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AND

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ERRATA.

Page 6, l. 14 from top, for "1847," read "1845."—P. 7, l. 7 from top, for "Regent," read "Honorary Member."

P. 76, l. 27 from top, for "produced," read "protected."

P. 53, line 13 from bottom, for "$t = 1, 2, 3$," read "$t = 0, 1, 2, 3$."

P. 136, foot-note 3, insert "om" before Stoffernes.

P. 146, line 18 from bottom, in table, for "92·3," read "72·3."

p. 148, in formula (47), for $\frac{de}{dt}$ read $\frac{dc}{dt}$.

p. 149, l. 19 from bottom, for γ' read γt.

P. 312, foot-note 1, for "this memoir," read "the memoir of which this is an abstract."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

BENJAMIN SILLIMAN.

OUR honored associate, Professor BENJAMIN SILLIMAN, the founder of this Journal, whose name has appeared upon the title page of every number, from the first until the present, is with us no more. He died at his residence in New Haven, early Thursday morning, Nov. 24, 1864 (the day set apart for a national thanksgiving), having reached the age of 85 years.

It becomes our duty to place on record in these pages, as an inscription to the monument which he has himself erected, an outline of his career and a tribute to his memory. Few men enter life with such promise as he; fewer still sustain themselves so evenly, and die so widely lamented.

Instruction in natural science has been his great work; and in it he was emphatically a man of the times. Beginning when almost nothing was known in this country of the departments to which he was especially devoted, he lived to see them carried forward to a high degree of progress, and their importance everywhere acknowledged. His life, which was one of few marked incidents, was passed in his native State, in connection with Yale College, the institution that early selected him as one of its faculty. Two or three times he was invited to become the president of colleges elsewhere, but New Haven continued his chosen home. Twice he visited Europe, first in 1805–6, in order to qualify himself for his work in life by attendance upon lectures in London and Edinburgh, and by observation of foreign institutions of learning; and again, near the close of his life, in 1851, when he was accompanied by his son, and made a more extended tour of observation and inquiry. Frequent journeys in his own country made him acquainted personally with the institutions and the men of every State, while his habits of prompt and friendly correspondence perpetuated the intimacies which he formed at home and abroad.

Without attempting a formal biography (which the late day of his decease renders impossible at this time), we propose to speak briefly of Professor Silliman's career as an officer of Yale College, and as a man of science, and then of his personal character and influence in the community.

The Silliman family has resided in Fairfield, Conn., since the early colonial days. Tradition says that Claudio Sillimandi, their earliest known ancestor, was driven, in 1517, from Lucca, Italy, to Switzerland, by religious persecution. The descendants resided in Berne, and afterward in Geneva, whence they emigrated through Holland to this country about the middle of the seventeenth century. A worthy pastor of the name, living with his family near Neuchatel, was visited by Prof. Silliman in 1851.

Ebenezer Silliman, the grandfather of Benjamin, graduated at Yale College in 1727, and Gold Selleck, the father, in 1752. The latter was a Brigadier General of militia in the Revolution, and was entrusted for a time with the defence of the Long Island coast. In 1775 he was married to Mary, the daughter of Rev. Joseph Fish of Stonington, and the widow of Rev. John Noyes. The two children of this marriage, Gold Selleck and Benjamin, became members of the same class in college, and have maintained through life an intimacy peculiarly fresh and cordial. The younger brother, Benjamin, was born in North Stratford, Conn. (now the town of Trumbull), August 8, 1779. The elder, who was born in 1777, is still living in Brooklyn, N. Y.[1]

Throughout his active life, Professor Silliman has been identified with Yale College. He entered the institution in 1792, graduated in 1796, became a Tutor in 1799, was appointed Professor of Chemistry and Natural History in 1804; and in 1853, having been relieved at his own request from further service as an instructor, he was designated, by the Corporation, Professor *emeritus*. Thus, during a period of nearly three-quarters of a century, his name has appeared as a student and

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[1] Prof. Silliman was twice married: first, in 1809, to Harriet, daughter of the second Gov. Trumbull of Connecticut, the mother of his nine children; and again, in 1851, to Mrs. Sarah Webb, daughter of John McClellan. Five children survive him, one son and four daughters. All are married, the eldest daughter to J. B. Church, the second to Prof. O. P. Hubbard, the third to Prof. J. D. Dana, and the fourth to Rev. E. W. Gilman. His descendants include twenty-three grandchildren, besides five deceased, and two great-grandchildren.

teacher successively on the catalogues of the college. He was a pupil both of Dr. Stiles and Dr. Dwight, and the colleague of the latter during eighteen years. With President Day and Professor Kingsley he was associated for half a century and more in the government of the institution.

In the capacity of a college officer, he was preëminent as a teacher. The professor's chair, in the laboratory or the lecture-room, was the place above all others in which his enthusiasm, his sympathy with youthful aspirations, his varied acquisitions, his acquaintance with the world of Nature and of Art, and his graceful utterance, exerted their highest and most enduring influence. The minds which he aroused to the study of Nature have become investigators and teachers in every portion of the country, and all his pupils, whether devoted to science or to letters, will bear testimony to the interest which he awakened in these pursuits. They will never forget the admirable tact with which the manipulations of the laboratory were performed, or the brilliant experiments in chemistry which the lecturer seemed to enjoy as if, like the class, he had never witnessed them before. The course in chemistry, in early years, extended through one hundred and twenty lectures. In later days it was not so long, but was followed by a course in mineralogy and another in geology. Here, too, Prof. Silliman had the same magnetic influence on his students, sending them off on long walks about New Haven and at home to search for specimens, or to study the phenomena of geology. The third of these annual courses, that on geology, he gave with peculiar zest and eloquence. He delighted to depict the catastrophes of geological history and to clothe the world with the plants and animals of former days.

Professor Silliman was less concerned in the government of the students than some of his associates; but questions were continually arising in which his counsel was of weight. He was prompt in rebuking every form of youthful delinquency, yet was never harsh or inconsiderate. No student ever left his presence feeling wronged or indignant. He would much rather sacrifice a rule than injure an offender. If he seemed sometimes to be lenient, it was the leniency of a father, for his mind regarded the improvement of his scholars rather than the enforcement of routine and discipline. His paternal lectures to the Freshman class on morals and manners were admirable in their influence, and many a graduate of the college will acknowledge that his habits for life were affected by the judicious hints which he received from his kind and sympathising teacher.

Mr. Silliman's labors began with instruction; but they did not end there. His active and versatile disposition led him to become interested in and to help forward whatever would contribute to the welfare of Yale College. When he went abroad, in 1805, to fit himself for the duties of his professorship, the purchase of books for the library was one of the duties with which he was especially charged. He was one of the library committee until his retirement. In his own departments, not only the Chemical Laboratory, but also the Cabinet of Minerals owed its existence to his energy. This collection is indeed so important, that something more than the mere mention of it seems due. About the time when Mr. Silliman was appointed a professor, the entire mineralogical and geological collection of Yale College was transported to Philadelphia in one small box, that the specimens might be named by Dr. Adam Seybert, then fresh from Werner's School at Freiberg, the only man in this country who could be regarded as a mineralogist scientifically trained. From this small beginning grew the present cabinet. In 1810, owing to personal regard for Prof. Silliman, Col. George Gibbs deposited with Yale College his valuable collection of minerals, and, after it had remained open to the public fifteen years, various friends of the college, chiefly through the instrumentality of Prof. Silliman, subscribed for its purchase the sum of $20,000. Other important accessions were also secured through his influence, not only from college graduates and other American gentlemen, but from various foreign collectors.

The Clark telescope is another of the donations to Yale College due to Prof. Silliman. This excellent glass, the best in the country at the time of its purchase, was the means of exciting among the students of the college unusual attention to astronomical pursuits for many years after its reception. The liberal donor, a farmer near New Haven, by this and other more important gifts, placed himself foremost among all the benefactors of the college up to that time, and Mr. Silliman was the medium through whom his benefactions were bestowed. The Trumbull Gallery of Paintings, a collection of priceless value, not only as works of art but also as illustrations of American history and biography, was secured to the college through the same enlightened instrumentality.[2] The Medical Institution of Yale College and the Sheffield School of Science, important branches of the University, were both greatly aided in their beginnings by the influential exertions put forth by Professor Silliman. He was one of the chief founders of the Alumni association of the college, and at their anniversaries and on other occasions, he was, as another has said, "the standing 'orator' of the college,

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[2] It is an interesting fact that as early as 1842, Prof. Silliman, in his Alumni address, pointed out the need of another edifice for the Fine Arts, or an extension of the Trumbull Gallery, at no distant day. This want is about to be supplied by the liberality of an alumnus of the college. Prof. Silliman was prevented by illness, a week before his decease, from taking a public part in the exercises of laying the corner stone of the proposed structure.

the principal medium between those who dwelt in the academic shade and the great public." Not unfrequently he was the college solicitor, asking funds for the expansion of the institution, and never asking in vain.

Although his services as a college officer were great, Prof. Silliman's strongest claim to the gratitude of men of science rests upon the establishment, and the maintenance, often under very discouraging circumstances, of the "American Journal of Science." The history of this undertaking has already been given, in his own words, in the introduction to the 50th or Index volume of the First series of the Journal; and it is for others, rather than for us, to give an estimate of his editorial services. It is but just, however, to call attention to a few circumstances, which all will regard as creditable to its founder.

He had the sagacity to foresee, as long ago as 1818, the scope which such a magazine should take. The prospectus which he then wrote is applicable almost exactly to our pages to-day. Experience has established the wisdom of the course which he marked out.

He maintained the Journal, from the beginning, at his own pecuniary risk. Its publication has often been a serious financial burden, and in its most prosperous days has not yielded a fair return for editorial labor. But it has been continued, at this personal inconvenience, for the sake of American science, that the labors of our countrymen might be made known abroad, and the labors of Europeans understood in this country.

The Journal has never been used for the benefit of any party or individual, but solely for the advancement and diffusion of scientific truth. Its pages have been always open to free scientific discussion, with truth as the single end in view.

The original investigations of Prof. Silliman are not numerous. In the early part of his career he began with energy some important experiments and researches. He undertook a geological survey of Connecticut; he published a paper in conjunction with Prof. Kingsley on the famous Weston meteorite; he applied the newly invented blowpipe of his friend, Dr. Hare, to the fusion of a variety of bodies, which were before regarded as infusible; he demonstrated in the galvanic battery the transfer of particles of carbon from one charcoal point to the other; he made scientific examinations of various localities interesting in their geological or mineralogical aspects. But he was too much needed elsewhere to be allowed to remain a close student in the laboratory, or to engage with constancy as an explorer in the field of geological research. He has probably been a more useful man in the wider spheres of influence to which he was called than he could have been in a life devoted to scientific investigation.

During a considerable part of his life, he was one of the few men in the country who could hold a popular audience with a lecture on science. The public early knew of his capabilities, and for many years he yielded to invitations from various parts of the country to deliver lectures on Geology and Chemistry. In 1833 he gave his first popular course on Geology at New Haven, which was repeated in 1834 at Hartford and Lowell, and in 1835 at Boston and Salem. At Boston, the audience desiring to attend was so much larger than the largest hall would hold, that each lecture was given twice for the accommodation of the public. From 1840 to 1843 inclusive, he gave four successive courses of the "Lowell Lectures" in Boston. Besides various other engagements in the Northern and Eastern states, he went in 1847 by invitation to New Orleans, and on his way appeared before crowded audiences in other cities of the South; and five years after the resignation of his professorship in college, when he had passed his 75th year, he made the long journey to St. Louis, in obedience to a call for a course of lectures from the citizens of that place.

In lecturing, his language was simple—his flow of words easy, generous and appropriate—his style animated, abounding in lifelike and well-adorned description, often eloquent, and sometimes varied with anecdote running occasionally into wide digressions. His manner was natural, and every feature spoke as well as his mouth; his noble countenance and commanding figure (he was nearly six feet in height, with a well-built frame) often called forth, as he entered the lecture hall, the involuntary applause of his audience.

In his popular courses he often lectured on the subject of "Geology and Genesis," and as he was widely known not only as a man of science, but as a sincere believer in the sacred Scriptures, he greatly aided in removing from the religious world the apprehension that science and religion were hostile in their teachings.

Mr. Silliman found great pleasure in helping forward other men of science. He rejoiced heartily in their progress; his house and his laboratory were always open to receive them, and if a friendly word or letter from him could advance their interests, he was ever ready to bestow it. He also felt a deep concern for the advancement of scientific investigations in every part of the country, and whenever, in halls of legislation, or before the public, the name of Benjamin Silliman would advance a useful project, it was not withheld. In more than one instance, the foreigner, or the exile, remembers his kindness with almost filial devotion.

Prof. Silliman's scientific publications, apart from his contributions to this journal, were chiefly text-books. He edited

Henry's Chemistry and Bakewell's Geology, for the use of his pupils, and also published a work on Chemistry, in two volumes.

His long labors for science brought him honors from all parts of the world. His name is on the roll of several of the principal scientific Academies or Societies of Europe, and of those of his own country. He was one of the original members of the National Academy of Sciences, and a Regent of the Smithsonian Institution.

Aside from Professor Silliman's influence as an officer of Yale College, and as a well known man of science, his personal hold upon the community at large was remarkably strong. This was due somewhat to the favor with which his popular lectures were received, and to the wide circuit over which he had journeyed. It was also owing in part to the pleasure and instruction which were afforded by his books of travel. Twice, as we have stated, Professor Silliman visited Europe, the interval between his journeys being nearly fifty years. Both these visits led to the publication of his observations in volumes which were widely read. The narrative of his earlier journey especially was received by the public with great delight. Few Americans then went abroad; and hardly any had published narratives of what they had seen. Mr. Silliman's volumes were fascinating to young and old,—and many were the testimonials which he received of the interest thus awakened in European institutions and manners. His Journal of a Tour to Canada was another contribution to the literature of the day.

But the general influence of Mr. Silliman must be attributed to his personal character rather than to any of what may be termed the accidental circumstances of his life. He was a man of vigorous understanding and sound judgment, led on, but never carried away, by an enthusiastic disposition, glowing and constant. With this was associated sterling integrity, which never harbored a selfish or dishonorable purpose, but rejoiced in doing and encouraging whatever was right. Every one could trust him. These fundamental traits were adorned by the outward qualities of affability and courtesy, or rather were expressed in manners at once so dignified and so kind that all with whom he came in contact were charmed at once, and on closer intercourse were bound to him as friends for life. Such friendships he never neglected or forgot. Even the sons and the grandsons of his early associates inherited a share in the regard which he had bestowed upon their parents. Blending with and ennobling all these virtues, was the child-like simplicity of his Christian faith.

A character like this shines the brighter the nearer it is seen. In his own family circle, Mr. Silliman has moved for years as a patriarch, surrounded by his descendants to the third and fourth generation. The very house which he occupied has become historic, reflecting in its arrangements, its family portraits, its interesting mementos of absent friends, and its long shelves of books, the controlling mind which has dwelt there.

In the neighborhood and town where he resided, Mr. Silliman was peculiarly beloved and respected. "New Haven will not be New Haven without him," said more than one of his associates, as he heard of his death. His hand was always open to the needy. He was given to hospitality. He frequently took part in public meetings, and was actively concerned in all questions of local improvement. He rarely, if ever, failed to discharge his duties as a citizen at the polls, and was always ready to express his opinions on questions of public policy.

A whole-souled patriot, he viewed with the deepest interest the complications brought into the affairs of the country by the system of slavery. His general benevolence ever led him to sympathize with the oppressed, and the wrongs of the African touched him deeply. We cannot better indicate his feelings on this subject than by quoting a few sentences from his private journal under the date of April, 1850. After mentioning the death of the champion of what have been called "Southern rights," John C. Calhoun, his former pupil and friend, he gives a brief sketch of his character, concluding as follows:

> "His public career has been highly distinguished. It is, however, very much to be regretted that he, many years ago, narrowed down his great mind to sectional views, and that he became morbidly sensitive and jealous of encroachment as regards the South, especially in reference to the protective tariff and to slavery. The former prompted his efforts for nullification, and the latter excited him to a vindication of slavery in the abstract. He, in a great measure, changed the state of opinion and the manner of speaking and writing upon this subject in the South, until we have come to present to the world the mortifying and disgraceful spectacle of a great republic—and the only real republic in the world—standing forth in vindication of slavery, without prospect of, or a wish for, its extinction. If the views of Mr. Calhoun, and of those who think with him, are to prevail, slavery is to be sustained on this great continent forever. I will not occupy my pages with any extended remarks upon this subject which is now agitating the national councils, and to a degree the nation itself. * * * It [the great question] is in better hands than man's; and I trust ultimately the colored men of all races on this continent will be received into the great human family as rational beings and as heirs of immortality."

As soon as the atrocities in Kansas revealed the determination of the advocates of slavery to perpetuate and extend that institution, even if they dissevered or destroyed the nation, Mr. Silliman came out with all his youthful ardor, and with the influence of his years and reputation, as the opponent of the slave-power. He thus became the object of personal defamation, even in the Senate chamber at Washington; but he still remained firm, for he recognized in this war a slaveholder's rebellion. All the lofty sentiments of patriotism which were awakened in childhood, as he witnessed the commencement of national life, were intensified by this struggle to maintain the Union. He was sure that the nation would be purified by the conflict, and liberty established through all the land.

Mr. Silliman has always been remarkable for uniform good health, and in his later years manifested but slightly the encroachments of age. To the last, his form was as erect, his brow as serene, and his features as full of life and cheerfulness as in his earlier days; and his gait was only a little slower and more cautious.

He continued as usual until the middle of November just past, when he was for a few days quite unwell, probably as an immediate consequence of exposure to cold in attending an evening meeting in behalf of the Sanitary Commission. He had gradually, to appearance, regained nearly his former strength during the following week, and, on Wednesday, was intending to join the family Thanksgiving festival the next day at the house of his son-in-law, Prof. Dana. On the morning of that day, Nov. 24th, he awoke early, after a night of quiet rest, feeling stronger, as he said, than he had done for some days. He spoke with his wife of the many reasons there were for thankfulness, both public and private, dwelling at length upon the causes for national gratitude, especially in the recent re-election to the Presidency of a man who had proved himself so true, so honest, so upright in conducting the affairs of the government as Mr. Lincoln. As was his custom, while still in his bed, he offered up a short prayer, and repeated a familiar hymn of praise. In resuming his conversation, before rising, he spoke of the possibility of his attending the public services of the day, of the happiness of his home, of the love of his children, and, in strong terms of endearment, of his wife. Just as these his last words of love were uttered, there was a sudden change of countenance, a slightly heavier breath and he was gone. At the advanced age of 85, life to him was still beautiful; and not less so was its close. His sun set in the blessedness of the Christian's faith, to rise on a brighter morrow.

Art. II.—*Notice of the Explorations of the Geological Survey of California, in the Sierra Nevada, during the summer of* 1864.

In the September number of this Journal (vol. xxxviii, page 298) a notice was inserted giving a sketch of the explorations of the Geological Survey of California in the High Sierra about the heads of King's and Kern rivers (latitude 36° 30′ to 37°). It was mentioned that Mr. King had, on the 6th of July, ascended a peak over 14,000 feet high, and that he was about to make an attempt to reach a point still higher, which was one of two in sight from, and higher than, the peak he had just been on. Since the last number of the Journal was issued, some further particulars have been received with regard to the peak ascended on the 6th of July, to which the name of Mt. Tyndall was given, and also a brief account of the attempt made to get on to the highest points.

The ascent of Mt. Tyndall was a very difficult and dangerous one; but the summit was reached without accident either to Mr. King or his companion, although the trip required that several nights should be spent at an elevation of over 12,000 feet, of course without fire, and with but scanty covering. From the summit of Mt. Tyndall, which is considerably over 14,000 feet high, there appeared two other peaks of equal elevation and two still higher, all within a distance of seven miles of this. Mr. King says, "of the two highest, one rose close by, being hardly a mile away; it is an inaccessible bunch of needles. The other was equally inaccessible from any point on the north or west side. The first-mentioned was about 150 feet above us, the other was six or seven miles distant and I should think fully 350 feet higher than the peak we were on. Within our field of view were five mountains over 14,000 feet and about fifty over 13,000. The five highest peaks are all in the eastern ridge. Owen's valley, a brown sage plain, lies 10,000 feet below on the one side, and Kern cañon, once the rocky bed of a grand old glacier, 4000 feet down on the other. About fifteen miles north of here, King's river cuts through the western ridge and turns at a right angle toward the plain. North of this point, again, the two great ridges unite in a grand pile of granite mountains, whose outlines are all of the most rugged and fantastic character. Twenty-five miles south, the high group ends, there (certainly for a breadth of sixty miles) forming one broad, rolling, forest-covered plateau, 8000 to 9000 feet in elevation."

"From Mt. Brewer to Kaweah Peak, the two culminating points of the western ridge, for a distance of fifteen miles, there is nothing that can be called a separate mountain: it is, rather, a great mural ridge, capped by small sharp cones and low ragged domes, all covered with little minarets. At one place the ridge forms a level table; upon this lies an unbroken cover of snow. To the eastward, all this range, from King's river gateway to Kaweah Peak, presents a series of blank, almost perpendicular precipices, broken every mile or so by a bold granite buttress. Between these are vast snow fields, and also numberless deep lakes, of which the most elevated are frozen."

"The few *Pinus contorta*, and the groves of our new pine[1] have a peculiar black color, or, rather, dark bluish-green, which rather augments than relieves the desolate, naked aspect of things." "The only bits of bright color to break the solemn monotony of granite and snow are the blue lakes, which lie everywhere in the ancient glacier beds."

"Far away in the north there is a broad red band in the granite; other than that, all is gray. Beyond Owen's valley is a low desert range. The Coso and Inyo mountains are in plain sight."

"To the eastward, parallel ridges, one beyond another, lie stretched before you, rigid and stony. They have the same aspect as the mountains near Washoe, the same brown color, with red and yellow shadings."

"Nearly due north, fifty or sixty miles distant, rises a lofty group of mountains, which culminates in a white cone. This must be very high, for unbroken snow covers fully two thousand feet on the south side of the peak, while even the highest mountains in our group have no snow except on the north flank. These are probably the 'White Mountains.' I venture 14,600 feet as a guess at the probable elevation of the highest point in the group."

The White mountains, of which Mr. King speaks, in the extract cited above, lie just on the borders of California and Nevada, in about lon. 118°, lat. 37° 30′; they were distinctly visible to us from Mt. Dana and the other high peaks near Mono lake. It is doubtful whether the highest points are within the State of California; but they are probably very near the line on one side or the other. As it is by no means impossible, although I do not consider it probable, that some points of this range of mountains are higher than any yet measured or ascended by our parties, we still have to remain for some time in uncertainty as to whether California can claim the highest elevations in the country as within her borders; we can, at least, say that the highest measured ones are.

The ascent of Mt. Tyndall was a successful one, the party returning to camp at the end of the fifth day, with bones and barometers unbroken, although several very narrow escapes are briefly mentioned in Mr. King's notes.

It appears from Mr. Brewer's notes, that Mr. King's barometrical measurements of Mt. Tyndall would make that mountain above the height of Mt. Shasta, or over 15,090 feet, Mt. Shasta being 14,440. The height of Visalia, which is the plane of

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[1] A new and not yet named species discovered by the Survey during this trip.

reference for all our observations this year in the High Sierra, has not yet been as accurately determined as it will be after a longer series of observations has been worked up, this place having been, during the past summer, and still continuing to be, one of Major Williamson's stations.

Not satisfied with the former attempt to reach the highest peak of the Sierra, Mr. King made another, starting from Visalia, July 14th, with the intention of following the Owen's lake and Visalia trail, and endeavoring to reach the culminating point from somewhere on this route. This trail leads up the Kaweah river, following up the South fork, from its junction with the middle fork of the same river. Full particulars of this reconnoissance have not yet come to hand; but from letters lately received, it appears that the base of the high point toward which his efforts were directed was, with difficulty, reached; but that the actual summit of the peak was found to be inaccessible—at least from any direction except that of Owen's valley, and probably not to be scaled from that side. The highest point attained by Mr. King on this trip was 14,369 feet above Visalia, which place is approximately 360 feet above the sea level, making the total height 14,729 feet, which is considerably above Mt. Shasta. But Mr. King was not, by estimate, within 300 or 400 feet of the summit, so that it appears that the elevation above the sea-level of this, so far as known, the culminating point of the Sierra Nevada cannot fall short of 15,000 feet.[2]

In the mean time, the main party, under Professor Brewer, made an attempt to work up the topography of this portion of the Sierra north to the Merced river, by keeping along at a high altitude, on the west side of the main ridge; but, after many perilous and laborious attempts, they found the ridges between the north and south forks of King's river to be impassable, so that they were obliged to cross the summit of the Sierra and descend into Owen's valley, in order to get north in that valley, and by crossing back on some other pass to reach the region of the head-waters of the San Joaquin. The pass by which the Sierra was crossed, going into Owen's valley, was about 11,600 feet high, all the region about the summit being exceedingly rough, sharp peaks of granite rising 2,500 feet or more above the pass on both sides. The party travelled for three days up Owen's valley, then turned and crossed the Sierra, by an old Indian foot-trail, to the head-waters of the San Joaquin, the summit of the pass being just about 12,000 feet high, and the mountains on each side 1,000 to 1,200 feet higher

From here the party made their way to Clark's ranch, between the Mariposa estate and the Yo-semite, through many privations and some perils, but without serious accident to life or limb.

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[2] To this peak the name of Mount Whitney was given by Messrs. Brewer, King and Hoffmann.

The result of the summer's reconnoissance has been that a general idea is secured of the topography of a region about as large as Massachusetts, lying wholly within the State of California, and of which nothing whatever was known previously to this. That mountain peaks should be found in this part of the State, higher than any known to exist in the United States, is a discovery equally interesting and unexpected. The details of this exploration will furnish many facts of great geological and geographical interest.

J. D. W.

Northampton, Mass., Oct. 15, 1864.

ART. III.—*On the Mineral Waters of Bath and other hot springs, and their Geological effects;* by Sir CHARLES LYELL, Bart.[1]

* * * * What renders Bath a peculiar point of attraction to the student of natural phenomena is its thermal and mineral waters, to the sanatory powers of which the city has owed its origin and celebrity. The great volume and high temperature of these waters render them not only unique in our island, but perhaps without a parallel in the rest of Europe, when we duly take into account their distance from the nearest region of violent earthquakes or of active or extinct volcanos. The spot where they issue, as we learn from the researches of the historian and antiquary, was lonely and desert when the Romans first landed in this island, but in a few years it was converted into one of the chief cities of the newly conquered province. On the site of the hot springs was a large morass, from which clouds of white vapor rose into the air; and there first was the spacious bath-room built, in a highly ornamental style of architecture, and decorated with columns, pilasters, and tessellated pavements. By its side was erected a splendid temple dedicated to Minerva, of which some statues and altars, with their inscriptions, and ornate pillars are still to be seen in the Museum of this place. To these edifices the quarters of the garrison, and in the course of time the dwellings of new settlers, were added; and they were all encircled by a massive wall, the solid foundations of which still remain.

A dense mass of soil and rubbish, from 10 to 20 feet thick, now separates the level on which the present city stands from the level of the ancient Aquæ Solis of the Romans. Digging through this mass of heterogeneous materials, coins and coffins of the Saxon period have been found; and lower down, beginning at the depth of from 12 to 15 feet from the surface, coins have been disinterred of Imperial Rome, bearing dates from the reign of Claudius to that of Maximus in the fifth century. Be-

[1] From the inaugural address at the opening of the meeting of the British Association at Bath, Sept. 14, 1864.

neath the whole, are occasionally seen tesselated pavements still retaining their bright colors, one of which, on the site of the Mineral-water Hospital, is still carefully preserved, affording us an opportunity of gauging the difference of level of ancient and modern Bath.

One of our former Presidents, Dr. Daubeny, has remarked that nearly all the most celebrated hot springs of Europe, such as those of Aix-la-Chapelle, Baden-Baden, Naples, Auvergne, and the Pyrenees, have not declined in temperature since the days of the Romans; for many of them still retain as great a heat as is tolerable to the human body, and yet when employed by the ancients they do not seem to have required to be first cooled down by artificial means. This uniformity of temperature, maintained in some places for more than 2000 years, together with the constancy in the volume of the water, which never varies with the seasons, as in ordinary springs, the identity also of the mineral ingredients which, century after century, are held by each spring in solution, are striking facts, and they tempt us irresistibly to speculate on the deep subterranean sources both of the heat and mineral matter. How long has this uniformity prevailed? Are the springs really ancient in reference to the earth's history, or, like the courses of the present rivers and the actual shape of our hills and valleys, are they only of high antiquity when contrasted with the brief space of human annals? May they not be like Vesuvius and Etna, which, although they have been adding to their flanks in the course of the last 2000 years many a stream of lava and shower of ashes, were still mountains very much the same as they now are in height and dimensions from the earliest times to which we can trace back their existence? Yet although their foundations are tens of thousands of years old, they were laid at an era when the Mediterranean was already inhabited by the same species of marine shells as those with which it is now peopled; so that these volcanos must be regarded as things of yesterday in the geological calendar.

Notwithstanding the general persistency in character of mineral waters and hot springs ever since they were first known to us, we find on inquiry that some few of them, even in historical times, have been subject to great changes. These have happened during earthquakes which have been violent enough to disturb the subterranean drainage and alter the shape of the fissures up which the waters ascend. Thus, during the great earthquake at Lisbon in 1755,[1] the temperature of the spring called La Source de la Reine at Bagnères de Luchon, in the Pyrenees, was suddenly raised as much as 75° F., or changed from a cold spring to one of 122° F., a heat which it has since retained. It is also recorded that the hot springs at Bagnères de Bigorre, in the same mountain-chain, became suddenly cold during a great earthquake which, in 1660, threw down several houses in that town.

It has been ascertained that the hot springs of the Pyrenees, the Alps, and many other regions, are situated in lines along which the rocks have been rent, and usually where they have been displaced or "faulted." Similar dislocations in the solid crust of the earth are generally supposed to have determined the spots where active and extinct volcanos have burst forth; for several of these often affect a linear arrangement, their position seeming to have been determined by great lines of fissure. Another connecting link between the volcano and the hot spring is recognizable in the great abundance of hot springs in regions where volcanic eruptions still occur from time to time. It is also in the same districts that the waters occasionally attain the boiling-temperature, while some of the associated stufas emit steam considerably above the boiling-point. But in proportion as we recede from the great centres of igneous activity, we find the thermal waters decreasing in frequency and in their average heat, while at the same time they are most conspicuous in those territories where, as in Central France or the Eifel in Germany, there are cones and craters still so perfect in their form, and streams of lava bearing such a relation to the depth and shape of the existing valleys, as to indicate that the internal fires have become dormant in comparatively recent times. If there be exceptions to this rule, it is where hot springs are met with in parts of the Alps and Pyrenees which have been violently convulsed by modern earthquakes.

To pursue still further our comparison between the hot spring and the volcano, we may regard the water of the spring as representing those vast clouds of aqueous vapor which are copiously evolved for days, sometimes for weeks, in succession from craters during an eruption. But we shall perhaps be asked whether, when we contrast the work done by the two agents in question, there is not a marked failure of analogy in one respect—namely a want, in the case of the hot spring, of power to raise from great depths in the earth voluminous masses of solid matter corresponding to the heaps of scoriæ and streams of lava which the volcano pours out on the surface. To one who urges such an objection it may be said that the quantity of solid as well as gaseous matter, transferred by springs from the interior of the earth to its surface, is far more considerable than is commonly imagined. The thermal waters of Bath are far from being conspicuous among European hot springs, for the quantity of mineral matter contained in them in proportion to the water which acts as a solvent; yet Professor Ramsay has calculated that if the sulphates of lime and soda, and the chlorids of sodium and magnesium, and the other mineral ingredients which they contain, were solidified, they would form in one year a square column 9 feet in diameter, and no less than 140 feet in height. All this matter is now quietly conveyed by a stream of limpid water, in an invisible form, to the Avon, and by the Avon to the sea; but if, instead of being thus removed, it were deposited around the orifice of eruption, like the siliceous layers which encrust the circular basin of an Icelandic geyser, we should soon see a considerable cone built up, with a crater in the middle; and if the action of the spring were intermittent, so that ten or twenty years should elapse between the periods when solid matter was emitted, or (say) an interval of three centuries, as in the case of Vesuvius between 1306 and 1631, the discharge would be on so grand a scale as to afford no mean object of comparison with the intermittent outpourings of a volcano.

Dr. Daubeny, after devoting a month to the analysis of the Bath waters, in 1833, ascertained that the daily evolution of nitrogen gas amounted to no less than 250 cubic feet in volume. This gas, he remarks, is not only characteristic of hot springs, but is largely disengaged from volcanic craters during eruptions. In both cases, he suggests that the nitrogen may be derived from atmospheric air, which is always dissolved in rain-water, and which, when this water penetrates the earth's crust, must be carried down to great depths, so as to reach the heated interior. When there, it may be subjected to deoxydating processes, so that the nitrogen, being left in a free state, may be driven upward by the expansive force of heat and steam, or by hydrostatic pressure. This theory has been very generally adopted, as best accounting for the constant disengagement of large bodies of nitrogen, even where the rocks through which the spring rises are crystalline and unfossiliferous. It will, however, of course be admitted, as Professor Bischof has pointed out, that in some places organic matter has supplied a large part of the nitrogen evolved.

