were made without the use of instruments, none of them can be supposed to have been entirely accurate. Although, after making the observations, I had immediate recourse to a celestial globe of the best construction, and marked the spot with as much accuracy as I could, yet as the light of day was advancing at the time of making the observation, and as the lines of direction could not be traced back to the center of radiation, but only to the vicinity of that center, I cannot rely on my own observations as accurate, to less than half a degree. Mr. Riddell had the advantage of a celestial map before him at the time; but having no other aid to the eye, he can hardly be supposed to have been much more accurate, if at all. Dr. Aiken, resorted to a celestial globe, soon after observing the point in question, and marked its position with a pencil. He remarks, (in a letter recently received,) that its declination must have been at least 24° 30′, unless the globe which he used was inaccurate; and to enable me to judge of this, he gives the R. Ascension and Declination of several of the neighboring stars as indicated by the same globe. On comparing these elements with those given by one of Carey's best globes, I find the dec. of Gamma Leonis to be 45′ less than that stated by Dr. Aiken. Hence, deducting 45′ from 24° 30′, we have 23° 45′, as the true observed dec. at lat. 39° 40′. The dec. observed at Worthington, lat. 40° 4′, was 21° 30′, and at New Haven, lat. 41° 18′, it was 20°.*

* Not 21° as before stated by mistake. In the original article published on the same day of the occurrence, it was given at 20°.

Although these elements indicate a parallax in the right direction, yet there is some want of correspondence between the amount of parallax and the differences of latitude in the several cases. Between New Haven and Worthington, the parallax in Dec. is only 1° 30′, the difference of Lat. being 1° 14′, while between New Haven and Emmittsburg, the parallax is 3° 45′, the difference of Lat. being 1° 38.′

Owing to the want of correspondence between these elements, we shall form a different estimate of the distance, according as we derive our data from one pair or another of these observations. The apparent radiant being stationary, we may suppose the Dec. taken by each observer, at the moment when that point was on his meridian. The difference of Lat. will represent the distance corresponding to the parallax. Neglecting for the present all considerations of the earth's spherical figure, and of refraction, we may in a very simple manner, obtain an approximate expression for the distance as follows.

Let AB represent a horizontal line, and the angle BCD, the altitude of the radiant point, 74° 5′, as seen at Emmittsburg, in Lat. 39° 40′, C being the place of observation. Let A be the place of observation at New Haven, in Lat. 41° 18′; then the angle at D will represent the parallax in Dec., namely, 3° 45′, and the angle 74° 5′ − 3° 45′ = 70° 20′ = CAD; and the side AC = 1° 38′ = 112.7 miles.

Hence, Sin 3° 45′	- - - -	8.815599
Sin 74° 5′	- - - -	9.983022
112.7	- - - -	2.051924
		12.034946
1657	- - - - -	3.219347

That is, the observations at Emmittsburg, compared with those at New Haven, give the distance from New Haven to the source of the Meteors, *sixteen hundred and fifty seven miles.*

Fig. 1.

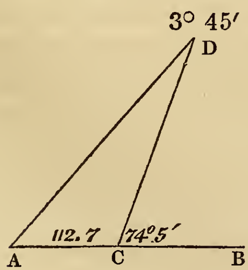
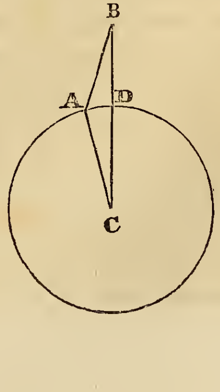


Fig. 2.



But applying to the same figure, the elements as deduced from the observations made at New Haven and Worthington, the calculation will be as follows.

BCD=71° 26′	Sin 1° 30′ - -	8.417919
ADC= 1° 30′	Sin 71° 26′ - -	9.976787
AC= 85.1	85.1 - -	1.929930
		11.906717
	3082 - -	3.488798

From this calculation we obtain for the distance from New Haven to the place of the Meteors, *three thousand eighty-two miles*, a result nearly twice as great as the former. I know of no way of accounting for this difference, but to impute it to want of exactness in observations made loosely with the naked eye, instead of being measured by instruments.

Referring the same estimates to the center of the earth, we obtain the following results.

AB=1657 AC=3956.·.AB+AC=5613		3.749195
AB−AC=2299		3.361539
	tan 10° 39′	9.274269
BAC=180°−21° 18′=158° 42′		
½ABC+½BCA= 10° 39′		12.635808
	tan 4° 25′	8.886613
ABC=15° 4′		
ACB= 6° 14′		

Sin 15° 4′	- - -	9.414878
Sin 21° 18′	- - -	9.560207
3956	- - -	3.597256
		13.157463
BC=5528	- - -	3.742585
DB=1572		

Hence, according to the observations made at Emmittsburg and New Haven, the *perpendicular height*, above the surface of the earth, of the place whence the meteors emanated, was *fifteen hundred and seventy-two miles.* But from the observations made at Worthington and New Haven, the perpendicular height, estimated in the same way, was 2955 miles.

We have at present no more satisfactory way of obtaining the perpendicular height of the place in question, than to take the mean of the two foregoing results, which gives 2263½ miles, as the approximate place of the radiant.

This estimate is entitled to greater confidence from the fact that according to the estimate of *Hon. S. DeWitt*, of Albany, (obligingly communicated to the writer,) the height estimated from the observations of Capt. Parker in the Gulf of Mexico, compared with those made at New Haven, is 2027 miles, differing less than one ninth part of the whole from the estimate made from the three other observations combined.

That this is an approximation to the truth, may be farther inferred from the correspondence of these estimates to one founded on the data of Prof. Thomson, (see p. 138,) which gives the perpendicular height above the earth's surface 2424 miles. Finally, taking the mean of all the foregoing estimates, we obtain 2238 miles, as the nearest approximation we are at present able to make to the perpendicular height of the source of the meteors, above the surface of the earth.*

3. *The meteors fell towards the earth, being attracted to it by the force of gravity.*

It seems unnecessary to assign any other cause for the descent of these bodies to the earth, than gravity, a known and an adequate cause. It is easy to conceive, that bodies situated in space at a distance from the center of the earth comparatively so small, as about six thousand miles, would be brought under the dominion of the earth's attraction, whatever may have been their previous tendency towards one another, or towards a central nucleus. Such a tendency indeed, if it existed, we shall hereafter see reason to believe was very slight, and would not materially oppose terrestrial gravitation.

4. *The meteors fell towards the earth in straight lines and in directions, which, within considerable distances, were nearly parallel with each other.*

The courses are inferred to have been in straight lines, because no others could have appeared to spectators in different situations, to have described arcs of great circles. In order to be projected into the arc of a great circle, the line of descent must be in a plane pass-

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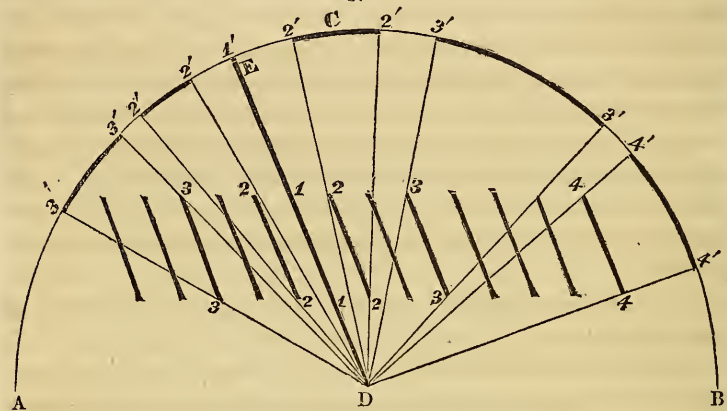
* I know of no way of accounting for the want of a parallax in R. A. corresponding to that in Dec. (as might be expected in some of the observations,) except to ascribe it to an uncertainty in respect to *time*, which would obviously greatly affect the observations in R. A.

ing through the eye of the spectator; and the intersection of such planes, passing through the eye of different spectators, must be straight lines.* The lines of direction are inferred to have been parallel, on account of their apparent radiation from one point, that being the vanishing point of parallel lines, upon well known principles of perspective. Some idea of the manner of falling, and of the cause of apparent radiation, may be gathered from the annexed diagram.

Fig. 3.



Let A B C (fig. 3) represent the vault of the sky, the center of which, D, being the place of the spectator. Let 1.2.3. &c., represent parallel lines directed towards the earth. A luminous body descending through the line DE, coincident with the axis of vision, would appear stationary all the while at 1′; a body descending 2,2; would appear to describe the short arc 2′,2′; and a body descending 3,3, would appear to describe the longer arc 3′,3′.

By considering thus, the manner in which the arcs described on the celestial vault would appear, according as the meteor was nearer to the axis of vision, or more remote from it, we shall arrive at the following conclusions; that those meteors which fell nearer to the axis of vision, would describe shorter arcs, and move slower, while those which were further from the same axis, would appear to describe longer arcs, and to move with greater velocity; that the meteors would all seem to radiate from a common center, namely, the point where the axis of vision met the celestial vault; and that if

* See a familiar illustration of this subject in Dalton on the Aurora Borealis, *Meteorological Essays*, p. 160.

any meteor chanced to move directly in the line of vision it would be seen as a luminous body, stationary for a few seconds at the center of radiation. All these circumstances are in accordance with the statements of various observers. Thus, Mr. Twining says, "in the vicinity of that point [the center of apparent radiation] a few star-like bodies were observed, possessing very little motion, and leaving very little length of trace. Farther off, the motions were more rapid and the traces longer; and most rapid of all, and longest in their trace, were those which originated but a few degrees above the horizon, and descended down to it." Professor Hitchcock observes, "that those nearest the point of radiation, had generally, a very slow motion, slower than in other parts of the heavens, and the apparent velocity as well as brilliancy, in some cases increased, as the meteor receded from the radiant point. In other instances, after a slow motion, over a very inconsiderable arc, they disappeared." (Amer. Jour. xxv. 356.) The *transient* appearance of a luminous body, at the center of radiation, is recognized in the account given in the Boston Christian Register, from which we have made an extract, on page 139; and the fact noticed by Mr. Palmer, (Vol. xxv. p. 384.) "that the circular space surrounding the center of radiation, from which no meteors appeared to fall, was small at first, but gradually enlarged its dimensions to the end of the observations, at which period it was many times larger, than at first,"—this fact is readily explained on the supposition that more meteors fell near the axis of vision at first than afterwards.

The westerly tendency, of the first variety of meteors, (See p. 394.) was probably owing to the earth's revolution on its axis, which would give to bodies falling towards the earth, but not endued with this motion, a relative motion westward.

By altering the position of the axis of vision, with respect to the horizon, in fig. 3. it will be seen that the meteors might, in certain cases, have appeared to the spectator to rise while they actually descended, a fact which was noticed by a few observers. Indeed from the above figure itself, those between the axis of vision and the zenith, appear to ascend.

5. *The Meteors entered the earth's atmosphere with a velocity equal to about four miles per second.*

Regarding the meteors as bodies falling from a state of rest, with respect to the earth, at the distance of 2238 miles from its surface, we may estimate the velocity they would acquire in falling any distance towards the earth, according to the laws of falling bodies. The atmosphere may either be considered as terminating at the height of fifty miles, or as being, beyond that limit, so rare, as to afford no sensible resistance to the descent of bodies; for, at this elevation the air would be nearly twenty times as rare as it can be made by the best air pumps. The question before us then is, *what velocity would a body acquire in falling from a point 2238 miles above the earth, to within fifty miles of its surface?*

If a denote any distance from the center of the earth, and x a less distance from the same center, r the radius of the earth, and m the space passed through in 1″ at the surface, then the velocity acquired in falling through $a-x$ will be given by the following formula.*

$$V=2r\left(m+\frac{a-x}{ax}\right)^{\frac{1}{2}}$$

That is, $V=7912\left(\frac{16.1\times 2188}{5280\times 6194\times 4006}\right)^{\frac{1}{2}}=4.1\dagger$ miles.

Hence it appears, that the meteors entered the earth's atmosphere with a velocity more than ten times greater than the maximum velocity of a cannon ball, and about nineteen times that of sound. We shall find reason to believe that these bodies were, in many instances, of great magnitude; and the immediate consequence of the falling of a body with this prodigious velocity must be, a powerful condensation of the air before it, thus retarding its progress, and producing also a great evolution of heat.

We may call to mind here, what is known respecting the density of the air at different elevations. At the height of seven miles above the surface, the air becomes four times as rare as at the surface, and this rarefaction increases in the same ratio as the height is increased, according to the following law:

Heights,	7	14	21	28	35	42	49
Densities,	$\frac{1}{4}$	$\frac{1}{16}$	$\frac{1}{64}$	$\frac{1}{256}$	$\frac{1}{1024}$	$\frac{1}{4096}$	$\frac{1}{16384}$

Hence, at the height of twenty one miles, the air is sixty four times as rare as at the surface; and at the height of forty nine miles, 16384 times as rare. It follows that *three fifths* of all the air contained in the atmosphere, is within four miles of the earth.

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* See Vince's Fluxions, Pr. XL. Ex. 5. Young's Mech. Art. 116.

† It is worthy of remark, that the velocity of the Weston meteor, as estimated by Dr. Bowditch, was three and a half miles per second. (Mem. Amer. Acad. III. 134.)

Rare as the medium must be at the height of fifty miles, where these bodies are supposed to enter it, yet we conceive that the air of the upper regions in the path of the falling body, would be put into violent motion, and would of course abstract an equal quantity of motion from the meteor; and since the body as it descended, would, at every stage of its progress, meet with denser and denser air, that the resistance would finally become such as greatly to retard or even to stop it, according as it were constituted of heavy or light matter; and if it were combustible, that heat would be thus evolved sufficient to consume it. For the sake of keeping the principal points distinctly in view we will, however, adhere to the method already pursued, of enunciating them in separate propositions.

6. *The meteors consisted of combustible matter, and took fire and were consumed in traversing the atmosphere.*

That these bodies underwent combustion, we had the direct evidence of the senses. We saw them glowing with intense light and heat, increasing in size and splendor as they approached the earth; we saw them extinguished in a manner in all respects resembling a combustible body like a sky rocket, burnt in the air; and in the case of the larger, we saw, for the product of combustion, a cloud of luminous vapor, which frequently spread over a great extent, and remained in sight, in some cases, for half an hour. To establish a case of real combustion, I do not know that we can either have or desire any better evidence than that of the senses. By combustion, however, it is not implied that every particle was converted into an aëriform product: the smoke is supposed to have consisted, as in most cases of terrestrial combustion, of vapors condensed in that region of cold, and of solid matter in a minute state of division, which escaped actual combustion, as is the case in the smoke of a common fire. The proportion of this solid matter would be greater, in consequence of the rarefied state of the air at the place of combustion.

That these bodies took fire *in the atmosphere*, we infer from the fact that they were not luminous in their original situation in space, otherwise we should have seen the cloud, or body, or whatever it was, from which they emanated; but they were not luminous except for the few seconds while they were within the atmosphere, for had they been so before, we should have seen them during the whole of their progress towards the earth, a period of about twenty and three fourths minutes.*

* 20′. 43.55″, calculated by formula in Young's Mech. Art. 116.

In the former part of this article, (p. 409.) the following query was raised: *Could bodies constituted like known aërolites, falling from any supposed height into the atmosphere, generate heat sufficient, by the abrasion or condensation of the air, to dissipate them in a cloud of smoke before they reached the earth?* It was deemed the most philosophical, first to inquire whether these bodies could be supposed to have the constitution of *known* aërolites, that is, of a class of substances which are known to fall from certain meteors, before assigning to them a different constitution. Several considerations, however, induce me to think that the bodies in question could not have been so constituted. In the first place, bodies as large as some of these were, composed like meteoric stones, of iron, silex, magnesia, &c. cannot be supposed to have undergone such complete destruction in a very few seconds of time. The Weston meteor of 1807, according to Dr. Bowditch, traversed the atmosphere, for more than one hundred miles, at the height of eighteen miles above the surface of the earth, nearly parallel with it, for the space of thirty seconds, but is not supposed to have been consumed or destroyed, but only to have thrown off a few superficial fragments, which fell to the earth.*

In the second place, had these bodies been thus constituted, falling as they did directly towards the earth, they would in many instances, at least, have reached the earth; and considering their immense numbers, we should have had multiplied and appalling proofs of the fact, in the destruction and ruin that would have marked the places where they fell. Yet no evidence has yet appeared of a single meteoric stone having been found; and it is even somewhat doubtful whether any palpable substance reached the ground, which could fairly be considered as a deposit from the meteors.

In the third place, these bodies were, according to the testimony of most observers, unaccompanied by any sound; and where such a sound was supposed to be heard, it was only a very faint one; but falling aërolites are attended by a sound like thunder, or the discharge of heavy artillery.

In the fourth place, if these bodies had proceeded from one large body in space, like the fragments which fell from the Weston meteor,

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* See the remarks of *Dr. Bowditch*, on the Weston Meteor, Tr. Amer. Acad. III. 213, also, *President Day* and *Professor Silliman*, on the same subject, Mem. Conn. Acad. I. 142-174.

and if that body had been analogous to this meteor, we should have seen it by reflected light at least, for the same reason that we see the moon, except at places where the sun was so far below the horizon, that it might fall within the shadow of the earth, and this could not have been the case, when the sun was at a less depression below the horizon, than 50° 41′. Or had it been a mere cloud made up of a congeries of the small bodies, such a cloud though less favorable for reflecting light than a solid sphere, would nevertheless have reflected enough of the sun's light to have been visible. Hence, we infer that neither the falling bodies themselves, nor the source from which they emanated, could have had a constitution analogous to known aërolites.

The immediate consequence of the prodigious velocity with which the meteors fell into the atmosphere, must as already suggested, be a powerful condensation of the air before them, retarding their progress, and producing, by such a sudden compression of the air, a great evolution of heat. If by forcing down a solid piston in a small barrel, we can elicit heat sufficient to set tinder on fire, an effect which takes place when the air is suddenly compressed into one fifth of its former volume,* what must be the heat evolved by the motion of a large body in the atmosphere with a velocity so immense. According to Dalton, when the rarefied air of the receiver of an air-pump is suddenly condensed to half its bulk, its temperature is raised fifty degrees.† We must remember, also, that as air expands, or enlarges its volume, its specific heat increases; and hence that the rarefied air of the upper regions will as truly afford heat on condensation, as the denser air nearer the surface, though less favorable to the support of combustion.

On account of the increase of capacity for heat which air undergoes by rarefaction, the absolute quantity of heat contained in a given volume of atmospheric air is, according to Leslie, the same at all elevations above the earth, more being in the combined, and less in the sensible state, as the rarefaction is greater; consequently, when a portion of air which had been rendered cold by expansion is powerfully condensed, a vast amount of sensible heat is suddenly liberated.‡ In accordance with this principle, Professor Leslie has given a *formula*,

* Library of Useful Knowledge, *Heat*, p. 38.

† Thomson's Chem. I. 135, Pictet, Phil. Mag., XIV. 364.

‡ *Leslie*, Encyc. Brit., *Sup*. Art. CLIMATE. *Thomson* on Heat and Electricity, p. 121.

by which can be determined the amount of heat extricated from air of any given degree of rarefaction, in reducing it to the state of air at the common density. The formula is as follows. Let θ denote the degree of rarefaction; then 45 $(\theta - \frac{1}{\theta})$ will express, in degrees of Fahrenheit, the heat evolved by condensation.

Now at such a height as forty nine miles above the earth, the air is supposed to be more than sixteen thousand times rarer than at the surface; and if the formula were applied to air in this state, the evolution of heat would be immense. But it is not to be supposed that, in the case before us, the air becomes reduced to the density of common air, until the meteor has descended through some distance. Considering, however, the great velocity of the falling body, we may suppose such a density to be attained at an average height of thirty five miles above the earth where the rarity of the air is 1024 times that at the surface. Hence, 45 $(1024 - \frac{1}{1024}) = 46080°$. This is the amount of heat which would be extricated by the condensation of air as it exists at the height of 35 miles; but the compression supposed is not that which results from air of this density, but of such as is less dense, commencing with that which is of an extreme degree of rarefaction. The amount of heat therefore would be greater than the estimate here made.

But to form some idea of the intensity of even this degree of heat, we may call to mind, that the temperature required to melt gold is only 5237°; the highest heat of a glass house furnace, 16000°; and the extreme heat required for the fusion of platina, 23177°.* The temperature, therefore, elicited by the falling meteors of Nov. 13th, can be compared only to those immeasurable degrees of heat which nothing can withstand, as that of Hare's blow-pipe and deflagrator.

It has been common to resort to electricity as the agent which produces the heat and light exhibited by meteoric bodies;† but Biot has satisfactorily shown, that lightning itself is no part of electricity, but is produced by the condensation of air before the electric fluid.‡

A combustible body falling into the atmosphere under such circumstances, would become speedily ignited, but could not burn freely, until it became enveloped in air of greater density; but on reaching the lower portions of the atmosphere, it would burn with great rapidity.

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* Henry's Chemistry.

† President Clap's Theory of Meteors.

‡ Traité de Physique, Tome II. 459. (Ed. 1816.)

6. *Some of the larger meteors must have been bodies of great size.* According to the testimony of various individuals, in different parts of the United States, a few fire balls appeared as large as the full moon. Thus Dr. Smith observes (p. 379): "By far the most magnificent meteor crossed the vertical meridian about 3 o'clock A. M. Its course was nearly due west, in length by conjecture about 45°, and at a distance of about 25° south of the zenith. *In size, it appeared somewhat larger than the full moon rising.* I was startled by the splendid light in which the surrounding scene was exhibited, rendering even small objects quite visible; but I heard no noise, although every sense seemed to be suddenly aroused, if I may so speak, in sympathy with the violent impression on the sight."*

This description implies not only that the body was apparently very large, but that it was at a considerable distance from the spectator. Now a body in order to appear as large as the moon, will have the respective diameters, calculated for different distances, assigned in the following table.

Miles Distant.	Miles in Diameter.
110	1†
55	$\frac{1}{2}$
22	$\frac{1}{5}$
11	$\frac{1}{10}$
$5\frac{1}{2}$	$\frac{1}{20}$
1	$\frac{1}{110}$

That is, a body appearing as large as the moon at the distance of 110 miles, must be one mile in diameter; at the distance of 22 miles, one fifth of a mile, or 1056 feet; and even, if only at the distance of a mile, it must be 48 feet in diameter. It may be impossible to decide at which of these distances the meteor seen by Dr. Smith, ought to be taken; but the position with regard to the spectator, the apparent distance, and continued brilliancy indicate, that its real distance was considerable.

Let us, (for a very moderate estimate,) suppose it to have been eleven miles off, and appearing as large as the full moon; it must then have had a diameter of 528 feet. But if any one should choose

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* Humboldt says some of the fire balls of 1799 were from 1° to 1° 15′ in diameter, and consequently more than twice as large as the moon. (Pers. Narrative III. 332.)

† 240.000 : 110 : : 2180 : 1 *nearly.*

to limit the distance still farther, say to 1 mile, he must even then admit the actual diameter to have been 48 feet. These considerations, indefinite as they are, are sufficient to show that the body in question must have been one "of great size," agreeably to our proposition.

We may farther infer the great magnitude of some of the meteors from the dimensions of the *trains* or *clouds* which resulted from their destruction. These often stretched over many degrees, and at length, were borne along in the direction of the wind, exactly in the manner of a small cloud.

From the remarkable appearances of these objects, it was early supposed that we might be able to identify certain meteors, proving that the same one was seen by different observers, more or less remote from each other, and that we should thus obtain data for estimating the height at which these meteors were extinguished. But the subject is not without its difficulties. On the one hand, there are many circumstances which seem to indicate that the same meteor was seen at different places. Several persons, on reading the notice which I gave of a fire ball that I observed, immediately supposed it identical with one seen by themselves. My account was as follows. "One ball, that shot off in the northwest direction, and exploded a little northward of the star Capella, left, just behind the place of explosion, a phosphorescent train of peculiar beauty. The line was at first nearly straight, but it shortly began to contract in length and to dilate in breadth, and to assume the figure of a serpent drawing itself up, until it appeared like a small luminous cloud of vapor. This cloud was borne eastward (by the wind, as was supposed, which was blowing gently in that direction,) opposite to the direction in which the meteor had proceeded, remaining in sight several minutes." The time was 15 minutes before 6 o'clock.

Mr. Daniel Tomlinson of Brookfield, about twenty five miles W. b N. from New Haven, characterizes a meteor which he saw as follows:

"The *time* of appearance was about half past five o'clock; it might vary a few minutes from that time. The *course* was from south to north, varying perhaps from 3° to 5° to the west of north. It started 4° or 5° south of the zenith, and passed directly over head to about the same distance north of the zenith; all who saw it here agree essentially in the foregoing particulars as well as in the following— that the train, when first seen was nearly straight, rather largest in the middle, and tapering nearly to a point at the south end; that it instantly contracted in length, and assumed the form of a serpent, with its head to the north; that it continued to contract in length, doubling and crossing itself, and forming a confused line, like that of a loose cord or a piece of tape dropped endwise on the floor, and then gradually dilated, and intermingled its folds, and assumed the form of a light cloud, passing off slowly in an easterly direction." To this description Mr. Tomlinson annexed a drawing representing the successive figures of the train, which agree in general with those observed by myself. The time and other circumstances also accord so well in the two cases, that we can hardly resist the impression that both spectators were observing one and the same object. Proceeding on this supposition, we may form some estimate of the height of this train, that is, of the place where the fire ball exploded. This place being 40° from the zenith of New Haven, and at or near the zenith of Brookfield, distant 25 miles, the elevation above the latter place must have been about 30 miles, that being the tangent of the angle of elevation. It favors the supposition, also, that the large fire balls were extinguished at a considerable height above the earth, that little or no *sound* was heard, while we should expect a heavy report from bodies of such magnitude, moving with such prodigious velocity through the lower regions of the atmosphere, whatever might have been their density. It appears moreover that whenever a cloud was so situated as to enable the spectator to compare its elevation with that of the falling meteors, the latter appeared the most elevated. Thus Capt. Parker in the Gulf of Mexico saw the fire balls pass *behind* a cloud, but not one passed *between the cloud and the observer;* and Professor Hitchcock says that "in no instance was a meteor observed between the clouds and the earth." (See his Essay in the last number of this Journal.) Brydone also testifies, that shooting stars seen from Mount Etna and the highest peaks of the Alps, always appeared as high as when seen from the lowest grounds.

Two difficulties, on the other hand, attending the supposition that the place of explosion was as high in this case as 30 miles, are very formidable. The first is, the *great apparent extent of the train*, and the second, the *velocity of the cloud* which resulted from it. The train of this meteor is judged to have been at least 10°, and others seen by Mr. Tomlinson were thought to be 40° in length. Since a body half a degree in diameter at the distance of 30 miles would have an *actual* diameter of $\frac{3}{11}$ of a mile, it follows that a body 10° in length, must have extended over a space $5\frac{5}{11}$ miles long. Again, the cloud moved eastward with a very perceptible progress, equal to that of an ordinary cloud a mile high, carried by a breeze of 10 miles per hour. But in order to move with such an apparent angular velocity at the height of 30 miles, it must have had a real motion of 300 miles per hour. The improbability of either of these suppositions, would lead us to believe, that the place of explosion where the trains were formed, was comparatively near to the earth.

This conclusion is much strengthened by several other considerations. It was the general impression of spectators, that the meteors descended almost to the earth. In this they were doubtless under a mistake, but still the impression is hardly compatible with the supposition, that those bodies were, at the time of their extinction, very high in the atmosphere. Those observers who were on the water, would not be so likely to be deceived in this respect, as those on land; and such, in various instances, testify that they appeared "to come quite down to the water's edge, to reach the tops of the masts," and even "to fall into the water."*

Again, the improbability that the same train, resulting from the destruction of a large meteor, was seen by observers remote from each other, is increased by the fact that trains, which must have been different, greatly resembled one another. Thus the one described by Dr. Hildreth, in a preceding article, (See p. 87,) resembles that mentioned by myself before referred to, almost or quite as much as the one described by Mr. Tomlinson. It was seen at about the same time of day, (twenty minutes before six,) had nearly the same course, exploded near the same place, and left a serpentine train. Yet these three descriptions cannot possibly refer to one and the same body. Indeed the *tortuous* figure which the trains successively assumed, is very characteristic of the trains of falling stars, and is even recognized in the history of Chinese meteors. (See p. 133.) This peculiar change of figure may be conceived to arise from the action of the wind. The train being left at first in the path of the meteor, is of course straight; but the oblique action of the wind would soon change its form, and may easily be imagined to give it the wavy outline exhibited. Moreover the trains, according to the testimony of various observers, were largest in the center and tapered towards either extremity. This appearance would result from the manner in which the combustion or destruction of the meteor

* See Mr. Schoolcraft's letter p. 139, and the last No. of this Journal, p. 392.

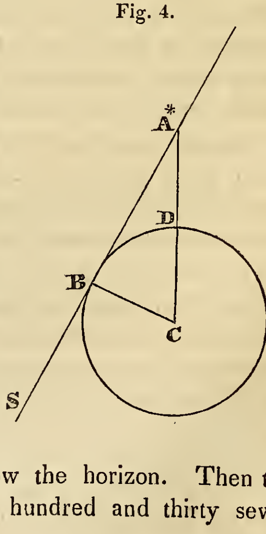
took place, the cloud being smaller at first because the body was not fully on fire, and smaller at last because it had mostly burnt away.

I feel constrained, therefore, contrary to my early impressions, to believe that the large meteors frequently descended to the region of the clouds. Nor is it difficult to apprehend the reason why a body should fall so near to the earth and yet not reach it, since the density and of course the resistance of the air increases so rapidly as we approach the earth, and becomes so much more favorable to the combustion of an inflammable body.

The short and fiery trains which followed the fire balls in their descent, are to be regarded as an ocular effect arising from the velocity of the body, the impression of the light remaining on the retina, as in the case of a whirled stick ignited at the end.

In the previous part of this article, the query was raised, *whether the trains were rendered luminous by being elevated above the earth's shadow into the region of the sun's light?* On submitting this inquiry to an easy calculation, we are compelled to answer it in the negative, since the height required for such a purpose, even when the sun is only ten degrees below the horizon, is sixty one miles; and since trains were seen as early as three o'clock, or even earlier, the height necessary to bring the train within the sun's light, becomes altogether too great to be admitted. This will be obvious from the subjoined calculation.

Fig. 4.



Let S, be the sun's place, D the place of the spectator, C the center of the earth, and AB the boundary of the earth's shadow. Then a body above the point A will fall into the light of the sun, and may be seen by reflected light in the same manner as the moon and planets are. To find the height of A, that is AD, we have in the triangle ABC, right angled at B, the angle BCD, which is the depression of the sun below the horizon, and BC, the radius of the earth. Hence, cos. BCA : BC : : rad. : AC, then AC − CD = AD.

Suppose the sun is fifty degrees below the horizon. Then the height of A would be two thousand two hundred and thirty seven miles.

The only remaining supposition is, that the matter of which the denser parts of the cloud was composed, (the parts which were dissipated without actual combustion) after having having been subjected to so powerful a degree of incandescence, remained luminous for a while after the combustion had ceased. This is a well known consequence of the exposure of various substances to a very intense light. Thus, after passing the electric spark from a large Leyden jar, through fluor spar, the mineral remains luminous for several minutes. It must be considered, also, that only a very small portion of light is required to render a body slightly luminous in the dark.

So great a number of bodies, some of which it appears, were very large, falling nearly through the atmosphere with so prodigious a velocity, must have produced *extensive derangements, in the atmospheric currents.* The first effect was a *westerly wind,* which suddenly succeeded the meteoric shower, in nearly every place where the shower prevailed. (See p. 385.) A westerly wind (as appears from the testimony of Sir H. Davy, already recited) commonly follows this phenomenon. Large volumes of air suddenly driven from the upper to the lower regions of the atmosphere, must have a relative motion eastward, since the velocity due to their greater distance from the earth, derived from the diurnal revolution, would not be instantly lost on their descending to a lower level. In our latitude, ($41°\ 18'$) a body of air descending suddenly to the surface of the earth from the height of twenty miles would have a relative velocity eastward of five miles per hour. For, the cosine of the latitude being 2972 miles, and the diurnal motion 750 miles an hour, $2972 : 2992 :: 750 : 755 =$ the velocity at the elevation of 20 miles.

A second consequence was, a sudden *production of cold,* an effect which would follow of course from the descent of such large quantities of air from the regions of perpetual frost.* A third consequence would be the *destruction of the equilibrium* of the atmosphere, and the prevalence of gales in various parts of the ocean. Nor is it altogether improbable that some *change of seasons* should result from so extensive a disturbance of the atmospheric equilibrium, and therefore that the remarkably warm weather of the northwestern parts of the United States as described by Mr. Schoolcraft, may have been

* The heat extricated by condensation might be supposed an equivalent; but this, when produced by sudden compression would not heat a gaseous medium, which is the worst of all conductors of caloric, but would escape by radiation.

one of the results of that disturbance. This uncommon mildness of weather, has pervaded a large portion of our country; but we are aware of the danger of ascribing too much to a natural agent which has taken possession of our minds, and would rather suggest this last conclusion, in the form of a *query* deserving of farther attention, than as a point fully established. If it should be found that the season in other latitudes has been colder than ordinary, it would strengthen the supposition that both effects were due to the cause assigned.*

7. *The Meteors, were combustible bodies, and were constituted of light and transparent materials.*

(1.) The fact that they burned, is sufficient proof that they belonged to the class of *combustible* bodies; but as this class is very numerous, embracing various substances in each of the three kingdoms of nature, this fact alone is insufficient to decide upon their specific constitution.

(2.) They must have been composed of comparatively *light materials*, otherwise their momentum would have been sufficient to enable them to make their way through the atmosphere to the surface of the earth. To compare great things with small, we may liken them to a wad discharged from a piece of artillery, its velocity being supposed to be increased to such a degree that it shall take fire, as it moves through the air. Although it would force its way to a great distance from the gun, yet if not consumed too soon, it would at length be stopped by the resistance of the air.

Although it is supposed, as already intimated, that the meteors did in fact slightly disturb the atmospheric equilibrium, yet had they been constituted of dense matter like meteoric stones, they would

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* A recent account of a shower of aërolites in Asia, exemplifies both the intensity of heat and light produced by powerful condensation, and the sudden cold, (for a dense fog is known to be owing to the influx of cold air,) that is occasioned by volumes of air brought down by falling meteors. "*Kandahor.*—A heavy shower of aërolites fell lately in this city; owing to the weight of the shower, the roofs of many of the houses fell in, and others were perforated. Zelfekar Aly Khan, the son of Olimala, having (although forbidden by his parents) gone to the court-yard of their house to gather some of these pebbles, which were very round and smooth, was killed by the fall of one of these fiery meteors, which struck him with such violence on the head, as to fracture his skull in three places. The flash which accompanied the stroke was so vivid, that it dazzled the eye of those sitting in the balcony of the house. The stone was found to weigh three seers, and many of the stones weighed upwards of two seers. This phenomenon was succeeded by so dense a fog, that the rays of the sun could not be perceived for three days that it lasted."—*N. Y. Journal of Commerce.*

have disturbed it vastly more. Their own momentum, it must be recollected, could be lost only as it was imparted to the air; and the fact that the agitations of the atmosphere were, comparatively, so slight, is a striking proof that the quantity of matter contained in the meteors was exceedingly small.

(3.) They were *transparent* bodies; otherwise, we cannot conceive how they could have existed together in their original state without being visible by reflected light. The body, or source, from which they emanated, must have had a great extent. If we contemplate it under the idea of a 'cloud,' and the phenomenon itself as a 'shower,' then, after accounting in part for the great area which the shower covered by a supposed progressive motion of the cloud, still it must itself have extended over a great space. (See p. 142.) Now a body only 20 miles in diameter, at the distance of 2238 miles, would appear as large as the moon; and one 200 miles in diameter, would appear ten times as large as the moon. Such a body, if opake, and constituted like the planets, ought to be visible like them by reflected light; nor can we imagine a body of such dimensions, under such circumstances, which would not be visible, unless formed of highly transparent materials.

If we were permitted to class unknown things with unknown, we should say, that the cloud which produced the fiery shower, consisted of nebulous matter, analogous to that which composes the tails of comets. We do not know, indeed, precisely what is the constitution of the material of which the latter are composed; but we know that it is very *light*, since it exerts no appreciable force of attraction on the planets, moving even among the satellites of Jupiter without disturbing their motions, although its own motions, in such cases, are greatly disturbed, thus proving its materiality; and we know that it is exceedingly *transparent*, since the smallest stars are visible through it. Indeed, Sir John Herschel was able to see stars through the densest part of the small comet (Biela's) which visited our planet last year. Hence, so far as we can gather any knowledge of the material of the nebulous matter of comets, and of that composing the meteors of Nov. 13th, they appear to be analogous to each other.*

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* In farther elucidation of this point, the reader is requested to consult the recent Treatise on Astronomy by Sir J. Herschel, Chap. X, or a well written article on Comets, from the Companion to the British Almanac, inserted in the American Almanac for 1834.

We have finally to enter on the inquiry, *What relations did the body which afforded the meteoric shower, sustain to the earth?* Was it of the nature of a *satellite*, or *terrestrial comet*, that revolves around the earth as its center of motion—was it a collection of *nebulous matter*, which the earth encountered in its annual progress—or was it a *comet*, which chanced at this time to be pursuing its path along with the earth, around their common center of motion?

We conclude that it could not have been of the nature of a satellite to the earth, because it remained so long stationary with respect to the earth. The time in which a satellite, at the distance of 2238 miles from the surface, or 6194 miles from the center of the earth, would complete its revolution, may be estimated as follows. The time in which a body would revolve about the earth *at its surface*, is given by the following formula,* $T=2\pi\left(\frac{r}{2\,m}\right)^{\frac{1}{2}}$, where π is the ratio of the circumference of a circle to its diameter, r the radius of the earth, and m the space through which a body falls at the surface of the earth in 1″. That is,

$$T=2\times 3.14159\left(\frac{3956\times 5280}{32.2}\right)^{\frac{1}{2}}=84.3 \text{ minutes.}$$

By Kepler's Law, $\overline{3956}^3 : \overline{6194}^3 :: \overline{84.3}^2 : \overline{165.2}^2$.

That is, the periodical time of a satellite, revolving in a circle at the distance of 6194 miles from the center of the earth, would be *two hours, forty-five minutes, and twelve seconds;* and consequently, its mean motion at the perigee, in a circle, would be 3.926, or nearly 4 miles per second; and its motion in an eccentric ellipse at the perigee, would be 5.552, or about 5½ miles per second.† This result is plainly incompatible with the supposition that the body in question was a satellite to the earth, since it remained stationary with respect to the earth, according to Dr. Aiken, for at least two hours,—a period sufficient to have carried it nearly round the earth in a circular orbit, and through many degrees of a parabolic orbit.

Nor can we suppose that the earth, in its annual progress, came into the vicinity of a *Nebula*, which was either stationary, or wandering lawless through space. Such a collection of matter could not remain stationary within the solar system, in an insulated state; and had it been in motion in any other direction than that in which the earth was moving, it would soon have been separated from the

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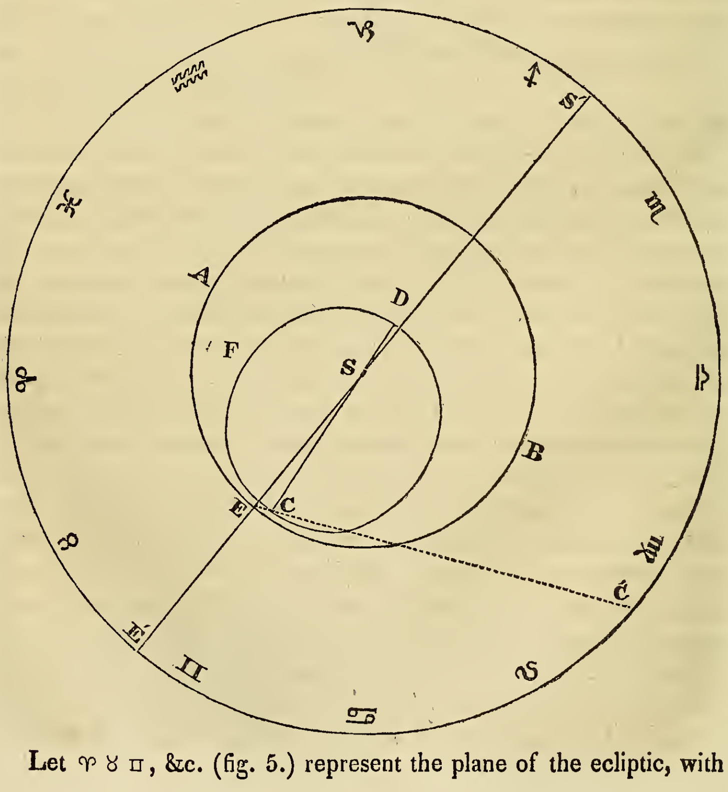
*** See Vince's Fluxions, Prop. xl. Ex. 6. † Gregory's Astron. Art. 586.**

earth, since, during the eight hours, while the meteoric shower lasted, (and perhaps it lasted much longer,) the earth moved in its orbit through the space of nearly 550,000 miles.

We have seen that the meteors appeared to be analogous, in their constitution, to the material of which the nebulous matter of comets is composed, in all the particulars in which we can compare the two. We may be permitted, therefore, in order to avoid circumlocution, to call the body which afforded the meteoric shower, a comet, while we pursue the inquiry, whether it exhibited the other attributes of that class of bodies.

The leading circumstances to be accounted for are the following: Why the phenomenon remained so long stationary with respect to the earth? Why it was seen in that particular part of the heavens? Why it returns at stated periods, having appeared at Mocha, in Arabia, just one year preceding, and, in a manner very similar to the present, as described by Humboldt, and by Ellicot, thirty-four years before?

Fig. 5.



Let ♈ ♉ ♊, &c. (fig. 5.) represent the plane of the ecliptic, with the twelve signs, AEB the earth's orbit, S the sun, and E the earth. On the morning of Nov. 13th, the place of the sun was in $21\frac{1}{4}°$ of Scorpio, and that of the comet in $23\frac{3}{4}°$ of Leo, (as observed at New Haven) being distant from the sun within $2\frac{1}{2}°$ of three signs or 90 degrees. The line of direction, therefore, as seen from the earth, was very nearly a tangent to the earth's orbit, and consequently coincided nearly with the line of direction in which the earth itself was moving. In other words, the earth was moving almost directly towards the comet. Therefore, S′ being the place of the sun among the signs, E′ that of the earth, and C′ that of the comet, join EC′, and the comet's place will be in the line EC′,* and, as was before shown, very near to E. Let it be at C.

Now the comet remained apparently at rest, and of course near the line EC′ for at least two hours. This it could not have done, unless it had been moving in nearly the same direction as the earth, and with nearly the same angular velocity around the sun. For had it been at rest, the earth, moving at the rate of 19 miles per second, would have overtaken it in less than two minutes; or, had it been moving in the opposite direction, the meeting would have occurred in still less time; or had not the angular velocities of the two bodies been nearly equal, they could not have remained so long stationary with respect to each other. Hence we conclude, (1.) *that the body was pursuing its way along with the earth around the sun.*

Taking it for granted that the orbit of the body is elliptical, like the orbits of all the other bodies of the system, we infer that, at the time of observation, it must have been either at its perihelion, or its aphelion, otherwise its angular velocity could not have corresponded so nearly to that of the earth. The regular return of the phenomenon, at short periods, indicates that the aphelion, and not the perihelion, is near the orbit of the earth. Another reason will be stated hereafter, which, it is supposed, confirms this conclusion. As the body was very near the earth at the time of observation, it must have been at its aphelion; and being seen then, only $7\frac{1}{4}°$ from the ecliptic, the plane of its orbit must be inclined at a small angle to the plane of the ecliptic, so that the body itself, if seen at all, will be seen within the zodiac. From all these considerations we conclude, (2.) *that the body revolves around the sun in an elliptical orbit, but little inclined to the*

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* $7\frac{1}{4}°$ northward of the plane of the ecliptic, as observed at New Haven.

plane of the ecliptic, and having its aphelion near to the orbit of the earth.

Let us inquire, next, what is the *periodical time?* Since the same phenomenon was exhibited at Mocha, on the morning of the 13th November, 1832, and on a much larger scale than that, in various parts of the world, on the morning of the 12th November, 1799, we cannot suppose such a coincidence in the time of the year to have been purely accidental, but must conclude that the periodical time of the comet, and that of the earth, bear to each other a ratio which can be expressed in whole numbers; so that after a certain number of revolutions of the two bodies, corresponding to the terms that express their ratio, they will come together again. They could not come together, as they did, on two successive years, unless the periodical time of the comet was nearly an *aliquot part* of that of the earth, such as one half, one third, &c. Now, if the time be any aliquot part of a year, it must be one half, so that the comet would perform two revolutions, while the earth performs one; for, were its period only one third of a year, the line of the apsides would not be long enough to reach the earth. This will be obvious from the following estimate. Let D represent the axis major of the earth, and d that of the comet's orbit, their times being as 3 to 1. Then, by Kepler's Law, $3^2 : 1^2 :: D^3 : d^3$.

Taking D=190,000,000 miles, d=91,343,000 for the whole major axis, which is not equal to the distance from the sun to the earth. But, supposing the times as 2 to 1, we have

$2^2 : 1^2 :: D^3 : d^3$, whence d=119,692,000 miles; giving for the perihelion distance 24,692,000, and for the aphelion 95,000,000 miles. Hence we conclude, (3.) *that the body has a period of nearly six months, and its perihelion a little below the orbit of Mercury.*

The transverse axis and the foci being determined, the ellipse may be described. Therefore, join CS, and produce the line CS to D, making SD equal to the perihelion distance, and upon CD describe the ellipse CFD, and it will represent the orbit of the comet.

This is to be regarded only as a *first approximation* to the true periodic time. The distance from the sun, instead of being taken, as here, at the *extremity* of the body, ought to be reckoned from the *center of gravity*, if we knew where to fix that. Nor can we suppose that the periodical time is very uniform, since a light nebulous body like the one in question, crossing as it does the orbits of Venus and Mercury, and having its perihelion near the orbit of the latter, would be subject to very great perturbations, sufficient to alter the dimensions of its orbit at every revolution. It might, for example, by coming into near conjunction with Mercury, have its periodic time greatly shortened, and be compelled, for a long period, to revolve nearer to that planet than it does at present; and again by coming into a similar position with respect to the Earth, its orbit might be enlarged, and its periodic time increased, so that it might for a long period revolve nearer to the earth than before. I am not able at present to assign the amount of these disturbing forces, but it is easy to see that they exist, and must greatly influence the motions of the body.

The reader will very naturally suppose that, if a comet had approached so near to the earth, having the plane of its orbit in the zodiac, it would have been visible, first on one side of the sun, and then on the other, like an inferior planet. There are grounds for believing that such is the fact, and that a body answering to the conditions of the supposed comet, has been seen, at intervals, ever since the 13th of November, and is still (March 31st) visible in the west after sunset.

By inspecting figure 5, it will be seen, that at the time of the meteoric shower, the body must have been westward of the sun, and if visible at all, must have been seen in the east before sunrise; that in consequence of the greater velocity of the earth,* the comet would almost immediately afterwards be in such a position with respect to the earth, as to appear very near the sun, and shortly would be seen to the eastward of that luminary, and set after him; and it would either move onwards before the sun, or backwards so as to disappear from the evening sky, according to the relative positions of the comet, the earth, and the sun. It will be farther manifest, on a little reflection, that a nebulous body of considerable extent, when brought very near to the earth, would cover a large space in the heavens. If, for example, the body were a comet of an elongated figure, as is usual in those bodies, it might, in certain positions, cover an immense arc in the sky, extending from the meridian to the horizon, or even much farther. We will endeavor shortly to make this matter plain by a diagram. Let us now see if we have any evidence of a body like the

* At the aphelion, the velocity of the body is determined as follows:

(semi axis major)$^{\frac{1}{2}}$: (Per. dist.)$^{\frac{1}{2}}$ :: Velocity in the cir. : Velocity in the ellipse. That is, 18.92 miles per second being the mean velocity of the earth, $(59.846)^{\frac{1}{2}}$: $(24.692)^{\frac{1}{2}}$ :: 18.92 : 12.15.

one supposed, having been seen in various positions, corresponding to those which a comet, revolving after the manner inferred in the foregoing paragraphs, must have assumed.

1. *Such a luminous appearance was exhibited on the morning of November 13th, being seen in the east before the dawn of day.*

Thus Mr. Palmer* says that "an auroral light, resembling day break, appeared constantly in the east from the time when his observations commenced," [2 o'clock, A.M.] Mr. P. stated to the writer, that this light was so bright, and so much resembled the morning dawn, that a member of his family got his pail to milk the cows, supposing it to be day break, but found it was only 4 o'clock.

Mr. *Darius Lapham* (p. 378) says, that at Cincinnati, "an aurora or boreal light, was seen during the meteoric shower, a little north of east. The lower edge of this bank of light appeared to be several degrees above the horizon."

Various other observers speak of seeing "an auroral light," or "an aurora borealis," but do not mention the points of compass. The greater number, however, of those who viewed the phenomenon, did not commence their observations till near day break; and others were too much occupied with the falling meteors, to notice such a light, although visible in the east. The writer quoted from in the Boston Centinel, (p. 367,) says, "there was a vapor in the atmosphere, visible round the horizon, which in the south east assumed a very beautiful appearance during ten minutes, about half an hour before sun rise."

2. *A peculiar light was seen eastward of the sun, visible in the west after sun set, as early as the first of December.*

I beg leave to repeat what was said on this subject in the former part of this article, p. 398. "The writer of this article observed an appearance resembling zodiacal light, between the hours of 7 and 8, on the evenings of Dec. 1st and 3d. It consisted of an auroral appearance in the west following twilight, being an apparent prolongation of the latter. It reached to a length of about 25 degrees, towards the head of Aquarius." Also on page 410, "The same appearance has been exhibited as late as Dec. 29th, in a form much more imposing than on either of the preceding occasions. It was observed immediately after twilight, being brighter than the zodiacal light, not len-

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* See the last No. of the Journal, p. 384.

ticular like that, and not extending along the Zodiac, but having its apex in a vertical circle, near Alpha Pegasi. Ridges of dark clouds, (cumulo stratus,) with intervals of clear sky, contributed to heighten the effect by contrast; and higher than these, was a thin vapor that became visible as it crossed Jupiter, which was near the meridian, being illuminated in a circular space around the planet. The vapor was so thin as hardly to diminish the light of Jupiter." The same appearance continued to present itself for several evenings, although in a manner less striking, until the presence of the moon prevented its being seen. After the moon light was withdrawn, it was seen again. I had the pleasure of pointing it out to several members of the Connecticut Academy on the evening of their meeting, the 28th of January. The following minutes are from my Note Book.

Feb. 3d. "The occidental aurora has been very conspicuous for the last two evenings, remaining until about 9 o'clock. Although it did not resemble the zodiacal light on the 29th of Dec., being then much more diffused over the southern and western sky, than is usual with that phenomenon, and extending much farther to the east, yet it has ever since appeared to extend along the zodiac, and to resemble that light in other respects. A faint halo about the brighter stars has been noticed of late by many persons."

The return of the moon prevented observations in the west until near the close of the month, and it never occurred to me to look for it in the east before the morning dawn, having, at that time, no correct ideas of the nature of its connexion with the meteors, nor of its importance to the theory of which it is now believed to afford a striking confirmation. Nor after the full moon, which occurred on Feb. 23d, did the light attract my attention again until the 8th of March, when I entered the following memorandum in my Journal. "This evening, the sky having been thoroughly cleared by a copious shower of rain, the luminous cone or lens, reached nearly to the Pleiades, through about 60 degrees of longitude, and was visible until nearly 9 o'clock." The return of the New Moon on the 10th, interrupted my observations; but on the 26th, a short interval was afforded me between the end of twilight and the rising of the moon, during which the same light again presented itself. On the 27th, 29th and 30th, it has been observed by myself, and a number of my friends. The last evening (the 30th) it reached in a direct line from the sun between the Pleiades on the west, and Aldebaran on the east, having its apex a little to the west of the latter, and making an angle with the ecliptic of 7¼ degrees, which is the inclination of the sun's equator, as has been observed heretofore, respecting the zodiacal light. Gen. DeWitt observed the same at Albany, also, early in February, and describes it "as extending towards, but not reaching Aldebaran."

Since this light was first observed by me on the 1st of December, it has advanced in the order of the signs through the constellations Sagittarius, Capricornus, Aquarius, Pisces, Aries and Taurus.*

Let us now inquire whether its different positions will be such as the supposed comet would assume, as seen from the earth.

The aphelion of the comet being in 21¼° of Taurus, we may estimate its true anomaly from that point at intervals of ten days during the whole period of its revolution, or 182½ days, and we shall have the positions of the body in its orbit at each of these times. Comparing these with the corresponding positions of the earth, we shall determine the relative places of the comet and the sun. They will be as expressed in the following table.†

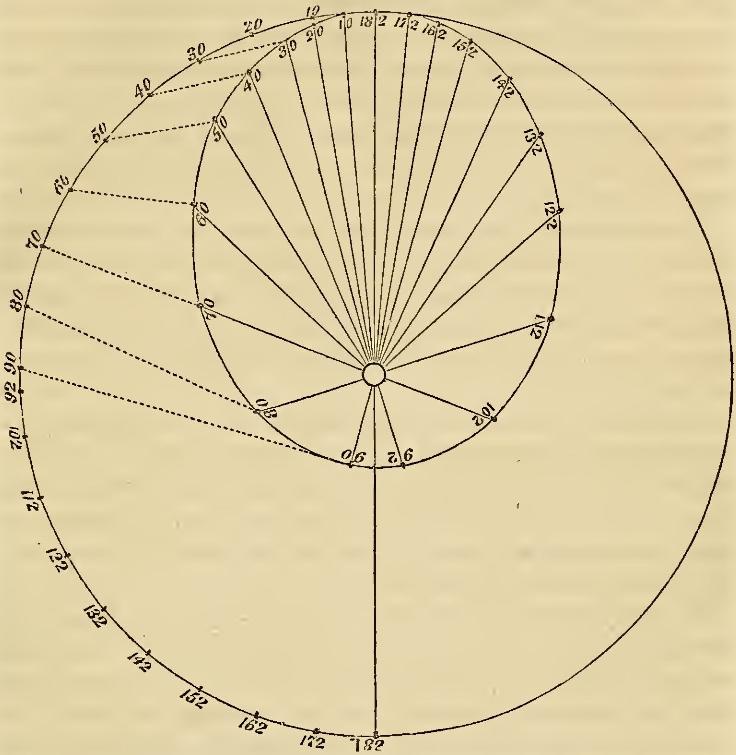
Date.	*Days from Aph.*	*Comet's true Anom.*	*Corresponding positions of the Earth.*
Nov. 13, . . .	0	0	0
23, . . .	10	5° 10′	10°
Dec. 3, . . .	20	10° 38′	20° 14′
13, . . .	30	16° 14′	30° 24′
23, . . .	40	24° 8′	40° 35′
Jan. 2, . . .	50	33° 40′	51° 2′
12, . . .	60	47° 0′	61° 13′
22, . . .	70	68° 12′	71° 24′
Feb. 1, . . .	80	105° 40′	81° 33′
11, . . .	90	170° 2′	91° 40′
13, . . .	92½	170° 2′	93° 42′
23, . . .	102	105° 40′	103° 46′
March 5, . . .	112	68° 12′	113° 48′
15, . . .	122	47° 0′	123° 47′
25, . . .	132	33° 40′	133° 42′
April 4, . . .	142	24° 8′	143° 34′
14, . . .	152	16° 14′	153° 23′
24, . . .	162	10° 38′	163° 8′
May 4, . . .	172	5° 10′	172° 50′
14, . . .	182	0	182° 30′

* The present appearance is almost precisely the same as that represented of the Zodiacal light in La Lande's Astronomy, tome I. 338.

† Calculated according to Ward's elliptic formula. See Vince's Astronomy, I. 109.

The relative positions of the earth, the comet and the sun, will, however, be more readily understood from the following diagram, constructed from the foregoing table.

Fig. 6.



The circle represents the earth's path, and the ellipse that of the comet. They set out together at the comet's aphelion, and revolve in the same direction around the sun, until the comet has performed an entire revolution.

It is manifest from the diagram, that immediately after the conjunction of November 13th, the comet would be projected, as seen from the earth, to the eastward of the sun, and would continue to be seen in this situation until about 70 days, or the latter part of January, when the line of projection passes to the western side of the sun, and continues there to near 112 days, or the 5th of March, when it returns to the eastward of the sun, and remains there until the circuit is completed, namely, the 14th of May. About the 29th Dec., it would be seen at the greatest possible distance from the sun. These results agree with our observations, as far as they have gone, with the exception of that of Jan. 28th to Feb. 3d. From the diagram we perceive that the comet would be to the westward of the sun, whereas we saw it after the evening twilight. This discrepancy indicates either that we have not yet learned the true periodical time, or that the comet has so great dimensions that, when on the side next to the earth, it may extend on both sides of the sun. This latter condition, indeed, may be fulfilled by a comet of a comparatively small size, as will be evident on substituting for the mere point at 70, or at 80, a small figure of a comet with its tail opposite to the sun, and inclined as usual towards its path.

From our theory we should farther anticipate, that the comet will disappear by or before the first of May, being too near the sun to be visible ; and that after the month of May, if seen at all, it will appear on the western side of the sun and rise before him, until the month of August, when it may possibly reappear for a little while in the evening sky.

Should future observations conspire with those already made, to establish such a period to this remarkable light, it will probably be regarded as a cometary body, and as the source of the meteors of Nov. 13th. But it will be remarked, that the several arguments alleged to prove the connexion of that phenomenon with a comet, are entirely independent of this light.

From all the foregoing considerations, I feel authorized finally to conclude, *That the Meteors of Nov.* 13*th, consisted of portions of the extreme parts of a nebulous body, which revolves around the sun in an orbit interior to that of the earth, but little inclined to the plane of the ecliptic, having its aphelion near to the earth's path, and having a periodic time of* 182 *days, nearly.*

I have supposed that a nebulous body, revolving about the sun in an eccentric orbit, might properly be called a comet; but should any one think that the analogy is not strong enough to authorise us to rank it among bodies of that class, he can apply any other name which seems more appropriate. Changing the name will not affect the validity of the theory. As the light spoken of in the preceding paragraphs, has many things in common with what is called the *Zodiacal Light*, it may appear to some to have been proper to denominate it thus ; but would not such an identity imply that the Zodiacal Light itself, is owing to a nebulous body, bearing to the solar system the relations which have just been developed ? Such is my present belief, but not having had leisure to examine all the facts recorded of that phenomenon, I would not venture to assert, positively, that this is the true explanation of that mystery. The explanation of the cause of the meteors of November 13th, may include that of the Zodiacal light, although it is not responsible for it. In March, the appearance becomes identified with that of the Zodiacal Light; but in Nov. and Dec. the Zodiacal Light was identified with that; and it may prove to be a fact that both appearances are dependent on the same cause.

Having now, as we suppose, arrived at a knowledge of the cause of the "Meteoric Shower," we may, as in other cases, go back and apply our theory to the correction of inferences made from independent sources of evidence. In fact, all the conclusions drawn in the former part of this article, as far as to the last head of inquiry, were wholly independent of the theory now developed, and without reference to any hypothesis whatever. Although I had early received the impression, that a nebulous body, or comet, had some connexion with the meteors, and intimated such an idea to the Connecticut Academy, at their session on the 24th Dec., yet I had formed no consistent views of the *nature* of this connexion, until nearly the whole of the preceding article was in print. Having come to the conclusion that the material of which the meteors were composed, was analogous to that which forms the tails of comets, I began to reflect on the connexion which such a body might have with the phenomenon observed, and was led successively to the several conclusions now submitted, nearly in the order in which they are here presented. Nothing but a strong conviction of their truth, would induce me to offer them to the public in so imperfect a state. The candid reader will appreciate the difficulty of maturing points of such intricacy, and establishing them by refined and elaborate calculations, while the press is waiting.

On comparing the theory with the propositions previously made out, the agreement appears to be, generally, good. Probably the origin of the meteors, was farther from the earth than the distance assigned in the second proposition; but it was necessary to form some estimate of the distance as a *starting point*, and that result was the best I was able to obtain from data so imperfect and discordant. But should the origin appear to have been at a much greater distance than was there assigned, the subsequent conclusions, built upon the supposition that the meteors fell towards the earth from a great distance, will be true for a stronger reason.

I cannot conclude without expressing my deep sense of obligation to various scientific gentlemen, who have been so good as to communicate to me their observations. I am particularly indebted for many valuable suggestions, to Gen. DeWitt of Albany, and to Alexander C. Twining, Esq., of West Point. Indeed, on comparing notes with Mr. Twining, I learn, that we have pursued nearly the same track of investigation, and arrived at some results very similar to each other; and I am happy to share with so able a coadjutor, the responsibility of bringing them before the public.

MISCELLANIES.

FOREIGN AND DOMESTIC.

British and American Journals of Science.—The diminution of the number of periodical works devoted exclusively to science in Great Britain, has, within a few years, been quite remarkable. One highly respectable quarterly journal, published in London, at the fountain head of science, has ceased to exist. Two of the monthly journals of the metropolis, which, during several years, maintained a friendly rivalship, at length coalesced, and they have since been joined by an Edinburgh quarterly journal, forming a monthly trio in uno, not larger than either of the original journals. The only quarterly at present in Great Britain, is Prof. Jameson's Edinburgh New Philosophical Journal, which has attained its twenty eighth or thirtieth number. This is a work of about the same number of pages as the American Journal. Many of the changes alluded to, have taken place since the commencement of our labors, and it cannot but be gratifying to us, to find so many of the pages of our Journal transferred to those of the only remaining quarterly, which promulgates science among British readers. The last two numbers which have come to hand, contain each twenty pages taken from one Vol. (23d) of our Journal. These extracts are not confined to original articles, but extend largely to the matter selected by us from the continental journals, and newly translated for our pages. While we are pleased with this tribute to the taste and judgment of our selections and translations, it may perhaps be no more than right to say, that when such materials are copied from one journal to another, the source from which they are derived, ought, we think, to be acknowledged.

Among other reasons, may be mentioned the fact, that when the British Journals arrive in this country, many of their most valuable articles are selected and published in the daily papers, and we have been for years in the way of seeing extracts thus published here, as interesting foreign matter, which might have been, had the editors known it, given to their readers months before from the pages of the American Journal. Articles, filling whole columns, have been thus unconsciously furnished their readers by our worthy friends of the National Gazette, and other daily prints.

CHEMISTRY, &c.

Extracted and translated by Prof. J. Griscom.

1. *Rapid sketch of the present state of* ELECTRICITY.—Professor A. DE LA RIVE has published, in four successive numbers of the Bibliotheque Universelle, for the year past, an able historical view of the principal discoveries made in electricity within the last few years. His memoir concludes with the following *Résumé.*

In terminating this historical sketch which we have endeavored to render as complete as possible, it will not perhaps be deemed amiss if we present, in a few words, the state in which it leaves the science of electricity.

1st. Two different principles are acknowledged to exist in electricity; the laws of action to which these principles give rise have been determined, both when they are isolated and at rest, and when they are in motion in order to unite. But the nature of them has not yet been determined: nothing has yet been done but to advance hypotheses which are still unsatisfactory,—such especially as that which regards them as very subtle fluids, endowed with certain distinct properties. It is probable that they are rather, both of them, different modifications of the ethereal matter which fills the universe, and whose vibrations constitute light; modifications, the nature of which cannot be known until the most intimate properties of electricity have been more thoroughly studied and ascertained.

2d. It has been successfully determined that magnetism is only the result of natural electric currents. But what is the disposition of these currents in magnetised bodies? What is the cause which gives rise to them, and what is the reason that a very small number of bodies only is susceptible of the magnetic virtue? These are questions which cannot yet be answered.

3d. All the sources of electricity are probably at present known; but with respect to the laws which, in each case govern its development, we are still very far from having discovered them.

4th. The influence which bodies may exert over electricity, either when placed in its track, or interposed between it and the points towards which its exterior action is directed, has been within a short time past, studied with great care. Many curious phenomena, in relation to this influence have been discovered; some laws even have been settled; but the number of anomalies and unexplained effects is still very considerable. It is probable that in the investigation of this class of facts, means may be found of arriving at some notions with respect to the nature of electricity, and the relations which connect this agent with ponderable matter.

5th. The effects which electricity produces on bodies are now well known; the laws to which they are subjected, are in general well determined; but their connection with the cause which produces them rests only on hypotheses, which can boast but very little solidity, and which have lately been very much shaken. It is by an enquiry into this connexion by a study in detail of those effects, that we are to find the means of arriving at a more just idea of the nature of electricity, and of the cause of the effects to which it evidently gives rise, as well as which are perhaps erroneously ascribed to it, and which, such as chemical phenomena, have probably no other relation to it but that of being occasioned by the operation of the same agent.

6th. Finally, after having, from its very origin, framed and demolished theories to account for the action of the Voltaic pile, philosophers are not yet united on that subject, and although at the present time, the chemical theory of this admirable apparatus may perhaps be most in vogue, it still requires the support of further observation to be generally adopted, and definitively substituted for the electromotive theory of Volta, the unsatisfactory nature of which is now fairly demonstrated.

This brief recapitulation is sufficient to show that notwithstanding the importance of the discoveries with which electricity has been enriched, that which remains to be done in this part of physical science, is perhaps more considerable than all that has hitherto been done, since almost all its laws and all its principles are still to be discovered.

2. *Electro-magnetic experiments.*—Professor Moll, under date of Utrecht, April 23, 1833, describes in the Bib. Univ. the result of various experiments to ascertain how far he could diminish the galvanic surfaces, and yet retain a considerable amount of magnetic power. His horse shoe was cylindrical, and when straight, measured twenty four inches (English) in length; diameter, two inches. It weighed with its spirals twenty nine pounds. The iron was surrounded with an envelope of silk, and around this were rolled two spirals (*sinistrorsum*) of *soft iron* of the diameter of $\frac{3}{16}$ of an inch. These spirals were not covered either with silk or any other substance. The ends of these iron wires were *soldered* together, and communicated by other wires with the simple galvanometric apparatus, consisting always of a single element. The other extremities of these wires were also *soldered* to the zinc and copper plates of the voltaic element; for his experience, (he observes,) and that of many others, has taught him that a contact as intimate as possible between the spirals and the zinc and copper of the battery, can alone secure complete success.

A little copper trough containing a plate of zinc of the surface (on one side) of seven eighths of a square inch, gave the horse shoe, (which had previously no sensible force,) a magnetic power which sustained twelve pounds. In a second experiment the iron sustained thirty nine pounds, and in a third forty eight pounds. Care was taken to prove that the horse shoe, prior to each experiment, had no sensible magnetic force.

Prof. M. then took a piece of the smallest of the copper coin, and a piece of zinc of the same diameter, viz. about $\frac{3}{4}$ of an inch. The horse shoe, with this, supported $6\frac{3}{4}$ ounces. He then used two of these half cents, (as they are called in Holland,) united them by a copper wire, and placed between them a disc of zinc of the same diameter, and plunged the apparatus in a little wooden trough. The horse shoe then supported $14\frac{3}{4}$ ounces. With a two cent piece of copper, ($\frac{7}{8}$ of an inch,) 2 lb. $5\frac{3}{4}$ oz. were sustained. With a twenty franc gold piece, it supported $13\frac{3}{4}$ ounces. With a silver piece of half a franc value, $\frac{3}{4}$ inch in diameter, the weight sustained was 13 lbs. $3\frac{3}{4}$ oz. With a zinc plate of $4\frac{1}{2}$ inches square, between two plates of copper in a wooden trough, 80 lbs. were supported. With a plate of zinc of $10\frac{1}{2}$ inches square in a trough of copper, 224 lbs. The acid menstruum was very strong.

3. *On the Isomeric bodies of* J. J. Berzelius.—The number of known bodies, which show different properties, notwithstanding their equal qualitative and quantitative composition, has increased of late very considerably. By the name of isomeric bodies, we signify such substances as possess by equal quantitative (per cent.) composition, also equal atomical weight; but Berzelius has, quite recently made more distinctions on the same, and defines them by the name of *isomeric*, *metameric* and *polymeric* bodies :—

1. Isomerism ; bodies possessing equal procentic (quantitative) composition, and equal atomical weight, or bodies composed of equal absolute and relative number of atoms, as for instance the two oxides of tin, both the phosphoric acids, &c.

2. Metamerism, (properly a modified isomerism,) where the same number of equal simple atoms are unequally distributed among compound atoms of the first order ; for instance, sulphate of tin ($\dot{S}n\,\dddot{S}$) and di-sulphate of tin ($\ddot{S}n\,\dddot{S}$) possess an equal, absolute and relative number of the same elements, they are however to be considered (should the latter salt be really in existence, which is not hitherto known,) as different bodies, which may however unite by transposing their constituents. The cyanuric and hydrocyanic acids exhibit another instance of metamerism.

3. Polymerism ; bodies having equal quantitative composition, but unequal atomical weight, or where bodies have an equal relative but unequal absolute number of atoms ; for instance, the relative atomical number of carbon and hydrogen in the olefiant gas and ethereal oil is quite alike, for the number of atoms of hydrogen is in both twice as much as that of the atoms of carbon ; but one atom of the gas contains but one atom of carbon and two atoms of hydrogen, (CH^2) whereas one atom of the oil contains four atoms carbon and eight atoms hydrogen (C^4H^8). It is very desirable that a general term should be given for designating these three classes, and it is the more necessary on account of many substances not yet having a determined atomical weight, and we should be at a loss to classify many bodies, which show equal properties and equal composition.—*Translated by Dr. Lewis Feuchtwanger.*

4. *Sparks in the freezing of water by ether.*—Pontus, professor of chemistry at Cahors, has observed, that if the freezing of water is performed in a small glass bottle, terminating in a thin neck of 1—2 cent. length, around which some cotton, moistened with ether is wrap-

ped, and if the atmospheric air be removed from it by the well known method, *clear visible sparks* may by day light be discovered disengaging themselves from the neck, just a few moments before the freezing takes place. This phenomenon appears to be a steady and a sure guide for indicating, whether the freezing will soon take place or not at all.—*Translated by Dr. Lewis Feuchtwanger, from the Central Blatt, July*, 1833.

AGRICULTURE, DOMESTIC ECONOMY, &c.

Translated by Prof. J. Griscom.

1. *Observations by M. Boutigny D'Evreux, on a new theory of the action of manures and of their employment; by M. de la Giraudieu.*—M. de la Giraudieu, President of the Agricultural Society of Loir-et-Cher, concludes from his own experiments that the weight of the produce of land is in proportion to the weight of manure with which it is enriched; and that soil, of whatever nature it may be, has only a mechanical agency in vegetation, and is no otherwise important than as a support to plants.

The first of these conclusions deserves notice, and agriculturists are indebted to M. de la G., for a communication of the curious and important fact, of a direct ratio between the manure and the produce. To assert, in fact, that the amount of produce is in proportion to the manure, is as much as to say, increase the number of your cattle that you may increase your manure, and thus double or treble your harvest. Better advice cannot be given, and no one has a better right to give it, than the President of the Agricultural Society of Loir-et-Cher. Since it is suggested by the result of actual experiment. *Experientia index.*

But I am far from uniting in opinion with M. de la G., with respect to the action of soils, independently of manure, and of the manner in which manure operates.

This author pretends, that calcareous, argillaceous, ferruginous or sandy soils act only as supports, like sponge, pounded glass, &c., this, I think, is erroneous and that no one can call in question the action exerted by the soil on vegetation. Who does not know that a soil composed in equal parts of siliceous sand, clay and carbonate of lime possesses great fertility, when well watered, although it may be completely destitute of vegetable remains? But how can this phenomenon be explained?

Admitting that the clay retains water, a portion of which however evaporates, an electric current is established;—admitting also, which is unquestionable, that the carbonate of lime yields carbonic acid to the vegetable, and that the lime absorbs a fresh portion from the air which establishes a new electric current, under which there is no vegetation or perhaps life, possible. Sand acts by dividing the other ingredients, by multiplying the points of contact, and increasing the number of *atomic piles.*

Some experiments, which I intend to repeat and publish, with all the needful developements, justify me now in concluding, first, that manures exert only an electro-chemical action on vegetation, second, that the best are those which are the most speedily decomposed, because they establish the most numerous and powerful electric currents.

These views may at first appear paradoxical; but if examined without prejudice or pre-conceived opinions, it will be seen that they are not inadmissible.

Every body has observed the influence on vegetation of a warm, moist and stormy atmosphere, or in other words, of one charged with electricity. It is sufficient, for this purpose, to walk in a garden, during such a spell of weather, and to return the next day. One hardly knows it again.

No one doubts the energetic activity of animal manure; now it is that kind which gives rise to the greatest number of new combinations, and consequently to the developement of the most active and numerous electric currents.

We know also that oxygin is necessary, and even indispensable to vegetation and especially to germination. Oxygen acts here in the same manner, in combining with carbon, or one of the various elements of the vegetable or grain and in establishing an electric current which ceases when the vegetable has passed through all the changes of its existence.

Does not oxygen act in this case as it does in fermentation? It can neither be affirmed nor denied perhaps at present.

With respect to the absorption of manure by vegetables, it is sufficient to notice the experiment of De Saussure, to refute the opinion. He found that a sunflower had absorbed only the twentieth of its weight of manure.

The action of plaster on meadows is yet but little known. I have reason for believing that it may be advantageously replaced by calcareous marl, pulverized and simply dried in the sun, or heated in an oven, after the batch has been withdrawn. If this opinion be correct, it will open an immense source of profit to the farmer. *Jour. des Connais. Usuelles.*

This article and the next, were translated by a lady and communicated by Mr. C. U. Shepard.

2. *New observations upon the action of sulphate of lime.*—The sulphate of lime (plaster of Paris, or gypsum) is employed with great success in agriculture.

It is especially in the culture of sainfoin, as well as in other artificial meadows, that its good effects have been proved. The sainfoin has even such an affinity with the lime, that the presence of the hedysarum onobrychis is almost always the indication of a calcareous soil, as the colts-foot (tussilago farfara) is of the blue clay, the arenaria rubra of a thin gravel, and the wild sorrel (oxalis acetosella) of the presence of *iron*. These are some of the botanical indications which answer very well in the analysis of soils for agriculturists in general. It does not appear nevertheless even to the present time, that plaster has been of great assistance in horticulture. But chemists are not agreed as to the manner in which the sulphate of lime acts upon vegetation. In employing it, it is scattered with the hand upon the crops when the leaves are in their full developement towards the end of April, or the commencement of May, at a moist, cloudy time but *not* rainy ; and those who perform the operation think in general, that they administer a stimulant, while some suppose that it is useful in obtaining for the leaves a favorable moisture; but the clover and the sainfoin naturally contain in their stalks a considerable quantity of gypsum, and when the soil appears tired of producing these plants, it is commonly thought that the soil becomes exhausted of its gypsum, and that it is no longer in a state to furnish to them the *necessary* ingredient. This observation leads to the presumption also, that the sulphate of lime enters, by a dose more or less considerable, into the composition of these plants. In this uncertainty, too much publicity cannot be given to any experiments which are likely to settle the question; and this consideration engaged M. Becquerel, member of the Institute, to communicate to the Academy of Sciences (in the session of the 7th of Nov., 1831) some observations made by M. Peschier, Apothecary of Geneva, upon the influence which the sulphate of lime exercises in vegetation. M. Peschier had disposed two equal vases filled with silicious sand slightly moist, and in each of which he had sown water cresses; one of the vases had been watered with pure water, and the other, with water containing sulphate of lime in solution. Afterwards he reduced to ashes, the cresses of the two vases, which had vegetated during the same time, and under the same atmospheric influences, and he found that in one hundred grains of the ashes of the cresses, watered by *pure* water, there were twelve grains of sulphate of potash, and twenty grains of carbonate of potash; while that in one hundred grains of the ashes of the cresses, watered by the water of sulphate of lime there were eighteen grains of sulphate of potash, and thirty grains of carbonate of lime. M. Peschier afterwards submitted the remainder of the cresses, already watered by the water of sulphate of lime, to the continued action of an electric current during many days, after which he found that the ashes of these cresses included twenty six per cent of sulphate of potash. M. Peschier made similar experiments upon lucerne or Spanish trefoil, and he observes "*the sulphate of lime ought to be made use of in a state of solution, and not in a solid state;*" he concludes from it that the sulphate of lime does not, in times of drought, act by communicating to the plant its water of crystallization; water, which it will re-absorb in a time of moisture, but that its principal action is considerably to augment the proportion of the sulphate and of the carbonate of potash, in the organization of vegetables. But here a question arises. Some earths are found in vegetables, as is proved incontestably; but do these earths make a part of their proper nourishment. Physiologists appear not to agree upon this subject. The experiments of Saussure tend to prove the contrary.

This observer has analized the ashes of two trees of the pinus abies (spruce) the *one* growing upon a granitic, the other upon a calcareous soil. In one hundred parts of the first he found thirty parts of silex, and fifteen of alumine, and forty eight of carbonate of lime. In one hundred parts of the second he found thirty parts of alumine, and sixty three of carbonate of lime.

It seems thence to result, that the silex was not necessary to the development of these trees, and that in the first experiment its presence was purely adventitious, and resulted from the qualities of the soil in which the tree had grown. Experiments carefully made will probably give a similar result for other trees, and we would solicit, for the sake of agriculturalists who *need* accurate information, whether it be correct to say that unassimilated inorganic mineral substances can strictly be said to enter into the organization of a living system like that of vegetables. It is obvious from the experiment of M. Peschier, that he introduced, at will, into the cresses, by means of water, containing sulphate of lime in solution, one third more of sulphate of potash, and of carbonate of lime than he ever had found in it, in its natural state, and that the action of an electric current augmented the quantity of sulphate of potash one third more still, but in concluding from this, that the sulphate of lime ought to be employed dissolved and *not* in a solid state, he appears not to have had in view merely, the introduction of the material into the plant—where it is not *altogether certain* that it contributes to its organic developement. This procedure, it seems, in the mean time withdraws the agriculturalists from a practice, whose advantages are established, without sufficiently considering that the farmer enters into the same views in not using the plaster in a solid state, except in weather which is not rainy, but cloudy and moist, which causes the slow and gradual dissolution of it, to the benefit of all the parts of the vegetable, without excepting the roots. *All this appears to merit attention. It is impossible to discover too much ardor in observing, or too much caution in forming conclusions.* S. B.

Ann. de l'Institut royal horticole de Fromont.

3. *Memoir upon valuable kinds of fruit trees, and their propagation from seed.*—In pursuing my researches upon the French Flora and Pomona, I have been led to make a new observation, and one which is contrary to all the received opinions of the last two thousand years, relative to the seeds of valuable fruits, such as pears, apples, plums, &c. M. Sageret, our associate, sowed about fifteen years since in his garden, in the street Folie Mericourt, a very great number of seeds of the best fruits. The young trees proceeding from their seeds, were put into a nursery. Four years after, he quitted his garden, and went to live in Montreuil street, No. 141, where he at present resides. His young fruit trees were taken up, and transplanted to his new garden, in Montreuil street, some of them having been twice transplanted. After two or three years, many pears, plums, &c., proceeding from these seeds, yielded fruit, and many among them good fruit: without being perfectly similar to the species from which they came, they often have some qualities which approximate to them. Having always heard it said that the best fruits propagated by their seeds degenerate, and that almost always acid and unpalatable fruits are the consequence, I wished to know the origin, or rather the experiments, which constitute the foundation of this opinion; I have read in consequence, and consulted a great number of works, and especially those of the most celebrated authors; but I

have not found any thing positive, or satisfactory. One fact like that which we offer, in the nursery of M. Sageret, is not able to overthrow, at once, a theory founded upon such a weight of opinion, but it seems to merit the attention of nurserymen the more, as this theory has been prejudicial to the perfection of our fruits; because, once admitted, we cease to make experiments, and to wait the result during fifteen years. I think then that the Royal and Central Agricultural Society ought to promote some experiments upon the produce of the seeds of our best fruits, by proposing a prize, which will be awarded in fifteen years, and which shall have for its object to know whether it is true, that the grains of the best fruits sown in a proper soil, yielding young trees, placed at first in a nursery, afterwards transplanted into good land, produce in a majority of cases, acid and degenerate fruits, as all ancient agriculturalists have supposed. I believe that transplantation is necessary, in order to ameliorate the fruits of trees proceeding from the seeds, seeing that all vegetables select in the earth, the moisture proper to *their particular nature*, and that they exhaust the earth in a few years, whence proceeds the theory of *rotations*. When a tree is planted, or a seed sown in any land whatever, we have not the means for knowing the elements with which they are nourished. It is a stranger that is established in the midst of a country, where the native inhabitants are capable of living for many years, without exhausting the soil; and although the trees, drawing their nourishment from greater depths than the annual plants, are *less* difficult than these, it is useful to change them, and to offer them an abundant and various nourishment.

It is objected perhaps, against the utility of transplanting, in order to ameliorate the species, that some of our good pears have been found wild in the forests where they have never undergone any transplantation; but five or six kinds of pears or of apples have come from the forests, from some thousands of seeds, of good fruits, scattered during many ages, either by birds or hunters, proving only that these grains have fallen into a vein of earth *so* favorable to their particular nature that they have not had need of a tender culture, or of transplantation. In proposing a prize for ascertaining if the ancients have deceived themselves upon the products of the seeds of good fruits, my opinion was at first founded upon conjecture, which was changed into a pobability, since Mr. Knight the President of the Society of Horticulture of London has announced that having sown some seeds of good pears he has already obtained twelve varieties of new pears of which some are of a quality superior to those which France and Belgium have heretofore furnished to England and that he is still expecting a greater number from many trees which sprang from the seeds. The experiments which I desire will have less for their object to obtain new varieties, than to afford positive ideas upon the production of fruit trees, upon the difference in the kinds of our fruits, and upon the connexions of the cultivated species with the wild species. This subject merits so much the more the attention and interests of physiologists, and of agriculturalists, inasmuch, as to the present time we have nothing satisfactory, or founded upon observation. They contribute, at the same time, to multiplying good fruits, which are the ornaments of the tables of the rich, and which offer each day to the poor, enjoyments within their reach, and in a season of want, an invaluable resource.

The Royal and Central Society of Agriculture, having adopted the proposition of M. Jaume Saint-Hilaire, Mirbel Sageret, Soulange Bodin, and Vilmorin.

In its session of the 15th of March, 1832, it approved of the following prospectus, which was presented, in the name of this committee, by the author of this memoir.

Premium to be awarded in 1848.—To the best memoir founded upon experiments, which tend to prove, whether it is true, as the ancient agriculturalists believed, that the seeds and the stones of our good fruits, being sown, and yielding young trees, placed at first in a nursery, and afterwards transplanted into good earth, produce in general, only wild and acid fruits, or whether it happens under these circumstances, on the contrary, that the majority are fine fruits resembling those of the trees, which furnished their seeds, or other varieties.

The first prize, the sum of one thousand francs.

The second prize, a medal of gold with the impression "Olivier de Serves."

The third prize, a medal of silver, *idem.*

The competitors will make known in their memoirs—

1. If the fruit from which the seeds have been sown, proceeded, in the case of the pear, from a tree grafted upon the same, or on the quince; as to the apple, whether upon the same, or upon the paradis; as to the peach, whether upon the prune, or the almond, or even upon a tree, which never before had been ingrafted.

2. They ought to select, in making the experiments, those fruits which are the most esteemed, and those whose distinctive characters are the best known, such as the Doyenne, the Crassane, the winter and summer Bon Chretien, the Beurre, the Virgonleuse, &c. in pears; the Cavilles, the Reinettes, the Api, the Fenouillet, in apples; the Reine Claude, the Mirabelle, &c.; in plums, &c. The number of the individuals of each species subjected to experiment ought to be at least fifty.

3. In 1834, they will be required to prove, the state and number of the trees proceeding from their seeds, by the commissioners of the Royal, and Central Society of Agriculture; or by the correspondents of this Society, and the members of the Societies of Agriculture of the cities of the departments, in the case where the experiments are made at a distance of more than six leagues from Paris.

4. In the interval of the years 1836 and 1839 the same formality must be observed for proving the number of the trees taken from the nursery and transplanted, in indicating also as exactly as possible, the nature of the earth into which they have been transplanted, and the particular care given to their culture.

5. Before the end of the month of December 1847, the competitors must send their memoirs, containing a description of the trees and the fruits which they have produced, and as far as that will be possible will present these fruits to the Society, conforming themselves, besides, to the general condition of the meeting.

6. In anticipation of the year 1848, and to sustain the zeal of the competitors, there will be awarded in 1834, a first prize, consisting in a medal of gold bearing the impression "Olivier de Serves," and for the second prize, a grand medal of silver, to those who will offer to the commissaries of the Royal and Central Agricultural Society or to those of the cities of the departments, the finest and the largest plants from their seeds.

7. In 1839, it will award anew, two medals, the *one* of gold, the other of silver, as in 1834, to the competitors who will present to the judgment of the commissaries of the Society, their experiment and its products, in the most satisfactory state.—*Ann. de l'Instit. royal horticole de Fromont.*

Translated by Prof. J. Griscom.

4. *Easy method of giving greater strength and firmness to thread, network, cordage and coarse cloth.*—The lixivium of oak bark has

been employed for scarcely any other purpose than that of the tanner, and yet it is applicable to a great variety of uses. If thread, cords, nets, coarse linen, &c. be steeped in it, they acquire greater firmness and durability. Fishermen have long resorted to this. Nothing is more apt to spoil than skins, and yet this preserves them. It is the same with hempen and linen cloth. They contain much gummy and resinous matter, which with tannin, forms an envelope and thus adds to their durability. Linen ought not to steep more than eight or ten days in this solution: it acquires a very brown color. When this color fades the operation may be repeated.

The best method of preserving nets and cordage is the following;—disolve two pounds of Flemish glue in fifteen gallons of water,—dip the nets, &c. into this solution and then steep them in a strong solution of oak or chesnut bark,—the tannin combines with the gelatin, and forms, between the fibres of the hemp, a solid net work which adds great strength to the cords. Any bark which contains tannin may be employed in making a decoction; so bones, parings of skin, remains of fish, &c. and generally all substances containing gelatine may be used in making a gelatinous solution. Fishermen who often throw away on the shore gelatinous fish, may use them for this purpose.—*Jour. des Connais, Usuelles.*

5. *Use of diablotins, or crackers of fulminating powder.*—Travellers in Germany use these crackers for the purpose of being awakened by the detonation when any one attempts to enter the room without permission. They are fastened across the crack of the door as if to seal it.

These explosive papers are made by taking strips of half an inch to an inch wide and of a convenient length. By means of a little gum water or paste, a small quantity of coarsely pounded glass is attached to one end, on one side of each strip about one fourth of an inch. A little fulminating powder is spread over the glass and the moistened end of the paper, and it is dried in the air: two of these strips are then laid with their covered surfaces nearly in contact and so that their uncovered ends may project different ways. A narrow strip of paper or parchment is then wrapt round the coated ends and fastened to one of them, but not binding them so tightly as to prevent their being drawn, by taking hold of the projecting ends, one over the other. The friction occasions their detonation.

The quantity of fulminating powder must be proportioned to the effect intended.—*Idem.*

6. *Cheap mode by which farmers and others may manufacture charcoal.*—Provide a hollow cylindrical cast-iron back log for the fire place, not so long but that it may become heated throughout its length. One end may be permanently closed and the other covered with a cap. Drill small holes in it from one end to the other, about a line in diameter. Fill this back log with blocks of wood or chips, and put on the cap. When heated, the inflammable air and tar that issue from the holes will aid the fire. In defect of a cast iron log, a joint of stove pipe may answer temporarily.—*Idem.*

7. *A tried recipe for burns.*—Keep on hand a saturated solution of alum (four ounces in a quart of hot water) dip a cotton cloth in this solution and lay it immediately on the burn. As soon as it shall have become hot or dry, replace it by an other, and thus continue the compress as often as it dries, which it will, at first, do very rapidly. The pain immediately ceases and in twenty four hours under this treatment the wound will be healed, especially if the solution be applied before the blisters are formed. The astringent and drying quality of the alum completely prevents them.

The deepest burns, those caused by boiling water, drops of melted metal, phosphorus, gunpowder, fulminating powder, &c. have all been cured by this specific.—*Idem.*

8. *To remove a hard coating or crust from glass and porcelain vessels.*—It often happens that glass vessels, used as pots for flowers and other purposes, receive an unsightly deposit or crust, hard to be removed by scouring or rubbing. The best method to take it off, is to wash it with a little dilute muriatic acid. This acts upon it and loosens it very speedily.—*Idem.*

9. *Scotch method of preserving eggs.*—Dip them, during one or two minutes in boiling water. The white of the egg then forms a kind of membrane, which envelopes the interior and defends it from the air. This method is preferable to the varnish proposed by Reaumur.

10. *Preservation of skins.*—J. Stegard, tanner at Tyman, in Hungary, completely preserves raw hides from putrefaction, and restores

those that are tainted, by applying to them, with a brush, a layer of pyroligneous acid. They absorb it very speedily, and it occasions no injury nor diminution of their value.—*Rceuil Industrielle.*

11. *Action of heat upon razors.*—It has been asked, why, in time of frost, a razor, unless it be warmed, will not cut without irritating the skin? It is because, when it freezes, the edge of a razor, examined by a microscope, is like a saw, and, as soon as warmed, becomes smooth.—*Idem.*

12. *Substitute for India ink.*—Boil in water, some parchment or pieces of fine gloves, until it is reduced to a paste.

Apply to its surface while still warm, a porcelain dish which has been held over a smoking lamp: the lamp black which adheres to it, will become detached and mingle with the paste or glue. Repeat the operation until the composition has acquired the requisite color. It is not necessary to grind it. It flows as freely from the pencil as India ink, and has the same transparency.

13. *To destroy caterpillars.*—To 15 gallons of water, add 1¼lbs. of common soap, the same quantity of flowers of sulphur, and 2lbs. of mushroons (the poisonous kind). Put the whole over a moderate fire and keep it stirring. Caterpillars, grubs, &c. watered with this liquor, immediately perish.

This recipe is said to come from Germany, where it has extraordinary success.

Notices by Dr. Alexander Jones, of Mobile, Alabama, addressed to the Editor.

14. *American Gypsies.*—I see Prof. Griscom, has translated from the "*Revue Encyclopedique*" an article on "*Gypsies*" for your Journal, in which he remarks that there are no "*Gypsies*" in America or that, "they have never appeared in America." In this, the writer is mistaken. There is a colony of "*Gypsies*" on *Biloxi Bay* in Louisiana, who were brought over and colonized by the French at a very early period of the first settlement of that state. They are French "*Gypsies*" and speak the French language, they call themselves "*Egyptians,*" or "*Gypsies.*" The French call them indifferently, "*Egyptians*" or "*Bohemiens.*"

What is remarkable, since their colonization in this country, they have lost the distinctive character of their idle and wandering habits.

They are no longer strolling vagrants; but have, in the lapse of time, become in all respects, like the other French settlers found in Louisiana. They appear equally polite, hospitable and intelligent. They also possess all the industry and enjoy all the ordinary comforts of settled life, that belong to the French inhabitants generally.

The only striking difference between them, is seen in their complexion and in the color of their hair, which is much darker in the "*Gypsies*," than in the French population. Their hair is also coarser and straighter, than that of the French.

Their intellectual vigor, appears to be as great, as that of any people. A young man of this colony, received a collegiate education at *Georgetown, D. C.*, and is residing in New Orleans; and there are probably few men to be found in the United States of his age, whose knowledge, and learning are more profound and varied than his. He is also a good and ready writer. The most of the foregoing facts were derived from an eminent and learned lawyer of Mobile, who speaks the French language fluently, and has travelled among, and conversed familiarly with these "*Gypsies.*"

15. *Bituminous Coal.*—This state, is very rich in *bituminous coal*, of a most excellent quality. It is in every respect, equal, if not superior to the best English coal. I am using some of it in my little laboratory. It is very heavy, and burns with a good flame and gives out much heat. It also yields the carburetted hydrogen gas, in immense quantity. The vein, or formation of this coal, is very extensive. It is first seen in the bed of the Black Warrior River, near Tuscaloosa, and next appears on the surface of the ground, to the north east, and east of that town, and pursues that course till it crosses the Alabama and Coossa Rivers at their falls, or just above them. It passes on probably, for some distance into Georgia, and not improbably in its south western or west direction into Mississippi.

Its principal width is found in *Shelby* and *Bibb* Counties, where it is forty miles wide; it occupies the whole ground just under the surface, and is covered by superficial patches of hard or soft slate stone, or shale, other minerals being rarely found near it. Blacksmiths in its neighborhood, dig it up, and work it in their furnaces. It is also used in an iron foundery in Shelby County.

The land is smartly broken. The growth consist principally of chesnut, oak and pine, and being more or less poor, it has never, much of it, passed yet out of the hands of the General Government, and can therefore be bought by any one, who wishes to own it, at $1.25 cents an acre.

In the winter season, this coal is brought down the river to Mobile from Tuscaloosa, in flat bottomed boats, and sold at the same price as the Liverpool coal, or at from $1 to $1.50 cts. a barrel. The strata of this rich and extensive coal bed, have an inclination of a few degrees, to the south south east.

I presume you will ere long, receive a correct geological account of this extensive and interesting coal formation, from some gentleman of the Alabama University at Tuscaloosa, which is a very favorable point for observing it.

The facts which I have communicated, were obtained from an eminent lawyer of this place who had visited the region and from a laboring man, who had worked the coal in a blacksmith's shop which he owned in that region. He informed me, that having worked at the coal mines in Virginia, near Richmond, he considered this coal deposit the richest and as containing the best coal he had ever seen.

16. *Miscellaneous facts.*—The diluvial region in the lower part of this state, contains numerous quarries of ferruginous sand stone, often including pebbles of pure quartz of various sizes. In many places, this sand stone is used for building; I have a specimen, obtained from a quarry within a few miles of Mobile. At *Blakely*, opposite to this place, are found some very interesting petrifactions, of different kinds of wood, among which, are specimens of petrified *live oak.*

The region of country just around Mobile, is very rich in botanical productions; many insects, are inhabitants of the swamps. This is a fine field, for the student of natural history, and as yet scarcely explored or cultivated at all.

17. *India Rubber Carpets.*—Having some India Rubber varnish left, which was prepared for another purpose, the thought occurred to me, of trying it as a covering to a carpet, after the following manner.—A piece of canvass was stretched and covered with a thin coat of glue, (corn meal size will probably answer best,) over this was laid a sheet or two of common brown paper, or news paper, and another

coat of glue added, over which was laid a pattern of house papering, with rich figures. After the body of the carpet was thus prepared, a very thin touch of glue was carried over the face of the paper to prevent the India Rubber varnish from tarnishing the beautiful colors of the paper. After this was dried, one or two coats, (as may be desired,) of India Rubber varnish were applied, which, when dried formed a surface as smooth as polished glass, through which the variegated colors of the paper appeared with undiminished, if not with increased lustre. This carpet is quite durable, and is impenetrable to water, or grease of any description. When soiled, it may be washed, like a smooth piece of marble, or wood. If gold or silver leaf forms the last coat, instead of papering, and the varnish is then applied, nothing can exceed the splendid richness of the carpet, which gives the floor the appearance of being burnished with gold, or silver. A neat carpet on this plan, will cost (when made of good papering,) about 37½ cts. a yard. When covered with gold, or silver leaf, the cost will be about $1,00 or $1,50 cents a yard.

18. *Stereotype Metalagraphic Printing.*—I offer this name, as I have nothing better to designate it by. It means simply the transferring of printed letters, from the pages of a book, or news paper to the polished surfaces of metallic plates, especially of soft iron. My experiments are not yet completed, yet I feel satisfied that the result is entirely a practicable one, if carefully conducted with proper instruments.

The best plan on which to conduct the experiment is as follows:—Take two plates of very soft iron, of moderate dimensions, give one face of each a very true and fine polish, so that when applied by these faces, they shall uniformly fit and adhere together. Moisten two slips of printed news paper, or parts of a leaf from a book of the size of the plates, apply one to the polished face of each plate, and interpose between them a fold or two of silk paper, and then clamp the plates together. Give them a gentle heat over the fire, then place them in a vice, and apply a strong screw power. On separating them and gently removing the paper, the letters will be seen, distinctly formed on the faces of the two plates. Now as printer's ink, is formed of *lamp black and oil*, upon which acid acts very little, the faces of the plates may be slightly touched over with diluted sulphuric or nitric acid, which if skilfully applied, acts on the iron and leaves the letters raised. When the printer's ink contains some bees wax, the experiment is more complete. These plates once formed, may be converted into steel, on the plan of Mr. Perkins; after which they would probably print from 10,000 to 20,000 copies without being materially defaced. An expert mechanic, with proper machinery, could in a day or two, form a sufficient number of plates to print off 20,000 copies (500 pages) of an octavo volume.

Other metals, as copper, brass and type metal with slight variations, can all have letters transferred to them in the same manner, and can be used as printing plates; but none of these will have the durability of iron.

19. *Materials for paper.*—By a series of experiments I have ascertained that *paper*, of an excellent quality, can be prepared not only from the husks of Indian corn, but also from a pulp made from various kinds of wood and bark, particularly from the bark of several kinds of poplar, and from the wood of birch and some other trees.—In conducting my experiments, my plan has been, first to select the vegetable matter, then, if it required whitening, to bleach it in chlorine gas, and afterwards to reduce it to a fine pulp, by pounding, and filing in water. When properly prepared, I would place a small portion of the pulp, between polished steel plates, slightly warmed, and strongly compress them by screw power; the degree of consistency and polish, assumed by the pulp, under such compression, would indicate the quality of paper capable of being prepared from the vegetable matter used. I trust, that the time will soon arrive, when rags, will not be considered as indispensable in the manufacture of paper, and will be, when economy or convenience requires it, superseded by different kinds of vegetable substances, which are so cheaply, bountifully and universally furnished by nature.

OTHER NOTICES.

1. *Notice of a work entitled Experiments and Observations on the Gastric Juice, and the Physiology of Digestion.* By William Beaumont, M. D., *Surgeon in the U. S. Army.* Plattsburgh, 1833, pp. 280.—This work of Dr. Beaumont, which has been for some time announced, is, to say the least of it, equal in interest to any one upon the same subject, that has ever been presented to the public. The opportunity afforded to Dr. Beaumont, to institute experiments upon the important and interesting subject of digestion, was

a rare one ; and we congratulate the public, and especially the medical profession, that it fell into the hands of one who appreciated its value, and who possessed the requisite intelligence, perseverance, and candor, to make the investigation which it afforded, and to state the results of such investigation, in a plain, simple, intelligible manner, without bias from preconceived opinions, or fanciful hypotheses.

The case which gave rise to these experiments will be understood, from a brief summary of a particular account given of it, in the introduction to the book.

In the year 1822, Alexis St. Martin, then in the employment of the American Fur Company, while at Michillimackinac, where Dr. B. was stationed, was wounded in the side by the accidental discharge of a musket loaded with buck shot. The contents of the gun struck him upon the left side, and passed in an oblique direction forward and inward, literally blowing off integuments and muscles of the size of a man's hand, fracturing and carrying away the anterior half of the sixth rib, fracturing the fifth, lacerating the lower portion of the left lung, and the diaphragm, and perforating the stomach. The progress of the case, after so extensive a wound, involving parts of so much importance, was, of course, slow. The soft parts in the vicinity sloughed away : the ribs and their cartilages were successively attacked and destroyed by inflammation, and removed by the surgeon ; and it was not until June, 1823, a year from the time of the accident, that recovery, so far as it took place, was completed. At that time, the parts were in the following state : the injured parts were all sound, and firmly cicatrized, with the exception of the aperture in the side and stomach. The perforation was about two and a half inches in circumference ; and the food and drinks constantly exuded, unless prevented by a tent, compress and bandage. At the point where the lacerated edges of the muscular coat of the stomach and the intercostal muscles met and united with the cutis vera, the *cuticle* and the *mucous membrane* of the stomach *approached* each other very nearly. They did not unite, like those of the lips, nose, &c., but left an intermediate marginal space, of appreciable breadth, between them, completely surrounding the aperture. This space is about a line wide ; and the cutis and nervous papillæ being unprotected, are as sensible and irritable as a blistered surface abraded of the cuticle. The only change which has since taken place, is the gradual falling down from the upper margin of the orifice, of a fold of the coats of the stomach, fitting itself to the aperture, and forming a valve, which effectually retains the contents of the stomach, even when completely filled.

From that time to the present, St. Martin has enjoyed as good health, and as much vigor, as men in general; has performed, with little or no inconvenience, the duties of a laboring man ; married and become the father of a family of children, and has subsisted upon the common food of men in his situation, except when a particular diet has been prescribed, for the purpose of experiment.

There are several plates which are intended to represent the state of the parts, under different circumstances. As these are too imperfect to render any assistance, we are glad that a mere verbal description will render the account sufficiently intelligible.

From this account it will be seen, that the cavity of the stomach is open to the view ; that the state of its surface, and of the secretions from it, can be readily examined ; that foreign bodies can be introduced and removed at pleasure, and that the changes which have been wrought upon them, at any time after they have been introduced, can be ascertained.

For several years, since his recovery, St. Martin has been retained in the service of Dr. Beaumont, at the expense of much time, patience and money, for the sake of examining into the functions of an organ, closed to most persons, but thus thrown open to him. This task he has performed in a manner highly creditable to his industry and intelligence ; and the manner in which he has related his observations and experiments, is such as to carry to every mind a conviction, if not of their absolute truth, certainly of the absence of all intentional error.

It is not our intention to give a detailed account of all the experiments and opinions which are contained in this book. Such a statement belongs appropriately to professional works. There are however many principles and facts in it, of such general importance and application, as to render them interesting to every scientific inquirer. Some of them we propose to lay before our readers, in as condensed a form as is consistent with their intelligibility.

Nearly one half of the book is occupied with preliminary observations, arranged under the following heads : 1st, *Of Aliment.* 2d, *Of Hunger and Thirst.* 3d, *Of Satisfaction and Satiety.* 4th, *Of Mastication, Insalivation and Deglution.* 5th, *Of Digestion by the Gastric Juice.* 6th, *Of the appearance of the Villous Coat, and of the Motions of the Stomach.* 7th, *Of Chylification, and of the Uses* *of the Bile and Pancreatic Juice.* The remainder consists of four series of experiments upon various points connected with the appearance, temperature, motions, and secretions of the stomach, and with the changes which aliment undergoes when submitted to its action, amounting in the whole to almost two hundred and fifty, each of which occupied in its performance several hours, and many of them several days.

The course which we shall adopt, is to state in the form of distinct propositions, without much regard to the order in which they are found in the work itself, a few of the most important principles which it contains, and to accompany them with the facts by which they are supported.

1st. There is a distinct fluid poured into the stomach, possessing peculiar and important properties: this fluid, Dr. Beaumont, following Spallanzani, calls the *Gastric Juice.*

The proofs of the existence of this fluid are complete. Dr. Beaumont has obtained it, almost in a state of purity, in many hundred instances, by exciting the action of the vessels of the stomach, when empty, and after fasting. His account of the manner of doing it, is this. "The usual method of extracting the gastric juice, is by placing the subject on his right side, depressing the valve within the aperture, introducing a gum elastic tube, of the size of a large quill, five or six inches into the stomach, and then turning him upon the left side, until the orifice becomes dependent. In health, and when free from food, the stomach is *usually* entirely empty, and contracted upon itself. On introducing the tube, the fluid soon begins to flow, first in drops, then in an interrupted, and sometimes in a short continuous stream. Moving the tube about, increases the discharge. The quantity of fluid ordinarily obtained is from four drachms, to one and a half or two ounces. Its extraction is generally attended by that peculiar sensation at the pit of the stomach, termed sinking, with some degree of faintness." P. 21.

The fluid thus obtained he describes as being, "a clear, transparent fluid, inodorous, a little saltish, and very perceptibly acid. Its taste, when applied to the tongue, is similar to that of thin mucilaginous water, slightly acidulated with muriatic acid. It is readily diffusible in water, wine or spirits, and effervesces slightly with carbonated alkalies." P. 85.

No exact chemical analysis of this fluid has been effected. The experiments upon it by Professors Dunglison, Emmett and Silliman, as stated at p. 78 and 80, warrant the conclusion that it contains, in addition to animal matter soluble in cold, but insoluble in hot water, together with the *Phosphates* and *Muriates* of *Potassa*, *Soda*, *Magnesia* and *Lime*, a considerable amount of free *muriatic* acid, some *acetic* and a trace of *sulphuric* acid.

It is hoped that a more perfect analysis may be obtained from Professor Berzelius, to whom a quantity of it, was sent the last summer.

The principal properties of this fluid are: it is insusceptible of putrefractive fermentation: it prevents the putrefaction of animal and vegetable substances: it coagulates animal and albuminous fluids and is a perfect solvent of most animal and vegetable substances.

2. The gastric juice is secreted into the stomach only when some foreign body, especially alimentary matters, are brought into contact with its mucous coat. The author's account of this process as well as of the appearance of the villous coat of the stomach is perfectly satisfactory and highly interesting.

"The inner coat of the stomach, in its natural and healthy state, is of a light, or pale pink color, varying in its hues, according to its full or empty state. It is of a soft, or velvet-like appearance, and is constantly covered with a very thin, transparent, viscid mucus, lining the whole interior of the organ.

"Immediately beneath the mucous coat, and apparently incorporated with the villous membrane, appear small, spheroidal, or oval shaped, glandular bodies, from which the mucous fluid appears to be secreted.

"By applying aliment, or other irritants, to the internal coat of the stomach, and observing the effect through a magnifying glass, innumerable minute lucid points, and very fine nervous or vascular papillæ, can be seen arising from the villous membrane, and protruding through the mucous coat, from which distills a pure, limpid, colorless, slightly viscid fluid. This *fluid*, thus excited, is invariably distinctly acid. The *mucus* of the stomach is less fluid, more viscid or albuminous, semi-opaque, sometimes a little saltish, and does not possess the slightest character of acidity. On applying the tongue to the mucous coat of the stomach, in its empty, unirritated state, no acid taste can be perceived. When food, or other irritants, have been applied to the villous membrane, and the gastric papillæ excited, the acid taste is immediately perceptible. These papillæ, I am convinced, from observation, form a part of what is called by authors, the villi of the stomach. Other vessels, perhaps absorbing as well as secretory, compose the remainder. That some portion of the villi form the excretory ducts of the vessels, or glands, I have not the least doubt, from innumerable, ocular examinations of the process of secretion of gastric juice. The invariable effect of applying aliment to the internal, but exposed part of the gastric membrane, when in a healthy condition, has been the exudation of the solvent fluid, from the above mentioned papillæ.—Though the *apertures* of these vessels could not be seen, even with the assistance of the best microscopes that could be obtained; yet the points from which the fluid issued were clearly indicated by the gradual appearance of innumerable, very fine, lucid specks, rising through the transparent mucous coat, and seeming to burst, and discharge themselves upon the very points of the papillæ, diffusing a limpid, thin fluid over the whole interior gastric surface. This appearance is conspicuous only during alimentation, or chymification. These lucid points, I have no doubt, are the termination of the excretory ducts of the gastric vessels or glands, though the closeset and most accurate observation may never be able to discern their distinct apertures.

"The fluid, so discharged, is absorbed by the aliment in contact, or collects in small drops, and trickles down the sides of the stomach, to the more depending parts, and there mingles with the food, or whatever else may be contained in the gastric cavity.

"The gastric juice never appears to be accumulated in the cavity of the stomach while fasting; and is seldom, if ever, discharged from its proper secerning vessels, except when excited by the natural stimulus of aliment, mechanical irritation of tubes, or other excitants."

This account of the phenomena attending the flow of the gastric fluid into the stomach, explains the fallacious nature of the experiments of Montegre, who could vomit at will, and who after analyzing the fluid so obtained, declared that it was not acid, not slow to putrify, not a solvent, and so much like saliva, that he regards it as saliva swallowed. The fluid which he thus obtained, was probably nothing more than saliva, mingled with the ordinary mucous secretion, of the inner coat of the æsophagus and stomach. Received in this light, all the deductions which he drew from his experiments, and which have been considered by some physiologists as so strongly opposed to the chemical nature of the changes which take place in the stomach, loose their whole weight. The substance which he obtained was not the gastric fluid, and all his experiments and reasonings are inapplicable to that as an agent in digestion.

As this fluid is poured out so readily and in such considerable quantities, Dr. Beaumont infers, that it has been already separated from the blood, and is retained in the minute secreting vessels; and in accordance with this opinion, he considers the sensation of hunger to be occasioned "by a distention of the *gastric vessels,* or that apparatus, whether vascular or glandular, which secretes the gastric juice; and it is believed to be the effect of repletion by this fluid." This opinion he defends at some length, and with much ingenuity, and places it upon as fair a ground of probability as any other explanation of this sensation which has been advanced.

3. The principal, if not the sole effect of the gastric juice, is the conversion of alimentary matters into chyme, by a chemical agency. Dr. Beaumont agrees with most preceding physiologists in the account which he gives of the chyme.

"The resulting compound of digestion in the stomach, or *chyme,* has been described as "a homogeneous, pultaceous, greyish substance, of a sweetish, insipid taste, slightly acid," &c. In its *homogeneous* appearance, it is invariable; but not in its *color;* that partakes very slightly of the color of the food eaten. It is always of a lightish or greyish color; varying in its shades and appearance, from that of cream, to a greyish, or dark colored gruel. It is, also, more consistent at one time than at another; modified, in this respect, by the kind of diet used. This circumstance, however, does not affect its homogeneous character. A rich and consistent quantity is all alike, and of the same quality. A poorer and thinner portion is equally uniform in its appearance. Chyme from butter, fat meats, oil, &c. resembles rich cream. That from farinaceous and vegetable diet, has more the appearance of gruel. It is invariably distinctly acid."

The manner in which chyme is formed, has been examined by Dr. B. with a great deal of care, and his experiments upon this subject are the most numerous and important which he has related. It is well known, that however nearly, different observers have agreed as to the appearance of the contents of the stomach at different periods after the reception of food, their explanation of the causes of these appearances has been widely diverse. The majority of physiologists, since the days of Spallanzani, have agreed with that distinguished and most accurate experimentalist, in considering these changes as produced by chemical action, and the chyme as the solution of aliment in the gastric juice.

Indeed his experiments were so varied and numerous, having been performed upon such a variety of animals, as well as upon man; and were attended with such uniform results, as to be scarcely capable of evasion. There are also the experiments of Dr. Stevens, performed at Edinburgh in 1777, which appear to have been strangely overlooked by many inquirers upon this subject, entirely confirming those of Spalanzani, in regard to the solution of aliment in the gastric juice both within and out of the stomach. Notwithstanding the weight of these facts, there were physiologists who denied entirely the peculiar agency of the gastric juice in the production of chyme; and more who denied that this agency, if it existed at all, was exerted in accordance with chemical laws. All question upon this subject we consider as entirely settled by the experiments of Dr. Beaumont. These have been so numerous and varied, as to leave no room for doubt or cavil. If our limits permitted, we should be glad to insert the most striking of his experiments upon this branch of the subject. As it is, we are restrained to a brief statement of the manner in which they were performed, with their results.

Alimentary matters, of a great variety of kinds, were suspended in the stomach, both uncovered and inclosed in such a manner as only to admit a fluid to come in contact with them. They were uniformly found, when the stomach was healthy, to be in a short time partially, and in a longer, entirely dissolved, and removed from whatever contained them, and a portion of chyme was formed.

Alimentary matters, of the same kinds, were placed in the Gastric juice *out of the stomach*, and the same result followed with the same uniformity.

Alimentary matters of the same kinds, were placed, at the same time, in the stomach, and in the gastric juice out of the stomach, and were examined at regular periods. The changes which took place in each of them were precisely the same.

Alimentary matters, partially digested in the stomach, were withdrawn from it and placed in an additional quantity of gastric juice, and the changes were found to be the same in this, as in the portion left in the stomach.

Experiments of this kind were made, not upon one or two substances only, but upon nearly all the common articles of food. In all the result was the same.

To bring about artificial digestion, it was only necessary that the aliment and the gastric juice should be kept at the natural temperature of the stomach, which was found, upon repeated trials, to be 100° Farenheit. The only difference between natural and artificial digestion was a difference in the time of its accomplishment, the latter requiring from twice to three times the length of the former. This is satisfactorily explained, by the difficulty of maintaining the articles at the exact natural temperature and by the impossibility of imitating perfectly the motions of the stomach. No similar change took place when food was exposed, under the same circumstances, to the action of the saliva.

4. To the formation of chyme it is only necessary that animal or vegetable substances, be exposed to the action of healthy gastric juice, at the ordinary temperature of the body. Mastication, insalivation and deglutition, although they facilitate and render it more speedy, are not essential to the process. The vital action of the stomach, which by many has been believed to be directly operative in the conversion of food into chyme, is shewn to be so only in an indirect manner. Its vital powers are exerted in two ways: first, in furnishing the gastric juice, and secondly in the conversion of fluid alimentary substances into solid, either by their coagulation,* if they are susceptible of this change, as is the case with milk; or by the absorption of their fluid parts if they are not, as when animal broths are introduced into the stomach.

We have enumerated but a few of the facts and principles which are either established or elucidated by the experiments of Dr. Beaumont. There are especially many facts, of general interest in relation to the different digestibility of different articles commonly used for food, which must be passed over for want of space. The following instances will serve to show how great the difference is in point of time, between some of the more common articles. The ordinary time occupied in the complete digestion of a full meal, of a mixture of the common articles of food, is from three to three and a half hours. When the stomach is diseased, or disturbed by narcotics; when the mind is agitated by anger or any other strong emotion; or when the food is in large masses, a longer time is required; and a shorter, when the food has been minutely divided and mingled with saliva, or when the temperature of the stomach, in common with the rest of the body, has been elevated by moderate exercise. Of the

* This is more probably the effect of the gastric juice than of the stomach itself.

common articles of food from the vegetable kingdom, rice, when boiled, requires one hour for its conversion into chyme; barley two hours; green corn and beans three hours, forty five minutes: sweet apples one hour and thirty minutes: potatoes, boiled, three hours and thirty minutes: cabbage, boiled, four hours and thirty minutes. Of the articles of animal food, beef roasted or broiled, is digested in three hours: do. fried, four hours; veal, broiled, four hours: fowls four hours: Pork, roasted, five hours fifteen minutes: oysters, raw, two hours fifty five minutes: eggs, soft boiled, three hours: turkey, roasted, two hours, thirty minutes. Venison, broiled, one hour thirty five minutes.

Without entering into additional particulars and omitting many things of great interest, we commend the work to all who feel an interest in such subjects, as one which contains more *facts*, plainy and honestly stated, upon the subject of digestion in the human stomach, than can any where else be found.

2. *The Cyclopedia of Practical Medicine and Surgery, a Digest of Medical Literature.* Edited by Isaac Hays, M. D. To be completed in 40 parts, 8vo. Philadelphia, Carey, Lea & Blanchard.—Works of this description have been executed on the continent of Europe, with great success, and have been highly instrumental in advancing the causes of medical improvement. Several works of this kind have, within a few years past, emanated from the British press. Cooper's Dictionary of Surgery has been of essential service in promoting the knowledge of that branch of the Medical profession.

Dr. Copland has also been, for several years, engaged in compiling a Dictionary of Practical Medicine, upon a similar plan with respect to medicine; and, as he expresses himself, "to go with," or "be a suitable companion for "Cooper's Dictionary of Surgery." Whether Dr. Copland's intention was generally known to the Medical profession in Great Britain, we know not; nor is it material to know. The British Cyclopedia of Practical Medicine was announced as preparing for publication, either before or simultaneously with, Dr. Copland's work. Some doubts seemed to be entertained in the minds of the medical public, whether both works could succeed. These doubts however have been removed on the appearance of the first numbers of the different productions. The Cyclopedia of Practical Medicine is a collection of Essays on the most important medical diseases; and though a very valuable work, emanating from the pens of eminent men in the profession, whose pursuits had led them to cultivate particular branches of it; yet it does not answer the purposes of a Medical Dictionary.

Dr. Copland's work answers the latter purpose better; but it does not profess to treat of surgical diseases. An American edition of Dr. Copland's work is now in the course of publication at Boston, (Mass.) and proves very acceptable to the medical profession.

Under all these circumstances it might seem a work of supererogation to publish an American Medical Cyclopedia. When we take into consideration, however, that Dr. Hays's Cyclopedia contains Surgery, and also answers the purpose of a Medical Dictionary, we are led to think that in these respects it may prove advantageous, and in some respects more so than the British works above named.

There is another consideration in favor of the American Cyclopedia, arising from a fact known to the medical profession in the United States. The diseases of our country are somewhat different in their nature and treatment, from those of similar classes in Great Britain.

Dr. Hays has availed himself of the assistance of some of the most eminent medical gentlemen in various parts of the U. S. The medical student and practitioner may therefore expect to have a more accurate description of the nature and treatment of diseases as they occur in our own country. Dr. H. has also the advantage of consulting those works of a similar character that have been published or are now publishing in Europe.

More attention has been paid by Dr. H. to the auxiliary branches of Anatomy, Physiology, Chemistry, Materia Medica and Botany, than was consistent with the design of the editors of the British works of a similar kind.

A copious Bibliography is attached to the different articles in Dr. Hays' work, which will facilitate the more extensive researches of the medical student and practitioner. We congratulate the members of the medical profession in our country, on the appearance of the work.

So far as it has proceeded, we believe the execution of the work has answered the expectations of the medical profession. Dr. H. has given evidence of his great industry and good judgment, in the compilation of the several articles, and we wish him success in his undertaking.

3. *Obituary of* Gen. Martin Field.—Gen. Field died at his residence in Newfane, (Vt.) in October last, at the age of 60. The early part of his life was assiduously devoted to the profession of law, in which, for many years, he was highly distinguished. On account of an incurable deafness, he, several years since, declined the active duties of his profession, and as a resource to an energetic mind, and a solace in hours that might have been tedious for want of some interesting object of pursuit, he turned his attention to scientific investigations. When he was educated, the natural sciences were scarcely studied in the schools, and much less extensively than now in the colleges of this country; he was therefore obliged to commence with the elements.

Meeting with the American Journal of Science, a new stimulus was given to his efforts, and a proper direction to his researches. He obtained the best scientific works, and sought the acquaintance of those who were pursuing the same path, or who had already made attainments in science. Commencing with mineralogy, he, for a time, was zealously engaged in collecting a choice and beautiful cabinet; but he found, that in order to become a skillful mineralogist, there was a kindred science to be grasped, and one without which he could not penetrate beyond the surface of the mineral kingdom; he saw that it was beautiful and curious, and felt a desire to know those mysterious laws of combination by which, from a few elements, the wonderful variety of material things is produced. This desire led him to the study of chemistry. He purchased chemical books and apparatus, and for a time, directed his inquiries to the elements of matter, and the laws by which they are governed.

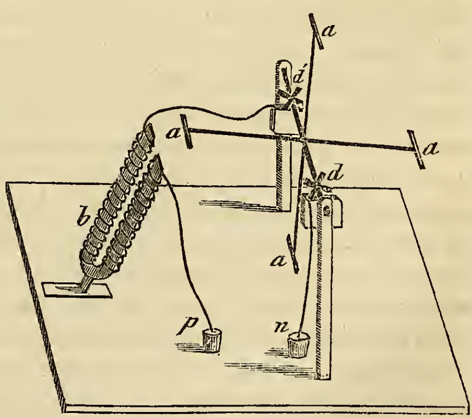
A mineralogist and chemist has attained two important requisites to enable him to become a geologist. Gen. Field was not satisfied with examining nature in his cabinet, and with reading the observations of others. He was, in science, what may be termed a working man. Few points of interest were there among the romantic scenery around him, that were not familiar to him; and many a rugged precipice, deep glen, and lofty summit of the Green Mountains, never before trod by human footstep, can bear witness to his persevering research into the nature and arrangement of the rocky strata, of which they are formed. In such expeditions, curious living reptiles and insects presented themselves, and fossil remains of beings that once had life, were found imbedded in the rocks*; he believed that

* In other regions than the green mountains, which are primary rocks.—*Ed.*

nature could not be less systematic and less interesting in her arrangement of living things, than in the inanimate creation ; and he was thus led to the study of Zoology. He was also a practical Botanist, and found health and contentment in the cultivation of plants. His minute observations of philosophical facts have been, in various ways, manifested in the pages of the American Journal of Science, a work in which he ever delighted, and to which he felt himself indebted for much of that love of science, and those acquirements which enabled him to endure, with cheerfulness, a misfortune by which he was in a measure cut off from the social enjoyments of life. It is a great thing for a man who has been active in business, to withdraw from those scenes in which his mind was stimulated to constant effort, to see the place he has filled occupied by others, and to feel that the world can move on without him ; but this condition is incident to human nature. Fortunate then are those who, at such a period, can, like him who is the subject of this sketch, find in the contemplation of the works of God, a resource against ennui, and a security against bitter and unavailing regrets. A. H. L. P.

We are indebted for the above notice, to the pen of a lady, well known and much respected in this country. Gen. Field we knew only as a correspondent, but he was a much valued one, and a steady friend to this Journal. We sincerely condole with his friends and his country.—Ed.

4. *The Rotating Armatures, by T. Edmondson, Jr., Baltimore.*



This instrument is intended to produce the rotation of a set of armatures, by causing a current of galvanism to pass, at certain times, through an electro-magnet, placed near the circle described by their revolution. The armatures are attracted by the induced magnetism as they approach the faces of the electro-magnet, and by the arrangement of the instrument, the current of galvanism is suspended, and of course the induced magnetism withheld, as the armatures recede from the electro-magnet. The armatures are nicely fixed upon an axis, and made to descend parallel to the faces of the magnet, and as they are attracted and suffered to pass, the momentum which at the time of passage is only left, brings the others into action.

a a a a are the armatures—*b* the electro-magnet, placed at an angle towards them and wrapped with a single coil of coated copper wire—*d d′* two small brass stars placed on each side of the armatures, and soldered to the axis. The points of the stars are made to descend into quicksilver troughs, placed underneath them, and to come into contact with the mercury at the time the armatures approach the electro-magnet, and to revolve out of the quicksilver as they recede from the electro-magnet; it is only at the time of their contact, that the current of galvano-magnetism, proceeding from *p*, where the positive pole of a galvanic element is placed, can pass through the coil to *d′* and along the axis by the intervention of the small stars, to *n* where the corresponding negative wire of the element is placed. The instrument will revolve for several hours.

5. *Remarks on Steam, as a Conductor of Electricity.*

Frederick, Md. Jan. 13, 1834.

Prof. Silliman—*Sir*—Upon reading an article in the April No. of your Journal for 1832 with the caption "Steamboats protected from the effects of Lightning" it appeared to me that the writer had subjected himself to unnecessary trouble and expense, to assure himself of a fact whose existence he might have predicted with absolute certainty. And in what manner? By asking the question, why is it, that an electrical machine cannot be charged in a moist atmosphere? Clearly, because the moist atmosphere, being an excellent conductor of Electricity, dissipates or conducts it away as fast as the machine generates it. Now, the only difference between a moist atmosphere and steam, is, that the vapor in the latter case is connected with a larger proportion of caloric. And therefore, methinks there is nothing novel in steam being a conductor of electricity. The writer goes on to say, "It is therefore pretty well proved that "*the steam generated in a steamboat completely protects it from the effects of lightning.*" So far from "the steam generated in a steamboat." protecting it from the effects of lightning, it may be proved, we think, from the writer's arguments that it has an opposite tendency. What does he say. These are his words. "The electricity of the clouds that would otherwise, in many instances, strike the steamboats loaded with so much iron; on coming in contact with the moist and heated column of steam, which ascends above the boats, immediately diffuses itself through the column of steam, and passes to the water without communicating any shock, or, in other words, the ascending steam performs the office of a Franklin rod." And again, "The stream being in such a case *a much better conductor than the iron* of the boat, the Electricity will always take *the steam in preference to striking any part* of the boat, and this it will do in so diffused a manner as never to be perceived or felt." It is to be inferred from the foregoing, that the safety valve of a Low-Pressure boat is to remain open during a thunder storm, so that the steam may rise above the boat and "perform the office of a Franklin rod."

It is clear then, that by furnishing this "Franklin rod" the electricity is not only invited but actually introduced into the *steam in the boiler*. Facilis descensus Averni, &c., the consequence of which is that a large quantity of caloric is evolved, which enters into the steam. The sudden expansion produced by this heat, may be so great that the boiler cannot resist it and an explosion is unavoidable. This sudden expansion, coupled with the additional quantity of steam which may be produced by the electric heat transmitted through the steam to the water, is we think, amply sufficient to cause an explosion. From these remarks, it appears, that steam so far from protecting steamboats from the effects of lightning, has rather a tendency to produce them. Probably one cause of their exemption from accidents of this nature, in addition to that assigned by the Editor in his "Remarks," is their want of masts. Their chimnies, may possibly not be sufficiently elevated to determine a cloud charged with electricity. With respect &c. GEO. SCHLEY.

6. *Sulphuric Acid.*—The Editor or any of his numerous scientific correspondents will much oblige an "Inquirer" if they will give the reasons for, and point out the sources of failure in the manufacture of sulphuric acid. Every chemist practically engaged, is aware, that although the same chamber and the same quantity of sulphur and nitre is used, yet the product of acid is extremely fluctuating. The books are unsatisfactory on this point. Gray in his "Operative Chemist" says, "as the cause of this circumstance (viz. the acid not being condensed) was not known, it was attributed to the chambers, which were said to be *sick* and would not work."

7. *Observations on the time of the appearance of the Spring Birds, in Williamstown,* (*Mass.*) *in the years* 1831, 1832 & 1833; by E. Emmons.

	Month	1831.	1832.	1833.	
Saxicola sialis, Bon.	*March.*	10 -	12 -	19 -	Sometimes a few appear in Feb., and even in Jan, in sheltered places. The blue bird uniformly precedes the robin.
Turdus migratorius, L.		12 -	14 -	18 -	
Sturnus Ludovicianus, L.		13 -	12 -	13 -	
Falco velox, Wils.		23 -		20 -	Appear quite numerous in September, associating with F. *Sparverius* and *Columbarius*.
Caprimulgus Virginianus, Briss.		22 -	20 -	24 -	Generally heard several days earlier, just at evening.
Muscicapa fusca, Gm.		23 -	20 -	20 -	
Icterus phœniceus, Daud		23 -	21 -	20 -	Appeared in February, in 1830.
Bombycilla Carolinensis, Briss.				20 -	In a flock. Did not remain. Weather still cold, and considerable snow.
Columba migratoria, L.		24 -		22 -	Irregular in the time of their appearance and continuance.
Scolopax minor, Gm.	*April.*	1 -	2 -	4 -	Often heard a few days earlier in the air, over marshy places, at twilight.
Picus auratus, L.		10 -		12 -	
Turdus Wilsoni, Bon.		10 -	11 -	12 -	
Charadrius vociferus, L.		13 -	12 -		
Hirundo rufa, Gm.		19 -	20 -	19 -	Sometimes seen earlier, flying high in the air, but do not remain.
" fulva, Vieill.					Ten days later than the H. *rufa*; first appeared in 1825.
Muscicapa tyrannus, Briss.	*May.*	1 -	2 -	4 -	
Cypselus Pelasgius, Temm.		3 -	4 -	1 -	
Sylvia aurocapilla, Bon.		7 -	6 -	9 -	
Turdus felivox, Vieill.		8 -	6 -	7 -	Remaining often till late in Germany, subsisting on the seed of various plants.
Fringilla tristis, L.		10 -	11 -	10 -	At this period the songsters of the woods and fields have all arrived, busily engaged in preparing their nests and rearing their young.
Tanagra rubra, L.		10 -	12 -	12 -	
Icterus agripennis, Bon.		12 -	10 -	9 -	
" Baltimore, Bon.		12 -	10 -	11 -	

Williamstown, Aug. 1, 1833.

8. *Recent Scientific Publications in the United States.*—Manual on the cultivation of the Sugar Cane, and the fabrication and refinement of Sugar. Prepared under the direction of the Hon. Secretary of the Treasury, in compliance with a resolution of the House of Representatives of Jan. 25, 1830. City of Washington. Printed by F. P. Blair, 8vo. pp. 122 with 4 plates.

Outlines of Geology: intended as a popular treatise on the most interesting parts of the science, together with an examiation of the question, whether the days of creation were indefinite periods. Designed for the use of schools and general readers. By J. L. Comstock, M. D. Hartford, D. F. Robinson & Co. 12mo. pp. xii, and 336.

A manual of the Ornithology of the United States and of Canada. By Thomas Nuttall, A.M. F.L.S.—Part 2. *The Water Birds*. Boston, Hilliard, Gray & Co. 12mo.

The Conchologist, with 17 plates. By John Warren, Boston, Russell, Odiorne & Co. 4to. pp. 204.

Republications.—General View of the Geology of Scripture, in which the unerring truth of the Inspired Narrative of the early events in the world is exhibited, and distinctly proved, by the corroborative testimony of physical facts, on every part of the earth's surface. By George Fairholme, Esq. Philadelphia: Key & Biddle, 12mo. pp. 281. (First republished in the Christian Library, Vol. 2.)

Alphabet of Botany for the use of beginners. By James Rennie. Revised and corrected for the use of American Schools, by Arabella Clark, principal of the Female Department, Mechanics' School, New York, 18mo. pp. 130. 1833.

A Treatise on Astronomy. By Sir John F. W. Herschel, Knt. Guelp. F.R.S.L. and E. &c. Philadelphia, Carey, Lea and Blanchard, 1834, 12mo. pp. 296.

9. *Cabinet of the late Dr. William Meade.*—This collection is offered for sale by Mrs. Catherine Meade of Newburgh, in the State of New York, at which place, Dr. Meade resided, during the later years of his life. We have not seen this collection, but we have the best reason for believing that it is both valuable and interesting. We have already stated (Vol. 25 pa. 216 of this Jour.) that Dr. Meade was, for twenty five years, an active collector of minerals; that he visited and explored many of the most interesting mineral deposits in the northern states, and that he was in the habit of exchanging specimens with eminent mineralogists abroad.

We are informed that his collection contains twelve hundred, well selected and fine foreign specimens; and about two thousand, belonging to four hundred varieties, of the best American minerals, of a large size. There is also a very beautiful small collection of fossils and slate impressions.

We are authorized to say, that the collection will be sold on the most reasonable terms either entire or in divisions. It would doubtless be an important object for one of our junior colleges.

A few hundred dollars, judiciously expended in forming the nucleus of a collection, will, with zeal and energy, soon produce an important effect in diffusing the knowledge of mineralogy; and a collection thus begun will grow beyond even the most sanguine hopes. We would therefore invite the attention of mineralogists and of schools and colleges to this collection and we presume that they will not find its value overrated.

We are requested also to mention, that the patent right for the Congress powders and the recipe for preparing the magnesian aperient are for sale. Application for the purchase of them or of the cabinet can be made to Mr. Charles Conolly, at Cornelius Dubois Esq. of New York.

10. *Ewbank's Tinned Lead-pipes.*—Mr. Thomas Ewbank of New York, has invented a method of tinning lead pipes "after they have been drawn to the proper size." This is ingeniously accomplished by drawing the lead tubes (properly prepared with rosin on their surfaces) through a bath of melted tin, kept at such a temperature as to avoid the fusion of the lead.

We have seen some of these tubes and their appearance promises a perfect protection to the lead. Should this, after sufficient trial, prove to be the fact, the discovery will be of great importantce, especially for aqueducts.

We have just seen a portion of a lead tube of two inches in diameter, which, having been laid down in a low meadow in Springfield Mass. where there are the remains of an ancient hemlock swamp, was, in the course of a few months, corroded through and through, and for this reason great expenses are incurred in taking up and replacing the spoiled tubes. We presume that the tinned tubes will not be liable to this accident.

11. *American Mangle or Domestic Callender.*—This instrument, invented and patented by Mr. I. Doolittle of Bennington, Vt., we have seen used and we have conversed also with those who have employed it, and find that its use saves a great portion of the labor and all the fuel usually employed in the process of ironing table and bed linen, towels, &c. besides being much more expeditions and giving the articles a better lustre and whiter appearance. It is regarded as a valuable auxiliary and by some is reckoned among the indispensable utensils of the laundry. We can confidently recommend it as a valuable acquisition to the conveniences of the family.

12. *Price of Platinum—Test Paper.*

Extract of a letter from Dr. Erastus F. Cooke, to the Editor, dated Wethersfield July 22, 1833.

Dear Sir—In your reply to a letter of mine, (sometime since, inquiring the price of platina,) you expressed a wish to know something of the result of my enquiry. I have to state, that I procured some friends to write to London and to Paris in relation to the subject, and I now send you the result. "We have just received an answer from

Paris, in relation to the platina alembic. Our partner says, that the alembic with the snout and syphon, of the most approved model, such as are made for the London market, to contain about 25 gallons, will weigh 600 oz. and that we should be able to deliver it here for $6 per ounce, or about $3600 for the machine."

The letter from London says—" A platina retort, with flange for concentrating sulphuric acid, holding 30 gallons, imperial measure, will weigh about 450 oz. at 24*s* per oz.=£540. A platina syphon suitable for ditto, will weigh about 45 oz. at 26*s*=£58 10*s*, made in the best manner." This letter was from R. & E. Kepp, 41 and 42 Chandos street, London. The first letter was from the Messrs Carnes, of N. York.

I take the liberty of sending you a piece of test paper, made from the purple Cabbage. I use it in all my manufacturing operations, where such a test is necessary, and find it a good one. The paper ought to be unsized. I do not recollect that I have ever seen Cabbage *paper* mentioned in any work on tests. The *infusion* I know, is in common use.

13. *Baker's Bread.*—To the Editor.—*Sir*—You will oblige one of your readers, in answering through the Journal of Science, the following question :—Does Baker's bread contain *any alchohol?*

The last No. of the Edinburgh Review, page 107, speaks of a patent taken out in England " for distilling from the quartern loaf, by collecting the spirit which evaporates during baking." Yours respectfully. *An inquirer.*

Reply.—Our readers are probably aware, that the generation of alcohol in the fermentation of dough, has been denied by the French chemists. Having never repeated the experiments, I can say nothing on my own authority.

As regards any scruples that may possibly be entertained by our correspondent or others, as to eating bread, it is obvious, that if there be alcohol in dough, it will not remain in the bread when baked, it will be expelled by the heat of the oven, and upon this fact is founded the arrangement for deriving profit from the alcohol supposed to exist in the dough. Baked bread evidently contains no alcohol.

14. *Loss of memory from the use of gin.*

Extract of a letter to the Editor, from a man of science, who had been a severe sufferer from asthma.

My health is better than it has been, since my asthmatic affection commenced, in 1824. I have been enabled to dispense with my hops and Holland gin preparations (tincture of hops) for about one year. I imagined that the gin part injured my memory; although my physicians would not assent to it. Since I am in a measure independent of its use, I am sure my memory is restored. Alcohol has its uses; but still, I verily believe, that none but Armstrong's "athletic fools" (where want of brain is compensated by having brawny limbs) can use it in the stomach with impunity. I had never swallowed a thimble-full, until I was forty-eight years old. I neither drank cider, nor wine, nor beer. While I used only a very little, as a tincture of hops, my mind seemed clouded, and I could not remember as well. Wine never aids me in the asthma; therefore I have tried the effects of old Holland gin only. It relieves, temporarily, by promoting expectoration. I assure you, that after a trial of several years, I have scarcely any confidence in any preparation of alcohol.