Carbonic acid gas is another of the volatilized substances discharged by the Bath waters. Dr. Gustav Bischof, in the new edition of his valuable work on chemical and physical geology, when speaking of the exhalations of this gas, remarks that they are of universal occurrence, and that they originate at great depths, becoming more abundant the deeper we penetrate. He also observes that, when the silicates which enter so largely into the composition of the oldest rocks are percolated by this gas, they must be continually decomposed, and the carbonates formed by the new combinations thence arising must often augment the volume of the altered rocks. This increase of bulk, he says, must sometimes give rise to a mechanical force of expansion capable of uplifting the incumbent crust of the earth; and the same force may act laterally, so as to compress, dislocate, and tilt the strata on each side of a mass in which the new chemical changes are developed. The calculations made by this eminent

German chemist of the exact amount of distention which the origin of new mineral products may cause, by adding to the volume of the rocks, deserve the attention of geologists, as affording them aid in explaining those reiterated oscillations of level—those risings and sinkings of land—which have occurred on so grand a scale at successive periods of the past. There are probably many distinct causes of such upward, downward, and lateral movements, and any new suggestion on this head is most welcome; but I believe the expansion and contraction of solid rocks, when they are alternately heated and cooled, and the fusion and subsequent consolidation of mineral masses, will continue to rank, as heretofore, as the most influential causes of such movements.

The temperature of the Bath waters varies in the different springs from 117° to 120° F. This, as before stated, is exceptionally high, when we duly allow for the great distance of Bath from the nearest region of active, or recently extinct, volcanos and of violent earthquakes. The hot springs of Aix-la-Chapelle have a much higher temperature, viz. 135° F., but they are situated within forty miles of those cones and lava-streams of the Eifel which, though they may have spent their force ages before the earliest records of history, belong, nevertheless, to the most modern geological period. Bath is about 400 miles distant from the same part of Germany, and 440 from Auvergne—another volcanic region, the latest eruptions of which were geologically coëval with those of the Eifel. When these two regions in France and Germany were the theaters of frequent convulsions, we may well suppose that England was often more rudely shaken than now; and such shocks as that of October last, the sound and rocking motion of which caused so great a sensation as it traversed the southern part of the island, and seems to have been particularly violent in Herefordshire, may be only a languid reminder to us of a force of which the energy has been gradually dying out.

If you consult the geological map of the environs of this city, colored by the Government surveyors, you will perceive that numerous lines of fault or displacement of the rocks are there laid down, and one of these has shifted the strata vertically as much as 200 feet. Mr. Charles Moore pointed out to me last spring, when I had the advantage of examining the geology of this district under his guidance, that there are other lines of displacement not yet laid down on the Ordnance Map, the existence of which must be inferred from the different levels at which the same formations crop out on the flanks of the hills to the north and south of the city. I have therefore little doubt that the Bath springs, like most other thermal waters, mark the site of some great convulsion and fracture which took place in the crust of the earth at some former period—perhaps not a very remote one, geologically speaking. The uppermost part of the rent through which the hot water rises is situated in horizontal strata of Lias, and Trias, 300 feet thick; and this may be more modern than the lower part, which passes through the inclined and broken strata of the subjacent Coal-measures, which are unconformable to the Trias. The nature and succession of these rocks penetrated by the Bath waters was first made out by the late William Smith, in 1817, when a shaft was sunk in the vicinity in search for coal. The shock which opened a communication through the upper rocks may have been of a much later date than that which fractured the older and underlying strata; for there is a tendency in the earth's crust to yield most readily along lines of ancient fracture, which constitute the points of least resistance to a force acting from below.

If we adopt the theory already alluded to, that the nitrogen is derived from the deoxydation of atmospheric air carried down by rain-water, we may imagine the supply of this water to be furnished by some mountainous region, perhaps a distant one, and that it descends through rents or porous rocks till it encounters some mass of heated matter by which it is converted into steam and then driven upward through a fissure. In its downward passage, the water may derive its sulphate of lime, chlorid of calcium, and other substances from the decomposition of the gypseous, saline, calcareous and other constituents of the rocks which it permeates. The greater part of the ingredients are common to sea-water, and might suggest the theory of a marine origin; but the analysis of the Bath springs by Merck and Galloway shows that the relative proportion of the solid matter is far from agreeing with that of the sea, the chlorid of magnesium being absolutely in excess, that is, 14 grains of it per gallon for 12 of common salt; whereas in sea-water there are 27 grains of salt, or chlorid of sodium, to 4 of the chlorid of magnesium. That some mineral springs, however, may derive an inexhaustible supply through rents and porous rocks, from the leaky bed of the ocean, is by no means an unreasonable theory, especially if we believe that the contiguity of nearly all the active volcanos to the sea is connected with the access of salt water to the subterranean foci of volcanic heat.

Professor Roscoe, of Manchester, has been lately engaged in making a careful analysis of the Bath waters, and has discovered in them three metals which they were not previously known to contain—namely, copper, strontium, and lithium; but he has searched in vain for cæsium and rubidium, those new metals, the existence of which has been revealed to us in the course of the last few years by what is called spectrum analysis. By this new method, the presence of infinitesimal quantities, such as would have wholly escaped detection by ordinary tests, are made known to the eye by the agency of light. Thus, for example, a solid substance such as the residue obtained by evaporation from a mineral water is introduced on a platinum wire into a colorless gas-flame. The substance thus volatilized imparts its color to the flame, and the light, being then made to pass through a prism, is viewed through a small telescope or spectroscope, as it is called, by the aid of which one or more bright lines or bands are seen in the spectrum, which, according to their position and color, indicate the presence of different elementary bodies.

Professor Bunsen, of Heidelberg, led the way, in 1860, in the application of this new test to the hot waters of Baden-Baden and of Dürkheim in the Palatinate. He observed in the spectrum some colored lines of which he could not interpret the meaning, and was determined not to rest till he found out what they meant. This was no easy task, for it was necessary to evaporate fifty tons of water to obtain 200 grains of what proved to be two new metals. Taken together, their proportion to the water was only as one to three million. He named the first cæsium, from the bluish-gray lines which it presented in the spectrum; and the second rubidium, from its two red lines. Since these successful experiments were made, thallium, so called from its green line, was discovered in 1861 by Mr. Crooks; and a fourth metal, named indium, from its indigo-colored band, was detected by Professor Richter, of Freiberg, in Saxony, in a zinc ore of the Hartz. It is impossible not to suspect that the wonderful efficacy of some mineral springs, both cold and thermal, in curing diseases, which no artificially prepared waters have as yet been able to rival, may be connected with the presence of one or more of these elementary bodies previously unknown; and some of the newly found ingredients, when procured in larger quantities, may furnish medical science with means of combating diseases which have hitherto baffled all human skill.

While I was pursuing my inquiries respecting the Bath waters, I learned casually that a hot spring had been discovered at a great depth in the copper mine near Redruth, in Cornwall, having about as high a temperature as that of the Bath waters, and of which, strange to say, no account has yet been published. It seems that, in the year 1839, a level was driven from an old shaft so as to intersect a rich copper-mine at the depth of 1350 feet from the surface. This lode or metalliferous fissure occurred in what was formerly called the United Mines, and which have since been named the Clifford Amalgamated Mines. Through the contents of the lode a powerful spring of hot water was observed to rise, which has continued to flow with undiminished strength ever since. At my request, Mr. Horton Davey, of Redruth, had the kindness to send up to London many gallons of this water, which have been analyzed by Professor William Allen Miller, F.R.S., who finds that the quantity of solid matter is so great as to exceed by more than four times the proportion of that yielded by the Bath waters.[2] Its composition is also in many respects very different; for it contains but little sulphate of lime, and is almost free from the salts of magnesium. It is rich in the chlorids of calcium and sodium, and it contains one of the new metals—cæsium, never before detected in any mineral spring in England; but its peculiar characteristic is the extraordinary abundance of lithium, of which a mere trace had been found by Professor Roscoe in the Bath waters; whereas in this Cornish hot spring this metal constitutes no less than a twenty-sixth part of the whole of the solid contents, which, as before stated, are so voluminous. When Professor Miller exposed some of these contents to the test of spectrum analysis, he gave me an opportunity of seeing the beautiful bright crimson lines which the lithium produces in the spectrum.

Lithium was first made known in 1817 by Arfvedson, who extracted it from petalite; and it was believed to be extremely rare, until Bunsen and Kirchhoff, in 1860, by means of spectrum analysis, showed that it was a most widely diffused substance, existing in minute quantities in almost all mineral waters, and in the sea, as well as in milk, human blood, and the ashes of some plants. It has already been used in medicine, and we may therefore hope that, now that it is obtainable in large quantities, and at a much cheaper rate than before the Wheal-Clifford hot spring was analyzed, it may become of high value. According to a rough estimate which has been sent to me by Mr. Davey, the Wheal-Clifford spring yields no less than 250 gallons per minute, which is almost equal to the discharge of the King's Bath or chief spring of this city. As to the gases emitted, they are the same as those of the Bath water—namely carbonic acid, oxygen, and nitrogen.

Mr. Warington Smyth, who had already visited the Wheal-Clifford lode in 1855, re-examined it in July last, chiefly with the view of replying to several queries which I had put to him; and, in spite of the stifling heat, ascertained the geological structure of the lode, and the exact temperature of the water. This last he found to be 122° F. at the depth of 1350 feet; but he scarcely doubts that the thermometer would stand two or three degrees higher at a distance of 200 feet to the eastward, where the water is known to gush up more freely. The Wheal-Clifford lode is a fissure varying in width from six to twelve feet, one wall consisting of elvan or porphyritic granite, and the other of killas or clay-slate. Along the line of the rent, which runs

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[2] See for the analysis, the last volume of this Journal, p. 447.

east and west, there has been a slight throw or shift of the rocks. The vein-stuff is chiefly formed of cellular pyrites of copper and iron, the porous nature of which allows the hot water to percolate freely through it. It seems, however, that in the continuation upward of the same fissure, little or no metalliferous ore was deposited, but in its place, quartz and other impermeable substances, which obstructed the course of the hot spring, so as to prevent its flowing out on the surface of the country. It has been always a favorite theory of the miners that the high temperature of this Cornish spring is due to the oxydation of the sulphurets of copper and iron, which are decomposed when air is admitted. That such oxydation must have some slight effect is undeniable; but that it materially influences the temperature of so large a body of water is out of the question. Its effect must be almost insensible; for Professor Miller has scarcely been able to detect any sulphuric acid in the water, and a minute trace only of iron and copper in solution.

When we compare the temperature of the Bath springs, which issue at a level of less than 100 feet above the sea, with the Wheal-Clifford spring found at a depth of 1350 feet from the surface, we must of course make allowance for the increase of heat always experienced when we descend into the interior of the earth. The difference would amount to about 20° F., if we adopt the estimate deduced by Mr. Hopkins from an accurate series of observations made in the Monkwearmouth shaft, near Durham, and in the Dukinfield shaft near Manchester, each of them 2000 feet in depth. In these shafts, the temperature was found to rise at the rate of only 1° F. for every increase of depth of from 65 to 70 feet. But if the Wheal-Clifford spring, instead of being arrested in its upward course, had continued to rise freely through porous and loose materials so as to reach the surface, it would probably not have lost anything approaching to 20° F., since the renewed heat derived from below would have warmed the walls and contents of the lode, so as to raise their temperature above that which would naturally belong to the rocks at corresponding levels on each side of the lode. The almost entire absence of magnesium raises an obvious objection to the hypothesis of this spring deriving its waters from the sea; or if such a source be suggested for the salt and other marine products, we shall be under the necessity of supposing the magnesium to be left behind in combination with some of the elements of the decomposed and altered rocks through which the thermal waters may have passed.

Hot springs are, for the most part, charged with alkaline and other highly soluble substances, and, as a rule, are barren of the precious metals, gold, silver, and platinum, as well as of tin, copper, lead, and many others, a slight trace of copper in the Bath waters being exceptional. Nevertheless, there is a strong presumption that there exists some relationship between the action of thermal waters and the filling of rents with metallic ores. The component elements of these ores may, in the first instance, rise from great depths in a state of sublimation or of solution in intensely heated water, and may then be precipitated on the walls of a fissure as soon as the ascending vapors or fluids begin to part with some of their heat. Almost everything, save the alkaline metals, silica, and certain gases, may thus be left behind long before the spring reaches the earth's surface. If this theory be adopted, it will follow that the metalliferous portion of a fissure, originally thousands of feet or fathoms deep, will never be exposed in regions accessible to the miner until it has been upheaved by a long series of convulsions, and until the higher parts of the same rent, together with its contents and the rocks which it had traversed, have been removed by aqueous denudation. Ages before such changes are accomplished, thermal and mineral springs will have ceased to act; so that the want of identity between the mineral ingredients of hot springs and the contents of metalliferous veins, instead of militating against their intimate relationship, is in favor of their being the complementary results of one and the same natural operation.

But there are other characters in the structure of the earth's crust more mysterious in their nature than the phenomena of metalliferous veins, on which the study of hot springs has thrown light: I allude to the metamorphism of sedimentary rocks. Strata of various ages, many of them once full of organic remains, have been rendered partially or wholly crystalline. It is admitted on all hands that heat has been instrumental in bringing about this re-arrangement of particles, which, when the metamorphism has been carried out to its fullest extent, obliterates all trace of the imbedded fossils. But as mountain-masses many miles in length and breadth, and several thousands of feet in height, have undergone such alteration, it has always been difficult to explain in what manner an amount of heat capable of so entirely changing the molecular condition of sedimentary masses could have come into play without utterly annihilating every sign of stratification, as well as of organic structure.

Various experiments have led to the conclusion that the minerals which enter most largely into the composition of the metamorphic rocks have not been formed by crystallizing from a state of fusion, or in the dry way, but that they have been derived from liquid solutions, or in the wet way—a process requiring a far less intense degree of heat. Thermal springs, charged with carbonic acid and with fluohydric acid (which last is often present in small quantities), are powerful causes of decomposition and chemical reaction in rocks through which they percolate.

If, therefore, large bodies of hot water permeate mountain-masses at great depths, they may in the course of ages superinduce in them a crystalline structure; and in some cases strata in a lower position and of older date may be comparatively unaltered, retaining their fossil remains undefaced, while newer rocks are rendered metamorphic. This may happen where the waters, after passing upward for thousands of feet, meet with some obstruction, as in the case of the Wheal-Clifford spring, causing the same to be laterally diverted so as to percolate the surrounding rocks. The efficacy of such hydro-thermal action has been admirably lilustrated of late years by the experiments and observations of Sénarmont, Daubrée, Delesse, Scheerer, Sorby, Sterry Hunt, and others.

The changes which Daubrée has shown to have been produced by the alkaline waters of Plombières, in the Vosges, are more especially instructive. These thermal waters have a temperature of 160° F., and were conveyed by the Romans to baths through long conduits or aqueducts. The foundations of some of their works consisted of a bed of concrete, made of lime, fragments of brick and sandstone. Through this and other masonry the hot waters have been percolating for centuries, and have given rise to various zeolites—apophyllite and chabazite besides others; also to calcareous spar, aragonite, and fluor spar, together with siliceous minerals, such as opal,—all found in the interspaces of the bricks and mortar, or constituting part of their rearranged materials. The quantity of heat brought into action in this instance in the course of 2000 years has, no doubt, been enormous, although the intensity of it developed at any one moment has been always inconsiderable.

The study, of late years, of the constituent parts of granite has in like manner led to the conclusion that their consolidation has taken place at temperatures far below those formerly supposed to be indispensable. Gustav Rose has pointed out that the quartz of granite has the specific gravity of 2·6, which characterizes silica when it is precipitated from a liquid solvent, and not that inferior density, namely 2·3, which belongs to it when it cools and solidifies in the dry way from a state of fusion.

But some geologists, when made aware of the intervention on a large scale, of water, in the formation of the component minerals of the granitic and volcanic rocks, appear of late years to have been too much disposed to dispense with intense heat when accounting for the formation of the crystalline and unstratified rocks. As water in a state of solid combination enters largely into the aluminous and some other minerals, and therefore plays no small part in the composition of the earth's crust, it follows that, when rocks are melted, water must be present, independently of the supplies of rain-water and sea-water which find their way into the regions of subterranean heat. But the existence of water under great pressure affords no argument against our attributing an excessively high temperature to the mass with which it is mixed up. Still less does the point to which the melted matter must be cooled down before it consolidates or crystallizes into lava or granite afford any test of the degree of heat which the same matter must have acquired when it was melted and made to form lakes and seas in the interior of the earth's crust.

We learn from Bunsen's experiments on the Great Geyser in Iceland, that at the depth of only seventy-four feet, at the bottom of the tube, a column of water may be in a state of rest, and yet possess a heat of 120° Centigrade, or 248° F. What, then, may not the temperature of such water be at the depth of a few thousand feet? It might soon attain a white heat under pressure; and as to lava, they who have beheld it issue, as I did in 1858, from the southwestern flanks of Vesuvius, with a surface white and glowing like that of the sun, and who have felt the scorching heat which it radiates, will form a high conception of the intense temperature of the same lava at the bottom of a vertical column several miles high, and communicating with a great reservoir of fused matter, which, if it were to begin at once to cool down, and were never to receive future accessions of heat, might require a whole geological period before it solidified. Of such slow refrigeration, hot springs may be among the most effective instruments, abstracting slowly from the subterranean molten mass that heat which clouds of vapor are seen to carry off in a latent form from a volcanic crater during an eruption, or from a lava-stream during its solidification. It is more than forty years since Mr. Scrope, in his work on volcanos, insisted on the important part which water plays in an eruption, when intimately mixed up with the component materials of lava, aiding as he supposed, in giving mobility to the more solid materials of the fluid mass. But, when advocating this igneo-aqueous theory, he never dreamed of impugning the Huttonian doctrine as to the intensity of heat which the production of the unstratified rocks, those of the plutonic class especially, implies.

The exact nature of the chemical changes which hydrothermal action may effect in the earth's interior will long remain obscure to us, because the regions where they take place are inaccessible to man; but the manner in which volcanos have shifted their position throughout a vast series of geological epochs—becoming extinct in one region and breaking out in another—may, perhaps, explain the increase of heat as we descend toward the interior, without the necessity of our appealing to an original central heat, or the igneous fluidity of the earth's nucleus.

ART. IV.—*On the Nebular Hypothesis;* by DAVID TROWBRIDGE, A.M.

[Concluded from vol. xxxviii, p. 360.]

1. *The Breaking-up of the Rings.*

34. The process of cooling would still continue after the rings had separated. The loss of caloric from radiation would cause the rings to contract their dimensions; and this, the unequal density of different parts, and the extraordinary perturbations to which they would necessarily be subject in their motions, would cause a separation of the rings in certain weak places. When once broken into parts while the density of the ring was very small, even if the parts were not many, the case would be extraordinary in which the parts would be prevented from re-uniting into a single planet, owing to the very great perturbations to which their motions would be subject, as separate bodies. It is impossible at present to tell just how a ring would be resolved into a planet. A system of waves would probably be developed, owing to perturbations; and by an accumulation in one part, owing to the nature of the disturbing force, such as calculation shows might exist under certain conditions in the case of the rings of Saturn, and thus a considerable portion of the matter of the ring would be driven into one side, and this accumulation would be the center of attraction around which the planet would be formed.

35. We may now ask in what direction the planet thus formed would rotate? The direction of rotation would depend on circumstances. Let us take the breadth of the ring from which Uranus was formed, the same as the diameter of his sphere of attraction in Kirkwood's Analogy. The diameter of the sphere of attraction of Uranus, as given by Professor Kirkwood,[28] is 7·438. The inner radius is 2·558, and the outer one, 4·879. If we suppose the inner and the outer parts of the ring each to have the velocity due it according to Kepler's third law, then, the velocity of the inner part being called 1, that of the outer will be 0·8287; and the angular velocities of the same parts will be to each other as 1 to 0·4715. If every part of the ring have the same angular velocity, and that of the inner part be 1, the angular velocity of the outer part will be 1·457. If the inner and the outer parts have the same velocity, the angular velocity of the inner part being called 1, that of the outer part will be 0·6866. But none of these cases can obtain in any of the rings, but one or another will be approximated to, accord-

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[28] This Journal, [2], xiv, p. 213.

ing to circumstances. The outer rings would approximate to the first case, and the inner ones, to the second case; while intermediate rings would approximate more or less, according to their position in the System, to the last case. In confirmation of this, we may refer to the case of Saturn's rings. It is demonstrated by mathematicians that the stability of the motions of those rings cannot exist unless the parts at different distances from the center of Saturn, have different angular velocities.[29] But observation[30] shows that the stability of the motion of the Rings exists. We hence conclude that those parts of the Rings situated at different distances from the center of Saturn do have different angular velocities. But it is concluded from observation that the rings of Saturn are liquid;[31] we hence conclude that there is still greater necessity for the different parts of a gaseous ring situated at different distances from the center of attraction, to have different angular velocities; just as we have concluded in speaking of the separation of the rings from the equatorial parts of the great solar spheroid.

36. In the case of the first two or three rings abandoned by the primitive spheroid, their density would be relatively so small that the velocity of the outer parts would be regulated approximately by Kepler's third law. Such being the case, it is probable that the velocity of the inner parts will exceed that of the outer parts. When the rings break up to form planets, the parts will in a great measure retain the velocity which they had in the complete rings, and the consequence will be that the rotation of such planets will, at first, be performed in a direction opposite to that of their motions around the central body.[32] In consequence of the great friction of the different strata of the fluid planetary rings abandoned by the solar spheroid, the outer strata would, in most cases, probably, have a greater absolute velocity than the inner strata; and the planets formed from them would naturally at first take the direct motion of rotation, because the external parts having a greater velocity than the inner parts, the excess of velocity would give the planet a direct rotatory velocity. Even if some of the outer planets should prove to have a retro-

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[29] Maxwell, *On the Stability of the Motion of Saturn's Rings*, pp. 3 and 45; Peirce in Gould's *Astronomical Journal*, ii, 17–18.

[30] Observation seems to show that the Rings of Saturn are gradually approaching the body of the planet; but it by no means follows that they will ever come in contact with the body of the planet. They have existed too long for us to suppose there is any probability that the Rings are to be precipitated upon the body of the planet before our eyes, as it were.

[31] See *Annual of Scientific Discovery* for 1852, p. 377.

[32] Professor Kirkwood has pointed this out in his article in this Journal, [2], xxxviii, 2–4; and he has also attempted to show that in certain cases the rotation will become direct from the action of other members of the System. See also, volume xxxvii, page 51, where Prof. Hinrichs has given a formula indicating the same thing that we have arrived at in the text.

grade rotatory motion, our reasoning would not lead us to conclude that such would be the case with many of them.

37. But are we certain that all the rings would break up; or if broken up, that each would form a single planet? In this part of our enquiry we cannot guess at it; nor will it do to be guided by the phenomena of the Solar System; for it is the phenomena of such a system of which we are to render a rational account. We must here be guided by strictly mathematical and philosophical deductions. Prof. Peirce, of Harvard University, in his investigation of the problem of the stability of the motions of Saturn's Rings, arrived at the remarkable conclusion that the dynamical equilibrium of the rings is preserved by the sustaining effect of the satellites in the very act of perturbation. He then makes the remark that the only place in the Solar System, among the primary planets, where we could, from the above conclusions, expect a permanent ring, is just within the powerful masses of Jupiter and Saturn.[33] But even here, he says, the ring could not exist, but must ultimately be destroyed. He says, "But had there been a ring at this part of the system, it must have been subject to such extraordinary perturbations that it would, in the course of time, have been vibrated up against the next inferior planet, *Mars;* and in this way have been broken into the asteroids. The orbits of planets, formed under such circumstances, must have been characterized by great eccentricity."

The above view of the formation of the Asteroids, needs some modification. According to Peirce's conclusions, drawn from his investigation of the problem of Saturn's Rings, a fluid ring might, perhaps, exist for some considerable length of time, within the orbit of Jupiter. Granting this to be true, it is difficult to escape the conclusion that the process of cooling, to which the ring would be subject, even if it were gaseous, would gradually reduce it to a condition approximating to that of the rings of Saturn at the present time; that is, a liquid state. The various conclusions, then, at which mathematicians have arrived, respecting the perturbations of those rings, will apply with greater or less force, according to circumstances, to the supposed ring within the orbit of Jupiter. Prof. Maxwell,[34] in his investigation of the problem of the motion of Saturn's Rings, discussed the case of a solid ring, a ring of disconnected particles, and a fluid (or more definitely a liquid,) ring. The first case he found to be one of unstable equilibrium. In the last two cases he found that if the perturbations to which the ring would be sub-

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[33] Gould's Ast. Journal, vol. ii, p. 18. Also Annual of Scientific Discovery for 1852, p. 379.

[34] On the stability of the motion of Saturn's rings. An essay which obtained the Adams Prize for the year 1856, in the University of Cambridge, Eng. By J. Clerk Maxwell, M. A.

ject could be propagated around the ring in the form of waves, subject to particular conditions, the ring would be permanent; but if these particular conditions were not fulfilled, the ring would be destroyed. In the case of a fluid ring, (incompressible,) he found that in general a wide ring could not revolve every part of which had the same angular velocity.[35] In his recapitulation[36] he says, "We next took up the case of a flattened ring, composed of incompressible fluid, and moving with uniform angular velocity. The internal forces here arise partly from attraction and partly from fluid pressure. We began by taking the case of an infinite stratum of fluid affected by regular waves. I found the accurate values of the forces in this case. For long waves, the resultant force is in the same direction as the displacement, reaching a maximum for waves whose length is about ten times the thickness of the stratum. For waves about five times as long as the stratum is thick, there is no resultant force; and for shorter waves, the force is in the opposite direction to the displacement.

39. "Applying these results to the case of the ring, we find that it will be destroyed by the long waves unless the fluid is less than $\frac{1}{42}$d of the density of the planet, and that in all cases the short waves will break up the ring into small satellites.

40. "Passing to the case of *narrow* rings, we should find a somewhat larger maximum density, but we should still find that very short waves produce forces in the direction opposite to the displacement, and that therefore, as already explained, these short undulations would increase in magnitude without being propagated along the ring, till they had broken up the fluid filament into drops. These drops may or may not fulfill the condition formerly given [in the essay] for the stability of a ring of equal satellites. If they fulfill the conditions, they will move as a permanent ring. If they do not, short waves will arise and be propagated among the satellites, with ever increasing magnitude, till a sufficient number of drops have been brought into collision, so as to unite and form a smaller number of larger drops, which may be capable of revolving as a permanent ring."

41. Basing our reasoning on the preceding results, we are led to the conclusion that under certain conditions—such as probably exist within the orbits of Jupiter and Saturn in the Solar System—the abandoned fluid ring may preserve its form for immense ages, and thus have time to cool down somewhat and approximate to the condition of an incompressible fluid, and then, with the changes already taken place, undergo the necessary transformation, to convert it into small, separate bodies. These bodies would continue to come into collision until the conditions of dynamical equilibrium were fully established, and thus en-

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[35] Maxwell, p. 45.

[36] Ib., p. 64.

able each body to move in its own independent orbit. Bodies so formed must vary in size from the largest of them to bodies too minute to be visible through our most powerful telescopes.[37] The original ring would necessarily have considerable width, and since it would be prevented from uniting into a single planet, any such zone of small planets would also be of considerable width, and the planets would range in mean distance from one limit of the broken ring to the opposite limit. The various disturbing causes would very probably cause the small planetary bodies to move in very eccentric (planetary) orbits, and the action of the short waves which would break up the ring, might, perhaps, cause the resulting planets to move in orbits considerably inclined to each other, and to the plane of the ecliptic.

42. The heterogeneousness of the materials composing the outer rings, and the great rarity of their physical condition, would, perhaps, cause some of the less dense portions, at the time of the breaking-up of the rings, owing to the action of the great disturbing forces to which their motions would be subject, to be projected to a considerable distance from either the outer or the inner parts of the rings, and such detached portions might never return to the parent masses, but would move around the central solar body in an elliptical orbit, having, perhaps, in some cases, considerable inclination to the plane of the equator of the rings from which they were projected. Such bodies would revolve around the sun as *Comets.* The theory of Central Forces[38] shows us that, when the distances from the central body and the initial velocity are given, the eccentricity is dependent on the angle of projection; that is, the angle formed by the radius-vector and the line of projection. Supposing comets to have been formed as we have just described, we see that the great eccentricity of their orbits results from the direction with respect to the radius-vector, in which the mass is thrown from the ring. Such comets would very probably move around the sun in the same direction as the planets. Those rings abandoned nearer the centre of the solar spheroid, would, very likely, be more symmetrical in the disposition of their materials, and consequently less likely to have any large collection of rare materials that could be projected from the rings. We hence conclude that those comets which have a comparatively short period of revolution, will not be very large and conspicuous objects when they approach their perihelia. We may further conclude that the last three or four rings would not throw off any comets. We hence conclude that periodic comets having a direct motion should be found in groups, perhaps, with periods of revolution a little greater or a little less than the planets to which they

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[37] May we not in this way account for the existence of meteoric rings!

[37] See Math. Monthly, ii, 160.

nearest correspond in their motions. Retrograde comets, and a portion of those which have a direct motion, cannot very well be accounted for upon this hypothesis of the breaking-up of the rings, nor should we expect to find retrograde comets with short periods. If, as Laplace supposed, and which, according to our hypothesis, is not improbable, comparatively small quantities of nebulous matter exist in different regions of the universe, the Solar System may, in its motion through space, come into their immediate neighborhood at different times, and such nebulous matter would be drawn, by the attractive influence of the sun and planets, into the system, and it would appear in the form of comets—either retrograde or direct, according to circumstances—and meteors. All comets so formed would necessarily have a comparatively long period of revolution, unless (which might be the case in some instances) the perturbative influence of the planets should change their orbits. We may hence conclude that all, or nearly all, comets having a short period, will have a direct motion; and of the comets of long periods, the direct and retrograde should, considering all time, be nearly equally divided, so far as we can determine.

2. *The Planets and their Satellites.*

43. After the rings from which the planets were formed were broken up, and all the parts of any one ring reduced to a planet, such planet, as we have shown, would have a rotation on an axis. This axis would, necessarily, be a *natural* or *principal axis.* It would be a delicate adjustment to give a solid body, differing from a sphere, a rotation around a principal axis; but a fluid body would readily adapt itself to such an axis. We do not doubt but that Infinite Wisdom and Power could cause any solid body to rotate around a natural axis; but we have not the least evidence that Infinite Wisdom ever works in that special manner. God always adapts means to ends, so that all things are produced under the action of fixed natural laws. We, therefore, conclude that the planets, formed as required by the Nebular Hypothesis, should all be found to rotate on *natural axes.*

44. After the parts of the broken-up rings had united, and the resulting bodies commenced their rotatory motion, the cooling of the masses, in consequence of the radiation of heat, would cause their rotatory velocities to be increased, and this process would continue till, in most instances, a secondary ring would be abandoned by such planetary bodies.[39] The outer planets being larger and much less dense than the inner ones, would abandon *several* secondary rings. In reference to the number of these secondary rings cast off by each of the planets, all that we can

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[39] We have attempted here to be guided by strictly reasonable conclusions, without any reference to the phenomena of the Solar System.

at present conclude on, with any degree of certainty, is, that the number should, upon the whole, increase from the sun outward. We cannot, at present, say that the outermost planet ought to have abandoned the greatest number of rings. These secondary rings would, in general, break up and form secondary planets, or satellites. Under certain conditions—such as Prof. Peirce has found to exist in the System of Saturn—a ring, or rings might remain entire, or at least, not break up into a single satellite. According to this view, rings, if they exist, must be found interior to several satellites. In every case—unless changed by disturbing forces—the planet should rotate on its axis in less time than is required for any ring or satellite to revolve around the primary. The same must hold in the case of the sun and the planets.

45. When the last ring has been abandoned by any one of the planets, the remaining part[40] must cool down and thus form a primary planet. The outer portions being exposed to the low temperature of space—at least 50° below zero Fah.—they will cool much more rapidly than the inner parts, and after the whole is reduced to a liquid, a crust would, comparatively soon, form around the liquid planet, and further radiation would take place very slowly compared with its former rate. A certain amount of heat would be received from the other bodies, particularly the sun, (whatever might be its condition), and finally a balance would be reached beyond which the planet would not cool,—or at most very slowly,—and thus the inner parts would remain liquid. The outer planets being from the beginning less dense than the others, a crust would, perhaps, be formed so comparatively early as to leave the mean density of the planet comparatively small. We should, therefore, look for an increase in mean density from the outermost planet to the innermost. We see no reason, however, to believe that any simple law regulates this variation of mean density from one planet to another.

46. It would at first seem as if the satellites of the primaries should follow the same law of rotation that the primaries themselves do; but we must recollect that the numbers representing the distances of the former, expressed in radii of the latter, may differ very considerably from the numbers representing the distances of the latter expressed in radii of the sun. Again, the rings from which the satellites were formed, were abandoned when the primaries were much reduced in temperature, and condensed, when compared with the condition of the primary rings

[40] In the Nebular Hypothesis, after a fluid body has abandoned all the rings possible, there *must* remain a central body of comparatively large dimensions, and of a mass far greater than the sum of the masses of all the rings separated. How would the author of the meteoric theory as given in the 204th number of the North American Review, account for the fact that our Solar System is constructed upon this principle?

when they were separated from the primitive solar spheroid. All the satellites will be much smaller than the primaries; and, being comparatively small bodies, they will cool down and become solid much sooner than the primaries will. In consequence of the forces acting on the planets, both primaries and secondaries, they will become spheroidal in shape as soon as the rings from which they are made are broken up and the parts united. The attraction of the primaries on the fluid satellites, will raise the fluid particles into great tidal-waves; and as the waves on opposite sides are slightly unequal, the result will be a retardation of the rotatory velocity of the secondaries. William Ferrel, Esq., of the Nautical Almanac Office, Cambridge, Mass., has given[41] a mathematical discussion of the retarding effect of the tides of the earth on its rotatory velocity. According to his conclusions, the retarding effect of the earth upon the moon's rotatory velocity, both acting under similar circumstances, would be between 500 and 600 times as great as that of the moon on the earth's. According to his results, it would amount to an entire circumference of the moon, or one period of rotation of the moon in a hundred years. Even if these numbers be wide of the truth, the investigation shows us that, in general, the retarding effect of the attraction of the primaries on their secondaries, when in a fluid state, is sufficient, in the course of immense ages, to reduce the periods of revolution and rotation of the latter to isochronism. We have, therefore, great reason to suppose that as a general rule the satellites turn on their axes but once during a revolution around their primaries.

47. If there exists a cosmical ether, as is at present pretty generally admitted, in order that it may remain spread throughout universal space, it is only necessary for it to possess an elasticity so great that the action of luminous bodies is sufficient to produce a mechanical action in it that will enable it to maintain its temperature and fluid condition under all circumstances. This cosmical ether being material in its nature, it would necessarily partake of the motion of those bodies with which it remains in contact for immense ages of time. In the Solar System, the motion of the ether around the sun would be in the general direction of all the planets. It would also be regulated in its motion

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[41] Gould's Ast. Journal, iii, 138–141. Professor Ferrel informs me that some numbers which he employed he now knows to be about ten times too large, and hence his results should be diminished in nearly the same ratio. He found that in one century the moon retarded the rotatory motion of the earth's equator 37·44 miles. Dividing this by 10, to correspond more nearly with more recent data, we have about 3¾ miles for the retarding effect of the moon in a century. The effect of the earth on the moon he found to be 562½ times as great as that of the moon on the earth; and this will give—since the diameter of the moon is only about one-fourth of the diameter of the earth—between 500 and 600 miles for the retarding effect of the earth upon the moon's equater, if it be only partly fluid. If it be wholly fluid, it would be somewhere near 1000 miles per century.

partly according to the third law of Kepler; and thus the planets, whatever might be their distances from the sun, would scarcely be retarded by it in their annual revolutions.[42] Since the ether is supposed to maintain its temperature, and therefore elastic force, it would not gather into separate collections, leaving some parts without it; but it would only be slightly condensed around the different bodies of the universe.

48. If we calculate the principal radii of gyration of Jupiter, at the time when each secondary ring was abandoned, upon the same principle as that adopted in the case of the sun, viz: by neglecting the masses of the satellites, we shall find such radii, at the time when each satellite ring was separated, commencing with the nearest and taking them in the order of distance—the equatorial radius of Jupiter being the unit—to be as follows:

TABLE IV.

$k_1 = 1·308$; $a_1 = 6·048$;
$k_2 = 1·832$; $a_2 = 9·623$;
$k_3 = 2·630$; $a_3 = 15·350$;
$k_4 = 4·018$; $a_4 = 26·998$;

a_1, &c., being the distances of the satellites. If we compare the radii of gyration, k, with the mean distance of the satellites, a, we see that in every instance the latter is not far from six times the former. If we make a similar comparison for the primary planets, using Table II, we see that the radius of gyration is only about the one-hundredth of the mean distance of the planet from the center of the sun. We hence conclude from this that the density of the primitive solar spheroid increased much more rapidly toward the center, than the primitive Jovian spheroid. In other words, the solar spheroid was relatively much more dense about the central parts than Jupiter. This we had reason to expect, since, according to the hypothesis, the ring from which the Jovian system was formed was already somewhat condensed when it was abandoned, and the general density much more nearly uniform than that of the solar spheroid; and hence the secondary rings were much denser relatively to the Jovian spheroid than was the ring from which Jupiter was formed relatively to the solar spheroid. This, it would seem, is a strong proof of the truth of the nebular hypothesis. The mass of Jupiter is 6000 times as great as the sum of the masses of his satellites; so that neglecting the masses of the satellites in comparison with the mass of Jupiter we neglected only the small fraction $\frac{1}{6000}$th. I supposed Jupiter homogeneous in finding his principal radius of gyration.

The periods of rotation of Uranus and of Neptune being un-

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[42] This conclusion differs from the basis assumed by Prof. Hinrichs in his article on the Age of the Planets, this Journ., [2], xxxvii, 36–56.

known, we cannot compute the radii of gyration of the planets at the time when the satellite rings were abandoned, as we have done in the case of Jupiter. And if Bessel's mass of the rings of Saturn ($\frac{1}{118}$th of that of Saturn) be admitted, it is probable that this, and the sum of the masses of the satellites, will make a fraction too great to be neglected. The same is true of the earth and moon.

49. Let us now collect together in this article the various facts which ought to be found to exist in the Solar System, according to the preceding view of the nebular hypothesis.

1st. The planets should all move in the same direction around the sun. 2nd. Some comets whose distances correspond with those of the planets within moderate limits, should have the same direction as the planets. 3d. The orbits of the planets should not be much inclined to each other. 4th. All the planets, except, perhaps, a few of the outer ones, should turn on their axes in the same direction as that in which they move around the sun. 5th. The satellites should revolve around their primaries in the same direction in which the latter turn on their axes. 6th. The periods of revolution and rotation of the satellites, should, in many cases, at least, be equal. 7th. The sun must rotate on an axis in the same direction as that in which the planets revolve around him. 8th. The outer planets of the system should be larger than the inner ones. 9th. The outer planets should have the greater number of satellites. 10th. The outer planets should rotate on their axes in less time than the inner ones. 11th. The outer planets should in general have greater masses than the inner ones. 12th. The planets should, upon the whole, increase in density as they are found nearer the sun. 13th. The satellites should be less than their primary, and the planets less than the sun, both in bulk and mass. 14th. All the members of the Solar system should be composed of similar materials.

50. We have thus, in the preceding pages, attempted to give a connected view of the nebular hypothesis, uncompared, except in a few cases, with the real phenomena of nature. In what follows, we shall show that the phenomena of the sidereal heavens, and of the Solar System, agree very closely with the preceding deductions.

3. *A Comparison of the preceding Theory with the Phenomena of Nature.*

51. The origin of material existence is at present a mystery to the human mind. To say that it was created by the Deity is an assertion that conveys to the understanding something of which we have no definite conception. On the other hand, to say that it has always existed is equally unsatisfactory, since the human mind always looks toward a beginning of everything.

But with such speculations, we have here nothing to do. The nebular hypothesis supposes matter and all physical forces to exist; and we have only to carry back our speculations to the most ancient and chaotic state in which it is possible for us to suppose matter to exist. I have supposed matter in its earlier chaotic condition to have been necessarily heterogeneous in structure. Others have different views.[43] Even if it[44] were at first, or at any time, perfectly homogeneous, but not of symmetrical form, it would soon become heterogeneous, because various centers of attraction would be established, around which matter would accumulate; and thus the change in the different parts would generate a rotatory motion. Thus, under almost any conceivable—certainly probable—circumstances, the nebulous mass must be considered as having sufficient potential energy to give it a motion of rotation. But we must not be confined to these speculative views. "Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate. For instance, the minute particles of dew which whiten the grass blade in early morn, are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form."[45] "It is remarkable of physical laws that we see them operating on every kind of scale as to magnitude, with the same regularity and perseverance. . . . . . Two eddies in a stream fall into a mutual revolution at the distance of a couple of inches, through the same cause that makes a pair of suns link in mutual

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[43] "I regard the gaseous as not the *ultimate* form of matter, but that in its ultimate state it was *absolutely* imponderable and perfectly elastic, and therefore it completely filled, absolutely, infinite space, and that the myriads of Solar vortices were developed out of this perfectly homogeneous imponderable fluid in a manner as definite and orderly as that which *regulates the production* of a plant or an animal on the earth."—*Dr. Joel E. Hendricks.*

[44] Some writers seem to doubt the existence of nebulous matter, because the telescope continues to resolve the nebulæ. But comets are evidently in a nebulous state, and the existence of these shows us that matter may assume the nebulous condition. It is a fact that the evidence derived from observation, that nebulous matter, in large quantities, exists, is by no means conclusive. In fact, the truth of the nebular hypothesis cannot depend on our knowledge of the existence of nebulous matter in large quantities. It is sufficient to show that matter in such a state can and does exist in *any* quantity, and after that we must look to the general and particular phenomena of nature. It is scarcely probable that we shall ever learn, at least for ages to come, directly from observation, that large quantities of matter exist in a nebulous condition. But the general appearance of starry clusters, may indicate the origin whence they were derived. The appearance and arrangement of the planetary system are more conclusive in reference to the origin whence that system was derived.

[45] Mr. Nasmyth. See Annual of Scientific Discovery for 1857, p. 187.

revolution at the distance of millions of miles. There is, we might say, a sublime simplicity in this indifference of the grand regulations to the vastness or the minuteness of the field of their operations."[46] We thus may learn from the minuter operations of nature of those grand revolutions which we have reason to conclude have taken place in past ages of duration. Binary, ternary, and multiple stars, in some, perhaps many, cases, may be the resulting motion of two or more stars—I should more properly say primitive stellar spheroids, coming so near together that their atmospheres came in contact and thus gave them a motion around their common center of gravity.[47]

52. All apparent motion in the region of the so-called fixed stars, is very slow; so slow, indeed, that in most instances it requires several years to elapse before *any* motion becomes apparent. Certainly, in most cases the nebulæ are situated far beyond the region of the visible stars. How much slower, then, than the stars, must the nebulæ appear to change their relative parts or positions in the heavens! We must, therefore, expect little from the motion of nebulæ or even clusters, in confirmation of the nebular hypothesis. We must look to the general conformation of nebulæ and of clusters. Lord Rosse has within a comparatively few years, by means of his great reflectors, shown that many of the nebulæ are of the *spiral* form. They appear as if they possess a rotatory motion on an axis, and this motion has so far increased as to project some parts of the nebulæ tangentially, and thus to break up the spheroidal form, causing parts of the nebulæ to fly off in one or more streams. The general principles which would lead to such a result, we have attempted to give from Arts. 3 to 12 inclusive. Mr. Nasmyth has also given[48] an exposition of the principles, though he has not based his explanations on mathematical formulæ and deductions as I have attempted to do. Many nebulæ present that appearance which we suppose our Solar System to have presented before the first planetary ring was abandoned, viz: a projected circular or elliptic appearance, not very bright in the outer portions, but increasing in luminosity with considerable rapidity near the center.

53. Some observations and deductions of a very interesting character, have recently been made. Many years ago, Sir William Herschel concluded, from a comparison of a figure of the nebula in Orion, made by Huygens many years before, with a figure of his own drawing, that some parts of the great nebula —(cluster, perhaps we should now say, since it has been par-

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[46] Vestiges of Creation. Harper's Ed., p. 17.

[47] I am unable to ascertain in what direction, if any definite one, double stars revolve around their common center of gravity. Who will give the required information? It is worthy of attention.

[48] Ann. of Sci. Disc. for 1852, pp. 187–8.

tially resolved since Herschel's day)—have changed. It is very probable that no such change has taken place; but that the supposed variation came from a defect in Huygen's representation, and from a difference in the telescopes employed. But it seems now quite certain that some nebulæ have changed. The nebula around *Eta Argus*, seems to be one of that character.[40] Also several others have been discovered in which some change seems to have taken place. Accurate micrometrical measurements and photographic representations only can here serve for the basis of positive knowledge in this interesting department of astronomy. It would seem that such patches of nebulæ, if, indeed, they *are* nebulæ, proper, cannot be very extensive, since they must be comparatively near our system. We must now leave this interesting part of our subject, since this, alone, could never, so far as we can see, furnish us the necessary data to enable us to say whether the God of Nature followed one or another method in the process of world-making. We must descend to the general and some of the particular phenomena of the Solar System, to enable us to draw a definite conclusion on this subject.

54. We know from observation that all the primary planets revolve around the sun in the same direction, and in nearly the same plane. The sun rotates on an axis in the same direction in which the planets revolve around him, from west to east. The period of the sun's rotation is about 25$\frac{1}{8}$ days, which is about $\frac{7}{24}$ths of the periodic time of Mercury. According to our theory, no planet will ever be discovered so near the sun that its period of revolution will be less than the period of the sun, viz: 25$\frac{1}{8}$ days, if this number be accurately determined. This is contrary to the supposed discovery of Mr. Lescarbault.

The inclination of the orbit of Neptune, as we have shown, corresponds very approximately with that of the invariable plane of the Solar System, as our theory requires. We also find that the distance between the planetary orbits increases with the distance from the sun. The following table will show

TABLE V.

Planet.	Mean dist.	Dist. to next Ex. Orbit.	Diam. of sphere of attraction.
Mercury,............	0·3870981	0·336235	0·200979
Venus,............	0·7233306	0·2766684	0·383390
Earth,............	1·0000000	0·5236923	0·521348
Mars,............	1·5236923	1·544983	0·779537
Ast. Planet,........	3·068675	2·134101	0·968693
Jupiter,............	5·202776	4·336010	4·876551
Saturn,............	9·588786	9·643604	8·618608
Uranus,............	19·18239	10·857110	7·437871
Neptune,..........	30·03950		

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[40] This Journal, [2], xxxvii, 294-5.

the difference between the mean distances of the planets, in terms of the mean distance between the earth and sun; and also the breadth of the primitive rings, according to Kirkwood's Analogy.[60] We have at present no method probably more accurate than Kirkwood's to determine the breadth of the primitive rings.

We thus see that, generally speaking, there is an increase of distance between the orbits of the planets, and also in the breadth of the rings abandoned by the sun; and yet it is not absolutely the case in every instance; nor is the increase according to any simple law, as we had occasion to point out from what was assumed to be the probable condition of the primitive solar spheroid.

55. If we compare the masses and the densities of the planets, we shall see here, also, a general increase in the masses, and a decrease in the densities, as we get farther from the sun, as can be seen in the following table:

TABLE VI.

Planet.	Magnitude.	Mass.	Density.
Mercury,	0·0595	0·0729	1·225
Venus,	0·9960	0·9101	0·908
Earth,	1·0000	1·0000	1·000
Mars,	0·1364	0·1324	0·972
Ast. Planet,		0·2869 [51]	
Jupiter,	1491·0000	338·7180	0·227
Saturn,	772·0000	101·3640	0·131
Uranus,	86·5000	14·2510	0·167
Neptune,	76·6000	18·9000	0·321

We see from this table what was concluded from theory to be very probable, viz: that in general the volumes and masses increase from the sun to a maximum—which is reached in Jupiter—and then decrease to the extremity of the system. The fact that there is no simple law perceptible which regulates the increase and decrease of the numbers in the two preceding tables, shows that the condition of the primitive solar spheroid was far from being symmetrical. The fact that the earth is more dense than Venus may be thus explained. The difference in the condition of the Earth-ring and the Venus-ring could not have been very considerable. But the former, after it united into a single spheroid, threw off a satellite-ring, taking off the rarer part and leaving the denser, so that the ultimate mean density of the earth became a little greater than that of Venus. There seems to be some peculiarity about Saturn in respect to density that cannot be easily accounted for.

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[60] This Journal, [2], xiv, 213.

[51] According to Kirkwood's Analogy, this Journal, [2], xiv, 213.

56. In examining the secondary planets, we see a general increase in number to each primary planet, as we go from the sun to the extremity of the system. The Earth has 1, Jupiter 4, Saturn 8, Uranus 6 and perhaps 8, Neptune 2 that have probably been seen. We cannot tell with certainty the number of satellites belonging to each of the last two planets, owing to the difficulty of observing such bodies at so remote distances. The satellites of Jupiter are distributed in the Jovian system very similarly to the primary planets in the Solar System. We find the masses to increase as the distance from the center of Jupiter increases, till we arrive at the third satellite, where we reach the maximum. The fourth satellite is the second in mass. The third satellite is also the largest, and the fourth the next in size, but the second, although it is of greater mass, yet is of smaller size than the first. The satellites of Saturn follow a similar general law. *Titan*, the 6th in the order of distance from the primary, is the largest, and *Japetus*, the most distant of them all, is next in size. Of the others we cannot so well judge, but it seems not improbable that they decrease in size as they are nearer the primary. We know still less of the satellites of Uranus, but the second and fourth, so-called, are very probably the largest. But within the second, (in the order of distance according to Sir William Herschel's discoveries), two, and possibly three, exist. We thus see it to be a general principle of the planetary system that the largest bodies are within some, and without some of the orbits of the others. We also observe this fact in reference to some of the secondary planets, viz: if we divide the distance of the remotest satellite of any primary, from the center of the planet, by the diameter of the sphere of attraction (which we shall consider to be the same as the breadth of the ring, or the diameter of the primitive planet,) we shall find that, in general, the quotient decreases from the earth to Uranus. We may therefore conclude that the outer planets had to condense much more than the inner ones before a satellite-ring was abandoned. It is a fact, also, that, so far as observation has determined, the satellites rotate on their axes in the same direction and in the same time in which they revolve around their respective primaries. All these motions are in the direction of the rotation of the primaries.

57. The rings of Saturn offer a living example of the primitive secondary rings. They open to us, in a measure, the nature and constitution of the primitive rings, both the primary and secondary. These rings have been retained as such, according to Prof. Peirce, by the attraction of the satellites. Should these rings entirely break up, they would probably form asteroid satellites. Since the rings of Saturn are very thin in comparison with their width, we conclude that the primitive planet was very

much flattened about the poles. If the rings of Saturn are fluid, they exist as such in consequence of the friction of their particles.

58. It is an interesting fact, and one confirmatory of our theory, that the rings of Saturn have a longer period of rotation than the planet itself, as required by the nebular hypothesis. According to the observations of Sir William Herschel, the rings of Saturn revolve around the planet in the space of 10^h 32^m 15^s.[52] The period of rotation of Saturn is 10^h 29^m 17^s.[53] We thus see that the rings require 2^m 58^s longer time to revolve about Saturn, than the latter does to turn on his axis. Here again we appear to have obtained conclusive evidence in favor of the nebular hypothesis. Had the period of Saturn's rotation been greater than that of the rings, it would have been very difficult to reconcile it with the nebular hypothesis.

59. The rings of Saturn are either fluid or composed of unconnected particles. According to Maxwell, even a fluid ring would be broken up into small satellites; but it does not follow that these satellites may not still be fluid and unite again into a ring—or perhaps several nearly concentric rings—after having been separate a sufficient length of time to restore the equilibrium by counteracting the disturbances to which the rings are subject. We are thus reduced to the very probable conclusion that the rings of Saturn are fluid (liquid). We are thus carried back one great step toward the gaseous condition from which we have supposed all the planets and their satellites to have been developed.

60. Observation shows that the satellites of Uranus revolve in a retrograde order. But we do not know in what direction, nor in what time, the planet rotates. If it should finally prove to be the case that the satellites of Uranus are in the plane of the planet's equator, and revolve in a direction opposite to that of the rotation of the planet, it will be very difficult, if not impossible, to reconcile the two motions with the nebular hypothesis. But should the axis of the planet be found to have an inclination to the plane of the orbits of the satellites differing considerably from a right-angle, the difficulty will not be insuperable.[54] But since the nebular hypothesis accounts so satisfactorily for so many of the phenomena of the solar system, we may venture to predict that the rotation of the planet will be found to be retrograde, or closely approaching that direction, if we may so express ourselves. But our theory, as we have

[52] Grant's Hist. of Phys. Astr., p. 262.

[53] Herschel's Outlines, Art. 514.

[54] By giving an artificial globe a motion of rotation on its axis, and then reversing the poles, we can easily see how readily the direction of rotation is changed to the opposite direction. The same primitive impetus carries it in the opposite direction.

shown, requires that the outer planets rotate in a retrograde direction. Kirkwood's Analogy seems to require that Uranus should occupy about 37 hours in performing one rotation. Is it possible that a retrograde motion will influence the time of rotation of the planet? It would seem that some of the great telescopes, either in this country or in Europe, ought to be able to settle the question of the rotation of Uranus. I have not attempted to account for the great difference between the greatest and least diameters of Mars. A mere supposition could be of no advantage.

61. The following table gives the time of rotation of the several planets so far as known:

TABLE VII.

Planet.	Time of rotation.			Diam. of sphere of attraction.
	h.	m.	s.	
Mercury, ...	24	5	28	0·200979
Venus, ...	23	21	21	0·383390
Earth, ...	23	56	4	0·521348
Mars, ...	24	37	22	0·779537
Jupiter, ...	9	55	26	4·876551
Saturn, ...	10	29	17	8·618608

By the above table we see that, roughly speaking, the time of rotation diminishes as the sphere of attraction increases. The original impulse of rotation communicated to a planet, by the ring from which it was formed, at the time of its breaking-up, would be dependent on the difference between the velocity of those parts of the ring lying upon the opposite sides of the orbit of the future planet; or, in other words, upon the difference of the living force of each part. This difference would not depend on the absolute velocity of either part, but rather upon the relative distance of the two parts from the sun. We hence see that since the outer primitive rings were wider than the inner ones, we should expect a more rapid rotatory motion in the outer planets.[55] Since Saturn has the greatest sphere of attraction we should at first expect that planet to have the shortest period of rotation. But such is not necessarily the case. If this whole planetary ring had condensed into a single body, such might and probably would have been the case. But every time a planet abandoned a ring to form a satellite, it, in effect, lengthened the period of rotation of the future planetary spheroid. Jupiter abandoned only four rings of which we have any knowledge, and the united mass of them forms only the $\frac{1}{6000}$th of the mass of Jupiter; while Saturn abandoned no fewer than eleven rings, three of which still remain. According to Bessel, the mass of

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[55] Since the outer rings were less dense than the inner ones, the planets would have farther to contract than the inner ones, and this also would increase the rotatory velocity.

the remaining rings alone equals the $\frac{1}{118}$th of the mass of Saturn. Even now the period of Saturn's rotation exceeds that of Jupiter by only $3^m\ 51^s$.

62. Humboldt has called attention[66] to the fact that the group of Asteroids divides the planets into two classes, those within and those without the zone of small planets, each class being marked by certain distinctive features. The outer group, "Neptune, Uranus, Saturn and Jupiter, are distinguished by considerable volume and slight density; the time of rotation about their axes is about ten hours, and from this there results in these bodies considerable flattening; moreover, of the 22 satellites of the solar system, 21 pertain to this group. The four *inferior* planets, Mars, the Earth, Venus and Mercury, have, on the contrary, much smaller volumes and much greater density; they revolve on themselves in nearly twenty-four hours, are but little flattened, and possess, among all the four, but one satellite, the Moon." Let us now see what account we can give of these peculiarities. We have already shown that according to Prof. Peirce's deductions a planetary-ring might exist for a great length of time just within the orbit of Jupiter; the same influence that would sustain a ring where the asteroids now exist, would tend to sustain one where Mars now revolves; and the same influence would extend to the orbits of the Earth and of Venus; and each of these latter bodies would lend their influence, and would thus influence the Mercurial-ring. These interior rings (as we will call them, being interior to the Asteroids,) being thus sustained for a great length of time, would loose much of their heat, and thus become condensed considerably perhaps, as have the rings of Saturn. When such rings broke up and assumed the spheroidal form, they would be much less likely to abandon satellite rings, than those planets whose rarity was greater. We have already shown that the mean density of those planets nearer the sun should be found to be greater than that of the outer planets; we now refer to the influence above mentioned to account for the sudden increase of mean density within the orbit of Jupiter; for evidently the mean density of a planet formed from a ring considerably condensed, would be greater than one formed from a ring less so; for a crust would form around one approximately as soon as around the other. The period of rotation of such planets would be greater than that of others, since they would not have so far to contract. The times of rotation would be approximately equal, since they would contract nearly equally.

63. The existence of 80 known Asteroids between the orbits of Mars and Jupiter, seems to confirm the view which we have given of their formation, in a preceding part of this paper. If we divide 360° by 80, we find an average of one Asteroid for

[66] Cosmos, iv, 422, Bohn's ed.

every 4½°, making approximately a ring of these small bodies. But we cannot for a moment entertain the idea that nearly all these bodies have been discovered. They are very small and necessarily difficult to detect; and judging from what we already know of the size of these small bodies, we must conclude that many more exist, much the larger share of which are too small to be detected by our best telescopes. The most rational theory of meteoric stones, is that which regards them as minute asteroids. The whole group, many thousands very probably, perhaps millions—between the orbits of Mars and Jupiter, must have very eccentric orbits, and be subject to extraordinary perturbations in their motions, and it is highly probable that many of them become satellites of some of the planets, and finally come in contact with their surfaces as meteoric stones. It is certainly a little curious that the orbits of the known Asteroids intersect. The mean width of the whole zone so far as known, lies between the limits 2·145 (Feronia) and 3·452 (Maxamiliana) giving a breath of 1·307, which is rather greater than the diameter of the sphere of attraction of Kirkwood's Asteroid-planet.

At present we shall add nothing respecting comets, as Prof. Kirkwood has called attention, in several places, to the orbits of these bodies.

Hector, N. Y., Nov. 11, 1864.

ART. V.—*On Brushite, a new mineral occurring in Phosphatic Guano;* by GIDEON E. MOORE, Ph.B.

(Communicated to the California Academy of Sciences, Sept. 5th, 1864.)

IN the spring of the present year, I received, through the kindness of Wm. E. Brown, Esq., of Mare Island in this State, a specimen of a mineral discovered by him in a cargo of phosphatic guano at Camden, N. J. The locality from which it was derived is not known,[1] and, though letters of inquiry have been sent to the parties to whom the cargo was originally consigned, no reply has been received up to this date. The texture and appearance of the guano would, however, point to some one of the Carribean islands, and more particularly to the island of Sombrero as its probable source. It is very probable that the mineral may be recognized among the crystallized products occurring in other guano deposits.

In the specimen in my possession, the mineral occurs filling seams in the guano, varying from ⅛ to ¼ of an inch in width. The matrix itself is of the variety known as rock guano. It

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[1] **In a letter from Mr. Moore, dated San Francisco, Nov. 13th, 1864, he states that he has ascertained the locality of the new mineral to be Avis Island in the Carribean Sea.—EDS.**

possesses an oölitic structure and a brownish white color, interspersed with small spots of pure white.

The mineral is in the form of small but very perfect and brilliant crystals, with a cleavage in the direction of their greatest length nearly equal to that of selenite, the laminæ being also slightly flexible, as in the case of the latter species. Hardness, 2·25. Specific gravity, 2·208 (mean of two determinations). Color yellowish white. Transparent. Lustre vitreous, splendent, inclining to pearly on the cleavage faces.

When heated in a closed tube before the blowpipe, it whitens and gives off water at an incipient red heat. In the platinum forceps, it fuses with intumescence at about 2 on von Kobell's scale, tinging the flame with the peculiar green characteristic of phosphoric acid. The button formed by fusion crystallizes on cooling, showing numerous brilliant facets. It readily dissolves, even in coarse crystals, in dilute nitric and chlorhydric acids.

A qualitative analysis revealed the presence of lime, phosphoric acid and water, with barely discernible traces of magnesia and alumina.

The quantity of mineral at my disposal was very small, scarcely exceeding one gram in weight. In each of the two following analyses, the water was determined in 0·2 gram, the remaining 0·3 gram being employed in the determination of the lime and phosphoric acid. The result was as follows:

	1.	2.
Lime, - - - - -	32·65	32·73
Phosphoric acid, - -	41·50	41·32
Water, - - - - -	26·33	26·40
	100·48	100·45

These figures agree exactly with the composition of the neutral tri-basic phosphate of lime, 2CaO, HO, PO$_5$, with the addition of four equivalents of water of crystallization (2CaO, HO, PO$_5$ + 4 aq), viz:

2CaO, - - - - -	56·26	=	32·59
PO$_5$, - - - - -	71·36		41·34
HO - - - - - -	9·00		
4 aq, - - - - -	36·00		26·07
	172·62		100·00

In the polarizing microscope, the mineral shows a vivid succession of colors. A sample has been sent to Prof. J. D. Dana, who has kindly undertaken the study of its crystallographic characters, and I hope in a short time to be able to communicate the results of his investigations to the Academy.

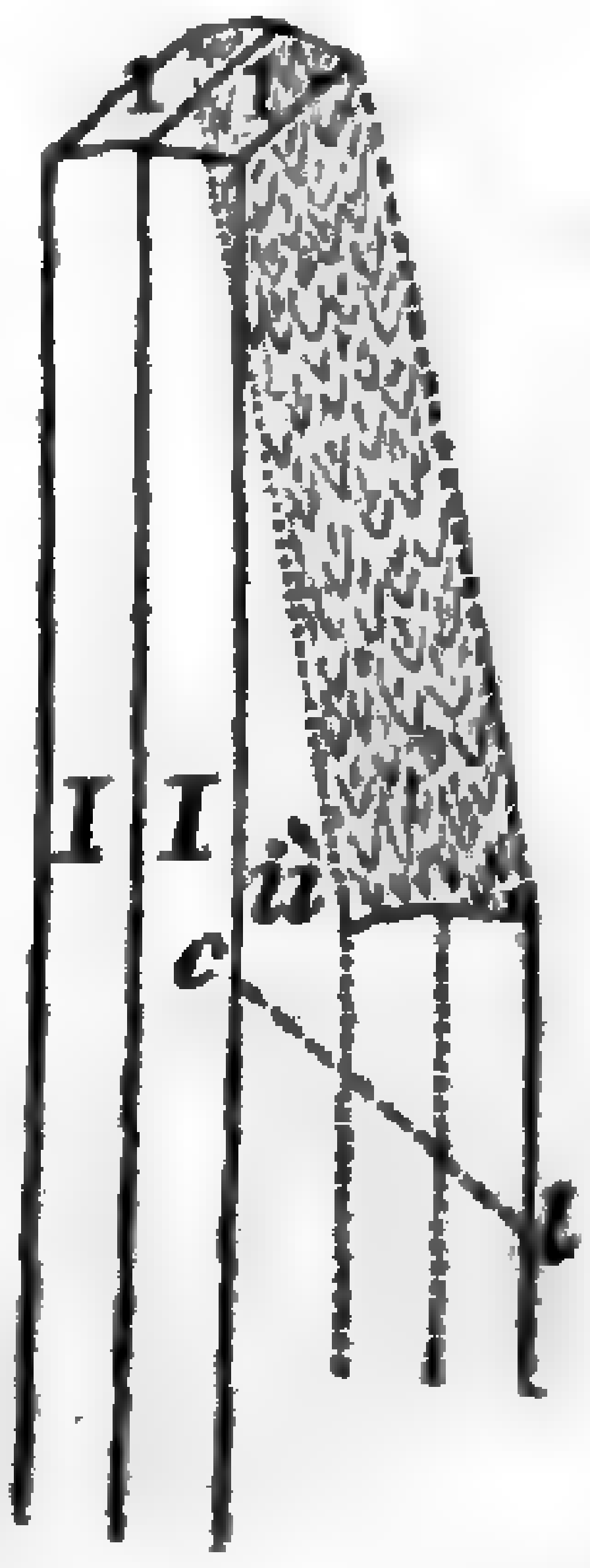
It is with great pleasure that I dedicate this species to Prof. G. J. Brush, of Yale College, to whose unwearied zeal and efficient labors American Mineralogy stands so deeply indebted.

San Francisco, Cal., Sept., 1864.

Art. VI.—*On the Crystallization of Brushite;* by James D. Dana.

(Communicated to the California Academy of Sciences.)

The crystals of the new mineral Brushite which I have had under examination were received from Mr. G. E. Moore, to whose chemical investigation science owes the first determination of the species.



The crystals are slender prisms, not over a third of an inch in length. A common form (containing all the occurring planes) is shown in the annexed figure. The prisms are monoclinic, and are often flattened parallel to the clinodiagonal, as here represented.

Cleavage is perfect parallel to the clinodiagonal section, or the plane *ii*; also distinct, parallel to the lines *cl*, as apparent often in the cross fractures of crystals, and by occasional striæ. This plane of cleavage may be called the basal, or *O*.

The planes *I* and 1 are brilliant, especially the former. The oblique plane, situated on the back side in the figure, which may be called *r*, is quite rough, owing to an oscillatory combination of two hemi-octahedral planes. In many of the crystals, only the right one of the two planes *I* is present, and also only the left one of the two planes 1. The prisms frequently terminate above in an irregular edge made by the meeting of the one or two planes *I* and the rough plane *r*, and this edge is sometimes cut off, more or less deeply, by a single oblique plane, which is one of the planes 1.

According to measurements with the reflective goniometer—

$I : I = 142° 26'$ $\quad$ $1 : ii = 101° 40'$
$I : ii = 108\ 47$ $\quad$ $1 : 1 = 156\ 20$ (approximately.)

The inclination of 1 on 1 could not be accurately measured on account of the minuteness of the planes in the crystals in which both planes occur, and the want of perfection in the reflection. The angle obtained for 1 : *ii* would give for 1 : 1 156° 40′.

By measurement with a goniometer attached to a compound microscope, the plane angle between the lines of cross cleavage, or *cl*, and the edge *I* : *I* (which equals the inclination of *O* on the orthodiagonal section or the plane *ii*) was found to be 117°–117½°; and that between edge *I* : *I* and edge 1 : 1 (which equals *ii* on 1*i*, both unobserved planes), 95° to 95½°; whence, *O* : 1*i* would equal, approximately, 147° 30′. The inclination of the rough plane *r* on the edge 1 : 1 is about 110°, but varies much.

The results of calculation, taking as data the above-mentioned angles $I:I$ and $1:i\hat{\imath}$, along with the inclination of O to ii = 117° 15′, and that of the edge 1 : 1 (or $1i$) to ii = 95° 15′, are as follows:

$$C\ (= O : ii) = 117°\ 15' \text{ and } 62°\ 45'$$

$$a \text{ (vertical axis)} : b \text{ (clinodiagonal)} : c = 0{\cdot}5396 : 1 : 2{\cdot}614$$

$$1 : 1 = 156°\ 46' \qquad -1 : -1 \text{ (unobserved planes)} = 164°\ 22'$$

The species is related in form to Vivianite, in which

$$a : b : c = 1{\cdot}002 : 1 : 1{\cdot}3843;$$

for, if we double the a of Brushite, and halve the c, we have for the ratio of its axes—

$$2a : b : \tfrac{1}{2}c = 1{\cdot}0792 : 1 : 1{\cdot}307.$$

The two species are also alike in the perfect and pearly clinodiagonal cleavage.