15. *Outlines of Geology, &c., by Dr. J. L. Comstock.*—Dr. Comstock is advantageously known to the public, by the compilation, with additional elucidations of his own, of several treatises on different branches of science, for the use of schools.

His last production is that whose title is stated above. In this work, Dr. C. gives sufficient proof, that he has industriously and carefully examined the principal modern treatises on geology; and his abstract, which abounds in interesting and important facts, will serve a valuable purpose to those who have not time and opportunity to examine the original works, and still less, the numerous original memoirs and reports which have supplied the materials. Dr. Comstock's "Outlines" are perspicuously written, and are illustrated by copies of diagrams and figures from the various works that were consulted.

A fuller description of the rocks would perhaps have been desirable, but this deficiency is, to a degree, compensated, by the facts cited, to support the general views contained in the work. As to *the days*, Dr. Comstock, (in common with the high authorities whom he has cited) has left the question embarrassed with all the geological difficulties. If we pay any regard to the Mosaic history, the remains of both organic kingdoms, must be disposed of, under the days, and it is impossible that they should be, upon the present limited view of time. All attempts which we have hitherto seen to solve this difficulty, without more time than the common interpretation allows, (not more than the history, fairly considered, permits) are in our view, utterly nugatory; the title of the last chapter of Rasselas, would well describe them all.

We object to the imputation of *contradicting* the Mosaic history, when christian geologists endeavor to prove that there is a sense in which philology and geology may be harmonized, and the facts and the record stand together in mutual consistency. Geology contradicts nothing in the sacred history ;—all that it requires is an extension of time ; whereas the modern astronomy is in exact opposition to the literal sense of the language of the bible ; still no one now dreams of any real inconsistency between them.

16. *Prof. Hitchcock's Report on the Geology, Mineralogy, Botany and Zoology of Massachusetts:* 1833.——We have no more time or space, than to make a passing remark upon this great work ; the most elaborate and complete in its kind which this country has produced.

The Geology is divided under the heads Economical, Topographical and Scientific ; and a fourth part contains a catalogue of animals and plants. There is also a descriptive list of the specimens of rocks and minerals collected for the government. The work is illustrated by numerous wood cuts, and a distinct atlas of plates : it fills 700 pages, and evinces, throughout, great zeal and industry, with sound scientific views, and much sagacity and discrimination. We have already had frequent occasion to consult this work, and always with much satisfaction, and with increasing respect for its meritorious author. Laboriously occupied as we know him to be with academical duties, we are surprised that he has been able to accomplish this arduous work in so short a time.

We are gratified to learn that the government of Massachusetts have already ordered a second edition to be printed ; this will afford opportunity for literary corrections, but we are sorry to learn that the respected author is not empowered to make any additional researches, and we much fear from what we learn of the entire cost* of this survey, that only a very small remuneration can have fallen to the share of the man who has accomplished a labor truly Herculean, involving a heavy responsibility ; a work which reflects great honor upon the State, and upon the enlightened and patriotic chief magistrate,† whose energy and perseverance carried the measure through.

17. *Second American Edition of Bakewell's Geology.*——This work, reprinted by H. Howe from the fourth London edition, is much improved by the author's revision. He has added several new

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* Not however, from the author.

† Gov. Lincoln.

chapters on important subjects, and has posted up the science to the date of his preface, April, 1833.

The favorable opinion which, many years ago we formed and expressed respecting this work, is now, at last, fully confirmed by that of the British scientific public; namely, *that Mr. Bakewell's Geology is admitted to be the best elementary work for instruction on that subject in the English language, and perhaps in any language.* We understand from direct and unquestionable sources, that this is now admitted in the Universities of England and Scotland, as well as by eminent* geologists in various parts of the United Kingdom. After investigating geological subjects through many volumes of the various existing treatises, we have usually returned to Mr. Bakewell for a final judgment, and have been often forcibly struck with his judicious summary of geological facts and opinions, and with the rare combination of copiousness, condensation and perspicuity, which his work presents.

18. *Magnetism.*—Dr. Locke, has within the last week, invented and constructed a very delicate Thermo-electrical battery, intended to show the electricity produced by animal heat. It is of the size of three sixpences laid upon each other, that is, three-fourths of an inch in diameter, and one tenth of an inch thick. This battery, being attached to his galvanometer, and the end of the finger applied to the top of it, the animal heat thus communicated to it, caused a current of electricity, which turned the magnetic needle 90°. Independently of this little battery, the two following experiments, have been made by the galvanometer. One fourth of a grain of metallic antimony being interposed between two copper wires, and the warmth of the finger applied, the needle was deflected 22°. One of the copper wires being laid upon the other, without the interposition of any other metal, and the warmth applied as above the needle was deflected 6°. The same results were obtained by substituting the warmth of the breath instead of that of the hand. It may be proper to note that the temperature of the room in which these experiments were made, was 65° Fahrenheit.

It has thus been actually shown that the production and expenditure of animal heat, are, constantly, attended by electrical and magnetical currents.—*Cincinnati Paper.*

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* It has been used since 1815, in connexion with the geological lectures in Yale College, and two editions of the work, each with an appendix, have been published under the revision of the Professor in that department.

19. *Mr. C. U. Shepard's private school of Mineralogy and other Branches of Natural History.*—In our last No. (Vol. 25 pa. 431) we mentioned this important private undertaking. We are happy to add, that several gentlemen, accidentally associated, from remote states and a still more remote foreign country, are now availing themselves of the important advantages which Mr. Shepard is able to afford them. We refer to our previous notice of this subject, as cited above; and with pleasure add, that the experiment is in very successful progress, and that not a doubt can be entertained of the entire success of this school, *provided a sufficient number of pupils should attend, to afford a fair compensation to the highly qualified gentleman whom* (with no other interest than that of a kindly feeling towards science and towards this its meritorious devotee,) *we are proud to recommend*, to the American public as entirely worthy of that confidence which he would be as slow to ask as he will be prompt to deserve. It is our earnest wish that this high advantage may be added to the other means of scientific instruction enjoyed in our country. We have the satisfaction to add that, since our last No. a small fund, contributed by the society of the Alumni of Yale College, has been made, in part, available to afford without charge to the pupils a course of lectures on conchology which in the cabinet of Yale College, Mr. Shepard, is now engaged in delivering, with the fine illustrations afforded by his beautiful collection of shells.

This course will be followed by one on Botany, by Mr. Shepard, for which, as there is no fund to support it, a small fee is paid. A public course of Mineralogy succeeded by an extended one on Geology is given, every spring and summer, in the Cabinet of Yale College; and this, for the present, completes the list of courses of instruction here in Natural History. We hope however to see Zoology added at a future day. There are full courses on all the branches of theoretical, and experimental science; as well as in medicine, law and divinity, in addition to the instruction by recitations and drilling in the class-rooms.

20. *Ligneous Stems of American Coal-fields desired.*—We take the liberty of inviting the attention of our scientific friends and correspondents to the interesting researches, now going on, in the hands of H. T. M. Witham, Esq. of Edinburgh, and also of Lartington, Yorkshire. Of that gentleman's discoveries we gave a notice in this Journal, Vol. 25, pa. 108; he is still prosecuting them with ardor

and success, and as we understand from him, that he is very desirous of receiving specimens or sections of the coal plants of this continent, whether from the bituminous or anthracite beds; we take the liberty of soliciting aid for him in this research which is so important to geological science; and if specimens are transmitted to us it shall be our care to forward them with expedition. Early in our editorial labors we made a similar request in behalf of Mr. Alex. Brongniart, and it was not without success in promoting the very interesting discoveries of his son, Mr. Adolphus Brongniart, respecting the Flora of the ancient world and especially of the coal formations, upon which he has thrown such important light.

21. *Crystalline Lenses of American Animals Desired.*—On this subject we have to prefer a request similar to the one stated above. We learn from Sir David Brewster that he has been engaged, for many years, in an examination of the crystalline lenses of animals and has just published in the Philosophical Transactions the first of a series of papers on the subject. As there are many fishes in America, which cannot be obtained in Europe, their crystalline lenses and especially those of the cuttle fish as well as those of any animals peculiar to this continent would be particularly acceptable to Sir D. B. whose brilliant researches in optics have shed lustre on his name and on this branch of science.

It is necessary, only, to throw the lenses, for a few seconds, into boiling water; they are then taken out and dried and wrapped in paper upon which should be written the name of the animal to which they belonged. We respectfully invite the aid of naturalists and especially of ichthyologists upon this subject. It is among the rewards of scientific zeal and labor that the friends of science and liberal knowledge are thus led to cherish a kindly feeling towards their co-workers in distant countries, which is as favorable to their personal happiness, as it is to the prosperity of the common cause.

22. *Mantell's Geology of the South East of England.*—This fine work contains a synopsis of all Mr. Mantell's Discoveries in the very peculiar and highly interesting district in which he resides. He has given to the scientific world, in an elegant octavo, the principal things contained in both his former quartos, with the addition of many new facts, the most interesting of which is the discovery of a new fossil Saurian of enormous size, called by him the Hylœosaurus. It was

our intention to give, in the present number, something like an analysis of this work; but, in substituting a meagre paragraph, we only yield to necessity, which has absolutely withheld the leisure requisite for our purpose; duties which could neither be postponed nor avoided, have entirely forestalled the future, and engrossed the passing hour. We have elsewhere remarked, that Mr. Mantell's various publications on local geology entitle him to rank with Cuvier and Brongniart, whose grand work on the environs of Paris, led the way in this species of research. Mr. Mantell's late work presents the results of a course of detailed and exact induction, involving an extensive and precise knowledge of several collateral sciences, and especially of comparative anatomy, conchology, botany and zoology.

Mr. Mantell, aided by the talent, taste and zeal of Mrs. Mantell, has rendered Lewes and its environs more famous for its geological, than it was before for its historical antiquities. Mr. Mantell has recently removed his residence to Brighton, upon the sea shore, seven miles from Lewes.

He has refitted his admirable museum,* and enlarged its accommodations, and we trust, that in his arduous profession† he will, at Brighton, meet the rewards to which he is so well entitled from his countrymen. Few persons, even in this country, need to be informed that Brighton is, in the summer, a grand focus of the fashion, rank and wealth of England, and moreover, during a part of the year a favorite Royal Residence. If Mr. Mantell's museum should bring him, in its present situation, many new calls, we trust they may prove advantageous to his well earned fame, and to the interests of his amiable family.

23. *Septaria of extraordinary size and beauty.*—These curious combinations of different mineral substances, are usually seen in cabinets, only of a few inches in diameter. We owe to the kindness of Mr. Mantell two magnificent specimens of Septaria, (one of them for the cabinet of Yale College.) They are respectively 22 and 24 inches in diameter, forming circular polished tables of exquisite beauty. They are from the Lias, an argillaceous limestone, and their locality was near Lime-Regis on the channel coast of England. Their prevailing ground is smoke grey, but they are superbly variegated, by both broad and narrow veins of calcareous spar of a light straw color,

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* Described in Vol. xxiii. p. 162 of this Journal. † That of a surgeon.

which, with the bordering edges of dark iron, and (apparently) sulphate of barytes, winds its way, in the most delicate flexions, through the sober ground work of the lias, and forms a picture, not unlike that of the ramifications of a great Delta, whose bright waters inosculate and cross in many mazes, dividing the territory into innumerable islands. It is not easy for an observer to persuade himself that these tables are natural pictures; the first impression is, that they are a kind of mosaic work.

24. *Fossil Jaws of the Tapir.*—Extract of a letter from Gideon Mantell, Esq., late of Lewes, now of Brighton, England, to the Editor, dated Jan. 1834.

"They have found two perfect lower jaws of the tapir at Darmstadt; strange to tell, it had two tusks at the anterior extremity of the lower jaws, and which point *downwards.* Was ever any thing so extraordinary: they must have been intended to enable the animal to grub up bulbous and tuberose roots from under the wattled fibrous roots of a forest!"

25. *Chalk and chalk fossils in granite.*—Extract of a letter to the Editor from Prof. Leonhard, of the University of Heidelberg, Germany, dated 29th Jan. 1834.

"I have performed recently a geological tour in Bohemia and Saxony, and have observed a multitude of interesting facts; among other things at Meissen upon the Elbe, fragments of chalk full of petrifactions—*imbedded in granite.*"

26. *Obituary.*—Died at Bethlehem, Pa., (the place of his birth,) early in February last, the Rev. Lewis D. De Schweinitz, the secular head of the Moravian Society, or Unitas Fratrum in America, aged about 52 years. Several of his early years were spent in the pursuit of study in Germany, during which period he devoted considerable attention to the investigation of cryptogamous plants. After his return to this country, the confidence of his brethren gave him an ecclesiastical charge in one of the Moravian settlements in North Carolina. While on that station, he employed a part of his time in studying and arranging the fungi of that region.

His various scientific publications are in great esteem among the learned, and justly entitle him to an eminent place among the botanists of his time. A list of his works, so far as we are acquainted with them, is given below.

He was indefatigable in the discharge of duties, conscientious and consistent on the subject of religion, and persevering in every description of study. His loss will not be less regretted by those who had the advantage of knowing him as a friend and companion, than in his more public character, in which it will be long and deeply felt both by the Moravian Church and by the friends of science.

Conspectus Fungorum in agro Niskiensi crescentium; socio J. B. ab Albertini, Lipsiæ, 1805. 8vo.

Specimen Floræ Americæ Septentrionalis Cryptogamicæ, 8vo. Raleigh, N. C. 1821.

Monography of the North American species of the Linnæan genus *Viola,* Am. Jour. of Science, Vol. 5. p. 48–81. 1822.

A Monograph of the N. A. species of Carex, Annals of the N.Y. Lyc. Vol. 1. p. 283—373. 1825.

Synopsis Fungorum in America Boreali media degentium. Trans. of Amer. Phil. Soc. for 1832.—*Poulson's Daily Adv. Feb.* 15.

The American Journal of Science and Arts.

The annexed prospectus is presented to the friends of science, and their aid is respectfully solicited, in promoting its interests so far as they may be thought to be connected with this Journal.

Since the 12th volume, its patronage, has been more than sufficient to pay the expenses, but to insure the stability and usefulness of the work requires renewed efforts, on the part of its editor.

As even England had no Journal of Science till about the beginning of the present century, it is cheering to remember, that the first attempts in this country, made only a few years later, have been, thus far, sustained by the public. Still, every periodical work must, occasionally, recruit its number of subscribers, or it will fall into jeopardy. The American Journal is not yet in immediate danger, but, *its subscription is far too limited to do all the good of which it is capable; and after a gradual decline, since* 1829, *it would be happy if it could be again increased as it was in that year. The simple expedient then adopted, was, for each subscriber to obtain one subscriber more, and in this manner the subscription was soon doubled.*

In this country, such a work, involving peculiar difficulties, can neither be got up, nor sustained, without great effort and perseverance. Avoiding local, personal, party and sectarian interests and prejudices, it thus entirely foregoes the support afforded by popular feeling, and therefore relies, *as it has a full right to do*, solely upon the intelligent, the patriotic, the philanthropic, and the interested.

It is worse than useless, to resort to indiscriminate solicitations. Subscriptions, obtained in that manner, will not continue long, and will produce only a delusive expectation of support, and a certain increase of expense. *Such persons therefore, and such only, should be addressed, as, from their considerate and correct estimation of the value of useful knowledge, or from their interests and taste, will probably become permanent patrons.*

PROSPECTUS.

In 1810, 11 and 12, the late Dr. Bruce, of New York, published his Journal of Mineralogy and Geology in one volume of four numbers. The American Journal, was however, the first, that in this country, embraced in its plan, *the entire circle* of the Physical Sciences, and their applications to the arts. It was begun in July, 1818, and has completed its twenty fifth volume.

While it has prompted original American efforts it has been sustained by them, and being devoted to important national interests, in a great measure common also to all mankind, it is, in that character, known and accredited, both at home and abroad. It has elicited many valuable researches and discoveries, and its miscellaneous department has presented a great variety of topics, of general interest. The Foreign Journals, (many of them sent in exchange,) often quote from its pages, which are in turn, enriched by theirs; and it has thus, become identified with the science and arts of the present day.

Terms.—For four quarterly Nos., of not less than 200 pages each, fully illustrated by plates, making, together, two annual volumes, of at least 800 pages; six dollars—*in advance.*

The quarterly *literary* journals, escape the heavy expense incurred by this, for plates; and as they enjoy, from obvious causes, a far more extended circulation, they can be much better afforded at $5 per ann. than this at $6. *With its present patronage, this Journal could not be sustained at five dollars, as the actual receipts would not pay for the paper and the mechanical labor.*

Complete sets, at a proper discount, are furnished, to order, in Nos. or bound. *Postage is to be paid on all orders and remittances, but not on communications. Postmasters are occasionally patrons of the Journal,* when of course their communications are franked.

A number is sent gratis, as a sample, when requested. Names may be lodged with any of the agents, or sent to the Editor or publishers, and the work may be obtained through all booksellers.

A compensation of one third will be allowed to all persons obtaining subscribers who pay the first year's subscription in advance; and agents and booksellers can, if they choose, retain upon their own books, the names which they may procure; due notice being given to the Editor.

For single subscribers, the mail is, decidedly, the best mode of conveyance: the postage is about that of a twice weekly newspaper, that is from $1.10 to $1.32 per annum.

J.J. Thomas del.

J.W. Barber sc.

Tab. T.

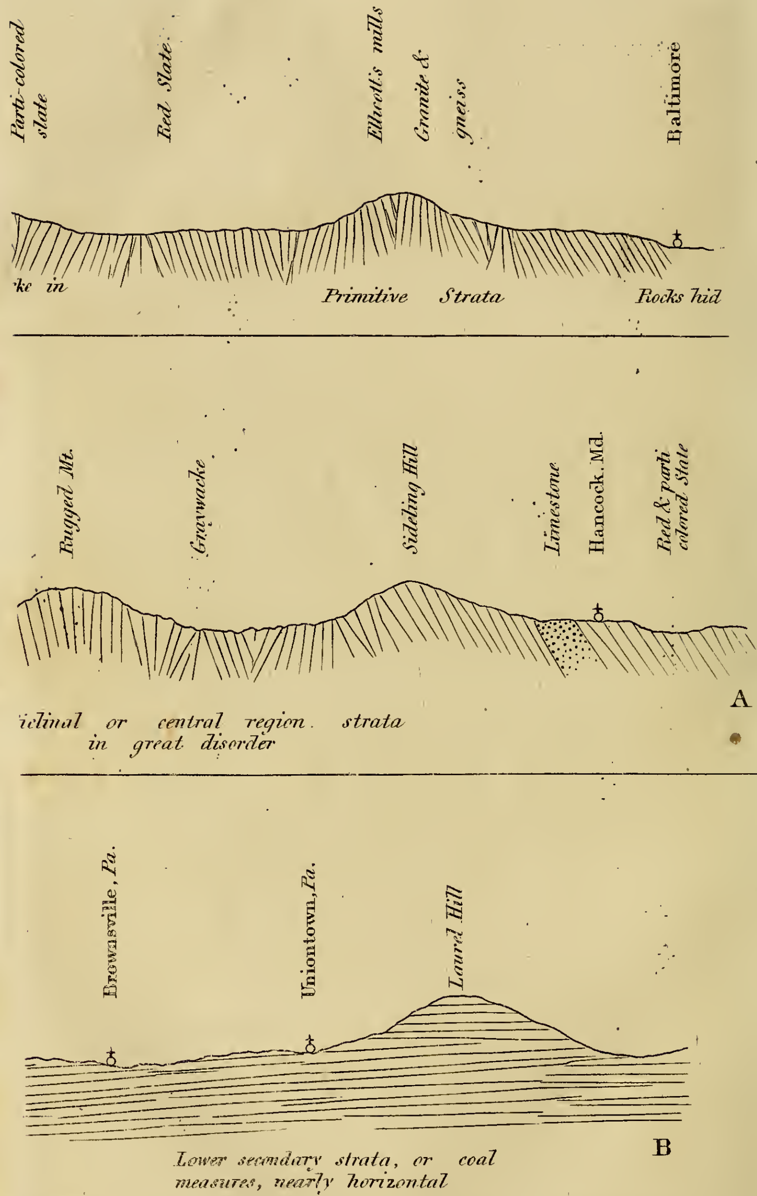
Fig. 61.

C. *Baldwinia*, D.

Vol. XXVI. *p.107*

Fig. 62.

C. *venusta*, D.



n BALTIMORE and the OHIO RIVER.

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

ART. I.—*Some notices of the Geology* of the Country between Baltimore and the Ohio River, with a section illustrating the superposition of the rocks;* by DR. WILLIAM E. A. AIKIN, Prof. of Nat. Phil. and Chem. in Mt. St. Mary's College, Md.

THE geology of the Appalachian chain has always appeared sufficiently enigmatical to me to merit minute investigation. The structure of its eastern ridges seems inexplicable, without reference to the operation of a cause, that must have been most intensely exerted along a line far to the west of any point, that I had an opportunity of examining previous to the last season. The following observations are the results of that examination, an examination much more cursory and hurried, than I could have wished, but yet one which has furnished some general results, that in the absence of more minute information may prove interesting. Moreover, it is hoped they will have a tendency to remove the darkness that has hitherto encompassed the subject, by stimulating others to note down their observations, to compare my remarks with the places referred to, and to verify or disprove my assertions. When proceeding westward from Baltimore, the scientific traveler is struck with the apparent confusion and disorder of the rocks in sight. Immense masses of granite and gneiss, with primitive and transition schists, are intermingled in every

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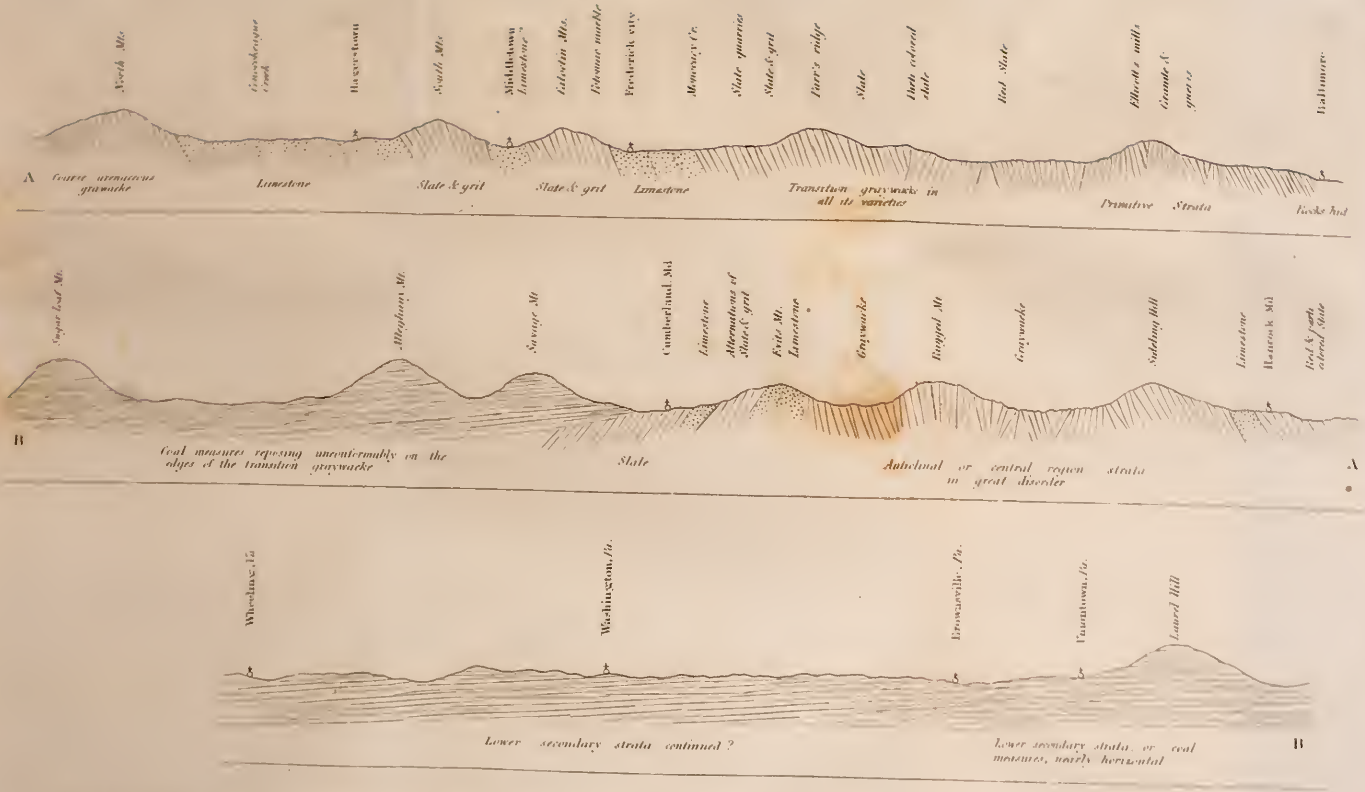
* Mt. St. Mary's College, Emmitsburg, Md., Feb. 5, 1834.

PROF. SILLIMAN.—*Sir*—With a wish to contribute my mite towards an elucidation of American Geology, I herewith send you some notices of the character and superposition of the rocks between Baltimore and the Ohio river, along the route of the national road. The accompanying section is intended to render my views more intelligible than could be done by bare description.

Most respectfully, your humble and obed't serv't.

W. E. A. AIKIN.



A TRANSVERSE SECTION illustrating the superposition of the ROCKS between BALTIMORE and the OHIO RIVER.

possible manner. It is no easy task to reduce this confusion to systematic arrangement, and there seems abundant room for diversity of opinion, with but little danger of any one proving his opponents in the wrong or himself in the right. It is indeed difficult to convey a correct idea of the primitive strata along this part of our section. Slaty and crystalline granite appear to predominate, mixed with slates and a rock approaching hornblende rock in external character. The granite may be well seen in the neighborhood of Ellicott's mills, where there are extensive quarries that furnish vast quantities for the Baltimore market. Succeeding the primitive rocks, next appear transition slates and sandstones, exhibiting the usual Protæan varieties of the transition graywacke formation. I use this last term in the general sense adopted by Humboldt, who designates by it* "every conglomerate, sandstone and fragmentary or arenaceous rock of transition formation that is anterior to the red sandstone and coal formation," with this addition, that I would also include within the same definition all those transition slates that we find interstratified with the above conglomerates and sandstones, and which must have been of nearly contemporaneous origin. This would include the argillite in all its varieties, the old red sandstone, the millstone grit and the graywacke, and graywacke slate of Prof. Eaton, as members of the same formation. There would at first sight seem but little analogy between the soft roof slate of commerce, and the harsh quartzose conglomerate that is quarried for millstones. But the wide difference between these disappears when we find, first, this conglomerate changing its character and passing gradually into finer sandstone, next, the sandstone becoming more slaty and alternating with beds of genuine argillite. *Prof. Eaton* has observed that Europeans do not understand *their graywackes*, and I might safely add that no one can well understand the graywackes of this country without visiting the mountains of Pennsylvania and Virginia. This formation, which is colored brown on the section, commences on the west of the primitive rocks, that are colored red, and continues uninterruptedly to the Monocacy, including the elevated ground known as Parr's ridge. Throughout this whole distance, its peculiar variable characters are strikingly exhibited. The slaty varieties often appear somewhat talcose, often shining as if varnished, at one time friable and rapidly disintegrating, and again firm and compact. At one place quarries have been open-

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* Superposition of Rocks, p. 201.

ed and furnish a tolerable, coarse roofing slate. The arenaceous varieties are often crossed with veins of quartz, and are occasionally colored with chlorite. All the varieties vary much in color, the most prominent are black, blue, green, chocolate color and red. And all the varieties of color and texture may be seen intermingled with each other for a distance of thirty miles. The glazed varieties are closely allied to what was once called par excellence, "primitive argillite." The talcose glazing of this parti-colored rock is noticed by Prof. Eaton,* who quotes Dr. Higgins for an ingenious explanation of the different colors. Being most probably caused, says Dr. H., by the combination of magnesia with iron in different degrees of oxidation which gives blue, purple, and red compounds, the green being produced by chlorite. This would suit our slates very well, for nearly all are more or less talcose, and frequently chloritic. The author's description of graywacke slate, in another part of the same volume, applies admirably to our entire graywacke formation. "This rock takes on the greatest variety of character of any rock in our district. It is coarse and harsh, soft and smooth, fissile and compact, brittle and strong, gray, blue, green and red. Notwithstanding these varieties, there is a peculiarity in the rock by which we recognise it after seeing it once."† Succeeding the transition graywacke, we next meet the genuine transition limestone of Frederick Co. Md.; it first appears on the right bank of the Monocacy, a few miles east of Frederick city. From thence it continues westward, underlays that city, and only disappears where the slate and conglomerate of the Catoctin ridge have been forced into view. Between the limestone of Frederick valley and the quartzose grit of the Catoctin ridge, lying below the former and above the latter, there is interposed a bed of calcareous breccia, the well known Potomac marble, which has furnished, though from another locality, the beautiful pillars of the capitol. I have not visited the quarry, (some twenty miles south of the section,) where these were procured, but have reason to believe that the formation is continuous from that point, to one a little west of Frederick city. That it continues still farther north is proved by its having been found by Prof. Ducatel, and Messrs. Tyson and Alexander, in the vicinity of Mechanics town, a distance perhaps of about fifteen miles in a right line. At this last place it exhibited the appearance of terminating, so that probably it would be best considered as an accidental bed, formed from the ruins of some previous calca-

* Canal Survey, p. 64. † Idem, p. 88.

reous deposit, and interposed between the limerock and graywacke. It is extremely interesting, as furnishing a beautiful ornamental marble that will amply repay the labor of polishing. The Catoctin ridge next succeeds, and presents very evidently a closer approximation to the primitive series, than is to be met with at any other point west of the primitive region of Baltimore. Adjoining the Potomac marble, there occurs a talcoseish looking slate often traversed with siliceous veins and alternating with arenaceous strata. The whole formation is colored with chlorite, sometimes very deeply. That mineral is often found in disseminated masses; epidote is also quite common, and a little farther north, genuine serpentine is found. We have here, as in all other parts of the world, searchers after gold and silver, to whom the copper and iron pyrites, abundant in these rocks, have proved no small source of disappointment. Should the talcose slate hereafter be found to exist extensively in this region, a well directed search for gold might be successful. Descending the Catoctin mountain, we enter Middletown valley, a comparatively narrow intervale, between the last named mountains and the Blue ridge. I have colored the section to indicate the occurrence of limestone in this valley, upon the authority of others. There can be but little doubt that it is found there, although I have not seen it myself. West of Middletown we meet another deposit of graywacke, constituting the south mountains of Maryland and Pennsylvania, known as the Blue ridge, where it crosses Virginia. Leaving the quartzose rocks of this range, we enter the extensive valley in which Hagerstown is situated, and again find the limerock showing its bassetting edges. It here agrees perfectly with that of Frederick valley, and continues until approaching the North mountains, which bound the valley of Hagerstown towards the west. The underlying rock of this valley may be taken as a type of the whole transition lime formation of this region. It is generally a dark blue rock, of very uniform texture and appearance, occasionally traversed with veins of calcareous spar, and almost entirely destitute of organic remains. The sparry variety is best seen in the vicinity of Frederick city, and there is hardly a quarry to be found in any direction within a few miles of that place, where these veins are not abundant. It is this variety that has been distinguished by the name of sparry limerock. A few miles east of Hagerstown, the exact spot I am not acquainted with, this stratum includes a bed of white and perfectly fine-grained limestone, which is quarried for white marble, and answers well for that purpose. The only organic remains, that I have ever known found in this rock, are some beautiful specimens of pentacrinites, found near Winchester, Va. It was in Virginia, also on the banks of Cedar creek, along the line of Frederick and Shenandoah counties, that I observed the only remains, that I have been able to discover in the graywacke formation of this region. They were two or three varieties of bivalves, generally so mutilated as to render any attempt to determine the species, a very discouraging task. One elongated variety, bore a slight resemblance to the outline of a butterfly, sufficiently so, to entitle the locality to the name of "the butterfly rock," by which it is known in the neighborhood. The fact is interesting, since it shows that there are other remains, besides those of the trilobite family, that are probably called petrified butterflies. At the foot of the North mountain, west of Hagerstown, we again meet the graywacke, which continues apparently uninterrupted as far as Hancock. Before arriving at that place, the road was for some distance along the left bank of the Potomac, where may be seen a fine exhibition of red and particolored slates, alternating with each other. A short distance west of Hancock, limestone again occurs, but is soon followed by arenaceous and conglomerate strata, and these in their turn are succeeded by parti-colored slate and red sandstone. On the east face of Sideling Hill, a conglomerate appears—on the west side near the foot, slate alternating with sandstones. This formation then continues a great distance, but varying considerably in external characters. The sandstone often assumes the appearance of the calciferous sandrock of Prof. Eaton, as that occurs in Rensselaer Co. N. Y., and evidently alternates with a soft argillaceous slate and a coarse conglomerate. The next marked on the section is Town Hill, similar in structure to those previously passed. Between this and Polish mountain, marked Rugged mountain on the section, the strata exhibit some of those contortions and twistings, so often remarked in other places, and which will be more particularly noticed, when speaking of the direction and dip of the strata.

This formation continues until reaching the eastern base of Martin's mountain, marked Evit's mountain, on the section. Here the limestone again appears, and seemingly constitutes the mass of this mountain, showing itself towards the top and again near its western base. On the western declivity, near the top, there are some appearances of a white sandstone. The limestone is then succeeded by alternating slaty and arenaceous strata, which continue until arriving within five or six miles of Cumberland. At that point, there is a very interesting exhibition of the manner in which the lime is interposed among the graywacke strata; we see slate supporting a harsh sandstone, this supporting a strata of limerock, which is again covered by slate. This last deposit continues as we progress westward, until the whole transition series is covered up by the coal measures of the west. I was anxious to ascertain if possible, the place of meeting of the two great formations, the transition and secondary; but although this object was kept constantly in view, I was disappointed in determining the precise point, although satisfied myself, it must occur within a very limited district. Somewhere between Cumberland and Savage mountain, I am convinced a proper search would exhibit the latter, lying unconformably upon the former. It is probable that the coal strata, approach a little nearer Cumberland than would appear by the section, as coal is said to be procured within a few miles of that place.

From Savage mountain, (which is colored to indicate the secondary strata,) for the remaining distance, we see no more those diversified slates and sandstones, that exhibit such an interesting appearance, east of this point. The dark blue limestone is no longer seen with its bassetting edges projecting above the soil. The sandstones and shales and limestones of the coal measures, are now alone visible. The most striking change along this part of the route, when compared with the country previously passed, is the great difference in the position of the rocks, the almost horizontal situation of the strata. A little west of Savage mountain, on the top of a low hill, there occurs a slaty limerock, called there "bastard limestone," and said to be unfit for burning. It agrees in external character with a similar rock found interposed among the coal strata around Pittsburg. West of the bastard limestone, the rocks in sight along the road, are red slate alternating with red and darker colored sandstones, and evidently abounding with coal, as the numerous pits along the road testify. On the top of Laurel Hill, there are immense masses of a coarse white sandstone, apparently in place, and west of this the dark sandstones and slates and accompanying beds of coal, continue to the Monongahela. I am unable to give the exact order of superposition of these different strata, along the line of the section, and leave it for those who have leisure to make the necessary investigations over so large a surface. Probably this order will be found nearly the same over the whole secondary district, subject perhaps, to comparatively slight variation. While examining the coal deposits around Pittsburg, I had an opportunity of examining minutely the accompanying strata, the nature and individual extent of these, will be more conveniently given in another place. From Savage mountain to the Ohio River, we have then undoubtedly the lower secondary coal measures, reposing at their eastern edges upon the more highly inclined transition series. I say undoubtedly, because the whole distance has not been examined by myself personally, my progress westward terminated at Brownsville, on the Monongahela. To that place the rocks continue as I have already described, and beyond that I have ventured in accordance with analogy, in accordance with all accounts received of that district, to represent the secondary strata, as continuous with those east of the Monongahela. These then constitute the formations visible along the route—1st. the primitive series of the immediate vicinity of Baltimore; 2d. the transition slates, sandstones and conglomerates of the adjacent country, and 3d. the lower secondary rocks of the west.

We have next to consider the relation which the several strata bear to each other, their dip and direction. And in these respects, the district we are considering, is peculiarly interesting. It presents some seeming anomalies, when compared with other transition districts, in other parts of our country, but these may be mostly referred to the fact, that here we have deposits of a peculiarly variable character, on a much more extensive scale, than can be found at almost any other place. It seemed highly desirable to me, to ascertain if possible, the range of mountains, that could be considered as the central or anticlinal ridge, if any such existed. With this view, I was watchful to note the dip and direction of the strata, at every point where these were visible, and when it happened that any considerable distance was passed over while these essential points remained obscure, this absence of positive information was also carefully noted. Upon the whole, it may safely be said, that there is hardly a tract of equal extent, where the dip and direction are as clear and satisfactory, as along the route traversed. The primitive rocks nearest Baltimore, exhibit pretty regularly a considerable dip towards the S. E., but this soon becomes confused, the strata are seen with a reversed inclination, (i. e. to the N. W.) at times also, they are seen nearly vertical, and again seemingly piled up unconformably on each other. Irregular cracks too, traversing compact masses in different directions, evince great disorder. These appearances are most striking between Ellicott's mills and the city, west of the mills the dip continues more regularly S. E. until we lose sight of the primitive rocks altogether. Throughout this whole region the inclination of the strata is very great, being often vertical and seldom less than 45°. After entering the graywacke region, the degree of inclination is subject to greater variety, the direction of the dip also appears frequently, and suddenly reversed, although the general tendency still continues to be towards the S. E. In the arenaceous varieties the planes of stratification are very distinct, and cannot well be misunderstood. In the slaty varieties, there would seem to be a little more difficulty. We are told by Bakewell, that "slate invariably splits in a transverse direction to that of the beds, making with that direction, an angle of about 60°"; he excepts however from this rule, "coarse graywacke slate and soft slate or shale." Now the greater part of the slate of the region of which I am speaking, is so soft that it might properly be called shale; whether this is sufficient to account for the fact, or whether the above transverse cleavage really exists and has been overlooked, the simple fact is, that nothing of the kind has been noticed. The rock in this respect agreeing with the proper argillaceous slate of Massachusetts, as described by Prof. Hitchcock in his late report. He observes* "excepting in the argillaceous slate connected with the graywacke, I have not been able to find in this rock planes of stratification, moving in a different direction from the laminæ, a circumstance very common it is said in Europe. But in general, strata seams are discoverable, lying parallel to the slaty structure, as in mica slate. The slate indeed contains numerous seams not coincident with those of the strata, but there is rarely any continuous parallelism among them." Even in regard to the "argillaceous slate connected with the graywacke," that is excepted from the above remarks, it would appear that this peculiar structure is far from being uniform, indeed I should rather infer it was only an occasional occurrence, and looked upon when occurring, as an exception to a different rule. "The slaty structure of the slates, included under graywacke, does not always coincide with the stratified structure."† The next stratum west is the limestone of Frederick valley, and this exhibits a dip and direction similar to that of the graywacke, the two are not seen in actual contact along the line of the section, but from their close approach, the observer can have but little doubt that *the former actually passes under the latter.* This opinion was stoutly contested for a long time, as being opposed to analogy, but accountable or unaccountable, such is the fact. The most convenient point to inspect the near approach of the limestone and slate, is near the viaduct of the Baltimore and Ohio rail road across the Monocacy; on the left bank is a high bluff of slate and on the right, a little farther up the stream, are the bassetting edges of true sparry limerock, the

* Report on Geology, &c. of Massachusetts, pp. 289, 290. † Ib. 273.

dip and direction of the rocks on both sides of the stream, are sufficiently evident to convince me that the limestone does in reality pass under the slate. Perhaps, if this had been the only case of the kind, I might have hesitated still longer before drawing the above conclusion; at least I might have been tempted to distrust my eyes or close them, but farther examination saved me from the dilemma; the case proved afterwards of comparatively frequent occurrence, and I am now fully persuaded, that all the limerock between the Monocacy and Cumberland, exists in strata alternating with those of the graywacke formation, and that it is interstratified with the slates and sandstones of that formation. To one who has examined only the limestone vallies, this view may appear to underrate their importance and extent. But in reality they are by no means as important as might be supposed, as will be evident by inspecting the section. I have there endeavored to give, as accurately as I could, the relative extent, longitudinally, of each formation, and we can there see how the limerock, extensive as it may be, dwindles in importance, when compared with the still more extensive graywacke. This view also renders more intelligible the dip of the limerock in Frederick and Hagerstown vallies, which otherwise would be rather inexplicable. In both it is towards the S. E. while the strata of the mountain ridges, separating the two vallies, is also towards the S. E. and the nearest slate east of Frederick city and west of Hagerstown also inclines in the same direction. Similar alternations also occur at other points along the section, and will be noticed in their place. It has happened that the actual contact of the limerock and overlying slate, has not been as plainly seen along the line of the section as at points a little on either side. A few rods east of Martinsburg, Va., the limerock on which that town is built, (and which is undoubtedly a continuous formation with that crossed by the section at Hagerstown,) may be seen, distinctly passing under the slate bordering the Opequon. Continuing a little farther east, and soon after crossing the stream, the slate again gives place to the limerock, and although the second junction of the two is not in sight along the road, there can be no doubt of their relative position. The dip of all the strata is S. E. Another spot that deserves mention, is a limestone quarry in Pennsylvania, along the road from Carlisle to Laudisburg. About a mile north of Waggoner's Gap, where the road crosses the ridge that is there called the Blue mountains, (called the North mountains, where our section crosses it, and quite distinct from the Blue ridge,) and close along the road, we find limestone quarried as a flux for some iron works close at hand. The mountain itself is a coarse quartzose grit, almost a conglomerate, overlaid with finer sandstone and lastly a soft slate or shale. The limerock near the furnace lies on the slate and is covered by similar slate that continues in sight along the road towards Laudisburg. The slate is very soft, rapidly disintegrates and as usual of various colors, grey, blue, green and red. In the vicinity of Harrisburg, Pa. the same order of superposition may be observed, although not within so small a compass. This view of the position of our limestone strata, agrees with observations made at other points on our continent, where similar deposits exist, and also with the views of many foreign geologists. By reference to the section prefixed to Prof. Eaton's canal survey, I find between Williams College and the top of Peterboro' mountain, the following alternations; limerock, argillite, limerock, argillite, calc sandrock, limerock, graywacke.—Three distinct alternations of transition limerock, with what I consider different members of the transition graywacke formation, occur within a very short distance. The author also observes, that "the sparry limerock is found, geologically lower as well as higher, than the argillite of Williamstown mountain range."* Prof. Hitchcock in speaking of the same region, says that the Berkshire limestone, after being traced eastward through West Stockbridge, Mass. and Chatham, N. Y. "is probably interstratified with graywacke slate, in Rensselaer and Columbia counties, N. Y."† Dr. Hayden of Baltimore,‡ noticed the same alternation a few miles south of Bedford, Pa. although he does not appear to have considered the superimposed sandstone as being in place. It seems to me rather probable, that it was in place, as I have myself observed alternations of sandstone and limerock, along the road a few miles east of Bedford. Dr. H. also observes "the whole region, however may, I believe, be considered as secondary." But if my observations are correct, that immediate vicinity, cannot be considered as secondary, according to those observations. Transition graywacke occurs directly east of Bedford, the town itself stands on a dark blue transition limestone, and the secondary country does not begin until several miles west of Bedford. I am unable to say how many miles exactly, as a matter of opinion I would say, less than twenty. Bakewell remarks, that "transition limestone occurs in beds alternating with slate, graywacke,

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* Canal survey, p. 59. † Report on Geology, &c. of Massachusetts, p. 304.

‡ See American Journal of Science, Vol. xix, p. 97.

graywacke slate, and coarse gritstone." *Some of these beds are of considerable thickness, and form mountain masses.* De La Beche, also speaks of patches of limestone, often continuous for considerable distances, intermingled with the arenaceous and slaty rocks of the graywacke series. Again he speaks of the limestones having a series of sandstones and slates, similar for the most part to those beneath, accumulated above. "In some districts, such as the north of Devon, there has been a return of causes, favorable to the deposit of limestone, and two bands parallel to each other have been produced. In other districts, more limestones have been formed, while in some they are nearly absent; a state of things we should expect from variations produced by local circumstances, or similar general causes in operation over a considerable area."* These remarks may be very appositely applied to the transition district on our section. We there see a very extensive graywacke deposit, often interrupted by patches of limestone, occurring very irregularly and of very variable extent. Until we are able to ascertain what those general causes were that operated to produce such calcareous deposits, we must be content to remain ignorant of the reason of this irregularity. The term bed, appears too trivial to be applied to deposits so extensive, (longitudinally,) as our limerock, since the term is most usually bestowed upon such as are very circumscribed. It seems preferable to consider the rock in question, as interstratified with the graywacke strata. As only one example of its extent, I have traced the limerock that crosses the Susquehannah, at Harrisburg, Pa., southwesterly and southerly in the direction of the valley, through Carlisle and Chambersburgh, Pa., Hagerstown, Md., Martinsburg and Winchester, to Woodstock, Va. With the exception that I did not pass in a direct line from Hagerstown to Martinsburg, the identity of the rock as seen at those two places, is inferred from its perfect agreement in the direction and inclination of the strata and in its external characters. I have no information how far this stratum extends N. E. of Harrisburg, Pa., probably to the Delaware River or beyond, perhaps it may even be connected with the blue cherty limerock of Orange Co. N. Y., described by Mr. Charles U. Shepard.† He found that intimately associated with an argillite, described as very similar to what exists in the same connexion in these parts and also connected with a white crystalline limerock. What agency the gneiss and sienite of Orange Co. might have had in converting the limestone to

* Geological Manual, p. 437. † See Amer. Jour. of Science, Vol. xxi, p. 321.

marble, it would be difficult to say. Their igneous origin seems to be universally admitted and at a time when it is so fashionable to refer all troublesome facts in geology to that mighty cause, I may be excused for conceiving the possibility of a blue limerock, becoming a white one, when exposed to its energetic agency.

By reference to the section, it will be seen that the strata, which along the eastern part are represented as having a southeasterly dip, at Cumberland and for some distance this side, have a reversed dip. It would be solving an interesting question, to determine the precise line where this change of dip occurs. Any one, however, who expects to find a well defined anticlinal line, continuous for any considerable distance, must necessarily be disappointed. There is no such line to be found. There is a district between Hancock and Cumberland, where the strata are found more confusedly disposed, and where sudden changes in the dip of the rocks are frequent. East of this district, the dip is very regularly S. of E. West of it, the dip is equally uniform towards an opposite point, N. of W. Beneath this space then, we are authorised in concluding, the eruptive power that was instrumental in upheaving the Appalachian chain, was most energetically exerted; this may be considered as the true anticlinal region. I regret that no opportunity was afforded me of exploring this region in the direction of its length. Subsequently, however, I had an opportunity of crossing it at another point, a short distance north of the Cumberland road. I copy from notes taken at the time. "About ten miles east of Bedford, and between that point and the Crossings, we may see slates and sandstones alternating and presenting an extremely interesting, although somewhat confused appearance. Within the compass of a few miles, there occur strata of various colors, although mostly red and chocolate color, of every variety of texture, and reposing in every possible degree of inclination, from vertical to horizontal, and often exhibiting complete semicircular curves. The appearances indicate that here was one point, where the eruptive power from beneath was principally exerted." By referring to a map, it will be evident how well these two observations coincide in pointing to the same tract of country, and I have no doubt, if further investigation were made along this line, north and south of the points already visited, that similar appearances would be found. It remains for future observers to ascertain also the direction of this anticlinal region; it probably follows more or less faithfully, the general curve of the mountain chains as represented on the maps. There are some indications, however, that render it probable that its course is in degree somewhat transverse, that as we go south "the central region" approaches the eastern border of the mountainous district, and north of the Potomac, that it approaches more nearly the western border, thus crossing the general direction of the mountain ranges, at a very acute angle. In regard to the agent so efficient in throwing up mountain chains, although there is some little difference of opinion as to the mode by which it operated, the agent itself is now almost universally conceded to be igneous. An opinion that gains confirmation, if any is needed, from the occurrence of thermal waters along the central line of the Alleghany region. The springs at Bath, Va. have a temperature of about 73° Far.; farther south, we find the warm springs at 96°; a little farther, the hot springs at 112°, and still farther the acidulous waters of the sweet springs, with a temperature similar to the water at Bath, or about 73°*. Farther examination will, perhaps, discover other and warmer waters, along the same line.

From the preceding remarks, the relation of the secondary rocks of the valley of the Ohio and its tributaries, may at once be inferred. I have represented them as reposing unconformably upon the edges of the transition strata, and I am persuaded that such is their real position, although I have had no opportunity of seeing their actual contact. Their proximity and difference in inclination, proclaim that such is the case, and their junction could probably be discovered within a short distance west of Cumberland, Md. or Bedford, Pa. The prevailing dip of the secondary strata, is from N. W. to a little S. of W., subject to some variations, although not greater than would reasonably be anticipated. The position of the rocks, may be satisfactorily studied by tracing extensive coal beds, where these are found cropping out in the perpendicular banks of streams. By tracing the same bed for a few miles, a slight dip will often be found, where none was before suspected. The coal beds of this region are equally interesting in a scientific, as in an economical point of view. But for want of time, I shall be obliged to postpone for a future number of this Journal, some remarks on the subject, that I had intended appending to this paper. For the same reason I must postpone the examination of many interesting deductions from the foregoing facts. I regret very much, that I have not had more leisure to examine the primitive and transition strata between Frederick city and Baltimore. The confusion and intricacy of the subject, added to my limited ob-

* Encyclopædia Americana, Art. Virginia.

servations, have restricted me to mere general views. Any omissions of mine, however, are of less consequence, since the region has been examined by Prof. Ducatel and his associates, who will undoubtedly, in their forthcoming report to the legislature of this state, bestow upon it the attention it deserves.

In conclusion, I cannot avoid reverting to the extremely interesting character of the line of country, to which the preceding remarks have been applied. To the practical man, it is recommended by its mineral treasures and its agricultural resources. Its quarries of marble, of granite, of freestone, of slate, of soapstone—its mines of copper, iron, manganese, chrome and lead, its inexhaustible beds of bituminous coal, the recently discovered deposits of anthracite in the graywacke slate of Virginia, and the unrivalled fertility of its limestone vallies, offer tempting rewards to industry and enterprise. To the geologist it is no less interesting, as offering to his research, one of the best fields on our continent, for obtaining a correct knowledge of our transition strata. Here every thing is seen upon an immense scale; deposits that would be appealed to as general strata, if seen in almost any other connexion, are here viewed only as subordinates; formations considered as distinct, when examined on a more limited scale, are here seen alternating with and passing into each other, in such a manner as to leave no doubt of their identity. It were to be wished that the labor of exploring so fruitful a soil, had devolved upon some one better fitted for the task than the writer of these notes, but until other laborers are found, he will endeavor to collect and collate from time to time, some few of the many interesting facts connected with this interesting region.

Explanation of the section.—The original section, was drawn on the scale of two miles to an inch, but when that came to be reduced within the present limits, it was found impracticable to preserve all the minutiæ. Consequently there may be in the preceding remarks, occasionly an inappropriate reference. The coloring is also omitted, to prevent confusion, and the different formations distinguished—the limerock by dots, and the other formations by lines, that are intended to show the inclination of the strata. The line between the primitive strata, and the adjoining graywacke is not defined, because I have not satisfied myself exactly where it should be placed—nor is it important for my purpose, that it should be located. The section is supposed to run a little north of west, and a little south of east, generally at right angles to the direction of the strata, and the spectator is supposed to stand on the south side, looking towards the north. It is intended to convey a pretty correct idea of the relative extent of each formation, viewed transversely; as the height of the different mountain chains, was not known, their comparative elevation could only be approximated.

Art. II.—*On Porcelain and Earthenware.*

The art of pottery has been practised by mankind from the remotest periods. The ingenuity of the savage has shapen vessels of earth for domestic uses, on the plains of Tartary—in the rocky caverns of ancient Greece—and on the sultry banks of the Oronoco. In the progress of science and refinement, it has advanced from the sun-dried bricks in the tower of Babel, to the beautiful and splendid porcelains of Dresden and Sevres—is now an important object in one department of manufactures—and in the present state of society is a necessary of life. Its fabrication combines the skill of the chemist and the taste of the artist, with the dexterity of the mechanic, and many of the choicest specimens are entitled to a distinguished rank in the fine arts.

The subject may be considered in four sections.

I. A history of the origin and progress of the art;
II. The nature of the materials wrought into pottery;
III. An outline of the process employed; and
IV. A description of the various kinds of ware.

I. *A History of the Art.*

The most ancient specimens of this art, are the bricks found in the ruins of Babylon. That city built by Nimrod, 2,200 years B. C. is now a series of mounds, overspread by the dust of its own decomposition, and lying in huge masses of undistinguishable ruin. Long narrow rifts and channels between the hills, indicate the ranges of its once populous streets; and the great tower of Babel, the witness and the cause of the confusion of languages, stands highest among the hills: a monument and a record of the advances made in some of the arts, as well as of the ambition of the inhabitants in that early age of the world. The city is given over to desolation—the Euphrates annually overflows all but its highest summits; but when the waters retire, the mounds are perforated in every direction for building materials, and in the hope of finding hidden treasures.* Mr. Rich a late traveller describes the Birs Nemrood the largest of the mounds, as seven hundred and sixty two yards in circumference, and one hundred and ninety eight feet high. It is supposed to be the tower of Babel, and consists of three receding stories. The interior of the

* See Keith on the Prophecies.

mass is filled up with unburnt bricks, set in clay, with layers of reeds between every five or six courses. The exterior wherever it remains entire, is faced with well burnt bricks set in bitumen. From the present appearance of the mound it is conjectured, that it was intended to consist of five stories—the three lower solid, and the two above to have contained chambers. At the top of this pile, there is another solid elevation thirty seven feet high, of burnt bricks set in lime mortar. Many of the heaps and hills are connected by galleries and passages of brick work, laid in lime mortar of exceeding toughness. In some of the excavations have been found earthen vessels which are presumed to be the most ancient specimens wrought by the potters wheel.*

Bricks were made also by the Egyptians, and Herodotus states, that one of the pyramids was built of unburnt bricks made of clay and chopped straw, probably like those required by the taskmasters of the children of Israel, when they were subject to Egyptian bondage. Mr. Aikin remarks, that sunburnt bricks were rather artificial stone than earthenware; and Pliny mentions that at Utica no bricks were allowed to be used until they had been dried five years.† Many buildings of high antiquity were formed of brick—such were the palace of Croesus king of Lydia, of Mausolus of Halicarnassus, and of Attalus at Tralles. The walls of Athens, which look towards mount Hymettus are also of brick, and some of the ancient temples of that city.‡

The Romans were skilful in their methods of making and burning bricks, and in all the remains of Roman walls, forts, and buildings in Great Britain, they are of an excellent quality, of a deep red color, very hard and well burnt. Throughout the wide valley of the Ganges, bricks appear to have been used from the highest antiquity; and in Nipaul, a hilly country north of Bengal, they are of such remarkably compact texture, and their ornamented surfaces so elegant, as to be peculiarly fitted for the decorations of architecture. In China bricks are made of a blueish clay, and after burning are of a semi-porcelainous texture.

After the Romans left England, bricks were not used for architectural purposes before the middle of the 14th century; and until lately they have been fabricated in a very rude manner.§ They are

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* Annals of Commerce.

† See a paper on pottery in the Transactions for the Encouragement of Art, by A. Aikin, Esq. ‡ Idem. § Idem.

now made of various kinds, and of superior quality in England, not only for home consumption, but largely for exportation.

It could not have been long after the discovery of the plastic quality of clay; and that by drying or burning it became impervious to water; before the wants and ingenuity of man suggested the application of it to vessels for domestic and culinary uses. Earthenware being peculiarly adapted to keeping water pellucid and cool, jars and vases for holding it, soon became articles of first necessity, where it was not plentiful, as in Syria, and many of the middle and eastern parts of Asia. Allusions to earthen vessels—to the potter's clay and the potter's wheel, occur in the most ancient writers. The plastic properties and consequent uses of clay are noticed in the book of Job, the most ancient book now extant; and the potter's wheel is referred to by Homer in his description of Achilles shield.* Earthen vessels were in use among the Hebrews when they received the law from Moses; and the prophets often refer to the power of the potter over that most ductile material the clay, as illustrating the relative position of man in the hands of him, who moulds our purposes at his will.

The arts flourished soon after the deluge, to a surprising extent in Sidon the capital of Phenicia, a narrow country, between mount Lebanon and the most eastern coast of the Mediterranean sea. This city was nearly coeval with Babylon, being founded soon after the confusion of languages, more than 2000 years before Christ. It excelled in manufactures of fine linen, embroidery, tapestry, metals and glass; of the latter there were many varieties; such as colored, figured, turned by the lathe, painted, cut or carved, and even mirrors; but no mention is made, at this date, of porcelain, unless the article named as painted glass was a species of that manufacture. To this people is ascribed the invention of boats—of navigation—of the application of astronomy to nautical purposes—of book-keeping—of writing—of arithmetic and of weights and measures. They sent colonies to Greece, and Italy, and with their little boats of wicker work, covered with leather, they coasted the Mediterranean, and made various settlements in the south and west of Europe, and on the northern shores of Africa. At a very early period a colony of Phenicians settled on the west coast of Italy, and carried the perfection of their arts and manufactures to Etruria;

* Moses wrote 1452 B. C. Homer in 907 B. C.

but Pliny ascribes the introduction of those beautiful earthen vases into Etruria, of which admired specimens have come down to our own times, to two artists from the isle of Samos. The Samians were famous 500 or 600 years before Christ, for their manufactures of gold and silver and for a fine earthenware resembling the modern porcelain, which Herodotus states was in great demand at Rome, for the service of the table.

Although the coarser kinds of earthenware were invented in the earliest periods, yet there are no records of a manufacture so elegant and complicated as porcelain, until near the christian era; unless the Samian vases claim that distinction.*

The celebrated Murrhine cups or vases, which were introduced into Rome 14 years B. C., have divided the opinions of antiquaries. Propertius speaks of them as baked in Parthian furnaces. Martial alludes to them as filled with heated wine. Pliny thought them made of a fossil substance, and says "they were first brought by Pompey to Rome, in his great triumph over Asia and Pontus. Murrha comes from Parthia and Caramania, and unwrought specimens together with the cups, were dedicated to Jupiter Capitolinus, and placed in his temple. They were not translucent, but peculiarly splendid, from the great variety of hues reflected from them in spots and waves —changing from white to purple—sometimes edged with a tint of flame color, and interspersed with variable irridescent rays." If they were not a variety of oriental porcelain, they were probably made of the *adularia*,† which is found in Arabia, Persia, and Ceylon.‡ They were in such esteem at Rome, in the first ages of the Christian era, that two of them were bought by one of the emperors at the price of 300 sestertium, more than £2000 sterling each. A cup capable of holding three sextarii, ($4\frac{1}{2}$ pints,) was sold for seventy talents; and a dish for three hundred, a talent being equal to £.180 English.§ The author of the Periplus of the Erythrean sea, says they were made at Diospolis in Egypt.

In the time of Herodotus, vessels of earthenware were very scarce and highly esteemed by the nations around. "Twice in every year" says he, "there is exported from different parts of Greece to Egypt,

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* The term porcelain, is of European origin. Whittaker derives it from the herb purslaine; the most ancient china brought to Europe, being the exact color of its purple flower. Mr. Aikin thinks it an Italian word, signifying an arched univalve shell, remarkable for its white, smooth texture and vitreous gloss.

† In its finest specimens, sometimes used as a gem.

‡ It was the opinion of Scaliger, that they were Chinese porcelain.

§ Aikin on Pottery. Trans. Soc. Arts, &c.

and from Phenicia in particular, wine secured in earthen jars, not one of which jars is ever after seen; for the principal magistrates collect all that are imported and send them to Memphis. The Memphians fill them with water, and afterwards transport them to the Syrian deserts." From Juvenal, who wrote in the 1st century of the Christian era, 500 years after Herodotus, it appears that earthenware was then made in great plenty in Egypt. Images covered with a deep blue glaze have been found enclosed with mummies, in a number of Sarcophagi, which is conclusive evidence, that the art had advanced to a very considerable degree of excellence nearly 2000 years ago; the blue being found on examination, to be a preparation of cobalt, the identical material employed by the potters of the present day. Gov. Pownall, describes certain vases and urns discovered on the Mexican coast "as curious exemplars of some of the first efforts of human ingenuity;" and adds "that remains of ancient potteries are visible in various parts of South America, particularly on the river Amazon." Mr. Parkes states, that "urns of Earthenware have been found in the barrows of England, supposed to have been the workmanship of the ancient Britons," and some have been imagined from their peculiar form to have been designed for Druidical rites. Pieces of a rude ware of Roman manufacture, have been drawn up by fishermen, in the mouth of the Thames, where 2000 years ago there was an island, which has disappeared with the changing sands of that coast. They were evidently designed for use in the religious ceremonies of the Romans.

Vessels of earthenware have been discovered in the Tumuli of the valley of the Ohio and its tributaries.* Those for domestic use were found deep in the mound, where duty and affection had placed them for the comfort of the endeared relation or friend; while others with emblematical designs, probably connected with idolatrous worship, hallowed the grave of the unknown race, with whom they were enclosed; and every sepulchre of a hero is made the temple of a god.

The mystery and concealment observed by the Chinese, in regard to their manufactures, kept this art from the rest of the world, long after they had arrived at a great degree of perfection. Specimens of Chinaware and the lacquer called japannery, were found both in China and the Japan islands, of excellent quality, by the earliest Eu-

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* Archæologia Americana.

ropean travellers; and in addition to vessels of the most delicate and beautiful texture, which appeared to have been in use from time immemorial, they make mention of temples encrusted with tiles of various colors and curious workmanship; but it was not until the conquest of China by Genghis Khan in 1212 that the art of glazing earthenware was made known to the rest of Asia. The empire of Genghis extended from China across the whole pastoral region of Asia, to the Caucasus, and in their progress they held both hostile and friendly intercourse with the Saracens. That splendid race were at this time not only warlike, but inquisitive, active and ingenious; and it appears probable that this art was transported by them from the confines of China to Spain and northern Africa. Many rooms in the Moorish palace of the Alhambra, are decorated with lacquered tiles, and the cupola and minarets on the tomb of the Sultan Mahomed, at Sultanyah, in Persia, built at the same period, are covered with green lacquered tile, the great architrave being formed of blue of corresponding quality.

In 1270 Marco Polo, a Venetian, visited the court, and was for several years in the service of Kublai Khan, the grand son of Ghenghis; during which time the merchants of Italy were travelling for commercial purposes in most countries between Syria and India. Earthenware covered with a vitreous glaze, was imported by them from the east, and Florence became a celebrated mart for this ware, which met a ready sale throughout Europe. The maritime laws of Barcelona, which bear date 1096, mention porcelain among imports from Egypt; but it was far from being common, or even generally known in Europe, in the 14th century. The sultan of Egypt, sent large vases of porcelain to Lorenzo de Medici in 1487, of Egyptian manufacture, and they are said to have derived their skill from the Corinthians, who had obtained the art from the east. The Persians also arrived at great perfection in the potter's art at a period of remote antiquity; and it is worthy of remark, that porcelain is not made in the Indies, but that all the countries of Asia, have been supplied from China, Pegu, Japan, and Persia.

Porcelain was not common in Europe before the first ages of the Christian era. Rome was supplied from Samos. Vases and utensils of oriental or Egyptian manufacture, have been disinterred from Herculaneum and Pompeii; also wine jars and drinking cups of terra cotta, which is a fine reddish, unglazed ware. Statues of the gods, were made in Rome of terra cotta, until the introduction of marble statues from Greece by Lucullus and Pompey.

The Etruscans were probably the first, who brought this art to any degree of perfection in Europe. They were from Phenicia, and whether it originated with their ingenious ancestors or whether it was transplanted from China to Sidon and Tyre, and thence to the poetical and picturesque regions of Etruria, neither tradition nor history give any certain information. Pliny states that Praxiteles moulded images and figures in clay, which were the models, and the origin of statuary in marble and bronze.

Raphael is said to have practised the art of painting on enamel in a high degree of perfection. He executed the arms of Leo X. which now adorn the vatican. Several pieces of this ware are known as Raphael china, and are in the cabinets of the curious. One splendid dish in particular, found in Carinthia, twenty inches in diameter, bears an inscription, purporting that it was made in 1542. The subjects are Pan and Apollo, Jupiter and Semele—Apollo surrounded with nymphs and satyrs, with entwined cupids on the rim.*

The white enameled ware made in Europe is indebted for its present perfection to Bernard de Palissy, who was born at Guienne in France, 1490. He became eminent for industry, learning and talents—was a philosopher and naturalist, and so interested in the subject of enamels, that he devoted his fortune and almost the whole of his life to experiments on enameled pottery. Although he reached at length, the degree of perfection to which he had aimed, and published many valuable works on various subjects, indicative of singular genius, commanding the esteem and admiration of all classes—yet the fanatics of the League, persecuted him on account of his adherence to the protestant faith; and dragged him to the bastile at 90 years of age where he died. His reply to Henry III. of France, is an example of firmness which "deserves commemoration." The king advised him to reconcile himself to the matter of religion, or he would be left in the hands of his enemies. "Sire," said Palissy, "neither your majesty, nor your whole people, have the power to compel a simple potter, to bend his knee before the images which he fabricates."

The first porcelain from China, which was brought to London, came in a Portuguese prize ship from India, about 1593.† The introduction of this beautiful fabric, soon awakened a desire in the

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* One of the Duke of Brunswick's palaces, in a small village a German mile from the capital, contains a large collection of Raphael china.—*Vide Hanway's Travels.*

† Annals of Commerce.

countries of Europe, to imitate it. The chemists and mineralogists of Germany, engaged with great eagerness, in endeavoring to find appropriate materials, and in combining them in the most advantageous proportions. Accident at length disclosed the mysteries of this art, where the most strenuous efforts had failed of success. The Baron de Botticher, a German alchemist, was thrown into prison on suspicion of his having in possession the philosopher's stone. Nothing daunted, he pursued his researches with inflexible perseverance, and on one occasion, he discovered that a crucible which he had subjected to the most intense heat, had become perfect porcelain, resembling in quality, the best oriental china. This discovery laid the foundation of the celebrated manufactory at Dresden, which soon became the first in Europe, and which rivals the best wares of the east. The greatest secrecy is maintained respecting these works. They are established within the walls of the fortress of Meissen on the Elbe, three miles from Dresden. Seven hundred workmen are employed, who are all close prisoners, and subject to arrest, if they are found without the walls. There is no admittance to this stronghold, without an especial order from the governor of Dresden.* About the same period, several manufactories for porcelain, were set up in Germany, and conducted with impenetrable secrecy. At this date also, a manufactory was established at Florence, where statues and groups were modelled from some of the finest antiques.

The government of France instructed the Jesuit missionaries, who penetrated China, to inquire into the particular processes, and to collect specimens of the materials employed by the Chinese. The Father de Entrecolles had the address to obtain some of the substances, with many details concerning the art, which were sent to France. The celebrated Reaumur entered upon the most severe analyses of the various ingredients; and after great labor and many disappointments, he succeeded in imitating the porcelains of China in 1727. These chemical examinations were the origin of the royal manufactory at Sevres near Paris, and the kings of France, and Poland, munificently, patronized establishments for the improvement of this art, which were too expensive for private adventure.