ART. VII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

THE *nebular hypothesis*—the boldest thought that ever elevated the human mind, by bringing us, as it were, in sight of the mysterious fiat of the Almighty—was, in its great general features, unfolded almost at the same time by Germany's deepest thinker, the Königsberg philosopher, IMMANUEL KANT, and by PIERRE SIMON DE LAPLACE, the greatest mathematician of France. It is truly the closing stone in the philosophy of the celestial vault; for Copernicus and Kepler made us behold the foundation,—the first, by placing the sun as the lantern of the world in the center, and surrounding it with the planets—the second, by destroying the cycles and unravelling the harmony of the spheres in his immortal laws; and after the existing phenomena had thus been rightly viewed, Newton made us behold the invisible bond that connects the members of the system, while at length Kant and Laplace pointed out to us the hand that at "the beginning" projected these celestial balls into space and thereby insured the continued existence of the system.

But notwithstanding this noble parentage and its being the *logical sequence* of the discoveries in the theory of cosmos made by Copernicus, Kepler and Newton, the nebular theory enjoys as yet but slight consideration among astronomers. Arago[1] is the only one of these who has deigned to consider it earnestly, and he probably did so more in his capacity as a physicist than as an astronomer.

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[1] Arago, Astronomie Populaire, ii, 7. Paris and Leipsic, 1855.

The reason of this neglect seems to be the incomplete state in which even Laplace himself left the theory. Direct observation, moreover, seemed to contradict some laws given as necessary consequences of this hypothesis.

We have already, in a former article,[2] tried to vindicate the theory in this last respect by showing that the hypothesis is really confirmed even in these apparently contradicting observations. We will now endeavor to give a somewhat more complete development to the fundamental principles of Kant and Laplace, and to exhibit the exact position of the nebular theory itself, hoping thereby to show that this theory, if we only study it earnestly and patiently both by experiment and analysis, fully deserves our confidence.

As this subject is as vast as it is difficult, we beg the critic always to keep in mind that we do not pretend to give a treatise, but merely offer an *introduction* to this almost new field of analysis.

We commence with a short survey of the fundamental principles and the aim of the theory of the solar system, in order clearly to understand why the nebular theory is necessary, what it will have to accomplish, and how far it already has done its duty.

§ 1. *The fundamental constants of the Solar system.*

As the discovery of a law of nature is but the reduction of the infinitude of observed quantities to a few constants by means of a function, the algebraic expression of the law—we see that *the progress of astronomy to a great extent must be identical with the reduction of the number of such constants.* This is fully borne out by the history of the science. For, while the Ptolemaic theory[3] of the planetary motion required the radii and inclinations of seventeen different circles to express the observed motions of Saturn, Kepler reduced this number of constants to three, the semi-major axis, eccentricity, and inclination of the orbit. This very principle is also placed by Laplace[4] at the head of his *Mécanique Céléste.*

We may therefore trust in this principle, and with Laplace try to reduce the number of indispensable constants. We must first, however, ascertain which are those constants that are now *considered* fundamental or indispensable.

The *constants of the solar system* now exclusively deduced from observation are:

1. The *mass*, m, of the planet.

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[2] The density, rotation, and relative age of the planets: this Journal, Jan., 1864.

[3] Fracastor; see Bailly, Histoire de l'Astronomie moderne; vol. i. Paris, 1779. Eclaircissements, livre iv, § 23, and livre viii, § 27.

[4] Il importe extrêmement d'en bannir tout empirisme et de la réduire à n'emprunter de l'observation que les données indispensables.—*Méc. Cél.*—*Plan.*

2. The *figure.* On assuming a primitive fluidity and a small velocity of rotation, theory gives an ellipsoidal figure of the planets; hence not independent of observation.

3. The *ellipticity* of the planet. Theory may assign to it a higher and a lower limit by means of the mass (1) and the angular velocity (6)—but though the connection of this constant with others thereby is manifest, still its exact value can only be derived from observation.

4. The *volume*, or *diameter*, of a planet. Combining this constant with the first (mass) we obtain the *density.*

5. The *plane* (or *inclination* of the axis),

6. The *direction*, and

7. The *velocity of rotation.* Though this last element bears relation to others, still its exact value can only be obtained from observation. Even if Kirkwood's law of rotation[5] should prove to be perfectly exact, this element would continue to be a fundamental constant as long as that law remains an empirical one.

8. The *distance*, *a*, of a planet from the sun, or the semi-major axis of its orbit. By the theoretically proved third law of Kepler, we get from this constant the *periodic time*, T, requiring only the *constant*, μ, *of gravitation* to be known, and this latter is the same for all planets.

9. The *plane* or *inclination*, *i*, of the orbit.

10. The *direction* of the motion. The *velocity* is given by the distance.

11. The *eccentricity of the orbit.* This constant *is* fundamental, for the theory of gravitation only proves the orbit to be a *conic section* of some kind. The eccentricity can only be found from observation.

12. The *number of satellites* of a planet is also fundamental—and for each of them the same eleven constants have to be taken from observation; the first seven even are required by the sun.

From these twelve empirical data, theoretical astronomy can deduce the motion of the corresponding planet. The whole number of empirical constants is not at all inconsiderable; for

8 principal planets,	constants	1–12,	96
80 small planets,	"	8–11,	320
23 secondary planets,	"	1–11,	253
The sun,	"	1–7,	7
Total number of constants,			676

to which the corresponding constants for the comets would have to be added.

What, in the face of this great number of constants that astronomy has to borrow from observation, shall we say about the boasted perfection of this science? Is it not in science, as in

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[5] This Journal, ix, 395, May, 1850; also xiv, 210, Sept., 1852.

morals, that self-adoration hinders progress? Can any astronomer who has not merely studied the details of the celestial mechanics, but also kept in mind the great principle laid down by its author in his "Plan"—can he still pretend that Newton's theory of the solar system merely needs further development, seeing that the few bodies of this system require him to borrow about seven hundred constants from observation?

We shall honor the memory of Newton much more by trying to go beyond the results of his labors than by stupidly worshipping[6] the same, and thus arresting the progress of that science to promote which he spent his life.

§ 2. *These fundamental constants sustain remarkable relations to each other.*

The Newtonian theory of gravitation simply accepts these constants as observation gives them. For if our earth had Jupiter's mass, the rings of Saturn, the moons of Uranus and its axis in the ecliptic, the latter perpendicular to the orbit of Jupiter, a retrograde motion in a hyperbolic orbit—it still would as fully and as beautifully confirm the theory of universal gravitation as it does now; for, let us openly and frankly acknowledge it, these constants are independent of the theory of gravitation because the latter is independent of the former.

But, though this theory does not give any reasons for any kind of dependence between the often mentioned constants, observation shows that they sustain very remarkable relations to each other;[7] or in other words, *there are relations and laws in our solar*

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[6] The literature of astronomy teems with implicit instances hereof; but we find also direct expressions of this feeling, like the following:

Enfin, nous avons vu que ces resultats eux mêmes peuvent se composer en un seul et se representer par une loi unique, celle de la Pésanteur universelle; parvenus à principe nous nous voyons en quelque sorte élévé à *la source commune de tous les faits astronomiques; tous en dérivent* de la manière la plus simple et ils y sont en quelque sorte comme concentrés. Nous avons donc pour ainsi dire décomposé le systéme du monde, *nous l'avons réduit à son élément unique*, et nous l'avons ensuite recomposé.—Biot, *Traité élém. d'astronomie physique.* Paris, 1805. Concluding remark of the work.

This "élément unique" is rather singularly unique, requiring no less than seven hundred elements to be borrowed from observation alone!

[7] Newton was aware of this—indeed, nobody can help seeing some of these relations. In the scholium to the third book of Principia he says:

"Planetæ sex principalis revolvuntur circum solem in circulis soli concentricis, eâdem motus directione, in eodem plano quamproximé. Lunæ decem revolvuntur circum terram, jovem et saturnam in circulis concentricis, eâdem motus directione, in planis orbium planetarum quamproximæ. *Et hi omnes motus regulares originem non habent ex causis mechanicis.*" Edit. *le Seur* et *Jacquier* Geneva 1749–42.

It is customary to censure Kepler's fancy in contrast to the solidity of all Newton's words; still a sentence like the above is much more objectionable in science than the boldest fancy, for the latter is not accepted without severe scrutiny, while the former is repeated as a sacred truth. If Newton had written "mechanical causes known to me" instead of by "mechanical causes," simply to imply that he knew them all, he would have prevented many a drawback that has encumbered *world of a still higher order than those deduced from gravitation.* Thus, the inclinations of the orbits of the principal planets, instead of being uniformly distributed over the first quadrant, are all very small; and their direction, instead of being as often retrograde as direct, are for *all* planets and most satellites *direct.* Instead of having the eccentricities regularly varying from 0 to ∞, we find them for all planets nearly zero! The same may be said of any of the above fundamental constants, and not least of the distance, as it is found *approximating* to Titius–Bode's law, that is, to

$$a_n \doteq 4 + 3\cdot 2^{n-1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

But quantities that sustain mutual relations to each other are the particular values of a certain function for definite given values of the variable quantities; hence, if we intend to be true to the spirit expressed by the words of Laplace above quoted, it is a problem legitimately belonging to astronomy *to find these functions of which the fundamental constants are but particular values.*

Let us boldly face this great problem and not desist though astronomers tell us that it is not part of their science.

§ 3. *The fundamental constants satisfy the conditions of stability of the system.*

Since relations exist between the values of the fundamental constants, we may ask for the most general expression of these relations. The principal of these relations are, by Lagrange and Laplace, proved to be such as to fulfill the conditions of stability of the system. These conditions are—

1. *Incommensurability of the times of rotation,* ensured by the distances forming an exponential series (1).

2. The *central mass* vastly preponderating, and the greatest masses revolving where the mutual distances are the most considerable.

3. The *direction of all motions* is the same.

4. *The plane of all orbits is and remains nearly the same,* because

$$\Sigma\, m \sqrt{a}\,.\, tg^2 i = c_1, \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

is and remains a small quantity (the letters having the same signification as in § 1).

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science. Less arrogant, because more true, he is when writing to Burnett (about 1680–81), "but yet I must confess I know no sufficient cause of y^e^ earth's diurnal motion."

Even Biot, notwithstanding his blind admiration, above cited, cannot help seeing something *more than* gravitation can account for, when noticing the harmony in the rotary and translatory motions. He says: "cet accord, *qui tient sans doute aux premières causes qui ont déterminé les mouvements planétaires, est un des phénomènes les plus remarquables du système du monde.*—Astron. physique, vol. iv, chap. v, p. 467.

5. *The eccentricity, e, of the orbit is and remains nearly the same,* because

$$\Sigma m \sqrt{a} . e^2 = c_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is and remains a small quantity.

6. The *density, d,* is such that the diameter of the bodies is small in comparison to their distances.

We may add—

7. The *form* of the planets is such that the influence of the deviation from a sphere is the smallest possible.

§ 4. *Gravitation is insufficient.*

The laws of Kepler are grand—as well as Newton's theory in accounting for them; but the above laws of Lagrange and Laplace are certainly of a superior order, and the theory of gravitation in failing to give even a shadow of a reason for these laws proves itself to be not the whole truth: we must go beyond this force!

Astronomers seem to forget the *history* of their own science; for how could they otherwise deny the legitimacy of accounting for the fact that the above laws express the stability of the world? Had not astronomers at the time of Kepler the same reason to be satisfied with his laws as astronomers have now in abiding by the laws of stability? And is it not as urgent to discover the causal connection between these laws ensuring great *duration* to our system, as it was to find in gravitation the mechanical cause of those laws ruling the spheres at the time being?

Now the hypothesis of Kant and Laplace will be found to account for the laws of stability as rigidly as the hypothesis of Newton accounts for the laws of Kepler; why, then, deride the former and adore the latter hypothesis? Or do we even forget that "the principle of gravitation" is but a "*hypothesis?*" Did not Newton himself consider it as such? Is not this force fully as mysterious and fully as much beyond the reach of direct observation as the chaos of Kant and Laplace? The former we *assume* as continually acting, because we find that *the motions* are such as this force would produce (*provided* a tangential force, of which gravitation knows nothing, also acts in a definite manner, etc., etc.). Why not also assume the latter as having been real, if we find by the same mechanical deductions that the existing harmony, as expressed in the stability of the system and ensured in the mutual relations of the fundamental constants follows directly from the above-named chaos? If in the one case we reason from fact or law to cause—why not also in the other, provided our conclusion is as legitimate?

Here astronomers will not fail to object that this last condition is *not* satisfied. We fully admit this; but beg them to remember that it has taken two centuries of labor to ensure this legitimacy to gravitation—that Newton did not leave gravitation as a mere suggestion (as such it had existence before him), but in his immortal *Principia* gave the necessary mechanical firmness to this hypothesis: how different has been the lot of Kant and Laplace's hypothesis! The first of these expounded it rather fancifully in connection with speculations on the inhabitants of distant globes;[8] the latter only gave a few bold and deep outlines of the nucleus of the theory! What would to-day be the estimation of gravitation if we, instead of Newton's *Principia*, only had a few of Hooke's sublime guesses, if these had only been considered by men like Fontenelle[9] instead of being investigated by Euler, the Bernoullis, Lagrange, Laplace, Gauss, Hansen, Plana, etc.?

Can anything be more unjust than exaltation of the hypothesis of Newton—this deservedly cherished subject of the master minds of two centuries—above the hypothesis of Kant–Laplace, which, being too early left even by its astronomical parent, has been ever since considered an outcast in the world, endangering the reputation of any one who would dare to touch it?

We will adopt this almost forlorn hypothesis as a *mere hypothesis*—we will patiently and carefully trace its bearings by means of as rigid an analysis as we can command in this most intricate field; we will minutely compare the results thus obtained with the actually observed state of things; and if we find *the correspondence between idea and phenomenon*, between analysis and observation, to be very close, we hope that those who have analysis more at their command than we, will pay as much attention to this high branch of astronomy as has been, and deservedly continues to be, bestowed on Newton's hypothesis of gravitation. If our feeble endeavors only succeed in making Kant–Laplace's *hypothesis* admitted as such among analysts, we shall have accomplished all we desire: for then this hypothesis will soon be considered as firmly established a principle as gravitation,[10] or as the fact of the rotation of the earth,[11] which latter—notwithstanding Foucault's pendulum and gyroscopes—still remains unproved by *ocular evidence*, all "demonstrations" being in fact

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[8] Allgemeine Naturgeschichte und Theorie des Himmels, 1755.

[9] Théorie des Tourbillons Cartésiens avec des réflections sur l'attraction (1752). Also his Entretiens sur la pluralité des mondes.

[10] Gravitation was always treated as a mere hypothesis by the Cartesians; the work of Fontenelle above cited offers the instance most generally known.

[11] The scientific prejudice existing against the nebular theory is perhaps as injurious as the religious prejudice once "*resisting*" the motion of the earth. See the amusing statement in the preface to the 'Dei massimi sistemi,' (ed. Padova, 1744.) that the "'Moto della Terra' non può né dee amettersi se non come pura ipotesi matematica, che serve a spiegare più agevolmente certi fenomeni." Also Galileo himself, in his Dialogue (*Opere compl.* Firenze. Vol. i, 1848, pp. 387 and 447).

inductive, reasonings with our senses. Thus, in the latter, we *see* the plane of the oscillations rotate, but *conclude* it to be the earth. It requires at least as much mental effort to apprehend its true bearing as the simple reference to the diurnal motion of sun, moon, and stars.

§ 5. *How far the nebular theory accounts for the stability of the system.*

The theory of gravitation can never, therefore, account for the stability of the solar system. How far, then, does the nebular theory explain those great fundamental conditions of the system that ensure not only the harmony of the solar world but even make this harmony (almost) permanent?

In order to invite physicists and astronomers to the perusal of the following introduction, we will try to give a simple answer to this most important question.

I. *The plane of all planetary orbits must be nearly the same* (see § 1, 9, and § 3, 4, also § 10).

This theorem has been clearly seen by Kant and Laplace; it is the most immediate expression of the hypothesis. Mr. Trowbridge has recently pointed out[12] to us a very interesting consequence hereof, viz: the most distant planet *must move in the invariable plane;* and, indeed, the inclination of the orbit of Neptune is 1° 47′, that of the invariable plane 1° 41′.

II. *The direction of all planetary revolutions must be the same;* this obvious consequence of the hypothesis accounts for § 3, 3, and § 1, 10.

III. *The eccentricity of the planetary orbits must be very small*—accounting for observation, § 1, 11, and condition of stability, § 3, 5 and § 10.

This proposition has been deduced in general reasonings by Kant and Laplace; in the following we will try to give a demonstration of it.

IV. *The planetary distances are such that the successive planets were evolved at equal intervals of time;* or if $t=1, 2, 3, \ldots$ respectively for Mercury, Venus, Earth ...., then the distance is

$$a_t = \alpha + \beta . \gamma^t, \quad \ldots \ldots \quad (4)$$

where α, β, γ are constants, and t *the age of the planets* above that of Mercury. From this follows the condition of stability that *the periodic times are incommensurable* (§ 3, 1). Besides, it is seen that this law accounts for the empirical law of Bode (§ 1, 8).

The analytical demonstration of this law is one of the principal objects attempted in the present introduction. (See § 13.)

V. *The mass of the more distant planets is the greater*, on account of the greater space from which the material of the planet was condensed (space increasing according to IV). This is confirmed

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[12] On the Nebular Hypothesis, § 24; this Journal, November, 1864, page 355.

by the fact that the sum of the *four great exterior planets* is 480 times the mass of the earth, while the sum of the masses of the *four interior planets* is but twice the mass of the earth. Furthermore, *the mass must increase toward the sun*—as the density from which the rings were formed was greater nearer the center. This is confirmed, as the mass of Neptune is 25·6, the mass of Uranus but 14·5, so that the mean of the two most distant is 20; the mass of Saturn is *five* times as great (101·6), and the mass of Jupiter, again, three times greater (339·2). The minimum in the case of the mass of Uranus is evidently produced by the simultaneous influence of both the above principles.

The mass of Jupiter is a maximum; it is so great that *the next following ring was broken up into fragments by the perturbing influence of so stupendous a mass*—thus originating the host of *asteroids*, and perhaps also the *meteorites.*

The very small mass of the interior planets as compared with the exterior ones is not astonishing, if we remember that the inter-planetary space between Jupiter and Saturn is to that between Venus and the Earth, as $10^3-5{\cdot}2^3$ to $1{\cdot}00^3-{\cdot}72^3$, or as 860 to ·63, or nearly as 1300 to 1. The mass of Jupiter is to the mass of our earth as 340 : 1, thus giving us still some margin for an increase in density toward the center of the nebula.

Being as yet unable to give the precise *theoretical law of the masses*, we are obliged to make the above few suggestions, in order to show that the nebular hypothesis at least gives a general law of distribution of the planetary masses in conformity with § 1, 1, and § 3, 2. (Prof. Kirkwood takes a similar view of the asteroids; see this Journal, 1852, xiv, 214.)

VI. *The figure of the planets* (being a *condensed* vapor) *must be an oblate spheroid* of

VII. *Small ellipticity*, because

VIII. *The velocity of rotation is but small* (compare § 1, 2, 3, 7, and § 3, 7).

This last proposition is based upon the fact that the moment of rotation is but the *difference* between the moment of revolution of the exterior and interior part of the planetary ring.

Still, *the exact amount of this velocity*, as well as the period of rotation of the sun, has not yet been deduced from the nebular hypothesis; we have often attempted it, but as yet have not been able to solve this difficult problem.

Prof. Kirkwood has found[13] the empirical law of the velocity of rotation, *a law analogous to the third law of Kepler.* We have repeatedly arrived at expressions similar to (but not identical with) Kirkwood's law.

IX. *The Plane;* and

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[13] As above, this Journal, 1850, ix, 395.

X. *The direction of rotation* of the planets has been considered at variance with the nebular theory, ever since the discovery of the lunar system of Uranus. We believe that our analysis of this problem[14] shows that the rotation of Uranus and Neptune, both as to position of the axis and direction of motion affords a very interesting confirmation of the theory. See § 1, 5, 6.

XI. *The density* of the planets has also been considered as being adverse to the theory; but if, as necessary, the influence of the age is taken into account, it is found that the minimum density exhibited by Saturn is demanded by the theory.[15] Compare § 1, 4, and § 3, 6.

XII. *The number of satellites* was already shown, by Kant, to increase with the distance from the center of the nebula. Though not usually given as such, it nevertheless is a condition of stability of the system—at any rate it is conformable to observation (§ 1, 12).

The rings of Saturn are best considered as a host of satelloids, corresponding to the planetoids (and meteorites) of the solar world—thereby accounting for the excessive thinness and the subdivisions of the rings.[16]

In looking back upon the preceding account of the present aspect of the nebular theory, it will be seen

A. That the four great fundamental conditions of stability referring to *the system at large* are now satisfactorily deduced from the hypothesis of Kant–Laplace (I–IV above).

B. That the problem of the *mass* (V) and the *number of satellites* (XII), though not completely evolved, still is sufficiently comprehended to enable us to say that the analytical solution is *possible;* and

C. That the elements referring to the *single planets*, or rather their subordinate systems, are, with the exception of the *exact* law of rotation (VIII), fully deducible from the fundamental hypothesis of Kant and Laplace.

We see, then, that the *fundamental constants* of the solar system, which number about seven hundred (§ 1), exhibit very remarkable mutual dependencies (§ 2), which are such as ensure the permanence or stability of the system (§ 3), which Newton's law of gravitation cannot account for (§ 4). Though they offer a higher problem for theory than Kepler's laws, astronomers have hitherto been unwilling to recognize the analysis of the above conditions of stability as part of their science. Laplace, while instrumental in bringing to light the great laws of the stability of the system, independently reproduced the bold hy-

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[14] On the density, rotation, and relative age of the planets; this Journal, 1864, xxxvii, 36, 48.

[15] As above, p. 49.

[16] On the density, etc.; this Journal, 1864, xxxvii, 54.

pothesis of Kant, and though this has been most grievously neglected by analysts and astronomers, still it now affords us a full solution of the four great harmonies ensuring the permanency of the solar world, and also solves most, and at least indicates the solution of, all other problems relating to the harmony of the fundamental constants of the solar system.

May we not hope that astronomers will begin to bestow on this theory some share of their labor?

§ 6. *The Hypothesis.*

We assume, with Kant and Laplace, as the primitive condition of the solar system, or as *nebula:*

The space of the solar system was filled with matter having a moment of rotation.

This *matter* is endowed with the same forces we know it to possess; a simple calculation will furthermore show that it was a highly rare vapor. Its chemical constitution we will leave out of consideration for the present; we therefore consider it as composed of the elements we know here on earth, many of which we *now* know to be found on the sun, and are probably also on the distant stars;[17] still there can be no doubt but that many more elements exist than we are acquainted with. Many of the spectral lines even of our own central star are irreducible to spectra of known elements. We therefore mean simply to say that at the above primitive period the *elements had been created.* I hope at some future time to publish an attempt at a *mechanical theory of the elementary bodies,* which has occupied my time for about ten years, and wherein I endeavor to show the physical properties of the known elements to be *definite functions* of their *atomic number and form.* Accordingly, there would yet remain a more primitive condition, the existence of the *one primitive matter* (Urstoff) which would be considered as the direct creation of the *one* God.

The *rotation* of the nebula is *not* to be thought regular, but simply amounting to a certain momentum. I have elsewhere[18] tried to show that such rotation may be considered as the *effect of a difference of any kind between the primitive forces of attraction and repulsion* wherewith we know matter to have been endowed.

If, therefore, these views should be well founded, we should have arrived at the grandest principle we can conceive of in the present state of our knowledge; we should be able to see how from *created matter alone the whole of the solar system has been developed;* we would be enabled to conceive the almighty *fiat* at

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[17] Rutherford, Astronomical Observations with the Spectroscope; this Journal, 1863, xxxv, 71. Above all, Bunsen's and Kirchhoff's memoirs on their great discovery.

[18] This Journal, 1864, xxxvii, 52.

one single act. How much such a theory would tend to elevate our conceptions of the great Author, we cannot here develop.

In the present paper I shall not go farther back in time than to the existence of the nebula of Kant and Laplâce as above defined.[19]

§ 7. *Plateau's experiment.*

Before entering upon the analysis of the nebula, we must refer to the experimental evidence of the nebular theory afforded by the beautiful experiments of Plateau, detailed in his *Mémoire sur les phénomènes que présente une masse liquide libre et soustraite à l'action de la pésanteur*, Pt. I (Neuv. mém. de l'Acad. de Bruxelles, vol. xvi, 1843). His results are:

1. A liquid, subject only to the action of its molecular forces assumes the form of a perfect sphere (§ 2).

2. This globe is flattened at its poles, if subject to rotation (§ 10). Although, as he thinks, the molecular forces are not *identical* with those acting in the nebula, still the results ought to be analogous, if they are not identical.

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[19] It is perhaps not out of place here to give a synopsis of the different distinct *ages* that are characterized by a further individualization or a new direct creation according to the views above indicated.

Three (or four) direct acts of the Deity may be recognized, viz: the creation of *matter*, of *life*, of *mind* (and the *redemption*). The formation of the *elements* out of matter characterizes the *first age;* the formation of the *solar world*, with its planets, moons and central sun, the *second age;* while the *third age* beheld the development of our *earth* from a vaporous ball to its present shape; in the *fourth age, life* was created in the form of *plants* and *animals;* in the *fifth age*, mind, the investigating mind, was introduced by the creation of *man*, the *cephalized animal;* and in the *sixth*, redemption took place; while a *seventh age* will behold the destruction of the whole system, occasioned by the *extinction* of the solar body and the *resistance* of ether. (See this Journal, 1864, xxxvii, 56.)

To *every age* correspond two sciences: the first relates to the development—Whewell would call it the *Paletiology* of the age—while the second relates to the actually *existing product* of the development or creation of that age, i. e., *the* science. Thus we obtain the following general view of the natural sciences:

Age	1	2	3	4	5	(6)	(7)
	I. *Creation of* MATTER. Development of			II. Creation of *Life.*	III. Creation of *Mind.*	*Redemption.*	Destruction of the world.
	Elements.	*Solar system.*	*Earth.*				
Paletiology	Atomology	Planetology.	Geology	Paleontology.	Archeology.	Old Testament	
Science.	Physics, Chemistry.	Astronomy.	Geography.	Botany, Zoology	History.	New Testament	

These names have of course to be taken in their widest sense; thus geography stands for physical geography, meteorology. etc., and history comprehends not only political but also the intellectual history of the human race, thus including again *all* the sciences in their historical development. We see how "planetology" is allied to geology and astronomy.

3. If the rotation becomes sufficiently rapid, the *whole globe* is transformed into a *ring* in the equatorial plane (§§ 11–14) which, by continuing the rapid rotation of the large central disc, even loses its regularity, and separates into small masses which immediately assume globular forms. "But," continues he (§ 19), "this is not all; one or more of these spheres are always seen to take, at the instant of formation, a motion of rotation on their own axes, and this motion is almost always in the same direction as that of the ring."[20] He even found that still smaller globes were formed.

4. If a small disc is put into very rapid rotation, a ring is formed, while a part of the original globe remains on the axis (§ 21), so that Plateau rightly concludes with saying (§ 27) that most of the phenomena of the relative configuration of the heavenly bodies have been reproduced by him on a small scale."[21]

[*To be continued.*]

ART. VIII.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School;* by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.—No. 2.

§ 1.

On the separation of chromium from aluminum, iron, manganese, cobalt, nickel, zinc and magnesium.—Sesquioxyd of chromium in an alkaline solution is readily oxydized to chromic acid by means of chlorine, bromine or deutoxyd of lead. When chlorine or bromine are employed as oxydizing agents, the alkaline solution may be neutralized by acetic acid, after the oxydation is complete, and the chromic acid may then be precipitated by acetate of barium, when the solution is free from sulphuric acid, and directly weighed in the form of chromate of barium. In place of free alkali it will be found in practice very much more convenient to employ acetate of sodium or potassium. When a solution of sesquioxyd of chromium is rendered nearly neutral by a solution of carbonate of sodium, and acetate of sodium is added in excess, a current of chlorine gas, or a solution of chlorine water, readily converts the whole of the chromium present into chromic acid, especially when the solution is hot, and when it is kept nearly neutral by occasional addition of carbonate of sodium. The excess of chlorine is easily expelled by boiling,

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[20] Of course; for in this oil the density is the same. See our article, this Journal, 1864, xxxvii, 51, $\delta=0$.

[21] Malgré la difference des lois que suivent les forces attractives dans ce cas et celui des grandes masses planétaires, nous avons vu se produire, en petit, une représentation frappante de la pluspart des phénomènes de configuration relatifs aux corps célestes.

after which, in the presence of bases not precipitated by ammonia, the chromic acid may be precipitated by acetate of lead, or acetate of barium, and weighed in the form of chromate, provided, of course, that no sulphuric acid is present. When sulphate and chromate of barium are thrown down together, the chromic acid may be reduced to sesquioxyd by boiling with concentrated chlorhydric acid and alcohol, after which the barium may be precipitated by sulphuric acid, and the sesquioxyd of chromium thrown down in the filtrate by boiling with ammonia in the usual manner. As the reduction of chromate of barium by means of chlorhydric acid and alcohol does not take place very readily, it is better to boil the chromate with an excess of carbonate of potassium or sodium, to filter off the carbonate of barium, and determine the chromic acid by means of nitrate of suboxyd of mercury, or by reduction to oxyd of chromium and precipitation with ammonia in the usual manner.

When aluminum and iron are to be separated from chromium by this process, the two oxyds may be precipitated together by simply boiling, the solution after the complete oxydation of the chromium to chromic acid, in the presence of excess of acetate of sodium. It is more convenient and equally accurate to neutralize the solution with ammonia, separate the alumina and sesquioxyd of iron by filtration, and determine the chromium in the filtrate by reduction and precipitation with ammonia.

When the oxyds of calcium, magnesium, zinc, nickel, cobalt and manganese are present in a solution containing sesquioxyd of chromium, it is best to oxydize the chromium to chromic acid as above, and then to precipitate with acetate of lead or barium.

I have stated in a former paper that chromic iron ore may be completely resolved by fusion with fluohydrate of fluorid of potassium. In this and in all similar applications of the fluohydrate, it is best to evaporate the finely pulverized mineral to dryness with a concentrated solution of the salt. On subsequently heating to low redness, the resolution of the mineral is effected with the utmost ease, a portion of the chromium being usually oxydized to chromic acid by the oxygen of the air. After expelling the fluorine by heating the fused mass with sulphuric acid, the remaining mass may be treated with acetate of sodium and chlorine in the manner already pointed out, the iron and aluminum separated by boiling, and the chromic and sulphuric acids precipitated by acetate of barium, after which the chromium may be determined as above.

In precipitating chromic acid by means of nitrate of suboxyd of mercury, hot solutions must not be employed, as a small portion of chromic acid is always reduced to sesquioxyd of chromium. The precipitated chromate should be allowed to stand some hours before filtering. In general, the precipitation by acetate of lead or acetate of barium is to be preferred even when the resulting chromate is to be weighed as such.

§ 2.

On the employment of acetate of sodium for the separation of iron and aluminum from other bases.—The facility with which iron and aluminum are precipitated from neutral solutions of the sesquioxyd, by boiling with acetate of potassium or sodium, has led to frequent analytical applications, though the method is not so generally employed as it deserves. Mr. C. F. Atkinson has devoted much time to a careful study of the subject, and has arrived at the following results, which appear to me worthy of attention. The sesquioxyds of iron and aluminum may be perfectly separated from the protoxyds of manganese, cobalt, nickel, zinc, magnesium and calcium, and from sesquioxyd of uranium, by boiling the neutral or nearly neutral solutions with acetate of sodium, provided that the following precautions are observed. The solutions from which the sesquioxyds are to be precipitated must be dilute: half a liter of the solution should not contain more than one gram of either sesquioxyd or of the two when both are present. The quantity of acetate of sodium should be sufficient to convert by double decomposition all the bases present into neutral acetates. The acetate should be added to the metallic solution when cold and the whole should then be heated together and boiled for a short time. It is not necessary to filter upon a water-bath funnel, but the beaker containing the solution should be kept nearly at the boiling point during filtration, and a ribbed filter should be employed. In all cases it is best to add a few drops of free acetic acid to the solution, to prevent the formation of basic acetates of the protoxyds. This is especially necessary in separating iron and aluminum from zinc and nickel.

Finally, it is best, whenever it is possible, to have all the bases present in the form of chlorids. The iron and alumina upon the filter in the form of basic acetates must, whenever an absolutely complete separation is necessary, be redissolved in chlorhydric acid and again precipitated by boiling with the acetate after rendering the solutions nearly neutral by means of carbonate of sodium. In this manner only is it possible to separate the last traces of the stronger bases. Finally, the basic salts of the iron and aluminum, after washing, must be redissolved in chlorhydric acid, and precipitated by boiling with ammonia in the usual manner, to free them completely from alkali. The precaution of a second treatment with acetate of sodium is more necessary with alumina than with sesquioxyd of iron alone. It is scarcely worth the trouble in the separation of iron from calcium and magnesium.

According to my own observations, the sesquioxyds of iron and aluminum cannot be separated from sesquioxyd of chromium by boiling with acetate of sodium, although the last mentioned oxyd is not precipitated when alone in solution. In this case it is necessary to oxydize the chromium to chromic acid by chlorine in the manner already pointed out.

§ 3.

On the separation of manganese from cobalt, nickel and zinc.—Schiel's method of separating manganese from the alkaline earths by adding acetate of sodium to the mixed solutions, heating the liquid gently and then passing chlorine through it so as to convert the manganese into a hydrate of the sesquioxyd, is better than that formerly given by myself in which peroxyd of lead is used as the oxydizing agent. With respect to Schiel's method, however, it must be remarked that it cannot be employed to separate manganese from nickel or cobalt, because both of these metals are converted into higher oxyds under the same circumstances. Nickel may, as Popp has recently shown, be completely precipitated as a deep blue hyperoxyd, while as I have myself observed, cobalt is also oxydized, though not precipitated, unless the solution is boiled with an alkaline carbonate. In separating manganese from zinc, calcium or magnesium, I have repeatedly found that a second treatment is necessary in order to obtain a perfect separation. This second treatment may be neglected in separating manganese from calcium and magnesium, but not in separating it from zinc, although the addition of a few drops of free acetic acid renders the process more exact.

Though the method of separating manganese from other bases by means of peroxyd of lead, which I formerly proposed, will hardly be used in future, now that we are in possession of more convenient processes, it will still be of some interest to chemists to know the precise nature of the insoluble black compound which is formed when peroxyd of lead, PbO_2, is digested or boiled with an excess of a solution of chlorid or nitrate of manganese and afterward thoroughly washed. An analysis of this body, made some years since in my laboratory by my lamented friend and former pupil, Mr. Theodore Parkman, gave the following results:

		Anhydrous.	Theory.
Manganese, - - -	35·10	37·96	37·53
Lead, - - -	32·49	35·13	35·26
Oxygen, - - -	24·87	26·90	27·20
Water, - - -	7·52	....	....
	100·00	100·00	100·00

Neglecting the water, which may have been, in part at least, mechanically combined, and which amounts to between three and four equivalents, we have the formula, MnO_2+4PbO_2, as the simplest expression of the results of the analysis.

A simple and perfectly satisfactory process for separating manganese from cobalt, nickel, and zinc, is the following: To the neutral or nearly neutral solution of the chlorids, acetate of sodium is to be added in excess together with a few drops of free acetic acid. The solution is then to be boiled, and a rapid current of sulphydric acid gas passed through it while boiling and continued for half an hour. Every trace of cobalt, nickel and zinc, is precipitated in the form of sulphid, while the whole of the manganese remains in solution. The precipitate is to be thrown upon a ribbed filter and quickly washed with cold water saturated with sulphydric acid gas. It is easily washed, and though the sulphids of cobalt and nickel precipitated in this manner are far more easily oxydized than when precipitated by boiling sulphid of sodium from boiling solutions, they will be found to present no difficulty as regards oxydation upon the filter. Manganese may then be determined in the filtrate by boiling with chlorhydric acid and precipitating in the usual manner with carbonate of sodium. The mixed sulphids upon the filter, supposing for the sake of generality that all three are present, are to be dissolved in chlorhydric acid and the metals converted into double cyanids by means of an excess of cyanid of potassium, after which the zinc may be precipitated by means of sulphid of sodium, as recommended by Wöhler.

When perfectly pure cyanid of potassium is not at hand, the following process will be found particularly convenient. Acetate of sodium is to be added to the solution of the mixed chlorids, after which the vapor of cyanhydric acid generated in a flask from sulphuric acid and ferrocyanid of potassium is to be passed directly into the solution. Cyanid of zinc is immediately precipitated more or less completely as a perfectly white powder. A solution of sulphid of sodium is then to be added as long as a precipitate is formed, after which the sulphid of zinc is to be separated by filtration. Cobalt and nickel remain in solution as double cyanids. The same process may be used to separate manganese from cobalt and nickel, sulphid of sodium throwing down under these circumstances a pure flesh red precipitate. It is easy to see that zinc and manganese together may be separated from cobalt and nickel by the same process and at one operation. No cyanid of manganese appears to be formed when cyanhydric acid is passed into a solution containing a salt of manganese, acetic acid, and acetate of sodium.

I have stated in a former paper[a] that the sulphids of nickel and cobalt are thrown down from boiling solutions by a boiling solution of sulphid of sodium in an insoluble form, so that in

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[a] This Journal, March, 1864.

fact even strong chlorhydric acid scarcely exerts upon them an appreciable action. This process has been applied to the separation of cobalt and nickel from zinc and manganese by my excellent assistant, Mr. Maurice Perkins, and gives results which are very satisfactory, especially for qualitative purposes, the sulphids of manganese and zinc precipitated under the same circumstances being readily soluble, even in dilute acid. The process is now substituted in this laboratory for that given in most of the recent works on qualitative analysis, and has been repeatedly tested with satisfactory results.

§ 4.

On the separation of cobalt from nickel.—A method of separating these metals, given some years since by Liebig,[4] consists in boiling the mixed double cyanids of nickel and potassium and cobalt and potassium with oxyd of mercury. Oxyd of nickel is precipitated, while an equivalent quantity of mercury is dissolved as cyanid. The method certainly gives good results but is not free from objection. Long boiling is necessary before the precipitation is complete, and it is difficult to prevent "bumping" during ebullition. The excess of oxyd of mercury must be separated from the oxyd of nickel by a special operation, and the nickel afterwards again precipitated by caustic alkali.

These inconveniences may be completely avoided by employing, instead of the oxyd alone, a solution of the oxyd in the cyanid of mercury. When this solution is added to a hot solution of the double cyanid of nickel and potassium, the whole of the nickel is immediately thrown down as a pale green hydrate of the protoxyd. Under the same circumstances cobalt is not precipitated from the double cyanid of cobalt and potassium. Mr. W. N. Hill, who has repeatedly employed this method and carefully tested it, has found that the separation effected is complete. No cobalt can be detected in the precipitated oxyd of nickel by the blowpipe, nor can nickel be detected in the cobalt (finally separated as oxyd) by Plattner's process with the gold bead. The solution of oxyd of mercury is easily obtained by boiling the oxyd with a strong solution of the cyanid, and filtering. According to Kühn[5] the cyanid formed in this manner has the formula $HgCy+3HgO$. The hydrated oxyd of nickel precipitated may be filtered off, washed, dried, ignited and weighed. The cobalt is more readily and conveniently determined by difference, when, as is always possible, the two metals have been weighed together as sulphates. I am not prepared to say that this modification of Liebig's method of separating nickel and cobalt gives better results than Stromeyer's process by

[4] Ann. der Chemie und Pharmacie, lxv, 244.

[5] Berzelius, Lehrbuch der Chemie, iii, 872.

means of nitrite of potassium, but it is at least very much more convenient and requires much less time. The complete precipitation of cobalt in the form of $Co_2O_3,2NO_3+3KONO_3$ usually requires at least forty-eight hours, and rarely succeeds perfectly except in experienced hands.

§ 5.

On the separation of uranium from zinc, cobalt and nickel.—The method which I have given above for the separation of manganese from cobalt, zinc, and nickel, by precipitating the sulphids of the three last named metals, by means of sulphydric acid gas, from a boiling solution of the acetates, may be also used, according to the carefully conducted experiments of Mr. Perkins, for the separation of uranium from the same metals. The process is in all respects the same, and requires, therefore, no further description. It will be found much simpler and more convenient than that described by Rose, by means of carbonate of barium.

§ 6.

On the electrolytic precipitation of copper and nickel as a method of analysis.—The precipitation of copper by zinc, in a platinum vessel, with the precautions recommended by Fresenius, leaves nothing to be desired, so far as accuracy, ease and rapidity of execution are concerned. The method labors, however, under a single disadvantage: the introduction of zinc renders it difficult, or at least inconvenient, to determine with accuracy other elements which may be present with the copper. It has occurred to me that this difficulty might be overcome, the principle of the method being still retained, by precipitating the copper by electrolysis with a separate rheomotor. The following numerical results, which are due to Mr. E. V. M'Candless, will satisfactorily show the advantages of the method for the particular cases in which it is desirable to employ it. The copper was, in each case, in the form of sulphate; the deposition took place in a small platinum capsule, which was made to form the negative electrode of a Bunsen's battery of one or two cells, in rather feeble action. The positive electrode consisted of a stout platinum wire, plunged into the surface of the solution of copper at its center. The following table gives the results obtained in the analysis of pure sulphate of copper:

Number.	Salt taken.	Copper found.	Percentage.
I.	1·2375	0·3145	25·41
II.	0·4235	0·1075	25·38
III.	1·0640	0·2705	25·42
IV.	1·3580	0·3440	25·33
V.	0·5665	0·1450	25·59
VI.	0·4735	0·1205	25·48

In seven determinations of copper in the alloy of copper and nickel employed by the government for small coins, the following results were obtained:

Number.	Weight of alloy.	Copper.	Percentage.
I.	0·4160	0·3640	87·50
II.	0·6180	0·5410	87·54
III.	0·4600	0·4090	88·91
IV.	0·5120	0·4481	87·51
V.	0·4220	0·3693	87·51
VI.	0·2525	0·2225	88·11
VII.	0·3705	0·3255	87·85

The percentage of copper required by the formula CuO, SO_3+5HO is 25·42, while the government standard alloy of nickel and copper contains 87·50 per cent of copper. The time required for precipitation varied from one to three hours, the separation of the last traces of copper being in each case determined by testing a drop of the liquid upon a porcelain plate with sulphuretted hydrogen water. The copper, after precipitation, was washed with distilled water, dried in vacuo over sulphuric acid, and weighed with the platinum vessel. The only precaution necessary is to regulate the strength of the current so that the copper may be precipitated as a compact and bright metallic coating, and to dry as quickly as possible. When the copper is thrown down in a spongy condition, it not only oxydizes rapidly, but it is impossible to wash out the last traces of foreign matter contained in the solution. This is well shown by number III and number VI of the second series, in both of which cases the copper was precipitated too rapidly. The solution from which the copper has been deposited contains the other elements present in the original substance. It may be easily poured off without loss, and the washings added.

It appears at least probable that nickel may be determined by electrolysis in the same manner as copper, the solution employed being the ammoniacal sulphate with excess of free ammonia. Mr. M'Candless obtained in two determinations in a commercial sample 91·36 and 91·60 per cent nickel. In both cases the nickel was thrown down completely as a bright coherent metallic coating upon the platinum.

Cambridge, Oct. 1st, 1864.

Art. IX.—*Note on the Planetary Distances;* by Prof. Daniel Kirkwood, Indiana State University.

To obviate any misappreheusion that may have existed in regard to the order of planetary distances proposed in this Journal for July, 1864, p. 12, it may be proper to state that by the term "radii of gyration of the primitive rings," was meant the radii of gyration of homogeneous circular rings, or, in other words, the radii of the circumferences which bisect the circular intervals between the orbits of Neptune and Uranus, Saturn and Jupiter, &c. The formula was given as empirical, in the hope that it might *suggest* a true law of nature, if not found itself in strict harmony with observation.

In the article referred to, the writer suggested the hypothesis of the contemporaneous formation of the different members of our planetary system. Some additional considerations bearing on this subject may not be destitute of interest.

The radius of gyration of the sun, regarded as a homogeneous sphere, is 0·002936; the unit being the mean distance of the sun from the earth. If we designate by K the principle radius of gyration when the rotation period was the same as Neptune's orbital revolution, we shall have

$$25^{d}\cdot325 : 60126^{d}\cdot7 :: (0\cdot002936)^2 : K^2 ; \text{ whence } K = 0\cdot143.$$

But the radius of gyration of a *homogeneous* sphere filling Neptune's orbit is 18·974. Hence we infer that before the formation of the planets the condensation of the solar spheroid had advanced much more rapidly about the center than toward the surface.[1] The consequent tendency to unequal velocities of rotation would produce a divellent force, thus separating the nucleus from the outer parts, and also breaking up these exterior portions into distinct zones or annuli. As subsequent condensation would, to some extent, increase the rotary velocity of the nucleus, the primitive circle of division between the latter and the surrounding nebulous mass must have been somewhat exterior to the present circle of equilibrium between the centripetal and centrifugal forces. The points in the revolving nebulous matter at which the planetary formatious would originate, were determined by certain conditions which in the present state of our knowledge we are unable to define. It is not, however, an improbable assumption that the intervals between the orbits of such nuclei would be arranged in regular order. This, in fact, is found to be the case, at least approximately, in regard to Ju-

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[1] This fact, together with its cause, as the writer has recently learned, was stated by Prof. Stephen Alexander, at the Montreal Meeting of the Am. Assoc. for the Adv. of Sci.

piter's satellites. The differences between the radii of the orbits, commencing with the greatest, are,

11·64811, 5·72677, and 3·57494.

But while the first is almost exactly double the second, the second is less than double the third. In other words, as in the case of Mercury, the theoretical distance of the innermost satellite is somewhat greater than the actual distance. Now, if the *original* intervals constituted an exact geometrical progression, can a probable reason be given for the *present* deviation? We reply (1) that in the case of the primary system considerable masses of meteoric matter are believed to be still revolving within the earth's orbit; portions of which are exterior to Mercury, and some even beyond the orbit of Venus; (2) that if the Nebular Hypothesis be true, the primitive constitution of the secondary systems was probably similar; (3) that in any system an increase of mass in the central body would diminish the orbits of the satellites; and (4) that the contraction of meteoric rings until portions originally *exterior* to a given orbit had become *interior*, would obviously have the same effect. In the manner thus indicated, the distance of the innermost satellite may have been diminished, and thus the original interval between the first and second orbits increased. The same explanation applies to Mercury.[2]

With an interval = 11·64811 between the third and fourth satellites, and a ratio = $\frac{1}{2}$, we find 3·70213 as the limit at which the central body separated from the exterior mass. This is a little beyond the present circle of equilibrium between the centrifugal and the centripetal forces, the radius of this circle being 2·299.

THE SATURNIAN SYSTEM.

In the primary system[3] and also in that of Jupiter, we have found that the original circle of separation between the nucleus and the surrounding mass was a little exterior to the present circle of equilibrium between the two central forces. This is what ought to be expected, as the contraction of the nucleus would accelerate the rotation. For Saturn, however, we found the primitive circle of equilibrium *less* than the present;[4] but in this calculation no allowance was made for the contraction of

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[2] This explanation, it must be confessed, is apparently inconsistent with the conclusions of Leverrier in regard to Mercury's motion. The distinguished director of the Paris Observatory "has found that Mercury's mean motion has gone on diminishing; as if the planet were, in the progress of his revolutions, receding further from the sun. This is explained, if we suppose that there is, in the region of Mercury, a resisting medium which moves round the Sun in the same direction as the Planets move."—*Whewell's Hist. of the Ind. Sci.*, 3rd ed., vol. i, p. 560.—Perhaps no satisfactory determination of the facts can be reached without long continued observations.

[3] This Journal for July 1864, p. 13.

[4] Ib., p. 15.

the orbits of the innermost satellites. If the radius of gyration for Mimas and Enceladus was diminished in a ratio equal to the mean of those obtained for Mercury and the innermost satellite of Jupiter, it was originally 3·8720. Adopting this value and interpolating the two missing terms, we have the following elements conforming to our hypothesis:

SATELLITES OF SATURN.

Names.		Distances.	Rad. of Gyr. of Intervals.	Intervals.
I.	Japetus,	64·359	54·9764	
	———	43·6203		32·0070
II.	Hyperion,	25·029	22·9694	
	Titan,	20·706		12·4154
III.	———	11·958	10·5540	
	Rhea,	8·932		4·8150
IV.	Dione,	6·399	5·7390	
	Tethys,	4·9926		1·8670
V.	Enceladus,	4·0319	3·8720	&c.
	Mimas,	3·1408	&c.	
		&c.		0·0000
			Limit=2·6862	

The ratio of the ascending series of intervals is 2·578+. These ratios in the systems of Jupiter and Saturn are to each other inversely as the orbital velocities of the two planets. The distance from the center of Saturn at which a satellite would complete its orbital revolution in the present period of the planet's rotation is 2·0075. The distance at which the nucleus originally separated from the surrounding mass has been found (theoretically) somewhat greater, as it ought to be, viz., 2·6862.

If, then, the arrangements of the Saturnian System should not be admitted as confirmatory of the empirical order of distances, we may at least conclude that it is not incompatible with it. The *exact* coincidences are of course produced by the interpolation of the two terms. With these, our formula gives three equations with three unknown quantities, and, consequently, whatever the distances of the known satellites, the roots of these equations, whether real or imaginary, must, in an algebraic sense, satisfy the conditions. At the same time it is easy to perceive that these algebraic results *might be* decidedly unfavorable to the proposed hypothesis.

The tendency in a rotating nebula to unequal angular velocities, resulting from the increased rapidity of condensation from the equator toward the center, may, perhaps, also account for the phenomena of spiral nebulæ. If, in a contracting mass of vapor, a free motion of the particles among themselves be established before the centrifugal force becomes equal to the centripetal, a spiral convergence, like that of No. 51 in Messier's catalogue, would naturally ensue.

Finally, if the original constitution of the Solar and Saturnian Systems was such as we have supposed, can any probable reason be given why the satellites of Jupiter should be found an exception?[*] It may be worthy of remark that if these bodies, or the rings from which they were derived, were originally double, the proximity of the members was such that they might be brought into collision by perturbation, while in the gaseous state. The ratio, moreover, of the ascending series of intervals is considerably less than in the case of Saturn, and much less than in the primary system.

ART. X.—*Caricography;* by Prof. C. DEWEY.

(Continued from vol. xxxv, p. 60, 1863.)

No. 281. *Carex conjuncta*, Boott, Illust. No. 282.
—— *vulpina*, Sullivant, Carey, Dewey.

Spica composita oblonga; spiculis ovatis sessilibus superné staminiferis et aggregatis, inferné laxis sæpe sub-ramosis et sub-remotis, bracteatis; fructibus *distigmaticis* ovatis acutis brevirostratis bidentatis stipitatis et interdum sub-cordatis margine subscabris divergentibus, squamam ovatam acutam paulo superantibus; spica plantaque pallido-viridi; culmo 1–3 pedali late-triquetri, cum foliis sub-radicalibus et longis.

This form was discovered some years since in Ohio, and named by Mr. Sullivant, after the European plant, C. vulpina of Linnæus. So it was held to be by Mr. Carey and others, and under that name described in this Journal, vol. viii, p. 348, 1849. Dr. Boott, however, thought it differed from C. vulpina, and gave it the name above. The figures in the Illustrations, No. 282, were taken from plants collected near Columbus, Ohio, and fully resemble those from Menard Co., Ill., now before me.

In the Illustrations, Dr. Boott gave the following particulars in which this plant differs from the European *C. vulpina*, viz: a very "acute-angled and flaccid culm," sheaths of the leaves transversely corrugated, spike more lax and pale, inflorescence of a pallid color, and its general appearance and habit. These or similar characters show the great difference between C. cephalophora and C. Leavenworthii, and doubtless should be considered important and distinguishing, even when, as in this case, the characters taken from the inflorescence are so nearly the same.

This species resembles C. sparganioides, but its *nerved* fruit separates it; as does the form of the fruit from C. stipata. The specimens of C. vulpina, *L.*, from England, Germany, and Swe-

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[*] For some suggestions in regard to Mercury, see this Jour. for July, 1864, p. 12.

den, have an inflorescence more *compact* and *reddish brown;* and the plant generally has a *deeper green*, and is also *stouter*.

282. *C. glabra*, Boott, Illust. No. 229.

Spicis distinctis cylindraceis pedunculatis; terminali staminifera, interdum ad apicen vel basin pistillifera, vel in medio; spicis pistilliferis, 3–4, sub-laxifloris, bracteatis, infernè longo-pedunculatis et nutantibus; fructibus *tristigmaticis* ovalibus sub-inflatis et infernè teretibus, supernè conicis brevi-rostratis, bidentatis glabris nervosis, squamam oblongam sub-acutam vel lanceolatam multo superantibus; planta pallida et glabra.

Culm 1½ foot high, slender, erect, leafy toward the root; staminate spike slender, sometimes with a few fruit at the apex, in the middle or at the base; pistillate spikes 3–4, cylindric, slender, rather loose flowered, on slender peduncles with sheathing bracts which equal the culm nearly; stigmas 3; fruit oval, tapering below and conic above, smooth and glabrous, nearly twice longer than the oblong acutish scale which is white on the margin and green on the back: whole plant light green.

This plant is the well-known *glabrous* form of C. flexuosa, *Schk.*, the C. debilis, *Mx.*, and blended with it till Dr. Boott separated them in 1860. More than 40 years ago it was in our country commonly labelled from fig. 124, Schk.

283. *C. Magellanica*, Lam. 1789, and Schk., 1802. Boott, No. 199.
C. limosa, var. irrigua, Wahl. 1803; Dewey, 1826.
C. paupercula, Mx. 1803, and Torrey, 1843, and Boott.
C. irrigua, Smith, English Botany, 1845.

Spicis pedunculatis cylindraceis vel brevi-oblongis ferrugineis; terminali vulgo staminifera brevi, raro apicen pistillifera; spicis pistilliferis 1–3, subremotis recurvo-pedunculatis, bracteatis, laxifloris, interdum basin stameniferis; fructibus, *tristigmaticis*, longo-rotundis obovatis sub-triquetris vix rostratis ore integris stipitatis, squama ovato-oblonga acuta vel lanceolata brevioribus; culmis, foliis, bracteisque glaucis.

The stamens at the base of the fertile spikes have not been noticed by Torrey, Carey, and others generally who have collected the plant. They seem to me to be less common than they appeared to Dr. Boott, though I find them, and they were found by Schk. The American plant seems to be a larger and stronger plant than the European, even to the European botanists.

Though the resemblance of the figure of C. Magellanica in Schk. to C. limosa may have been noticed, it was reserved for Dr. Boott to prove, by examination of the specimens of Lamarck, their identity with common C. irrigua of Europe and North America. Lamarck's name, being the oldest, must be the accepted name of this species. It was briefly described in this

Journal, volume x, page 42, 1826, under the above name of Wahlenberg.

In cold marshes in the Northern and Western States, and Canada.

284. *C. rariflora*, Smith, Eng. Bot., 1845.
C. limosa, var. rariflora, Wahl., 1803 and Schk., 1812.

Spica terminali staminifera brevi; spicis pistilliferis 1–3, sæpe 2, linearibus brevibus laxifloris pedunculatis et nutantibus, bracteatis vix remotis; fructibus, *tristigmaticis*, ovatis oblongis vel ellipticis triquetris subcompressis obtusis apiculatis ore integris, squamam ovatam obtusam vel acutam sub-fuscam subæquantibus; planta glauca brevi et gracili.

White Mountains, N. H., on borders of Blue Pond—Dr. Barratt: northern parts of Europe and America. As a variety of C. limosa, this was very briefly described in this Journal with the preceding species.

In the *Summa* (p. 233, 1846) of the acute Fries, he has spoken of this C. rariflora, "quasi forma reducta," *as if a reduced form*, "scarcely a span high," of his preceding species, C. Stygia; but C. Stygia seems more nearly related to C. limosa, *L.*, than is C. rariflora. Hence, we find that Lang, in his *Scandinavian Carices*, admits both as proper species; and he will doubtless be approved by others.

Note. The following rare variety of C. limosa *L.*, was discovered the last summer by Rev. John A. Paine, of Utica, who has been actively and successfully engaged in looking up the *rare* as well as the common plants.

C. limosa, L., var. *Painei*, Dew.

Culm with the terminal spike staminate, and one pistillate spike of the common size and form on a slender or hair-like peduncle, from five to nine inches long, rising from near the root. In one instance there was a single pistillate spike on the culm and near the staminate spike, and the long radical pedunculate spike as before. The common form and the variety here described were found growing on different culms from the same root.

This is a very curious variety, and seems not to have been discovered before. Dr. Boott says of the lowest pistillate spike of C. Magellanica, "rarius subradicalis;" but this is on C. limosa and is truly radical, while he has figured that as subradical and short pedunculate instead of very long.

285. *C. mirata*, Dew.

Spicis 3–5, longo-cylindraceis inclusè pedunculatis longe-folioso-bracteatis; spicis staminiferis 1–3, sæpe 2, approximatis, interdum ad basin vel erga apicen pauco fructiferis, cum glumis longis arctis attenuatis scabro-subulatis; spicis pistilliferis 1–2, laxifloris suberectis sub-remotis ad apicen vulgo staminiferis; fructibus parvi-ovatis longo-conicis vel lanceolatis vix inflatis nervosis vel striatis longo-stipitatis divergentibus rostratis, rostro profundi-fisso bicuspidato interdum bifurcato vel bidentato; glumis fructiferis lineari-lanceolatis scabro-subulatis, fructu superno spicæ brevioribus, fructum inferiorum æquantibus, atque fructus infimos plus duplo superantibus: culmo superne scabro, inferne obtusi triquetri et lævi; foliis bracteisque nodosis et margine scabris.

Culm 15–20 inches high, stout and stiff, triquetrous, obtuse-angled and smooth below the lowest bract, but rough above it, with long and broad leaves from the base and long leaf-like bracts, longer than the culm, and the lower the longer, both net-veined and very scabrous on the edges; staminate spikes 1–3, commonly 2, long-cylindric, near but the lower more remote, covered with long narrow bristle-form glumes attenuate and rough-subulate, with a few fruit at their base or toward the summit; pistillate spikes 1–2, often 1, long cylindric, suberect, exsert-pedunculate, at their summit staminiferous: stigmas three; fruit round and small ovate, long-conic, diverging, scarcely inflated, long-stipitate, with a beak deeply bifid, sometimes bicuspidate or bifurcate or bidentate with scales ovate lanceolate, rough-subulate, shorter than the fruit at the upper part of the spikes, equalling the fruit along the lower, and sometimes several, four to ten, of the lowest scales even twice as long as the fruit. Plant pale green.

In Greece, eleven miles west of Rochester and six south of Lake Ontario, in 1829, by Dr. S. B. Bradley. It was not named for a long time, as its anomalous characters made more specimens desirable. But no others have been discovered in the vicinity of this locality, which the clearing of the forest destroyed. In Belleville, Canada West, in 1863–4 by John Macoun, Esq. One form, sent by him, accords with the above description, especially in the varieties of the fruit, of the rostrum, and the very long scale of the fruit at the base of the lower pistillate spike.

Dr. Boott in Illust. No. 58, refers this species to C. aristata, *R. Brown*, by a mistaken[1] reference to my description of C. aris-

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[1] His reference, "C. mirata, *Dewey*, Sill. Jour., xxvii, 240, v, 49, 48; Wood's Bot," should have been *C. aristata*, Dewey, *Sill. J.*, xxviii, 240, vol. xlix, 48 p.; *C. mirata*, Dewey, *Wood's Bot.* Not only was the volume wrong, but *mirata* was unintentionally substituted for *aristata* by Dr. Boott, which changed the whole subject. Indeed the full description of C. aristata, Sill. J., vol. xxviii, 1835, referred to by Dr. Boott, was printed years before I had heard of C. mirata, proving that the description was of C. aristata evidently, and of no other, it may properly be added, as Dr. Boott implies in 1858.

If the "Note," Sill. Jour., xlix, 48, 1845, is noticed with care, it is obvious a mistake is made, because the last sentence *denies* what the preceding *asserts*. It will tata, which has no connection with any other specimens, and by that self-destructive "Note," vol. xlviii, p. 49.

When No. 58 was published, Dr. Boott had not seen any specimen of C. mirata; but since that time he has seen two specimens of it, which I forwarded to him, and one of which was a present to him from Dr. Bradley. Circumstances led me to suppose he had changed his opinion in consequence; but his death, so unfortunate for this science, has prevented the publication of the 4th Part, which was to contain it and was known to be nearly ready for publication. What measures can be taken to secure its being printed and circulated?

Another form, judged by Mr. Macoun to belong to this species, has been provisionally named

C. mirata, var. minor, *Dew.*

Staminate spikes 2–3, often 3, long and short, and scales slightly larger; pistillate spikes 1–2, some shorter with stamens at the apex, or below it, or both, as in C. mirata, with fruit slightly more inflated, and its scales generally shorter than the fruit; plant older, with brown spikes and scales.

In Belleville, C. W.; Mr. Macoun.

Note. C. rostrata, *Mx.*, commonly obtained from ponds near the base of the White Mts. N. H., was found most abundant by Rev. Mr. Paine of Utica, in August last, at a pond or small lake near the well known Bald Rock or Mt. in the N. E. part of Herkimer Co., at the west foot of the Adirondack Mountains. Muhlenberg had not seen it, as he referred this name, *rostrata*, to his C. tentaculata. It was not recognized from the time of Michaux to 1840, when it was described in this Journal, vol. xxxix, p. 52.

C. lenticularis, *Mx.*, was found also by Mr. Paine along the chain of the "Eight Lakes," in Herkimer Co.

C. Houghtonii, *Torr.*, has been found, the last season, 60 miles north of Belleville, by John Macoun, Esq., an active and discriminating botanist. The same species has been found, the last two seasons, by Rev. Mr. Blake, of Gilmanton, N. H., and in Milford, north of Bangor, Me. With great pertinence was it named by Dr. Torrey.

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be rectified by the insertion of C. aristata, so as to read, "For description and figure of C. aristata, *R. Br.*, refer to vol. xxviii, p. 240, of this Journal; tab. v, fig. 67. This species the fig. 57 finely represents, while it is very unlike C. mirata, as is that description in vol. xxviii. So certain is it, that the "Note" assumes the great difference between C. aristata and C. mirata, instead of uniting them.

ART. XI.—*On the Action of Ozone upon Insensitive Iodid and Bromid of Silver;* by M. CAREY LEA, Philadelphia.

A STATEMENT has recently appeared in the French scientific Journal *Les Mondes*, from which it has been copied,[1] to the effect that ozone is capable of giving sensitiveness to insensitive iodid of silver. Mr. J. P. Kaiser states that he has found that certain vapors, such as the vapor of benzine, exercise a powerful effect of this kind, converting one insensitive variety into one that was highly sensitive: that he attributed this effect to the ozone produced by the contact of this vapor with atmospheric air:—that he therefore experimented with air ozonized by a galvanic induction apparatus, and found the same results produced, but in a much more marked degree.

This statement was not altogether the first suggestion of the matter which I had seen. A French Photographic Journal narrated, a few months back, a case in which a photographer, whose plates had refused to afford any image at all, restored their sensitiveness and got them into satisfactory working order by simply generating ozone in the room. To this latter statement, I was disposed to attach little importance. But the explicit and circumstantial one just above referred to, was a different matter.

Those who put forth these statements do not seem to have been aware of their true importance. Not only would such a fact be of great theoretical interest, but its practical applications would be immense, especially in reference to out-door photography, the easy execution of which on wet collodion would at once have become practical. For plates could be coated with insensitive iodid of silver in ordinary light, (say, for example, after Mr. Sayce's formula). This plate could be placed in an ordinary dark slide; and a vessel containing the means of generating ozone could be placed in the camera. On withdrawing the dark shutter from the slide, the plate would be uncovered, and exposed to the ozonized air of the camera. In due time the lens would be uncovered, and the image allowed to fall on the now sensitive surface. The development would then be effected in one of Weiske's developing cases, and thus the whole process would be reduced to the utmost simplicity.

In view, therefore, of the interest which attaches to this question, I was led to make the following trials. In place of using ozone generated by electricity, as done by M. Kaiser, I preferred to generate it by chemical means, viz: by phosphorus, and also by the action of sulphuric acid on chameleon mineral. As the

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[1] See British Journal of Photography, 1864, p. 392. Also Phot. Mittheilungen, Nov. 1864, in which it is suggested that ozone acts by destroying a trace of alkaline iodid present.

source of the ozone was not the same as that used by M. Kaiser, my results cannot be considered as strictly controlling his, but the action of ozone from the two sources is so similar that we should naturally expect similar results in the two cases.

A. OZONE BY PHOSPHORUS.

The ozone was generated in a large bell glass, and the experiments were not commenced till paper impregnated with starch and alkaline iodid exhibited an immediate and strong ozone reaction.

I. *Action on Iodid of Silver.*

Paper was plunged into an ordinary negative bath, and dried. Strips were immersed in solution of iodid of potassium, and without leaving them in too long, were next thrown into clean water and washed.

(1.) A piece of this paper, thus imbued with washed insensitive iodid of silver, was placed in the ozone apparatus for two minutes. It was then exposed to diffuse daylight for six seconds. The application of an iron developer produced no darkening whatever. A longer exposure to light was also without effect. The paper was just as insensitive as before being exposed to the ozone.

(2.) The effects of a longer exposure to the ozone were next tried. The paper, prepared as before, was exposed for half an hour to the action of the ozone, and exposed to a moderate diffuse daylight for twenty seconds. The iron developer produced no effect whatever.

(3.) Same as (2), but exposed to light for thirty seconds. Result as before.

(4.) The paper was prepared as before, but, after immersion in the solution of iodid of potassium, the washing in water was omitted, and the strip was placed in the ozone apparatus just as it left the solution of iodid. The paper immediately changed to a deep chocolate brown, while still in the ozone apparatus. This effect was at once attributed to the action of the ozone on the free alkaline iodid, but to place the matter beyond doubt, the paper was thrown into a solution of hyposulphite of soda, which instantly bleached it.

It seems hardly likely that this reaction, so well known, could have been mistaken for an indication of sensitiveness to light. Perhaps, if the exposure to ozone had been conducted in diffuse daylight instead of in a dark room, a careless experimenter might have been misled, by the similarity of the chocolate-brown color produced to that so often occurring in photography, into a misapprehension of the agency at work, and might have supposed that the ozone had rendered the insensitive iodid of silver sensitive to the diffuse light of the room. Such an error seems, however, unlikely.

(5.) Paper was prepared as before, was exposed fifteen minutes to the action of ozone, then one minute to the light of a strong argand gas burner, one half being protected from the light. An iron developer was then applied. No effect was produced, nor could the side exposed to the light be distinguished in any way, after the application of the developer, from that which had not been exposed.

[It was previously ascertained that a sensitized collodion plate placed in the same position as respects the same light, was powerfully impressed in 20 seconds, or one-third the time just mentioned.]

(6.) Paper was prepared in the same manner, but after placing in the solution of iodid, and washing, it was dried, and in this condition exposed to the ozone for fifteen minutes. Then to the same gas light for one minute, partly covered. Result same as number 5.

(7.) After treatment with silver and iodid, and washing, the paper was exposed to ozone for fifteen minutes, then to the same gas light as in (5) and (6) for one minute, partly covered. It was then developed with pyrogallic acid to which nitrate of silver and citric acid had been added, and this was kept on till the paper was blackened by the deposition of silver. Not the slightest distinction could be traced between the parts that had been exposed to light, and those that had been produced by thick yellow paper.