The European manufactories, which approach the nearest to the most perfect chinese porcelain, and in the style of painting, excel even the Chinese, are, the one at Dresden; the king of Prussia's at

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* Wraxall's Memoirs.

Berlin; and that which belongs to the king of France at Sevres. The second Frederick of Prussia conceived so high an opinion of the works at Dresden, that when he conquered Saxony, he took all the best workmen, and conveyed them to his own pottery at Berlin.* Five hundred men have constant employment at those works, which are carried on "for his Majesty's private account, with success and good taste."† The Elector of Saxony "valued himself" on the perfection, to which the manufacture had been carried in his dominions. "There are porcelain figures, in his cabinet at Dresden," says Mr. Hanway, "of wolves, leopards, bears, &c. as large as life, with a prodigious collection of birds, and a curious variety of flowers." When he became king of Poland, he bartered a whole regiment of dragoons with the king of Prussia, for forty eight large vases of Chinese porcelain.‡

It is probable, that Holland received the art of making glazed earthenware from Italy. The Venetians, Genoese, and Florentines, had commercial dealings with the cities of Antwerp and the low-countries. The potters of Holland, who made the best tobacco pipes, in due time, acquired the knowledge of glazed ware, and in the town of Delft, were fabricated the tiles, known as Dutch tiles, and the table service called Delft ware. The Dutch fell short of the Italian potters, in the style of ornamenting their products, which was partly owing to their imitation of oriental patterns of blue and white, which they imported in large quantities from China and Japan. It is not more than 200 years, since some Dutch potters went over to England, and established themselves in Lambeth; and by degrees assembled a colony in that village, consisting of twenty manufactories, from which they supplied London, and other parts of the country with tiles, and glazed Delft ware, for table use. They continued in a flourishing state for more than a century and a half, when the Staffordshire potters, by their improved wares, took possession of the market, and the Delft ware went almost out of use.

The Staffordshire potteries comprehend a district of ten miles in extent, where it is believed that earthenware has been made, although of inferior quality, ever since the time of the Romans.§ The great variety of clays upon this tract, rendering it unfit for the purposes of husbandry, together with an inexhaustible supply of coal, are evidently the reasons why this district was selected for this manufacture.

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* Wraxall's Mem. † Parke's Essay. ‡ Hanway's Travels.
§ Plott's History of Staffordshire.

In 1690 when the ware was of the coarsest kind, two brothers from Holland by the name of Ellers, settled in Burslem, in the heart of the potteries, and improved and extended the works, which were then in operation. They prosecuted their enterprise with great success; but it was enveloped in such total mystery, that the jealousy and enmity of the inhabitants, compelled them to leave the country. With them the art would have been lost, had not a man by the name of Astbury, discovered it by a singular stratagem. He feigned himself to be of weak intellect, and obtained employment in the works, where he submitted to the drudgery, and contumely heaped upon him for his supposed imbecility. He thus acquired a knowledge of all that was done in the manufactory, and made models of all the tools, unsuspected by his employers. It was the same man, who discovered the use of calcined flint, as an ingredient for porcelain. In travelling to London, he saw a hostler, reducing some burnt flint to an impalpable powder, as a remedy for the eye of a horse. It immediately occurred to him, that this brilliant white powder, might form an excellent ingredient with the clay used by his craft, to improve the body and color of his ware. It succeeded beyond his hope, and originated the white Staffordshire ware. Thus in science and art, the acute observer turns the merest accident into a resource for discoveries, and improvements. Thus the fall of an apple, suggested the law of gravitation.

But it was not until 1763 that the most important improvements were made in English pottery, by Mr. Josiah Wedgewood. The singular merits of the Burslem artist, and his beginning improvements, were almost lost sight of in the blaze of Mr. Wedgewood's fame. Such were the advances made by his invention, skill, taste, liberality and enterprise, that he soon assembled around him the most celebrated artists and modellers. He engaged the early talents of Flaxman, and the pencil of Webber, and his wares obtained the patronage of the royal family and the nobility, throughout the kingdom. His works became so extensive, and emulation and industry were kindled to such a degree, that the district called the Potteries, consisting of some scattered villages, over an area six miles by ten, has become a dense population, resembling a single town, forming in the sterile clay vallies of Staffordshire, a new Etruria. Mr. Wedgewood was a scholar and philosopher, and his success shews the value of science to the arts. By chemical analyses and experiments, he ascertained the due proportions of his materials—

the appropriate degrees of heat—the colors, and the curious chemical combinations, essential to their enduring the furnace, and incorporating with the glaze of the ware. In the oxides of metals, he found an endless variety of hues; and for his forms and ornaments he took models of grace and beauty from the ancients. His imitation of the Barberini or Portland vase, one of the most admired remnants of antiquity, is sufficient alone, to insure him distinction, in the annals of the arts. This elegant vase, was discovered in the tomb of Alexander Severus, and is believed to be the work of Grecian genius. It is a semi-transparent urn, of a deep blue color, with brilliant opaque white ornaments upon it in bas relief, cut by the lapidary in the same manner as the antique cameos on colored grounds.* Mr. Parkes states "that several of the nobility and gentry, being desirous to possess a copy of this beautiful specimen of ancient art, engaged Mr. Wedgewood to attempt an imitation of it; and he actually produced a vase of porcelain, which for elegance and beauty was considered fully equal to the original." The height of the vase is ten inches, its diameter at the broadest part only six inches. It has two curiously wrought handles, one on each side. The sculpture is in the greatest perfection; the figures full of grace and expression—every stroke and delineation, as fine, sharp, and perfect, as any drawn by a pencil.

It cannot be too often repeated, that a knowledge of chemical science, is essential to success in this art. Mr. Chisholme, an associate of Mr. Wedgewood, and a superior chemist, devoted his whole life to this business. Mr. Wedgewood must be deemed a public benefactor. The great improvements, which he made in Earthenware, and the low price at which he managed to have it afforded, almost displaced foreign china; while the poor dismissed their gourds and wooden trenchers, for the enameled pitcher, the neat plate and teacup, introducing into the cottage, taste, comfort, and cleanliness. He refused to obtain patents, saying "the world is wide enough for us all." Such are the excellent qualities and ornaments of the Staffordshire ware, that it is now sought for, and employed in almost every part of the world; even in the interior of Africa, Clapperton found dishes of English manufacture. Not more than one sixth of the goods manufactured, are consumed in Great Britain—five sixths

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* It was long believed to be porcelain, but is ascertained to be glass.—*Lardner and Parkes.*

are exported. Vessels are loaded with it for the East Indies, and the continent of America.

In addition to Mr. Wedgewood's improvements, his inventions command great admiration. Those of terra cotta, resemble porphyry, granite and Egyptian pebble; and his imitations of Jasper, by which cameos, and white figures in relief, are raised on a colored ground, are exquisitely beautiful.

The Warwick vase, which is mentioned in this place, only as its design is a beautiful model for porcelain manufacturers, is also a monument of Grecian art; the production of Lysippus, statuary to Alexander the Great. It was dug up in Adrians villa, at Tivoli, and was sent to England by Sir Wm. Hamilton, in 1774. It is of sculptured marble, adorned with elegant figures in high relief: vine leaves, tendrils, fruit and stems, forming the rim and handles.

The first true porcelain made in England, was in 1768, by Mr. Cookworthy, who discovered mineral substances in Cornwall, similar to the porcelain earths of the east. The discoverer and his associates, were successful in the quality of their products, but it was not as profitable as they had anticipated; and the manufactory declined upon the introduction of Mr. Wedgewood's improved earthen ware, when porcelain came to be less in demand.

Fine porcelain earths are found in Wales and a manufactory was established at Nungarrow, where the ware was made of very superior quality: but that was also given up, as prices could not be obtained for it, that would cover the cost. Nungarrow porcelain is now an admired rarity, much sought for by the curious.

A vitreous, fragile kind of china, called soft porcelain had long been made at Bow and Chelsea, much esteemed for its beauty, but it was soft, and fusible at a low heat. True porcelain of superior quality is made, however, in many parts of England. The manufactories in Worcester, Shropshire, and Yorkshire, have produced very excellent specimens which for elegance of design, and goodness of workmanship are nearly equal to the best Dresden. A tablet in possession of Lord Wentworth, has been pronounced equal to some of the admired productions of Sevres. It is a copy of Vandyke's representation of the Earl of Strafford, dictating his defence to his secretary. The subject is one of deep interest, and in expression and coloring does justice to the masterly original.

Choice wares are made at Berlin, at Vienna and in some of the smaller German states, also at Hammer in Bohemia. The Dresden china surpasses all other of European manufacture. The value of porcelains stands in the following order.

1st. The Chinese, or incomparable king te-tching;

2d. The Persian;

3d. The Dresden scarcely inferior to the Chinese;

4th. The China of Sevres, is perhaps even more splendid in its snowy whiteness and beautiful decorations, than the Chinese; but inferior in the solidity and infusibility of the ware. The Berlin, English, and German porcelains have attained a high degree of excellence, some being superior in the enamel; others in colors, and others again in grace of form and quality.

The potter's art has not been unknown or wholly neglected in the United States. Bricks, and the common red-earthen, and stone wares, have been made in various parts of the country, sufficient for home consumption from an early period of its history. Within a few years, establishments for making porcelain have been attempted but probably owing to the high price of labor, the first efforts were not as successful as was hoped by those who engaged in the enterprize. There is an excellent establishment in full operation near Philadelphia; and another, one commenced six or seven years since at Jersey city near New York, is now used for the manufacture of the printed Staffordshire, and a superior fire proof stone ware. The manufactory exhibits a highly interesting series of the various processes of the art, whose products merit approbation, and patronage.

II. *Nature of the Materials.*

Having traced the history of this art from the remotest antiquity, a description will be attempted of *the nature of the materials wrought into pottery.*

The component parts, and the best methods of making bricks will not be considered here, as this essay is becoming longer than was the design of the writer; but may perhaps be the subject of a future communication.

The art of pottery depends more for success upon the purity, and the appropriate combination of materials, with their adaptation to the precise degree of heat required, than upon the power of machinery, or the dexterity of the artizan. Without the most accurate knowledge of the former, and practice of the latter all the expense and labor would end in disappointment. It is so complicated, and so nice, and rests so much upon a knowledge of chemical affinities, and principles, that it is cause of surprise that the Chinese could have brought it to perfection, without the aid of Science. The agency of water on most of the operations of nature and art, being "a solvent for alkalies, and most of the acids, and earths, and by its decomposition imparting oxygen to one principle, and hydrogen to another;" if not well understood, will at times frustrate the expectations of the manufacturer. More important to the potter if possible is a "thorough knowledge of the properties of different kinds of clay. Macquer who examined more than eight hundred specimens, says that he did not find one entirely free from metallic matter." It requires much practice also, to be able to understand the nature of clays so as to employ them to advantage. Alumine* when pure, is a white, opaque, tenacious earth, with an oily feeling and *is the plastic material* in all the varieties of clay. If beaten up with water it forms a ductile paste, easily moulded, smooth and compact in its texture, but if exposed to the heat of the furnace, it contracts greatly in its dimensions, becomes rifty, exfoliates, and falls to pieces. But silex† for which it has a great affinity, when combined with it in certain proportions, obviates these defects: exposed to heat it becomes impenetrable, resists the percolation of fluids, and is incapable of decomposition from the action of the atmosphere. Pure crystallized silex is transparent; alumine is also transparent in some of the precious gems, but both earths in their powdery state are opaque. These earths either saperate or in combination, are the basis of most of the gems. Alumine is wholly insoluble in water—diminishes in volume and increases in hardness proportioned to the degree, and long continuance of the heat to which it is subjected. Flint is silex in a state nearly pure, is obtained in great abundance in chalk hills, and sometimes occurs in secondary limestone. It has a glimmering lustre; its fragments are sharp edged, and its fracture conchoidal. The best flints are translucent, of a dark grey color. Those which shew ferruginous spots of yellow or brown should be rejected, as they would discolor the ware. Silex is held largely in solution in certain hot mineral waters, and in volcanic fountains, probably by the aid of soda, which is a solvent for it, as is well known in the manufacture of glass. Klaproth detected twenty five grains of silex in a thousand ounces of the mineral waters of Carlsbad in Bohemia;§ and the Geysers, or hot springs, in Iceland, hold so much silex in solution, that solid hollow basins are formed around the cavity of

* Pure clay. † Rock crystal and white beach sand are examples.

§ Lardner's Cyclopedia.

each, where the waters fall. Clays are found in a natural state of various degrees of purity; seldom if ever without some admixture of foreign matters. That is best which burns whitest, and is capable of combining with the largest quantity of flint or sand without cracking; that being the limit beyond which it cannot be advantageously united.

The proportion of silex to alumine in Chinese porcelain is 74 per cent. of silex, to 16 of alumine; yet although the silex predominates, the argillaceous substance gives the character to the compound; imparts the cohesive and ductile properties, which make it capable of being turned and moulded into forms; and after being subjected to a red heat renders it indestructible by the action of the atmosphere. According to Vauquelin, European porcelains of good quality contain at least two thirds silex, and alumine from a fifth to a third. A very small amount of magnesia in the mixture, lessens the tendency which the other earths have to contract in baking. Too large a quantity of magnesia however, is to be avoided, as it renders the composition too fusible. A small addition of lime, in place of magnesia produces corresponding effects.

The best clay found in Europe is the felspar obtained from the decomposition of granite rocks.* It is sometimes found in lumps in the clefts of mountains, but may be obtained by pounding or grinding, and washing over, the white or grey granite; by which means the quartz and mica are separated, and the felspar obtained in a fine powder of extreme whiteness. This is done by throwing the ground stones into a running stream, the quartz and mica subside, while the argillaceous parts run off in a thick cream upon the surface of the water. "At the end of these rivulets are catch pools, where the waters are arrested, and time given them to deposite the pure clay, when the water is drawn off, and the solid matter is taken out in square blocks and dried for use."† This clay approximates in some degree to the kaolin of China.

Pure silex is procured from calcined flint quenched in water while hot, which breaks them through their whole substance. They are

* Pure argillaceous earth may be obtained, by dissolving alum in water, and then decomposing it with an alkali. Cornish clays are very smooth and ductile and extremely white, and by Wedgewood's analysis, contain sixty parts alumine, and twenty silex. He adds "the granite itself is sometimes used with the clay, on account of its binding quality, knitting the other materials more closely together by its fusibility." Its fusibility is caused by the alkali contained in its felspar, while in its undecomposed state.

† Parkes' Essay.

then ground in water in a mill of very hard stones, and in this semifluid state, passed off in troughs, through a succession of sieves, each finer than the other, until divested of every coarse particle. Great care is to be used that the stones employed in the mills do not contain any calcareous substances.* A very hard siliceous stone called chert has been employed in the English manufactories for this purpose.

Porcelain clay with 100 or 200 per cent. of sand would make a perfectly opaque body, therefore it is essential to add some alkaline material as a flux, to give the ware its semitransparency. The clays used for porcelain in China, probably contain this ingredient in their native state, which may account for the superior fineness, hardness and semitransparency of the Chinese wares. None of the European clays are identical with the Chinese. The difficulty of adding correctly the fusible ingredient which is so critical in its adjustment; too much rendering the ware vitreous and liable to crack, too little, forming an opaque substance, incapable of that semifusion essential to the real porcelanous texture; is sufficient cause why European porcelain is inferior to the Chinese. By some the superiority of the Chinese is imputed to the great degree of heat which is never reached except in the furnaces of Persia and China; but the more probable cause is a difference in the materials.

It is a curious fact which the manufacturer should not lose sight of, that alumine, silex and lime when separate cannot be melted in furnaces, but when mixed in certain proportions are readily fused; the one mineral acting as a flux upon the other. The perfection of porcelain appears to be obtained when the proportions of the pure ingredients are such, as that the highest and longest heat of the furnace, reaches the point of fusing the silex, and thoroughly incorporating it with the alumine, without melting it, or diminishing its volume. Undecomposed felspar is sometimes added to the porcelain clay and flint, to produce the desired semi-transparency. Its fusible property is owing to the presence of about an eighth part of potash, which acting as a flux upon the silex, causes a semi-vitrification of the whole mass. Vauquelin says that silex forms two thirds of most pottery—alumine from

* Mr. Parkes states that a severe loss was sustained by some large manufacturers, in consequence of having been supplied with prepared flint, which had been ground on stones, containing carbonate of lime. The abrasion of the stones mixed an unknown quantity of lime with the flint.

one fifth to one third and lime from one five hundredth to one two thousandth part. Iron, from the minutest trace to twelve or fifteen per cent, is found in the more common wares, but not in the perfectly white porcelains. Another substance called hoache is used for a fragile, light, but very beautiful ware. It is employed for glazes, and, with some other ingredients, makes a splendid enamel.

The two famous materials employed by the Chinese are kaolin and pe-tunt-se. Reaumur found by analysing them, that they were combined in the following proportions, kaolin consists of silex 74, alumine 16.5, lime 2, and water 7. Petuntse contains of silex 74, alumine 14.5, lime 5.5.

The substitute in Europe for petuntse, is calcined flint: and decomposed felspar for kaolin, the lime or potash being adjusted by art; whereas in the Chinese the alkali is an ingredient in the native compound. The difficulty of adjusting large masses, where success depends on the accuracy of the nicest chemical tests, makes it obvious, that art can scarcely hope to equal that, which is done in the great laboratory of nature. Clays of tolerably good working quality are found in several parts of Europe, similar to, although not identical with those of China and Persia.

Magnesian clays obtained from steatite, have been employed of late years in the composition of porcelain. A small amount of it with other clays limits the contraction of wares.

III. *Processes employed.*

An outline of the processes employed in the fabrication of porcelain forms the next part of the subject under consideration.

The first operation is that of mixing the clay with pure water to the consistence of cream which is effected in vats, by long wooden instruments, which the men move backwards and forwards forcibly through the whole mass. It is is called blunging, and is an operation of great labor. The flint is prepared in the same manner, but in a separate cistern. The grosser parts soon subside, and the finer are drawn off and mixed by measure; the specific gravity of each previously ascertained; the standard being twenty four ounces the wine pint for the clay, and thirty two ounccs the wine pint for the flint. When the clay and flint are mixed in suitable proportions, the whole mass, in a semi-fluid state, is passed through sieves made of the finest silk lawn, in order to detain any particles that had not been sufficiently levigated, and to reduce the whole to the utmost uniformity and smoothness, in which state it is called *slip*. It is next poured into a large vat or cistern, called a slip kiln built with flues under it, connected with a furnace large enough to produce an ebullition in the mixture, which is continued until so much of the water is evaporated as will bring the mass to the desired consistence. When the materials have thus been consolidated into a paste, it is removed from the slip kiln, beaten with mallets and turned over with spades until it is as thoroughly tempered as it can be by this mode of operation. It should not be forgotten that this prepared clay is a mixture of all the ingredients for the body of the ware. After the beating comes the process of slapping, which is done by placing a large lump on a bench or table; when a workman cuts through its diameter with a brass wire, or a twine, and lifting one half with both hands as high as his head, brings it down with all his force upon the lump. He cross cuts and unites it again and again until all the air bubbles of which it was full are driven off. This is an extremely laborious operation, but it is essential to expel all the atmospheric air, before it is exposed to heat, otherwise when it became expanded in the furnace, it would blister and ruin the goods. Mr. Wedgewood and others have employed machinery to effect the results produced by the severe labor of slapping and blunging, which by some is thought equally efficient.

The prepared paste when brought to this state, it is much improved by being kept a long time, the materials thus acquiring a union which they do not acquire by mere mechanical force. It is usual in China to keep the prepared clay fifteen or twenty years, before it is thought fit for use. " In some districts it is the custom for the father to prepare as much clay as will be sufficient for the son, throughout the whole period of his life."*

The French manufacturers of porcelain do not observe so much mystery about their operations as do those of Dresden, and many other European potters. Little is known of the Dresden works, except that they employ none but rain water that has been purified and make their fires only with white wood that has been seasoned. Such was the rivalry between the three royal establishments of Dresden, Berlin and Sevres, that for a long period each made a profound secret of every process and improvement. The same spirit actuates many

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* Parkes' Essay.

of the English manufacturers at the present day, in regard to glazes and enamels, and their proportions of clay and flint. M. Brongniart, director of the Sevres works, with a liberality and patriotic feeling which merit the highest praise, published accurate statements of many details and processes, based upon scientific principles.

It can scarcely be too often repeated that all earthy compounds when exposed to a red heat, act chemically upon one another; and as the precise nature of those actions is unknown, every manufacturer who would avoid disappointment and loss, should examine his materials with the severest chemical analyses.* It needs the most accurate observation, with a sound judgment, so to proportion the ingredients for the body of the ware, that it will receive the glaze without crazing; and so to adapt the heat to both, as to reach the precise point of fuzing the glaze, and uniting it with the ware previously semi-vitrified, and liable to farther changes in the furnace.†

When the paste has gone through all the preparatory processes and has become thoroughly amalgamated by lying long in a mass, a result which time alone can give it, it is ready to be fashioned into any form designed by the artificer.‡ This is performed in three ways, by *throwing*, *pressing* and *casting*.§ The first is done by that ancient machine the potter's wheel, where circular vessels of almost every form and size are made by a succession of lathes. For plates, saucers, tablets, &c. the workman has a mould fixed to the center of a circular board, or table, which revolves horizontally. Over the mould he places a covering of the clay, which by the aid of his hand, and occasionally a small instrument of metal or wood, is turned in a moment to the figure of the mould—is taken off and set on a shelf by an assistant workman—is replaced by another, which in its turn is worked off: and thus in a few hours a vast number of pieces are prepared for the next step of the process. The moulds are made of plaster of Paris which has the property of ab-

* The necessity of chemical analysis will appear, from comparing three native substances much in use. The porcelain earth of Limoges, which is often used without any admixture, is composed of sixty two parts silex, ten alumine, twelve magnesia, seven sulphate of Barytes; whereas the porcelain clay of Cornwall, is a compound of twenty per cent silex, and sixty per cent alumine.

† Crazing is a technical word, signifying the cracking of the glaze, arising from a defective union of the glaze with the body of the ware.

‡ Clay does not readily part with water, beyond a certain amount, therefore the mass does not dry by evaporation. The *surface* would dry if exposed to the sun.

§ New Edinburgh Encyclopedia.

sorbing the water rapidly from the ware, and causes it to slip or "deliver" itself easily from the mould. In the course of three or four hours, they are sufficiently hardened to be taken from the moulds when they are set singly in a room shelved on all sides from the floor to the ceiling and heated by a stove.

Common table plates and saucers, will in this way be ready in two hours to be removed from the moulds, which are again employed for a fresh parcel. When they are taken from this stove, the edges are pared if necessary with a knife, and the whole surface rubbed over gently with the hand, or a piece of soft flannel when they are ready for the biscuit oven.

Articles of oval or irregular forms are made by plaster moulds divided in halves for the convenience of taking out the ware, whenever it is dry enough to be removed, one half the figure being respectively on the two sides of the mould. The clay is rolled into two flat pieces, of the thickness of the ware intended to be made, and after being pressed into the moulds, the two halves are brought together forming a perfect junction. This is called *pressing*, and it is the manner in which handles, spouts, mouldings, &c. are made. They are adapted to the vessels for which they were intended by dipping them in "slip," which is the prepared clay in a semi-fluid state, and when affixed to the ware, the point of union is as perfect as the most solid parts. Figures in relief of the finest workmanship, tables, vases, images, flowers and other curious works in porcelain are thus fashioned.

In *casting*, the clay is poured in a pulpy state into moulds of plaster, which soon absorbs the fluid from that part contiguous to their surfaces; the liquid part is then poured out, and that which remains stiffens so rapidly, that in a few minutes the mould may be removed, when the exterior of the cast is an exact copy of the mould, and its thickness in proportion to the time allowed for the operation.

When the articles have been formed agreeably to the design of the artist, and dried in the stove room, they are placed with the greatest care in saggars and taken to the oven. Saggars are oval cases or boxes made of fire clay, and being flat at the bottom they fit exactly, one forming a cover for another, and being of the same diameter, the workmen place them in piles nearly to the top of the kiln, perfectly enclosing the ware from immediate contact with flame or smoke. The kiln or oven is a conical building, with the receptacles on the outside for fuel, with flues opening into it capable of holding twelve hundred dozen, or more pieces. The bottom of each saggar is carefully covered with sand, and then sprinkled over with the powder of decomposed felspar, before receiving the pieces which are separated from each other by small triangular pieces of biscuit to prevent their adhering to the saggar or to each other, while in the kiln.*

The heat is kept low at first, but gradually augmented until the kiln and its contents, attain the proper maximum. The operation of burning usually lasts two days and two nights, but the process varies in the different charges, and the workmen ascertain the state of the kiln, by examining "trial pieces," which are placed so as to reveal the exact state, of the whole interior of the furnace. Small pieces of porcelain are often enclosed in one case or saggar, but large, or very choice pieces are separately enclosed : in no instance must one piece be in contact with another, and the greatest care is necessary to place them so that every part shall be subjected equally to the heat. After the saggars are placed in the furnace, the door by which they were carried in is walled up, and the heat raised gradually for thirty hours, when fuel is incessantly applied at small openings on the hearth by two men who relieve each other at intervals. The wood employed is well seasoned and cut in slender pieces about a foot long, that the combustion may be effected with the greatest rapidity. When the firing is discontinued, and the smoke ceases, the chimney and all the apertures are closed ; and the kiln with its contents are left to cool as gradually as possible, for thirty hours. This delay in withdrawing the pieces is deemed important, lest the sudden alternation of temperature should cause them to crack. The ware is now in the state called biscuit, and is ready for printing painting and other ornaments, previous to receiving the covering of enamel or glaze. If these were added before the conversion of the ware into biscuit, the shape and texture of the pieces would be injured by the water in the glaze ; the colors would spread ; printed patterns could not be transferred, or other ornaments applied, as the tenderness of the pieces would cause them to warp and crumble, and the truth of the original forms to be destroyed.

Division of labor facilitates this part of the manufacture with the same profit as has been experienced in other departments of the arts. The copper plate printer sits at one end of a room, which

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* Biscuit is ware baked without glazing.

has a counter or table affixed to the sides, like the writing bench in a country school, in front of which, sit a convenient number of workmen, each provided with a little boy, or girl, at hand, as a runner or assistant. The printer moistens a bibulous paper with a brush and applies it to his copper plate which has the engraved pattern upon it designed for the ware in the hands of those sitting on the sides of the room. It is the work of a moment to run it through the press at his left hand, when he delivers it to one of the runners, who hands it to a child with scissors, when the print is cut from the superfluous paper and given to the workman who places it on the piece of biscuit in the mode for which it was designed. The next person on the bench, rubs it smartly with the brush end of a cylinder made of small cords, into the pores of the ware. These manipulations are so methodically conducted that no time is lost, each moving with the regularity of machinery. The papers are easily removed by being immersed in cold water, and gently rubbed off with a piece of flannel or a soft brush, leaving a perfect impression on the ware. When thus printed it is placed in an oven at a low heat in order to evaporate the oil or gum employed in the printing, preparatory to receiving the glaze.

The *glazes* are a part of the potter's art requiring a thorough knowledge of chemical science and a faithful application of that practical skill which is acquired only by experience. A rule stated by Mr. Parkes, is, that a glaze should be capable of expanding and contracting by heat and cold in the same proportion as the ware to which it is applied. To adapt a vitrifiable compound to one composed of both fusible and infusible materials, and which is liable to contract at every additional exposure to high heat, is a process of obvious difficulty. If too easily fused it will not unite with the ware, but will peal off and craze; if too infusible, and the heat is pushed to a greater degree than when the ware was in the state of biscuit, it will warp and become crooked, or perhaps fall into a shapeless mass.

For the Staffordshire or printed ware a beautiful glaze is made of flint, white lead and borax. Flint, which remains unaltered in the focus of the most powerful heat, is easily vitrified when combined in the proportions of ten parts of lead to four of ground flint. When borax is employed the lead may be diminished. The efficacy of borax in promoting the fusion of vitrifiable substances is unrivalled; but it is expensive, and not often employed for common wares. Granite is sometimes substituted for flint in the proportion of eight parts to ten of lead. These ingredients are reduced to a fine powder, and mixed with as much water as will make them into a thick cream. The mixture must be well stirred to keep them equally suspended in the water. The pieces are then dipped in the fluid, and turned rapidly from side to side to equalize the glaze, when they are set on a board for a few minutes, and then are ready for the saggars. The gloss oven completes the series, and after being subjected to a degree of heat sufficient to vitrify the glaze, and unite it to the body of the ware, the oven and its contents are again gradually cooled, the manufacture is completed, and the ware is ready for the market.

It is a desideratum with manufacturers to find some glaze in which lead may be dispensed with on account of its noxious effects upon the health of the workmen, and the injury produced by the decomposition, of its oxides when exposed to the action of acids. The glazing introduced in England by the Ellers, (of throwing salt into the apertures of the kiln, when the baking was nearly completed,) is still practised for some of the common kinds of ware.

M. Brongniart, states that "real or hard porcelain, which is that of Saxony, has for its base a very white clay mixed with a siliceous, and calcareous flux, and for its glaze or covering, felspar fused without an atom of lead."*

Twenty seven parts of felspar, eighteen of borax, four of Lynn sand, three of nitre, three of soda, and three of China clay melted together, and when cold, with three parts more of calcined borax, ground to a fine powder, make a glaze which is successfully employed in one of the English establishments, and has met the approbation of the Society for the Encouragement of Arts. Ground flints, ground flint glass and common salt form another glaze, while another and better still is of ground porcelain, flint and calcined gypsum. The variety of glazes, however, is almost endless, and they are adopted as they are found advantageous in practice, though still treasured up as secrets in most countries, except France.

As alkalies are so powerful in promoting the fusion of intractable bodies, it might be anticipated that they should supersede the use of lead; but it is found in practice that when they are employed beyond a certain amount they do not expand in the same proportion as the

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* Brongniart's Essay on Colors obtained from Metallic oxides, Philosophical Magazine. Vol. xiii. p. 346 et xiv. p. 17 et seq.

bodies on which they are laid; and the result is that they crack and peel off and not only is the ware defaced, but becomes permeable by fluids, is useless, and perhaps falls to pieces.

Soft porcelain was made at Bow and Chelsea, in England, and at the Sevres works, before the disclosures of D'Entrecolles. "It has for its base" says M. Brongniart, "a vitreous frit rendered almost opaque, and susceptible of being worked with clay, and is glazed with an exceedingly diaphanous glass, into which there enters a great deal of lead." It is very white and approaches the condition of enamel, which according to the same authority is "glass made opaque by the oxide of tin and rendered fusible by the oxide of lead."

The vitreous frit alluded to above consists of one part pure clay, three parts of a compound of nitre, soda, alum and selenite, with a large proportion of sand and a little common salt. Another and better rule assigns nine parts prepared flint, nine parts fragments of porcelain ground to powder, four parts calcined gypsum, and one hundred parts porcelain clay. Arsenic was formerly used at Sevres, for some of the work, but the government has ordered a discontinuance of that branch of manufacture. Soft porcelain is very beautiful, and in the painting and brilliancy of colors the most perfect specimens are scarcely inferior to the Saxon or Chinese; but it does not possess the gem like solidity, fineness and translucency, with the almost velvet surface of the genuine pieces of those admired manufactures.

Stone ware is a very perfect kind of pottery, approximating in density and infusibility, to the character of porcelain. When properly made, it will strike fire from steel. Vessels containing sixty imperial gallons are made of this ware, and are found very useful in the arts.

Lustre ware is produced by giving the surface a metallic covering. This is effected after the vessels have been glazed and baked in the gloss oven, by mixing the oxide of a metal levigated to a fine powder with some one of the essential oils, and this mixture is then brushed over the surface. They are then taken to the enamelling kiln, where "the heat dissipates the oxygen, and restores the metals, to their metallic state." Platina produces a lustre resembling polished steel. Gold lustre is of a dark greenish yellow color.

Of Colors.—Those colors employed in painting on porcelain, which will endure the heat of the furnace, are obtained only from metallic oxides. M. Brongniart, describes these vitrifiable colors as being unchangeable by heat, when prepared with such ingredients, as form a flux surround and thus give them protection and brilliancy.*

Carmine, purple and violet, of the most delicate and beautiful shades are obtained from gold; they answer well on enamels, but will not endure the heat of the porcelain furnace. As a fine *rose color* however, and *all shades of red* are obtained from oxide of iron prepared with nitric acid. This oxide is calcined and then fused with a flux composed of borax, sand and minium.

Yellows are produced from the oxide of lead, white oxide of antimony and sand. They may be deepened by red oxide of iron in small quantity.

Blue is derived from oxide of cobalt. The harder and more infusible the porcelain, to which it is applied, and the greater the degree of heat, the more intense will be the color.

Greens may be obtained from the green oxide of copper, and from mixing blue and yellow, but will not endure a high heat. "Pure chromate of lead gives a beautiful green of great intensity on porcelain."

Browns are obtained from oxide of iron; and *Bistres* and *Russets* from manganese, brown oxide of copper, oxide of iron and umber earth.

Black is made by darkening blue, with oxides of manganese, and iron.

Soda and potash are not used as fluxes, because being volatilizing in great heat they abandon the colors which will not then adhere to the porcelain. Brongniart prefers a flux of glass, lead, and borax; while Montamy advises one made of powdered glass, calcined borax, and refined nitre. With either of these fluxes, each color is ground in a mortar of glass, until perfectly comminuted, when they are fused in a crucible until the swelling ceases. The greatest accuracy is required in proportioning the relative quantities, that no more of the menstruum is employed, than is necessary to reach the point of vitrification. If too little were used the colors would be dull—if too much they would spread, and the fine touches of the artist would be lost. After being properly fused, and cooled, they are ground for use. When the artist employs the colors, he rubs them on a glass palette, with some liquid until they are of a suitable consistence to be applied with a hair pencil on the surface of the porcelain. Oil of

* See Brongniart's Essay on Colors.

lavender is preferred in some of the French manufactories as a vehicle; at Sevres gum water is substituted for the volatile oil; oil of turpentine is generally used in England. By due combinations of the vitrifiable colors, every shade may be obtained; but to insure success, it requires on the part of the artist great judgment and skill in combining materials with reference to their chemical action on one another.

Colors should be pounded quickly in a covered glass or agate mortar, and as much care used in rubbing them on the palette as for miniature painting: and the fluidity of the mixture should be kept exactly at the point where the finest strokes can be produced with facility. When the paintings are finished, the pieces are put in the enamel furnace at a low heat, just sufficient to vitrify the flux with which the colors are incorporated. If the execution proves imperfect, they are retouched and burned in again and again, until they are satisfactory to the artist. Eight or ten hours firing are sufficient in the enamel kiln, to burn the colors into the glaze. From the foregoing details it appears that three degrees of heat are required in the different processes of firing porcelain. The heat to which it is subjected in the state of biscuit, is raised to the highest point which the ware will bear: the next firing is to unite the glaze or enamel with the body of the ware and must be only sufficient to vitrify the covering, and so far soften the body, as to cause the union of the glaze with the surface pores of the ware; again, the comparatively low heat of the enamel kiln, must be raised only so high as to vitrify the flux in which the colors are embodied, and to soften the glaze so far, as to permit the colors to unite with it, as the glaze did with the body of the ware in the preceding furnace.

Porcelain is gilded by the use of gold in leaves, and by reducing it to powder with a solution of aqua regia after which it is mixed with gum water and applied with a brush. The fire causes the oxygen to fly off, and restores the gold to its metallic state. Japanners size moistened with oil of turpentine, is spread on parts designed for leaf gold, and when nearly dry, it is laid on with cotton wool. In both cases it is burnt into the glaze in the enamelling kiln. It is then burnished with agate or blood stone, and rubbed off with white lead and vinegar, which is the final process in the manufacture of porcelain.

IV. *Varieties of ware.*

A short description of some of the *different kinds of ware* will conclude this imperfect account of a manufacture, so abundant in particulars of scientific interest, that a volume would be insufficient for the details.

The most ancient specimens which have come down to our own times are the *Babylonian*, the remains of which are the bricks, and some vessels of earthenware, found in the ruins of Babylon. The bricks are thirteen inches square by three thick, with curious inscriptions stamped upon their surface, in a character wholly unknown at the present day. The vessels are a "fine red earthenware," but of their form or design we have no information.

The ancient *Egyptian* is an earthen substance similar to enamel, of a deep blue.

The *Persian porcelain* is so perfect that the body of the ware is like a fine translucent enamel within and without; its grain is so compact and so well resists the fire, that for culinary uses it is equal to vessels of metal. The best Persian is made at Schiraz, though at Yezd in Caramania, and at Ispahan, it is a subject of great interest and competition.

The real porcelain of China is an artificial gem, and furnishes the most perfect examples of this beautiful art. There is a mystery about this fabric however, that has not yet been fathomed by Europeans. Both Reaumur and Wedgewood ascertained by chemical analysis, that there is an inherent difference between the Chinese, and European porcelains: for while many of the latter, particularly the English became perfectly vitrified, and the best Dresden began to bend—the real King-te-ching did not even soften, but remained unaltered at the highest possible degree of heat. Whether this infusibility which is the basis of its superiority, is caused by different proportions of the constituent parts, or by some peculiarity in the original condition of the native earths—or some difference in conducting the processes, is not known. It appears that when the combination is such, as that the vitrifiable constituent can be fused only by the greatest possible heat, and when the heat of the furnace reaches that point, the choicest porcelain is the result. The body of the Chinese ware is a compact and shining substance, the infusible ingredient being enveloped by the vitrified part, producing a smooth impenetrable, lustrous semi-transparent texture of great durability and beauty. The surface of this elegant material is finished with a glazing made of burnt alum—silex—and an alkali obtained from calcined lime and fern ashes; or with hoache which is a very white magnescian earth combined with pure silex.*

"At King-te-ching, a district in the province of Kingsi, there are 500 manufactories, which give employment to more than a million of artisans." This will not seem incredible when it is considered, that such is the division of labor, that sixty hands are employed in completing a single piece.

Petuntse and Kaolin those celebrated materials for porcelain which are unrivalled and perhaps unequalled in other countries, are found in immense quarries of great depth, within twenty or thirty leauges of King-te-ching. Genuine Kaolin is totally infusible in the tremendous heat of the Chinese furnace, which easily melts the solid granite. The constituent parts of Kaolin are silex 52· alumine 42· oxide of iron 0·33. The quarries of Alencon and St. Yrieux in France approximate nearly to the Kaolin of China.† The colors and decorations upon the best Chinese porcelain are very superb, but the paintings are inferior in design to the European.‡

The porcelain tower at Nanking is an astonishing monument of the durability of this unparalleled manufacture. It is of an octagonal shape consisting of nine stories, three hundred feet high, and is covered over its whole surface with the choicest porcelain. This beautiful edifice has withstood the elements, and the changes of seasons for four hundred years, without alteration or injury.

The Dresden China approaches nearest to the oriental, and in some respects excels all other European porcelains, resisting the power of heat with greater obstinacy than any other. In compactness of texture, and infusibility, it is second only to the Chinese. It is not equally white with the best French, but is very splendid in its gilding, and painting, especially in miniature heads, and battle scenes, and generally in the taste and elegance of its forms.

The Sevres royal establishment near Paris, surpasses, perhaps, even the Chinese, in the snowy whiteness of the ware, with the

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* Steatite, or soapstone—see Lardner's Cyclopediæ, article earthenware, &c. Steatite is much used by the porcelain manufacturers at Worcester, &c. in England, see Parkes.

† Lardner's Cyclopediæ.

‡ The Chinese make inferior wares also, and by some it is said, the best is never suffered to go out of the Empire.

splendor of the gilding; while in brilliancy of colors, and elaborate drawings, it is inferior to none. In magnificence and taste, the inventions are unequalled. The vases, urns, tables, and other furniture, are among the most excellent works of art. Some of the paintings are exquisite: miniature heads, historical and classical representations, birds, animals, landscape, flowers, trees, and every picturesque object in art or nature, are executed in a style worthy of the best masters.

Superb porcelain is made in England, inferior only to the French and Dresden, in the whiteness, and infusibility of the ware.

When the complicated character of porcelain, with all its various materials, is considered; the critical adjustment of substances of opposite qualities; the heat to which they are subjected; the varying colors, and the fluids in which they are prepared; the elegant designs—the splendor of the ornaments—the great labor, and the long series of processes which enter into the manufacture, it appears evident that a perfect porcelain is a masterpiece of both science and art. Leaving the dust of the workshops, and following the granite rock from its storm-rifted pinnacle through all the transmutations of nature and art, until it becomes one of the most beautiful ornaments of the saloons of nobles and the palaces of kings; we see realized, the golden visions of the Saxon alchemist, who, it is said, rediscovered the art* while searching for the philosopher's stone. Lithographic drawings of several celebrated vases are annexed.

New-York, March, 1834.

Art. III.—*Researches respecting the radical of Benzoic Acid; by Whöler and Liebig.*

From the third Vol. of the "Annalen der Pharmacie," of R. Brandes, Ph. L. Geiger and J. Liebig.—Translated by James C. Booth.

When in the dark province of organic nature, we succeed in finding a light point, appearing to be one of those inlets whereby we may attain to the examination and investigation of this province, then we have reason to congratulate ourselves, although conscious that the object before us is unexhausted. With such a view, let us examine the following experiments; which, as it regards their extent and connection, present a wide field for cultivation.

The substance with which we commence our undertaking, is the fluid oil of bitter almonds, distinguished from other similar bod-

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* Long before known in China and Japan.

ies, by the property, first rightly investigated by Stange, of being converted in the air, by the absorption of oxygen, into an acid, into the benzoic acid, and which appeared to lay claim to the highest interest from the manner in which it arises from bodies apparently so different. Another peculiarity, which long since drew the attention of chemists and pharmaceutists to the oil, is its containing prussic acid, whose presence seems to bear fixed relations to the nature of the oil. Among the many researches to which these properties have given rise, we mention only the latest by Robiquet and Boutron-Charlard.* As one of the facts most worthy of remark, they observe in their essay, that the fluid oil of bitter almonds, as a whole has its constituents in the almonds and appears to proceed from these constituents first by the action of water. For by the use of alcohol, it disappears altogether and can then in general be no more produced from the almonds; but in place of it they obtained a crystallizable body, formerly unknown to exist and which appeared to them to be the only cause of the peculiar bitter taste of the almonds, and one of the compound elements of the fluid bitter almond oil.†

We have been obliged to leave out of the limit of the present essay, the consideration of the question, whether this oil exists ready formed in the almond, or is generated in the course of the producing process from the fixed constituents,—and a closer examination of amygdalin and its connection with the supposed generation of the oil. The clearing up of this point must be made the subject of particular experiments. To fix firmly the station from which the inquiry took its rise, we make the general remark beforehand, that in consequence of our experiments, we believe that there is a body composed of three elements, always remaining the same in its behavior towards other agents, and which can be considered not alone as the radical of benzoic acid, but at the same time as the root perhaps with slight variations of a multitude of similar combinations. But here we

* Annales de Chimie et de Physique, Vol. xliv. 352.

† In the same essay, Messrs. Robiquet and Boutron-Charlard, express their conviction of the preexistence of benzoic in hippuric acid; now the chief reason on which they rely is an evident error in the Annales de Chemie, V. 43. p. 197, thus instead of saying " Si l'on cesse de chauffer au moment même qu'on sent les vapeurs sulphureuses qu'on mêle la masse noire avec de l'eau ei qu'on la fasse bouillir avec de la chaux, l'acide hydrochlorique en separe ensuite de l'acide benzoique," it should read, " n'en separe point ensuite de l'acide benzoique."

The conclusion as drawn from the unrectified phrase, is in itself contradictory; and this caused the correctness of the sentence to be questioned, which the German copy would have confirmed.

venture to assert, that it would be improper to look for this in the camphorid, whose very existence appears to us questionable, although it is placed here by Dumas without a single demonstrative experiment. A series of phenomena intimately connected with each other was the only guide which presented itself to our view. Suffer us to say that to a certainty we believe a multitude of similar radicals will readily be discovered by calculation and spontaneous changes in the analyses of organic substances, which chemists have undertaken; but here we stop, for science is but little profited by the raising of expectations, as yet unsupported by facts.

Bitter Almond Oil.—The crude oil, which served as the material for our experiments possesses a faint yellow color, the well known peculiar odor and proved itself in all its reactions, and other relations to be a decidedly pure product. We are indebted for it to the kindness of Mr. Pelouze.

Treated with alkali, acid, or a salt of iron, this oil contains a considerable quantity of prussic acid, and apart from the air, either by itself or with potassa, readily changes into benzoic acid.

We were soon convinced that the content of prussic acid bears no relation to the formation of benzoic acid, and endeavored therefore to obtain a pure oil, free from the benzoic and prussic acids and from water. This purpose was fully accomplished in the following manner.

The crude oil was carefully mixed with hydrate of potassa and a solution of chloride of iron by strong agitation and then submitted to distillation. The whole of the oil passed over with the water, and perfectly free from prussic acid. By means of a tube, it was separated from the water, and redistilled in a dry apparatus over freshly burned, powdered chalk.

The oil obtained in this manner is pure, free from benzoic and prussic acids and water, perfectly colorless, very fluid, and has a strong refractive power; its odor is but little different from that of the crude oil; its taste is burning aromatic. It is heavier than water, its sp. gr. being 1·043. Its boiling point is so high that we could not determine it with our thermometers, which extended not above 130° centigrade.* It is easily inflammable, burning with a bright sooty flame.

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* When temperature is mentioned in this essay, the degrees will be understood to refer to the centigrade thermometer.—J. B.

Urged through a red hot glass tube, it remains undecomposed. In the air, in moist or dry oxygen, it is entirely converted in crystallized benzoic acid. In the sun's ray this change is remakably hastened, beginning in the course of a few moments. The same change takes place in the air by the presence of water and potassa, with the formation of benzoate of potassa. If these experiments be made in a glass tube closed with mercury, the rise of this metal proves the absorption of oxygen.

Beside this conversion of the oil into benzoic acid, no third body is formed.

The manner of its purification shews that it is not decomposed or changed by anhydrous alkali, but to the hydrated, its behavior is different. Heated with the hydrate of potassa, apart from the air, it forms benzoate of potassa and evolves pure hydrogen gas.

If the oil be introduced into solution of hydrate of potassa in water, or into alcohol saturated with ammoniacal gas, it is immediately dissolved, and if the air be wholly excluded, a benzoate appears which when potassa is employed, is soon deposited in large shining lamellar crystals. By the addition of water which dissolves the salt, an oily body is separated, which is no longer the oil of bitter almonds.

In the concentrated nitric and sulphuric acids, the pure bitter almond oil is soluble without change. By heating the latter solution, it first becomes a purple-red, and then black with the evolution of sulphurous acid.

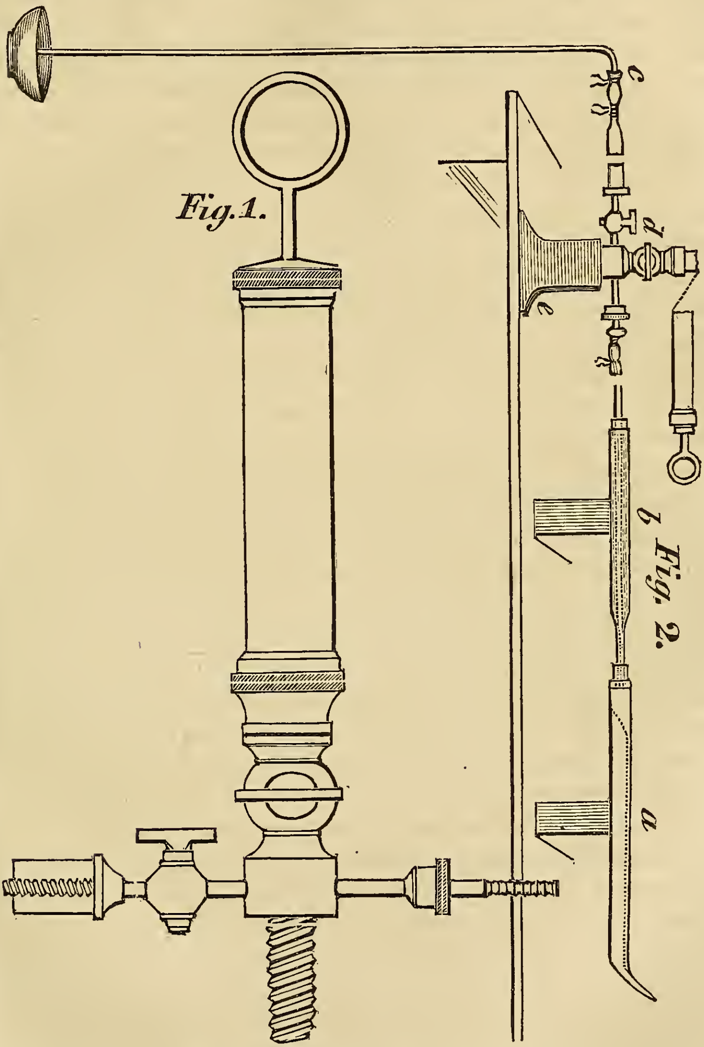
From the action of chlorine and bromine, new compounds arise which will be described in another part of this essay.

The composition of this pure oil was ascertained in the usual way by ignition with the oxide of copper. To expel the hygroscopic moisture from the oxide of copper, we have employed in our experiments a small air pump invented by Gay-Lussac. Since it has not been described by himself, we take the liberty of annexing a sketch of the same; for it may undoubtedly be viewed as one of the most important improvements with which organic analysis has been enriched, both as regards its convenience in use and the safety it ensures in hydrogen examinations.

Fig. 1. is the pump alone of half the actual size; it is furnished with common bladder valves, and terminates beneath in a strong screw, to fasten it firmly for use.

Fig. 2. shews the pump as connected with the ignition tube *a*, which is united by means of a well fitting cork with a long tube *b*,

filled with chloride of calcium. *c*, Fig. 2. is a glass tube about thirty inches in length fastened above to the pump by a short and broad piece of a tube, and dipping below in mercury. It has no other



object than to prove by the rise of the mercury that all the connections of cork and caoutchouc are tightly closed, and it is removed as soon as the pump is put in operation. Indeed it may be dispensed with altogether, since the tightness may be judged of after a little practice, by the force with which the air rushes in through the opened cock *d*, after exhaustion.

To the table is screwed a strong wooden post *e*, Fig. 2. on which the pump is fastened by its screw. The moisture contained in the oxide of copper mixture is expelled at the same time with the air by exhausting the ignition tube, from which by degrees the last trace is removed, since the air dried by the chloride of calcium is often admitted by repeated exhaustions and opening of the stop-cock.

It is evident that the expulsion of moisture may be hastened, from substances from which we have to fear no loss by warmth, if the ignition tube be put into a tin tube filled with hot water.*

This small air pump presents yet another advantage, of which we frequently availed ourselves in our experiments. The oil and other fluids submitted by us to analysis possess so high a boiling point, that the small bulb filled with them, was emptied of the portions of the liquid, not before this part of the tube had almost attained a red heat. It thence frequently happened, that the gas was suddenly evolved with such violence, as to throw some oxide of copper into the chloride of lime, and thereby the experiment became unavailing, at least for the determination of hydrogen. This is completely avoided by turning the open end of the small bulbs towards the closed end of the ignition tube, introducing the oxide of copper in layers and then exhausting. The small bubble of atmospheric air in the bulbs, now suffices to expel all the contained moisture, particularly if the ignition tube be brought to a more vertical position, and exhaustion be repeated. In order not to mention the accuracy of the result disadvantageously, it may be added that this manipulation with very fluid substances is throughout superflous.

We return to the pure oil of bitter almonds. Ignited with these precautionary measures, it yielded,

I.	0.386 gramme*	=1.109 carbonic acid,	and	0.200 water.
II.	0.341 "	=0.982 "	"	0.175 "

---

* This pump may be used with great convenience for drying substances which suffer drying only in a vacuum, at the common or a slightly raised temperature. In place of the ignition tube, a short tube is fastened on which is closed beneath, or a small glass globe, in which is placed the substance to be dried.

† The gramme is always to be understood as the weight or series of weights employed.

Which for 100 parts gives its composition as

	I.	II.
Carbon, .	79.438 .	. 79.603
Hydrogen, .	5.756 .	. 5.734
Oxygen, .	14.808 .	. 14.663

These proportions calculated by volume give

14 atoms of carbon, .	1070.118 .	79.56
12 " hydrogen, .	74.877 .	5.56
2 " oxygen, .	200.000 .	14.88
	1344.995	100.00.

According to the composition of this body, the formation of benzoic acid by the mere reception of oxygen is wholly inexplicable, because in this change no other products could be detected. According to the analysis of benzoic acid by Berzelius, it contains 15 atoms of carbon, 12 of hydrogen and 3 of oxygen. This circumstance induced us to repeat the analysis of the crystallized acid, and of the same united to a base.

Analysis of Benzoic Acid.—We employed for this analysis not only the common benzoic acid obtained from resin, but also a portion prepared expressly for this purpose from the oil. In both cases, we assured ourselves of their purity. The acid was fused, weighed and introduced by pieces into the ignition tube ; this was then warmed to the fusion of the acid, and equally parted at half the length of the tube upon the sides. It was then filled with warm oxide of copper, again brought before the air-pump, and submitted to ignition, which with this very volatile substance could be but slowly conducted.

	Carbon.	Water.
I. 0.523 gramme of acid yielded	1.308 .	0.238
II. 0.522 " " "	1.302 .	——
III. 0.305 " " "	0.760 .	0.136.

According to these results then, the analyses gave for 100 parts,

	I.	II.	III.
Carbon, .	69.155 .	68.970 .	68.902
Hydrogen, .	5.050 .	Water was lost.	5.000
Oxygen, .	25.795 .	. . . .	26.098

These numbers give the atomic composition of the same, as

14 atoms carbon, .	107.0118 .	69.25
12 " hydrogen, .	7.4877 .	4.86
4 " oxygen, .	40.0000 .	25.89
	154.4995	100.00

The variation of the composition of benzoic acid, thus obtained, from that which Berzelius found by the analysis of the benzoate of lead, caused us at first to mistrust our own analysis. Upon nearer inspection however, we found it necessary to admit, that the cause of the difference between the two analyses should be sought in the composition of the salt analyzed by Berzelius. We therefore undertook the analysis of the acid united to a base, and chose the benzoate of silver because of the facility with which it is obtained pure and crystallized, and because the oxide of silver shews but little disposition to form compounds.

Neutral nitrate of silver mixed with an alkaline benzoate in solution, gave a thick white precipitate, which by warmth became crystalline, and completely dissolved in a greater quantity of boiling water. By cooling the solution, oxide of silver was deposited in long shining crystals, which by drying under the air pump, neither lost their lustre, nor diminished in weight.

By heating in a porcelain crucible, this salt melts, puffs up, and after the deposited charcoal is consumed, leaves very white metallic silver. In this manner we determined the atomic weight of the acid.

I. 0.391 gramme of benzoate of silver left 0.184 of metallic silver.
II. 0.436 " of the same gave 0.205.

According to these numbers, the composition of the salt is,

	I.	II.
Oxid of silver,	50.56	50.52
Benzoic acid,	49.44	49.48

And the atomic weight of the acid as the mean of both analyses, is 142.039.

We now submitted the salt of silver to ignition with oxide of copper, and obtained from 0.600 grammes of the salt, 0.797 grammes of carbonic acid, and 0.122 of water.

The composition of the acid, as deduced from these numbers, consists in 100 parts of

Carbon,	74.378
Hydrogen,	4.567
Oxygen,	21.055

Calculating from the atomic weight already found, we obtain,

14 atoms carbon,	107.0118	74.43
10 " hydrogen,	6.2397	4.34
3 " oxygen,	30.000	21.23
	143.2515	100.00

By comparing the analysis of the crystallized acid with that combined with the oxide of silver, it is at once evident that the difference between them is that the former contains one atom of water, which is wanting in the latter.

The only difference then between the analysis of Berzelius and our own, lies in this content of water. For from the atomic weight found by Berzelius, as well as from the behavior of the oxide of lead, it follows that the oxide by union with benzoic acid, does not separate the water of the same, but that this water enters into the composition of the salt. On being heated, and especially in the crystallized state, when, as we have just seen, it contains one atom of water, it loses a portion of its acidity.

In fact, if from the atomic weight of benzoic acid, as obtained by Berzelius from the oxide of lead, namely, . 152.1423

We take off one atom of water, . . 11.2479

We get for the atomic weight of the dry acid, 140.8944.

If according to this corrected atomic weight, we calculate the carbon and hydrogen of Berzelius' analysis, we likewise obtain 14 atoms of carbon and 10 of hydrogen.

These comparisons will suffice to remove every doubt respecting the composition of benzoic acid and the statement of Dumas, that this acid contains hydrogen and oxygen in the same proportion as water, is certainly an error, which he will undoubtedly correct.

Returning from this digression to the consideration of the oil of bitter almonds, and its conversion into crystallized benzoic acid, we now find this phenomenon capable of an easy explanation. The acid is formed by simple oxydation, the oil absorbing in the air or in oxygen gas, 2 atoms of this element.

The formation of benzoate of potassa from the oil, when the latter is heated with hydrate of potassa, depends upon the decomposition of the water in the hydrate, whereby the oil takes one atom of oxygen, while hydrogen escapes in the form of gas.

We have farther mentioned that the oil with a solution of potassa in alcohol, forms likewise without the access of air a benzoate of potassa, and that then by the addition of water, an oily body separates from the alcohol possessed of different properties. As far as we have examined this new body, it admits of no doubt, that in case the constituents of alcohol do not enter into its composition, it originates either form taking oxygen from the bitter almond oil, or from the decomposition of water. In the former case it would be composed according to the formula $C^{14} H^{12} O$, in the latter the formula $C^{14} H^{14} O^2$.

After the determining of this point, and a reviewing of the combining relations of bitter almond oil yet to be considered, we believe it naturally follows that this oil is in its pure state a hydrogen compound, wherein the radical of benzoic acid is combined with 2 atoms of hydrogen, instead of with oxygen as in the acid. This radical as yet unobtained insulated, is composed of $C^{14} H^{10} O^2$. We call it *benzöyl*, (the ending from ὕλη material, matter.) The consequent name for the pure oil of bitter almonds is *hydrobenzöyl* (hydroguret of benzöyl,) and for the benzoic acid, *benzöylic acid*, (benzöyl acid.) We will however use the common names benzoic acid and bitter almond oil, except in theoretical demonstrations. We will see how easily the remaining relations, to which we now come, will be perceived and comprehended.

Chlorobenzöyl.—If through the bitter almond oil we conduct dry chlorine gas, the latter is absorbed with considerable heat, and hydrochloric acid is evolved ; but besides this, no other product which warrants the conclusion of a farther decomposition. As soon as the formation of hydrochloric acid begins to cease, the liquid becomes yellow from the solution of chlorine, but the overcharge of this gas is again expelled by boiling. Finally, if the liquid, be heated to boiling, in contact with chlorine, and the formation of hydrochloric acid is no longer perceived, we obtain a new compound, perfectly pure. This is the chlorobenzöyl, (chloride of benzöyl.)

The chlorobenzöyl, is a transparent fluid of the sp. gr. 1.196. It possesses a peculiar odor in the highest degree penetrating; in particular, strongly affecting the eyes, and reminding us of the pungent odor of horse-radish. Its boiling point is very high : it is inflammable, burning with a bright, green-edged, sooty flame.

It sinks in water as an oil, without solution. After a considerable time, or sooner by boiling, it separates entirely into crystallized benzoic acid and hydro-chloric acid. It suffers the same change if kept in moist air for a length of time. If chlorine be conducted through a mixture of hydrobenzöyl and water, the oil disappears, and the water congeals into a crystalline mass of benzoic acid.

The chloride of benzöyl may be distilled unchanged over anhydrated baryta and lime.

Warmed with alkalies and water, this chloroide forms at the same time a chloride of the metal and a benzoate of the alkali.

In all these decompositions beside the benzoic and hydro-chloric acids, no third body is formed, whence it clearly follows, that in this compound, chlorine and benzöyl must be in such proportion, that by the separation of water into its constituents, these last, exactly suffice to form on the one side hydrochloric, and on the other anhydrated benzoic acid,—the latter, at the moment of its formation, taking up one atom of water. Hydrobenzöyl (bitter almond oil) consists of,

$$(14\,C+10\,H+2O)+2H.$$

By the action of chlorine, two atoms of hydrogen unite with two atoms of chlorine to form hydro-chloric acid, which is evolved. But the hydrogen gives place to two atoms of chlorine according to the following formula;

$$(14\,C+10H+2O)+2\,Cl.$$

With the constituents of water this body is decomposed in such a manner that two atoms of hydrogen unite with two atoms of chlorine to form hydrochloric acid, while the freed oxygen unites with benzöyl and forms benzoic acid.

By analysis we proved the correctness of the composition. We dissolved it in dilute ammonia, super-saturated it with nitric-acid and precipitated by the nitrate of silver. 0.719. grm. Chlorobenzöyl gave 0.712 gm. chloride of silver. This gives for 100 pts. 24.423 of chlorine.

Ignition with oxide of copper in the common way, where the fluid in small bulbs is placed in the ignition tube, proved altogether impracticable and indeed upon the grounds already mentioned. All these experiments failed us, since every time, even by the most cautious heating, the content of the small bulb, or the fluid present in the oxide of copper, was at once converted into gas, and thereby either the oxide was converted into the chloride of calcium, or a part of the substance was carried away unignited.

It was therefore necessary to introduce the weighed fluid by drops among the *oxide* of copper; by a slow progressive heating we succeeded perfectly in terminating the ignition without difficulty.

0.534 gm. Chlorobenzöyl yielded 1.188. carbonic acid and 1.180 of water, which in 100 pts. gives,

Carbon,	-	-	-	60.83
Hydrogen,	-	-	-	3.74
Oxygen,	-	-	-	11.01
Chlorine,	-	-	-	24.42

From these numbers we obtain by volume as the theoretical result,

14 atoms of	Carbon, - -	107.018 - -	60.02
10 "	Hydrogen, - -	6.239 - -	3.51
2 "	Oxygen, - -	20.000 - -	11.55
2 "	Chlorine, - -	44.265 - -	24.92
		177.522	100.00

By calculation the numbers yield a somewhat smaller quantity of carbon and hydrogen than was obtained by analysis. The reason undoubtedly is that in preparing the chlorine compound, perhaps $\frac{1}{1000}$ of the *oil of bit. alm.* escapes with the chlorine. In no case is the difference of such importance, that the conclusion to which we arrive respecting the composition of this body, can be considered false.

With respect to the properties of chlorobenzöyl we have yet to remark that by warmth it dissolves Phosphorus and sulphur, which by cooling again separate in the crystalline form. With sulphuret of carbon, it may be mingled in every proportion, and, as it would seem, without suffering decomposition. With solid chloride of phosphorus, it becomes strongly heated, with the formation of liquid chloride of phosphorus and an oily, strongly smelling body which we have not farther examined.

The very remarkable behavior of chlorobenzöyl in dry ammoniacal gas, and its decomposition with alcohol, we will treat of in a separate part of this essay.

If chlorobenzöyl be treated with metallic bromides, iodides, sulphurets or cyanurets, such an exchange of constituents ensues, that a metallic chloride on the one hand, and a combination of benzöyl with bromine, iodine, sulphur or cyanogen on the other hand, are generated, which are composed similarly to the chlorobenzöyl.

Bromobenzöyl.—This compound is formed directly by mixing bromine with hydrobenzöyl (bitter almond oil.) The mixture becomes heated and throws forth thick vapors of hydrobromic acid. By heating still farther, this acid as well as the excess of bromine is expelled.

The bromobenzöyl (bromide of benzöyl) is a large foliated, crystalline mass of a brownish color, soft and at common temperature nearly semifluid. It melts by a gentle warmth into a brownish yellow fluid. It possesses an analogous odor to the chloride, though much fainter and therefore aromatic. In the air it smokes faintly, but fumes strongly upon a slight elevation of temperature. It is combustible and burns with a bright and smoky flame.

By water it is slowly decomposed. Warmed under the same, it becomes a brownish oil, and after long boiling separates into the hydrobromic and crystallized benzoic acids.

In alcohol and ether it is readily soluble without decomposition. By dilution it may be obtained from both in a crystalline form.

Iodobenzöyl.—This compound seems not to be formed by the direct union of its constituents. It may however readily be formed by warming iodide of potassa with chloride of benzöyl. It may be distilled over as a brown fluid, which congeals to a brown crystalline mass. It then contains iodine dissolved. In its pure state it is a colorless, foliated, crystalline substance, easily fusible, but by this operation is always decomposed with the formation of some iodine. In odor, behavior to water and alcohol, and in inflammability, it does not differ from the preceding compound.

Sulphurct of benzöyl.—This may be obtained by distilling the chloride with sulphuret of lead. It passes over as a yellow oil, which congeals to a yellow, soft and crystalline mass. It possesses a disagreeable odor resembling sulphur. It seems not to be decomposed by boiling with water. With a boiling solution of caustic potassa, it slowly forms benzoate of potassa and sulphuret of potassium. With alcohol it is not decomposed. It is inflammable and burns with a bright, sooty flame, evolving sulphurous acid.

Cyanobenzöyl.—Hydrobenzöyl dissolves a certain quantity of cyanogen and receives also its odor, but by warming this gas may be again expelled without change.

The true compound we obtained by distilling chlorobenzöyl over cyanuret of mercury. The compound passes over as a gold colored oil, and mercury remains behind.

In its pure, fresh state, the cyanobenzöyl, (cyanuret of benzöyl) is a colorless fluid, but it rapidly changes to yellow. It possesses a pungent odor, strongly exciting tears, and at a distance resembles oil of cinnamon. Its taste is biting, sweetish, and afterward much like prussic acid.

It is heavier than water, in which it sinks as an oil, and by which it is soon converted into benzoic and hydrocyanic acids. If a drop be suffered to spread upon water, it will be found completely changed in a day by the sun's light into benzoic acid crystals. By boiling with water, it is rapidly decomposed into the benzoic and prussic acids. It is readily inflammable, burning with a white, but very smoky flame.

Benzamide.—By conducting dry ammonia over pure chlorobenzöyl, the former is absorbed with much heat, and the liquid is converted into a white, solid mass, consisting of a mixture of muriate of ammonia and a new body which we call *Benzamide.* For in its behavior and composition it bears a perfect analogy to oxamide.

The perfect saturation of chlorobenzöyl with ammonia takes place at first with such violence, that it is slowly and difficultly attained; for the rising solid mass soon begins to protect the yet unsaturated portion from farther contact of the gas. It is therefore often necessary to take the mass out of the vessel, crush it and again continue the action of ammonia.

It may be inferred from the formation of muriate of ammonia at the same time, that by the union of the two bodies, a decomposition of ammonia takes place; for, as we have before remarked, the chlorine in chlorobenzöyl is contained as chlorine and not as hydrochloric acid.

It is indeed imaginable that the exchange of elements happens when water is poured over the white mass to expel the muriate of ammonia; but the behavior of cyanobenzöyl sufficiently proves, that this separation first occurs, the moment the ammonical gas comes in contact with the chlorobenzöyl.

The cyanobenzöyl suffers an altogether analogous change in ammonia to the chlorine compound; it forms benzamide and cyanuret of ammonia, which latter rises in consequence of its fluidity, with the excess of ammonia, and sublimes in the form of brilliant crystals.

To isolate benzamide, the muriate of ammonia formed is washed out of the white mass with cold water, and the remaining benzamide is dissolved in boiling water. By cooling, the solution deposites crystals.