II.—*Action on Bromid of Silver.*

(8.) In order to give a greater variety to the experiments, the bromid of silver submitted to trial was not disseminated through paper as was done with the iodid in the foregoing trials, but was used in a collodion film. For this purpose a collodion was made somewhat similar in character to that proposed by Mr. Sayce, in which the bromid of silver is suspended in the collodion with its sensibility destroyed by the presence of a small excess of alkaline bromid. The proportions used were as follows:

Ether, - - - - -	½ ounce.
Alcohol, - - - - -	1 "
Pyroxyline, - - - -	10 grs.
Bromid of ammonium, - - -	9 "
Nitrate of silver, - - -	13½ "

These proportions leave a slight excess of bromid of ammonium present, which ensures that the bromid of silver is in the insensitive form.

(9.) A portion of this collodion was poured on glass and exposed to a highly ozonized atmosphere for three minutes. It was then exposed to a large gas light for fifty seconds. On the application of an iron developer no darkening was observable.

(10.) In order to afford a term of comparison, a film of ordinary collodion was sensitized in the negative bath in the usual manner, and was exposed to the same light for twenty seconds. On the application of the same developer it was strongly darkened.

(11.) The same collodion as in (9) was exposed to the same ozonized atmosphere for 45 minutes. It was next exposed to the same gas light as (8) and (9) for thirty seconds. The application of the developer produced no darkening.

(12.) Same as (10), but exposed to light for one and a half minutes. Result as before.

(13.) Same, but exposed to light for three minutes. Result as before. In no case was the slightest reducing effect produced.

B. OZONE BY CHAMELEON MINERAL.

Action on Bromid of Silver.

The bromid of silver was used in the same form as before, viz: in collodion containing a small excess of bromid of ammonium. I should have mentioned before that the insensibility to light of this argento-ozonized collodion was first carefully tested and proved. Ozone was generated in a closed box by the action of undiluted sulphuric acid on chameleon mineral; the vessel containing which was set aside for a short time to let the purple vapors pass off. It was then set in the box, and the condition of the atmosphere was examined from time to time by appropriate test paper.

(14.) Before commencing with the ozone trials, a piece of glass was collodionized with ordinary collodion and sensitized in the negative bath. It was then exposed to a weak diffuse daylight for thirty seconds, after which it immediately darkened under the developer. This was to determine the action and power of the light in question.

(15.) The collodion containing insensitive bromid was exposed to a weakly ozonized atmosphere for ten minutes. It was then exposed to the same daylight as the ordinary sensitive plate mentioned in (14), and for the same time. An iron developer was then applied, but not the slightest indication of sensitiveness was observable.

(16.) The last experiment was repeated under the same conditions, but with a treble exposure to the action of the same ozonized atmosphere, viz: thirty minutes instead of ten. The exposure to light was the same, and the insensitiveness the same.

(17.) Instead of a weakly ozonized atmosphere, as in the last two cases, a strong one was prepared, and a plate similarly prepared with insensitive argento-bromid collodion was placed in it ten minutes. It was then exposed to the same light and treated in the same manner as in the experiment immediately preceding, and with the same results:—not the slightest indication of sensibility.

In this last experiment, the atmosphere was ozonized sufficiently to render iodid of potassium and starch paper instantly blue. In the two previous, this reaction showed itself more gradually.

In remarking upon the experiments just detailed, I may observe in the first place that the ozone was unquestionably always present in such strength as to bring out its marked chemical effects. This was demonstrated not only by its action on the characteristic test paper, but also by its chemical action exhibited in experiment (4).

Again, the experiments were tried in the most varied ways. The ozone was produced in two different manners, and of varied strength. The surfaces to be tested were also exposed for widely varied times, from two minutes to forty-five.

The nature of the light was also varied, experiment having been made both with daylight and artificial light. The time of exposure to light was very various, and in several cases, after the first results had been noted, the paper or film still wet with developer, was carried into the light and exposed for some time; to see if any faint sensibility existed and would manifest itself by the prolonged action of light. No such result appeared. The experiments included both iodid and bromid of silver, and both paper (of which two different sorts were intentionally used) and collodion were used as the vehicles for the silver compound.

The result appears to show pretty clearly that ozone has no power of giving sensibility to insensitive iodid or bromid of silver formed in the presence of excess of alkaline iodid, whether the excess be left present, as in the bromid experiments, or be removed, as in those made with iodid. Or at least that this is true in respect to ozone produced in the two manners which I have described. If Mr. Kaiser has really succeeded in sensitizing plates in the manner described by him, I think that result might possibly be ascribable to some direct effect of the induced electricity which he employs, and not to the ozone produced. But the description of his experiments which has reached me in the *British Journal* is so brief (no details whatever are given) that the above can only be considered as a mere suggestion of an explanation of the opposite results obtained by him. The whole subject is an interesting one, and the effect of all the forms of physical force, especially electricity and heat, on the silver haloids might no doubt be advantageously studied.

Art. XII.—*Prize for applications of the Electric Pile.—Heterogeny.—Influence of Light on Proto-organisms.—Association for the Advancement of Meteorology.—On the intensity of action of the Solar disk.—Acclimation of Salmon in Australia.—Production of the Sexes.—Cutting of the Isthmus of Suez.—First idea of Electric Telegraphy.* Correspondence of Prof. J. Nicklès, dated Nancy, France, Oct. 20, 1864.

Prize for applications of the electric pile.—The prize of 50,000 francs, which was founded in 1852 by the Emperor Napoleon, has just been decreed to Ruhmkorff, for the induction apparatus known as "Ruhmkorff's coil," the mechanism of which we were among the first to make known.[1] The committee, consisting of Messrs. Pelouze, Rayer, Serres, Becquerel, H. St. Claire Deville, and others, was presided over by Dumas. We make the following extracts from his report:

"Mr. Ruhmkorff was at first a workman for some of our best constructors of physical apparatus, afterward had his own workshop, and finally became head of a house of constantly increasing celebrity.

"His education was gained, little by little, through reflection, study, and the lectures of certain professors heard as it were by stealth in his occasional hours of leisure. Modest, of unyielding perseverance, and of a self-devotion which has earned for him the highest encomiums, Mr. Ruhmkorff will ever remain the type of his class—a model for the numerous intelligent workmen who fill the higher order of workshops (ateliers de precision) of Paris. To those, who, like him, know how to control their desires, who faithfully strive for perfection in work and clearness in conceptions, who bend their attention to one object, and labor untiringly until a high superiority is gained and also for themselves the satisfactions of a ripe age, the compensation for the sacrifices and privations of youth will not be lacking in a country where, more than ever, merit finds recompense.

"Since 1851, Mr. Ruhmkorff has devoted himself to the construction and perfection of his apparatus, and he has ended by securing for it his own name, by giving it a scientific value which no one contests, and by rendering it of so great power as to become a means of numerous practical applications.

"The Ruhmkorff apparatus unites the two forms of electricity, which had been separated as by an abyss—the old machine electricity, characterized by a capability of producing sparks and by strong tension, and the electricity of the pile, characterized by very feeble tension and by its inability to produce true sparks.

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[1] This Journal, 1853, xv, 114; 1862, vol. xxxiii.

"If Franklin's discoveries placed beyond doubt the identity of mechanical electricity and lightning, there remained, nevertheless, among the phenomena which accompany storms, many circumstances the explanation of which was yet inaccessible to science. We must therefore regard as a valuble acquisition to meteorology the observation that the spark of the Ruhmkorff apparatus consists of two parts—an instantaneous line of light and an areola of measurable duration. The magnet divides the latter; a breath, or any body in motion, draws it out, and the electric spark thus divided continues its route in these two directions at once, as long as the passage of the current continues uninterrupted.

"In a vacuum, the electric spark develops light, and that developed by the Ruhmkorff spark takes different colors in different gases; it brilliantly illuminates fluorescent bodies, and divides itself into parallel bands separated by dark spaces perpendicular to the axis of the recipients. These colored luminous bands obey the action of the magnet which attracts or repels them, and which impresses upon them, at the pleasure of the operator, those movements of translation or of rotation by means of which De la Rive[2] has reproduced the appearances observed in the Aurora Borealis, justifying, thus, the analogy recognized between the electric light produced in a vacuum and that of the polar auroras.

"Glass tubes illuminated by the same means give out a light sufficiently bright to be used in mines that are liable to explosion: they also serve under water as a lamp for divers—and in surgery, to carry into the back part of the mouth, and other cavities of the body, a light which gives no sensation of heat.

"The spark of the Ruhmkorff apparatus inflames combustibles, and explodes gaseous mixtures; and it has thus supplied to Lenoir's gas-engine the means necessary for producing the successive inflammations to which it owes its mechanical power.

"The working of quarries, the boring of tunnels, the explosion of heavy charges in mines, give to-day regular employment to the Ruhmkorff apparatus.[3] Mines had been exploded previously by the aid of the pile; but the Ruhmkorff apparatus has left all other modifications far behind, by reason of the small number of elements which it requires—three instead of one hundred—the power of its spark, and finally the possibility of exploding eight or ten mines at once. In the expedition to China, in 1860, the Ruhmkorff apparatus was used to blow up, by means of eight mines simultaneously exploded, the principal fort of the Peiho, as well as the iron stockade at the bottom of the river."

Dumas then reviews the principal applications of electricity to Mechanics, such as the *automatic break* of Achard, the weaving-

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[2] This Journal, 1853, vol. xvi.

[3] This Journal, 1854, vol. xviii.

looms, the pentagraph of Caselli, the writing telegraph of Prof. Hughes, etc. He then spoke of illumination by electricity, and mentioned the electric regulators of Staite, Serrin, and Dubosc, the illumination of light-houses by induction currents, observing that successful experiments have been made with it by the Light-house Board: at Havre, upon Cape de la Hève it erected, several years ago, an electric lighthouse equivalent to 3000 Carcel burners, by the side of one lighted with oil; the electric light was readily distinguished, by its brilliancy and intensity, from its neighbor which appeared red.

Under *Medical electricity* there was a reference to the observations of Dr. Duchenne of Boulogne and those of Middledorf, of which we have given an account when treating of Galvano-caustic, and according to which, by means of platinum wires brought to incandescence, the tissues may be divided, and the removal effected of polypi and tumors from organs that are deeply seated or otherwise difficultly accessible.

A new competition will take place, five years hence, to which all applications of electricity will be admitted, whether to medicine, the mechanic arts, or industry, without distinction of origin or of nationality.

Heterogeny.—The discussion which has existed for several years between the partisans of spontaneous generation and their opponents, is entering upon a new phase. The two parties desire to unite and experiment together, for it is in that direction that a beginning must be made. Thus it has happened, as in politics, at first fighting, then, conferences or conventions.

Whatever be the opinion on this great subject of generation, it is impossible that new and instructive facts should not be brought out by this convention, in which chemists of known skill are assisted by such physiologists as Pouchet and Joley. There is every reason to believe that each party holds some truth, and that their differences have arisen from their different methods of experimenting. At any rate, there is no opportunity for temper in the debate.[4]

Influence of light upon the production of proto-organisms.—One element of the solution of this question of heterogeny resides in the sun's light, which produces some effects that are not known to result from artificial light or heat, as has been already made known by Messrs. Pouchet and Ch. Morren. Some new facts in

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[4] Since the above was written, several meetings have been held at the Garden of Plants in Paris. Nothing has resulted from this convention except the very sad fact that theological considerations seem to have got the better of the attentive observation of facts, just as in the time ef Galileo. There is, then, nothing to do but to observe that the savants have all agreed that reason, which doubts, observes and studies, has nothing in common with that kind of faith which believes without examination and without proofs.

their support have been observed by Professor Montegazza at the University of Pavia.

Two female frogs were quickly killed, by the destruction of the spinal marrow, and placed in two glass vases each of which contained 115 cubic centigrams of well-water perfectly transparent and free from all foreign bodies. One of these vases was left in diffused light, the other was placed in a box which did not permit a single ray of light to reach it.

The experiment was continued seventeen months. It is necessary, before giving the results of this long trial, to state that every time that the two vases were compared, they remained exposed to the air for an hour or more, and that the air was renewed at each observation. Germs could, then, easily fall in; and yet the results were very different in the two cases.

The following are the observed results:—

1. Two identical bodies exposed to free air may present very different phenomena of putrefaction, according as they are exposed to the influence of the light, or shut off from it.

2. The chemical and biological phenomena of the two forms of putrefaction are very different, that is, we have in each case some special chemical products, and some peculiar animal and vegetable productions.

3. In darkness, there was a marked tendency to the production of vegetable organisms and very simple infusoria; the frog that underwent putrefaction while shut off from the light produced only some Mucedines, Monads and Vibrios; while the other afforded a very complicated fauna—Bacteriums, Vibrios, Spirellas, Monads of different species, Amœbas, Kerones, Alysum, Enchelides, Trachelius,—and finally Infusorians still undescribed, much resembling the Zoosperms of the Tritons.

4. The abundance and superior organization of the Infusoria depend much more upon the progress of the putrefaction than upon the amount of putrescible matter. The more simple species always appear first.

5. The production of species of Bacterium takes place many times during the course of a long putrefaction.

6. When the liquid presents a new fauna, the new species are from the outset represented by a number of individuals at once; from one day to the next, they are simultaneously produced.

7. In the course of a long putrefaction, there are some generations which endure for some days; others exist for a much longer time.

8. Rapid changes in the chemical composition of a putrescible liquid are always, or nearly always, followed by new sets of animal and vegetable microscopic life.

9. When circumstances are little favorable to heterogeny in a very long putrefaction, there may be intervals of time of greater or less length, in which the liquid presents no organism. Who

ever should content himself with observing at such a moment, might say that there had been no generation; while some days before, or some days after, there had been, or there would be, a very abundant production of vegetables, or of animals, or of both at once.

From all the comparative tables, I select an observation made on November 20th, after more than seven months of putrefaction. Temperature at the time 10° C.

In the light.—The liquid had a slight odor of boiled meat.—On the surface many very lively Kolpodes, a great quantity of Vibrios, some Alysca.—Some Infusoria which strikingly resemble the Zoösperms of Tritons.—At the bottom, some dead Kolpodes, and some in the act of multiplying by division into two or four individuals. Many Vibrios; some Monads.

In the dark.—The liquid had a very strong odor of mushrooms.—No organisms; the whole mass liquid.

Association for the advancement of Meteorology.—This association, founded at the commencement of the year 1864, under the presidency of Le Verrier, is a great success. It meets periodically at the Observatory of Paris. It has just founded prizes for the encouragement of meteorological studies, and especially the study of the general movements of the atmosphere. All meteorologists are admitted to competition, without distinction of nationality.

The following are some of the details of the programme:

"According to the generally received opinion, the storms of the coasts of France come, already formed, from the Atlantic. An extended series of meteorological observations made over this vast sea, is, then, the necessary basis of the work proposed for the principal prize. This prize will be of 4000 francs. The memoirs must be delivered to the Secretary of the Society before December 31st, 1865.

" A sum of 3000 francs will be divided between the authors of the best observations made at sea, or in places little known in a meteorological point of view.

"Finally, two prizes, of 500 francs each, are offered for the best memoirs upon the application of Meteorology to agricultural questions. The prizes of 300 and 500 francs may consist of instruments for observation."

Upon the intensity of action of different parts of the solar disk.—With regard to the late researches by Secchi—according to which the calorific radiation of the center of the solar disk is greater than that of the borders, nearly in the ratio of 2 : 1, Mr. Volpicelli writes that the fact was very exactly observed in 1614 by Luc Valerio, a mathematician of Naples, author of a work, *De centro gravitatis solidorum*, and of another *De quadratura parabolæ per simplex falsum.* He was a professor in the Roman University, and has been called the Archimedes of his age.

In one of his letters to Galileo, Luc Valerio considers the rays proceding from the central part of the solar disk as the more active.

Analogous facts have been observed by Mr. Roscoe, according to whom the center of the disk exerts a more intense chemical action than the borders. He has also observed that the south polar zone is more active than the north.

Acclimation of Salmon in Australia.—Recent experiments carried forward by the Acclimation Society have shown that it is possible to transport to distant countries the eggs of fertile fishes. One of its members, Mr. Millet, having observed that melting ice diminished the pulsations of the young fish of the Salmonidæ and delayed the hatching of the eggs, took the idea that this method would serve for the transportation of eggs of the Salmonidæ to Australia and Tasmania. The plan has succeeded, in spite of damaged eggs; a very large number have arrived there in a healthy state, and have been deposited in the rivers. Starting from London on January 15th, 1864, the eggs arrived at Melbourne on April 5th, and at Hobart Town, (Tasmania,) on the 23d. Everything indicates success.

Similar attempts have been made in the French possessions in Algeria, the rivers of which are very barren of fish. Eggs of salmon and trout have been carried there from Huningue (Haut-Rhin), where, as we have previously seen, are found the principal basins for pisciculture. In spite of the differences of climate, these eggs have arrived safely, and have hatched in the basins prepared for them; they already begin to people the rivers. It is a result of no moderate interest to see the salmon of the Rhine and the trout of the Vosges transported to Africa, Australia, and Tasmania, and living and becoming acclimated there.

Production of the sexes.—What are the causes of the production of the sexes? This question, which has occupied the earnest attention of physiologists, has been thoroughly studied by Mr. Thury, according to whom the product is always of the male sex when the fertilization of the ova occurs at complete maturity, and is always female when it takes place at a less advanced period.

There is a very simple way of solving this problem. It is to select for experiment species that come to maturity in succession, and, that during a single impregnation, fertilize the whole series of ova which detach themselves from the ovary during a period of eight, ten, twelve, fifteen, or even eighteen days. We know, indeed, that, in the case of the hen, a single coupling suffices for the fertilization of five, six, or seven eggs which she is about to lay and which are arranged in her ovary in the order of their maturation. Now, in such a case, if the theory is exact, the first egg laid ought always to produce males and the others females without any possibility of the inversion of this order.

This is very near what has been observed by Messrs. Coste and Gerbe:

A hen, separated from the cock at the time of her first laying this year, gave five fertile eggs in the space of eight days.

The egg laid on March 15th produced a male; that on March 17th a male; that on the 18th a female; that on the 20th a male; that on the 22d a female.

A characteristic fact in this experiment is the production of a male after a female, which ought not to have taken place according to the theory. But is it only a simple exception? Or is it necessary to consider the fact a radical objection? We may learn by and by on this point, from the researches in which Mr. Gerbe is now engaged.

On the occasion of the preceding note, Flourens recalled an experiment which he made, thirty years ago.

"Aristotle had observed that the pigeon ordinarily lays two eggs, and that of these two eggs one commonly produces a male and the other a female. He wished to know which was the egg that gave the male, and which the one that produced the female. He found that the first egg always gave the male, and the second the female. I have repeated this experiment as many as eleven times in succession, and eleven times in succession the first egg gave the male and the second egg the female. I have seen again that which Aristotle saw."

Cutting of the Isthmus of Suez.—The almost certain success of the canal across the Isthmus of Suez fixes attention, more than ever, upon other projects of the kind. The cutting of the isthmus of Malacca and of that of Darien await only the completion of the Suez ship canal. In France, they are talking of uniting the Atlantic Ocean with the Mediterranean by a ship canal which would borrow part of its route from the old Southern Canal. In Holland, a society is incorporated, under the title of a "Company for the cutting of the isthmus of Holland," to establish a canal joining Amsterdam and the North Sea by a direct route, while, at the same time, the Dutch government has undertaken to establish another navigable route from Rotterdam to the sea.

A new project is presented, which, if realized, will complete the series of maritime communications in the north of Europe. It is proposed to establish a canal, navigable for ships of war and commerce, between the North Sea and the Baltic, to avoid making the passage of the Danish islands. This project, often thought of, is now very seriously considered.

Finally, they are speaking of cutting one other isthmus, and this time it is Spain which has the honor. It is proposed to pierce the Spanish isthmus in such a way that Gibraltar will be an island. The canal is to start from Trafalgar and end at Andalusia. This canal, which would cost hardly a hundred millions of francs, has for its object to prevent more than 4000 vessels every year from lying to before the strait of Gibraltar without power to get out.

This project is just now submitted to the examination of the Spanish government, as well as to some others, such as the English and French governments.

First idea of an electric telegraph.—This first idea was brought forward in the 16th century in an anonymous work published at Rouen at that time. The question has already been discussed here. A discovery has just been made of a letter of a clerk, the abbè of Barthélemy, well known as a literary man. He writes to Madame du Dessaud, April 8th, 1772:—

"I think often of an experiment which would give us happiness. They say that with two clocks whose hands are magnetized it is sufficient to move the hands of one for those of the other to take the same direction, so that causing one to strike twelve the other will strike the same hour. Suppose that we could perfect artificial magnets so that their power would communicate itself from here to Paris: you would have one of these clocks, we would have another; upon the dial we should have the letters of the alphabet. . . . You perceive that we could thus facilitate the operation; the first movement of the hand might sound a bell which would announce that the oracle was about to speak."

Bibliography.—Among recent publications by Hachette, at Paris, are the following:

Les Assolements et les Systèmes de Culture, par M. Heuzey; 1 vol., 8vo, 540 pp.—This work contains the lectures delivered by Mr. Heuzey at the Imperial School of Agriculture at Grignon, of which he is one of the most distinguished professors. The principles which are here set forth are applicable to all countries which are to be put under cultivation.

Le Médecin des Salles d'Asile, par Dr. Cerise; 2d ed., 8vo, 200 pp.—The "Salles d'asile" of France are charitable institutions for the education of poor children. After a short historical account of the institution itself, the author proceeds to set forth the principles which should govern physicians in their supervision of these infant schools and all which relates to the care to be taken of a miscellaneous collection of children.

Abrégé de Géographie commerciale et industrielle, avec un tableau des monnaies, poids et mesures de tous les pays, par M. Gardon; 4th ed., 8vo, 400 pp.—While referring to all countries, the author deals especially with France, each department of which he describes fully as regards statistics, agricultural, mineralogical, metallurgical, manufacturing and commercial industry, importation, exportation, navigation, etc.

La terre avant le Déluge, par Louis Figuier; 4th ed., 8vo.—This is a popular work designed to initiate the people of the world in the grand phenomena of geology. It is illustrated with many colored geological maps, and more than 300 figures and ideal views of landscapes of the ancient world.

De l'Electricité considérée comme cáuse principale d'l action des eaux minérales sur l'organisme, par Dr. Scoutettin; 8vo, 420 pp. The material which mineral waters hold in solution being often in insufficient proportions to account for their health-giving effects, the author has recourse to another explanation, viz: the electrical action which these waters may exert upon the human organization. We find in his work not merely a simple ideal explanation, but, on the contrary, the recital of numerous experiments made by the author upon the principal mineral waters of Europe.

L'Astronomie au XIXe siècle, par A. Boillot, 12mo, 340 pp. This work, written for universal use, gives a very faithful picture of the progress which astronomy has made from ancient times up to our day. Mr. Boillot is a very distinguished mathematician and makes astronomy his favorite study. His work has been received in France with much favor.[*]

Nancy, Oct. 20th, 1864.

ART. XIII.—*Discovery of Emery in Chester, Massachusetts;* by CHARLES T. JACKSON, M.D., Geologist and State Assayer.

IT has been said, in England, that "a good mine of emery is worth more to a manufacturing people than many mines of gold." Such being the case, it affords me great pleasure to be able to announce the discovery of an inexhaustible bed of the best emery in the world in the middle of the State of Massachusetts, in Chester, Hampden county, quite near to the Western Railroad, which with its ramifications leads to the largest armories and manufactories of metallic articles in this and the adjacent States.

For more than two years, the existence of important beds of magnetic iron ore, originally discovered by Dr. H. S. Lucas, has been known, and endeavors were made by that gentleman to organize a company for the purpose of smelting these ores. In consequence of this agitation, I was employed by John B. Taft, Esq., on the 19th of August, 1863, to examine the locality and to make report of my results to him.

On examination of my specimens of minerals, after returning to Boston, and my notes for sectional profiles of the rocky strata containing the iron ore, I found that the minerals, margarite and chloritoid, in talcose, hornblende, and mica slate rocks, indicated the occurrence of emery, the association of the rocks and minerals being identical with conditions known to exist in the localities of emery in Asia Minor.

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[*] The following titles of literary works are also contained in the letter of Mr. Nicklès:

Histoire de la Littérature française, par M. Demogeot; 5th ed., 12mo, 680 pp.

Eléments de Rhetorique, par M. Filon; 12mo, 300 pp.

Les grandes scènes de la nature, par M. de Lanoyé; 12mo.

Les Champs d'or de Bendigo (*Australie*), par M. Perron d'Arc; 12mo, 200 pp. A description of the Australian placers.

Souvenirs d'un Sibérien, par Rufin Piotrowsky; 12mo, 200 pp. This is a very touching recital of the misfortunes which afflict the unhappy Poles, for whom Europe has only barren sympathy.

I therefore called the attention of the owners of the property to these facts, and directed search to be made for emery, and that every mineral resembling it should be sent to me for examination. Little attention was paid to this prediction at the time, and not until I had invited Dr. Lucas, who resides in Chester, by personal representations and solicitations, to make the required search, the characters of emery being fully described to him.

On his return to Chester he soon learned that the miners were complaining of the great hardness of the supposed iron ore, and that no less than forty drills were dulled in boring a single hole for blasting. He then sent me pieces of this hard rock, in the belief that it was the emery I had predicted. On examination it was found to scratch quartz and topaz readily, and to have all the properties of emery; a chemical analysis proved it to be identical with the emery of Naxos.

The owners, resident in Boston, being notified of this discovery, went with me to the locality on the 11th of October last, when a full exploration of the premises was made. There are several large beds of rich magnetic iron ore at this locality, and the emery being magnetic (as it always is) had caused it to be mistaken for magnetic iron ore, and many tons of it had been smelted with the carbonate of iron and hematite in the Berkshire county iron furnaces, without a suspicion, notwithstanding its refractory nature, that the ore was emery, with only a small admixture of iron ore.

The principal bed of emery is seen at the immediate base of the South Mountain, where it is four feet wide, and cuts through the mountain near its summit, at an angle of 70° inclination or dip to the eastward. Its course is N. 20° E., S. 20° W., and its known extent four miles. Near the summit of the mountain the bed expands to more than 10 feet in width, and in some places is even 17 feet wide.

The alternations of rock in two sections are as follows, beginning to the eastward:

1. *a*, Mica slate; *b*, 15 ft. soapstone or talcose rock; *c*, 2 feet crystallized talc; *d*, talcose slate; *e*, 1 ft. granular quartz; *f*, chlorite slate; *g*, 4 ft. *Emery;* *h*, chloritoid and margarite; *i*, magnetic iron ore; *j*, hornblende rock highly crystalline.

2. *a*, Mica slate; *b*, 6 ft. magnetic iron ore; *c*, talcose slate; *d*, 6½ ft. magnetic iron ore; *e*, chlorite slate; *f*, hornblende rock, crystallized; *g*, 7 ft. *Emery*, chloritoid and margarite; *h*, magnetic iron ore; *i*, hornblende rock.

The elevation of the upper outcrop of this bed above the immediate base of the mountain is 750 feet. There are curious rounded masses of remarkably pure emery three feet in diameter in this bed entirely invested with a coat of delicate rose-colored margarite, and a thick layer of bright green chloritoid, the investing coat being from half an inch to two inches in thickness. It is found extremely difficult to break up these masses of solid emery, drilling holes in them for blasting being very slow and laborious, and no grip can be had on their rounded sides by the sledge. A heavy drop hammer will be required to break them to pieces—or they may be cracked by fire if heat does not injure the emery.

A branch of Westfield river separates the South from the North mountain, a hill nearly 750 feet high, through the summit of which the great emery bed also cuts. On this hill the emery is more largely crystalline and less mixed with magnetic iron ore. It is more like corundum, but still contains the combined protoxyd of iron, characteristic of true emery. Its specific gravity is from 3·75 to 3·80, while that from the South mountain is from 4·02 to 4·37; Naxos emery being from 3·71 to 3·72, according to my trials of it, in comparison.

On the North mountain, three large beds of rich magnetic iron ore, distinct from the ore accompanying the emery, occur, the ore yielding $54\frac{1}{4}$ per cent of metallic iron. This ore is mined, and is smelted into bar iron by forge fires, and is also sold to mix with the hematites and carbonates of iron worked at the Lenox and Stockbridge furnaces.

Digesting the South mountain emery in fine powder with nitro-muriatic acid and sulphuric acid for a long time, it was found that 73 per cent of it was wholly insoluble in acids; and on microscopic examination the grains were seen to be translucent, and exactly like the Naxos emery prepared in the same way; but these translucent grains are readily taken up by the magnet. I therefore infer that protoxyd of iron is a chemical constituent of true emery.

Chemical analysis of the coarsely crystalline emery of the North Mountain, Chester. Sp. gr. 3·75. H.=9.

Alumina,	46·50
Protoxyd of iron,	44·00
Titanic acid,	5·00
Silica & loss,	4·50
	100·00

Emery of the South hill.[*] Sp. gr. 4·02. H.=9.

Alumina,	45·50
Protoxyd of iron,	43·00
Silica & titanic acid,	11·50
	100·00

Regarding the oxyd of iron which can be dissolved out from emery by acids as accidental, and that which cannot be so removed as an essential constituent, we shall have for the compo-

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[*] The highest specific gravity of any sample from the South mountain was 4·3734.

sition of three samples of emery analysed, after digestion in acids,

	Chester, 1.	Chester, 2.	Naxos best selected emery.
Alumina,	60·4	59·05	62·3
Protoxyd iron,	39·6	40·95	37·7
	100·0	100·00	100·0

If this view is adopted, emery must be ranked as a distinct species, and not as a mere granular form of corundum or sapphire.

In conclusion, I would state that practical trials of the Chester emery, in several of the large armories and machine shops of this and the adjoining States, have proved it to be fully equal in value to the well known emery of Naxos, which I have no doubt it will wholly supplant in this country, and that it will ere long become an article of export to Europe, either in its native form, or in a manufactured state.

It may be proper to add, that John B. Taft, Esq., of Boston, in behalf of his associates, owners of the emery mine, has the sole management of the business connected with the mine.

I would express my obligations to Mr. J. L. Smith for the valuable information contained in his articles on the emery of Asia Minor and on the associated minerals of the emery localities, published in vols. x. and xi. of this Journal. Also to Dr. H. S. Lucas, of Chester, for kind assistance in the field.[7]

32 Somerset St., Boston, Dec. 12th, 1864.

ART. XIV.—*On the Solubility of the Sulphate of Baryta in Sulphuric Acid;* by Prof. J. NICKLÈS.

THE sulphates of baryta, strontia, and lime, are slightly soluble, as is known, in boiling sulphuric acid. I have found that they are soluble in *cold* acid when in a nascent state.

To obtain this result, it is only necessary to throw a little chlorid of barium, or of strontium, into a sufficient quantity of monohydrated acid: the chlorid is by degrees decomposed, chlorhydric acid escapes, and the sulphate produced dissolves in the surrounding acid.

This is especially true for the chlorid of barium; but to succeed, the acid should be concentrated, and it is best to have the chlorid well dried and in powder. On adding water to the solution thus obtained, the sulphate of baryta falls as a white precipitate.

The chlorid of strontium gives the same results, and the acid solution affords a similar precipitate with water, though less in amount, since less is dissolved.

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[7] **On page 87, the date 19th of August should be 19th of October.**

The sulphate of lime is still less soluble in sulphuric acid, and the solution takes several days to become limpid. Moreover, water does not then becloud it; a light precipitate is obtained with alcohol.

The solubility of these sulphates in sulphuric acid, is then the reverse of their solubility in water, except for the sulphate of strontium, which, in both cases, is a mean between those of the other two.

Nancy, Oct. 20, 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On a new saccharimeter.*—Wild has described a new saccharimeter which appears to possess great advantages over the well-known instrument of Soleil. In Wild's apparatus the incident light is polarized by means of a Foucault's calc-spar prism, which is capable of being turned round its axis, the amount of rotation being measured by means of a vernier applied to a small circle graduated to single degrees. The analyzer consists of a modification of Savart's polariscope, that is to say, of two crossed plates of quartz, each 20mm. in thickness, cut at an angle of 45° with the optical axis. The fringes produced are viewed with a small astronomical telescope of low power (objective 33mm. focal length, ocular 24mm. focal length). The telescope is adjusted for parallel rays, has cross wires, and a Nichol's prism in front of the eye piece. The tube containing the solution of sugar is of course placed between the polarizer and analyzer. The author instituted comparative experiments to test the degree of accuracy attainable with Soleil's instrument and with his own, employing for the purpose a solution containing 298 grams of cane sugar to the litre. The mean error of a single compensation in Soleil's instrument amounted to 1°, when the eye was properly protected, while it reached $1\frac{1}{2}°$ by direct observation in an ordinary room without exclusion of side light. From this it results that in the most favorable case there is with Soleil's instrument an uncertainty of ±13·9 gr. in a litre of the solution, or rather more than one per cent. With Wild's instrument the error of observation with white light amounted to 12′, which corresponds to an error of ±2·78 gr. of sugar in a litre of solution, and consequently to a degree of accuracy five times greater than with Soleil's apparatus. When this instrument was directed toward an Argand gas burner in a dark chamber, and a dark red glass was placed between the eye and the ocular, the error of observation amounted to only 3′. Finally, when a homogeneous soda flame was employed, the error amounted to only 1′. In the first case, the error in the quantity of sugar would amount to ±0·7 grs.; in the second, to only ±0·2 gr. in a litre.[1]—*Pogg. Ann.*, cxxii, 626.

W. G.

[*Note.*—Wild's Saccharimeter, independently of its purely technical

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[1] Wild's Saccharimeter can be procured of Hoffman, optician, 3 Rue de Buci, Paris.

uses, will form a most acceptable addition to physical cabinets. To the chemist it offers the most precise method of determining the rotatory polarizing powers of liquids at present known, provided that homogeneous light be employed, since the characteristic lithium, sodium and thallium lines afford perfectly definite degrees of refrangibility. In Broch's measurements of the rotatory polarization in quartz by means of the fixed lines in the spectrum, the mean error of a single measurement varied from about 6′ to about 30′, according to the refrangibility of the light. In a series of measurements undertaken to determine the degree of accuracy with which the change of tint could be determined in plates of quartz placed between two Nichols' prisms and examined with white light, I found, as might be expected, that the error of observation increased as the intensity of the incident light diminished, the minimum error of estimation being about 15′. Hence it appears probable that the degree of accuracy attainable in Wild's instrument might be still farther increased by using a condenser so constructed as to give an intensely brilliant beam of parallel and homogeneous rays.—W. G.]

2. *On the mechanical energy of chemical action.*—Schröder van der Kolk has communicated an interesting and suggestive memoir upon the application of the theory of mechanical energy to chemical processes, especially in connection with the relations of chemical affinity to heat. We shall endeavor to give some idea of the train of reasoning in this paper, referring however to the original for more ample illustration. The author in the first place cites the opinion expressed by H. Sainte-Claire Deville, in his paper on dissociation, that all chemical compounds may be resolved into their elements by a sufficiently high temperature. In some cases the separated atoms reunite at a lower temperature; in other cases they remain separate. This gives us from this point of view two classes of bodies, and the author connects the phenomenon with the facts observed by Favre and Silbermann that certain combinations are effected with evolution, others with absorption of heat. The relation between the two classes of phenomena appears from the following considerations. If we conceive of a body at the temperature of 0°, and then apply heat, the body at a definite temperature will have absorbed a definite quantity of heat. This heat is applied partly in raising the temperature, partly in molecular changes, and partly in external work. The entire quantity of heat continually increases with the temperature, and heat is always taken up both in raising the temperature and in changing the state of aggregation from the solid to the liquid, and from this to the gaseous condition. To find the quantity of heat in the body after the process, we must subtract the quantity which is consumed in external work. The quantity remaining is termed by Thomson the mechanical energy of the body. Starting as we have done from 0°, we obtain in this manner only the accumulation of energy in the body from 0° to the particular temperature under consideration, and not its absolute quantity. Every body has therefore in any definite condition a definite quantity of energy. If now we consider two substances, as, for example, oxygen and hydrogen, which combine with each other under the influence of the electric spark, we have in each, before combination, a definite quantity of energy. During the combination, heat is evolved, and if we now cool the vapor of water

which is formed to the temperature of the components before combination, the steam will contain a quantity of energy less than that of its components by the quantity which is given out in the combination. We here suppose the combination of H and O to take place without external work. When the vapor of water is decomposed at the same temperature, just as much energy must be added as became free during the combination. Two cases may then occur: either the body has more energy than its components, or else the contrary is true. In the first case heat is given out in the decomposition, in the second, it is absorbed. It follows that heat must become free when we heat a body of the first class to the point of decomposition, and the components will not unite on cooling, since the quantity of energy in them is not sufficient for the formation of the compound at the same temperature. This could only take place in two cases: either the body must take up heat from the surrounding medium, which, however, has at most an equal temperature, or else the compound must become suddenly colder. So far as known, no such cooling occurs. From the above we obtain the following law: Bodies which evolve heat in being decomposed by heat are not again formed in the subsequent cooling. This law may be tested by the results of the experiments of Favre and Silbermann. These physicists found that protoxyd of nitrogen, NO, peroxyd of hydrogen, HO_2, chlorous and chloric acids, $Cl\,O_3$ and $Cl\,O_5$, evolve heat during decomposition, while the elements separated do not recombine on cooling. The same is true for the chlorid, iodid and sulphid of nitrogen, which are suddenly decomposed on heating, with evolution of heat, and the components of which do not recombine. The above law is also true when dimorphous or polymorphous bodies change their physical condition. Thus, aragonite on heating passes into calc-spar with evolution of heat; the converse change does not take place on cooling. Fused sulphur crystals (System V) on cooling become crystallized in System IV with evolution of heat; the converse change does not take place. Various other illustrations are given, and the author generalizes the law already stated as follows: When bodies on heating pass into a different physical state, with evolution of heat, the previous state is not resumed on cooling. It is of course possible that in certain cases the purely chemical affinity between the molecules may be so powerful that combination will ensue, heat being abstracted from the surrounding medium. The converse proposition is as follows:

If heat be absorbed in decomposing a body by means of heat, the contrary action will take place on subsequent cooling.

This proposition cannot be demonstrated theoretically, and is therefore not to be regarded as proved. It is frequently found by experiment to be true: thus, carbonate of lime, according to Favre and Silbermann, absorbs heat when decomposed by heating, and has therefore less energy than its components together. CO_2 and $Ca\,O$ recombine on cooling. On the other hand, much heat is evolved in slaking lime, and CaO, HO has therefore much less energy than its components. On heating, the hydrate is décomposed, and is again formed on cooling. In the formation of CO_2 and HO heat is evolved, and the compounds have therefore less energy than the components. The elements only unite at a high temperature, and the decomposition takes place at a still higher temperature.

On cooling, the components ought to recombine, but in point of fact this does not happen, though the components have much more energy than the products. The explanation of this depends upon the action of the purely chemical affinities, which at lower temperatures are not sufficient for the formation of the compound. Two conditions are in general necessary for the formation of a compound. First, a degree of chemical force or affinity sufficient for the combination, and secondly, the necessary energy. A single cause is insufficient. The author here distinguishes between reversible and unreversible processes. When a body changes under the action of heat, and with evolution of heat, it passes into a new condition of less energy, and therefore cannot in itself pass back into its original state. This is the unreversible process, and occurs most frequently. When heat, on the contrary, is absorbed, the new body has more energy, and may therefore on cooling pass of itself back into its previous condition. This is illustrated by the latent heat of fusion and evaporation. Other consequences of this theory are these; the heat of combination remains the same whether the compound be formed suddenly or by degrees; the heat of combination of a compound body is in general less than that of its components taken singly. The following examples serve to illustrate still further the application of mechanical energy to the explanation of chemical changes. An electric spark causes combination in two ways. Certain bodies, H and O, H and Cl, CO and O, &c., combine suddenly in unlimited quantities by means of a single spark, and with evolution of heat. Others, like N and O, combine only gradually; no heat is evolved, and the combination ceases with the cessation of the spark. The formation of ozone is also a case in point. In the first case, the energy of the components is greater than that of the compound. Sufficient energy is present, but not sufficient affinity. The electric spark here increases the affinity; some atoms of H and O combine, and the heat produced causes a further combination, since heat within certain limits increases affinity. In the case of the combination of N and O to form nitric acid, the components have less energy than the compound. Then the spark furnishes the requisite energy, and each spark yields only a definite amount of work. The case of NH_3 forms an exception which for the present must remain unexplained. The author also applies the theory to the catalytic action—so called—exercised by platinum upon H and O, but not upon N and O. In conclusion, the author takes ground against the assumption often made that the heat of combination is to be taken as the measure of chemical affinity. He considers that this heat, regarded as difference in energy, is the measure of the stability of the compound. Thus, water with the heat of combination expressed by 29413 has more stability than chlorhydric acid with a heat of combination of 23783 units. In each case, besides the requisite energy, the requisite affinity must be present. Thus, in a simple mixture of O and H no combination takes place until the affinities are sufficiently increased, although the necessary energy is always present. A further argument against considering the heat of combination as the measure of affinity is to be found in the fact that in certain combinations heat is absorbed, so that in these the chemical affinity would be negative.—*Pogg. Ann.* cxxii, 439.

W. G.

3. *On the replacement of hydrogen in ether by chlorine, ethyl and oxethyl.*—LIEBEN and BAUER have studied the action of monochlorinated and bichlorinated ether upon zinc-ethyl and ethylate of sodium, and have obtained results of much theoretical interest. By the action of chlorine upon ether, at ordinary temperatures, the author obtained a mono-chlorinated ether with the formula, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4Cl\end{matrix}\right\}O_2$. The action of zinc-ethyl upon this compound gives rise to two products which have respectively the formulas, $\left.\begin{matrix}C_4H_4 . C_4H_5\\C_4H_4 . Cl\end{matrix}\right\}O_2$, and $\left.\begin{matrix}C_4H_4 . C_4H_5\\C_4H_4 . C_4H_5\end{matrix}\right\}O_2$, and in which, as will be seen, one or two atoms of hydrogen in the radical ethyl are replaced by one or two atoms of ethyl itself. When mono-chlorinated ether is heated with an alcoholic solution of potash, or with ethylate of sodium, an oily liquid is formed, which boils at 157° C., is heavier than water, and has a most refreshing and agreeable odor. The formula of this body is, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4 . C_4H_5O_2\end{matrix}\right\}O_2$, so that it must be regarded as ether in which one atom of hydrogen in the ethyl is replaced by chlorine and one atom by the radical, $C_4H_5O_2$, which the authors term oxethyl. By the continued action of ethylate of sodium upon the last mentioned substituted ether, the authors obtained an oil lighter than water, boiling at 168° C., and having the formula, $\left.\begin{matrix}C_4H_4 . C_4H_5O_2\\C_4H_4 . C_4H_5O_2\end{matrix}\right\}O_2$. In this, two atoms of hydrogen are replaced by two of oxethyl. Finally, the action of ethylate of sodium upon this body, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4 . C_4H_5\end{matrix}\right\}O_2$, gives rise to a new liquid, boiling at 148° C. and presenting the formula, $\left.\begin{matrix}C_4H_4 . C_4H_5O_2\\C_4H_4 . C_4H_5\end{matrix}\right\}O_2$. The new substances described are evidently the types of a large class of similar substituted bodies, and deserve in themselves an attentive study, especially with reference to physical properties and to products of decomposition.—*Comptes Rendus*, lix, 445. W. G.

4. *On a new mode of preparing oxygen.*—Robbins has given a method of preparing oxygen which is particularly interesting from a theoretical point of view, and which may hereafter be of much practical value. The process is based upon an experiment due to Schönbein, and consists in pouring dilute sulphuric acid upon a dry and powdered mixture of three equivalents of peroxyd of barium and one equivalent of bichromate of potash. Ozone and antozone are given off simultaneously, and unite to form common oxygen, which escapes with effervescence.—*Cosmos*, xxvi, 386. W. G.

II. GEOLOGY.

1. *On buried stems and branches in Illinois;* by J. S. BLISS. (From a communication to the Editors, dated Columbus, Wisconsin, Nov. 7.)—While passing through the county of Adams, in the State of Illinois, four years since, I learned of the existence of a well, which had been dug the year before, from which small branches of trees and twigs had been obtained. I repaired to the spot, and found the well to be twenty-five feet in depth; and was informed that, at a depth of twenty-three feet, the owner

came upon a layer of *fine black soil*, full two feet in thickness, in which were found branches an inch in diameter; I procured a shovel, and obtained several specimens three-eighths of an inch through, in a tolerable state of preservation. Just above this bed of *soil* the material was clay.

In another well, three miles from this, the same kind of soil was met with, after passing through clay for twenty-seven feet from the surface. In the soil twigs were found; but on being brought to the air they crumbled to pieces, though the *bark* and *heart* were plainly to be seen.

Four miles from the first mentioned well, there was another, still more remarkable, which was 30 feet deep. At a depth of 27 feet, the diggers came square on a log, twelve inches in diameter; they cut it off on both sides of the well, and on taking it out pronounced it to be *Black walnut*; but I think that it was probably some other kind of timber, that had been dyed in consequence of having been submerged, and that this caused the mistake. In the same soil was also found a *Buckthorn* in a tolerable state of preservation. The top of the tree was lying southeast.

Another fact, I have noticed in Wisconsin, is that stones protruding through the surface, such as those known as *hard-heads*, present their perpendicular sides to the north, northeast, and northwest. This is not confined to any small portion of the State, but to quite an extended region.

2. *On the Geology of Eastern New York*; by Professor JAMES HALL and Sir WILLIAM E. LOGAN. (Read before the Natural History Society of Montreal, Oct. 24, 1864, by T. STERRY HUNT.)—Professor James Hall and Sir William Logan spent a few days together last summer in examining some points of the geology of Eastern New York, and propose to continue their examinations next season, when we may expect from them a detailed account of their results. Their principal object was to compare the rocks of that region with some of those of Eastern Canada; and I have now permission, in the absence of these gentlemen, to lay before this Society some of the results of this exploration.

The shales of the Hudson River group, which are seen for a considerable distance north and south of Albany, disappear a few miles east of the Hudson, and are succeeded by harder and coarser shales, sometimes red or green in color, and passing into green argillaceous sandstones. These various strata, which are associated with concretionary and shaly limestones, are now recognized as belonging to the Quebec group. The line of contact between this and the much more recent Hudson River group has nowhere been clearly seen in this region, but the two series are readily distinguished by their differences in color, texture, and hardness,—differences which were formerly supposed to depend upon the partial metamorphism of the eastern portion, when this was looked upon as a part of the Hudson River group. The green sandstones and conglomerates of Grafton Mountain, formerly looked upon as a portion of the Shawangunk conglomerate, are recognized as belonging to an outlying portion of the Sillery formation. This mountain Professor Hall had found in a previous exploration (1844–45) to have, at a point farther south, a synclinal structure, and it probably lies in three low synclinal axes. The Sillery formation scarcely extends south of Rensselaer County.

Canaan Mountain is also apparently synclinal, and, while limestones appear in the valleys on each side of it, consists chiefly of slates, the highest beds being a hard green sandstone, sometimes shaly, without any of the conglomerates of the Sillery, although boulders and angular fragments of these are found in the adjacent valleys. To the east of this, Richmond Mountain, in Massachusetts, presents in its upper portion a compact green slate, passing upward into a harder rock similar to that of the summit of Canaan Mountain. To the southward, as far as Hillsdale, the sparry limestones of the Quebec group appear in the valleys, while the hills are of slate. Proceeding thence westward toward the river, only the lower portions of the Quebec group are met with, until we come upon the rocks of the Hudson River group.

Washington Mountain is also of slate, flanked by limestone, all of the Quebec group, and is probably synclinal in structure. The valley to the south of the mountain exhibits limestones, apparently alternating with slates. Columbia and Duchess counties appear to be mainly occupied by the shales of the Quebec group, with broad exposures of its limestones, until we approach the river to the westward, when the shales of the Hudson River group are met with, extending a considerable distance below the city of Hudson. The gneiss of the Highlands occupies the southeast part of Duchess County.

From Fishkill the explorers proceeded to Coldspring, crossing what Mather called the Mattewan granite, but which they found to be an altered sandstone. Soon after this they came upon the great gneiss formation of the Highlands of the Hudson, which continues beyond Peekskill. They failed to find the sandstone described by Mather as coming out at this place; nor was anything representing the Potsdam sandstone detected in approaching the Highlands from Fishkill, nor elsewhere along their northern limits. Near to Peekskill, in the valley of the creek, was found a low ridge of black slate, supposed to belong to the Quebec group, and a similar slate was observed along the north side of the Highland range, not far from the gneiss. The gneiss of the Highlands presents all the aspects and characteristics of that of the Laurentian system, as seen in northern New York and in Canada.

Further examinations are necessary to determine the extension to the northeast of the Laurentian rocks of the Highlands, and also the succession of strata to the southeast of them. The recognition of the Sillery and of the Quebec group in this region are great and important facts of its geology, and not less so the identification with the Laurentian system of the gneissic district of the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs. This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestones of Orange County and of New Jersey underlie unconformably the Lower Silurian strata.[1]—*Can. Nat. and Geol.*

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[1] See Am. Jour. Sci., [2], xxxii, 208, and also Kitchell's 2nd Annual report on the Geology of New Jersey, (1855,) page 184 and onward.

3. *Action of Glaciers*; by J. Ruskin.—What a river carries fast at the bottom of it, a glacier carries slowly at the top of it. This is the main distinction between their agencies. A piece of rock which, falling into a strong torrent, would be perhaps swept down half a mile in twenty minutes, delivering blows on the rocks at the bottom audible like distant heavy cannon,[1] and at last dashed into fragments, which in a little while will be rounded pebbles (having done rough damage to everything it has touched in its course)—this same rock, I say, falling on a glacier, lies on the top of it, and is thereon carried down, if at fullest speed, at the rate of three yards in a week, doing, usually, damage to nothing at all. That is the primal difference between the work of water and ice; these further differences, however, follow from this first one.

Though a glacier never rolls its moraine into pebbles, as a torrent does its shingle, it torments and teases the said moraine very sufficiently, and without intermission. It is always moving it on, and melting from under it, and one stone is always toppling, or tilting, or sliding over another, and one company of stones crashing over another, with staggering shift of heap behind. Now, leaving out of all account the pulverulent effect of original precipitation to glacier level from two or three thousand feet above, let the reader imagine a mass of sharp granite road-metal and paving-stones, mixed up with boulders of any size he can think of, and with wreck of softer rocks, (micacous schists in quantities, usually,) the whole—say half a quarter of a mile wide, and of variable thickness, from mere skin-deep mock-moraine on mounds of unsuspected ice—treacherous, shadow-begotten—to a railroad-embankment, *passenger*-embankment—one eternal collapse of unconditional ruin, rotten to its heart with frost and thaw, (in regions on the edge of each,) and withering sun, and waste of oozing ice; fancy all this heaved and shovelled, slowly, by a gang of a thousand Irish laborers, twenty miles down hill. You will conjecture there may be some dust developed on the way?—some at the hill-bottom? Yet thus you will have but a dim idea of the daily and final results of the movement of glacier moraines;—beautiful result in granite and slate dust, delivered by the torrent at last in banks of black and white slime, recovering itself, far away, into fruitful fields, and level floor for human life.

Now all this is utterly independent of any action whatsoever by the ice on its sustaining rocks. It *has* an action on these, indeed; but of this limited nature, as compared with that of water. A stone at the bottom of a stream, or deep sea-current, necessarily and always presses on the bottom with the weight of the column of water above it—plus the excess of its own weight above that of a bulk of water equal to its own; but a stone under a glacier may be hitched or suspended in the ice itself for long spaces, not touching bottom at all. When dropped at last, the weight of the ice may not come upon it for years, for that weight is only carried on certain spaces of the rock bed; and in those very spaces the utmost a stone can do is to press on the bottom with a force necessary to drive the given stone into ice of a given density (usually porous); and, with this maximum pressure, to move at the maximum rate of about a third of an inch in a quarter of a hour! Try to saw a piece of marble

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[1] Even in lower Apennine. "Dat sonitum saxis, et torto vertice torrens."

through (with edge of iron, not of soppy ice, for saw, and with sharp flint sand for feldspar slime,) and move your saw at the rate of an inch in three-quarters of an hour, and see what lively and progressive work you will make of it!

I say "piece of marble;" but your permanent glacier-bottom is rarely so soft—for a glacier, though it acts slowly by friction, can act vigorously by dead weight on a soft rock, and (with fall previously provided for it) can clear masses of that out of its way, to some purpose. There is a notable instance of this in the rock of which your correspondent speaks, under the Glacier des Bois. * * * *

[Mr. Ruskin continues with further explanation of his views, in the course of which he expresses the opinion that "the Glacier des Bois has not done more against some of the granite surfaces beneath it, for these four thousand years, than the drifts of desert sand have done on Mt. Sinai;" and "it never digs a hole" like the bed of a lake. He closes with a paragraph containing the following statement as to his early tastes and studies.]

I find it difficult to stop, for your correspondent, little as he thinks it, has put me on my own ground. I was *forced* to write upon Art by an accident (the public abuse of Turner) when I was two-and-twenty; but I had written a "Mineralogical Dictionary" as far as C, and invented a shorthand symbolism for crystalline forms, before I was fourteen: and have been at stony work ever since, as I could find time, silently, not caring to speak much till the chemists had given me more help.—*Reader, Nov.* 26, 1864, *the communication dated Denmark Hill, Nov.* 21.

4. *Geological Survey of California;* J. D. WHITNEY, State Geologist. —PALEONTOLOGY, Volume I: *Carboniferous and Jurassic Fossils,* by F. B. MEEK; *Triassic and Cretaceous Fossils,* by W. M. GABB. 244 pp. Small 4to, 1864. Published by order of the Legislature of California.—The principal results of the geological survey of California, up to the present time, have been briefly stated in our last volume at p. 256. Their number and importance are sufficient to show that the survey could not have been in better hands. Through the labors of Professor Whitney and his assistants, we are beginning to have some definite knowledge of the geology of the western border of the continent,—of the distribution of its rock-formations, of the age and structure of its mountains, of the origin of its metallic veins, of the action of rivers and glaciers in educing the features of the surface, besides many other points of geological and economical interest. The Report on the general Geology, as far as the subject is completed in the yet unfinished survey, will, we understand, be soon published. The volume just issued is the first of the two on the Paleontology of California. It treats of the fossils of the Carboniferous formation, the oldest fossiliferous beds yet observed, and of the Triassic, Jurassic and Cretaceous formations, leaving the Tertiary for the second volume.

The Carboniferous fossils make but a meagre list, the rocks of the age having small extent. Twelve or fourteen species are all that are described. Three of them are referred by Mr. Meek, though with some hesitation, to the widely spread *Lithostrotion mammillare, Productus semireticulatus* and *Spirifer lineatus.*

The Triassic species, described by Mr. Gabb, are 28 in number; the Jurassic, described by Mr. Meek, 12; the Cretaceous, described by Mr. Gabb, nearly 300, the larger part being new, and all pertaining, according to Mr. Gabb, to the Upper or White Chalk. Some of the general facts respecting the extent and character of the formations have already been published by us in the article in our last volume above referred to.

A volume of plates will soon be ready for delivery. It will contain figures of all the new species. The plates are lithographic, and thirty-two in number; and, as we know from an examination of many of them, are excellent in drawing and engraving.

The work is brought out in the first style of the art, both as to paper and printing. We know nothing superior, in these respects, from any American press.

5. *Oil region of Pennsylvania.*—[Extracts from a communication by Ira Sayles, dated Meadville, Crawford Co., Pa., Nov. 13, 1864.]—As shown in the Geological Map of Pennsylvania by Professor Rogers, the surface-rocks of the higher country in the oil region of western Pennsylvania are mostly Carboniferous, while those of the valleys appear to be Devonian of the Chemung and Portage groups. In the borings, a whitish sandstone, called the *first sand-rock* is met with at a depth of 70 to 200 feet; and wells penetrating only through this bed generally yield a thick oil. 100 to 200 feet below this, lies the *second sand-rock*, of similar aspect to the first; and from beneath this there is usually another "show of oil," a heavy oil, though less so than the former. From this rock proceed nearly all the wells of the Alleghany river, while the wells of French Creek, with few exceptions, descend only to the *first* sand-rock. Below the *second*, 100 to 200 feet, there is a *third* sandrock, and from beneath this comes the pure, limpid, light oil. To this depth descend most of the "flowing" or fountain wells, four of which, when first opened, (the Empire well, one of them,) deluged the region around with 3000 barrels of oil per day. The wells of Oil Creek are mostly in this rock.

The valleys have been selected in nearly all cases for the borings under the impression that breaks in the hills indicate chasms in the rocks beneath. But there appears to be no reason why oil should not be reached also by borings through the higher land. There are evidently two kinds of sources; open cavities affording the free flowing wells, sometimes intermittent; and the oil-bearing shales giving a slower yield, as if by a gradual percolation from rocks that partially retain it; and the latter at least should be encountered as well through the hills as the valleys, if the boring is carried down to the same level. Moreover, there is no certainty that fissures or chasms in the rocks may not underlie the hills.

It is a little remarkable that the oil from beneath the upper sandstone should be the heavier, while that from beneath the lowest is the lightest. Although there is no evidence of any connection between the oil repositories at these three different levels, the sources of the same level often communicate. This has been abundantly proved, by a sudden diminution in the supply of an older well in consequence of sinking a new one near by.

The quantity of oil produced now does not materially differ from that of two years ago—viz: nearly 6000 barrels daily. The number of wells has been increased in that time; but about as many old ones have been abandoned as new ones opened. Fewer of the new wells are great *spouting* wells than formerly. Pumping wells are very constant in their yield.

6. *Petroleum in California.*—A petroleum region on the coast of Santa Barbara, near Buenaventura, in Southern California, has recently been visited by Prof. Silliman, Jr., from whose Report we extract a few facts. Ten miles North of Buenaventura there is a mountain ridge, 2000 feet in height, extending for 13 miles from east to west, which consists in part of bituminous shales supposed to be of either the Cretaceous or Tertiary period. The dip of the layer is 70° to 80° to the north. From these shales, mineral oil comes out in many places, and at some points very abundantly. One of the wells is 30 feet in diameter, and is full of a tar-like oil, boiling with the escape of marsh-gas. There is also an area of asphaltum three-fourths of a mile long by half a mile wide, exuding tar and rock-oil at numerous points, besides several other oil springs, the places of discharge in all exceeding twenty. The range of bituminous shales extends to the south, occurring at intervals for a hundred and fifty miles, and also to the north as far as Glenroy, in Santa Clara County, or about 80 miles from San Francisco.—*Extract from Prof. Silliman's Report.*

III. BOTANY AND ZOOLOGY.

1. *Dioico-dimorphism in the Primrose Family.*—Mr. John Scott, late of the Edinburgh Botanic Garden, has communicated to the Linnæan Society an elaborate paper, entitled, *Observations on the Functions and Structure of the Reproductive Organs in the Primulaceæ*, which has recently been published in the 8th volume of that Society's Journal of Proceedings. Mr. Scott has followed up Mr. Darwin's well-known researches by many additional experiments, has confirmed his results, and brought out others of no small interest. The paper is long, and, on the whole, clear and satisfactory, but with occasional vagueness or obscurity of language, from the attempt of an acute, but perhaps rather untrained mind to indicate the bearings of the subject further than they have been clearly made out. A good summary appended to the paper sets forth the principal points.

Of 54 species or forms of Primula which he has examined (many of them only in the herbarium),—

"36 are truly dimorphic, presenting both long- and short-styled forms; 13 in which the long- or short-styled forms, respectively, have alone been observed by me; and 5 species and one variety with non-dimorphic characteristics, i. e., presenting stamens and pistils of an equal length.

"The allied genera *Hottonia* and *Aretia* have also truly dimorphic species; whereas other allied genera, *Dodecatheon, Soldanella*, and *Cortusa*, are very generally characterized by species presenting the structural characteristics of the long-styled form only, without, however, any decreased fertility arising from their hermaphrodite conjunctions.

"The general differences of the two sexual forms may be thus briefly summed up:—First, the long-styled forms have pistils equalling in length the tube of the corolla; stigmas usually larger and rougher; stamens attached to, or frequently below, the middle of the corolla-tube, whose diameter is thus expanded upward; pollen-grains generally smaller and more transparent. Secondly, in the short-styled form the pistil is short, not rising above half way up the corolla-tube; stigma generally smoother and depressed on the summit; stamens attached to the mouth of the corolla-tube, causing an abrupt expansion; pollen-grains generally larger and more opaque. According to all the trials, these structural differences are accompanied by equally remarkable functional differences,—the pollen of the long stamens being alone adapted to fertilize the long pistils, and the pollen of the short stamens to fertilize the short pistils. By applying, on the other hand, either form pollen to own form stigma, i. e. effecting a homomorphic union, the degree of fertility relatively to the above, or heteromorphic union, is greatly decreased. Analogous, though less striking, functional differences, however, occur without any appreciable change of structure, as shown by the *P. verticillata*, e. g., yielding a much higher grade of fertility by its diœcious than its hermaphrodite conjunctions. Such an instance from a genus whose members are so generally characterized by a sexual dimorphism, naturally leads me to regard it as indicative of the acquirement of similar characteristics. An objection to this view may be urged from the occurrence of species which, having no immediate affinity with any structurally dimorphic species, nevertheless present individuals incapable of fertilization by own pollen, though perfectly susceptible to reciprocal fertilization, either with another individual of the same species, or one of a distinct species. To this category, at least, those who disbelieve in the genetic affinities of organic beings will no doubt refer the case of *P. verticillata*, and simply regard it as further illustrative of our ignorance of the conditions upon which sterility, in its varied grades, depends. Those, on the other hand, who believe in the existence of these genetic relations will look with an intelligent interest upon these functional peculiarities of the *P. verticillata*, and regard them, mayhap, as the primary indications of a tendency to assume those remarkable sexual characteristics of the correlated species, and thus presenting an illustration of incipient dimorphism.

"The usual differences in the fertility of the heteromorphic and homomorphic unions will be best appreciated by giving the mean results from the unions of several species. Thus, taking the five heteromorphic and homomorphic unions given above, namely, *P. Auricula*, *Sikkimensis*, *cortusoides*, *involucrata*, and *farinosa*, we see, from the mean results of their combined products, that for every 100 seeds yielded by the heteromorphic unions, only twenty-four are yielded by the homomorphic unions —the heteromorphic thus exceeding the homomorphic unions in about the proportion of five to three! I have also shown the remarkable fact that the pollen of a distinct species will produce a much higher grade of fertility than an ordinary homomorphic union, i. e., a flower's own pollen!

"It is well known that A will fertilize B, and B will not fertilize A. I have given instances of this law with Primulas. I have also shown the new and remarkable fact, that of the two forms of the same species the pollen of the one, but not of the other, will fertilize a distinct species!

For example, the long-styled *P. Palinurii* can be fertilized readily by pollen of the long-styled *P. Auricula;* yet, after numerous trials, I have failed to effect a single union between the long-styled form of the *P. Palinurii* and the short-styled *P. Auricula.* How utterly inconsistent, then, are such facts with the teachings of those who would have us believe that an absolute causal relation exists between the sterility from hybridism and systematic affinity! On the other hand, how unequivocally do these cases show us that the greater or less facility of one species to unite with another is, as Mr. Darwin has sagaciously argued, 'incidental on inappreciable differences in their reproductive systems. And that there is no more reason to think that species have been specially endowed with various degrees of sterility to prevent them crossing and blending in nature, than to think that trees have been specially endowed with various and somewhat analogous degrees of difficulty in being grafted together in order to prevent them becoming inarched in our forests.'[1]

"Probably the most remarkable result from my observations is that when the dimorphic species cease to be dimorphic, their reproductive functions are greatly modified. Thus, in the case of the Cowslip, for example, we have seen that an ordinary homomorphic union yields about fourteen seeds per capsule, the heteromorphic about twenty-four seeds per capsule, whereas the form with stamens and pistils of an equal length yields, when fertilized with its own pollen, thirty-four seeds per capsule! Thus the non-dimorphic form by own pollen exceeds, first, the homomorphic unions in the proportion of 5 to 2, and secondly, the heteromorphic in the proportion of 3 to 2! Again, from the four different unions of the long- and short-styled forms with the non-dimorphic form, the seed-results in each case fall considerably below an ordinary homomorphic union: thus the mean results of the unions of the non-dimorphic with long- and short-styled are six seeds per capsule, whereas the pure homomorphic unions of the latter give an average of thirteen seeds per capsule—that is, as two to one!

"Connected with these are the remarkable changes in the fertility of the [differently] colored varieties of the Primrose, the red variety yielding no seed when fertilized by pollen of either yellow or white varieties: the reciprocal crosses of these, i. e. the pollen of the red variety applied to the stigmas of the yellow and white, are also absolutely sterile! On the other hand, fertile unions may be effected by the reciprocal crossing of the yellow and white varieties, though in every case we have found that the average seed-result of such unions is considerably under that of the pure unions of these forms.

"Whether or not the ultimate tendency of dimorphism is a complete separation of the sexes, I think we have the clearest testimony that dimorphism has not always been a genealogical characteristic; and furthermore, that the two forms did not *per saltum* assume these structural and physiological characteristics. I here allude to the evidence afforded by the non-dimorphic Cowslip—namely, the resumption of perfect hermaphrodism, and the occasional production of intermediate stages between this and the normally dimorphic. These, taking us back in the

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[1] Origin of Species, 3d ed., p 299.

genealogical line, show us an original non-dimorphic progenitor, and the graduated plan by which it gave rise to a dimorphically characterized race."

The most noteworthy points are:—1. That, in a genus which has many species with dimorphous flowers, there are some which present, so far as known, only one of the two forms (say the long-styled, which is perfectly fertile *per se*), as well as others with stamens and pistil of equal length. 2. That the Red Cowslip, a clear variety of the common one, has become non-dimorphic, and, with this change of structure, has become much more fertile of seed than the heteromorphic unions of the Common Cowslip. And the Auricula offers a similar case. But, on the other hand, a non-dimorphic variety of *Primula farinosa* is less fertile. These changes in the genetic system of a species seem very remarkable. 3. Still more remarkable is the result of fertilizing the Red Cowslip with its ancestor the common yellow form; this union, and also that of the Common Cowslip fertilized by the Red, being very sterile;—thus supplying that *desideratum* which has been called for as a test, "a physiological species produced through variation." 4. That the two forms of a dimorphous species hybridize with very different degrees of facility with distinct species. And that such a cross sometimes yields more seed than either homomorphic unions with own pollen. 5. Mr. Scott confirms (though it is not stated in his summary) Mr. Darwin's statement, that the long-styled forms artificially fertilized with their own pollen are about twice as fertile as the short-styled similarly fertilized with its own pollen; while, as is well known, when left untouched, the short-styled is most fertile, owing to the relative position of the anthers and stigmas. So that the advantage of position in the latter is counterbalanced by an increased differentiation of pollen and stigma with respect to their mutual action, Nature thus rendering the short-styled equally dependent with the long-styled on external agencies,—thus keeping it in existence, and more completely effecting the great object of dimorphism,—the intercrossing of individuals.

We may here append the remark that the Thymelæaceous genus *Lewcosmia* is dimorphous, and some species of *Drymispermum* exhibit one if not both of the two forms. A. G.