By neglecting perfectly to dry the ammonia over burnt lime or hydrate of potassa, a corresponding mass of benzoate of ammonia is formed, by the action of the moist gas upon the chlorobenzöyl, and the same proportion of the new body is lost.

Also when the chlorobenzöyl has not been fully saturated with ammonia, then upon treating the mass with hot water, the formed benzamide, as is proved by its behavior to acids, is either wholly or in part decomposed according to the quantity of free chlorobenzöyl.

Lastly under certain circumstances, (of which we yet know but little, but it is probable chiefly when the chlorobenzöyl was not perfectly free from chlorine), by saturation with ammonia an oily body may be observed, possessing an aromatic odor resembling bitter almonds, and by which the contained benzamide has the property, before it is dissolved, to melt to an oil by warming with water, and again to separate from the solution in the form of drops of oil, which congeal in a short time.

Pure benzamide shews a remarkable phenomenon in its crystallization. It deposites from a boiling hot solution by rapid cooling, pearly, leafy crystals very similar to chlorate of potassa. By long cooling and at a certain concentration, the whole liquid congeals to a white mass consisting of very fine, silky crystals resembling caffein. After one or more days and often after a few hours, large cavities may be observed in this mass, in the centre of which may be observed one single or several large well formed crystals, into which the silky fibre has been converted; and gradually this change of form spreads throughout the mass.

The form of the crystals of benzamide is a right-rhombic prism, which by the enlarging of two opposite planes becomes tabular. They have a highly nacreous lustre, are transparent and exhibit upon water a fattiness, easily swimming on its surface.

At 115° C. it melts to a water-like liquid, which congeals by cooling to a large-leaved crystalline mass, wherein are frequently found cavities with well formed crystals. At a stronger heat it boils and distills over unchanged. Its vapor is similar in odor to bitter almond oil. It is easily inflammable and burns with a sooty flame.

In cold water, the crystallized benzamide is so little soluble that the solution scarcely possesses taste. In alcohol on the contrary it is readily soluble. In boiling ether it is also dissolved, and from this solution in particular can be obtained in well defined crystals.

Covered with caustic potassa at common temperature, the benzamide evolves no ammonia. Nor does its solution mingled with a salt of iron at common temperature give a precipitate, as indeed it in general gives no reaction with a metallic salt. By boiling the benzamide with a concentrated solution of caustic potassa, ammonia is evolved in abundance and a benzoate of potassa remains. By heating to boiling the solution of benzamide mixed with a salt of iron, it becomes cloudy and throws down a benzoate of iron.

By dissolving benzamide in a strong acid with boiling, it disappears, and in its place benzoic acid in crystals separates from the cooled solution, while an ammoniacal salt has formed. By employing hot concentrated sulphuric acid, the formed benzoic acid sublimes. If boiled with pure water, even for a considerable length of time, this change into benzoic acid and ammonia does not take place.

The analysis of benzamide is easily effected by ignition with oxide of copper. The relative proportion of nitrogen and carbon was ascertained by the ignition of the substance "*in vacuo.*" The ignition tube was provided at one end with a thirty inch tube, the one end dipping in mercury, and the other drawn out to a strong point, which by means of a caoutchouc tube could be connected with the small air-pump.

The air was then exhausted, and as soon as the mercury had risen in the tube to the height of some twenty seven inches, the point at the other end of the ignition tube was closed by the blowpipe, and the ignition was commenced.

From these experiments it followed that nitrogen and carbon were evolved in the proportion of one to fourteen. It yielded farther,

	Carbonic acid.	Water.
I. 0.400 grm. benzamide	=1.012	+0.208
II. 0.489 " "	=1.235	+0.253

Calculated from these, benzamide contains in one hundred parts,

	I.	II.
Carbon,	69.954	69.816
Hydrogen,	5.780	5.790
Nitrogen,	11.563	11.562
Oxygen,	12.603	12.832

By volume these numbers give as the theoretical result,

14 atoms Carbon,	107.0118	69.73
14 " Hydrogen,	8.7360	5.69
2 " Nitrogen,	17.7036	11.53
2 " Oxygen,	20.0000	13.05

This composition clearly explains not merely the manner of the formation of benzamide, but also its behavior with acids and potassa, that is, its conversion into benzoic acid and ammonia.

If 2 atoms of ammonia be added to the composition of chloride of benzöyl, we obtain the formula;

$$14C+10H+2O+2Cl = \text{chlorobenzöyl}$$
$$12H \quad +4N = \text{ammonia}$$
$$\overline{14C+22H+2O+2Cl+4N.}$$

Abstract from this 2 atoms muriate of ammonia,

$$\begin{array}{l} 14C+22H+2O+2Cl+4N \\ \quad\quad 8H \quad +2Cl+2N, \text{ we then obtain} \\ \hline 14C+14H+2O+ \quad 2N, \end{array}$$

which is the true composition of benzamide and by adding to this last 1 atom of water, the formula becomes, $14C+16H+3O+2N$ which expresses the true composition of neutral dry benzoate of ammonia. This salt consists of

$$\begin{array}{lll} 1 \text{ atom benzoic acid,} & = & 14C+10H+3O \\ 1 \text{ “ ammonia} & = & \quad\quad 6H \quad +2N \\ \hline & & 14C+16H+3O+2N. \end{array}$$

Benzamide exhibits some phenomena in its decomposition which deserve an ampler consideration than we have bestowed upon them. Heated with a larger quantity of dry caustic baryta, it partially fuses, appears to become a hydrate, evolves ammonia, and then distills over a colorless, oily body, deserving notice. It is lighter than water, in which it is insoluble. It possesses an aromatic, sweetish taste not unlike that of fluid chloride of carbon (C^2Cl^5) and discovers itself particularly by its almost sugar-sweet taste. This oil burns with a clear flame, and is changed neither by caustic alkali nor by acids; even potassium may be melted in it by gentle warmth without change.

The same substance is evolved in considerable quantity and unaccompanied by ammonia, when benzamide and potassium are melted together, in which case the latter without much violence appears to be almost wholly converted into cyanuret of potassium.

If the vapor of benzamide be conducted through an ignited narrow glass tube, only a small portion is decomposed and that without depositing a trace of carbon. The greatest part passes over unchanged, mingled with a small quantity of the sweet tasting oil before mentioned. This is evidently a peculiar substance, which by its behavior intimates an altogether simple composition and certainly deserves much attention.

Clorobenzöyl and alcohol.—The chlorobenzöyl may be mingled with alcohol in every proportion. By observing the mixture, it will be remarked that it begins to grow warm in the course of a few minutes and this warmth increases to such a degree, that the fluid enters into self-ebullition, throwing off at the same time thick vapors of hydrochloric acid. If water be poured over it when the action is ended, then separates an oily body, colorless, sinking in water and possessing an aromatic, fruit-like odor. Washed with water and heated with chloride of calcium, it is freed from water, alcohol and acid, with which it may be adulterated.

We could not long remain in doubt respecting the nature of this new product; it must be benzoic ether. For if the decomposition of chlorobenzöyl by alcohol be analogous to that by water, as is suggested by the formation of hydrochloric acid, then through the decomposition of water in the alcohol, must benzoic acid on the one hand be formed and ether on the other; which two at the moment of their origin unite to form benzoic ether. On account of its unexpected appearance, we sought to assure ourselves by an analysis, particularly as this analysis would give us great control over the composition of benzoic acid.

We did not employ the fluid for analysis, until after careful washing with water, the latter was entirely abstracted, by repeated digestion with muriate of lime, and by several distillations in a dried apparatus. It is to no purpose to distil it over chloride of calcium, because its boiling point is so high, that water passes over at the same time. 0.622 grm. gave 1.632 carbonic acid and 0.375 water, which in 100 parts is equivalent to,

Carbon,	72.529
Hydrogen,	6.690
Oxygen,	20.781

Or by volume,

18 atoms	Carbon,	137.5866	72.37
20 "	Hydrogen,	12.4796	6.56
4 "	Oxygen,	40.0000	21.07

These proportions exactly point out a compound of,

	C H O
1 atom dry benzoic acid,	=14 – 10 – 3
with one atom of ether,	= 4 – 10 – 1
	18 – 20 – 4

To assure ourselves of the perfect identity of benzoic ether thus formed, with that prepared after the common method, we produced the latter in abundance by the distillation of benzoic acid with a mixture of alcohol and hydrochloric acid. By comparing the two properties of the two bodies obtained by such different methods, not the slightest difference was observable. Odor, taste, sp. gr. and behavior to acid and alkali, were in both precisely the same.

The analysis of benzoic ether, by Dumas, varies materially from ours in the hydrogen content. Let this serve to shew how difficult it is to become free from preconceived opinions in researches of a similar kind.

Benzoin.—The body which we would denote by this name on account of its relation to the substances treated of in this essay, has indeed been already examined by Stange, but scarcely farther than in its external properties. It is the same which is introduced into chemical works as bitter almond oil, camphor or camphoroid.

Benzoin arises under certain circumstances from the oil of bitter almonds. We obtained it, for example, accidentally, as others have done before us, by the rectification of the oil with caustic potassa, where it remains swimming upon the potassa. We produced it however in greater quantity by suffering the bitter almond oil, to stand several weeks with a concentrated solution of caustic potassa. Robiquet and Charlard, have also noticed the same change in contact with alkali when free from the access of air. We can confirm this observation. In our experiment the oil was almost wholly converted into solid benzoin, though only after several weeks. Lastly, we produced it by saturating water with bitter almond oil and mingling the solution with some caustic potassa. In a few days the benzoin begins to deposite in needle-form crystals.

In all these cases the benzoin possesses at first more or less a yellow color, but is rendered perfectly pure and colorless by solution in hot alcohol, treatment with blood charcoal and frequent crystallization.

Benzoin forms clear, highly lustrous, prismatic crystals. It possesses neither taste nor smell. At 120° it melts to a colorless liquid, which again congeals to a large striated crystalline mass. At a stronger heat it boils and distils over unchanged. It is inflammable, and burns with a clear, sooty flame. It is insoluble in cold water, but if the latter be boiling, a small portion is dissolved, which again separates by cooling in capillary crystals. It is taken up by hot alcohol in much larger quantity than by cold.

It is decomposed neither by concentrated nitric acid, nor by a boiling solution of caustic potassa. With concentrated sulphuric acid on the contrary, it at first gives a violet blue solution soon becoming brown and by warming it takes a deep green color, with the evolution of sulphuric acid and blackening of the whole mass.

This body as we perceive, offers but little interest in its properties; but it is the more remarkable in its relation to hydrobenzöyl, from the fact, as analysis proves, that it has a perfectly similar composition, and is therefore an isometric modification of the same, as is apparently shewn by its unintelligible origin from the oil, through the equally inexplicable action of potassa, but from the access of air.

1.00 grm. of benzoin yielded by ignition, 2.860 grm. Carbonic acid and 0.512 of water. This gives its composition in one hundred parts;

Carbon, . .	79.079
Hydrogen, . . .	5.688
Oxygen, . . .	15.233

consequently the same (atomic) proportion of the same elements as in hydrobenzöyl.

It is indeed imaginable that the very different properties of benzoin and hydrobenzöyl arise from the manner of the hydrogen's combination, which in the first may be united to one atom of oxygen as water. But, as this difference depends on such a very different manner of the hydrogen's combination, the idea, that the hydrogen, can no longer as in the oil, be replaced by another body, as chlorine, seems to be refused by the behavior of benzoin to bromine.

Thus if bromine be poured over it, it becomes heated and boiling and evolves hydrobromic acid in abundance. After this acid and the surplus of bromine are expelled by heat, the benzoin will be found changed to a brown viscous fluid, smelling like bromobenzöyl, but unlike the latter it is soft. With boiling water it appears to be decomposed either not at all or exceedingly slowly. With caustic potassa, it is indeed decomposed by boiling, but even then with difficulty. The alkaline solution mingled with hydrochloric acid deposites fine needle-form crystals, which do not appear to be benzoic acid, nor can they be unchanged benzoin, since they readily dissolve in alkali. Could we consider the above mentioned bromobenzoin, as an isometrical modification of the corresponding benzöyl compound, then were it imaginable that a new acid had formed by the above decomposition with alkali, which might be an isometric modification of benzoic acid.

We in vain endeavored to reconvert benzoin into hydrobenzöyl. Fused with hydrate of potassa, it changes like the oil into benzoic acid, with the evolution of hydrogen. But again it differs from the oil, in its behavior with a solution of potassa in alcohol. It is dissolved in the alkaline solution with a purple color, and presently the whole congeals to a mass of fine, foliated crystals. With water these form a milky fluid, from which by cooling after it has been heated, thick flakes of needle-form crystals are deposited, which are unchanged benzoin.

General Observations.—Reviewing and collecting together the relations described in the present essay, we find that they all group around one single compound, which does not change its nature and composition in all its combining relations with other bodies. This stability, this consequence of the phenomena, induced us to consider that body as a compound base and therefore to propose for it a peculiar name, i. e. benzöyl.

The composition of this radical we have expressed by the formula $14\,C+10H+2O$.

In combination with one atom of oxygen, benzöyl forms dry benzoic acid, and in combination with one atom of oxygen, one of water, the crystallized acid.

In combination with two atoms of hydrogen, it constitutes pure bitter almond oil. When this oil changes in the air into Benzoic acid, it takes up two atoms of oxygen, one of which with the radical generates benzoic acid and the other with the two atoms of hydrogen forms the water of the crystallized acid.

Farther, the hydrogen of the oil or the oxygen of the acid may be replaced by chlorine, bromine, iodine, sulphur or cyanogen, and the bodies proceeding thence, comparable with the corresponding compounds of phosphorus, all form, by their decomposition with water, on the one side a hydracid, and on the other benzoic acid.

The replacement, of two atoms of hydrogen in the bitter almond oil by an acidifying base, appears to us in all cases a strong argument for adopting the opinion, that this hydrogen is in a peculiar manner combined with the other elements; this peculiar method of combination may be hinted at rather than pointed out by the idea of the radical, which is borrowed from inorganic chemistry.

Although both benzamide and benzoin were originally in connection with the radical, they are wholly without its sphere, and must be considered as peculiar bodies, bearing no nearer relation to benzöyl, than the cuttings of bone to ammonia.

Since we cannot compare the ternary base with cyanogen, because the greater number of elements must occasion far more complicated decompositions, and because they have no prevailing resemblance, we believe it not improbable, that there is more than one group of organic bodies, for example, the fluid oils, which may have this same radical as a compound basis. How far such a conjecture is correct may be ascertained by accurate analyses of other fluid oils in which the formation of benzoic acid, has been observed by mere oxidation in the air, or by action of nitric acid, particularly analyses of the oils of fennel-seed, anise-seed and cinnamon.

If an inference be allowed from the behavior of chloro and cyanobenzöyl, respecting the peculiar nature of the combination, which by the admission of water to the bitter almonds, causes the formation of prussic acid and hydrobenzöyl (crude oil of the bitter almonds), then it appears to us possible, without wishing to anticipate the experiment, that there is contained in the almonds a union of cyanogen with another body which is different from the hydrobenzöyl merely in the content of oxygen, so that by the admission of the constituents of water, hydrobenzöyl on the one side, and prussic acid on the other are formed; it farther seems to us probable, if amygdalin is a decomposition product of this combination with alcohol, that a similar exchange takes place as in the decomposition of chlorobenzöyl by alcohol, with this only difference that the cyanogen or its constituents enters into the new combination.

Benzoin in regard to its formation and physical properties possesses great similarity to the solid crystalline substances which are formed in other fluid oils; accurate analyses will unfold whether these (camphoroids) are the same in composition with the fluid oils from which they proceed, and whether the cause of their different states or their other varying properties lies in the manner in which their constituents are combined.

Letter from Berzelius to Wöhler and Liebig respecting Benzöyl and Benzoic acid.

Accept my thanks for the very interesting communication of your united and important researches respecting the bitter almond oil.

At your request I have examined my former experiments in regard to the composition of benzoic acid and find the result of your analysis wholly confirmed.

I have also made as you desired an analysis of benzoate of silver, and from 100 parts of the salt previously dried at 100° obtained by careful ignition, 46·83 gm. of metallic silver, which agrees as nearly as could be expected with the theoretical result calculated by you (46·86.)

You have remarked that my analysis of benzoate of lead, perfectly agrees with the same. A later analysis made with sulphuric acid and alcohol gave the same result and consequently confirmed the atom of water of crystallization found in my first analysis.

I herewith transmit the result of an analysis made in 1813 of sublimed benzoic acid, which I ignited in a tube with chlorate of potassa and chloride of potassium.

After this method 0·335 gm. of the acid yielded 0·138 gm. water and 0·855 of carbonic acid, which gives in one hundred parts;

Carbon, - - -	68·85
Hydrogen, - - -	4·99
Oxygen, - - -	26·66

These numbers agree exactly with the composition of the hydrated acid $C^{14} H^{12} O^4$.

In vain did I endeavor to separate water from the benzoic acid, by saturating the crystallized acid with a given quantity of oxide of lead, and therefore could not infer the presence of water of crystallization; this analysis farther gave four atoms of oxygen, although I had previously found by analysis of the salt of lead that the acid in it saturated three times as much of the oxide as in the neutral benzoate, I was therefore induced since the results did not correspond, to reject this analysis of the crystallized acid.

I next ignited given quantities of the neutral benzoate of lead, after endeavoring to free the same from water of crystallization by fusion.

Each analysed quantity of the salt was produced by itself; I have always followed the same principle, because a fault in one single operation may become a constant fault in every analysis; I therefore fused each portion of the salt by itself and always obtained varying results; the cause of which difference I believed must be ascribed to to the volatilization of undecomposed benzoic acid. Upon now comparing the results of these analyses it is evident that different quantities of water still remained in the fused salt.

In order to prevent the volatilizing of the acid I employed the salt of lead; this is the analysis I have described. The result calculated from the corrected atomic weight and compared with your analysis is as follows;

	Result of the former.	Result of the correct analysis.
Carbon, - -	75·405 - -	74·703
Hydrogen, - -	4·951 - -	4·356
Oxygen, - -	19·644 - -	20·941

The former analysis differs therefore from the theoretical composition by 0·702 carbon and 0·595 hydrogen, which overplus just as much diminished the oxygen.

The results consequent upon your examination of the bitter almond oil, are the most important which vegetable chemistry has thus far received, and promise to diffuse an unexpected light over this part of science.

The circumstance that a body composed of carbon, hydrogen, and oxygen, combines with other bodies, particularly with such as form salts, after the manner of a simple body, proves that there are ternary composed atoms (of the first order) and the radical of benzoic acid is the first example proved with certainty, of a ternary body possessing the properties of an element. It is true indeed that we have before considered *sulphuret of cyanogen* (Schwefelcyan) as such, but you are aware that its combinations may be viewd as sulphurets and the body itself seems to be a sulphuret of cyanogen.

The facts proved by you give rise to such reflections, that we well may view them as the dawning of a new day in vegetable chemistry. I might for this reason propose to call the first discovered radical, composed of more than two elements, *proin* from πρωï dawn, in the sense ἀπὼ 'πρωῒ ἕως ἑσπέρως Acts xxviii. 20.) or *orthrin* (from ὀρθρὸς aurora,) from which the names *proic acid*, *orthric acid*, and *chloroproin chlororthrin* can be employed with more facility. In consideration however, that the long received name benzoic acid would thereby become changed, and that we are always accustomed to respect names in general use where they do not embrace a double idea, by deriving new names from them, as *boron* from borax, potassium from potash, &c., it therefore appears to me in every respect more proper to employ the word proposed by yourselves and to change the term benzoic, into benzöylic acid.*

From the moment we know with certainty of the existance of ternary atoms of the first order, which combine after the manner of simple bodies, it will greatly facilitate expression in the language of formulas, to denote each radical by a peculiar sign, through which the idea of the combination to be expressed, instantly and clearly strikes the reader. I will illustrate this by a few examples. Thus if we put benzöyl $C^{14} H^{10} O^2 = Bz$, then we have,

* We at first chose the name benzoin, as it properly stands in Berzelius' letter, and have since substituted the word benzöyl, that benzoin may be used for isometric hydrobenzöyl; by the ending in *yl*, we are the less reminded of strychnin, salicin, &c.—W. & L.

$\dot{B}z$ = Benzöylic acid.

BzH = Bitter almond oil.

$BzCl$ = Chlorobenzöyl.

$B'z$ or BzS = Sulphuret of benzöyl.*

$Bz+2NH^3$ = Ammoniuret of benzöyl, (benzöyl ammonia.)

If we put Amid = NH^2, we have

$Bz+NH^2$ = Benzamid or bitter benzöylamid.

$\ddot{C}+NH^2$ = Oxamid.

$K+NH^2$ = Potassiumamid, (Berzelius' Chemistry, I. 794.)

$N+NH^2$ = Sodium amid.

If we further place *oil of wine,* which I propose to call etherin, C^4H^8 = E, we find

$E+2\dot{H}$ = Alcohol.

$E+\dot{H}$ = Ether.

$E+HCl$ = Muriatic ether.

$E+\dddot{N}\dot{H}$ = Nitric ether.

$E+\dot{B}z\dot{H}$ = Benzöylic ether.

$E\dddot{S}+\dot{H}\dddot{S}$ = Sulphovinic acid according to Hennel and Serullas.

$E+2\dot{H}\dddot{S}$ = Sulphovinic acid according to Wöhler and Leibig.

$2E\dddot{S}+\dot{H}$ = Sulphuric oil of wine.

$E+\bar{A}\dot{H}$ = Acetic ether.

$2E+\bar{A}\dot{H}$ = (Pyroacetic spirit?) (Brenzessiggeist. German.)

$E+2PtCl$ = Zeise's ethereal salt.

$E+2\dot{P}t$ = Ethereal oxid of Platinum.

$E+2Pt$ = Ether platinum.

Suppose by way of experiment, there is an oxid of etherine = $\dot{E}$, then †$\dot{E}+\dot{H}$ = Ligneous spirit. (Aolzspiritus. German.)

$2\dot{E}+\dot{H}$ = Acetal or Doebereiner's new oxy-ether.

From these two last formulas, we perceive that acetal has the same relation to ligneous spirit, that pyroacetic spirit bears to acetic ether.

But I think it necessary to insist, that such formulas be employed only when the ideas they express, are advanced in some measure to confirm truths, else they would lead to a Babylonian confusion.

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* It will be of great interest, to ascertain the behavior of this body to sulphurets.—B.

† According to the results of analyses of both bodies, which I sent by letter to Berzelius, and which will appear in one of the succeeding numbers of this Journal.—J. L.

ART. IV.—SUPPLEMENT *to a letter to the Hon. W. J. Duane, (late Secretary of the Treasury of the United States,) as published in this Journal, Vol.* xxv, No. 2, p. 290; *respecting the saving of houses and their inhabitants from fire, and the obtaining supplies of water and warm air.*

1. THE interview which I had with Dr. Franklin, as noticed in the preceding letter, occurred at Paris; and probably, at a moment when news had just arrived there of an extensive conflagration in England; and I remember moreover, that it was late in the evening. Hence, the conversation is to be considered as *casual*, and also as having been *brief.*—It was confined to the great fact, that fires were *often extensive* in England, but were rarely so in France; and it assigned two *causes* for this difference; 1st, that the staircases and passages in the houses of the French, were usually constructed with incombustible materials; and 2d, that the interior of their rooms had little *exposed* wood-work in them, unless such as had mortar or some other incombustible material placed close behind it. Thus the Doctor's conversation regarded merely the *spreading of fire* from room to room, and from house to house.

As the object of my letter is more extensive, I add here, from myself, that the French mode of constructing and of finishing houses, is attended with two *other* important advantages, besides that of checking the *extension of fires.* First—*Life* is evidently favored, if a secure passage in case of fire, is made from every apartment in a house, down to the ground floor; since such inmates in the house as are awake, will then take care of themselves; and their neighbors from without may fearlessly enter the house to look after those who are sleeping, and also after others who are unable to assist themselves, (as the sick or infirm, and little children.) Secondly—*valuable moveables*, in such circumstances, may easily be carried off and placed out of the reach of danger.—The plan of the French, therefore, especially in a country like the U. States, where so many houses are built throughout with *wood*, is not to be overlooked in great towns, notwithstanding the expertness of their firemen, and their ample provision of fire-engines, ladders, &c. and still less is it to be disregarded, where these advantages are wanting.

I may be allowed to add here, that I have conjectured, that at the time of Dr. Franklin's communication as above mentioned, this philanthropic man, whose mind was ever meditating upon what might be useful to others, had the design of writing, when at leisure, a *memoir* on these subjects *at large;* but nothing of this kind appears to have been left behind him.

2. The second article selected for notice in the present supplement, is the *Chevalier Aldini's* publication respecting "*the art of preserving from fire:* [that is, as] applied to firemen and persons exposed to fire; with a series of experiments in Italy, Germany, and France." Prof. Griscom, having ably and clearly analyzed this work,* I shall select from his account of it, a *few particulars* belonging to my present subject; since they regard the security of those professionally employed in *extinguishing* the flames in burning buildings.

These particulars are as follows.—The bodies of persons exposed to the action of fire, are directed to be covered with dresses formed out of Amianthus, or with Sir Humphry Davy's metallic gauze; and their hands also are to be protected by suitable gloves. The parties when they move among flaming materials, are likewise to carry before them, metallic gauze shields; and to be provided with pincers and other suitable implements, to perform such work as the case may require. They are moreover directed, when surrounded by flames, to keep themselves as much as may be in a state of motion; and are particularly cautioned not to remain long in a body of *compact* smoke.

The methods prescribed in the Chevalier Aldini's book, are not denied to be susceptible of farther improvement; but enough has been established to make the art, even in its present state, appear valuable as to practice. The experiments of several persons of high estimation, as Messrs. Gay-Lussac and D'Arcet, are among those cited to support its credit; besides the patronage of several governments, and besides other distinctions which were profitable as well as honorable.—One particular is added of moment, in favor of the plan, as regards expense; namely, that *wool* may be so prepared, as to become in "*ordinary*" cases, a substitute for amianthus in applying this art to use. In short, enough is said, to make it appear of consequence, not only to import into the U. States, the necessary dresses, &c. which belong to this art; but also to engage persons well acquainted with what belongs to this subject, to visit the U.

* See Vol. xx, No. 1, of this Journal, p. 96.

States, in order to communicate their knowledge to such persons here, as may be depended upon, to apply it to use when required. Whatever expense may be incurred in doing this, must fall short of the amount of the value put in Boston, upon a *single* picture, not yet exhibited, as well as of the sums actually paid or pledged for a *single statue* in several great cities of the U. States.

3. It is known that feather-beds, elastic cushions, and sometimes straw, notwithstanding its combustibility, have been placed in front of houses in flames, in order to receive persons who might venture to leap upon them from the upper apartments; but nothing of this kind seems in any degree so promising, as what has been called *a stretched sheet;* the invention of which is attributed to an Englishman of the name of Wecks; the plan being stated to be as follows. A sheet is provided, in the borders of which large hoop-holes are left all round; in which holes, persons standing below (passengers and others) place their hands, in order to grasp the sheet firmly, and keep it extended. Hence, when the party leaping down from above strikes the sheet, the following results may naturally be expected. 1st. The holders of the sheet will drop it a little, in consequence of their slackening their hands, as soon as the weight of the descending person is felt. 2nd. The sheet is lowered farther, from the stretching given to the threads of which the sheet is composed. 3d. The sheet suffers a new depression, when the form of its surface, which at first resembles the *cavity* of the section of a hollow *sphere* turned with the open part upwards, is made to approach that of the horizontal section of the apex of a hollow *cone* also turned upwards. Each of these principles adding to the effect in diminishing the impulse of the descending weight, so as to make it *gradual*, the leap becomes harmless.—Accordingly, when an exhibition of the operation of this sheet was made in Southwark (which is considered as a part of London,) in the presence of numerous spectators; among whom were some officers of the London Police who may be said to have been there, professionally; various persons leaped upon the sheet, one after the other, from the elevated parts of a house; and all reached the ground without accident.

The statement of this exhibition has probably not reached the U. States upon any other than newspaper authority; but as the case is recent, it may easily be inquired into; and, in the mean time, it is open to new experiments in this country. If the matter be substantiated, we may then congratulate ourselves on the discovery of an apparatus so simple, cheap, portable, and easily preserved, that few towns possessed of a moderate population need be without one; and smaller collections of people ought to be instructed in the mode of providing themselves with an *extempore substitute;* which however will probably require some experiments before it can be fully relied upon.

4. In casting my eyes lately over the paper published by *Lord Mahon,* (now Earl Stanhope) on the subject of securing buildings from conflagration, the *following passage* struck me (if I may be pardoned the liberty of stating it) as containing an oversight. The passage is this, being correctly copied, including its italics: "In most houses it is necessary *only to secure the floors.*"—Perhaps by *floors* his lordship means only the *upper boards* on which we tread, in passing over a house; and if so, I have nothing more to remark;—but if he designs to intimate that in most houses we may omit attention to the *stairs,* he differs from French architects and from Dr. Franklin. In any event, as his lordship in his paper tells us how to treat *stairs* and *even partitions,* so as to make them fire proof, we are at no loss how to proceed.

5. Whoever wishes to see the *whole of Lord Stanhope's paper* as given in the London Philosophical Transactions, will find it reprinted with very trifling variation in *Dodsley's Annual Register for* 1779; and consequently he will there learn *how* to carry his lordship's plans into execution; and he will also learn the *cost* of each part of the work (as prices stood in England in 1778).—Should his lordship incline to listen to the voice of some of the friends of humanity, he will leave behind him minutes of the *theory* by which he conducted his practice, as well as an account of some of his more important unrecorded experiments.

6. In the present supplement I may be allowed to confess, that I spoke too transiently in my letter of the probability of *steeping timber in certain liquids;* in order to render it difficult for slight flames to take hold of it. Our chemists and artists are however now ripe for making important discoveries on this subject. The same may be said as to the invention of modes for *extinguishing flames when raging.*—The offer of *prizes* for successful methods of accomplishing these two purposes may have the *double* effect of exciting the attention of skillful men to these matters; and of engaging the public to become sufficiently interested in them to think of appying what shall be brought into their view to actual practice.

7. In what follows in this article I speak with diffidence. I have merely hinted in my letter, that *iron beams* have been employed in houses, to prevent the progress of fires. I now add, what I recollect to have heard, that a building in England furnished with iron beams and having in it a number of stories, being in *flames*, *the floors fell in together;* and again, we have been told lately, of a conservatory for plants near London, surmounted by a *dome* framed with iron, which suddenly fell in, *no fire* having *concern in the event.*—This has led me to suspect the operation of a new principle, in these cases, to which, perhaps, due attention has not yet been paid; namely, the *expansibility of iron* in consequence of an increase of its temperature; notwithstanding iron is the least susceptible of expansion of any of the common metals. In the case of the *inflamed building having different stories*, (as mentioned above); even if it were ascertained that its iron beams were placed, *some* horizontally and *some* perpendicularly, still it will be allowed to be possible that all of these beams were *not heated alike*, as being placed at different distances from the main body of the fire; and in this case, a want of symmetry in the expansion of the different beams may have led to a contrariety of action in different parts of some of the main supports of the building, in consequence of which the downfall in question occurred.

In confirmation of this hypothesis I relate what follows. 1. The celebrated Thomas Paine once had in his possession an iron bar of more than one hundred feet in length, which he placed in a horizontal position on rests, along the side of a brick wall; and fastening one end of it immoveably in the wall, he left the other end free to move itself; when he found the free end so much affected by the changes of temperature in the air, that it served him as a sort of thermometer as to the temperature of the atmosphere. 2. An engineer in the service of the United States has lately proved by observation, that when a coping-stone of *great length* is used for covering the upper tier of masonry in a wall, it cannot at times be prevented from shrinking, so as to leave a chasm between itself and the next coping-stone, by which rain can have access to the walls.* 3. The frequent difference of opinion of men of science and artists as to the cause of the ex-

* It should be observed here, that it has been said, that some stones are capable of imbibing a great quantity of water, with *little* alteration in their dimensions.

plosions of boilers in steam engines, seems a *third* ground for the conjecture, that these gentlemen have not in their examinations on such occasions, invariably comprehended *all the principles* which have had a bearing on the subject.*

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* It is only in a note that I am entitled to speak of the *tenacity* of metals, as having some concern in the case of machinery, where heat is employed; there being nothing in my letter to call for the mention of it; but a note may be admitted in this place to shew that both tenacity and expansion in metals may, on these occasions, have a singular operation. The case will become more striking where two metals are employed; for the metals may differ from each other on these points, as also be operated upon in different proportions as to these points at different rates of temperature. Of all metallic substances mercury has the least tenacity and most expansion; but both iron and copper have peculiarities worth noticing in these respects; particularly where both metals are employed together in the same machinery.

What I have to state on this subject will be founded chiefly on different data collected from the able report made on the explosion which occurred to the boilers of the steam boat New England, at Essex in Connecticut, on Nov. 9, 1833; to which report are affixed the names of three truly scientific gentlemen in the vicinity; namely, Benjamin Silliman, W. C. Redfield and Denison Olmsted; and those also of an expert artist, Daniel Copeland and an experienced navigator by steam John F. Lawson.

1. *Datum.* Mr. Smeaton gives the expansion of iron as 151 ten thousandth parts of an inch at 180 degrees of Fahrenheits thermometer; and that of copper as 204 ten thousandth parts of an inch at the same temperature; and Mr. Smeaton says, that his results sufficiently agreed with what others had ascertained at the same period. See Hutton's Mathematical Dictionary, article expansion; and the Phil. Trans. Vol. xlviii.—The report above mentioned says (p. 20), that the experiments of Guyton Morveau, shew, that the tenacity of iron compared with copper is nearly as 540 to 302; or more than 80 per cent in favor of iron.—2. *Datum.* Copper is weakened by the action of heat at about 250°; and a copper boiler will bear a greater pressure when cold, than when heated. An iron boiler if heated not beyond 480°, will bear a greater pressure, than when cold, (p. 14.)—3. *Datum.* Melted copper at a white heat, will pass through a high column of water, and remain for some time ignited at the bottom of the vessel. According to the statement of Mr. Adam Hall, ten pounds of copper heated to such a degree of redness as to be merely visible in the dark, will convert a pound of water into steam; making more than 27 cubic feet at the common atmospheric pressure. It follows therefore, that copper flues as they have, on the whole, an extensive surface and great density, may produce an uncontrollable quantity of steam, even at a heat far below redness. (See, says the report p. 17, the valuable experiments of Prof. Walter Johnson, in the American Journal of Science.)—4. *Datum.* There were a few square feet of iron plate in the chimnies [that is to each chimney, there being two boilers to the engine;] to which [plates] the steam had access on one side; and there can be no doubt that these flues frequently became *red hot.* (p. 17 and aslo p. 5.)—5. *Datum.* The position of the safety valve on the steam pipe was 20 or more feet from the boilers. * * * The diameter of the steam pipes which led from the boilers was about 10 inches. (p. 8.) —6. *Datum.* The copper varied in thickness in different parts. (p. 6.)—7. *Datum.* A heavy fall or crash, or a sudden cracking, preceded the first explosion, which was(by all but one person who heard it,) thought unusual. (See 9, 10, and 20;

As to the fall of the *dome* which covered a lofty building, lately erected near London, and where it does not appear that there was any fire; if the dome was defective neither in its form nor in its architectural construction, it may be asked whether the *iron* contained in its frame in the parts exposed to the *sun*, was not preternaturally heated, compared with other parts not so exposed; and whether this did not cause a *distortion* in the *whole* of the fabric; or in other words, did not cause a rupture in some parts, not to be resisted, which was followed by the downfall of the dome.

The preceding *seven articles* in this supplement have had reference to circumstances connected with *conflagrations*.

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and 15.)—8. *Datum.* The first explosion instantly or within three minutes followed this noise. The second explosion followed very closely, "lapping on to the first." (15.)—9. *Datum.* It is not said in what manner "the few square feet of iron plate were connected with the copper parts of the engine;" but it is plain, that the changes in the two metals were not exactly of corresponding natures.—10. *Datum.* The second explosion may have been aided by the sudden generation of steam, consequent upon the water being thrown (during the commotion which things soon underwent) upon heated copper, capable of generating an "uncontrollable quantity of steam;" which steam had to pass a tube of 20 feet in length and 10 inches in diameter, before it reached the safety valve.—11. *Datum.* The copper was bent in parts in an extraordinary manner; that is, folded in massy doublings like a garment. In other words, the copper may be supposed to have contracted its dimensions: that is, I think we may say that in the case of the copper of the boiler, it was not merely ruptured; especially as the body of water and of steam may be supposed to have given an even distention to the copper from within. (See report p. 5 and other places.)—12. *Datum.* We may conclude, that it will in future be wise to direct, that all the connected parts of the steam engine containing water or steam, should be made out of one metal: and that such other precautions should be taken as to steam engines in steam boats, as foreign governments (English and French) have discovered by experience to be *preventive of mischief.*—We add, that a strong suspicion is to be entertained, that the safety valve was not sufficiently attended to, during the stoppage of the steam boat New England at Essex; and it is clear, that the door of the furnace was not closed, but was left open so as to admit the entry of air to increase the heat. Under these circumstances, the boiler, &c. had not a corresponding strength.

To the above *Data* collected from the interesting Report on the cause of the explosion of the boiler of the steam boat New England, we add the following particulars from Mr. Dalton. 1. Iron expands nearly one eight hundredth part in length or 265 part in bulk by 180° of Fahrenheit. 2. Iron is to copper in expansion as 2 to 3. 3. Although Mr. D. differs from Mr. Smeaton on these subjects, it is because Mr. S. has omitted some points in his calculation, and not because he was incorrect. 4. We mention by the by, as something curious, that Mr. D. places the greatest density of water at 36°. Fahrenheit, and not at 40°. See new system of Chemical Philosophy, Part I. pp. 28—33, and 43. N. B. It is unfortunate that the expansions of metals have not been tried beyond the temperature of 180° of Fahrenheit.

But although another very important article on the same subject will call for attention before we conclude, yet as it will be of some length from having two incidental circumstances of a more general nature connected with it; we shall pass at present to three separate articles, which regard a different topic; namely, *supplies of water*, which of course may be noticed here as belonging to my original paper. I proceed therefore as follows.

8. Article. Since my original paper was printed, I have met with the following passage in Earl Dundonald's *Treatise on the intimate connection between agriculture and chemistry;* which will be read with pleasure; for although it contains little not referred to by myself, yet the statements and hints of an author of some note, will of course be more valued than those of an anonymous writer.

"It has been neglected by me, (says Lord Dundonald,) under the article Peat and Peat Masses, to state, that their waters are very injurious to cattle:—and that such defects may be remedied by collecting the *rain water* that may fall on the roofs of the dwelling houses and offices, in tanks, properly constructed, and having no communication with the soil.

"Should not the buildings be conveniently placed for affording the cattle a supply of water from the tank, or should the extent of such be insufficient to collect the quantity of water that may be required; *sheds or hovels*, covered with tile, should be erected in a central field, conveniently situated for securing to the cattle their daily supply.—A further benefit will ensue by the shelter and protection that such sheds or hovels will afford the cattle.

"This method of collecting rain water, (and which is practised in many countries,) may with great advantage be adopted in the upland, chalky, or gravelly soils; or in the marshes near the sea-shore, where the springs either are at a great depth, or where the water is brackish. In upland countries, water may be collected into tanks in great abundance, during the rainy seasons, by leading the surface water into such receptacles, without increasing the expense of sheds or hovels. But in fens, morasses, peat mosses and marshes, or flat grounds, where the soil is full of vegetable or animal matters, or where the water is brackish, a supply of water can be obtained only by the assistance of the roofs above mentioned." So far Lord Dundonald. (See his edition of 1803, addenda, pp. 241, 242.)

I cannot omit repeating here, for the sake of horticulturalists of all descriptions, that by collecting rain water in a garden by means of a shed, they may always have a supply of water at hand for their plants, of a proper quality and proper temperature, which may be kept in casks or in a tank, under the shed; freezing being out of the question in the season when plants call for water.

9. We have in the present article to turn to a subject truly mortifying to those who have to contend with it, namely, the *contaminated waters belonging to certain closely peopled cities.* But in discussing this subject, we shall in order to obtain credit for our statements, appeal to competent evidence, already before the public, as regards one of these cities, namely, the city of New York.

We begin, then, with what Dr. Hosack has asserted in a discourse before the College of Physicians and Surgeons of New York, on November 5, 1829; (which the Common Council of New York, *three weeks afterwards*, by a formal vote, requested to have published.) The passage to which we refer is as follows. (See p. 41.)

"At present (says Dr. Hosack) it is calculated, that nearly one twelfth part of the surface of the city of New York, is occupied by privies; and consequently, (the soil consists of a very porous sand,) a constant percolation from the deposits of filth into the wells of the city, must be the result. This (he says) accounts for the impurity of the water, drank by the inhabitants of New York; and the effects it produces upon strangers, before they have become habituated to its use." Dr. Hosack then proceeds to quote as follows, from the famous traveller, Mr. Volney. "It is so true (says Mr. Volney who passed some time in New York,) that the water drank in the lower parts of the city receives filtrations from the *cemeteries and privies;* that in Front Street, I found the water in my decanters become *ropy*, if kept three days in the month of May, and at length acquire a *cadaverous stench.*"—To this passage from M. Volney, I shall add another from the same author, when speaking of Philadelphia. "Burying grounds, (says M. Volney,) should be removed from the hearts of cities. Philadelphia has four vast cemeteries, in the finest and most populous parts of the city; of the smell of which, in summer, I was quite sensible."—So far M. Volney.

Founded on these authorities, (of which of course that of Dr. Hosack is the most weighty, particularly from the great superiority of his means of information,) we shall now consider what are the *remedies or palliatives*, which a case like that of New York admits; directing, however our chief attention to the effect of privies; for as Dr. Hosack well observes (in another part of his discourse,) the evil of cemeteries may be rendered trifling. In effect, bodies in cemeteries are often placed in tombs; worms also soon devour the soft parts of the corpse, or lime may be used for the same purpose, where tombs are wanting; cemeteries also, occupy only small portions of a city, whereas privies are spread throughout the whole of it; lastly the dead body of a man is an object of small bulk, whereas the *living man* contributes daily, and it may be, during many years, to the mass of offensive accumulations which are not always immediately removed.*

But to return to our subject, we say that the *preventive* or *remedial* expedients offered in cases like that before us, appear to be *four.*

1st. The most direct is the establishment of *common sewers* for carrying off that filth, by which wells and springs of towns in circumstances like New York, are polluted. The utility of the sewers in Rome, beginning from the time of the Tarquins, is well known; and they may be adopted every where; a rush of water, however, being always provided for their being occasionally cleansed. The course of these drains will necessarily be *short;* but to be durable, like those of Tarquin, they should have a bottom paved, with solid sides and a solid roof. It is said that a loaded cart of hay might have been drawn through those of Rome.† 2d. The next measure to be proposed under the present head, is to *bring good water into a city*, when it ceases to have good water of its own; and this was a second resource employed by Rome, which abounded in *aqueducts*, so as even to supply baths and fountains.‡ In our day, however, we may confine ourselves to the introduction of water for *beverage* and *cooking;* for what regards the washing of our houses and meaner objects, may be accomplished by the aid of water that is always at hand.—Sometimes *river* water is used to great advantage for drinking; or water from a powerful spring may be introduced by an *open* channel, (as is the case of what is called the *new river* water

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* In some places, vaults under churches, have been found very offensive, owing to local causes, and particularly so in the case of graves dug in a clayey soil, and holding water running into them from the roof of the church. But this is a very limited object.—For a most revolting account of some of the vaults in the city of Rome, see Dr. Hosack's quotation from Mr. Theodore Lyman, Junior's account of the "Political state of Italy!" Boston, 1820; chap. xvii, p. 210.

† Dr. Hosack says that such sewers have lately been introduced into Philadelphia, and they have long been known in London and other cities in Europe.

‡ See Fabretti de aquis and aqueductibus Veteris Romœ. Romœ 1680.

in London ;) although this last water does not satisfy every one as well as drunk out of decanters. Paris and Philadelphia employ river water : eastern princes also often have had river water carried after them in their distant journies and expeditions : and an emperor of China is known likewise to have had his daily supply of drink, for himself and family, taken out of a *particular part* of the river passing by his palace at Pekin. The water of our rivers in its origin, comes to us out of the heavens ; it has, indeed, as it flows in its bed, many additions, but it also makes many depositions ; it is well ventilated ; and if it receives some mineral substances which may affect us in one way, it generally receives others, in its course, which neutralize these, otherwise its waters would lose their character for salubrity ; and by evaporation, it is freed from many of its impurities. Rivers cannot remove the bad effects of marshy margins ; but we must avoid such spots, which are equally injurious, whether the waters concerned be moving or stagnant, salt or fresh.—We must not be too nice also as to the water of rivers,* for our bread, butter, cheese and wine, undergo processes to form them, which shall not be named here, yet they afterwards become relished by all ; just as certain river waters, when taken to sea, first ferment, and then become useful. The water to be adopted for aqueducts, we may add here, should come from a good level, to avoid the expense of raising it artificially to that level ; it should be such as will not only satisfy our chemists, but should have its purity confirmed by the looks and testimony of those in its neighborhood who have been accustomed to it ; no manufactories should be allowed to interfere with it ; it should be plentiful for present purposes, and sufficient to meet any early expected increase of demand for it ; and if not quite so clear as might be wished, it ought to stand for a short time in a reservoir, before its final distribution ; (two reservoirs in such cases being likely to be wanted.) If not at last of a purity, however, to satisfy every one,

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* Our enjoyments may suffer by too much refinement. Cleanliness has been called the "grace and virtue of exterior life;" and Mr. Lieber, in his recent publication respecting the Girard college, has an ingenious enumeration of its uses ; (see p. 69—71.) but what lover would apply a microscope to the skin of his mistress ? It is this microscopic nicety which must be kept out of view ; our happiness being better consulted when we seek to govern ourselves by the principle of *content.* Let us take up our different objects then in the fullest perfection in which they first offer themselves to us, and take care afterwards that they suffer no unnecessary change for the worse when in our hands.

perhaps a smaller aqueduct of a still choicer quality may be provided for decanters; and private subscriptions may defray the expense. So much for our second expedient for obviating the impurity of the water in certain cities.—We proceed to remark that, 3d. *Rain water* is still an useful resource; which, if it does not give content, as collected from the roofs of our houses, may be obtained in sufficient purity by means of fine table cloths stretched out in the time of rain, and pressed down in the center by a smooth and clean stone. Such water, being properly preserved in a cellar, in close vessels or in glass, may be used for decanters, for our tea and coffee, and for soups. Fabrics of less delicacy may be used for collecting rain water to be employed in boiling our meats and vegetables. With respect to water for common cleansing in house work, it will always be found in the common springs and wells of cities. 4th. *Distilled water* is the last resource to be mentioned on the present occasion; and it may be resorted to by those to whom it may give pleasure. An admirable man, the elder Dr. Heberden, is said to have used distilled water for the drink of himself and family; and as it can easily be had good by various processes, it cannot be improper to mention it here; were it only to show how careless those must be who are content with such water as was given to M. Volney. Better would it be to have country water brought into a city by its milk carts or other conveyances, than to be satisfied with such foul and perhaps unhealthy water, as that which becomes spontaneously "ropy," and sends forth ultimately a "cadaverous stench."*

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* On the general subject of this article, we add, that Mr. Dalton arranges *water* according to its *purity* thus: first distilled water, then rain, then river, then spring water.—By purity, he means freedom from foreign bodies held in solution; but he says, that the hardest spring water seldom holds in solution more than one thousandth part of its weight of foreign matter; and that this is usually carbonate and sulphate of lime. See Dalton's New System of Chemical Philosophy, Part 2, p. 271.

The perfect accuracy with which water may be separated in a *pure* state from the waters of the sea, and other masses in which it may be found combined or intermixed, is remarkable. But these powers of elimination or extraction are not confined to evaporation by the sun, or to distillation. Fishes possess similar powers; for although they drink a salt element, and are wholly enveloped in it, yet, excepting oysters, &c., no salt is found in the *flesh* of fish. Nor are the wonders of creation confined to diminutions or extractions, (as we have called them) since they are also seen in conversions and in combinations. And such is the *connection* in these works of creation, that most of the greater agents in nature are employed by turns in a variety of these operations.—Where the aid of a natural *labor-*

10. We now come to the last of those of our articles which regard merely the "*collection of water;*" and it will be very brief, for it relates solely to the more frequent use of *solid poles* to assist the descent of water from roofs. I have only to state here that as hollow pipes are subject to have water frozen within them, which is an inconvenience in the winter season, solid poles may in various instances, be more useful for the above purpose, than *hollow* tubes.

Here then shall be finished what I shall term my *interlocutory articles* on the subject of "the collection of *water*."

11. We now return to the case of *conflagrations* in *dwellings;* but they are dwellings of a *new description* of which we have now to speak; for under this head it will be found, that we may include *steam boats;* and especially those employed in *close waters*, but without forgetting altogether steam boats in *open seas*.

The Chinese, it is well known, have villages and towns composed entirely of large *covered boats;* that is, of *inhabited tenements*, which have not only the property of *floating*, but of being *transferred, from place to place*, on the surface of the water. *Travelling* steam boats, carrying passengers and *goods*, (such as move up and down many rivers in the United States), may be considered then, in effect, as *moving taverns*, with a huge *ware house* annexed; and the whole of this vast *combination* is subject to the most terrible disasters from fire; which fire, whether it arises from one part of the combination or another, generally ends in the *common ruin of the whole;* especially if the *water* which the steam boats are traversing, is able to lend its aid in rendering the disaster still more complicated.

It is the object of this article to treat of the *whole* of this *mass of evils* briefly, in order to find (as far as may be,) what are the means of *prevention* or of *cure* which may be adopted respecting it.

In *close waters*, it will be proper as a *first step*, to remove the *machinery of the steam engine* wholly out of the carrying vessel, and place it in a small separate vessel of its own; which indeed was the

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atory is wanting for completing any thing in the organized part of creation, such a laboratory is always provided; and mechanical laws also are resorted to, as well as chemical laws, if requisite for accomplishing a given purpose.—In short, in all around us we see evidence both of unity and of variety, of knowledge and of efficiency; Human science however being often unable to decipher what is thus exhibited to its view, till the study of successive ages has been employed in unveiling the secret. So great is the Creator, so little is man!

first arrangement made for steam boats about a century ago; as we know by a pamphlet then published, and by the engraving annexed to it, where was seen a small steam boat, towing an English frigate into harbor.—A second step must follow this; which is that of removing *two other articles* out of the principal vessel, namely, whatever is subject to *explosion*, and whatever is peculiarly liable to produce *combustion;* or in other words, gunpowder, and quick lime. These articles must be placed in a *separate vessel;* to be drawn by a towing line.—Thus, instead of *one single vessel for the whole* of the steam boat concern in close waters, as at present; we shall have a little *flotilla;* of which the steam boat properly so called, will form the *van*, the principal vessel the *centre*, and the little vessel (with the articles of lading excluded from the principal vessel) the *rear*. This arrangement is simple, and yet it seems effective; especially if we add, that the *stewards room* and the *kitchen* may be placed *respectively*, at the opposite *sides* of the principal vessel, and be protected within by linings from sheets of iron, (such as were mentioned in my letter when speaking of Mr. David Hartley.) And be it here observed, that in case of fire in the principal vessel, the two smaller vessels may be useful in affording an asylum to those whose escape we wish to secure.

To these greater arrangements, *minor* ones are yet to be added. For example, both the principal vessel and the vessel in the rear, may have one of Mr. S. V. Merrick's small fire engines on board; each engine being provided with a suction pipe, to draw water directly out of the river; and the principal vessel may have holes in different parts of its sides to receive the pipes of these engines, in case of fire.—*Bamboo canes* (in abundance) may be provided at a most trifling expence to be applied, in the Chinese manner, under the arm pits of those passengers in the principal vessel, who may be obliged to throw themselves into the water without being able to swim; (for let nothing be thought too small for notice, where *life* may be saved).—As to *lightning rods*, I have heard of no accident from the want of one; and perhaps the construction of the boat and the element on which it swims, make a steam boat, by its very essence, a conductor of lightning; not to mention the security which has sometimes been derived from masts and rigging.

But a *separate question* now arises; namely, whether *two steam engines*, each on board a vessel of its own; will not be better than one steam engine, with one vessel alone to carry it? By means of two engines, each placed in its own vessel, sharp turns in the navigation may be more easily made; in case of an accident also to one engine, still the other engine is left to do such duty as the case admits; but what is more important than all is, that when *sunken timber* lies in the course of navigation, cords stretched out at different depths between the two steam boats, may detect these impediments to the navigation of the principal vessel, and give more or less timely notice of the danger. Indeed even a single steam boat may have projecting apparatus at its head, which may of itself furnish an important warning.—When danger is lessened, it will soon lessen insurance, and increase the applications for passages and for conveyance of goods in steam boats.

As to vessels having *steam engines* on board, and navigating *open seas*, it is plain, that what is done with respect to them, must be confined chiefly to their steam engines; and to the prevention of fire derived from the use of them.

It is doubtless true, that the theory and practice of steam engines are still so imperfect, that the government can only make partial, and those chiefly *negative* regulations on the subject of them; but such regulations it may make, and (we venture to say) it ought to make, as speedily as possible. If wisely framed, they are likely to be copied in several other countries, although not perhaps by legislative enactments.

I have already asked leave to make *two incidental remarks*, as the sequel of the present article of my Supplement: and they are suggested by reflecting on the *means of extending the use of steam boats.*

1. The Mississippi and Missouri, with their connected waters, may be made equivalent to rail roads by land, if the obstructions to their use be removed, by freeing them from sunken trees; and by preventing new additions to this evil.—I do not know whether jointed and moveable heavy wooden booms have been used for sweeping the bottoms of these streams, but I may enquire whether the cutting down and burning of trees liable to be swept down by floods into these streams, has been thought of; although the time may come when this expedient it will be worth noticing. Steam boats however are now said to be used in removing sunken trees. Here our first *incidental remark* may be left.

2. Our second *incidental remark* regards the Straights of Magellan. A practical north west passage from the Atlantic into the Pacific being now hopeless; the improvement of a south west passage is next to be contemplated, through the straights in question, by the means of steam boats. A very excellent map or chart has been furnished, in Hawksworth's collection of voyages, Vol. I.; formed at great leisure and with proper instruments by the British Circumnavigators, Com. Byron and Captains Wallis and Carteret, in which this passage is divided into lengths, taken from point to point; (in other words, into reaches). By the aid of steam boats and the document in question, it is to be hoped, that the strong tides, strong currents, and strong swells, belonging to this passage may be overcome; and also, that the gales and bursts of wind, to which the neighborhood of Highlands in close waters is subject, may be rendered harmless. Proper moments for stirring in these waters must be chosen, by the aid of experience; since the safety of the steam boats themselves or their apparatus, may otherwise be endangered. This matter seems worth the few words bestowed upon it, since, if we except the passages across the Isthmus of Suez and across the Isthmus of Darien or Panama, no other spot on the globe affords a more advantageous *short cut*, than the straights in question. The saving of time here to be made, and the benefit of avoiding the usual rudeness of a passage round Cape Horn, will not only benefit navigators coming from a distance who wish to pass to and fro between the Atlantic and Pacific; but also the inhabitants of the east and west sides of the continent of South America.*—But here we finish our eleventh article.

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* See Hawksworth's Collection of Voyages, 1773, vol. 1. p. xvi of the general Introduction; also at p. 84, where Commodore Byron is spoken of as having passed these straights; and at p. 189 to 198, where Capt. Wallis is noticed on this occasion; and at p. 309 to p. 416, where Capt. Carteret's account appears. The great chart or map to which I have referred, takes in the particulars furnished by all these circumnavigators, and Captain Wallis gives materials to make a little pamplet to accompany it.—Dr. Hawksworth (who was an editor of very inferior merit) is against attempting the passage of these straights; and says, that although it was the opinion of Com. Byron (who spent seven weeks and two days in passing through the straight) that it may be passed in three weeks at the proper season, yet the passage cost Capt. Wallis near four months. But the truth is, that Capt. Wallis had to wait for the repairs of two consorts, &c. in the straights; and Com. Byron said, that not only a single vessel, but a large squadron, might pass the straight in *less* than three weeks. (See as above p. 16 and p. 84 compared.)

12. Our last article regards a repetition of some of our hints: *first*, as to the importance of a wise plan for the construction of our buildings, so as to prevent the commencement of a conflagration in them, and then the extension of that mischief. In a country where the facility of obtaining lumber is great, we often see brick walls for buildings, the roofs of which, are covered with wood; and various other hazards of the kind are wantonly incurred in the U. States. When we add to this, the neglect of guarding staircases and passages from fire, in the French manner, so sagaciously pointed out by Dr. Franklin, we must perceive, that we have yet some important lessons to learn as to conflagrations in buildings.—Omitting to speak of frequent fires among the frail buildings of the Asiatics, and of the burning of Rome under Nero, and of Moscow, in our day, where design had the chief share in the catastrophe; we must admit, that the *greatest conflagration known in history* of a *casual* description, was that of London, the metropolis of our ancestors, in 1666. The ground floors of the houses then burned, were indeed in many instances covered with *rushes*;* but a considerable fire occurred in London in the last century, the London tavern being built on a part of the ruins; and numerous fires still occur in that city, although many useful regulations to prevent it are by law, constantly imposed on builders. But in England, they have not yet applied to use, Dr. Franklin's discovery above mentioned; the principles of which, are perhaps, not universally understood in France itself; and these principles, probably, are as little known in England, as in the U. States. But it is time that they should be known in both countries; and particularly in the U. States, where the increase of population will make the houses in large towns every day more and more to approach each other, so as to favor the spreading of fire in them.†

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* Dr. Jortin in his life of Erasmus, Vol. i, p. 77, has this passage. "Erasmus (in a letter to a friend,) ascribes the plague, from which England was hardly ever free, and the sweating sickness, partly to the incommodious form and bad exposition of the houses, to the filthiness of the streets, and to the sluttishness within doors. The floors, (says he,) are commonly of clay, strewed with rushes, under which lies unmolested, an ancient collection of grease, fragments, bones, &c."—Dr. Jortin in his second vol. (pp. 341, 342,) has given the original letter by Erasmus, which is still more pointed than the above summary.—Neither plague nor sweating sickness, has occurred in London, since 1666.

† Even "Rome itself, (according to the proverb,) was not built in a day." Martial, in his time saw great improvements made in it, even as to the streets; for which

But I leave the subject of conflagration by land, to say *secondly*, a parting word as to the dreadful destruction of life and property, consequent upon the use of badly managed Steam boats; in which species of calamity, we seem to surpass all nations in the world. Mr. Webster's motion on this subject, it is hoped, will produce some effect before the present session of Congress shall expire; and that something may be done to save the public from these disasters, but yet not all (I am sure,) that Mr. Webster would wish. As regards myself, I may be allowed here to take my leave of these topics.

By way of return, however, to those who have followed the wanderings of the writer of these pages, his piece will conclude with an interesting and authentic anecdote, respecting a conflagration, which was the *origin of the present Cathedral of St. Paul's in London*; which is certainly the most splendid building, not in a Gothic style, known in the christian world among the seceders from the church of Rome. The anecdote which I have to furnish, is extracted from Mr. Evelyn's dedication to Sir Christopher Wren, of his account of architects and architecture; and is as follows; being prefaced by a well known and not unmerited compliment. "If the whole art of building (in the Greek and Roman style,) were lost, (says Mr. Evelyn,) it might be recovered and found again in St. Paul's, the Historical Pillar,* and other monuments of your happy talent and extraordinary genius. I have named St. Paul's, and truly not without admiration, as oft I recal to mind * * * the sad and deplorable situation it was in, when (after it had been made a stable for horses

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consult his amusing epigram; (Epig. 60, book 7,) the concluding line of his last couplet being

Nunc Roma est: nuper *magna taberna* fuit.

Even Nero, however, is said to have issued proper orders for the construction of the new buildings, expected to appear in the restoration of Rome.

Evelyn, in his dedication of his translation of Freart's *parallel of ancient and modern architecture*, to Sir John Denham, (the poet and a knight of the Bath,) when Denham was superintendent and surveyor of buildings and works, to Charles II, relates, that the first orders for *paving the streets of London*, were issued by Sir John Denham, only *two years before the great fire of London in* 1666.

Boston is an American city, in which the most resolute and fundamental changes have lately been made, on principles introduced by its late spirited Mayor, Mr. Quincy, now President of Harvard College; but still the great rule of Dr Franklin, borrowed from what the Dr. observed in France, is probably to this moment, unknown even in Boston.

* The "Monument," so called, on Fish street Hill, London.

and a den for thieves,) you, with other gentlemen and myself, were by the late king Charles, named commissioners to survey the dilapidations; and to make report to his Majesty, in order to a speedy reformation. You will not, I am sure, forget the struggle we had with some, who were for patching it up any how, (so the steeple might stand,) instead of new building which it altogether needed;—when, to put an end to the contest, five days after, that dreadful conflagration happened [namely, the great fire of London,] out of whose *ashes this Phœnix is risen.* The circumstance is so remarkable, that I could not pass it over without notice."

N. B.—On revising the above Supplement, a seeming neglect will be observed. It is said, that steam boats have not been liable to suffer by lightning, which is correct; and is owing to the influence of the edges and points of the *iron tubes* of steam boats, and to the vapor thrown forth by them, &c. and perhaps to masts. But, when the *steam engine is withdrawn*, it will be found, that the principal vessel, and the little vessel in the wake of this principal vessel, will *then* be left unprotected as to lightning. Masts and rigging for these two vessels, were not hinted at, although they might have been; nor will they now be insisted upon; but if provided, it is plain in what manner these vessels *may* be guarded from lightning.

March, 1834.

Art. V.—*On the Parallelogram of Forces;* by Prof. Theodore Strong.

Whatever moves a body, or tends to move it, or alters its motion in any manner, is called force. The direction of the force, is that in which it tends to affect the motion of the body. Two forces are equal, when being applied to a material point in opposite directions, they destroy each other's effects. If any number of equal forces, each represented by unity, are applied at once to a material point in the same direction; then if x denotes their number, the point is said to be acted on in that direction by the force $x \times 1$; or simply, by the force x. When any number of forces acting in any directions, are simultaneously applied to a material point, if they do not balance each other, the point will evidently move, or tend to move, in a certain direction, (by their action,) in the same manner that it would do by the action of some single force in that direction; the single force is called the *resultant* of the applied forces, and its direction is that of the resultant; also each of the applied forces is said to be a *component* of the resultant. It is evident that if the resultant is applied to the point in the opposite direction, it will balance the components; and that it will produce the same effect on the point in any direction, as its components; therefore the resultant may be substituted for the components, and reciprocally, the components for the resultant in any calculation. If two forces are applied to the point in the same direction, their resultant evidently equals their sum, but if in opposite directions it equals their difference; if the directions of the forces form an angle, the resultant will manifestly be in the same plane with the components, and its direction will be intermediate between their directions; if the components are equal, the direction of the resultant will obviously bisect the angle formed by their directions.

We will now proceed to determine the direction and quantity of the resultant. Suppose then, that two forces x and y, whose directions form a right angle, are at once applied to a material point M; to determine the direction and quantity of their resultant.

Put $P=3.14159$ etc. $=$ the semi-circumference of a circle whose radius $=1$; let z denote the resultant, θ the angle which its direction makes with that of x, then $\frac{P}{2}-\theta=$ the angle which its direction makes with that of y.

If x and y are changed to nx and ny, it is evident z will become nz, and that θ will not be changed, or if $\frac{x}{z}$ is invariable, θ will be invariable; but if $\frac{x}{z}$ varies, θ must vary; reciprocally, if θ varies, $\frac{x}{z}$ must vary: hence the relation between $\frac{x}{z}$ and θ, may be expressed by $\frac{x}{z}=\varphi(\theta)$, (1), also by changing θ into $\frac{P}{2}-\theta$, and x into y, we have $\frac{y}{z}=\varphi\left(\frac{P}{2}-\theta\right)$, (2); where $\varphi(\theta)$ denotes a function of θ, whose form will manifestly remain the same, however θ may vary. Again, we may suppose z to be the resultant of two equal forces R and S; R acting in the direction of x, and S in the plane of x and y, its direction being on the same side of x with that of z, and making an angle with the direction of x, which equals 2θ; then z evidently bisects the angle 2θ, formed by the directions of R and S, it also equals the sum of their components which act in its direction; hence and by (1), we have $\frac{z}{R+S}=\varphi(\theta)$, $\therefore x=(R+S).\varphi(\theta)^2=z\varphi(\theta)=$ the resultant resolved in the direction of x; by resolving S in the direction of x, and adding R which acts in that direction, we have $R+S\varphi(2\theta)=$ the components resolved in the direction of x, which must equal the resultant resolved in the same direction; hence we shall have $(R+S).\varphi(\theta)^2=R+S\varphi(2\theta)$, or since $R=S$ we shall have $2\varphi(\theta)^2=1+\varphi(2\theta)$, (3); which must evidently be an identical equation.

It is manifest by (1), that if $\theta=0$, $\varphi(\theta)$ and $\varphi(2\theta)$ will each $=1$; hence supposing $\varphi(\theta)$ to be converted into a series, arranged according to the ascending powers of θ, its first term must $=1$, and the powers of θ must be positive, for should any of them be negative, $\varphi(\theta)$ would be infinite when $\theta=0$, instead of being $=1$, as we have proved it must be; $\therefore$ $\varphi(\theta)$ must be of the form, $\varphi(\theta)=1+A\theta^a+B\theta^b+C\theta^c+D\theta^d+$, &c. (4), and by changing θ into 2θ, we shall have the expression for $\varphi(2\theta)$; by substituting the values of $\varphi(\theta)$, $\varphi(2\theta)$ in (3), we have $2(1+A\theta^a+, \&c.)^2=2+2^a.A\theta^a+, \&c.$, or $4A\theta^a+2(A^2\theta^{2a}+2B\theta^b)+4(AB\theta^{(a+b)}+C\theta^c)+2(B^2\theta^{2b}+2D\theta^d+2AC\theta^{(a+c)})+, \&c.=2^a.A\theta^a+2^b.B\theta^b+2^c.C\theta^c+2^d.D\theta^d+, \&c.$ (5). Since (5) is to be identical, (so that θ may be indeterminate,) it is evident that the coefficients of θ^a, must be equal; $\therefore$ $4=2^a$, which gives $a=2$, but A remains undetermined; by substituting the value of a, and comparing the next higher powers of θ, we have $2(A^2\theta^4+2B\theta^b)=2^bB\theta^b$, which requires that $b=4$, $\therefore$ $A^2+2B=8B$, or $B=\frac{A^2}{2.3}$; in the same way we find $c=6$, $d=8$, &c. $C=\frac{A^3}{2.3^2.5.}$, $D=\frac{A^4}{2.3^2.4.5.7}$, and so on. It is evident that $\varphi(\theta)$ must generally be less than 1, $\therefore$ put $A=-\frac{k^2}{2}$, and we have $B=\frac{k^4}{2.3.4}$, $C=-\frac{k^6}{2.3.4.5.6}$, $D=\frac{k^8}{2.3.4.5.6.7.8}$, and so on, where the law of continuation is manifest; hence by substituting the values of a, b, &c., A, B, &c., in (4), we have $\varphi(\theta)=1-\frac{k^2\theta^2}{2}+\frac{k^4\theta^4}{2.3.4}-\frac{k^6\theta^6}{2.3.4.5.6}+, \&c.$ which is the well known expression for cos. $k\theta$; hence $\varphi(\theta)=\cos. k\theta$, which substituted in (1) gives, $x=z\cos. k\theta$. To determine k, let

$k\theta=\frac{P}{2}$, then cos. $k\theta=0$, $\therefore$ $=x0$; but when $x=0$, we evidently have $\theta=\frac{P}{2}$, $\therefore k\frac{P}{2}=\frac{P}{2}$, which gives $k=1$; hence $x=z$ cos. θ, (6), and by changing θ into $\frac{P}{2}-\theta$, (as in (2),) x into y, we have $y=z$ cos. $\left(\frac{P}{2}-\theta\right)$, or $y=z$ sin. θ, (7); by adding the squares of (6) and (7), we have (since $\cos.^2\theta+\sin.^2\theta=1$,) $x^2+y^2=z^2$, (8); hence it is evident that the resultant is represented in direction and quantity by the diagonal of the rectangle whose adjacents sides denote the components x and y.

Suppose now, that the directions of x and y include any angle a: let z denote their resultant, θ the angle which its direction makes with that of x; then by resolving y in the direction of x, and adding x, we have $x+y$ cos. $a=$ the components resolved in the direction of x, but z cos. θ = the resultant resolved in the same direction, $\therefore$ $x+y$ cos. $a=z$ cos. θ; and by resolving the components, and the resultant in a direction perpendicular to that of x, we have y sin. $a=z$ sin. θ; by adding the squares of these equations we have x^2+2xy cos. $a+y^2=z^2$: hence the resultant is represented in direction and quantity by the diagonal of the parallelogram whose adjacent sides denote the components x and y.

Again, let three forces x, y, z be applied to M, in such a manner that the direction of each is at right angles to the directions of the other two: let r denote their resultant, whose direction makes the angles a, b, c, with the directions of x, y, z severally; then we shall have $x=r$ cos. a, $y=r$ cos. b, $z=r$ cos. c; whose squares, when added, give (since $\cos.^2 a+\cos.^2 b+\cos.^2 c=1$,) $x^2+y^2+z^2=r^2$; hence the resultant is represented in direction and quantity by the diagonal of the rectangular parallelopiped, whose adjacent sides denote the components x, y, z.

Let us now suppose that any number of forces acting in any directions, are applied to M, to determine the quantity and direction of their resultant. Draw any three rectangular axes denoted by x, y, z, through M; and let r, r', &c. denote the forces, a, b, c, the angles which the direction of r makes with the directions of x, y, z respectively, and let a', b', c' denote the corresponding angles for r', and so on; let R denote the resultant, A, B, C severally, the angles which its direction makes with the directions of x, y, z. Then by resolving the components and the resultant in the directions of x, y, z respectively, we shall have $r \cos. a + r' \cos. a' +$, &c. $= R \cos. A'$ $r \cos. b + r' \cos. b' +$, &c. $= R \cos. B$, $r \cos. c + r' \cos. c' +$, &c. $=$ $R \cos. C$, (9) ; whose squares, when added, give $(r \cos. a + r' \cos. a,$ $+$, &c.$)^2 + (r \cos. b + r' \cos. b' +$, &c.$)^2 + (r \cos. c + r' \cos. c' +$, &c.$)^2 = R^2$, (10) ; hence R being found, the angles A, B, C are easily found by (9) ; and it may be remarked that the known rules for the algebraic signs of the cosines must be observed.

If M is free, and the conditions of equilibrium are required, then we must have $R = 0$, $\therefore$ the first members of (9) being each put $= 0$, will be the conditions required ; if the first members of (9) are each identically $= 0$, then $R = 0$, and M will not be affected by the forces. If M is not free, but is pressed by the forces against any line or surface, it will be necessary that R should be at right angles to the line or surface, so that it may be destroyed by the reaction.

We will now consider the subject after the manner of La Place, at pp. 4, 5, Vol. I. of the Mecanique Celeste.

Let the two forces x and y, whose directions form a right angle, be applied as before to M, also let z denote the resultant, θ the angle which its direction makes with that of x, then $\frac{P}{2} - \theta =$ the angle which its direction makes with that of y. Hence we shall have (1) and (2) in the same manner as before ; if $y = 0$, $\theta = 0$, $\therefore \varphi\left(\frac{P}{2} - \theta\right)$ becomes $\varphi\left(\frac{P}{2}\right) = 0$; if y is indefinitely small relative to z, θ will be indefinitely small, and may be denoted by $d\theta$; hence representing the value of y in this case by dy, we shall have by (2) $\frac{dy}{z} = \varphi\left(\frac{P}{2} - d\theta\right) =$ $\varphi\left(\frac{P}{2}\right) - k d\theta$, by neglecting quantities of the orders $d\theta^2$, $d\theta^3$, &c. as is evident by the method of indeterminate coefficients, or Taylor's theorem ; but we have proved that $\varphi\left(\frac{P}{2}\right) = 0$, $\therefore \frac{dy}{z} = -k d\theta$, (1') ; where k evidently $=$ a constant quantity. Suppose that x and y are each resolved into two forces x', x'', and y', y'' ; x', y' acting in the direction of z, and x'', y'' perpendicular to it ; then we evidently must have $x' + y' = z$, (2'), $x'' = y''$, (3').

Since x forms the angle θ with x', and $\frac{P}{2} - \theta$ with x'', and that y

makes the angles $\frac{P}{2}-\theta$, θ, with y' and y'', we have by (1) and (2) $\frac{x'}{x}$ $=\frac{x}{z}=\varphi(\theta)$, $\frac{x''}{x}=\frac{y}{z}=\varphi\left(\frac{P}{2}-\theta\right)$, $\frac{y'}{y}=\frac{y}{z}=\varphi\left(\frac{P}{2}-\theta\right)$, $\frac{y''}{y}=\frac{x}{z}=\varphi(\theta)$, $\therefore$ (3′) is satisfied, and (2′) becomes $\frac{x^2}{z}+\frac{y^2}{z}=z$ or $x^2+y^2=z^2$, (4′); which shows that the resultant is represented in quantity by the diagonal of the rectangle whose adjacent sides denote the components.

We will now find the direction of the resultant. Let x and y become $x+dx$, $y+dy$, and let z' be their resultant which makes the indefinitely small angle $d\vartheta$ with z. Let $x^{\cdot}$, $y^{\cdot}$ denote the values of $x+dx$; $y+dy$, resolved at right angles to z, and suppose that $x^{\cdot}$ is $\triangle y^{\cdot}$, then it is evident that z' is between z and x; by (1) and (2) we have $\frac{x^{\cdot}}{x+dx}=\frac{y}{z}=\varphi\left(\frac{P}{2}-\theta\right)$, $\frac{y^{\cdot}}{y+dy}=\frac{x}{z}=\varphi(\theta)$, $\therefore x^{\cdot}=\frac{y}{z}(x+dx)$, $y^{\cdot}=\frac{x}{z}(y+dy)$, and $x^{\cdot}-y^{\cdot}=\frac{ydx-xdy}{z}=$ the whole force which is perpendicular to z.

It is evident that $x^{\cdot}-y^{\cdot}=z'$ resolved at right angles to z, $\therefore$ by (2) $\frac{x^{\cdot}-y^{\cdot}}{z'}=\varphi\left(\frac{P}{2}-d\vartheta\right)$ or by (1′), and substituting the value of $x^{\cdot}-y^{\cdot}$, we have $\frac{xdy-ydx}{zz'}=kd\vartheta$; but by neglecting quantities of the orders dz^2, dz^3, &c. we may use z for z', $\therefore$ by substituting the value of z^2 from (4′), we shall have $\frac{xdy-ydx}{x^2+y^2}=\frac{d.\frac{y}{x}}{1+\frac{y^2}{x^2}}=kd\vartheta$, whose integral gives $\frac{y}{x}=\tan.(k\vartheta+c)$, or by (4′) $x=z\cos.(k\vartheta+c)$; where $c=$ the correction. When $\theta=0$, $x=z$, $\therefore\cos.c=1$, and $c=0$, hence $x=z\cos.k\theta$; put $k\vartheta=\frac{P}{2}$, then $\cos.k\vartheta=0$, $\therefore x=0$; but when $x=0$, we evidently have $\theta=\frac{P}{2}$, $\therefore k\frac{P}{2}=\frac{P}{2}$, which gives $k=1$; hence $x=z\cos.\theta$; which shows that the direction of the resultant is the same as that of the diagonal of the rectangle whose adjacent sides denote the components.

We will now find the direction of the resultant in another manner.

Suppose then, that x is changed to $x+x'$, but that y is the same as before; let z' be the resultant of these forces, θ' the angle which its direction makes with that of x; then it is manifest that θ' is $\angle\theta$, and that $\theta-\theta'=$ the angle formed by the directions of z' and z. Put $\theta-\theta'=v$, (a), then by resolving z' in the direction of z by (1), we shall have $z'\varphi(v)=$ the resultant resolved in the direction of z; but $x'\varphi(\theta)=x'$ resolved in the same direction, and by adding z, we have the components reduced to the same direction; hence $z'\varphi(v)=z+x'\varphi(\theta)$, (b).

Put $\frac{x}{z}=m$, $\frac{y}{z}=n$, $\frac{x+x'}{z'}=m'$, $\frac{y}{z'}=n'$, (c), then by $(4')$, $m^2+n^2=1$, $m'^2+n'^2=1$, (d); by (c), $x=mz$, $x+x'=m'z'$, $y=nz=n'z'$; hence we shall have $z'=\frac{nz}{n'}$, $x'=m'z'-x=\left(\frac{nm'-mn'}{n'}\right)z$, but by (1), $\frac{x}{z}=m=\varphi(\theta)$, hence $z+x'\varphi(\theta)=(n'(1-m^2)+nmm')\frac{z}{n'}=$ (since $1-m^2=n^2$,) $=(n'n+mm')\frac{nz}{n'}=(mm'+nn')z'$; hence by (b), we shall have $\varphi(v)=mm'+nn'$, (e). It is evident that θ and θ' are independent of each other, and that m, n are functions of θ without θ', and that m', n' are functions of θ' only; hence by putting $\frac{d\varphi(v)}{dv}=\varphi'(v)$, and taking the partial differentials of (e) relative to θ and θ', we shall have $\varphi'(v)\times\frac{dv}{d\theta}=m'\frac{dm}{d\theta}+n'\frac{dn}{d\theta}$, $\varphi'(v)\times\frac{dv}{d\theta'}=m\frac{dm'}{d\theta'}+n\frac{dn'}{d\theta'}$; but by (a), $\frac{dv}{d\theta}=1$, $\frac{dv}{d\theta'}=-1$, $\therefore\varphi'(v)=m'\frac{dm}{d\theta}+n'\frac{dn}{d\theta}=-\left(m\frac{dm'}{d\theta'}+n\frac{dn'}{d\theta'}\right)$, (f). Now by (d), $\frac{dn}{d\theta}=-\frac{m}{n}\times\frac{dm}{d\theta}$, $\frac{dn'}{d\theta'}=-\frac{m'}{n'}\times\frac{dm'}{d\theta'}$, by substituting these values in (f), we have by reduction, $\frac{dm}{d\theta}\div n=\frac{dm'}{d\theta'}\div n'$, (g). Since the first member of (g) is a function of θ only, and the second of θ', it is evident that each member $=$ a const.; $\therefore$ let $-k$ denote the constant, and since by (d) $n=\sqrt{1-m^2}$, we shall have by multiplying by $d\theta$, $\frac{-dm}{\sqrt{1-m^2}}=kd\theta$, whose integral gives $m=\frac{x}{z}=\cos.(k\theta+c)$, $c=$ the correction; hence (as before,) we shall have $x=z\cos.\theta$.

Art. VI.—*Notices on Thermo-Electricity and Electro-Magnetism, in a letter to the editor, from Prof.* John P. Emmet, dated, University of Virginia, May 8th, 1834.

Sir,—I have been induced to offer the following brief observations, and to request their publication, in order that I may have it in my power to make a timely correction of a statement made in my former communication "upon caloric, as a cause of voltaic currents." This opportunity will likewise enable me to announce a very interesting law of thermo-magnetism, which I altogether omitted to notice in that communication. I shall also, in conclusion, be able to offer to the medical portion of your readers, a notice respecting my form of the *coil-magnet*, which I think promises fair to become a substitute for the leyden jar and common electrical machine, in all such cases as require the sanative agency of the latter instrument.

The results of my communication, above referred to, and which may be found in Vol. xxv, No. 2, of this Journal, were obtained by means of a galvanometer, delicate it is true, but far from being perfect; and which did not indicate currents of low intensity. Shortly after the manuscript was forwarded, I constructed a multiplier of excessive delicacy, and which, in all its details, exactly resembles and may be understood from the instrument which I see described in the last number of your Journal. I was not a little struck by the coincidence, and pleased to see the notice.* The object which I had in view, was to give the maximum effect with the smallest current, and, as there is always a great loss of power when the coil of the multiplier is extensive, I limited the wire to a few turns over and under a couple of connected needles, rendered perfectly astatic. With a view also, of applying the current as advantageously as possible to the needles, the coil, instead of being wound upon the same spot, as usual, was spread out, laterally, so as to form a kind of box within which the lower needle traversed. By this arrangement, the tangential magnetic force of the voltaic current was applied close to the extremities of the needles in every portion of their revolution. The needles were suspended by raw silk, and the whole instrument included within the glass frame of a Coulomb's balance of torsion. The delicacy of this multiplier is so great that a declination of 90° may be obtained

* By Dr. Locke of Cincinnati; see Vol. xxvi, p. 103.

by taking two pieces of bismuth and antimony in the fingers and simply touching them ; 50°, 30° and 20° may also readily be obtained by the contact of bismuth with the other metals under like circumstances. Examining some of my former results with this instrument, I immediately discovered that I had assigned wrong places to antimony, arsenic and gold. The two former must be included with bismuth and other positive metals, while the gold falls in with the negative ones.

Although the elementary conditions of the metals appear to be sufficiently constant, some very singular results follow, when dissimilar portions of metallic matter come into contact. I have shown that oftentimes the voltaic currents are reversed, by reversing the direction of the caloric ; and subsequent research has furnished new and interesting particulars. With my present multiplier, I find that antimony and arsenic are capable of giving off *both currents* from contiguous portions of the surface, when heated and *touched by other metals.* These opposite electrical states seem to be connected with crystallization or irregularity of surface, I cannot say which, and they disappear, at temperatures varying for each metal brought into contact.

Thus, upon heating a bar of antimony and touching it, from time to time, as it cooled, by the other metals, I found the opposite currents to disappear, with silver at a temperature near 280° F.—with gold, near 90°—with lead at 82°—whereas, with bismuth, these opposing currents might be traced even at the mean temperature. It is a most singular fact that they may be drawn off by the galvanometer wires, from portions of the antimony not more than $\frac{1}{8}$ of an inch apart. They are constant, as to position, while they last.

The next law, as it seems to me, is one of still greater interest. In all the metallic combinations which I have yet examined, I have observed that "*the voltaic currents produced by the contact of dissimilar metals, are the reverse of those occasioned by the friction of the same metals.*" Some combinations, as lead and tin, will scarcely produce currents under any circumstances, and bismuth and antimony are most effectual, but the law may be observed even between particles of the same metal, as bismuth.

The two following tables are regarded as denoting with sufficient accuracy, some of the thermo-electric relations.

1st. *When particles of the same metal, unequally heated, are brought into contact.*

Increase of temperature developes a —
- pos. current in platinum, gold, silver, copper, nickel, (*Law. caloric and+ε moving together.*)—neg. metals.
- neg. current, in tin, lead, zinc, iron, mercury, arsenic, antimony, bismuth, (*Law. caloric and+ε moving in opposition*)—pos.

In other words, increase of heat may be supposed to make the first group of metals neg. and therefore to diminish the attraction for oxygen, chlorine, &c., while the metals of the other group become more pos. by this operation and thus admit readily, of oxidation, &c. It is not known whether these relations ever change at high temperatures; as often happens during the contact of *disimilar metals.*

2nd. Table,—*shewing the order indicated by* SIMPLE FRICTION *of dissimilar metals, at the ordinary, temperature.* Contact, it has been stated furnishes directly opposite results. The series commences with the metals that are regarded as being most negative, because they transmit a pos. current when rubbed against any of those that follow them.

(Negative) bismuth, nickel, gold, platinum, silver, copper, mercury, lead, tin, iron, zinc, arsenic, antimony, (positive.)

I avail myself of the opportunity which this notice offers of making known to your medical readers that, by simply increasing the coil and observing the arrangement which I have already described in the last number of this Journal, I have succeeded in giving shocks, with the magnet, so powerful that they can scarcely be taken *through the arms and shoulders* without great inconvenience. They exactly resemble, in roughness and celerity, those from a leyden jar and are superior to any from a galvanic battery. As this new instrument is always active, powerful and portable, I am induced to offer it strongly to the notice of the medical electrician, as a substitute for the common electrical machine.

ART. VII.—*Botanical Communications;* by H. B. CROOM, Esq.

I. *Account of a new species of Plant.*

Malva * *nuttalloides*, (or *Nuttallia* * *ambigua?*)

Plant herbaceous, prostrate, one to two feet long; *stem* hairy; petioles long (5—6 inches) hairy; *leaves* digitate, *lobes* from 3 to 5, long, margins and nerves bristled; *peduncles* long, (5—6 inches) hairy; *calyx* usually double, the interior 5 parted, divisions acuminate, margins and nerves furnished with acute bristles or *setæ; exterior calyx* 3 leaved; leaves lanceolate (sometimes nearly linear) terminated at the points and clothed on the margins with bristles; (sometimes the *exterior calyx is wanting*,) petals 5, obtuse, fringed or laciniate on the exterior margins, dark purple, large.

Some specimens of this plant which I sent to Mr. Nuttall were destitute of the outer calyx, from which circumstance he considered it a Nuttallia, and pronounced it to be new. It is probably the connecting link between the genus Malva, and the genus Nuttallia.

Grows in pine woods in Florida, and the southern parts of Georgia. Flowers in May.

II. *Localities of Plants.*

1. *Taxus* . . . . . . (Yew tree.)

A tree from 20 to 40 feet high, and from 6 to 12 inches in diameter. It grows plentifully, on calcareous knolls, at and near the little town of Aspalaga, on the Appalachicola river. Flowers in April. It was with no little surprise that I met with this tree in this climate. I am uncertain as to its species, but think that it is identical with the *Taxus baccata* of Europe.

2. *Kalmia cuneata*, Mich.

Found in bloom, in May or June 1832, on the margin of a sphagnous morass, near Newbern, N. C. by Dr. Loomis. Flowers white. Specimens were sent to Mr. Nuttall, who supposes that this is the first time the plant has been found in flower since it was discovered and described by Michaux.

3. *Magnolia grandiflora.*

I know not how it happened that Michaux (Arb. For.) should have assigned the *Neuse River* in North Carolina, as the northern limit of this tree. I have not met with it, nor heard of it on that river. The *Magnolia tripetala*, however, which he mentions as its usual concomitant, grows on that river, in the neighborhood of Newbern. The true northern limit of this splendid tree is probably where William Bartram (Travels, p. 470) has placed it, to wit, on the little stream at or near the boundary line between North and South Carolina. On an estate near Wilmington, N. C. there is a large tree which is said to have been planted there by the proprietor some fifty years ago. In a swamp near this place, as Dr. McKee informs me, there are a number of young plants, the offspring, as he supposes, of this tree.

In going south from Augusta to Florida, the traveller first meets with it on the Ogeechee River; again at the Oconee, and again on the Oakmulgee, at Hartford. In Middle Florida it is the predominant growth of the *hummocks*.

4. *Chamærops palmetto*. (Palmetto tree. Cabbage tree.)

Michaux appears to have carried the limit of this palm a little too far north. It grows on the coast of North Carolina some ten or twenty miles north of Cape Fear inlet, but I have never met with it in the neighborhood of Cape Lookout or Topsail Inlet.

5. *Sabal Adansoni.*

This palm grows as far north as Neuse River, where it attains a height of four or five feet.

6. *Dionæa muscipula*. (Venus's Fly-trap.)

Later botanists appear to have overlooked the fact mentioned by Bartram (Travels, p. 470,) that he found this plant in the Savannas *south* of the Cape Fear River. Mr. Nuttall, (Genera, 1. p. 278,) supposes it to be confined to the *north* side of that river. It is probable that it will be found to extend from the Albemarle Sound to the Pedee River, at which last place it was observed by Gen. C. C. Pinckney. See Elliott's Botany.

7. *Olea americana.*

Appears to have its northern limit near Wilmington, N. C. where it was observed by Bartram. I have not seen it on Neuse River.

8. *Schœnus effusus*. (Saw-grass.)

Grows as far north as Neuse River. Grows in Florida.

9. *Cerotiola ericoides*. (Sand hill Rosemary.)

In going south, I first met with this elegant shrub on the sand hills 5 miles south of Columbia, S. C. and again on the sand hills at Brier Creek, Georgia. Also on gravelly hills near Augusta, at a place called the *chalk hills*, where however there is no *chalk*, but an abundance of *porcelain clay*, the decomposed feldspar of disintegrated granite. This interesting locality was shown me by my friend, Dr. Wray of Augusta.

10. *Silene Baldwynii*, Nutt. ?

Grows at Aspalaga, Florida. Petals large, fimbriated, rose colored.

11. *Pedicularis canadensis*. (*P.* * *asplenifolia*, nobis.)

Grows in Florida. Found in bloom on the 11th of March.

12. *Pinckneya pubens.*

Grows plentifully in sphagnous swamps of Florida and Southern Georgia.

13. *Acacia farnesiana.*

Grows on the banks of the Mississippi, near New Orleans.

14. *Petalostemon corymbosum.*

Grows abundantly on the poorest soils of Florida and Southern Georgia.

15. *Oxycoccus macrocarpus.*

Grows abundantly in some of the swamps in the North Eastern part of North Carolina.

16. *Cupressus thyoides.* (White Cedar, Juniper.)

Grows in some of the swamps of Florida, and in those of Alabama between Mobile and Pascagoula.

III. *Remarks upon the genus* Sarracenia.

Ever since I met with the species of Sarracenia, of which I gave some account in the number of this Journal for October last, under the name of *S. pulchella*, I have felt a suspicion that it is the true original of Michaux's *S. psittacina*, which later botanists have united with *S. rubra*, Walt. from which this species is very distinct, and forming apparently an intermediate species between S. variolaris and S. rubra. Mr. Elliott, has remarked the discrepancy between Michaux's description of his S. psittacina, and Walter's S. rubra, particularly in the "recurved, fornicated appendix" of the former. Michaux's description of his S. psittacina applies very well to the species under consideration. As quoted by Pursh it is as follows: "*Foliis brevibus*, superne *coloratis*, venoso-reticulatis, ala ventrali, sursum subcuneatim latescente, tubo sensim in appendicem recurvatam rotundatim fornicatam mucronatam desinente." As I before remarked, the appendix of this species, resembles the head of a *parrot*, and it is the only species in which such a resemblance is striking. The *leaves* too are *shorter* than those of either of the other species, and therefore peculiarly deserving the application of the word "*brevibus*" while those of S. rubra, so far as my observation has extended, are as long, as and even longer than those of S. variolaris. The *white spots* on the leaves, which I have mentioned, may be what Michaux intended by the term *coloratis*, while their *purple veins* (which I omitted to mention,) are well expressed by "*venoso-reticulatis*." In my former account the description which I gave of the *longitudinal wing* is faulty. Instead of "lanceolate," the term "semi-lanceolate" would have better conveyed the idea I intended—broad above, narrowing to a point below—which is very well expressed in the words "sursum subcuneatim latescente."

I think it probable that the *Sarracenia Catesbæei* of Elliott is the same with the *S. heterophylla* of Eaton. I gathered near Newbern, N. C. in the last year, a species which agrees pretty well with the descriptions of both. It had that *diversity* in the leaves remarked by Eaton, agreed in its appendix with Elliott's plant, and is apparently intermediate between S. purpurea and S. flava.

If my views of these species are correct the divisions of the genus will then stand thus:

Flowers purple.

1. *S. pupurea.* The most northern species, extending to Canada, and according to Elliott, reaching south to the middle districts of Georgia. Between Mobile and Pascagoula, I saw a large purple flowered Sarracenia which probably belonged to this species, but which circumstances did not allow me to examine.

2. *S. rubra.* I have gathered this species only, in the middle parts of South Carolina, in swamps between Columbia and Augusta. Leaves from twelve to fifteen inches in length.

3. *S. psittacina*, Mich. (S. pulchella, nobis.) The leaves of this species, about three to four inches in length, coming out from a common center, recline backwards, touching the ground in half their length, and forming a circle somewhat resembling the nest of a bird. Their white spots, purple veins, and curious appendix, render them quite attractive.

Flowers yellow.

4. *S. variolaris*, Mich. (*S. minor*, Walt.)
Abundant in the wet pine woods of Florida.

5. *S. Catesbæi*, El. (*S. heterophylla*, Ea.?)
This is probably a rare species.

6. *S. flava.* I have sometimes gathered the leaves of this species full three feet in length.

IV. *Remarks upon the Botany of Middle Florida.*

Florida has been celebrated, both by botanical and historical writers, for the beauty and variety of its vegetable productions; and it appears in this respect, to deserve all the encomiums it has received. In the language of the poet it may be said :

> And there is many a summer flower,
> Which tasks not one laborious hour,
> Nor claims the culture of his hand,
> To bloom along the fairy land.

During the spring, the woods are thickly studded with various species of Viola, Phlox, Pinguicula, Lupinus, Sarracenia ; the Verbena aubletia, Chaptalia integrifolia, &c. During the summer, the Yuccas, Pancratiums, &c. appear and in fall the Rudbeckia, Silphium, Helianthus, Gerardia, Gentiana, Aster, Chrysopsis, &c.

There are perhaps few scenes in nature more beautiful than some of the *Hummocks* of Florida. Frequently, after passing through a sandy and sterile tract, covered only with pine, scrub oak and poverty grass, (Pinus palustris, Quercus Catesbæi and Aristida) you arrive, suddenly, on the borders of one of these Floridian *oases*. Here the scene changes as if by magic. The soil becomes one of great fertility, and a dense forest succeeds, comprising many elegant evergreens, and other trees and shrubs of great beauty. Among these stands pre-eminent the stately Magnolia grandiflora, accompanied by its relative the fragrant Magnolia auriculata. To these we may add the Olea americana, Illicium floridanum, Laurus carolinensis, Ilex opaca, Hopea tinctoria, Pinus heterophylla, Gordonia lasianthus, Magnolia glauca and the elegant Mylocarium. Here too are found the showy Redbud (Cercis canadensis) the lofty Tulip-tree (Liriodendron tulipifera) the shady beech (Fagus sylvatica) and the beautiful snow drop (Halesia diptera and H. tetraptera.)

Such a spot generally occupies the declivity of a hill, at the foot of which runs a stream meandering with sudden and innumerable turns, overhung at intervals, by the beautiful Azalea nudiflora, the Hydrangea quercifolia, the Stuartia virginica, and the almost unrivalled Kalmia latifolia :

> Fresco e chiaro rivo, che descende
> *   *   *   tra fiorite sponde,
> E dolce ad ascoltar mormori rende.

This is the very paradise of botanists ; a fit place for the romantic and contemplative "to muse o'er flood and fell," and to realize those words of the poet :

> How often we forget all time—when lone,
> Admiring Nature's universal throne ;
> Her woods, her wilds, her waters—the intense
> Reply of her's to our intelligence !

Mylocarium ligustrinum. (Buck-wheat tree.)

Pursh and Nuttall give the height of this plant from eight to twelve feet. In the rich *swamp* of the Tologie in Middle Florida, it grows with a slender trunk from twenty to forty feet high!

It is one of the most beautiful productions of the southern forests; its delicate and pure white flowers contrasting elegantly with the deep verdure of its glossy leaves.

In swamps between Mobile and Pascagoula it reaches the height of twenty five feet!

Azalea nudiflora. This elegant shrub, with all its varieties, is very abundant in Middle Florida. In the rich swamps it sometimes reaches the height of twenty feet!

Illicium floridanum. (Aniseed tree.)

Grows in swamps from six to fifteen feet high.

Halesia diptera and *H. tetraptera.* (Snow-drop tree.)

Both species are frequent. The former is very abundant on the banks of the Chattohochie.

Styrax glabrum. Occurs frequently, from six to twelve feet high.

Prunus caroliniana. (Evergreen cherry.)

Occurs frequently—abundant on the banks of the Chattohochie.

Prunus virginiana. (Common wild cherry.)

On some of the rich hummocks, grows into a large tree from two to three feet in diameter.

Magnolia. The following species grow in Middle Florida. M. grandiflora, M. auriculata, M. macrophylla; and M. glauca, with its two varieties.

Chamærops palmetto on the sea coast. *C. histrix* on rich hummocks. *C. serrulata* in wet pine woods. *Kalmia latifolia*, grows on the margins of some of the creeks. *Hydrangea quercifolia*, on the margins of streams, and the declivities of hills from five to twelve feet high. Remarkable for the exfoliation of its bark, and hence called "seven bark" and "nine bark."

Taxus (*baccata?*)(Yew tree.) Grows at Aspalaga on calcareous knolls.

Dirca palustris, *Ptelea trifoliata*, *Zanthoxylum tricarpum*, *Calycanthus floridus*, grows at Aspalaga. *Arundinaria gigantea*, from twenty to forty feet high, in dense masses, on the alluvion of the Appalachicola.

Quercus. (Oak.)

Q. virens, (live-oak,) grows frequently around the lakes and ponds, more abundant on the sea-coast.

Q. laurifolia. Very frequent on sandy soils of little fertility. Its leaves are perennial.

Q. Alba (*white oak*), *Q. obtusiloba* (Post-oak) *Q. falcata* (Black oak) are common.

Pinus Palustris, with *Quercus Catesbæi,* and *Q. nigra* cover vast tracts, some of which are very sterile, and others tolerably productive. In the former, the Pinus palustris is accompanied by the *Quercus Catesbæi,* and in the latter it is accompanied by *Quercus nigra.*

Pinus tæda var. *heterophylla,* El. grows in the hummocks to a large tree, called, in the country, "white pine," from its resemblance to the northern white pine.

Pinus variabilis grows on the Spanish old fields near Tallahassee.

Yucca aloifolia grows on the sea-shore. *Y. recurvifolia* and *Y. filamentosa* in the interior. Of the last species there is a variety possessing a longer and narrower leaf than the common variety.

Heuchera americana grows from five to eight feet high.

Hymenopappus scabiosæus grows plentifully around Tallahassee.

Datura Stramonium (Jamestown weed) and *Verbascum thapsus* (Mullein) are not found except in a few places where they have been lately introduced from the states. This confirms the opinion of botanists that these plants have been introduced on the American continent.

Lake Lafayette, near Tallahassee, March 1, 1834.

Art. VIII.—*Investigations respecting the Meteors of Nov. 13th, 1833.—Remarks upon Prof. Olmsted's theory respecting the cause;* by Alexander C. Twining, Civil Engineer and late Tutor in Yale College.

The writer of this article had the privilege of witnessing the meteoric display, on the morning of Nov. 13th, 1833, from a few minutes past 5, by the watch, till day. Since that time, he has had opportunity to collect well authenticated facts from observers in different places, and, by reflection upon the facts which he himself witnessed and those which he has learned from other observers, has arrived at conclusions satisfactory, in some measure, to his own mind, respecting both the *facts* and their *cause.* So far as these conclusions have coincided with those embodied in the extensive and valuable discussion of the same subject which has been given to the public from the pen of Prof. Olmsted, it will not be necessary to dwell upon them at large. Although such coincidences have existed—as that gentleman has, in a very friendly manner, remarked at the close of his memoir—and although they have related to the most material questions which are the subject of discussion, yet (as might be expected from the fact that the writer's ideas were formed and, in fact, communicated to Prof. Olmsted when he could not possibly know that similar ideas existed in Mr. Olmsted's mind) the grounds of argument are, in some respects, widely different and lead to additional, and sometimes to different results. The writer has himself taken up the pen with a threefold purpose,—to discuss and settle, if possible, the question, *what are the facts ascertained by observation*—to notice generally the *principal results* which may be derived from the ascertained facts and more particularly those results which have not yet been made known in any article which has issued from the press;—and finally to review the real strength of the hypothesis which assigns to the meteors a *celestial*, to the exclusion of a terrestial origin. Although this hypothesis is one among the coincidences to which Prof. Olmsted has alluded, it will be treated of here as a hypothesis which is due to that gentleman solely; for the several reasons, that it depends mainly for its evidence upon facts collected by him—that it has appeared already under his name,—and that the writer is willing to hold it only as a highly *probable* hypothesis—the best which our present knowledge admits of our entertaining.

First. *The observed facts.*

It is established upon the witness of Prof. Thompson and other authority specified in Mr. Olmsted's memoir, and upon a statement to be given at large in a succeeding paragraph, that the first meteors showed themselves as early as nine o'clock in the evening of Nov. 12th, in parts of the United States the most distant from each other—for example in the state of Mississippi, at Augusta in Georgia, at Charleston, S. C. and on the Hudson river above the city of New York. Probably they might have been seen, faintly and in small numbers, at the same early hour, in any part of the United States, if attention had chanced to be directed to the heavens. At fort Jesup, on the Red River, Lon. $93\frac{1}{2}^{\circ}$ at the hour of half past nine or ten, "not later than ten," the meteors were seen in considerable numbers. Other authorities at different places date the earliest hour of the meteoric appearances at ten o'clock; and at eleven they were sufficiently remarkable to attract notice very extensively. The following statements, which have not appeared before, throw light upon the commencement and progress of the phenomena as they developed themselves in the region of New York.

Capt. Isaac Faurot, known as an intelligent man who sails constantly between West Point and New York, was on the Hudson, the whole night, on the downward passage. His account is; that, being in Tappan bay at 8 or 9 P. M. and the wind being south west, he saw, as frequently as once in every minute, common shooting stars coursing against the wind to the S. W. by W. or W. S. W. They rose mostly in the N. E.; and those which started down in the S. E. were very short: about midnight the stars became too numerous to be counted; but maintained the same general course, shooting on every side to the west of S. W.,—some were very short and others long and brilliant, and many were seen coursing over head. Between twelve and one o'clock the trains were longest and most brilliant; but though less brilliant afterwards, the meteors continued progressively to become more numerous till about day break when they seemed like rain drops beginning to fall from a shower. They still kept the same direction as before but "dropped down," as it were, instead of coursing across the sky as they seemed to do at first. Capt. Seymour, of the steam boat De Witt Clinton, was upon the Hudson in one of the North river boats, on the downward passage, and witnessed the display entire. About half past twelve, when opposite Newburg, he observed an unusual number of shooting stars principally, he thought in the east; but not till half past two, in Haverstraw bay, did he notice any surprising splendor in the scene. Then he first observed the radiant, which was in the S. E. about 45° high. From that time to morning, the radiant did not seem to vary in azimuth or in altitude, but was lower, if any thing, at six o'clock, by which time the boat was at New York. During the night, he observed as many as fifty brilliant rocket-like meteors of large size and long trains; the most remarkable instance of which occurred just before dawn, in the case of a meteor which left a vivid train that remained straight four minutes, by the watch, then wavered in the middle and progressively towards the extremities, and finally coiled up into a cloud as bright, at first, as the train itself. The cloud continued in sight two minutes, and left a shade still visible. This meteor, being identified by the time of its appearance and by its distinct and remarkable changes with the one seen at New Haven which Prof. Olmsted has described, (Vol. xxv. p. 366) will be referred to again in connexion with other observations upon the same.

The accounts of the meteoric shower, which dated its commencement at 11 P. M. and which ascribed to it a progressive increase till an hour or two before day light, were numerous and highly credible; but, without referring to them more particularly we may infer safely, *that the meteoric shower commenced about* 9 *o'clock P. M. on the evening of Nov.* 12*th; that it became remarkable at* 11; *came to its maximum at* 4 *or half past* 4 *A. M., and was concealed from view by the day light at* 7. The time of the maximum was placed very uniformly at 4 A. M.; but the writer has observed in all persons who describe the *morning* phenomena a tendency to place events earlier than the true time of occurrence. For this reason it may be suspected that the maximum took place about half past 4 A. M.

This meteoric shower, to say the least, was vast in its extension. It has been traced already, in one direction, from the North American lakes to the middle of the gulf of Mexico; and, in the other, from long. 61° on the Atlantic to Central Mexico—long. 100°. There is nothing which makes it probable that these limits approach the extreme boundaries of the shower excepting on the south and, possibly, on the east. Through this whole region, the extreme south excepted, the general appearances were every where alike. The incessant and active motions which prevailed in the sky it cannot now be doubted were every where directed, whether the fact was observed or not, along the arcs of great circles having a common intersection. At a certain distance from the point of intersection, the paths were long and the angular motions rapid; nearer to that point the paths were shorter and the motions less rapid, till, in the immediate vicinity of that point, the paths were so short and the motions so gentle as only to excite the conception of a little elliptical cloud, or nebulous star, softly swelling out from the heavens, and subsiding. The point itself, as will be imagined from the preceding description, was a centre of divergence for apparent motions; from which centre the luminous lines left upon the sky, by meteors shooting in its vicinity, appeared to *radiate;* and in consequence it has been termed, not inaptly, *the radiant:* at or near the very point, it was not uncommon that spectators noticed, now and then, a bright spot or a star perfectly stationary for the brief period of its continuance. Mr. J. N. Palmer, who viewed this radiant with great attention at different times from two o'clock till morning, is positive that he distinguished, at an early hour, a circular or elliptical space surrounding it of perhaps, at first, three times the moon's diameter and towards morning, three times larger still;

which circular space was more luminous than the heavens generally and was as discernible and well defined as a halo round the moon;—within this space, and towards its edges especially, there was perceptible, upon close scrutiny, "an uneasy sensation," caused by multitudes of cloudy spots or circles succeeding one another and, towards the boundaries of the space, drawing out and departing, like a viscid substance, with reluctant motion. This effect which Mr. Palmer describes is readily explicable on the common principles which regulated the other phenomena, except the multitude of the spots or circles, which may have been the exaggerated effect upon vision of small meteors, really numerous, but which in the body of the heavens at large, could not be seen, being too minute to affect the retina with a distinct figure unless when their line of motion was nearly coincident with the line of sight. Mr. Palmer is sure that the radiant did not change its altitude or azimuth all night; but he made no observations with an express view to the determination of that question. When Mr. Palmer attempted to divide the meridian passing through the radiant into spaces, and to estimate the comparative lengths &c. of the meteors' flights within those spaces (see vol. xxv. p. 383.) he observed a general uniformity, as to length of flight and angular velocities, among those meteors which showed themselves in a given space or zone, whether it were the zone most distant from the radiant or nearest to it. Actual measurement made towards morning in the quarter from N. E. to N. W., in the case of ten or twelve of the most distinct and longest in the distant zones gave very uniformly 40° of length and four seconds of time. It will appear hereafter, that these were probably the very extremes both of angular distance and of duration.

If the writer's recollection is not amiss there was, on the morning of the 13th, between 5 and 6 o'clock, a twilight issuing from the whole south eastern and southern quarter; while the western and northern quarter was dark as usual. The meteors, every where in action, presented every gradation of apparent magnitude from mere points to the disc of Venus and Jupiter; and several of them, after the dawn was so far advanced that the meteors were shorn of their dazzling brilliance, were seen to have a disc of such magnitude that its perfect circular figure was evident to sense. In addition to these, which made up the multitude of the moving fires (whose number was certainly within the limits of estimate, though, on the whole, much greater, it is probable, than the number assigned by any one who has attempted an estimate) we have accounts from every district of the land which speak of individual meteors that were of great size and that left behind them brilliant trains which continued brilliant for several minutes and assumed tortuous and other remarkable shapes, such as might identify them to observers over a wide region. Four of these, so widely separate in distance, time or direction that they could not be confounded one with another, are known to have formed their train into a cloud and sometimes to have floated away eastward as if borne by winds.

The remark should be particularly attended to, that the appearances, above described, were *every where presented by the meteoric shower*, as far as our evidence reaches. The language of many newspaper articles would lead to error by their having described the meteors as proceeding in all directions and towards every quarter, without noticing the fact that those which started out in any given part of the heavens uniformly proceeded in a fixed direction; and the fact that all the directions obeyed a *regimen* among themselves in relation to a single point. Some, who did not notice this point, or radiant, have been misled by conceiving wrongly of its nature,—supposing the word radiant to intend a spot whence the motions *originated* and proceeded, and not merely a point through which the lines of motion produced backward would have passed. It has thus come to pass that the existence of this radiant has been discredited, in particular places, by individuals who had observed that the motions originated and proceeded from every assignable spot in the hemisphere, —a fact certainly true, and certainly consistent with the fact first spoken of. Again, the failure of numerous observers in particular districts to notice the radiant has led, in some cases, to the suspicion of its non-appearance at these places: Prof. Olmsted himself appears to have wavered on this subject, in the early part of his memoir, out of regard to the strong testimony of intelligent observers —that, particularly, of Doct. A. Smith, of North Carolina, which will be found at p. 379 in that memoir. But it is evident in the first place, from the language used, that Doct. Smith, was not speaking of the radiant which has been described as the invariable attendant of the meteoric shower, but of a supposed *origin* of the motions; and next it is evident that the one body whose motion he has particularly described was in fact moving from the very region in which the radiant must have been, to have conformed to its location in numerous other places. It is also a fact that, in places where the radiant did certainly show itself so evidently, that it would have seemed impossible, for it to pass unobserved, it failed to attract the notice of persons even of eminent attainments, who with the best opportunities, were engaged, at the time, in scientific observations upon the phenomena. The true reason why this remarkable feature of the meteoric shower passed unobserved by so many is to be found in its high location—in the morning at least—lying above the range of natural and easy vision. To the numerous testimonies in relation to the universal presence of this point of radiation which have been already brought together in previous numbers of this Journal, from the most distant parts of the United States there need be added, in this place only one, that of the Rev. Mr. Wisner, Sec. of the board of Foreign missions who, travelling in North Carolina, between Wilmington and Fayetteville, saw the meteors in action all the night; and, near the morning, on leaving the stage saw the radiant directly overhead, as he judged it to be. We may call his lat. 35° N. the Lon. 79° 30′ W.

One phenomenon which was observed, an hour or two before the morning light, is too surprising and anomalous in its character to be admitted, did it not rest upon unquestionable evidence: The phenomenon alluded to is *the observed fixedness of the point of radiation in a particular part of the Constellation Leo*,—both the radiant and the constellation moving round by the earth's rotation and crossing the meridian, as if the two had risen and would set together. The reader is already apprised that a fact of this nature and purport was observed at New Haven, from a quarter before six A. M, to a quarter before seven, by Prof. Olmsted, who watched the point with this express determination in view,—that at Worthington, Ohio, it was observed for nearly two hours before the morning light, by Mr. J. L. Riddel, with a map of the constellation Leo before him, and (not to speak of other observations to the same effect continued during a shorter period) that it was observed at Emmettsburg, Md. for two hours by Prof. Aikin. To these testimonies collected by Prof. Olmsted, may be added that of Mr. D. A. Strong of Buffalo; with whom the writer of this article was led to communicate by observing the remarks made under his name in the American Mechanics Magazine for January. Mr. Strong states that, at a quarter before six, he fixed the range of the point by means of an object in view; but, returning to his range twenty minutes later, he found that the point had moved away to the west and was still near to a star of the second magnitude (probably η Leonis) where it had been at the time his range was taken. A very valuable testimony to the same effect has been derived from letters written by Mr. Frederick Merrick, a gentleman who was at the time pursuing his studies as a member of the Wesleyan University at Middletown. Mr. Merrick in company with many others, one of whom was Lieut. W. W. Mather, of the National Mil. Acad. observed this point with precision, from a quarter before 5, A. M. to a quarter before 6, and assigned its position by the stars, in a spot hereafter to be mentioned; he observed indeed a small motion from S. to N. by E. but none from W. to E. Neglecting anonymous testimonies, from different places, which might be referred to, if the authors and the evidence were known, we shall finally mention a circumstance related by Mr. Wm. Lester, a surveyor of established reputation in Connecticut, who watched the meteoric shower for a considerable time before sunrise, from a spot near Norwich, Con. A little after sunrise, Mr. Lester moved into the shadow of a house which was established by the cardinal points, and directed his eye to the zenith, by the S. W. corner as a guide. Standing thus, he saw over head, in or near the zenith, several meteors which had a course to N. E. and E ; and the point from which they proceeded was, to appearance, about 8° west of the meridian. Mr. L. was now certain of what he had before judged to be the fact, that the radiant lay W. of South.

This concurrence of testimony from the most credible observers leaves no room to question the fact of a fixed position of the radiant among the stars, during the two hours and a half* to which the testimony applies; but it is a disappointment that we have not been able, after unwearied efforts, to meet with one individual in this section of the country who can give a consistent and certain account of the location of that point at an hour earlier than a quarter before five, A. M. It must be the fact that there are persons in the United States who can, even now, furnish valuable information upon this subject, if they should happen to become apprised of the high importance of the question; but, as the evidence now stands, we are reduced to the discussion of mere probabilities.

If the location of the point of radiation remained fixed in Leo, during the whole meteoric display it would follow that at two o'clock its altitude could be but little more than 30°. In so low a position of the radiant the meteors would have been seen, for the most part, in *ascending* directions ; yet only two statements, it is believed, one at

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* That is from a quarter before five at *Middletown, Conn.*, to nearly a quarter before six at *Worthington, Ohio.*

Buffalo, N. Y. and one at Concord, N. H. make mention of ascending meteors. To account for this silence, if indeed the radiant occupied at any time so low a position (where of course, by reason of the early hour, it would be seen by only a few observers) let it be noted that the meteors, after traversing a certain space in the atmosphere, invariably became extinct: Those meteors therefore which should be looked for *below* the radiant and near the horizon would generally become extinct beyond the limits of horizontal vision; while those near the radiant, by reason of the near coincidence of their motion with the line of vision, would be very short, and, of course scarcely noticed. On the other hand those meteors *above* the radiant which might assume ascending directions and which did not, on account of their proximity to the radiant, appear so short, as to escape notice, for the greater part, from common observers, would start high in the heavens and pass towards or through the zenith of the spectator, or high up towards it on either side. These, from their situation relatively to the eye, would exhibit long and brilliant flights, and would attract the observer's principal attention and give the predominant impression— the *character* of the scene: but these long and brilliant meteors, passing indeed from a lower altitude to a higher, but still in that part of the heavens which lies over head and which the eye estimates as level with the ground would be invariably estimated and spoken of by a common observer as moving *level*. This consideration the writer had occasion to notice particularly; for, on putting the question to intelligent observers who witnessed the display early or late, "did any meteors shoot upwards?" the reply invariably was that they did *not;* but on varying the question to this, "did any shoot from the radiant towards the zenith?" the reply, wherever any recollection of the subject was entertained, was always, "that they *did*." It may be well to remark here that the meteors did, undoubtedly, shoot continually upon every side of the radiant. Mr. Palmer, who has been quoted already as authority upon some other parts of the subject, states that during certain hours of the night, the number which shot from the radiant towards the zenith was greater than the number in any other one direction. Little is therefore to be inferred, in contrariety to the idea of a fixed position of the point of radiation, from the general silence respecting upward motions.

It has been mentioned already that Capt. Seymour at half past two o'clock, in Tappan bay, saw the radiant in the S. E. at 45° of elevation. As the point of Leo, which the radiant occupied towards morning was at half past two o'clock 37° high, it might be naturally inferred that the radiant did, in fact preserve the same position, nearly, among the stars which it afterwards occupied,—as observers not on their guard are inclined to overestimate altitudes; but this inference is embarrassed by the fact that Capt. Seymour saw the radiant in the S. E. whereas Leo, at that time, lay almost due E; as well as by the fact that Capt. S. supposed the radiant not to have moved from the S. E. nor to have changed its altitude at six in the morning—an hour when it certainly was near the S. and 68° in alt. The explanation of this might be found perhaps to be that at 6 o'clock Capt. S. saw the point above the high houses of N. York, whereas before, in Tappan Bay, he had a large and unobstructed horizon. On the whole the probability that an observer having a clear horizon would estimate 68° to be 45° is so small as to make it credible that in this case, the radiant was in fact, *at some time of the night*, seen at a low or medium altitude.

The observations of Capt. Parker, (Vol. xxv. p. 399.) who being in the Gulf of Mexico, Lat. 26° N. Long. 85° 20′ W. saw the meteors at three o'clock A. M. confined to the N. E. and saw also the radiant 45° high in the due N. E. confirm the same conclusion. The point of Leo before alluded to was then, in fact, as seen from that spot, 43° 30′ high; but its azimuth, instead of E. 45° N. was only E. 5° N. making that point 28° distant in the heavens from the radiant. This aberration in azimuth would suggest a doubt whether these observations, made on the very edge of the shower, can assist our conjectures respecting the early positions of the radiant elsewhere, if there were not reason to believe on other grounds that the radiant occupied a more northerly position in southern latitudes than it did in latitudes farther from the equator; while, as we shall see hereafter, the variation in right ascension was inconsiderable.

Mr. Palmer, who viewed the radiant many times between half-past two o'clock and morning, is decided in his belief that it neither changed in altitude nor azimuth; and although no observations were made by him having this question expressly in view, and although it is certain that the radiant did move towards morning more than 30° in the heavens,—yet if the altitude of the radiant, when first seen, had been as low as the constellation Leo, it is inconceivable that so great a change of position should not have been perceived. It must also be considered that, if the radiant was the vanishing point of parallel motions, and if the meteors became luminous by their entrance into the atmosphere, no meteors could be seen while the radiant was below the horizon; and therefore the appearance of the meteors so early as 9 o'clock of the preceding evening must dissipate the idea of a constant divergence from a radiant which was at that time 15° below the horizon, and even in some of the latitudes 20°, and which did not rise till near eleven o'clock. This fact must lead to a suspicion that the radiant, at that time, lay far to the west of its position in the morning; and in that case the only tenable supposition would seem to be that from that time till four A. M. it moved with a progressively decreasing velocity eastward among the stars, till towards the morning it became stationary,—after which, according to the laws of accelerated or retarded action, it might begin to return upon its track.

The statement of Capt. Faurot, that in the earlier part of the night, the motions were more horizontal and towards morning more perpendicular, gives countenance to the idea of a change in the altitude of the radiant from low to high; but, on the whole, it must be admitted that the evidence of this point, previous to a quarter before 5, A. M., is too uncertain and contradictory to draw conclusions from; and that the time during which we have certain proof of a fixed position of the radiant is too limited to admit of our founding any certain reasonings respecting the cause of the meteors upon the assumption of such a fact, unless there can be found other grounds to sustain that supposition.

There is however a second fact, entirely independent of the one just now considered, which is equally remarkable, and rests upon incontestible evidence,—the fact that the radiant, as seen from places on the earth far distant one from another, *had a location manifestly independent of meridians and verticals, and not conformed to any geodesic lines.* The truth of this remark will be manifest, if we compare in declination and right ascension the different situations of the radiating point which are derived from authorities already mentioned, either in Prof. Olmsted's memoir, or in this article. Arranging our authorities after a tabular form we have :—

Observer.	Observer's place, N. Lat.	Dec. of radiant.	Prop. diff.	Observer's place, W. Long.	A. R. of radiant.	Hour of day; mean time, A. M.
Mr. D. A. Strong,	42° 52′	17°	——	78° 58′	153° ±	5*h* 45*m*
Mr. F. Merrick,	41 33	{ 15 { 23½	1.71	72 42	{ 148 { 148¼	4 45 5 45
Prof. Olmsted,	41 18	20	1.92	72 58	150	5 30 to 6*h* 30*m*
Mr. J. L. Riddel,	40 4	{ 21¾ { 21½	1.66	83 3	{ 149 { 151	5 — 5 20 to 6 45 ±
Prof. Aiken,	39 40	23¾	2.11	77 10	148⅙	4 45 to 6 45
Prof. Thomson,	34 30	33	1.91	89 —	146½±	5 —
Capt. Parker,	26 —	49	1.90	85 20	149½±	3 —

In addition to these specific locations given above we observe, that Mr. V. H. Barber at Frederick, Md. lat. 39° 24′, long. 77° 22′, describes the radiant as being at half past 5 o'clock, A. M. in the "neck of the Lion in the constellation Leo." A writer, S., in the Christian Register of Jan. 25th, a paper published at Boston, says that from the place where he stood it "appeared to be in the constellation Lion's heart, near Regulus," at a few minutes before 5, A. M. By different observers at Providence, R. I., West Point, N. Y., and in the western parts of Pennsylvania, places not widely differing in latitude, but distant in longitude 8° or 9°, the radiant was described as being within the bend of the sickle and near its centre,—that is, near Dec. 23° A. R. 148°.

The first observation in the table—that of Mr. Strong, was made at Buffalo, as already mentioned; it consists of an altitude taken with a quadrant of tin, constructed for the purpose, and an azimuth which was fixed and estimated by means of terrestrial objects in its range. There is reason to confide in its general accuracy. The true zenith distance, stated in his letter at 30°, Mr. Strong remarks may have been a few minutes less: the azimuth was estimated by judgment at S. 35° E; the time being a quarter before six o'clock.

Mr. Merrick placed the radiant twice among the stars, with the interval of an hour between the two locations. He did not mark them upon a globe at the time; yet a distinct recollection enables him to identify both—not supposing either, however, to be *exact*: Mr. M. remarks,—"The latter point (23½° N. dec.) I think you may depend upon as being very near correct; the former is rather more doubtful. I observed only the stars near which the point of radiation appeared to be and it is by referring to them that I have fixed the point, as above." The observation made by Mr. M. acquaints us with an anomalous fact (respecting which other authorities leave no doubt) that at Middletown there was a decided motion of the radiant in a direction a little E. of N. Mr. Olmsted's, Riddel's and Aikin's locations were made by means of stars, and were marked upon a globe or a map *at the time*. The observation following is derived from the data furnished by Prof. Thomson and printed at page 138 of this vol. From the nature of the data, viz. the general course of the meteors and the angle of their descent, it is uncertain whether Prof. Thomson, intended to be understood as speaking with precision;—yet so much as this is certain, that if the declination had not varied northward considerably, when compared with the same at New Haven, the course of the meteors, at 5 o'clock, would have lain far N. of W. and the radiant would have wheeled round rapidly towards the south, as morning approached. For Capt. Parker's observation see his statement (p. 399 vol. xxv) of which more will be said hereafter.

From the above mentioned sources of information, and upon such authorities the table above was constructed; each computation being made upon its own independent data, without the slightest effort at accommodation; and, where there is supposed to be an uncertainty amounting to several degrees in any result, that uncertainty is indicated by the double sign ± attached to that particular result. Although it is reasonable to suppose that the correspondence of the results derived from the less definite observations with those results which are best settled may be in part accidental, yet, by comparing the second and third columns of the table it will be strikingly evident, not only that there is a progressive increase of north declination in southern latitudes, but that the differences of declination compared with the differences of latitude are strikingly correspondent. To exhibit this correspondence (whatever be its cause) the third column was calculated, by dividing the interval between each successive declination and the declination at the head of the column by the corresponding difference between each successive latitude and the latitude at the head of the column. Considering that the table includes *every observation* that is known, and that is sufficiently definite to afford data for a specific location and that the observations were necessarily loose in their nature, being made by the eye alone,—the degree of proportionality which the fourth column exhibits must go far to satisfy the mind, that there was, in fact, a more northern declination in the more southern latitudes.

This change of declination is in the proper direction to be the effect of *parallax*, and might be made the basis for calculation respecting the distance of some imagined body or place from which the meteors took their departure, were it not that the supposition of a parallax in declination is totally annulled by the entire absence of a corresponding parallax in right ascension. This absence is so striking, as one may see by reference to the table, that the right ascension at New Haven and at Worthington—places more than 10° of longitude apart in the latitude of 40°, and more than five hundred miles asunder in absolute distance—was the same at the same moment of absolute time,—the same, for example, at 5 o'clock of Worthington time, and 5h. 40m. of New Haven time. Also by comparing Prof. Olmsted's observations with Prof. Thompson's—made at a distance from it of 16° of longitude, while there ought to be, on the supposition of parallax, a difference of A.R amounting to 25° at least, there is in fact a difference only so great as might be accounted for by indefiniteness of observation; and that difference, such as it is, in the direction opposite to parallax. Again, in consequence of the movement of the earth on its axis, the observers themselves, who noticed the fixed position of the radiant, changed place, during the period of their observations, 15°, 20° and, in one case (that of Prof. Aikin) 30° of longitude; and the absence of a large parallactic motion to the west is only to be accounted for either by the *absence* of parallax or by a supposed *easterly motion* of the meteoric *magazine*, so to call it, just keeping pace with the earth's revolution. The latter supposition is astronomically untenable except at a distance which would not account for more than one tenth of the parallax observed. We are compelled therefore by these two equally decisive circumstances—namely the absence of both *parallax* and *parallactic motion*, to apply elsewhere for a solution of the apparent progressive augmentation of north declination as the observer's place moves southward.

It is worthy of remark that if we investigate, by means of the data furnished in the table, the parallel in which the radiant was vertical when on the meridian, we shall find it to be that of lat. 34° N.; and in like manner we shall find that in lat. 3° N. the radiant would lie in the northern horizon; and that, in lat. 65° N., it would lie in the southern horizon: beyond these two parallels therefore, of 3° N. and 65° N., it would seem that the meteors could not be visible. Additional observations upon the radiant are highly desirable; and it is hoped, even now, that such persons as may have it in their power to collect and give them to the public will not fail to improve the opportunity,—particularly persons far to the south, or west.

The statements of Capt. Parker of the ship Junior, which have been alluded to already, deserve a special notice. Capt. P. it will be remembered being in the gulf of Mexico, lat. 26° N. lon. 85½° W., saw the meteors confined to a small space of action, and the radiant, at 3 o'clock, in the due N. E. 45° high; the space of action spread wider during an hour and a half, and the radiant rose 5° to 10° in the mean time, without however moving from the N. E. at all, and about 5 o'clock the meteors spread over the whole heavens. *The analogy between these appearances and those which Humboldt witnessed at Cumana in* 1799 *is striking*. In that year while Mr.

Ellicott, who was far to the north, observed the meteors in all parts of the sky possessing evidently, as we judge from his very brief account, the same general motions and directions as those which were seen in the United States during the last meteoric display, their flight at Cumana was confined to a particular part of the north eastern and south western sky; the meteors proceeding towards the south and not above 40° in their highest altitude. A third instance of a similar nature, observed in November 1832 by Capt. Briggs of the ship Morrison of New York, has come to the writer's knowledge within a few weeks, and will be detailed in a following paragraph.

In the case of an observer less accurate than Capt. Parker, it might be deemed an over nice enquiry, considering that the observations were made only by the judgment and recorded only in the recollection—why the point of radiation should rise during an hour and a half only 5° to 10°, and that without veering from the N. E.; while the rise of a fixed point would have been 15° and its motion toward the north as much,—but the high authority which we have, in this case, and the excellent opportunity for observation—the ship standing steadily S. E. so that the meteors were seen directly abeam, encourage us to remark that the question will be solved by supposing the radiant to have retained some part of the absolute motion from W. to E. (or a little S. of E.) which we have already pointed out as a necessary conclusion from the early appearance of the meteors; which motion must have been rapid at first and have progressively decreased, till at 3 or 4 o'clock the radiant possessed but little motion and became towards morning nearly or quite stationary. This supposition, if it be admitted, may throw light upon the enquiry why no observer noticed the radiant at an early hour very low towards the horizon; for the radiant, being doubtless above the horizon at 9 P. M., would rise from that time forward, though but slowly in consequence of its eastward motion,—yet it would not at first be noticed by reason of the atmospheric obstructions to vision and to meteoric motion, and by reason likewise of the small number of the meteors as well as their obliquity to the line of sight. It is probable also that the number of shooting bodies was not sufficient till about 2 o'clock to disclose evidently the radiant which, having been gradually rising for five hours, might occupy by that time a very considerable elevation, although itself moving east among the stars.

In drawing to a close this part of our subject which relates to the *observed facts*, respecting the meteoric shower of Nov. 13th, we now present, in a single view, the following deductions and conjectures, as the best which it is in our power to form.

1st. The shower commenced feebly about 9 P. M. Nov. 12th throughout the United States, and every where came to its greatest intensity at about the same hour,—probably half past 4 of the morning following.

2nd. The general phenomena, exhibited in the body of the shower, were every where alike; particularly that remarkable one of the existence of a point of apparent radiation.

3d. The radiant point was every where stationary, or nearly so, in the heavens, for two hours and a half at least, at some time towards morning; and was nearly stationary an hour farther back (the time when Capt. Parker saw it).

4th. The radiant point, in the beginning of the shower, was *probably* far west of its place on the following morning; and probably it arrived at its ultimate position by a progressively decreasing motion.

5th. The radiant's position was not affected by difference of longitude, nor by parallax.

6th. Its position *was* affected by a difference of latitude, at the rate of about 1° 54′ of north declination increased for each degree of terrestrial north latitude diminished. With the exception of particular accidental coincidences, these changes do not manifest a tendency to follow the magnetic dip.

SECOND. *The nature and cause of the meteors.*

Obs. 1*st.* A careful attention to the phenomena which were presented immediately to the eye in the meteoric shower of Nov. 13th might satisfy an observer of the truth of certain general ideas which have been already advanced by Professor Olmsted in his memoir; viz., that the meteors *derived their existence from a cause beyond the region of the earth's atmosphere; that they were impelled or projected with immense velocity*; *that they became luminous by entering into the atmosphere and were consumed or dissipated by their motion through it.*

In the memoir alluded to, it has been illustrated in the most ample manner that the radiant point which was the *directrix* of the apparent motions, and at which nothing, it will be recollected, could be seen by the eye even when assisted by the telescope, was an imaginary radiant—the vanishing point of sight for parallel motions:—But, whether that radiant be considered an imaginary point or a real origin of the meteors, no reason can be given why the meteors, if they were *self luminous*, should not have been seen to commence their flight at the very origin itself, or in its vicinity; or, at least (if the thought should occur that the origin may have been too distant to admit of such small bodies producing impressions on the retina at the instant of their departure from it) why they should not have been seen as minute points swelling gradually into magnitude and brightning gradually into brilliance;—whereas the fact was observed to be that none except the meteors of very short and gentle flights, started in the immediate vicinity of the radiant—and not one, directly at the radiant, excepting now and then a motionless meteor that disappeared at the point where it was first discovered: and, in place of the meteors swelling gradually on the sight, the fact was observed to be that each in its own place was suddenly lighted up, as if an opake body in the act of rapid motion, had instantly become intensely brilliant. Nor if the meteors were self-luminous, can any reason be given (either in case the point of apparent divergence was the real origin or an imaginary radiant) why the meteors which were largest and most luminous, among those which traversed any given part of the visible heavens, were not discerned in general the farthest back in their path and nearest to the radiant;—whereas the fact was observed to be, that the largest and most luminous meteors started into view at every distance from the radiant, promiscuously with the minutest points. In fact the most brilliant meteor which the writer saw on the morning of the 13th, blazed into view as far as 90° from the radiant, having an altitude of 20°, or perhaps a little more, and an azimuth of N. 15° W.; falling vertically down to the tops of the hills which were about 4° elevated, where its train tapered to a point. It was a fiery ball of a deep red color, and perhaps 6′ in diameter when divested of the glare which made its appearance full as large as 10′, and it travelled down the sky with majestic rapidity, carrying an impression of united force and splendor. Its track was marked by a train of uniform breadth, but little exceeding that of the ball, and of the same deep color, excepting that it was prismatic in its tints—certainly near the point where the meteor seemed to be expiring. The general aspect of the phenomenon was the same as if a column of glowing melted metal had been poured down from the spot whence the meteor issued.*

* From what is now known respecting the height and magnitude of one of these brilliant bodies it is probable that this very meteor which has been thus described,

The instantaneous and perfect development of the meteors indiscriminately, whether large or minute, in places farther removed from the common *directrix* in proportion as the paths were longer and the flights more rapid could not but carry to the eye and the understanding together the impression of an upper limiting surface in which these sudden developements and orderly arrangements had their location. In harmony with this impression the "dashes of light" with which the smaller meteors marked the firmament and the bright bands of fluid fire which lingered in the paths of the larger, and which in several instances we know to have collected into a cloud and floated away to the east, could do no less than intimate as they vanished that they owed existence to the substance, whether gaseous, fluid or solid which the meteor, to its own extinction, had expended on the neighboring air. Probably no one who actually witnessed the meteoric shower and has followed out these reflections will strongly question that the meteors were each a distinct congeries of undefined substance and owed their illumination and extinction to their own rapid motion in the atmospheric medium which they entered. It may be that the meteor by rapidly parting with its substance would become at last exceedingly minute, and leave a train pointed at the lower extremity: but the trains were so evanescent that, by the time the meteor's course was finished, the far extremity had narrowed to a point; and thus these trains were left upon the sky in the similitude of a filament of yellowish cloud tapering towards either extremity like two very acute triangles united at the base. It was an observation of Mr. Palmer's that the point which faded last was not at the middle of the train but near two thirds of its length towards the part where the meteor had vanished.

Obs. 2nd. Some of the trains left by the meteors were so peculiar in their aspects or remarkable in their changes, *that a meteor seen at different places might in some cases be known as one by means of its train and thus data be obtained for calculation of the meteors height.*

Four meteors at least—one in each quarter of the United States—and which must have been distinct one from another, are known to have given origin to a train which curled up into a luminous cloud and

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as it showed itself at West Point, must have been seen from Catskill and Poughkeepsie, and the whole tract north and west even to Schenectady and Utica. This remark is made in the hope of exciting individuals to attempt to rescue from oblivion the facts which may yet be ascertained respecting this meteor or any other.

remained in sight several minutes. One of these which was borne east as if by the wind, was seen at New Haven in the northwest by Prof. Olmsted and is described by him (vol. xxv p. 366;) another certainly, and perhaps more than one, also borne east by the wind, is described as seen in Ohio by Mr. Riddel (p. 377;) a third, in N. Carolina, was seen in the south by Dr. Smith (p. 379;) and a fourth was seen east of south in Louisiana, near Fort Jesup, by Mr. Peter Peterson, and is described by Dr. Leavenworth, in a letter to Prof. Olmsted.

From other letters which the gentleman last named has put into our hand we extract two descriptions which contain facts that ought not to be lost and the reading of which may induce persons who saw the like, or who saw other remarkable trains in the sky, to put down in writing what they did see and commit the facts to some gentleman of science who can make use of them for valuable purposes.

The following is extracted from a letter written by Mr. E. Wade, dated Union Ville, Geauga Co. Ohio, Dec. 24th, 1833. "Another peculiarity was the luminous train left behind the larger meteors for some seconds, and in one (and I presume the largest which could have been seen during the time of my observation) for a space of from five to ten minutes. I was particular in the observation of this. It started from near the zenith and shot off nearly north west to within perhaps fifteen degrees of the horizon, illumining the whole heavens "*above the brightness of the sun*" for a few seconds. The luminous train of this meteor (distinctly visible for five minutes) was probably not less than forty degrees in length. It began gradually to bend from the point towards the zenith eastwardly until, while it was distinctly to be seen, the upper half (for the angle was formed about the middle) formed a right angle with the lower half of the train, and in this position gradually vanished."

Mr. James Sperry, in a letter dated Henrietta, Monroe Co. N. Y. writes,—"The large ball which you describe as having shot off in a N. W. direction, resembled one which I saw at fifteen minutes past five, moving to the S. W. This left a streak of light apparently as broad as the moon, and extending at least 30° of the arch of the heavens, that was visible three minutes, shining at first with such splendor that small objects on the earth could be as easily distinguished as at the full of the moon: it was straight at first; but, after continuing about one minute, contracted and crooked in the middle—the bend

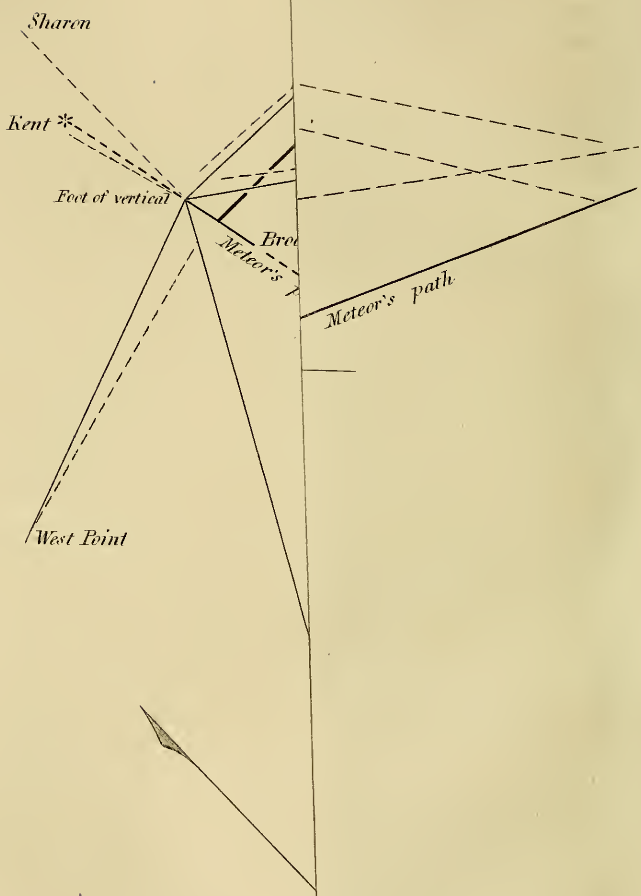

Fig. 1.

TEOR'S path:

forming nearly a right angle with the other part, and then gradually grew more dim until it disappeared."

Fortunately, the writer has been able to obtain data for calculating the height of the one meteor which Prof. Olmsted has described, as we have mentioned above. From Prof. Olmsted we learn that the meteor exploded north of *Capella*, at a quarter before six, mean time, near a spot which, being pointed out, proved to be in altitude 45°, azimuth W. 37° N. Its length of flight, being also pointed out between certain limits, proved to be 30°, at the very *least*. The train of this meteor remained straight and motionless for a time—then crooked near the middle—assumed a meandering line like a serpent, and drew itself up at last into a cloud, of about five times the diameter of the moon, which floated east, with a motion considerably more rapid than the common cumulous clouds, in a moderate wind, till after several minutes it disappeared nearly in the north.

Mr. Daniel Tomlinson of Brookfield, in Connecticut, being at that place, saw the light of a meteor and, immediately looking up to the zenith, caught its expiring flash and saw its bright and straight train, which was rather broadest in the middle and was pointed at the last or northern extremity :—it rested directly in the zenith, one extremity being about 5° S., and the other extremity as many degrees N. of the zenith :—its course was, by the *needle*, 5°, or a very little more, W. of N.; or, if we add the variation of needle, N. 12° W. In order to judge of the length of this meteor, Mr. Tomlinson, at a subsequent period, selected two stars at the same distance apart in the zenith, according to his own judgment and that of several other persons, and measured the arc instrumentally. This being done, his conclusion was, that the flight of the meteor was *at least* 20°. Mr. Tomlinson's observations were made in the most judicious manner; he examined the position of the meteor's train by facing first to the E., then to the W., and finally to the S.; and the result was invariable—that the *centre of the train* was directly in the zenith.

This train retained its rectilinear form but a very few seconds; in the course of one minute (others think as many as three or four minutes) it had assumed shapes like the figures two, three, four and five; and after curling up, like a tape dropped on the floor, the bounding lines of the train, at the end of three minutes, at the farthest, were completely effaced in a luminous cloud, five or ten degrees broad, which dilated itself and was borne away eastward by the wind. Mr. T. watched it, he thinks fifteen minutes, and saw it last at 45°,

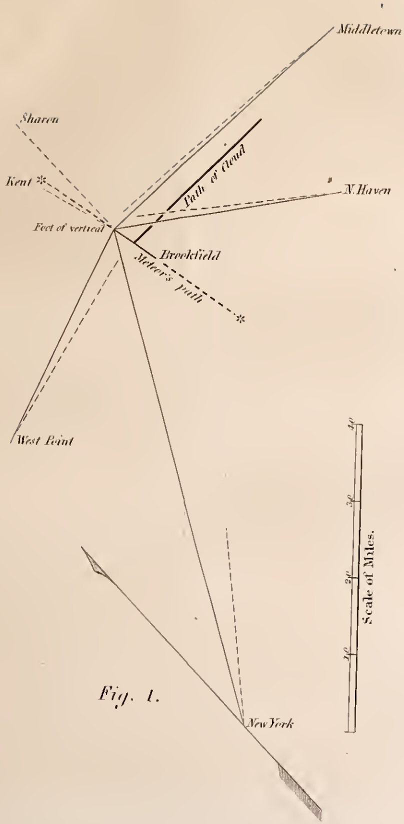

Fig. 1.

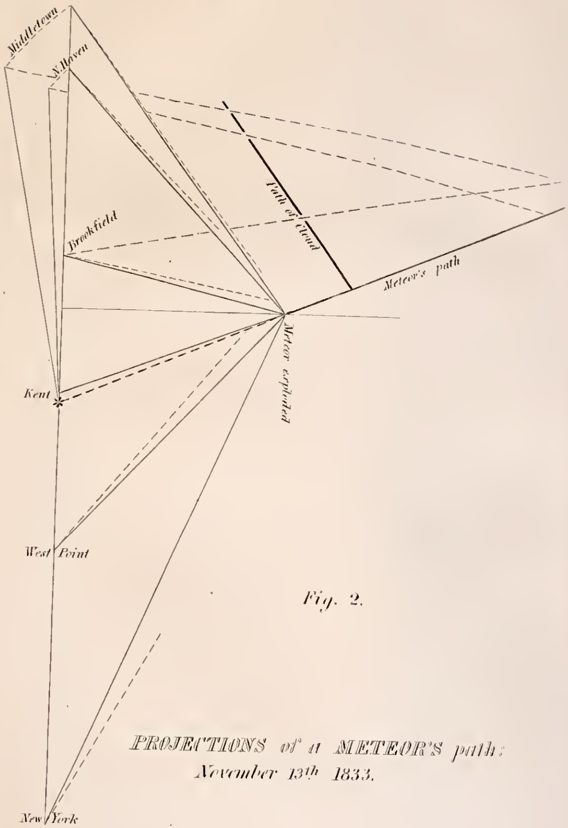

Fig. 2.

PROJECTIONS of a METEOR'S path:
November 13th 1833.

or 40° of altitude in the due east. After this he saw three other meteors, still more brilliant; but their trains vanished speedily.

Through Mr. Tomlinson, we have also been favored with the observations of an intelligent young man from Kent, Mr. Knibloe, who, attracted by the light of the meteor, looked up and saw it a little east of S., high up towards the zenith, and in the form of a cloud—having only so much train as to give a faint impression that its course was north-easterly. It will be seen, in the result, that Mr. Knibloe stood very near the spot to which the meteor's flight was directed, and at which the meteor would be seen without motion. Inasmuch as the radiant lay 22° from the zenith, and the body had a little apparent motion northward, we will call the elevation 70°. By a traveller on the road a little east of Sharon this meteor was seen, in the south east, moving west of south, with a long train.

Mr. Merrick, at Middletown, in company with Lieut. Mather and others, saw this meteor towards morning, shooting down in a path 30° long, at the utmost, that made an angle of 10° or 20° with the vertical and lay from a little S. of W. to a little N. of W. Its time of flight was estimated by recollection at between two and three seconds. It was noticed at the time that its apparent velocity was the same with that of the other smaller meteors that were moving around it. It left a train, through two thirds of its flight, which contracted in length and expanded in breadth—assumed a serpentine form—and finally collected into a cloud which remained visible about ten minutes:—during that time, it moved 8° or 10° in a line nearly towards the zenith—*a direction opposite to that in which the meteor had been moving*, and nearly in the direction of the wind. The circumstances in which this meteor was seen to disappear, fix the azimuth of the place within very narrow limits. It was W. 3° N. by the best estimation. Its altitude, when disappearing, was judged to be 35° to 40°; we may call it 37½°.

Capt. Seymour, as already mentioned, saw this meteor, just before the dawn. His eye caught it first in the due N. E., a little lower than the radiant, and it appeared as large as a twelve pound rocket. It moved in a descending direction perhaps 10° towards the N.—leaving a train of bright white light, inclined to the horizon about 26°. It burst at an elevation which, as Capt. S. pointed it out, was 29°; and threw out brilliant sparks on every side. With a watch in hand, Capt. S. observed the train to continue straight four minutes. It then began to waver in the middle and coiled up into a cloud, as bright as the train itself, which continued bright two minutes and left a shade still visible,—how long after the end of the six minutes the cloud continued visible is uncertain. He did not notice any motion in the cloud. It was the most remarkable meteor that he saw during the whole night. There is an uncertainty about the altitude and azimuth of the place where this meteor disappeared—they being supplied by mere recollection without reference to known objects: the best determination that can be made is alt. 29°, azimuth E. 53° N.

Lieut. Tho's. J. Cram, of the U. S. Mil. Acad. West Point, saw a meteor which, by the time of its occurrence and by the order and aspects of its transformations, is identified with the one which Capt. Seymour saw. The place of explosion lay, by judgment, E. 15° N., and it was observed, at the time, to be a little lower than the polar star. We call the altitude 40°. About ten minutes afterwards, Lieut. Cram saw, to the W. of N. the meteor which was seen, and which has been already described, by the author of this article. He thinks the last to have been decidedly the more brilliant of the two.

By these data, and with the aid of the best maps that exist*, we give, in fig. 1. of the accompanying plate, a graphical delineation of the exact situation of the different observers. The observed azimuths are represented in the figure by the *broken lines;* while the *full* lines are drawn from the places of observation to the foot of the vertical line in which the meteor is supposed to have exploded, and are supposed to represent the *true azimuths*, very nearly. The *broad line* represents the trace of a vertical plane in which the *cloud* is supposed to have moved; or in other words, it delineates the line upon the earth's surface directly underneath the path of the cloud. The *star*, in the vicinity of Kent, is meant to designate the place at which *the meteor would have struck the ground, had its course been continued*, and from which the meteor would have been seen merely as a bright spot or cloud, without train or motion. To determine that place, the meteor's line of motion was assumed in the known position of the radiant, which, at a quarter before six, mean time, in the region of the meteor's appearance, was 22° from the zenith, and S. 22° E. in azimuth. With this assumed line, the azimuth of the meteor, as seen at Brookfield, does not exactly agree, that being N. 12°

* Map by Geo. Gillet Esq., 1831.—In our plate the *scale* is corrected and made to correspond to 69.03 miles to a degree of the meridian.

W; but the uncertainty of a few minutes in the time, and the possibility that the meteor may have passed a little eastward of the zenith, dispose us to rely, in our diagram, upon the *direct* observation; especially, as the change happens to be such as would scarcely affect the result of our diagram. The *remarkable observation made at Kent confirms the justness both of this course and of all our conclusions.*

In determining the foot of the vertical in which the meteor exploded, or expired, the Brookfield and Middletown azimuths have been chiefly relied upon. The New Haven azimuth coincides almost exactly with the result; as it should be expected to do, having been fixed by means of a star. The other azimuths, at New York, West Point and Kent, being given by judgment merely, without reference to fixed objects, present, notwithstanding, an accordance more nice than could have been anticipated.

For determining the specific place or *height* of the meteor, at the time of its explosion, the planes passing through the above named vertical line and the different points of observation are revolved around (see fig .2 of the plate) into the vertical plane of the meteor's motion. In delineating the altitudes, the effect of curvature has been regarded. To determine the exact place in the vertical, the New Haven altitude has been principally relied upon, and next, that of West Point; although it happens that the Middletown observation is almost exact, and all the others are near to the truth. The result is, that *this meteor exploded, or expired, at the height of twenty nine miles and a half, above the surface of the earth.*

In attempting to ascertain the height of the *upper extremity* of this meteor's *visible* course, we revolve the New Haven and Middletown planes of observation around the meteor's line of flight, as an axis, till those planes coincide with the vertical plane in which the meteor's motion took place (or the Brookfield plane); and then throw back from the lines which join the observer's eye and the place of the meteor's disappearance, angles equal to the observed arcs of the meteor's flight. The result is made apparent to the eye in the dotted back lines seen, in fig. 2, proceeding upward from the three places just mentioned. The Brookfield observation, which threw the point of first appearance ten degrees at least south of the zenith, is considered to be an observation which may be relied upon as near the truth; and for the reasons following, it has been, with some confidence, adopted as one of the data :—

The *Middletown azimuth* appears to have been certainly north of the western point. In fig. 1, the foot of the vertical is placed south of that azimuth, and as little north of the due west (only 1⅓°) as could possibly be admitted. Again, the height in that vertical is settled, to a certainty, by the circumstances of Prof. Olmsted's observation at N. Haven, and Lieut. Cram's at West Point, to have been, to say the least, *not far above* the height fixed upon in the diagram. From these considerations combined, it seems to be certain, that the zenith distance at Brookfield, which Mr. Tomlinson estimated to be 10°, was in truth as much as 13°; and if the meteor's path was, in fact, exactly centered in the zenith it must, instead of "20° at least," as estimated by Mr. Tomlinson, have been 26°, as estimated by a second observer in the same place. But, as the eye cannot be considered competent to detect a variation of less than 3°, we prefer to suppose the southern zenith distance to have been but 10°—which was considered by Mr. Tomlinson, after his actual measurement, spoken of before, the least supposable angular distance.

The obliquity of the meteor's path is determined equally by the known zenith distance of the radiant (22°) and the Kent observation, which co-incides with it. The intersection of this path with the Brookfield back line takes place in a vertical, shown in position, in fig. 1, by the star south of Brookfield; and *at a height, above the surface, of* 80.40 *miles.* The same height, by the Middletown observed arc, would be 73.80 miles; and by the New Haven *minimum* arc, which is the one made use of in constructing the figure, it would be but 64.75 miles: the New Haven *maximum* observation however, had that been adopted, would have mounted beyond the Brookfield altitude, which we have relied upon, as correct.—In accordance with these principles the meteor's visible path was 55 miles; which must be near the truth. As there is no reason to suppose this meteor to have been luminous before its entrance into the atmosphere, we see, in these facts, a presumption *that the atmosphere itself must have its limit much higher than is generally supposed.* Our calculations may suggest to philosophers the propriety of attempting a determination of the atmospheric elevation, by means of the upper extremity of what are called "shooting stars", which certainly seem to bear a close affinity to the meteors of Nov. 13th, and which are so common that observers, at the distance of several miles, might by mutual arrangements for observation upon almost any clear evening obtain the data for accurate calculations. As to the *cloud*—its path, which is marked upon the plate, by a *broad line*, has been determined by the following considerations :—The cloud, being formed by a contraction of the train from each extremity towards the middle, must have occupied a place higher, by several degrees at least, than the lower extremity. Let us suppose this amount of elevation to have been, at Middletown, one quarter of the meteors path, or 7½°. As the cloud, by its motion, rose 8° or 10°, before it faded, its final altitude, at Middletown, must have been about 53°; while at Brookfield it was observed to be about 42½° high, nearly in the due east. These inferences, of course, are only presumed to afford an approximation to the truth; but they have been deduced from the known facts, and have therefore a general correctness. The result is, that the cloud was borne east a distance of 23½ miles, in a direction such that it would in fact appear to a spectator at Middletown *to trace back the course which the meteor had pursued*, in the same manner as Mr. Merrick described. At New Haven also, it would be seen *to float around nearly to the north*, in the manner described by Prof. Olmsted. The cloud must therefore, when it vanished, have held an elevation of about twenty one miles—having descended probably more than seventeen miles in addition to its horizontal motion. The distance of twenty three miles and a half—which appears to have been the effect of a current in the atmosphere—was passed in less than four minutes, if we reason from the angular velocity attributed to it by Prof. Olmsted (See p. 158); but, as the impression there described was probably that of the velocity when it was greatest, the true time was possibly longer. Thus Mr. Merrick estimated the time as ten minutes; and various considerations which cannot now be mentioned lead us to the opinion that the time was probably six or seven minutes, *and the velocity of the cloud three or four miles a minute.*

But we return to the meteor itself, with a view to the determination of its absolute velocity. The length of its course being already ascertained, within narrow limits, we have only to discuss the question of *time*. Unfortunately no observation of the number of seconds which the meteor occupied in its flight was made on the spot; but this omission may be supplied with a useful degree of certainty—although of course not with accuracy.

All the circumstances in the meteoric shower combine to show that the meteors, both large and small, which were seen by any one observer in any one quarter of the heavens, had the same angular velocity; and that, if we class the meteors with reference to the distance of their starting point from the radiant, the *very longest* meteors of every class, when compared together, although varying greatly among themselves in angular velocity and length, had the same general *time* of flight. Mr. J. N. Palmer—being requested to conceive and follow in his mind the motions of some of the most remarkable, among the short and gently moving meteors, in the immediate vicinity of the radiant, and to give signals corresponding to their appearance and extinction—made the intervals $1\frac{1}{2}$ to $1\frac{3}{4}$ seconds. In like manner, at a medium distance, the greatest time of flight was judged to be $1\frac{3}{4}$ seconds; and the flight, at a great distance from the radiant, of the very longest and most brilliant, $2\frac{3}{4}$ or 3 seconds. Mr. Palmer had however on the spot, *with a watch at his ear*, noted the time of several flights—he thinks ten or twelve—of about 40° in length; he purposely selecting the very longest. Their flight was measured very uniformly by ten beats of the watch; which, on examination, proved to be four seconds. These measurements of length and of time were made by Mr. Palmer with an express view to deduce the angular velocity. In addition to these observations we may state that a gentleman of great intelligence, and of astronomical skill, estimated, from recollection, the times of flight of the meteors to have been from half a second to something less than four seconds. A lady also described, from recollection, the flight of one particular meteor in the heavens, and also the time.—The former was 25°; the latter $1\frac{3}{4}$ seconds, by repeated trials. This was seen in a situation to present nearly its greatest angular velocity. Mr. D. Tomlinson thinks the very longest flight which he witnessed was $2\frac{3}{4}$ seconds. The writer himself would call the path of the longest meteors which he observed 30°—and 3 seconds the very extreme of time. The brilliant meteor which he has before described and which traversed a distance in space as great, it may be, in absolute dimensions as this one whose velocity we are seeking to ascertain, occupied a duration much less than 3 seconds. Mr. Merrick particularly observed that the angular velocity of this, was the same as of the other meteors (in its vicinity, as we understand him) and has estimated the time of flight at between 2 and 3 seconds. He also estimates the length at 30°, while Mr. Olmsted estimated it at 30° or more; and it may be deduced, from the relation of this meteor's flight to the radiant and the two observers, that it must have subtended to their view about its maximum angular extent. From all these considerations combined—particularly from comparing the length, 30°, of this meteor with those *in the same part of the heavens* which Mr. Palmer found to expire after four seconds, and which were in length 40°—from Mr Merrick's estimate of time, and his remark respecting its angular velocity—and, above all, from what the writer himself witnessed respecting the rapidity of other meteors' flights, one cannot venture to assign to this meteor a longer duration than *three seconds*, or certainly than *four*. Other means of verifying the result have been attempted,—one of which was the revolution, in a dark room, of a wheel of considerable diameter having a brilliant spark attached to its circumference—whose angular velocity was calculated, at the moment when the spark presented an apparent motion like that of the similar sparks which were recollected in the sky. Without stating particular results, we may say that it is only from a reliance upon Mr. Palmer's measurements that we have been willing to assign a time of flight quite so long as even three seconds. To the velocity deduced from these considerations something still must be added, for the effect of retardation in the atmosphere—an effect which we shall not now attempt to estimate. Our conclusions come to this; that *this particular meteor, and probably all the meteors, entered the atmosphere with a velocity not less, but perhaps greater, than fourteen miles in a second; that they became luminous many miles from the earth—in this case over eighty miles; and became extinct high above the surface—in this case nearly thirty miles.*

Obs. 3. We are now prepared to enter upon the question whether the meteors are to be ascribed to a *celestial* or a *terrestrial* cause; and to review, as was proposed in the beginning, the real strength of the hypothesis advanced by Prof. Olmsted. In making this review, however, the author proposes, as the more effectual method of trying the basis of the hypothesis, not to tread upon ground occupied by that gentleman, except in relation to one or two general and obvious ideas, which are common to the two arguments; but to pursue that line of investigation which originally led the author to those coincident conclusions which Prof. Olmsted has alluded to, in his memoir. The author begs leave here to repeat the remark that he is not able, as yet, to adopt even his own inferences respecting the cause, in any other way than as *conjectural* and *highly credible*—and even that, only in obedience to a limitation which will be pointed out in the end of the argument. This being understood, we put down, briefly and decidedly, under a few general heads our ideas respecting the present aspect of this subject.

First.—If the meteors were generated under the control of terrestrial influences—whether *atmospheric*, *electrical*, *magnetic*, or even unknown and merely *imaginable*—it would seem to be a necessary result that the bodies so generated should conform, in arrangement or motion, to geodesic lines. For example, had the meteors been formed in the upper regions of the atmosphere, and drawn to the earth by gravity, the line of descent ought to have exhibited a change, at different points of observation, corresponding with the change in the place of the zenith :—no such change, however, was observed. We are aware that Prof. Olmsted, in his *third* proposition (p. 147) has assigned the force of gravity, as "an adequate cause" of the meteoric motions ; but, besides the total independence of the vertical which those motions exhibited, we think it has been shown that the amount of motion was twice as great, at least, as that which terrestrial gravity is adequate to create. Again, had the meteors been generated, or set in motion, by the cause, whatever it be, of the north-lights, or the auroral arch, or of terrestrial magnetism, we should certainly look for a regular coincidence with the magnetic dip and variation :—but the reverse of such a coincidence do we find.* And in like manner the supposition of *any* terrestrial origin seems to be cut off and made untenable by the same circumstances, in the observed motions of the meteors, by which a similar supposition is made untenable in the case of gravity and magnetism. It must, however, be admitted that the change of declination, in a fixed ratio to the change of the spectator's latitude, which there is reason to believe existed, may chance to turn the point of this argument against us ; unless that change shall appear to have been a result of the position of the earth's axis relatively to the plane of the meteors' proper motion—in case such motion existed.

But when we consider that the meteors were actually seen by many individuals to manifest an independence of the earth's rotation, for two hours and a half—if we compute from the *earliest* observation in the east, and the *latest* in the west—and for still an hour more, if we compute from Capt. Parker's observation in the Gulf of Mex-

* It may be observed, in passing, that a scientific gentleman of our acquaintance observed, both during the meteoric shower, and through the day succeeding, a magnetic needle which was very delicately suspended by a film of silk, without detecting any change of variation or of dip, or any of those agitations which the needle is subject to, during the prevalence of auroral lights. This observation was made upon the banks of the Hudson river, fifty miles above New York.

ico,—and when we reflect, that this stationary condition in the heavens could not be an instantaneous effect of any cause, but must have been either pre-existent to the very earliest observations in our possession, or an ultimate state of motions gradually becoming quiescent—the conclusion seems inevitable, that our planet owed the brilliant decorations of its atmosphere on the morning of Nov. 13th to the presence of foreign and celestial visitants.

Second.—The flight of the meteors was not tangential, as that of secondary bodies would have been; but was directed to the centre,—in latitude 34° probably truly so: they could not therefore be bodies which, having a revolution around the earth as a primary, were diverted by some accidental force from their established orbit, and made to wander into the atmosphere. No analogy connects these bodies with those occasional solid meteors which traverse the upper regions of the air and explode, throwing down masses to the surface: explosive or fitful impulse is an idea not in place, when we reason upon the combined actions of a whole system of bodies, vastly extended, yet all moving with entire harmony and concert. When this harmony and wide extent of action and the uniform direction of the motions *to* the center and not *around* it are well considered, they seem to cut off every idea of a *secondary* dependence upon our planet, on the part of the meteors—as other considerations have already cut off all idea of a *direct* dependence.

Third.—It only remains then that the existence of the meteors was independent of every terrestrial cause or agency. But, as they were certainly within the planetary spaces and under the dominion of solar gravitation, the supposition implies that they were in rapid motion either *towards* the sun or *around* it. That they were not in rapid motion towards the sun is evident from the direction of their motion in the atmosphere, which was nearly—and probably in lat. 45° exactly—the same as if the earth in its annual motion had encountered these bodies in a state of absolute rest; for the radiant, there, must have lain near the ecliptic, and in about the same longitude as the point towards which the earth was moving. Their line of motion was therefore co-incident with the line of the earth's motion; so far at least as to imply, *that the meteors had an orbit of revolution around the sun.*

Fourth.—Had this orbit been *exterior* to the earth's orbit, the meteoric velocity must have been *in excess* of the earth's velocity; and the radiant could not have lain in the spaces *preceding* the earth, but must have lain in the spaces behind it,—unless the meteoric revolution was retrograde, or opposite to the earth's: in that case the relative velocity of the meteors—being compounded of the earth's motion, the meteors' proper motion and the motion generated by terrestrial gravitation—could not be less than about forty five miles a second; which is a velocity certainly greater than the meteors did exhibit. The orbit was therefore *interior* to the orbit of the earth. The determination of the meteors' velocity which has been attempted is not however sufficiently precise to guide us in the enquiry whether the revolution in this interior orbit must be supposed to have been *direct*, or to have been *retrograde*.

Fifth.—Hitherto we have reasoned from the know laws alone of the solar system; but the idea now forces itself upon every reader, that if these bodies had an *orbit*, they had also a *period;* and ought again to encounter the earth, at some future time, or even to have encountered it in times past, in the same part of its orbit—that is, at the same time of the year. When, therefore, the startling confirmation of our theory springs up before us, that both the meteors of 1799, seen by Humboldt at Cumana, and by Ellicott in the vicinity of the United States, and those of 1832, seen at Mocha and in Switzerland, and on the Atlantic, appeared at the same annual period with those of 1833—that is, the 12th and the 13th of November, we begin to feel as if further doubt is irrational:—But there is a condition which must be attended to, and which will be stated, after giving certain observations upon the meteors of 1832, that have not been published heretofore, nor publicly known.

In making enquiries on ship-board in the port of New York, the writer was so fortunate as to meet Capt. Briggs, of the ship Morrison, from China. After stating that, at Canton, the whole month of November in 1833 was cloudless, both day and night; and that, from particular circumstances, it is certain that a flight of meteors could not have passed unnoticed, if such had occurred at that place, Capt. Briggs remarked that in Nov. 1832, being then first mate of the ship President, he saw and minuted in the log book a remarkable flight of meteors, which he describes as follows.

By reference to the log book, the minute appears under date of *Nov. 12th. nautical time.*—Capt. Briggs, then first mate, held the watch on deck from 8 P. M. to 12 P. M., and again from 4 A. M. to 8 A. M. The ship was in lat. 43° N., lon. 40° W. During the whole watch, from 8 o'clock to 12, meteors larger than common shooting stars were seen moving from an altitude in the N. E., of about 20°, to one in the S. E. of from 45° to 60°; and with flights, from the quarter first mentioned towards that last mentioned, of from 20° to 40° in length. Though the directions were uniform, the meteors finshed their flight at very different distances from the starting point in the N. E. Many drew trains after them to the termination of their course, where they burst into sparks; and so close was the resemblance to rockets that the sound of the bursting could be *almost* heard. The *time of flight* was two or three seconds. The meteors (the larger are probably intended) were not more frequent than perhaps two in a minute; but several small ones might be seen shooting at the same time. They were visible only in the quarter of the heavens from the N. E. to the S. E. The wind all night was N. E., and the ship's course steady—standing for New York. Upon coming on deck, at 4 A. M., our informant found by enquiry that the display had lasted through the night. The meteors still continued visible in very small numbers—only a few in an hour—till day. Capt. Briggs has never, at any other time, seen a similar display.

This twice repeated co-incidence, in the month and day of the month, as it has been remarked already, would make further doubt respecting the celestial origin of the meteors irrational, but for a limitation not to be neglected;—namely, the necessity of ascertaining, before we adopt an hypothesis in full, that it will certainly explain *all* the phenomena. The whole question has, by this time, reduced itself to an astronomical problem, resting upon several conditions. —We shall state the conditions, without knowing whether they can be fulfilled or not. This want of knowledge is occasioned by a twofold cause, and is in part unavoidable and in part voluntary. It is in part unavoidable; for the writer has no time, at present—if he have the ability—to attempt so complicated a problem: it is also in part voluntary; for some of the most important conclusions of this paper rest upon the discussion of probable evidence; and in such a case it is better that the mind feel its way, without a theoretical guide to tempt it to stray from the path of rigid and impartial investigation. It is upon direct evidence that our conclusions have rested—whether in the case of facts which are unquestionable, or such as are only highly credible, and we put down the problem and its conditions as we conceive them, without having any concern, at present, about the possibility of a consistent solution. But if a consistent solution shall come to light, then indeed the writer will not hesitate to adopt in full a hypothesis, respecting the cause of the meteors, based on the general principles which have been already advanced. But to proceed to a statement of the *conditions of the problem.*

It is required so to arrange a system, or cloud, of meteoric bodies in an orbit around the sun that it shall, at stated periods, encounter the earth; and that the disturbing force of the latter shall draw *a part of them only* from their orbit; and give to them such motions as to account—

1*st. For the appearance of the meteors, in small numbers, early in the evening of Nov.* 12*th;*

2*d. For the stationary situation of the radiant, for two hours at least, on the morning of the* 13*th—and its observed position in the heavens;*

3*d. For the change of declination, relatively to a change of latitude—while the position in right ascension was unvaried;*

4*th. For a relative velocity of the meteors, of from about* 14 *to* 20 *miles a second;*

5*th. For an eastward motion of the whole meteoric shower, equal to the velocity of the earth's rotation, for a part of the time at least of its duration—as observed in different years and places, by Humboldt, Capt. Parker and Capt. Briggs;*

6*th. For a duration of the meteoric shower, less than a day, at the utmost.*

Final Obs.—When we glance back upon all the known circumstances of the meteoric shower, the extent and magnitude of the powers in exercise fill the mind with wonder. The one body whose flight we have been able to calculate, and which moved with such inconceivable rapidity, could not have been less—after allowing for the deceitful glare of surrounding and enveloping flame—than a hundred feet in diameter; and doubtless very many others were as large. The multitude of bodies was such as no man can venture with confidence to limit by numbers; and, had they held on their course unabated for three seconds longer, half a continent must, to all appearance, have been involved in unheard of calamity. But that Almighty Being who made the world, and knew its dangers, gave it also its armature—endowing the atmospheric medium around it with *protecting*, no less than with life-sustaining properties: and, considered as one of the rare and wonderful displays of the Creator's preserving care, as well as the terrible magnitude and power of his agencies, it is not meet that such occurrences as those of Nov. 13th, should leave no more solid and permanent effect, upon the human mind, than the impression of a splendid scene. To return, however, to our immediate subject and its connexion with the cause of human knowledge:—we may well regret that, with so much to be known apparently within our reach, so little that is definite should be, as yet, disclosed. Yet the success—partial though it be—which has rewarded the few investigations which have been perseveringly made, yields an inducement to more systematic effort; and we trust that individuals who have the time at command, will deem it, even now, a labor well bestowed, to rescue from oblivion any one well ascertained fact respecting these wonderful phenomena.

Art. IX.—*Communications by* Dr. Hare.

1. *Description of a process, and an apparatus, for Blasting Rocks, by means of Galvanic Ignition.*

Remark.—That portion of this article which precedes the first cut, was published in this Journal (Vol. xxi. p. 139.)—but, it is thought best to republish it now in connexion with the additional illustrations, rather than give the reader the trouble of looking into the separate volumes.—*Ed.*

I have observed various accounts in the newspapers of workmen killed or dreadfully lacerated, by the blasting of rocks.

This, and many like occurrences will, I presume, create sufficient interest in the following communication, to justify its appearance in the American Journal of Science.

I have ascertained that by a new application of galvanism, rocks may be riven with less danger than that which attends the firing of a pistol. I was induced to attempt this improvement in consequence of an application by a patentee (Mr. Moses Shaw,) for assistance in perfecting his patented mode of blasting rocks, by an electrical discharge from a Leyden jar.

In a letter dated June 1st, 1831, he says, "I have been engaged in blasting rocks by means of a fulminating powder, introduced into several cavities, and ignited in all of them simultaneously, by a spark from an electrical machine, by which means masses of a much larger size, and of a much more suitable shape, for any object in view, may be procured, than by the old plan. I have, however, to lament my inability to succeed in this method of blasting, during a great part of the year, when, in consequence of the unfavorable state of the weather, the ignition cannot be effected by electricity in any mode which I have devised, or which has been suggested by others, although I have consulted all the best informed professors to whom I have had access."

It occurred to me, as soon as this statement was made by Mr. Shaw, that the ignition of gunpowder, for the purposes he had in view, might be effected by a galvanic discharge from a deflagrator, or calorimotor, in a mode which I have long used in my eudiometrical experiments to ignite explosive gaseous mixtures. This process is free from the uncertainty, which is always more or less attendant upon the employment of mechanical electricity, for similar purposes.

The expectation thus arising, has since been fully verified. I have ignited as many as twelve charges of gunpowder at the distance of one hundred and thirty feet, from the galvanic machine employed. This distance is much greater than is necessary to the safety of the operator, as the deflagrator may be shielded so as not to be injured by the explosion, and by means of levers and pulleys, it may be made to act at any distance which may be preferred. There is no limit to the number of charges which may be thus ignited, excepting those assigned, by economy, to the size of the apparatus employed.

These remarks have reference to the principal and highly important object of Mr. Shaw's project, which is to ignite at once a great number of charges, in as many perforations so drilled in a rock, as to co-operate simultaneously in the same plane. By these means it is conceived that the stone may be separated into large prismatic, or tabular masses, instead of being reduced to irregular fragments of an inferior size. The object to which I propose now to call attention more particularly, is a modification of the common process of blasting by one charge, which renders that process perfectly safe.

This part of the subject I shall introduce by premising, that almost all the accidents which have taken place in blasting rocks, have occurred in one of the three following modes :—

1st. The explosion has taken place prematurely, before the operator has had time to retire.

2nd. A premature explosion has ensued from a spark produced by the collision arising from ramming into the perforation, containing the powder, the brickdust, or sand, or other matter, employed to close it.

3d. The fire not reaching the charge after the expiration of a period unusually long, and the operator returning to ascertain the cause of the supposed failure, an explosion ensues when he is so near as to suffer by it, as in the instance near Norristown, published some years ago.

The means of communicating ignition, to which I have resorted, are as follows:—

Three iron wires, of which one is of the smallest size used for wire gauze, the others of the size (No. 24,) used by bottlers, are firmly twisted together. This is best accomplished by attaching them to the centre of the mandril of a lathe, which is made to revolve while the other ends of the wires are held by a vice, so as to keep them in a proper state of tension. After being thus twisted, a small portion is untwisted, so as to get at and divide the larger wires by means of a pair of nippers. In this way the smaller wire is rendered the sole mean of metallic connexion between the larger ones. These are tied in a saw kerf, so made in a small piece of dogwood as to secure them from working, which, if permitted, would cause the smaller wire to break apart. At one end, the twist formed of the wires is soldered to the bottom of a tin tube of a size to fill the perforation in the rock to such a height as may be deemed proper. This tube being supplied with gunpowder, the orifice is closed with a cork, perforated so that the twisted wire may pass out through it without touching the tube at any point above that where the finer portion alone intervenes. To the outside of the tube, a copper wire, about No. 16, is soldered, long enough to extend to a stout copper wire proceeding from one of the poles of a galvanic deflagrator or calorimotor. The wire passing through the cork from the inside of the tube, is in like manner made to communicate with the other pole. The connexions between the wires and the poles, should be made by means of soft solder, previously to which we must imagine that the tube has been introduced into a perforation made for its reception in a rock to be blasted. The tin tube may be secured within the rock by the usual method of ramming in brickdust or sand, by means of a plug, having holes for the protection of the wires of communication already described.*

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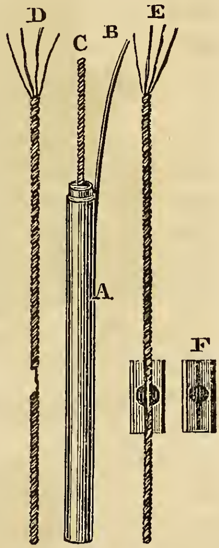
* It has occurred to me that plaster of Paris might be used advantageously, as it would require no ramming, and might set with sufficient firmness.

The apparatus being thus prepared, by a galvanic discharge, produced by the movement of a lever through the quarter part of a circle, the finer wire is ignited, in the place where it intervenes solely in the circuit, so as to set fire to the surrounding gunpowder.

As the enclosure of the gunpowder in the tube, must render it impossible that it should be affected by a spark elicited by ramming, as no means of ignition can have access to the charge besides the galvanic discharge; and as this can only occur by design, without an intention to commit murder or suicide, or by unpardonable neglect, it is inconceivable that an explosion can take place in this method of blasting, when any person is so situated as to suffer by it.

It must be obvious that in all cases of blasting under water, the plan of a tin tube, and ignition by a galvanic circuit, must be very eligible.

At A is represented a cylinder or tube of tinned iron, charged with gunpowder. At C, the twisted wires are represented as they protrude from the cylinder through a cork, by which the latter is closed at the upper end. The other ends of the wires are soldered to the metallic disk which forms the bottom of the cylinder. D represents the twisted wires as they appear when all the larger ones are cut, the smaller wire still uniting them. F represents the piece of dogwood, duly prepared; and E the wires as when supported by the wood. The reader has only to imagine the hole in the wood, to be supplied with the fulminating composition, and covered by a fillet of paper or cloth, glued or pasted around the wood, in order to complete his conception of the wires as finally accoutred and situated within the cylinder A.



Besides affording support to the larger wires, and thus protecting from fracture, the smaller wire, which unites them to the piece of dogwood that has been described, by means of the small hole represented in it, serves to hold, and to preserve in contact with the little wire, some fulminating powder. This not only facilitates the incipient ignition of the contents of the cylinder, but must make it extend more rapidly throughout the mass, and must, of course, cause it to be more powerful. Metallic arsenic, and chlorate of potash finely powdered and mingled, made an excellent explosive powder for this purpose; being more ready to explode from heat, and less so from other causes, than fulminating silver or mercury. Sulphur may be used in lieu of arsenic. Yet the use of these is not necessary, as the gunpowder will take fire directly from the wire, at least as effectually as in the usual mode.

Description of a Galvanic Machine, for producing ingnition in Rock Blasting.

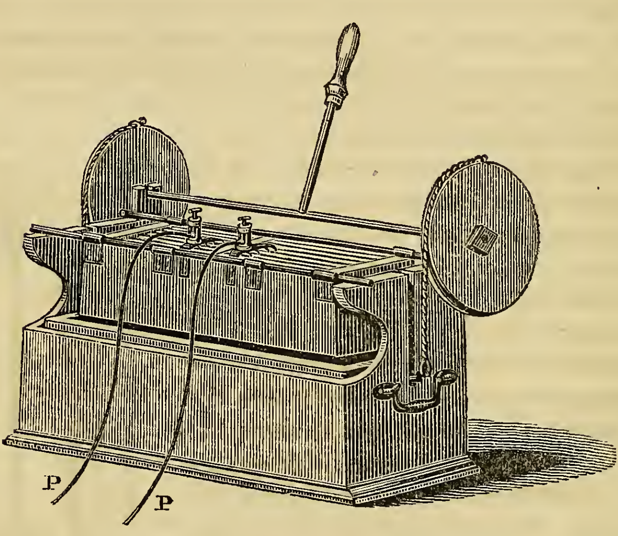
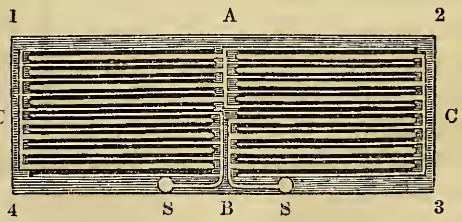


Diagram illustrating the arrangement of the plates.



This machine consists of sixteen plates of zinc, and twenty plates of copper, each twelve inches by seven, arranged in four galvanic pairs. The plates are supported within a box with a central partition of wood, A B, dividing it into two compartments. Each of these may be considered as separated into two subdivisions by four plates of copper between the letters C C. Of course the box may be considered as comprising four distinct spaces, No. 1, No. 2. No. 3, and No. 4. The circuit is established in the following manner. Between the zinc plates of compartment No. 1 and the copper plates of compartment No. 2, a metallic communication is produced, by soldering their neighboring corners to a common mass of solder with which a groove in the wooden partition between them is filled. With similar masses of solder, two grooves severally made in the upper edges of each end of the box are supplied. To one of them, the corners of all the copper plates of space No. 1, and the zinc of space No. 4, are soldered. To the other the zinc plates of space No. 2, and the copper plates of space No. 3, are soldered in like manner. Lastly the zinc plates of No. 3 are connected by solder in a groove, and the copper plates of No. 4 are in like manner connected by solder in another groove. Upon the ends, S S, of the solder just mentioned, the gallows screws are severally soldered, and to these the rods, P P, called poles, are fastened.

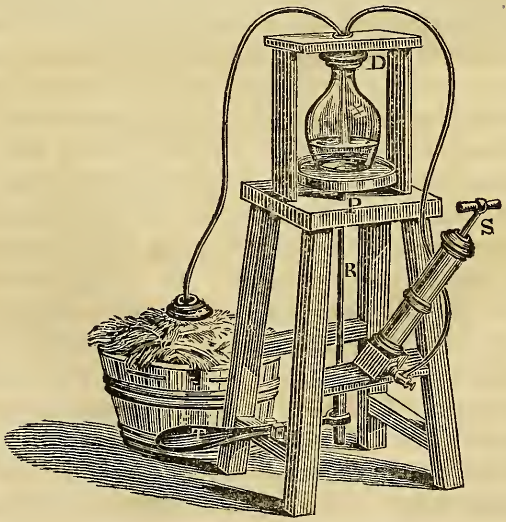
Rationale.

The zinc and copper surfaces of No. 1 and No. 2, communicating, have their naturally opposite electric powers exalted, and induce in the plates with which they are alternated, a like exaltation, still higher. By the communication of the latter with the surfaces in No. 3, and No. 4, a similar effect is induced, and again by induction the electric powers of the plate alternating with those last mentioned are augmented. Hence a discharge between the latter will have a quadruple intensity, and hence the poles, or rods, communicating with the gallows screw, soldered as above described to the zinc and copper plates last mentioned, will make a discharge through any conductor whenever the apparatus is put into operation by raising the acid, so as to enable it to surround and act upon the galvanic surfaces.

When, agreeably to the project of Mr. Shaw, several masses of gunpowder are to be simultaneously ignited, in as many different holes in the same rock, I purpose to introduce into each hole, a cylinder prepared as here represented and to secure them by ramming into the hole, sand, brickdust, or other suitable matter, through which the wires are allowed to pass so as to project on the outside. All the wires corresponding with that represented at B in the engraving, are then to be soldered to a rod proceeding from one pole to a calorimotor; and all those corresponding with C to another rod proceeding from the other pole. In case the calorimotor cannot be placed at a sufficient distance to secure it from injury, it may be shielded by a strong cover. Under this, the *operator* might be protected; but if it be not convenient to have the shield large enough for his protection, a cord may be resorted to, which being attached to the lever of the machine, and made to pass through one pulley or more, will enable him to cause the acid to act upon the plates, at any distance which may be desired.

It can scarcely be necessary to point out that the method of communicating ignition described here for the purpose of rock blasting, may be applied as the means of exploding a mine. As, for instance, the mines associated with the fortifications erecting near Newport, as a part of the means of annoyance, might have a communication through copper wires with a galvanic apparatus in those situations to which the besieged might be expected to retire, putting it thus completely in the power of the commanding officer, to select that time for the explosion when its effects would be most serviceable.

2. *Apparatus for transferring a liquid from a carboy, or cask, to bottles; especially useful in the case of sulphuric acid, the decanting of which is always more or less dangerous to the manipulator, especially as detached globules may reach the eyes.*



By means of a treadle T, the rod R, and platform P, the bottle is, by the foot, pressed against a brass disk D, coated with gum elastic. In the centre of this disk are two holes, one of which receives a leaden tube communicating with an exhausting syringe S, while into the other hole another tube is soldered, which extends to the bottom of an adjoining carboy of sulphuric acid. By means of the syringe, the bottle being exhausted, the contents of the carboy are forced into it by atmospheric pressure.

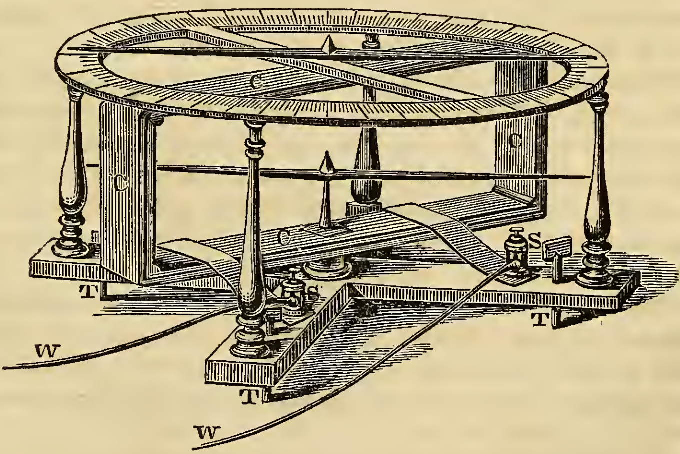
The gum elastic being stretched over the disk, is secured by a clasp which is fastened round the periphery by a screw.

This apparatus may be employed to raise liquors into a bar room, from casks in a cellar, with this advantage over the pump now used for that purpose; that the liquor does not pass through a pump. It has only to come into contact with a pipe of from one fourth to three eighths of an inch in bore.

The attachment to the cask is easily made by a gallows screw soldered to the nozzle of a cock, or simply to a ferule which may be driven into a cork hole, punching the cork before it.

The foaming of fermented liquors would be promoted by the exhaustion, lessening, for a time the atmospheric pressure on the carbonic acid.

3. *Improved Galvanometer or Multipher, of an unusually large size.*



This engraving represents a large multiplier, or galvanometer, the needles of which are each about eighteen inches in length. The instrument is furnished above with a circle graduated into three hundred and sixty degrees. Agreeably to the usual construction, the needle being within, the coil is subjected both above and below to the concurring influence of a current passed through the coil. In this predicament is the lower needle in the adjoining figure. When a needle is situated outside of the coil like the upper one in this figure, the influence of the lower portion of the coil, so far as it operates, must counteract that of the upper one. Yet when the lower portion of the metallic coil is at a distance from the upper portion of about one third of the length of the needle, and this is situated very near to the upper portion as here represented, the influence of the latter may so far predominate as to render the indications very nice; while they are as much more easily seen and estimated by means of the graduated circle, when, as in the situation of the upper needle, nothing intervenes between it and the eye.

In another instrument of the same dimensions I have used only a semicircle for the graduations, which, excepting the appearance, answers as well.

In lieu of wire, a coil of tin foil of about an inch in breadth and eighty feet in length, separated by thin paper, may be used, but a copper wire of No. 16, and of about one hundred and eighty feet in length, coated with shell lac varnish, will be more efficacious.

The coil of tin foil or varnished copper wire, is wound about the paralellogram C C C C. The ends of the coil are severally soldered, or screwed, under the basis of the gallows screws S S.

When both needles are placed upon the pivot at the same time by the repulsion of their similar poles, they will diverge from the meridian unless they be in a reversed situation, in which case they will appear as in the engraving, the north pole of one pointing north, the north pole of the other needle south. When, under these circumstances, a discharge is made through the surrounding coils, the consequent movements are very striking.

The clean surfaces of disks of zinc and copper, each an inch in diameter, separated by paper moistened with pure water, are sufficient to move the needles sensibly. The wires W W are used for the purpose. They are attached to the instrument by gallows screws S S.

The level of the machine is preserved by the aid of four screws, of which only three can be seen in the drawing at T T T.

Art. X.—*Analysis of Shells;* by William B. Rogers, Prof. of Chem. and Nat. Philosophy in William and Mary College, Va.

I propose, at present, to give you a sketch of some analyses of the recent oyster shell. The enquiry is one of some interest from the importance which has hitherto been attached to the supposed presence of animal matter. The existence of this has been made a reason among agriculturalists for anticipating peculiar benefit from the application of oyster shells to the soil. Medical men have included the preparations from testaceous shells among the articles of our modern pharmacopeias, ascribing the conceived superiority of these preparations in producing comparatively little irritation of the stomach to a considerable quantity of animal matter, thought to reside in the shell.

It will be seen by the following analysis, that the proportion of this ingredient, is so minute, as to render it not worth estimating, either when we compare the different modes in which the shell may be applied to the earth, on the different forms in which we may use it in medicine.

The subject is also one of interest to geologists as shewing, that the absence of animal matter in fossil shells, is not to be ascribed invariably to fossilization, but may result in certain genera, from none being contained in the recent state.

Mr. Hatchett, the English chemist, was the first who undertook a systematic investigation of the chemical composition of bones, shells, and other analogous substances. He establishes the general result, that, shells and bones, although greatly diversified in structure, may be arranged into two distinct classes—both of them containing animal matter—indurated in the one case, by carbonate of lime, and in the other by phosphate. The shells of many marine animals, and several zoophytes he found to occupy an intermediate rank, containing animal matter, with variable proportions of *both* the calcareous salts. The observations of this eminent experimenter appear in the main to be correct—but in some of his details, especially in regard to the oyster shell, he seems to have fallen into error. Murray, in his analysis of Hatchett's labors, speaking of the animal matter contained in shells—remarks that, "this substance often constitutes a *large* part of the shell, as in that of the oyster or muscle;"—a statement which has since been disproved by the analysis of Bucholtz and Brandes, and which is equally inconsistent also with the experiments about to be detailed.

The shells submitted to analysis were of middle size, had been some weeks opened, and were perfectly dry, and as clean as they could be made by washing in cold water with friction. There was therefore a small portion of the dark crust adhering to their outer surface, and this was intentionally permitted, to remain. It did not, however, form one half per cent, of the entire mass.

The shells were finely pulverized and then sifted through a very fine sieve.

1. Two specimens of the powdered shell each 200 grains, were enclosed in small glass matrasses of known weight; these were placed on a sand bath and exposed sometime to a temperature of 200°, this having been found by previous trial, to be about the highest heat which the substance would bear without a change of color indicative of incipient decomposition. A considerable dew collected in the long neck of each vessel, which by continuing the heat was all finally expelled.—Each flask with its contents was now accurately weighed. The difference of weight, now and before heating gave the amount of moisture which in one instance was 3 grains, in the other 3.2 grains, giving an average of 3.1 grains in 200 grains of 1.55 per cent. Results almost precisely the same were obtained in a similar experiment afterwards.

2. 400 grains of the powder were now digested in pure muriatic acid. Two filters were prepared, dried on the sand bath and counterpoised. Through one of these the liquid was filtered, the insoluble matter carefully washed, and the filter and contents dried. Both filters were now exposed to the sand bath to bring them both to the same degree of dryness, and then placed in opposite scales. The excess of weight arising from the insoluble matter on the filter was 3.4 grains; of this obviously only a part was animal matter, and it became a matter of delicate experiment to determine accurately how much.

3. The filters were successively burnt in a platinum crucible, until nothing but a white ash remained,

The results weighed as follows—

That of the filter containing the residue 2.2 grains,

That of the filter alone, - - 0.6 grs.

The former residue, viz: 2.2 grains, obviously consisted in part of the proper ashes of the filter itself. This, amounting to 0.6 gr.

of incombustible matter left on the filter. Deducting this from 3.4, the entire quantity before combustion, we have 1.8 gr. of combustible or animal matter. The incombustible matter was found to be silex. The 400 grs. of shell therefore contained,

Of silex, - - - 1.6 gr.

Of insoluble animal matter 1.8 gr.

4. The solution (2) was now examined for the phosphate of lime, of which it was suspected to contain a small quantity. Pure caustic ammonia was added in excess; the liquid became cloudy, and after some time threw down a flocculent precipitate. The precipitate was thrown upon a filter previously dried at 200° and weighed; it was then well washed and dried again at 200°. The filter now weighed 7.5 grs. more than at first, which of course is the weight of the precipitate. It was found to be insoluble in sulphuric acid which showed that it was not alumina, and before the blowpipe, it had all the characters of phosphate of lime. We have therefore in 400 grs. of the powdered shell, 7.5 grs. of phosphate of lime.

The solution which was now freed from phosphate of lime, was next tested for magnesia, but no indication of its presence could be obtained. The solution evidently contained no other earth but lime.

As a small portion of animal matter might probably be taken up, by the acid, and thus the quantity, as obtained by the above process, (3) fall short of the amount really present. Another mode of examination was adopted for the purpose of comparison, of which the following are the details.

5. Of the powdered shell 400 grs. were dried at the temperature of 200° and lost by the operation 6.5 grs. The mass was then exposed in a platinum crucible, to a dull red heat, over a spirit lamp. It became of a light brown color and lost several grains weight. It was now exposed to a very bright heat in a furnace for half an hour. On cooling, it weighed 379 grs. Its color was pure white. It was somewhat caustic and had obviously lost a good deal of carbonic acid as well as all its animal matter. To restore the former, the mass was heated in a porcelain capsule, with a concentrated solution of carbonate of ammonia, and the heat was continued at about 200; until the mass became dry and the odor of ammonia ceased to be exhaled. It now weighed 389.7 grains.

Repeating the same operation on another 400 grs. and exposing it to an intense white heat, the result was 272 grs. so that the mass was almost pure caustic lime. Treated with carbonate of ammonia to restore the carbonic acid, its weight was 390 grs. The average of the two is 389.85 grs. for the earthy or saline matter in 400 grs. of the powdered shell.

Two parallel experiments were now performed, each with 500 grs. of the powdered shell, exposed to a white heat in a Hessian crucible. Treated as before with carbonate of ammonia, they gave 487 and 486.6 respectively, the average of which corresponds to 389.44 for 400 grains.

This differs from the results with the platinum crucible, by four tenths of a grain, caused no doubt by a small portion adhering unavoidably to the interior of the crucible. I therefore accept the result given by the platinum.

Subtracting 389.85 grs. from 400, we have 10.15 for moisture and animal matter. The moisture was already (5) found to be 6.5 grs. The remaining 3.65 grs. would therefore appear to be animal matter. By the direct process however we found the animal matter to amount only to 1.8 grs. It is to be observed that a small portion of the difference between these two numbers, 3.65 and 1.8 must be ascribed to the unavoidable loss in transferring the calcined powder from the crucible to the capsule in treating it with carbonate of ammonia, and again, in the operation of weighing. The remainder of the difference may probably arise from the disappearance of a small portion of animal matter by solution in the acid. We may therefore put down for soluble animal matter and loss 1.85 grains.

6. The mass remaining after the treatment with carbonate of ammonia, consisted as is evident from preceding experiments, of carbonate of lime, phosphate of lime and silex. The total weight of the mass was found to be 389.85 grains. Deducting from this the sum of 7.5 and 1.8 grs. the quantities of phosphate and silex, we have 380.75 grs. for the quantity of carbonate of lime in 400 grs. of oyster shell.

The results when summed up are as follows,

Carbonate of lime, - - - - -	380.75
Phosphate of lime, - - - -	7.50
Silex (probably accidental,) - - - -	1.60
Water, - - - - - - -	6.50
Insoluble animal matter, - - - -	1.80
Loss, and soluble animal matter, - - -	1.85
	400.00

One hundred grains of oyster shell will therefore give,

Carbonate of lime, - - - - -	95.18
Phosphate of lime, - - - - -	1.88
Silex, - - - - - - -	0.40
Water, - - - - - - -	1.62
Insoluble animal matter, - - - -	0.45
Loss, &c. - - - - - -	0.46
	100.00

As the quantity of carbonate of lime above stated was estimated by deducting the ascertained amount of certain ingredients, it was deemed important to verify the calculation by a direct determination of the proportion of the carbonate. Accordingly 100 grs. of shell were treated with muriatic acid,—filtered and then supersaturated with ammonia to precipitate the phosphate of lime. The liquid filtered from the phosphate was then acted on by oxalic acid and the oxalate of lime as before converted into carbonate. The carbonate when dried agreed in weight very closely with the computed number, being very slightly more than 95.18 grains.

The oyster shell is therefore a carbonate of lime, nearly in a state of purity, and it is in this light that it should claim attention either in agriculture or medicine.

The scollop shells (Pecten Jeffersonius and P. Madisonius) of the marle beds of the southern states of which I have recently analyzed several specimens, yield a larger proportion of animal matter than the recent oyster shell, and indeed in many instances these shells would seem to have sustained no loss of this or in fact of any of their original constituents.

The large variety of coral (Astrea) which belongs to the same marl, contains a very minute proportion of animal matter, and nearly the same per centage of phosphate of lime, as the oyster shell.

Further observations upon the chemical structure, of our living and fossil shells I reserve for a future communication.

Art. XI.—*Description of some new Shells, belonging to the coast of New England;* by Jos. G. Totten.

Solemya *borealis.* See plate in front of book

See the plate, Fig. 1. *h. i.*

Shell thin and fragile, oblong : *hinge* edentulous, placed near the anterior end, with a slightly prominent cartilage, and an interior elevated callus, which is hollowed and forked beneath, the lower portion being directed towards the anterior-basal margin : *valves* distinctly wrinkled concentrically along the anterior-dorsal margin, elsewhere radiated with from fifteen to twenty sets of lines, which consist of two lines, and are distinct on the anterior and posterior portions, and of more numerous and rather indistinct lines on the middle of the valve : *epidermis* dark brown, lighter between the sets of lines, very glossy, extending much beyond the basal and lateral edges of the valves, and, at the hinge-margin connecting them together nearly the whole length of the shell : *within* grayish blue : *umbo* destitute of the slightest elevation : *anterior* and *posterior* margins rounded : *superior* and *inferior* margins nearly parallel, the former rectilinear except at the ligament where it is slightly emarginate, the latter a little arcuated.

Length,	2·65 inches.
Breadth,	0·85 of an inch.
Diameter,	0·55 of an inch.

Inhabits the coast of Rhode Island.

I have found this shell only in the vicinity of Newport, where it is very rare—and I do not know that it has been found elsewhere.

On comparing the above with Say's description of S. *velum* (Journal of the Academy of Natural Sciences of Philadephia—vol. 2. p. 317) it will be perceived that there is a close accordance —his language, wherever it would apply, having been purposely adopted : still there is thought to be a specific difference, because,

1st. as to size.—A great many shells answering his description have been found near the locality of the above shell, but none of greater length than one inch.

2d. as to color.—There are now before me specimens of the present shell from the length given above down to the length of one inch, and the color is, alike in all, dark brown without, and grayish blue within, and—

3d. There is, at least so far as my examination has extended, a constant difference in the form of the callus at the hinge. The under side, arched and forked to embrace the posterior part of the anterior muscle, has, in our species, a less radius of curvature, and the hinder portion points obliquely forward, while in S. *velum* ? it points across the shell.

An enlarged sketch of the anterior end of S. *velum*? is given fig. 1. *k*.

Venus *gemma.*

Fig. 2. *a. b. c.* are enlarged—*d.* natural size.

Shell sub-rotund, nearly equilateral, concentrically furrowed, glossy ; anterior portion and basal margin, both within and without, white or pale reddish-violet, remainder reddish purple, darker at and near the superior and posterior margins : no *lunule: beaks* small, incurved, separate, generally eroded : *teeth* divergent, the medial tooth of each valve stout triangular, the anterior tooth of the right, and the posterior of the left valve, thin and not easily distinguished : inner margin crenulate.

Length,	0·15 of an inch.
The length being represented by	13
the breadth will be	12
and the diameter,	6.

Inhabits the coast of Massachusetts and Rhode Island.

I found this beautiful little shell, first, on the beach at Provincetown, Cape Cod, (Mass.); it has since been found in Newport harbor.

The largest specimen I have seen among many hundreds, is barely $\frac{3}{20}$ of an inch in length; the more common length being about $\frac{1}{10}$ an inch.

It is often much eroded on the disks, and then the color is bluish [illegible]le.

Modiola *glandula.*

Fig. 3.—*e. f. g.*

Shell obliquely oval, or sub-ovate, close, somewhat inflated, convexity generally rising regularly and gradually from the margins, sometimes an indistinct umbonial ridge reaches the posterior-basal margin : *valves* concentrically slightly wrinkled and minutely striated, and rayed with very numerous rounded ribs proceeding in all directions from the apex, increasing in number on the disk, and rather deeply cut by lines of growth : *epidermis* dull brownish-yellow, beneath which the surface is white : substance of the shell thin, translucent : *beaks* rather prominent, separate, recurved, decorticated : all the inner margin not occupied by the ligament crenulate : *within* white and pearly.

Greatest length, viz. from beak to posterior-basal margin, } 0·45 of an inch.
Greatest breadth at right angles to the above line, 0·35 "
Diameter " " " " 0·25 "

Inhabits Provincetown harbor, (Mass.)

This very pretty Modiola I have found only at the above place. The form is remarkable for the genus, being almost truly oval, and quite regularly convex. In the smaller specimens the umbones are, however, sometimes more prominent. The above dimensions are of the largest specimen I have seen.

Genus *Acteon*, Montfort.

A. trifidus.

Fig. 4.—*a*, enlarged—*b*, natural size.

Shell small, acute-conic, glossy-white, pellucid: *whirls* about eight, flat, with about six impressed revolving lines, the one immediately above and the two next below the suture, wider, deeper, and distinct, the intermediate lines, and also about twelve on the lower portion of the body-whirl, generally obsolete; transverse striæ obsolete: *spire* gradually tapering to an acute apex: *suture proper* not impressed: *mouth* elongate, one third the length of the shell, acutely angular above, produced and rapidly rounded below, base not effuse: columella-edge slightly recurved below: *columella* with one oblique fold: *outer lip* entire, not thickened and sometimes showing within, and on its sharp edge, the impressed lines: *operculum* horny.

Length, 0.20 of an inch.
Greatest breadth, 0.08 do.

Inhabits the shores of Rhode Island. Found adhering to Pecten concentricus.

Genus *Turbo*, Lin.

T. inflatus.

Fig. 5.—*a* and *b*, enlarged—*c*, natural size.

Shell small, sub-globular, umbilicated, thin, shining, translucent: *whirls* five, convex, obsoletely and transversely striate, and furnished with a few obsolete revolving lines on the lower side of the body-whirl: *spire* low, convex, obtuse, shorter than the aperture: *suture* a little impressed: *mouth* round: *outer lip* sharp: *color* pale brownish-yellow, or horn color, immaculate: *operculum* round, horny, multispirate.

Length, 0.19 of an inch.
Greatest breadth, 0.23 do.

Inhabits Plymouth harbour, (Mass). This shell has a somewhat pearly lustre, and is feebly iridescent. We have, as yet, seen no Turbo on this coast of which this can be the young.

T. *minutus.*

Fig. 6—*a*, enlarged—*b*, natural size.

Shell small, conic, thin, translucent : *whirls* about six, convex, faintly striated transversely : *spire* gradually tapering to near the apex, then suddenly obtuse : *suture* impressed : *mouth* more than one third the length of the shell, oval, entire, rounded at the base, angular above : *outer lip* sharp : lower portion of the columella lip slightly recurved ; a calcareous deposit connects the lips, and, in adult specimens, rises above the surface of the shell, forming a prominent connection of the labium with the labrum : *color* brownish-yellow, or horn color, immaculate : *within* whitish : *operculum* horny.

Length, 0.13 of an inch.
Greatest breadth, 0.08 do.

Inhabits coast of Mass. and R. I.

Genus *Pasithea,* Lea.

P. nigra.

Fig. 7—*a*, enlarged—*b*, natural size.

Shell small, acute-conic, turrited, thin, black : *whirls* about eight, flat, having a distinct shoulder, and made to appear granular by four equal, raised, rounded, revolving lines, filling the space between the sutures, these lines being cancellated by numerous equal lines which are longitudinal as respects the shell—inferior portion of the body-whirl not cancellate, occupied by about six slightly raised revolving lines : *spire* acute-conic : *suture* deeply impressed : *mouth* more than one third the length of the shell, elongate, sub-ovate, acutely angular above, widely rounded below, and slightly effuse at the base : a thin, glossy-black calcareous deposit connects the lips : *outer lip* without a sinus, not thickened, and faintly modified on its sharp edge by the raised revolving lines: *operculum* horny.

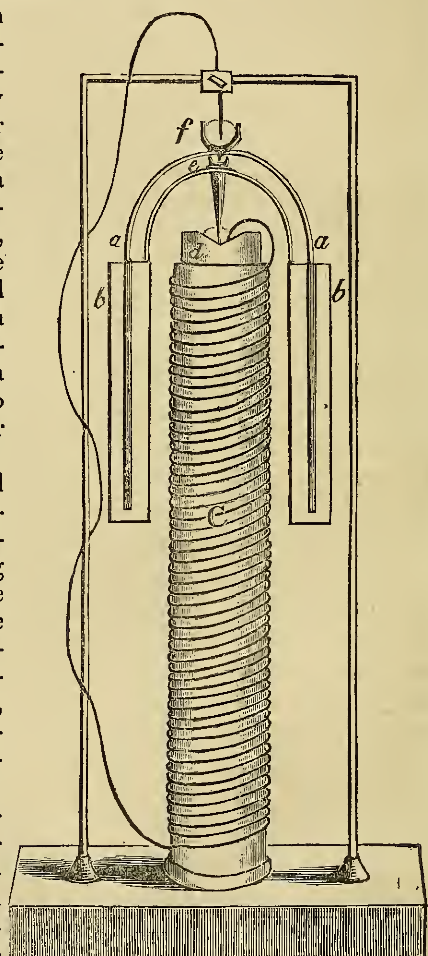
Length, 0.15 of an inch.
Greatest breadth, 0.08 do.

Inhabits the shores of Rhode Island. Found with Acteon trifidus adhering to Pecten concentricus. I know no group in which this shell can be so well placed as in the above new genus of Lea.

Note.—The crenulation on the inner margins of Venus *gemma*, and the fold on the columella of *Acteon trifidus*, have been omitted by the engraver.

Art. XII.—*Modification of Ampère's Rotating Galvanic Element;* by T. Edmondson, M. D. Baltimore.

The instrument is constructed in such a manner, that, the current of galvanism proceeding from the zinc side of the small element, will pass through a helix arranged as an electro-magnet, as it circulates onward to the copper side of the element.



a, the zinc cylinder in section,—*b*, the double copper cell,—*c*, the electro-magnet, made, simply by wrapping a coated copper wire around a tube of the common tinned iron,—*d*, a small metallic cup containing a globule of quicksilver, into which is dipped one end of the helix,—*e*, a small agate center, intended for a non-conductor,—*f*, a metallic cup, also containing a globule of quicksilver, into which is dipped the other end of the helix.

Upon exciting this small element with an acid solution, the current of galvanism prevented from passing directly from the zinc to the copper, on account of the non-conductor at *e*, is obliged to traverse *f*, and circulate through the helix to *d*, before it can reach the copper side of the galvanic arrangement.

The instrument has frequently revolved two hundred times in a minute, the copper cell moving fast in one direction, and the zinc cylinder in the opposite direction with great rapidtiy.

The rotating armature, has excited a great deal of curiosity here. The mechanics were very much puzzled by it and supposed it went by steam. I took care to conceal the battery which excited it and conveyed the galvanic fluid along two slender wires, without attracting their attention.

I send you the above little instrument, the peculiarity of it is, that it makes its own magnet while revolving.

Art. XIII.—*A Method of obtaining Iridium and Osmium from the Platinum residue;* by F. Wöhler. Translated by J. C. Booth, received in a letter to the editor, dated Berlin, April 20th, 1834.

The black powdery residue, which remains upon treating the platinum-sand with nitro-muriatic acid, contains, as is well known, a certain quantity of a natural alloy, osmium-iridium and probably no inconsiderable proportion of uncombined iridium in powder. It has always been a difficult matter to separate the two last metals from the ore, as well in consequence of the difficult decomposition of the alloy, as on account of the presence of large quantities of foreign ingredients, and particularly of titanate of iron. The discovery of a simple and unexpensive method of decomposition was so much the more desirable, since this remainder has accumulated in places where platinum is largely worked, and it is not improbable that an easy method of obtaining iridium, a metal nearly allied to platinum, may lead to a highly useful mode of application in the arts. It was for this purpose necessary to find the means of extracting osmium and iridium alone without affecting the titanate of iron; for since the particles of the latter cannot be separated by the magnet or otherwise mechanically, the presence of such large quantities of iron and titanic acid might render the employment of such a method difficult. The method I am about to describe appears to me sufficiently adapted to the purpose, and could be employed in the large way without difficulty. It is founded on the means resorted to by Berzelius of bringing iridium, insoluble in aqua-regia, to a soluble state and one suited for the preparation of its other combinations,—viz. by passing chlorine over a heated mixture of iridium and chloride of sodium. The process for the platinum-residue* is as follows:—

* It is unnecessary to powder it. The trouble of mineralogists, will however be amply repaid, by previously spreading it out upon paper and seeking out the larger

It is mingled with an equal weight of previously dried and powdered chloride of sodium and introduced into a tube of green glass of sufficient length and width. (In the large way, tubes of earthen ware could be used.) This is laid in a tube furnace (and that employed by Liebeg, in organic analysis seems best adapted to the purpose ;) one end of the large tube is attached to the chloride of calcium tube, to dry the chlorine which passes throughout the apparatus, while from the other end proceeds a balloon to retain the subliming osmic acid. From the opposite end of the balloon, passes a small bent tube into dilute ammonia in order to catch the last portions of the same volatile acid. Ignited charcoal is then laid under the whole length of the tube, so that the mixture occupying two thirds of the interior diameter, is gently ignited. Chlorine is now slowly evolved and passed through the apparatus. It is at first so fully and in such quantity absorbed, that no bubbles pass through the ammonia. When this occurs, which is the case after the lapse of a couple of hours by employing one hundred grm. the operation is completed, the apparatus suffered to cool and taken apart.

The explanation of the operation is, that the soluble chlorides of iridium, and sodium, and of osmium and sodium, are formed and titanate of iron remains undecomposed. Through moisture in the chlorine, chloride of osmium seem to be constantly decomposed in such a manner that hydrochloric and osmic acids are formed and osmium precipitated, to be anew subjected to the action of chlorine. A small quantity of deep green or red chloride of osmium is generally found in the farther part of the tube. This much is certain, that the greater part of the osmium is obtained as osmic acid. The separation of the two metals is likewise founded upon the experiments of Berzelius.*

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particles of osmium-iridium, since they are even by the above treatment only superficially acted on and might otherwise be lost. It farther deserves attention, that there must be among these particles some insoluble in aqua-regia, an alloy of gold probably with osmium or iridium. At least in one of my experiments, according to the above method, I obtained no inconsiderable quantity of gold. In succeeding trials, with new portions of the remainder, I could not detect the slightest traces of this metal; whence the conclusion that in the first portion particles were present, probably a rarely occuring alloy of gold with another metal. It is lastly to be remarked that silver is contained in the platinum-remainder. For in that which I employed (from America) I found no inconsiderable quantity of chloride of silver, which was extracted by ammonia,—an operation which the remainder must undergo to obtain the silver.

* Poggendorf's Annalen, XIII. 467, XV. 208.

The greater part of the osmic acid, is likewise deposited in a beautiful crystallization in the balloon. In order to preserve it as such, the balloon is gently warmed and it is suffered to flow into a closely shutting vial or into a tube which may be closed by the blowpipe. But the greatest precaution must be observed by this operation, for there is probably no substance which highly diluted with air is more capable of violently and dangerously affecting the eyes and lungs than osmic acid. Nothing is easier than repeatedly to crystallize this most curious substance. If the vessel containing it be placed in a window or in such a manner that one side is cooler than the other, it sublimes like camphor, from one part to another, sometimes in well formed and often very long crystals.

To the ammonia, which is colored yellow from the absorption of more or less osmic acid, muriate of ammonia and carbonate of soda are added, the whole evaporated to dryness and heated at a low red heat in a retort. The acid is by this means reduced to the metallic state, and upon treating with water remains as a black powder. It is washed and dried. The osmic acid in the balloon may be reduced in the same manner after solution in ammonia.

The content of the large tube is somewhat caked together (agglutinated). By placing this tube in a large cylinder of water, the mass is easily loosened and the soluble parts extracted. A deep brown solution is obtained of a double salt of iridium. It smells faintly of osmic acid arising from decomposing chloride of osmium. The solution is decanted from the remainder, which consists principally of titanate and chromate of iron and in which may be observed the larger leaves of osmium-iridium. The decanted fluid may be subjected to distillation to obtain more osmic acid, the receiver containing ammonia. When one half the liquid has passed over, the operation is terminated and the remainder filtered.

It is now placed over the fire in an evaporating dish and during its vaporization, carbonate of soda is gradually added until it is in excess, by which a precipitate, at first brown but at length blue-black, is formed. The dried black mass is gently ignited in a Hessian crucible and after cooling, heated with water. A coal-black powder remains consisting chiefly of the deutoxide of iridium ($\dddot{\overline{\mathrm{Ir}}}$). It is washed and dried. The solution is thrown away, since in addition to common salt and carbonate of soda, it contains merely, an alkaline chromate producing its yellow color.

Expecting a portion of osmium, to separate which a particular treatment is required, the deutoxide of iridium still contains oxide of iron. It is laid in a glass tube and hydrogen passed over it Thus treated it generally becomes ignited of itself and may be reduced without the application of heat. It is however better to lay a few coals under the tube and pass the hydrogen over as long as water is generated.

Thus obtained, iridium is a black powder, containing much caustic soda which was chemically combined with the oxide and may now be extracted with water. Iron is separated by digestion with muriatic acid. After being washed, the powder is laid between many folds of blotting paper and powerfully pressed for several hours, in a screw press. After the cake thus obtained has been fully dried it is laid in a crucible and subjected to the heat of a forge. This iridium is gray, somewhat solid and capable of receiving a polish.

In the same state it may be immediately obtained from the impure oxide, but then it requires long continued digestion in aqua regia to free it from iron, a portion of iridium is likewise dissolved and if it contained silver this is also found in the solution.

A still shorter process is to evaporate the tritochloride of iridium and sodium to dryness and to expose it to a strong red heat till it fuses and chloride of sodium begins to vaporize. The metal is thus perfectly reduced and remains, by dissolving the salt in water, as a heavy gray or black powder. It is however impure and on account of its compact state can scarcely be freed from iron.

Palladium is not contained in the chloride solution nor as it appears, rhodium. Muriate of ammonia and chloride of potassium added to the same to saturation, throw down but a small part of the iridium even when, in order to convert the whole into a chloride, the solution is previously saturated with chlorine. If the reddish black chloride of iridium and potassium washed with chloride of potassium, be exposed to a powerful white heat until all the chloride of potassium is vaporized, pure iridium remains in the form of shining, silvery, crystalline scales.

By treating the platinum remainder with chlorine and chloride of sodium, it loses on an average 25—30 pr. ct. of its weight, but is by no means exhausted; for exposed a second time to the same operation it loses 5—7 pr. ct., and still the same particles of iridium and osmium are to be seen in the remaining black mass. After the first treatment, there may be distinctly seen powdery metallic osmium generated by the action of water upon chloride of osmium.

Mr. Booth of Philadelphia, who is at present engaged in my laboratory, and who rendered me much assistance in these experiments, discovered at the same time a *cyanid of iridium and potassium.* He obtained it according to the method given by Gmelin for the corresponding platinum salt, viz., by gentle long continued ignition of a mixture of dry protocyanide of iron and potassium (prussiate of potassa) with iridium in powder. This must be performed apart from the air, for otherwise, when heated to a certain degree, the mass undergoes combustion. The coagulated mass is powdered and dissolved in water. Upon evaporation of the almost colorless solution, a portion of undecomposed iron and potassium-cyanid crystallizes and at length the salt of iridium.

Cyanide of iridium and potassium (probably protocyanide) crystallizes in long four prisms, generally in twin-crystals similar to selenite, to judge from the angle on their terminating planes. They are perfectly colorless and clear, and have not that blue and yellow opalescence, peculiar to the corresponding salt of platinum. They are soluble in water and not in alcohol, and their solution is not precipitated by sulphuretted hydrogen. They contain no water;—decrepitate strongly on being heated and become black;—by stronger heat they fuse and iridium separates, often covering the glass with a metallic mirror.

The excess of iridium, which had been added to the potassium—iron-cyanide, and which remained after dissolving the ignited mass in water, has taken up much iron and carbon. It has now become so combustible that when ignited in one point, it continues the ignition of itself throughout the whole mass, like a pyrophorus. The iron may afterward be easily separated by digestion with hydrochloric acid.

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

Appendix, continued from Vol. xxvi. p. 108.

No. 132. Carex *incurva*, Lightf.
Wahl. No. 19.
C. *juncifolia*, All. et Wahl. No. 17.
Schk. Tab. Hh. fig. 95.

Spiculis androgynis *distigmaticis* apice staminiferis in caput ovato-globosum aggregatis; fructibus ovatis subconicis teretibus subrostratis ore integris, squama ovata acuta paulo longioribus.

Culm one to two inches high, curved, with leaves sheathing at the base and longer than the culm; spikelets several, aggregated into a dense head, staminate above; stigmas two; fruit ovate, roundish and tapering, subrostrate, entire at the orifice, a little diverging; pistillate scale ovate, acute, a little shorter than the fruit: plant light green.

This is a small, beautiful species found in the northern Alps of Europe; and on the Rocky Mountains, by Dr. Richardson.

No. 133, C. *supina*, Willd.
Wahl. No. 106.
Schk. Tab. I. fig. 41.

Spicis distinctis, staminifera solitaria; pistilliferis subbinis *tristigmaticis* subrotundis subsessilibus approximatis; levibus ore bilobo, squamam ovatam subæquantibus.

Culm about six inches high, slender, leafy at the base; leaves narrow and flat; staminate spike single, slender, with oblong scales whitish on the margin; pistillate spikes one or two, nearly sessile, globose, approximate, the lower with a short lanceolate bract; stigmas three; fruit roundish, subglobose, smooth, brown, rostrate, emarginate or with a two-lobed orifice; scale ovate, acutish, very nearly as long as the fruit.

This species, found in Austria and Tyrol, &c., was found also near Bear Lake in the Northern regions, by Dr. Richardson.

No. 134. C. *laxa*, Wahl. No. 93.
Schk. Tab. Aaa, fig. 78.

Spicis distinctis, staminifera solitaria oblonga; fructiferis tristigmaticis oblongis exserte-pedunculatis pendulis subdistantibus; fructibus ellipticis triquetro-depressis brevi-rostratis ore integris, squamam ovato-oblongam obtusam aequantibus.

Culm 6—10 inches high, sub-prostrate and flaccid; leaves shorter than the culm, sheathing, brownish towards the base; spikes distinct, staminate single, cylindric, with oblong scales sometimes slightly mucronate; stigmas three; fruitful spikes one to three, pedunculate, pendulous in fruit, with short sheaths; fruit ovate and obtuse or elliptic compressed and triquetrous, entire at the orifice; scale oblong, or ovate-oblong, obtuse, sometimes with a short point, about equalling the fruit.

This carex closely resembles C. *limosa*, as both Wahl. and Schk. remark, and belongs in the section with this species. It is a more delicate plant, its fruit more elliptic, and its pistillate scale is shorter and obtuse, while the other is more lanceolate. In Europe it is found in Lapland; also at the Norway House in the northern regions of America, by Dr. Richardson.

No. 135. C. *ovalis*, Gooden.

C. *leporina*, L. and Wahl. No. 35.

Schk. Tab. B. fig. 8.

Spicis androgynis, basi staminiferis, subsenis ovalibus sessilibus subapproximatis; fructibus *distigmaticis* ovato-ovalibus compressis acuminatis membranaceo-marginatis ore bifidis, squamam ovato-longam æquantibus.

Culm twelve or twenty inches high, scabrous above, with leaves sheathing towards the base and nearly as long as the culm; spikelets about six, ovate or oval, sessile, tawny, staminate at the base; stigmas two; fruit ovato-oval, acuminate, compressed, membranaceous at the margin, bifid at the orifice; scale ovate, oblong, acutish, about as long as the fruit, whitish on the edge; plant light green.

This is the C. *leporina*, L., but not his plant in Flor. Dan., which is the C. *lagopina* of Wahl. and C. *approximata* of Hoppe and has only three or four spikelets different from these. The C. *ovalis*, Gooden. is found in England, &c., and lately on the Rocky mountains. By the plant under this name Pursh probably meant C. *scoparia*.

No. 136. C. *Vahlii*, Schk.

C. alpina, Vahl.

Sch. Tab. Gg, fig. 94 and Tab. Ppp, fig. 154.

Spicis subternis, tristigmaticis, suprema androgyna oblonga infernè staminifera, ceteris pistilliferis ovatis vel oblongis subpedunculatis brevi bracteatis; fructibus subrotundo-ellipticis triquetris ore integris, squama ovata vel oblonga subacuta majoribus.

Culm 6—10 inches high, triquetrous, smooth, leafy towards the base; leaves flat, soft, sheathing, upper ones long as the culm; stigmas three; spikes three or four, sub-umbelform, upper one oblong, staminate below; the others pistillate, ovate or oblong, subsessile, bracts longer than the culm; fruit oval, roundish, triquetrous, orifice entire, light green; scale ovate or oblong, acute or somewhat obtuse on the same spike, shorter than the fruit, and *black.*

Found in Alpine meadows of Lapland; and by Dr. Richardson, on the Rocky mountains, and also at the sea-coast of the Arctic regions of America, full in fruit June, 1826.

Figures of two Carices are in this volume;

C. *Baldwinia*, D. Tab. T. fig. 61.

C. *venusta*, do. 62.

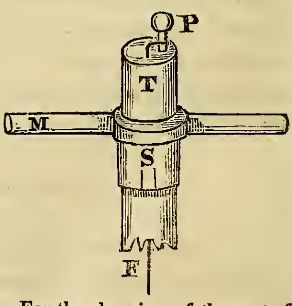
Art. XV.—*Communications by* Dr. John Locke, of Cincinnati.

I. *Improved modification of Dr. Locke's Galvanometer.*

Cincinnati Female Academy, Feb. 14, 1834.

TO PROFESSOR SILLIMAN.

Dear Sir—In my last, I sent you some account of my galvanometer as finally made in the discoid form on a wooden ring. My communication was accompanied by a drawing of the instrument as enclosed in a cylindrical box. I now communicate to you an improvement which I have since made by adding a stationary magnet to neutralize the effect of the earth's magnetism on the needle. This magnet I have adapted to the brass tube which rises from the center of the glass cover, and encloses the filament suspending the needle. It is two and a half inches long, and one eighth of an inch in diameter, having a large perforation in the middle, in which is inserted a short piece of brass tube having an inside diameter adapted to the suspension tube of the instrument. The tube and the magnet thus connected surmount the instrument with an ornamental cross. T The suspension tube. P the ivory pin for winding the filament by which the needle is suspend-



For the drawing of the rest of this instrument, see p. 105 of this volume.

ed. M The neutralizing magnet. S The socket slitted and springy so as to move "finger tight" on T.

Experiments 1. Having adjusted the magnet to such height as nearly to neutralize the effect of terrestrial magnetism, I brought the needle to north 45° west and raised a wine-glass of river water to the battery of the instrument, consisting of a five cent piece of silver and a corresponding disc of zinc. The needle turned with an equable velocity to the north, and finally quite to the east having described an arc of 135° in twenty one seconds.

2. Having procured a rude piece of antimony and a strip of sheet copper I connected them with the wires of the galvanometer and finally with each other, having previously warmed the copper *by holding it in my hands.* The needle was deflected 80°.

3. I applied the two wires to the two extremes of the piece of antimony, which was about three inches long and one a half thick, pressing one wire to it with a piece of wood and the other *with my thumb and finger.* The warmth of the hand, thus applied caused the needle to turn 76° when the wood and the fingers were changed to the opposite extremes, the needle reversed its motion.

Whether the above experiments indicate any *superior* delicacy in the instrument I am not able to say. So far as my own experience goes, it excels by far all other modifications.

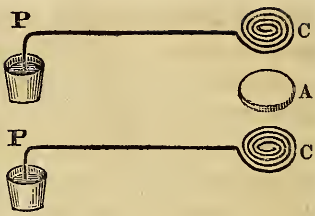
II. *Experiments*, March 12th, 1834.

The most delicate experiments which I have made, are as follows:—

1. When the needle is nearly neutralized by the fixed magnet, the application of river water to the 5 cent piece battery will turn the needle 90°; and often by impulse throw it entirely round.

2. The poles (copper wires) of the instrument being held to the extremes of a piece of antimony as large as a hen's egg, the one by a piece of wood and the other by the fingers, the needle will turn 90° in consequence of the thermo-electricity generated by the heat of the fingers.

3. Two discs of copper attached to the poles, and half an inch in diameter including a disc of antimony of the same size caused the needle to turn 90° upon the application of the end of the finger to one of the copper discs.

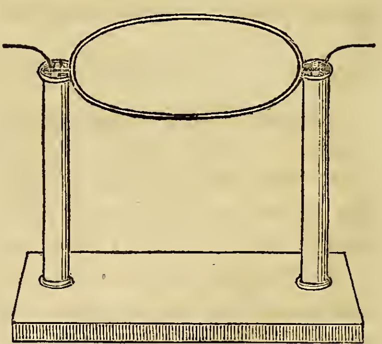


A convenient apparatus for this digital experiment consists of two copper wires coiled at one end into a close flat spiral C and bent at the other end P at right angles downward. The coils being placed flat on the table with the disc of antimony A between them and the bent ends in the mercury cups of the galvanometer, the apparatus is ready for experiment.

4. With one fourth of a grain of antimony between two straight wires the needle was deflected 22° upon the application of the fingers.

5. A piece of copper wire one sixteenth of an inch in diameter and 2 inches long being laid with its ends across the copper wires (the poles or termination of the coil of the instrument) and one of the junctures pressed between the thumb and finger the needle was deflected 6 degrees.

I have constructed an electro-magnetic multiplier upon the plan of Schweigger with 120 coils within one inch of the needle above and below it. It is affected by water; but not by the thermo-electricity produced by animal heat.



I have also constructed a dipping ring. It is a coil of about 40 turns in a circle of $5\frac{1}{2}$ inches in diameter. The ends of this coil terminate in two brass pivots diametrically opposite to each other. These pivots which are small, roll on two horizontal straight edges and have their extreme ends, which are amalgamated running in mercury contained in little copper troughs. The mercury rises, by its tendency to form a globule, above the trough which contains it, thus uniting itself to the pivot which is straight. The ring being nicely balanced like a dip needle, and a current of electricity sent through it, by bringing the poles of the galvanometer into the mercury troughs, will turn on the pivots till its plane is perpendicular to the axis of the true dip of the place. It is difficult to make the pivots so true that the ring will turn at once into the plane unless we go to the expense of the machinery of a dipping needle; but oscillations can be obtained at various points by which the tendency may be ascertained. Temporary balances may be applied at any point to counteract the imperfections of the machinery.

The experiment of the dipping ring is so strongly indicated by many other experiments that I think it probable it has been made; but I have seen no account of it.

III. *Additional Experiments*, April 21*st*, 1834.

Finding my discoid galvanometer to be a very delicate indicator of thermo-electricity, I have been able to attempt to ascertain the thermo-electric powers of all of the metals, by experiments similar to those of Prof. Emmet detailed in the last number of the Journal of Science.

Whilst I enter the same field of original investigation with the distinguished philosopher just named, and arrive at conclusions in some points slightly different from his, let it be understood that I regard his labor and discoveries with no other sentiment, than that of admiration.

My object was chiefly, to form a scale of the thermo-electric powers, of the several simple metals as indicated by the galvanometer, when *two parts of the same metal* unequally heated are placed in contact. In order to do this in the most unexceptionable manner, I made the following preliminary experiments.

1st. I made a circuit of copper wire six feet long and connected its extremities with the poles of the galvanometer. To this wire I applied the flame of a spirit lamp and moved it rapidly along from one point to another to ascertain whether the more progressive motion of heat in metal was a cause of an electrical current. No electricity was indicated by the needle.

2d. Cutting the same wire in the middle, and connecting the cut ends by twisting them together, I applied the flame of a spirit lamp close to one side of the joint. The needle of the galvanometer was turned 10 deg. in 33 seconds, and 20 deg. in 48 seconds, in such direction as to indicate a current of electricity passing through the joint from the heated side.

3d. To acertain whether the rupture of the wire produced any effect when the heat did not reach it I applied the lamp to a point remote from it. No motion of the needle followed.

4th. To ascertain whether the connexion of my apparatus with the galvanometer by means of mercury cups, or solder would influence an experiment I cut the same circuit of wire again near one of its ends and soldered into it a disk of bismuth about three fourths of an inch in diameter, this forming a *compound circuit.* I then applied the lamp near the twisted joint as in No. 2. The effect was the same as before the bismuth had been inserted.

5th. Applying my finger to the solder between the bismuth and the copper of the compound circuit, the needle was put into such motion by the electricity generated by the animal heat that in three seconds, it acquired a velocity sufficient to turn it several times round. It was impossible to touch this soldered joint for the fourth of a second without putting the needle in motion.

6th. Finding this compound circuit to be so sensible a differential thermometer, I took the opportunity to determine the progress of heat through the wire. The following table shows the result of the application of the lamp at different distances from the bismuth.

The adjacent table shows that the times in which the heat moves along the wire are nearly as the squares of the distances. This appears from 2 and 4, and 3 and 9, in the first and second column; and from 4 and 16, 5 and 24 (25), 6 and 36, 7 and 50 (49) in the first and third columns.

	Time from the application of the heat		
Distance	till the needle began to move.		till the needle had moved 6°.
Inches.	Min.	Sec.	
18	4	0	No motion.
12	3	0	slight agitation.
9	0	45	75 seconds.
7	0	35	50 "
6	0	25	36 "
5	0	16	24 "
4	0	12	16 "
3	0	9	12 "
2	0	4	6 "

My only object, however, in these experiments was to determine what length of uniform uninterrupted metal must intervene, between the point where I applied the heat and the connexion with the galvanometer and also the length of time which I might occupy in the operation. My conclusion was that the metal under experiment must extend in form of a wire, one sixteenth of an inch in diameter, at least twelve inches from the part intended to be heated, and the heating and experiment must then occupy less than three minutes.

Having procured several metals in suitable forms I proceeded to my main experiments. Heating one piece of the metal in the first experiments from 56° Farenheit, to about 95°, by the warmth of the hand, and in the second from 56° to 212° by a spirit lamp, I applied it instantly to its fellow at 56°, both being connected with the galvanometer, and noticed at each the number of degrees through which the needle moved in four seconds. I used equal times that the motion whether uniform or accelerated might be in proportion to the force. I found that the result varied in some degree with the varying circumstances of each experiment; some of the causes of these fluctuations were ascertained, but others could not be detected. Perhaps they were produced by the various points which happened in each instance, to be in contact. I found that the electricity passed in some metals through the rupture *with* the heat and in others in opposition to it. The former metals may be called as by Prof. Emmet *positive* and the latter *negative*. For conciseness, the terms *coincidence* and "*opposition*" may be used. To show how much these experiments may be varied by circumstances, I will state the particulars of those which I made with silver. Having procured a silver wire, one sixteenth of an inch in diameter, and two feet long, drawn without annealing,—I cut it in two and connected the other ends with the galvanometer, warmed one of the cut ends by holding it in my hands and laid it upon the other, the needle turned three degrees in four seconds in such direction as indicated a *negative* current. After letting it cool I warmed the other end and applied it in the same manner to its fellow. Although the course of heat had been *reversed* the needle still turned *in the same direction*, indicating a *positive current* of the same force as the negative one! here it seemed *the electricity passed in the same direction along the wires whether the one or the other was heated.* After heating the ends of them to redness over the spirit lamp I let them cool and heating one to 212° Far. applied it to the other, and letting them cool again, reversed the experiment by heating and applying the other. Both of these experiments showed a negative current but in one it was scarcely perceptible while in the other it was 12° in four seconds. This change produced by heating the ends of wires gave me the the hint to ANNEAL them throughout. I did so, and then found the results to be rational and uniform. They were *positive* four degrees in four seconds by hand heat, and sixteen degrees in four seconds by boiling heat. The same result was obtained by reversing. It seems by the results of these experiments that the metals must not only be of proper lengths without joints or flaws but must have their particles in *crystalline repose* in order to act unequivocally. It also appears that *when one part of a wire has been annealed and the other not, the two portions have different capacities for caloric and act like two different metals soldered together at the point of difference.* Drawn, hammered or rolled metals therefore give equivocal results.

I find by my experiments that the metals give generally positive or negative results according to their relation to oxygen ; the less oxidable metals being *positive,* and the more oxidable ones *negative.* Gold, which Prof. Emmet is surprised to find, by his experiments, on the *negative* side appears in my experiments on the *positive* side and that whether it has been annealed or not. It is to be regretted that experimenters in these interesting subjects do not give us more of the circumstances of their manipulation to enable us to judge of their conclusions. I apprehend that some of Prof. Emmet's very interesting experiments have been made equivocal by some of the circumstances so liable to make them so ; and I am *not quite* sure that my own may not be corrected by future investigation. I have however detailed some of the circumstances to enable other persons to judge of them. The following is the result of my best observations. *Positive metals* in which the current of electricity from one portion of metal to the other proceeds from the heated portion in *coincidence* with the caloric. Deviation of the needle in 4 seconds,

	By hand heat, from 56° to 95°.	By boiling heat, from 56° to 212°.
Antimony,	+8° ?	+32° ?
Silver,	+4	+16
Copper,	+2	+ 8
Gold,	+1½	+ 6
Lead,	+0	+ 0

My experiment on antimony is questionable as to the *degree,* because I could not fashion this refractory metal into the desired form. Lead is sometimes null and sometimes variable.

Negative Metals, in which the current of electricity from one portion of metal to the other, proceeds from the colder in *opposition* to the caloric. Deviation of the needle in 4 seconds,

	By hand heat, from 56° to 95°.	By boiling heat, from 56° to 212°
Bismuth,	11°	44°
Iron,	4	16
Zinc,	1	4
Tin,	Imperceptible.	0½
Lead,	0	0

I have made no experiment on platinum, because I could procure no specimen of a size and form parallel with my specimens of the other metals.

The problem of mercury remains unsettled because of its fluidity.

I am inclined to think that crystallization or the condition of the ultimate particles, has much influence over the direction of the currents. Hence antimony and bismuth stand at the head of each class. The other metals not remarkably crystalline arrange themselves nearly according to their relation to oxygen. I have made no scale of the thermo-electric powers of the metals upon each other. This would be the easier task because generally the electrical current is there much more decided and certain. Take for example that of bismuth and copper. Bismuth and bismuth by the heat of the hand turned the needle 11 degrees in 4 seconds; while bismuth and copper, under the same circumstances deflected the needle at the rapid rate of 160 degrees in the same time. The needle of my galvanometer weighs 20 grams and is $2\frac{3}{4}$ inches long. It is so far neutralized by a fixed adjustable magnet as to perform but three vibrations per minute. The magnetic force acting on one end of the needle as a pendulum is the 17424th part of the full force of gravitation, or the 1742d part of a grain. This is the parallel force, and the deflecting force for one degree would be the *one hundred thousandth part of a grain.* I despair therefore of making the instrument more sensible, unless I exhaust it of air which seems by its "viscidity" to be an impediment to motions and forces so delicate, in the same manner as it counteracts the gravitation of a downy feather so as to prevent it from falling.

All of my experiments indicate that the thermo-electric current is confined to a narrower sphere about the conductor than the galvanic current, hence my discoid galvanometer which has its needle almost in contact with the conducting wires is so sensible to the former. Perhaps the most astonishing experiment is that made with a disc of bismuth included between two flat coils of copper wire not larger than a shilling. The end of the finger being applied to the upper coil when the temperature is 50 will give the needle such a velocity in 3 seconds as will carry it four times round; even to day the temperature being 72, warming one of the coils between the fingers and applying it suddenly to the disc of bismuth lying on the other, the needle was deflected in 1 second 7 degrees, in 2 seconds 28 degrees, in 3 seconds 60 degrees, and in 4 seconds 100 degrees.

MISCELLANIES.

FOREIGN AND DOMESTIC.

The following notices are translated from Berzelius's last yearly report on the progress of science; by Dr. Lewis Feuchtwanger of New York.

1. *A new ore of Antimony* has been discovered in the Hartz mountains by Zinkin, which resembles zinkenite and appears to be also a subantimonio-sulphuret of sulphuret of lead with sulphuret of silver.

2. *A new Tenantite* has been examined by Hemming from a newly opened mine in Cornwall which consists of Arsenic, 11.5

Copper,	48.4
Iron,	14.2
Sulphur,	21.8
Silex and Gangue,	5.

3. *Vanadiate of lead.*—Johnson discovered some species of vanadiate of oxide of lead at Wanlockhead in Scotland, one species occurs on calamine in form of warts and as large as a pin's head. It is of a dirty white and appears to be dew of a pale red powder, has a resinous fracture and specific gravity of about 7. The other is black and looks like earthy manganese; a third has not yet been described, but Johnston has sent specimens to the Collection of the *Royal* Swedish Academy; it is regularly crystallized and appears to be a bivanadiate of the oxide of lead.

4. *Plumbacalcite* has been described by Johnson as a mineral found at Wanlockhead in Scotland; it consists of carbonate of lime and carbonate of lead; it is crystallized in the primative rhomboid of the calcareous spar; it occurs both transparent and opaque and consists of 92.2 carbonate lime, 7.8 carbonate of lead. By heat the carbonic acid is evolved, and the mineral assumes a reddish color. This mineral offers an interesting proof of the isomorphism of oxide of lead with a rhomboid of carbonate of lime.

5. *Pelokonite.*—Richter describes an uncrystallized mineral, occurring in Chili, with malachite and copper-green under the above name, (from πελος brown and κόνις powder). It contains phosporic acid and the oxides of iron, manganese and copper; it has a blackish blue

color gives a liver brown streak of a conchoidal fracture and weak lusture; its spec. grav. is between 2.5 and 2.57.

6. *Wolchouskoit.*—Kammerer described a new mineral from Siberia which is amorphous, bluish green opake, and of conchoidal fracture; its touch is greasy, gives a bluish green streak and adheres slightly to the tongue; consists of silica, alumina, oxide of chrome and water.

7. *Wörthite.*—This new mineral has been described by *Hess* and found in a scapolitic boulder from the neighborhood of Petersburg. It is colorless, crystalline foliated, of specific gravity of about 3.0; harder than quartz, infusible before the blow-pipe and dissolves with difficulty and with effervescence; in soda it becomes transparent and yields water by heating in a tube; it becomes dark blue with nitrate of cobalt. Hess found it composed of Silica, 40.58

Alumina, - - - - - - - -	53.80
Magnesia, - - - - - - -	1.00
Water, - - - - - - - - -	4.63

The formula is $AAq+SS$.

8. *Pyrargillite.*—Nordenskjöld discovered and analyzed two new minerals from Finland, the one he calls by the above name from its character of diffusing a clayey smell by heat. It is partly black, light and lustrous like the Lordawalite and partly bluish granular or red and without lustre; it seldom occurs pure in uncrystallized masses, the form of which approaches to a four sided prism with truncated angles sometimes it is traversed with chlorite so as to appear sparkling by polishing; its specific gravity is 2.505; its hardness 3.5; it is decomposed completely by muriatic acid; it occurs in granite and it consists of

Silica, - - -	43.93
Alumina, - -	28.93
Protoxide of Iron, -	5.30
Magnesia with some Protoxide of Manganese,	2.90
Potassa, - - -	1.05
Soda, - - -	1.85
Water, - - -	15.47
Loss, - - -	0.58

Formula. $\left.\begin{matrix}F\\M\\Mg\\N\\K\end{matrix}\right\}S^3+4AS+Aq$

9. *Amphodellite.*—This is the other mineral discovered by the above author from the lime quarries of Lozo in Finland, its crystallized form bears much analogy to that of felspar; it is clear reddish, re-

sembles in fracture scapolite with two passages which form an angle of 94° 9′ its hardness is 4.5 and its specific gravity 2.793; it consists of

Its Formula, $\left.\begin{matrix}C\\F\\Mg\end{matrix}\right\} S+3AS.$

Silica,	45.80
Alumina,	35.45
Lime,	10.15
Magnesia,	5.05
Protoxide of Iron,	1.70
Moisture and Loss,	1.85

There are a few more minerals described which seem to require a thorough examination in order to establish their claim to be called new, as for instance *Skugisan* and *Monophan* belonging to the family of the zeolites and noticed by Breithaupt.

The Mengite from Siberia, and the Monticellite from Vesuvius and noticed by Brooke and Berzelius from Galloro near Rome, a kind of anhydrous zeolite which is difficult to fuse and becomes gelatinous with muriatic acid; it is noticed by Necker De Saussure.

In regard to the Xanthite, described in a former number of this Journal which occurs in Orange County, N. Y. which consists of

Silica,	32.708
Lime,	36.308
Alumina,	12.280
Oxide of Iron,	12.000
Protoxide of Manganese,	3.680
Moisture,	0.600
Loss,	24.24

Berzelius observes that if this analysis approaches to accuracy and if a small part of the oxide of Iron is contained in the mineral as a protoxide the formula would be $2\left\{\begin{matrix}C\\Mg\\F\end{matrix}\right\}S+\frac{A^2}{F^2}S.$

10. *Ozokerite, a new combustible mineral.*—This mineral occurs at Slauik Moldavian District near the Karparthes and has been called by Glooker, Ozokerite (ὀζεῖν to smell, κηρος wax,) it is of a talcose structure, the color between green and brown, of the spec. grav., 0·955 to 0·970, it may be kneaded between the fingers, melts into a clear mass in the flame of a candle, is soluble neither in alcohol nor water, even when boiling, and but slowly so in ether and spirits of turpentine.

This mineral may serve as an excellent material for lamps or tapers burning like wax with soft clear flame and diffusing on its being extinguished an agreeable odor. Specimens are shortly expected from the locality.

11. *Platinum in France.*—A specimen of platinum has been exhibited before the academy at Paris, which has been extracted in combination with silver from galena and which contains 0·00022 of Platinum and since 1100*lbs.* of the galena, are daily produced from the mines, the daily produce of platinum will be 1lb. 4 oz. 4 drams and 28 grains. The mines of Cohfolens and Alloue Depart. of Choraute are the localities for this platinum.

12. *Carrageen or Irish Moss.*

Communicated by Dr. Lewis Feuchtwanger.

Chondrus crispus, Lyngbye, Hydropt. Dan. p. 15, t. 4. Greville, Algæ Brit. p. 129, t. 15.

Sphaerococcus crispus, Agardh, Sp. Alg. 1. p. 256.

Fucus crispus, Lin. Syst. Nat. ii. p. 718. Turn. Hist. Fuc. p. 216—217.

This moss is common on rocks and stones, along the coast of Europe; it is also a native of the United States. A very variable species, but easily recognized, when the eye is accustomed to it. The genus Chondrus, belongs to the order Florideae of the great natural family of the Algae. All the species have a cartilaginous frond, which is flat, without nerves, dichotomous, dilated at the extremity, and of a livid reddish color; the fructification consists of scattered capsules, mostly immersed in the frond, rarely pedicellate; seeds minute, rounded.*

This moss abounding on the southern and western coasts of Ireland, has been used by house painters for sizing; it has likewise been highly esteemed by the inhabitants, as a dietetic remedy for various diseases; more especially for consumption, dysentery, ricketts, scrofula and affections of the kidneys and bladder. Dissolved by being boiled in water, a thick jelly is produced, more pure and agreeable, than that procured from any other vegetable, which is found to agree better with the stomach, than any prepared from animal substances. Its chemical composition appears to me, as far as I have been able to trace it, of very considerable importance, the jelly formed by dissolving it in hot water is not only composed of starch but contains a large

* I am indebted to Dr. John Torrey, for the communication of the botanical description.

proportion of pectic acid a considerable quantity of sulphur, and some chlorine and bromine and another acid combined with lime have been discovered, the latter proves to be the oxalic acid.

Neither the fungic nor boletic nor lichenic acids could be detected and the existence of iodine I have not been able as yet to detect. By extracting the pectic acid with caustic potassa, I found the moss taken up and altogether dissolved and after treating the gelatinous mass with chloride of calcium, muriatic acid and applying alcohol to separate the acid, at least 0·6 of this last was the result.

By reducing the moss to coal and dissolving it in water, sulphuretted hydrogen gas was evolved very abundantly; protoxide of iron, subcarbonate of potassa, diluted sulphuric acid and lime water gave copious precipitates.

The chemical characters of this moss are too interesting to be considered as completely determined by the few superficial experiments, undertaken to discover its properties, especially as they were made at a time when I have been continually interrupted by an attention to the duties of my profession, and I consider them as having been undertaken more for the satisfaction of my curiosity, than as tending to a complete and scientific investigation of such an invaluable medical substance as carrageen, but I hope to be able very shortly to develop with more accuracy the entire composition of this singular moss.

The carrageen seems to possess qualities similar to the Iceland moss, which according to Berzelius' last analysis, (a masterpiece in every respect,) consists in 100 parts of 3.6 syrup, 1·9 bitartrate of potassa, tartrate and some phosphate of lime, 3·0 bitter principle, 1·6 green wax, 3·7 gum, 7·0 coloring matter, like extract, 44·6 lichen starch, 36·2 starch-like matter; but carrageen is without bitter principle, contains nothing but soluble matter and the quantity of nutritious jelly, produced by a small portion of it, gives it the most indisputable preference.—It was first introduced by Dr. Reece, who considered it a most important article of food for invalids, and Doctors Sulby, O'Reilly and Sir Henry Halford, speak of the carrageen as the most nutritious article of diet for invalids they are acquainted with as well as a light nutritious food for delicate and weakly children. In this respect, it is certainly superior to arrow root, sago &c. being more highly nutritious, easy of digestion, and pleasing to the taste.

Prepared with warm milk and sweetened, it is most particularly recommended as a breakfast for consumptive patients.

Decoction of moss, made by boiling half an ounce clear moss in a pint and a half of water or milk until reduced to a pint, is recommended as food for children affected with scrofulous or rickety diseases, for such as are delicate and weakly and for infants.

There are some printed direction for the manner of using the carrageen moss for medicinal, and culinary purposes, accompanying some imported from England, from which I shall make here an extract.

Directions for using the Moss Medicinally.—Steep a quarter of an ounce of Moss in cold water for a few minutes—then withdraw it, (shaking the water out of each sprig) and boil it in a quart of new or unskimmed milk, until it attains the consistence of warm jelly—strain, and sweeten it to the taste with white sugar or honey, or if convenient, with candied Eryngo Root; should milk disagree with the stomach, the same portion of water may be used instead. The decoction made with milk is recommended for breakfast for consumptive patients; and with water will be found a most agreeable kind of nourishment, taken at intervals during the day, the flavor being varied with lemon-juice or peel, Seville orange-juice, cinnamon, or wine, of any sort most congenial to the taste.

The decoction in water is also taken for the relief of cough, at any time in the course of the day, when it is troublesome, and it is, for this purpose, simply sweetened with honey.

In Dysentery, the decoction either in milk or water, may be administered with equal advantage, and in addition to the sweetening matter, if a tea-spoonful of the Tincture of Rhatany be mixed with each cupful of it, tone will thereby be given to the intestines, at the same time that nourishment will be conveyed to the system, and irritation prevented—a large tea-cupful of the decoction may be taken three or four times a day.

As a pleasant strengthening food, boiled with milk and strained with the addition of a little sugar, it is unrivalled for infants. Persons take it in this way for breakfast or supper, with the happiest effect, who are sustaining an attack of the cholera.

Culinary Directions.—To make Blanche-Mange :—take half an ounce of Moss, and having cleansed it by the process above described, boil it in a pint and half new milk, until it is reduced to a proper thickness to retain its shape; to be sweetened and flavored with lemon, white wine, or any thing to suit the palate.

To make Orange Lemon or Savory jellies :—use a similar process, substituting water for milk—add lemon, orange, herbs, &c. according to taste.

To make white soup:—dissolve in water, afterwards add the usual ingredients.

It only remains to state, that the Carrageen or Irish Moss, as a domestic article is peculiarly interesting; it is the best thickener of milk, broths &c. makes excellent jellies, and for Blanch-Mange is equal to most expensive ingredients, whilst the cost is comparatively nothing; it may be used instead of isinglass, jellies, &c.

13. *Oil of Copaiva as a test for the purity of the sulphuric ether.*—If ether is not fully deprived of water and alcohol, it forms when united with oil of copaiva an emulsion without dissolving it completely, whereas it is soluble, when pure, in every proportion.

14. *Discharge of the stain of indelible ink, by corrosive sublimate.*—Dr. John Dickson, of South Carolina, in a letter to the editor, dated October 24, 1833, states that Mrs. Dickson, had accidentally observed the discharge by corrosive sublimate from a handkerchief of the color produced by the nitrate of silver and it was soon found that the same effect was produced upon linen, cotton and the human skin. On the cloth* the stain partially reappeared after washing, but on the skin it did not return.

The following circumstances led to Mrs. Dickson's observation,—a weak solution of nitrate of silver, (warmly recommended in Eberle's Practice,) was used as a lotion in treating some cases of erysipelas; its power of producing a dark tint was well known to Dr. D. but as no caution is given in the above work, it was presumed that no such consequence would follow from using so weak a solution: but Dr. D. had the mortification to find the effect produced and a solution of corrosive sublimate was then successfully applied to remove the stain produced by nitrate of silver. The following suggestions are made by Dr. D.

1. Not to use the nitrate on the face at all, especially in the case of females.

2. To take care that the solution reach only the portions of the surface that are in a state of erysipelatous inflammation, since there is reason to believe that it is less likely to stain the inflamed part, and at any rate, vesication, desquamation or absorption will soon remove the skin and stain together in the diseased surface, while this will not

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* It is not specified what cloth is intended but it is presumed that it is the fibre of the linen and cotton.

happen so soon in the healthy portion. Of course, this is not of so much importance in parts habitually covered by the dress.

3. To have the parts to which the nitrate is applied protected from the light, during the use of it.

4. To wash away the solution before it dries in the least from the fingers &c. dipped in it in making the application. This may be done in common clean water, and if not effectually done in that, it may be certainly done in a weak solution of corrosive sublimate, as appears from our experience.

15. *Dissection of the eye of the Halibut* (Pleuronectes Hyppoglossus, L.) *by* W. C. Wallace, *one of the Physicians to the Northern Dispensary, N. Y.*—The eye is oblong. It is every where surrounded by a firm elastic cartilage excepting the cornea and a short space around the entrance of the optic nerve that is occupied by a fibrous covering. Beneath this cartilage there is a considerable quantity of a glairy watery fluid, between it and the next coat, which has a silvery appearance. Within the cartilage, around the entrance of the optic nerve, the ophthalmic artery ramifies into a great number of branches: these pass into a red spongy body nearly surrounding the nerve and at a distance of from two to six lines from it. From this arises a plexus of very minute red vessels which proceed to the circumference of the iris. Some of them go to the completely circular lens and enter at one point on the inner and at another on the outer side, so that it is poised like a terrestrial globe on its axis. A nerve of considerable size enters with the optic nerve, proceeds along the choroid coat on the inner side to the iris where it divides into three branches, two of which go to the iris, while the most considerable passes to the lens at its internal axis. The lining membrane of the cornea is reflected over the iris except at its inferior internal part, where there are different attachments leaving sufficient space for communication between the anterior chamber and the cavity between the cartilage and the eye-ball, containing what may be termed the aqueous humor.

When the cartilage is compressed, the aqueous humor finds its way into the anterior chamber and the cornea becomes more convex. When the pressure is removed, the cartilage by its own elasticity resumes its shape and the cornea by external pressure is flattened. Some of the muscles act upon the transparent cuticle that covers the cornea, and in this way it may be more accurately adjusted.

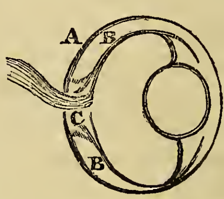
As there is no appearance of ciliary processes on the anterior edge of the choroid coat and as in some fishes the spongy body surrounding the nerve very much resembles them when bruised and separated by the fingers, there is reason to believe that it performs a similar office. One use of the ciliary processes seems to be to separate fibrine and other matters from the blood and thus to prepare it for the formation of the humors. As the lens in fishes is large in proportion to the size of the organ and very dense towards the center, the size of the apparatus to prepare its nourishment appears also to be large and if placed at the anterior edge of the choroid as in terrestrial animals it would be disadvantageously situated.

Transverse section.

A. Cartilage.

B. Aqueous humor.

C. Spongy body supposed analogous to ciliary processes.



16. *Discovery of a Muscle in the Eye of Fishes.*

Extract of a letter from W. C. Wallace, to the Editor, dated 386 Hudson St., New York, 16th June, 1834.

Sir—I beg leave to announce to you the discovery of a muscle in the eyes of fishes, solving the problem of the accommodation of their eyes to distances in a more satisfactory way, than by a greater or less degree of convexity of the cornea. The muscle is triangular, apparently attached to a nerve at one extremity. Another extremity is attached to the capsule of the lens at its axis. The third passes through a loop in the iris, and is attached to the vitreous humor. The lens is drawn backwards, when the portion attached to it contracts. When the portion that passes through the loop, is called into action, the vitreous humor is pulled forwards pushing the lens before it. This structure exists in the streaked bass, the sheep head, the blue fish, the sea-bass and the poigee, are the only fishes, in which I have had an opportunity of looking for it. If it be not too late, please to announce this discovery, which I believe to be new. I shall forward a more particular account, with drawings by an artist before the publication of the October number.

17. *Abstract of Meteorological Observations, made at Middletown, Monmouth, Co., N. J.*, Lat. 40° 26′ N., Long. 73° 59′ W., from May 31st, 1832, to June 1st, 1834; by John F. Jenkins, Principal of the Middletown Academy.

1832, 1833, and 1834. *Months.*	THERMOMETER. Mean temperature at sun rise.	Mean temperature at 2 P. M.	Mean temperature at 9 P. M.	Aggregate of mean temp.	Maximum of temp. Day.	Maximum of temp. Hour.	Maximum of temp. Highest deg.	Minimum of temp. Day.	Minimum of temp. Hour.	Minimum of temp. Lowest deg.	Range of therm'r.	WEATHER. No. of days of diff. vari'n. Fair.	Cloudy.	Rainy.	Snow & hail.	WINDS. N.	N. E.	E.	S. E.	S	S. W.	W.	N. W.	MISCELLANEOUS. Thunder.	Showers.	Aurora Borealis.	Inches of snow.	Inches of rain.
June,	64.6	73.5	67.5	68.5	16	4 P.M.	88	4	5 A.M.	54	34	25	5	2	—	3	2	3	5	1	8	4	4	3	4	—	—	1.16
July,	67.7	77.3	71.4	72.1	7	2 "	90	24	4½ "	61	29	21	10	1	—	3	2	0	5	2	9	5	5	5	9	—	—	3.55
August,	69.4	76.8	72.	72.7	15	2 "	86	26	5½ "	54	32	20	11	5	—	1	4	0	4	4	12	3	3	9	9	—	—	6.77
Sept.	61.5	70.	64.7	65.4	16	3 "	79	14	6 "	49	30	20	10	2	—	2	3	0	4	5	4	6	6	1	5	—	—	2.86
October,	54.2	62.1	56.2	57.5	10	2 "	77	29	7 "	38	39	24	7	3	—	4	5	3	0	3	9	3	4	—	6	—	—	2.75
Nov.	46.3	53.1	49.3	49.6	18	2 "	67	16	7 "	31	36	17	13	3	—	2	2	2	0	3	6	10	5	—	2	—	—	3.84
Dec.	37.5	43.5	39.	40.	31	3 "	55	23	7½ "	16	39	15	16	7	5	3	5	2	2	0	6	6	7	—	0	—	—	6.04
January,	35.	42.7	35.6	37.7	5	2 "	66	11	7½ "	14	52	19	12	2	5	1	4	2	1	3	6	8	6	—	3	—	2.2	2.97
Feb.	31.	41.1	33.6	35.2	24	3 "	60	25	6½ "	18	42	20	8	1	5	3	3	3	1	1	6	5	6	—	1	—	16.5	0.80
March,	35.	48.	37.1	40.	21	11 A.M.	76	3	6½ "	8	68	22	9	1	2	1	3	1	5	4	3	5	9	—	1	1	10.5	1.66
April,	48.1	64.2	52.4	54.9	30	2 P.M.	88	15	9 P.M.	37	51	24	6	0	—	4	3	2	5	2	6	2	6	1	5	—	—	1.65
May,	61.2	73.3	61.9	65.4	1	2 "	89	4	5 A.M.	50	39	16	15	3	—	1	5	5	6	5	3	3	3	4	9	1	—	5.37
Agg. tem.	50.9	60.4	53.4	54.9	*Recapitulation.*							243	122	30	17	28	41	23	38	33	78	60	64	23	54	2	29.2	39.42
June,	60.8	74.9	62.4	66.	15	2 P.M.	88	11	9 P.M.	50	38	22	8	3	—	3	3	0	7	4	3	4	6	2	5	—	—	4.10
July,	68.9	78.5	72.2	73.2	22	4 "	90	10	4½ A.M.	62	28	26	5	1	—	0	2	0	5	2	6	8	8	8	9	1	—	3.33
August,	66.	75.8	69.3	68.1	27	2 "	84	30	5½ "	52	32	25	6	2	—	4	3	3	5	2	4	4	6	5	5	—	—	6.31
Sept.	62.3	71.5	66.1	66.6	20	2 "	85	14	6 "	48	37	22	8	1	—	2	2	2	2	3	5	7	7	2	4	—	—	2.22
October,	52.	60.	54.7	55.6	17	2 "	74	31	9 P.M.	29	45	16	15	5	1	3	3	2	3	3	3	7	7	—	2	—	—	8.36
Nov.	37.	49.2	41.5	42.6	9	2 "	70	1	6½ A.M.	22	48	22	8	3	2	2	3	1	1	1	9	8	5	—	3	1	—	1.30
Dec.	33.8	40.7	36.3	36.9	8	9 "	50	21	7½ "	20	30	13	18	3	2	4	12	1	1	0	2	2	9	—	7	—	7.2	3.85
January,	27.6	36.6	29.3	31.2	18	2 "	62	5	9 P.M.	6	56	17	14	2	5	2	6	2	0	0	6	7	8	—	4	—	4.4	1.65
Feb.	36.2	46.7	39.6	40.8	15	2 "	70	13	7 A.M.	18	52	16	12	2	1	1	9	1	1	2	6	4	4	1	3	—	.1	1.95
March,	38.6	50.1	41.	43.2	20	1 "	76	22	6 "	23	53	19	12	1	—	0	2	1	6	3	7	6	6	2	4	1	5.0	1.85
April,	49.5	57.6	52.1	53.1	17	3 "	82	26	5 "	37	45	15	15	3	3	2	1	7	7	2	1	3	7	2	8	—	—	2.56
May,	56.1	64.6	59.4	60.	20	1 "	80	15	5 "	38	42	22	9	4	—	2	1	6	4	3	6	4	5	5	5	—	—	6.16
Agg. tem.	49.1	58.8	51.9	53.1	*Recapitulation.*							235	130	30	14	25	47	26	42	25	58	64	78	27	59	3	16.7	43.64

The following table presents a comparative view of some of the more important particulars, for three years past, commencing each year with June.

	1831–2.	1832–3.	1833–4.
Mean temp. at sunrise, - -	50°1′	50°9′	49°1′
Do. 2 P. M., - -	57 2	60 4	58 8
Do. 9 P. M., - -	52 5	53 4	51 9
Aggregate, - -	53 3	54 9	53 1
Highest degree, - -	92	90	90
Lowest degree, - -	2	8	6
Whole yearly range, -	90	82	84
Mean monthly range, -	39 1	40 9	42 1
Days Fair, - -	244	243	235
" Cloudy, - -	122	122	130
" Rainy, - -	33	30	30
Thunder, No. of times,	30	23	27
Showers, - - -	61	54	59
Aurora Borealis, -	0	2	3
Inches of snow, - -	22.2	29.2	16.7
" " rain, - -	34.40	39.42	43.64
Deepest snow, - -	3.5	12.	7.
Most rain in one month,	5.21	6.77	8.36
Warmest month, -	Aug.	Aug.	July.
Coldest month, - -	Dec.	Feb.	Jan.
Earliest frost in fall, -	Sep. 18.	Sep. 14.	Sep. 13.
Latest frost in spring, -	May 27.	April 25.	May 15.

A series of observations of the weather as connected with the moon's phases has been made during the last three years, in which were recorded the changes from *rainy* or *cloudy* to *clear*, and vice versa, that occurred within twelve hours, either before or after the change of the moon. The results are, that in 37 lunations, or 148 phases, there were 79 changes of weather, of which 18 were at new moon, 18 first quarter, 23 at full, and 20 last quarter. Rain fell on the day of new moon 11 times, first quarter 8, full 8, and last quarter 13.

In the same period rain, including showers fell on 264 days; of which 126 were in the increase of the moon, and 138 in the decrease. Only 93 of these could be called rainy days, and of them 39 were in the increase, and 54 in the decrease. Also, the quantity of rain in the decrease, was to that in the increase as 7 to 4.

The changes of weather when the moon was in Apogee were 25, and the same when in Perigee. In this case the preceding and following days were included.

It appears then, that it has rained more frequently, and in greater quantity in the wane than in the increase of the moon. This result differs from that of observations made in Germany by Arago; but it confirms the opinion of Toaldo, that the new moon is not the most active of the phases.

The meteoric shower on the 13th of Nov. last, presented appearances similar to those which have been often described, except that no particular point of radiation was observed.

Corn and garden vegetables were generally cut off by the frost on the 15th ult.

18. *Recent Scientific Publications in the United States.*—New Fresh Water Shells of the United States with colored illustrations; and a Monograph of the genus Anculotus of Say; also a Synopsis of the American Naiades. By T. A. Conrad, Memb. Acad. Nat. Sci. of Phil. 12mo. Plates. Philadelphia, Judah Dobson.

Boston Journal of Natural History, containing Papers and Communications read to the Boston Society of Natural History, and published by their direction. Part I. No. I. Boston, Hilliard, Gray & Co. 8vo.

Contents.—

Report of the Managers of the Franklin Institute, of the State of Pennsylvania, for the promotion of the Mechanic Arts, in relation to Weights and Measures. Presented in compliance with a resolution of the House of Representatives. Philadelphia, printed by Jesper Harding, 8vo. pp. 82.

19. *Meteorological Journal for the month of May*, 1834, *of the Oneida Conference Seminary, Cazenovia, Madison County, N. Y. June*, 1834. Communicated by JOHN JOHNSTON.

Day.	Thermometer.				Winds.		Weather.		Rain Gage.
	Morn.	After'n.	Eve.	Mean.	A. M.	P. M.	A. M.	P. M.	
1	30	50	41	40.33	N.W.	N.W.	Fair.	Fair.	
2	30	60	45	45.83	N.W.	N.W.	Fair.	Fair.	
3	35	60	50	48.17	N.W.	N.W.	Fair.	Fair.	
4	34	64	52	52.00	N.E.	N.E.	Fair.	Fair.	
5	46	46	44	44.33	S.E.	S.E.	Fair.	Rain.	
6	42	67	58	57.00	S.	S.W.	Fair.	Cloudy.	.65
7	50	48	46	47.50	N.W.	N.W.	Rain.	Rain.	1.04
8	47	56	46	49.00	N.W.	N.W.	Fair.	Cloudy.	.04
9	43	58	50	49.83	N.W.	N.W.	Cloudy.	Cloudy.	
10	40	51	43	44.83	N.W.	N.W.	Fair.	Fair.	.10
11	41	64	53	52.17	S.	S.W.	Fair.	Cloudy.	.04
12	38	38	35	35.50	W.	W.	Cloudy.	Rain.	.13
13	29	31	36	32.50	N.W.	N.W.	Snow.	Cloudy.	.04
14	32	46	26	33.17	N.W.	N.W.	Cloudy.	Cloudy.	.05
15	23	34	30	29.83	N.W.	N.W.	Cloudy.	Cloudy.	
First half month.				44.13					2.09
16	28	48	38	40.00	N.W.	N.W.	Fair.	Fair.	
17	40	58	48	49.00	S.W.	N.W.	Cloudy.	Fair.	
18	42	74	56	59.33	N.W.	N.W.	Fair.	Cloudy.	
19	54	75	56	61.67	W.	W.	Fair.	Cloudy.	
20	54	72	54	60.33	N.W.	N.W.	Fair.	Fair.	
21	56	78	66	67.00	N.W.	N.W.	Fair.	Fair.	
22	58	82	68	70.17	N.W.	N.W.	Fair.	Fair.	
23	63	88	66	72.83	W.	S.W.	Fair.	Fair.	.79
24	66	74	67	68.00	W.	N.W.	Cloudy.	Rain.	.34
25	60	74	65	64.67	N.W.	N.W.	Cloudy.	Cloudy.	
26	50	78	64	64.33	W.	W.	Fair.	Fair.	
27	52	83	66	68.67	S.E.	E.	Fair.	Fair.	
28	62	75	64	65.67	S.E.	S.E.	Cloudy.	Cloudy.	
29	54	58	55	55.33	S.E.	S.E.	Rain.	Cloudy.	.16
30	52	80	68	68.00	S.	S.	Fair.	Fair.	
31	60	69	67	65.33	S.W.	S.W.	Rain.	Cloudy.	.04
Ju. 1	60								
Second half month.				62.10					1.33
Monthly mean.				53.11					3.42

Winds.—N. 0 days; N. E. 1; E. 0.5; S. E. 3.5; S. 2; S. W. 3; W. 4; N. W. 17.

Weather.—Fair 16.5 days; cloudy 14.5; rain 3·5; snow 0,5. Prevailing wind N. W.; rain 3.42 inches.

Warmest day 23d; coldest 15th.

Highest degree 88; lowest 23, range 65.

7th. Primrose in blossom. 8th. Dandelion and plumtree in blossom. 9th. Cherry and shadbush in blossom. 12th. Snow squalls; 13th. and 14th ground covered with snow most of the time, —at times more than one inch in depth.—Surface of the earth much frozen.

20th. Apples in blossom.

23d. Violent wind with thunder and lightning at 5 o'clock in the P. M.

20. *Influence of Electricity on Capillary Attraction.*

Extract of a letter from Jno. W. Draper to the editor, dated Christiansville, Mecklenburg, Va., May 31st, 1832.

To Prof. Silliman.—*Sir*—Without a personal acquaintance, I write to inform you of some scientific points of interest which have recently been made known in England, but which from the lateness of the discovery have not I believe been published in this country.

You will remember, that Laplace in his Theorie de l'action capillaire in the supplement to the tenth book of the Mecanique Celeste (after shewing how the + or − action of the bounding meniscus of a liquid in a capillary tube determines its position therein) leaves the *nature* of the force of capillary attraction entirely out of the question; the adhesion of plates of glass to the surface of liquids he allows to depend on the same cause. It was in investigating the latter phenomenon that the mystery of all these singular appearances was discovered. I will indicate, as succinctly as I can, the chain of reasoning.

If you place a disc of glass upon the surface of mercury and attempt to lift it, you will immediately be sensible of a strong attraction between them; the value of that attraction is measurable by the balance. Whilst the glass is reposing on the mercury, if the latter is connected with a sensible gold leaf electrometer you will not find the smallest indication of developed electricity. But on separating them by means of a fibre of gum lac fastened to the back of the glass, the gold leaves, in a moment, diverge, and the glass disc is found to be electrified, oppositely, to an equal amount—place them in contact and all electrical signs vanish.

That this adhesion which has been hitherto ascribed to capillary attraction is due to electricity may be proved thus. It has been found that the force with which glass, gum lac, sealing wax, sulphur, &c. &c. adhere to the surface of mercury is directly proportional to the quantity of electricity which they develope, as measured by the torsion balance.

If we are to attribute capillary action to electricity, it would follow of course, that the position of liquids in tubes ought to be deranged by electricity and accordingly it is found, that a voltaic arrangement of half a dozen or ten alternations has as complete a control as it has over the magnetic needle. Nay it will even reverse their natural motion, causing those fluids that sink to rise immediately and those that rise to sink, and if the tube be capable of motion, it immediately moves in an opposite direction.

The details of this theory offer explanations of some interesting chemical facts, such as the decomposition of peroxide of hydrogen and persulphureted hydrogen by metallic peroxides. And from the rise of temperature which it indicates in a membrane undergoing gaseous endosmosis, may perhaps be found some key to the origin of animal heat.

If sir there be any point on which you would wish more particular information it will be a pleasure to me to give it. The original papers are, I believe, to be read before the Royal Society of London, they were forwarded to Mr. Faraday and Dr. Turner for that purpose.*

21. *Safety of Lead Pipes, protected by Tin.*

Extract of a letter from Mr. George Chilton, to the Editor, dated New York, June 23, 1834.

Dear Sir.—Observing in the last No. of the Journal, a notice of Ewbank's *patent* tinned lead pipes, and having had many applications for information concerning the danger attending the use of metal pipes for conveying water, beer, cider, &c.—I have been induced to subject the pipes of Ewbank, to a few trials, for the purpose of ascertaining whether, from the occasional contact of acids, any deleterious solution of lead, would attend their ordinary use. It is well known, that the common beer pump, with a leaden pipe, has frequently given to the liquor, a dangerous impregnation, especially after remaining stag-

* We should like more ample details from our correspondent.—*Ed.*

nant for a time, and the beer in a sour state. The substitution of block-tin would remove the apprehension of danger, but its greater price offers a strong temptation to the use of lead. It appears to me that the lead tube lined with tin, will answer the ends of cheapness, safety and durability. I would therefore, invite your attention to the following experiments, which if you think them of any importance to the public, you may insert in your Journal.

Experiments.—Various portions of lead tube coated some witn pure tin, and others with different alloys of tin, and lead were bent into the form of a semi-circle, and filled with vinegar of different degree of strength. After standing, some a month, and others six weeks, with occasional disturbance, the clear solutions were tested, first with sulphate of soda and afterwards with bi-hydro-sulphuret of ammonia. The application of the first of these tests, viz. sal. soda produced no alteration in any of the solutions; from which it must be inferred that they contained no lead.

The application of the second test produced, as was anticipated, a brown precipitate of sulphuret of tin. In the same manner, two fresh pieces of tube were filled with a strong solution of common salt, which remained in contact for some time. The solutions, when assayed with the same tests, shewed that nothing but a little tin was dissolved.

It appears that in all these cases, which I regard as galvanic effects, the tin was the most oxidable metal, although, when not under the influence of polar arrangement and in the open air, lead appears to oxidate sooner than tin. It is scarcely necessary to remind you, that results similar to these were obtained thirty years ago by the celebrated professor Proust at Madrid, who undertook, for the Spanish government, an extensive series of experiments on the different alloys of lead and and tin, with the express view of determining whether the popular prejudice against the coating of copper vessels with an alloy of tin and lead, which is the common practice, was ill, or well founded. Nothing can be more satisfactory than the conclusions he drew from his labors, viz. that as in all his numerous experiments neither lead nor copper were dissolved, there is little reason to fear the solution of lead from the tinning of our kitchen utensils. I may just mention here, that I am in the habit of cleaning out my soda fountain every spring, with dilute muriatic acid, which uniformly dissolves the oxide of tin without touching the copper, which I am persuaded will remain as securely, as the sheathing copper in Sir Humphry Davy's great experiment and for the same reason.

22. *New Comet.*—Professor Schumacher, Astronomer Royal of Denmark, announces in his "Astronomische Nachrichten," of the 7th inst. the discovery of a new Comet on the 8th ult. by Professor Gambart, of the Marseilles Observatory. Although it disappeared on the 13th, and from the state of the weather, and temporary imperfection of his micrometer, his observations were interrupted and imperfect, Prof. Gambart assigns its place on the 10th. at 16 h. 32 m. 45 s. of sidereal time, to 20 h. 9 m. 7 s. of right ascension, and 22° 33′ of south declination. When first seen, it was near the horizon, having a nebulous apearance and situated in the constellation Sagittarius very near the nebula 2064 of Sir John Herschel. The comet was of a pale light color, of a very round form, and with a diameter of about four or five minutes.—*The Athenæum, (London,) May* 17, 1834.

23. *British Association for the Advancement of Science.*

Extract from a letter to the Editor, from Mr. Henry D. Rogers, dated Philadelphia, May 22, 1834.

Sir—I have been requested by the Secretary to the Council of the British Association for the advancement of Science, to insert in your Journal a notice, that the next meeting of that body will be held at Edinburgh, in the week commencing, Monday, September 8th, 1834.

It is understood that this is an invitation to the scientific men of the United States, and such as may find it in their power to attend, may assure themselves of a most cordial welcome, and a rich repast of science, to repay them for the journey.

24. The veteran German chemist, Prof. Sigismund Hernbstadt of Berlin, died suddenly about six months ago, after having occupied, for more than thirty years, the first chair of theoretical chemistry. He had acquired deserved celebrity by making very great improvements in almost every branch of practical chemistry.

Circumstances, beyond our control, have obliged us to omit the greater portion of the miscellany, for the present number.—*Editor.*

INDEX TO VOLUME XXVI.

The following notices of new works were omitted in their proper place.

Essays on some of the most important articles of the Materia Medica, comprising a full account of all the new proximate principles, and the popular medicines lately introcuced in practice, detailing the formulas for their preparation, their habitudes and peculiarities, doses and modes of administration; with remarks on the most eligible form of their exhibition: to which is added, a catalogue of medicines, surgical instruments, &c. &c., adapted for a physician at the outset of his practice, with the doses and effects attached to each medicine, &c. &c. By G. W. Carpenter. Second edition, revised and enlarged; 8vo. pp. 320. Philadelphia, G. W. Carpenter.

Mr. Carpenter's Essays are well known, and the present edition is much improved.

Elements of Geology, for the use of schools. By Wm. W. Mather; 18mo. pp. 139. Norwich, Wm. Lester, Jr.

This is a judicious, correct, and perspicuous work, containing in a small compass, a selection of many of the most important facts and theoretical views in geology; it is well adapted to the object for which it was written.

Mr. Mather has also published a Sketch of the Mineralogy and Geology of New London and Windham counties, in Connecticut, with a map. We cannot doubt that these are executed with Mr. Mather's usual accuracy.

Letters from the Canary Islands, by D. J. Browne, with a lithographic view of the Peak of Teneriffe, and a Map of the Canary and Madeira Islands, and a part of the coast of Africa.

This is an instructive and interesting work, replete with valuable information.

Prof. H. D. Dewhurst, of London, has recently published in one vol. 8vo. with numerous plates the natural history of the Cetaceæ. We have perused this work with much satisfaction; we presume that it is the most complete account of these animals that has been published.

Mr. Dewhurst, has published also synoptical tables of an improved nomenclature for the features of the cranium not only of man, but of the various orders of mammalia; also an improved anatomical arrangement of the regions of the human body.

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TO CORRESPONDENTS.

The titles of pieces and persons, must be fully given, or the Editor will not be responsible for mistakes or omissions.

Several communications have been unavoidably postponed, for want of room. Most of them will appear in the next No.