2. *Observations upon Dimorphous Flowers;* by H. von Mohl; in *Bot. Zeitung*, Oct. 1863, translated in *Ann. Sci. Nat., Bot.*, April, 1864.—These observations are principally upon that case of dimorphism in which, besides the ordinary hermaphrodite (but often infertile) flowers, there are other and surely fertile ones, of simplified structure, apetalous, or rather cryptopetalous, and which, "their development being as it were arrested in the bud," and fertilized without opening, we had long ago termed "precociously fertilized." Some remarks were made upon them in this Journal, in November, 1862, (p. 419), stating that Nature, in this case, takes as much pains to secure self-fertilization as she does in the other case of dimorphism (represented by *Primula, Houstonia, &c.,*) to secure cross-fertilization. This is the conclusion which Mohl also reaches after a careful investigation of the common instances, viz: in *Specularia, Impatiens, Viola, Oxalis, &c.*, clearly showing that such flowers, with a possible exception in violets, must needs be self-fertilized. The record of his

own observations is preceded by a good history of what was known upon the subject, from the beginning made by Dillenius down to the communication of Michalet in the *Bull. Soc. Bot. de France*, 1860. That the little tympanum which covers the minute anthers and stigmas of *Specularia perfoliata* until after fertilization, is a rudimentary corolla, however, was shown by Dr. Torrey, in a paper read before the Lyceum of Natural History, New York, in 1830, but not published.

One of the most interesting things to notice, and a curious confirmation of the intention to self-fertilize in the cases here considered, is the unusual paucity and the greater activity of the pollen in the anthers of the small and closed flowers. In those of *Oxalis Acetosella*, as Mohl informs us, the larger anthers contain only about two dozen, the smaller scarcely a dozen pollen-grains,—only a few times more numerous than the ovules they are to fertilize, while in the normal blossoms they are in countless excess,—the economy in number being in proportion to the sureness that they will do their work. Moreover, these pollen-grains never leave the anther, but, (as was observed by D. Müller and by Michalet, but misunderstood, and neatly shown by Mohl,) send out their tubes to seek the stigmas, adjacent, indeed, but often at a considerable relative distance, these tubes directing their course with the same precision, and in the same mysterious manner, apparently, that they ordinarily do when having reached the ovarian cavity they seek the orifice of the ovules. This is well seen in *Viola*, where, though it is barely possible that extraneous pollen might sometimes be introduced, yet, in view of the immediate contiguity of the stigma to the line of dehiscence of the two more fertile anthers, through which the pollen-tubes are emitted, close-fertilization here cannot be doubted. So Mohl concludes that, "considering that these little flowers are constantly fertile; that in some plants they are the only ones that are fertile, while in others they are far more plentiful than the large flowers, and that upon these the propagation of the plant mainly depends, it results that it is not a general law in hermaphrodite flowers that Nature should favor fecundation by the pollen of another flower in preference to its own pollen." Mohl proceeds to state that self-fertilization is equally secured in some non-dimorphic flowers, and instances *Fumariaceæ* as a case where this is "absolutely necessary, the transport of pollen from one flower to the stigma of another appearing to be absolutely impossible, on account of the special structure of the corolla, and of the intimate connection of the exterior [interior?] petals which enclose the anthers and stigma." We had thought the same, although the nectariferous sacs should have made us pause; but Mr. Darwin called our attention to the great facility with which the cap or united tips of the inner petals would be pushed off to either side by a bee visiting the blossom and seeking honey from its nectary. To this it sufficed, as we thought, to answer, that pollen was actually falling from the anthers upon the stigma they surrounded a full day before the tips of the outer petals opened, i. e., before there could be any access whatever from without. The reply to this seeming demonstration duly came in Mr. Darwin's capital discovery that, in *Linum* and other cases, the pollen may be less potent or even impotent upon its own stigma, while that from another flower is prepotent! Still these little precociously-fertilized flowers, of various and diverse families, stand as veritable exceptions to the universality of the law which Darwin has ably deduced.

What causes the pollen-grains to emit their tubes without contact with the stigma, and indeed at a considerable distance from it, is an utter mystery. Mohl remarks here an analogy with the case of *Asclepius*, and that in *Viola*, *Specularia*, &c., as Brown has stated for *Asclepias*, there is no liquid poured out by the stigma which might somehow place the pollen in relation with the stigma.

Finally, Mohl calls attention to an early remark of Linnæus, that, in the year 1753, a considerable number of plants raised in the Upsal garden, for which the Sweedish climate was too cool, flowered clandestinely, yet produced fruit. We may note that in the Cambridge Botanic Garden, *Oxybaphus nyctagineus* copiously fruits from the bud in cool and cloudy weather: so do one or two Pavonias for a considerable part of the season. And, several years ago, *Nyctaginia capitata* of Texas seeded abundantly all one summer without expanding a single flower, or even producing a full-grown flower bud. The next year it blossomed freely.

Mohl justly notes that these diminutive bud-fruiting flowers are often produced earlier in the season, as in *Specularia*, *Impatiens*, &c., but sometimes later than the normal blossoms, as in *Oxalis* and *Viola*. And he shows that the former conform (he looking upon such flowers as preponderantly female) to Knight's hypothesis, (drawn from cucumbers and melons,) that a high temperature favors the development of male flowers, while a lower temperature is more propitious to female blossoms, —a view which Thury took as the starting point of his interesting investigation upon the production of sexes in the animal kingdom. But Naudin has appended to the French translation of Mohl's paper, an important note, in which he declares that his sedulous observations upon *Cucurbitaceæ* for the past ten years are far from confirming Knight's theory; that in this family male and female blossoms appear independent of conditions of temperature, but intimately connected with peculiarities of temperament, which vary from species to species, from race to race, or even among individuals of the same race. And he notes especially (what we suppose applies generally) that the races of melons, squashes, and gourds which have long been cultivated in northern Europe are comparatively more precocious, and need much less heat for maturing their fruit, than those of the same species which have been recently brought from tropical regions. Also that the same *Cucurbitaceæ* arrive at adult age, i. e., to the age for flowering and fruiting, far sooner under the hot and cloudless sun of the south of France than under the cooler and obscurer skies of Paris, where they vegetate richly, but hardly blossom. Naudin remarks, also, that many monoicous *Cucurbitaceæ* tend to become dioicous, without assignable cause; and, as especially fatal to Knight's view, that *Coccinia diversifolia*, cultivated simultaneously at Paris and at Hyères, in a great number of individuals, has produced only male blossoms at Paris, but an abundance of both sexes (monoicous) at Hyères. A. G.

3. *Najas major*, *Ruppia maritima*, *&c.*, *discovered at Salina*, *N. Y.* —The September number of Dr. Seemann's *Journal of Botany* contains a *Revision of the Genus Najas*, by Professor Braun of Berlin. To the section *Eunajas* three species are assigned, viz., *N. major*, All., *N. mari-*

cata, Del., of Egypt, and *N. latifolia*, Braun, from the lake of Valencia, Venezuela. The section *Caulinia* has *N. flexilis*, which is found here and there in the North of Europe, and several varieties in the West Indies, Venezuela, &c., *N. arguta*, H.B.K., in New Granada, *N. minor*, All., in Europe, Egypt, and India, *N. fasciculata*, Braun, in India, and *N. graminea*, Del., in India, Egypt, &c. *N. major*, to which our attention is at present directed, under several varieties, is widely distributed throughout the Old World, in slightly brackish or fresh water, but it was also detected by Chamisso in the Sandwich Islands. What is equally remarkable, Prof. Braun has seen specimens collected in Florida by Cabanis.

Shortly before Prof. Braun's article came to hand, *Najas major* was discovered in Onondaga Lake, first by Judge G. W. Clinton, on the northern border of the lake (between Salina and Liverpool), and soon after by Mr. John A. Paine, Jr., on its western side, growing luxuriantly and abundantly, and quite fruitful. The marshy borders of the lake, as is well known, are brackish, at least in the vicinity of the salt-works, and abounding in maritime and submaritime plants. To the species formerly known there, the above gentlemen have added *Chenopodium glaucum*, *Leptochloa fascicularis*, and we believe some others. But a more interesting discovery, of a water-plant not before met with away from the sea coast, was recently made by Mr. Paine, he having detected *Ruppia maritima* in Onondaga Lake, along with *Najas major*, equally in a fruiting state.

A. G.

4. *Spontaneous return of hybrid plants to their parental forms.*—We have been prevented from preparing, as we wished to do, abstracts of the two rival essays upon Hybridity in Plants, viz., that of Mr. Naudin, of the Jardin des Plantes, and of Dr. Godron, of Nancy. The latter was published in full in the *Annales des Sciences Naturelles*, 4th series, volume xix, and also a part of the former, the whole memoir of Naudin, with details of the experiments and figures, having been accepted for publication in the *Mémoires* of the French Academy. The prize for which the essays competed were awarded to Naudin. A brief account of the two essays appeared a year ago in the Natural History Review. As respects one, the principal question, to use the summary of the Review, "the general result is, that Dr. Godron says: simple hybrids are absolutely sterile. Mr. Naudin says: hybrids are fertile whenever their anthers contain well organized pollen. Of 30 to 40 hybrids experimented upon, three-fourths were found to produce seeds capable of germination."

Upon another point of equal interest, Naudin's testimony is explicit, he maintaining that hybrid plants, however constant at first, tend in subsequent generations to a separation of the two specific elements, which are, as he expresses it, rather intermixed than truly combined, so that they would at length resolve themselves into the two parental types, or, by failure on one side, return to the one on the other. Naudin returns to this subject in a note in the *Flore des Serres* for July, 1864, where he gives the result of his experiments upon our common sorts of thorn-apple, *Datura Stramonium* and *D. Tatula*. These have more commonly been taken for varieties of one species; but their specific distinction has been maintained, especially of late, by various arguments. According to Naudin they are truly distinct species, which do not sensibly vary. One always exhibits green stems and pure white flowers; the other, dark-purple stems and violet-tinged flowers. These two thorn-apples Mr. Naudin crossed, in 1855, and obtained a hundred or more hybrids, both *Tatula-Stramonium* and *Stramonio-Tatula*, both just alike, and exactly intermediate between the two species in the coloration of the stems and flowers. They had, however, the peculiarity of a gigantic size, attaining at least twice the size of their parents, and a tendency to sterility, which was manifested in the failure of all the flower-buds which were produced at the first forkings of the stems. The later flower-buds opened, however, and were perfectly fertile, the pods being as large and as full of good seeds as those of either parent. In 1861, these seeds (of *Stramonio-Tatula*) were sown, and produced a second generation like the first. Seeds of this crop were sown in 1862, and twenty-two seedlings were preserved for experiment. Five of these developed as *D. Stramonium* in all its purity, i. e., had green stems, white flowers, only the ordinary height, and all the flowers fertile, ripening their pods in the first forks, three months earlier than did their immediate parents. Nine individuals returned as completely to *D. Tatula* as the five did to *D. Stramonium.* Two others seemed to be *D. Tatula*, and were equally reduced in size and fertile from the first forks, but they still showed in their paler coloring a trace of the other ancestor. The remaining six of the twenty-two showed somewhat more of it, both in color and in the tallness and lateness of fructification; expressed numerically, Naudin estimates that they were, say, nine-tenths *D. Tatula* and one-tenth part *D. Stramonium.* "Here, then," says Naudin, "is a hybrid completely intermediate between the two parent species, which, left to itself, fecundated by its own proper pollen, is spontaneously dissevered at the second generation, dividing its offspring between the two species. And it is to be remarked that the division is very unequal, *D. Tatula* taking the lion's share, totally or nearly reclaiming seventeen individuals out of the twenty-two. This unequal division is common in hybrids, and sometimes goes to the extreme, one of the parents wholly disappearing from the hybrid progeny, which thus passes over entirely to the other species."

As to crossing species and their hybrids, in view of obtaining diverse and particular varieties, Naudin shows, accordingly, that, to obtain the desired results, the hybridizer should take pains to cross those mongrels which tend toward one parent with those which tend to the other, thus opposing the tendency to return to the specific types, which would otherwise surely take place. By doing this for a few generations, the two hereditary tendencies become, as it were, thoroughly confused, and a multitude of new variations break out in all directions, from which the amateur may select what he likes for preservation and further experiment. This is according to a principle first expounded by that excellent investigator, the late Louis Vilmorin. A. G.

5. *Flora of the British West Indian Islands;* by A. H. R. GRISEBACH, M D.; Professor of Botany in the University of Göttingen. London, Lovell Reeve & Co. 8vo, parts VI and VII, 1864.—These parts, extending from p. 507 to p. 789, complete this most valuable work, which, as was intended, closes with the Vascular Cryptogamia. A full index of the

species is appended, and a list of colonial names. The preface gives an account of the circumstances under which the work was undertaken, and of the materials which the author has so sedulously and promptly elaborated. West Indian botany was very difficult and confused: "Almost all the principal authors who have written on West Indian plants belong to the last century, and consequently to the Linnæan school, and a general synopsis of West Indian plants has never before been attempted, not even by Swartz, whose Flora contains descriptions of his new species only, with a few remarks on allied forms." Moreover, the British West Indies offer only the separate fragments of a larger flora; and Trinidad, as its geographical situation indicates, naturally belongs to the flora of Venezuela and Guiana. The northern Bahamas might be supposed to have a vegetation very like that of East Florida, from which they are separated by the Gulf Stream; but this seems not to be the case. "Jamaica, again, from its mountainous character and more distant position, most of the leeward islands, from being wooded volcanos, and the majority of the windward ones, with a dry climate and a low calcareous soil, form three divisions of this tropical archipelago, which show as many peculiarities. Thus the whole of the British West Indies, as comprised in this flora, may be divided into five natural sections, each with a distinct botanical character." Altogether, they amount to about 15,000 English square miles, or nearly twice the area of Wales. But yet Hayti alone is nearly twice, and Cuba nearly thrice, as large as all the British Islands together, and not only far richer in vegetation, but far less explored, the publications of Jacquin, Swartz, &c., having been almost confined to the British possessions; so that it was with old species mainly, that Dr. Grisebach had to deal, those which were "the foundation, indeed, of our scientific knowledge of the flora of tropical America." And these "have so often been misunderstood that their synonyms are far more numerous than their numbers." A general West Indian Flora being out of the present question, we learn with interest that Dr. Grisebach is preparing a special paper on the geographical range of the West Indian plants, including the capital island of Cuba, which Mr. Charles Wright has so industriously and successfully explored through its length and breadth, and is expecting still further to explore.

We may here add the remark that Mr. Wright's third distribution of phænogamous plants and ferns—the fruits of his labors for the last three years—has just been made, consisting, in the fullest sets, of about 1800 species or numbers, (including some which have been redistributed under old numbers,), and that the greater part of them have already been determined by Dr. Grisebach, the now recognized authority in West Indian botany. An equally rich field probably remains in Hayti, to tempt an adventurous explorer. The Bahamas, also, bare as they seem to be, "have proved very interesting," and are thought to promise still many novelties.

The series of Natural Orders adopted in this work being different from the ordinary ones, and especially from that of the other colonial floras, mainly owing to the intercalation by Dr. Griesbach, of the apetalous or reduced forms among the polypetalous orders—where many of them clearly belong,—the author has added a tabular arrangement more in accordance with the Candollean series, and with that employed in the new *Genera Plantarum* of Bentham and Hooker, which will be the natural companion of these colonial floras. These volumes are all written in English; and it is interesting to see how nearly at home the Göttingen Professor makes himself in a language foreign to him. A. G.

6. *Florula Australiensis: a Description of the Plants of the Australian Territory*, by GEORGE BENTHAM, F.R.S., P.L.S., assisted by FERDINAND MULLER, M.D., F.R.S. & L.S., etc., vol. II, *Leguminosæ* to *Combretaceæ*. 1864. pp. 521, 8vo.—That the second volume of this important work has been brought out so soon after the first, may be attributed, not merely to the indomitable industry and force of the author, but also to the fact that four-fifths of its pages are devoted to the great order *Leguminosæ*, to which Mr. Bentham has previously devoted much attention. In fact, to him the reformation of the order from the state in which DeCandolle left it, is almost wholly due. He first indicated, we believe, the floral characters which distinguished the Cæsalpineous from the Papilionaceous suborder, settled the tribes of the latter upon their present basis, and, besides various special memoirs, revised the *Mimoseæ* several years ago in a complete monograph. The huge genus *Acacia* is most largely Australian, where, indeed, of all Phænogamous genera it is most numerous in species; and the Phyllodineous Acacias are almost exclusively Australian. The present flora describes 293 species of *Acacia*, of which 271 are of the Phyllodineous section. These are so neatly analyzed by a tabular key, of the sort which Mr. Bentham makes so much of,—that the crowd of specimens in the herbarium may be most readily determined and arranged. *Chrysolalanceæ* are here recombined with *Rosaceæ*. *Saxifrageæ* (we know not why the form *Saxifragaceæ* is refused, and *Saxifrageæ* therefore used as the name both of an order and of a tribe) are well made to follow immediately after *Rosaceæ*; then come *Crassulaceæ*, of course; and *Droseraceæ*, retained as a good order. The volume is smaller than its predecessor, closing with *Combretaceæ*, not to sever the *Myrtaceæ*, so largely represented in Australia. A. G.

7. *Upon a form of budding in some Insect Larves.*—In the last number of the *Zeitschrift für wissenschaftliche Zoologie* are two articles of interest,[1] upon a form of multiplication by budding occurring in some larves of dipterous insects, and also containing a description of a new genus and species of Cecidomyians founded on one of the insects in which it was observed. It seems that Nicolaus Wagner published, in the memoirs of the Kazan Institute for 1862, an account[2] of a larval multiplication by a sort of internal budding, and that C. E. v. Baer confirmed the discovery last year in the 6th vol. of the St. Petersburg Bulletin, supplementing it with the full history of the larve, of its development into a perfect insect, and of the deposit of a few large eggs thus fructified. In July of 1864, Dr. Meinert published in the Danish *Naturhistorisk Tidsskrift* an account

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[1] Weitere Erläuterungen über die von Prof. Nic. Wagner beschriebene Insectenlarve, welche sich durch Sprossenbildung vermehrt, von Fr. Meinert. Aus dem Dänischen mit Bemerkungen übersetzt von C. Th. v. Siebold.

Die ungeschlechtliche Vermehrung der Fliegenlarven von Prof. H. Alex. Pagenstecher. Zeits. f. Wiss. Zool. xiv: 4 Oct., 1864.

[2] This was noticed in the Zeitschrift f. Wiss. Zool., xiii, 513, but the writer of this has been so situated that it wholly escaped him, and he has not yet seen that notice.

of the same facts as observed by him, and also a description of the perfect insect as a new genus and species, with the name *Miastor metraloas.* The first of the two articles cited above, is a translation of a part of Dr. Meinert's paper, with some remarks by Dr. Siebold, the editor, which, he says, was just completed when he received the second article. This latter is a well illustrated and detailed account by Dr. Pagenstecher of a similar larval multiplication occurring in another species of dipterous insect.

It will be sufficient here, after calling attention to these articles, to give the principle fact confirmed in them. This is, that an insect larve has been seen to have egg-like bodies within the abdominal cavity; that these have been seen to develop into larves, which finally bursting out became free and resembled in every way the stock larve; that then, or perhaps after a second similar larve-brooding, the larves developed into a perfect dipterous insect which laid eggs and started anew the circle of species-life; and lastly, that this is seen in more than one species.

There are some points not altogether agreed upon by the observers above referred to. One is as to the place and the material at and from which these larves are formed in the larve-stock. Dr. Meinert describes them as formed from the granular fatty mass of the abdominal cavity, which he regards as a sort of remnant of the embryonic yolk division. Dr. Pagenstecher, on the contrary, says he saw none of these fat cellules in the way of developing into eggs, and thought these latter wholly different from any cells approaching their size, and sums up as follows: "In some unknown part of the body, cells are set free, which, without being specially fructified, assume the character of eggs. These develop into embryos that exist in a sort of pupa state within the mother-body, and develop at the expense of the parent structures. Finally they break forth, the larve-stock dying, and become independent themselves."—pp. 414–5.

W. C. M.

IV. ASTRONOMY.

1. *Discovery of a new Planet, Terpsichore,* (81).—Mr. Tempel, at Marseilles, discovered on the 30th of September a planet equal in brightness to a star of the 10th magnitude. The following elements are given by Mr. Tietjen:

Elements of Terpsichore (81).

T = 1864, Oct. 23·0, B. m. t. i = 8° 45′ 43″·0.
M = 351° 31′ 52″·0, φ = 7 32 35 ·4.
π = 23 40 12 ·1, μ = 765″·494.
☊ = 2 48 29 ·3, log. a = 0·444043.

2. *New Comet.*—Another Comet was discovered on the 9th of September by Mr. Donati, at Florence. It was very faint, and seen with difficulty. Mr. Celoria, of Milan, gives the following elements, computed from observations of Sept. 9th, 11th, and 13th:

T = 1864, July, 27·8829 m. t. of Milan.
☊ = 175° 11′ 57″·4,
π = 190 10 34 ·7,
i = 44 56 53 ·6,
log q = 9·787184, motion retrograde.

The comet, according to these elements, passed its perihelion before those heretofore known as Comet I, 1864, and Comet II, 1864.

3. *Numbers of stars in the northern hemisphere.*—In the last edition of his *Wunder des Himmels*, Prof. Littrow gives a summary of the numbers of stars that are in Argelander's charts of the northern hemisphere.

From N. declination, 0° to 20°, 110,987 stars.
" " 20° to 40°, 105,082 "
" " 40° to 90°, 108,131 "

Classified according to magnitude there are,

Mag.	No. of Stars.	Mag.	No. of Stars.
1–1·9	10	6–6·9	4,328
2–2·9	37	7–7·9	13,593
3–3·9	128	8–8·9	57,960
4–4·9	310	9–9·	237,544
5–5·9	1,016		

There are, besides these, 60 nebulæ and 64 variable stars.—*Heis' Wochenschrift, Sept. 28th.*

4. *On the age of the moon's surface;* by J. Nasmyth.—The views I entertain on the subject in question are these, namely, that, as a direct consequence of the small mass of the moon, and its comparatively large surface, it must have parted with its original cosmical heat with much greater rapidity than in the case of the earth, and consequently the moon must have assumed a final condition of surface-structure ages before the earth had ceased from its original molten condition. And as the moon in all reasonable probability never possessed an atmosphere or water envelope (it certainly has none such now), while the earth has both, the action of the earth's atmosphere, and especially that of its ocean when it existed in the first instance as a vast vapor envelope, ere the earth had cooled down so as to permit the ocean taking up its final position *as an ocean*, this mighty vapor envelope must have retarded the escape into space of the cosmical heat of the earth millions of ages after the moon had assumed its final condition as to temperature.

Therefore it is from such considerations I am led to the conclusion that the surface features and details of the moon present to us a sight of objects the antiquity of which is so vast as to be utterly beyond the power of language to express, and scarcely less so for the mind to conceive.

Such considerations appear to me to enhance so vastly the deep interest which ever attends the examination and contemplation of the moon's wonderful surface, that I would earnestly urge those who agree with the soundness of these views to bear them in mind the next time they have an opportunity to behold the marvellous details of the lunar surface, as I am fain to think that in doing so the interest of what is there revealed to them will be rendered vastly more impressive.—*Proceedings Manchester Lit. and Phil. Soc.*, Nov. 15, 1864.

5. *On the probable error of a Meridional Transit-observation, by the eye-and-ear, and Chronographic method;* by Edwin Dunkin, Esq.—At the close of an elaborate discussion of the observations made at the Royal Observatory at Greenwich, in 1853 and 1857, presented to the Royal Astronomical Society in May, 1864, Mr. Dunkin says:

In laying before the Society the preceding abstracts, which are the results of a great mass of computations, I consider that I can with some confidence offer the following conclusions, which are derived from this discussion:—

(1.) In "*eye-and-ear*" observations, the probable error of a Greenwich transit observed in 1853 over *one wire* is $\pm 0^{s}\cdot078$, while that of a *complete* transit over the seven wires is $\pm 0^{s}\cdot029$. In *chronographic* observations, the probable error of a Greenwich transit observed in 1857 over *one wire* is $\pm 0^{s}\cdot051$, and that of a *complete* transit over the nine wires is $\pm 0^{s}\cdot017$.

(2.) There does not appear to be any certain difference in the probable error of transits of stars between the first and sixth magnitudes.

(3.) In "*eye-and-ear*" transits, for stars whose N.P.D. is greater than 60°, it would seem that the probable error of a transit increases slightly as the N.P.D. decreases; while in the *chronographic* transits the corresponding changes are insignificant.

(4.) In "*eye-and-ear*" transits, the personal discordances are liable to a considerable variation between the different observers; in *chronographic* transits, the differences between the observers are comparatively small. The general steadiness of observing by the latter method is very remarkable, and shows the great advantage obtained by its adoption.

(5.) The probable error of a Greenwich result for Right Ascension in the year 1853, as determined from "*eye-and-ear*" transits, is $\pm 0^{s}\cdot048$; while the corresponding probable error resulting from *chronographic* transits, in the year 1857, is $\pm 0\cdot034$. By arranging the separate results into groups, according to the stars' magnitude, it is found that no certain difference can be distinguished in the accuracy of the observed transits, excepting only that there is a tendency in both methods towards an increase in the probable error when transits of stars of the first magnitude are observed. The amount of increase in this instance, however, is in reality small, and is probably to some extent accidental. H. A. N.

5. *Shooting Stars in November*, 1864.—Arrangements were made at various places in the United States to watch for the shooting stars on the mornings of Nov. 13th and Nov. 14th. The sky was entirely overcast in most places, and nearly so in all. Through openings in the clouds, which would last a few minutes, occasional stars were seen, but whether there were more than usual it is impossible to determine. There was certainly nothing like the great display of 1833.

In San Francisco the sky was overcast. It is possible that some parts of California were favored with clear skies, and that we may yet obtain observations from them. Mr. Quetelet writes that a similar clouded sky prevented observation in Brussels. H. A. N.

6. *Supplementary note to the article on the Nebular Hypothesis.*—The form of Mars, as determined formerly, did not seem to agree with what we might expect from the primitive fluidity of the planet. The attention of astronomers having been recently called to the ellipticity of Mars, it seems now to be improbable that it has so large an ellipticity as has been heretofore received. (*This Journal*, [2], xxxviii, 436.) We thus see that this unconformable case is likely to yield to the requirements of the nebular hypothesis. We may venture to add that it is extremely improbable that Mars has an ellipticity differing much from that of the earth. D. T.

VI. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *National Academy of Sciences.*—The Report of the National Academy of Sciences, made by the Academy to the General Government, for the year 1863, has been published. It forms an octavo volume of 118 pages, with several maps. It contains only such Reports as had been made by committees of the Academy on the call of the General Government, with a brief statement of the operations of the Academy during the year 1863. The more important contents are a Report on the protection of bottoms of iron vessels from corrosion, etc.;—Report of the chairman of the Compass committee appointed to make experiments for the correction of local attraction in vessels built partly or wholly of iron; —Report of the committee appointed to examine the "Wind and Current Charts" and "Sailing Directions" issued from the Naval Observatory.

At the last meeting of the National Academy, held in August at New Haven, Ct., three vacancies in its list of members were filled by the election of Dr. J. C. Dalton, of New York, Leo Lesquereux, of Columbus, Ohio, and S. F. Baird, Assistant Secretary of the Smithsonian Institution.

2. *American Philosophical Society.*—The *Magellanic premium* of a gold medal has been awarded by the Philosophical Society to Mr. Pliny Earle Chase, for the discovery of numerical relations between gravity and magnetism.

3. *Medals of the Royal Society.*—At the Anniversary meeting of the Royal Society, on Wednesday, the last day of November, the Copley medal was awarded to Charles Darwin, Esq., F.R.S., "for his important researches in Geology, Zoology, and Botanical Physiology;" and the Rumford medal to J. Tyndall, F.R.S., for his researches on the Absorption and Radiation of Heat by Gases and Vapors.

4. H. R. SCHOOLCRAFT.—This distinguished traveller, and investigator of the manners, history, and language of the American Indian tribes, died at Washington on the 19th of December, in the seventy-second year of his age.

5. F. W. STRUVE, the celebrated astronomer of Pulkowa, Russia, died on the 23d of November last. His son, Otto Struve, was appointed his successor last year.

6. *Thoughts on the Influence of Ether in the Solar System, its relation to the Zodiacal Light, Comets, the Seasons, and periodical Shooting stars.* By ALEXANDER WILCOCKS, M.D. Extracted from the Transactions of the American Philosophical Society.—Dr. Wilcocks advances the hypothesis that there is a constant circulation of ether induced by the heat of the sun; that the cold ether descends near the poles of the sun, and that heated ether ascends from the sun's equator, or rather from a parallel of solar latitude just north of the equator; that this heated current as it rises is compressed by the cold ether into a thin conical sheet; that the ether bears with it matter capable of reflecting light, and thus causes the zodiacal light; that the tails of the comets are lighter than the ether, and arise for that reason from the sun, except when near the poles of the sun, when they are made to descend in the vortices of descending ether, so as to present double and multiple tails; that twice in the year, in August and November, the earth plunges through the sheet of warm

ether, causing a warm period in each month, the dog days and the Indian summer; and that in the middle of these two periods it causes the return of the August and November meteors.

Dr. Wilcocks has brought together and framed into a plausible hypothesis many facts, some of them hitherto not connected with any theory. But we do not believe that astronomers and physicists will be likely to accept his explanation. What he says relative to the seasons, and to the shooting stars seems peculiarly open to criticism. Why, for instance, the two periods of heat (supposing them to have an existence,) should not, according to his hypothesis, be equi-distant from the 5th of June and 5th of December, the two days when the earth is in the sun's equator, he does not explain. For the middle day of one period he names the 11th of August, which is 67 days from the 5th of June, while that of the other, which he calls the 12th of November, is only 24 days from the 5th of December. In what way the periodical star-showers could possibly be produced by such an ether we find it difficult to imagine. H. A. N.

7. *Report of the Superintendent of the Coast Survey, showing the progress of the Survey during the year* 1862. xx and 434 pp., 4to, with 49 diagrams and charts. Washington, 1864.—Besides the General Report on the Survey and its maps, there is the usual Appendix of Special Reports relating to various collateral physical observations. The subjects include—Tide tables for mariners; Cotidal lines for the Gulf of Mexico; Longitude of America from Europe, by Prof. Peirce; Lunar tables by the same; Longitudes in Maine, Alabama and Florida, by B. A. Gould; various series of Magnetic observations; Solar spots; Bessel's periodic functions; calculation of the depth of the Pacific from the earthquake wave of Simoda; Origin of the Florida Reef, by E. B. Hunt, etc.

8. *Eulogy on Joseph S. Hubbard;* by B. A. GOULD. 44 pp., 12mo. —This eulogy was written in compliance with the call of the National Academy of Sciences, which in its constitution provides for such a tribute in commemoration of each member that may be removed by death. Prof. Gould had in his friend a congenial and inspiring theme. High mathematical powers combined with astronomical tastes, an energetic, investigating mind, and a frank and genial disposition tempered by a true Christian spirit, had accomplished much for science. The tribute is admirable as a biographical notice, appreciative, eulogistic but not beyond justice, and of special value as a chapter in the history of American astronomy.

9. *Chambers's Encyclopedia.*—A new volume—the Sixth—of this popular Encyclopedia, a work well filled out and illustrated in the departments of science, as well as in other varied branches of knowledge, has just been issued by the American publishers, J. B. Lippincott & Co., of Philadelphia. The volume (826 pp.) carries the alphabet nearly through N.

10. *Abstracts of Meteorological Observations made at the Magnetical Observatory, Toronto, Canada West, during the years* 1854 *to* 1859, *inclusive.* 136 pp. 4to. Toronto, 1864. *Results of Meteorological Observations made at the Magnetical Observatory, Toronto, during the years* 1860, 1861 *and* 1862. 84 pp. 4to. Toronto, 1864.

11. *Review of American Birds;* by S. F. BAIRD.—The publication and issue of this work has already reached the 148th page.

12. *Monograph of the Bats of North America*; by H. Allen, M.D.—Smithsonian Miscellaneous Collections, No. 165. Washington. 86 pp., 8vo, with numerous wood-cuts.—The species included in this important monograph are those of North America north of Mexico.

13. *American Journal of Conchology.*—A prospectus has just been issued by G. W. Tryon, Jr., of Philadelphia (625 Market st.), proposing the publication of a Quarterly Journal of Conchology, and soliciting subcsriptions. Price ten dollars a year; single numbers three dollars. We commend it to all interested in the progress of science.

The Plurality of the Human Race; by G. Pouchet. Translated from the French by H. J. C. Beavan. London, 1864. Published for the Anthropological Society.

The Phenomena of Hybridity in the genus Homo, by Dr. Paul Broca. Published for the Anthropological Society.

Reptiles of British India; by Albert C. L. Gunther. Published by the Ray Society. 1864.

A History of the Spiders of Great Britain and Ireland; by John Blackwall, F.L.S. Published by the Ray Society. 1864.

Metamorphoses of Man and the lower animals (in French); by A. de Quatrefages. Paris, 1864.

Beschreibung und Eintheilung der Meteoriten; von Gustav Rose. 162 pp., 4to, with 4 plates. Berlin, 1864.

Meteorologische Waarnemingen, of the Netherland Meteorological Institute, for 1863. 326 pp., 4to. Utrecht, 1864.

Proceedings of the American Academy of Arts and Sciences, Vol. VI.—Page 97, On the Aluminates of Baryta; *W. P. Dexter.*—p. 106, Embryology of Asteracanthion berylinus, &c., with a plate; *A. Agassiz* [noticed at p. 130, vol. xxxviii, of this Jour.]—p. 114, On the velocity of light and the Sun's distance; *Lovering.*—p. 143, On the observed motions of the Companion of Sirius; *T. H. Safford.*—p. 166, Analysis of a Dacotah Meteorite; *C. T. Jackson.*—p. 169, On the new form of the Achromatic Object-glass introduced by Steinheil, with a plate; *Bond.*—p. 177, List of new Nebulæ seen at the Observatory of Harvard College; *Bond.*—p. 182, On Streptanthus and the plants referred to that Genus; *A. Gray.*—p. 188, Revision and arrangement (mainly by the fruit) of the North American species of Astragalus and Oxytropis; *A. Gray.*—p. 241, On the causes of Insanity among women; *Storer.*—p. 242, On the R. A. of the pole star as determined from observation; *T. H. Safford.*—p. 251, On a process of organic elementary analysis by combustion in a stream of oxygen gas; *C. M. Warren* [see last vol. of this Jour., p. 387].—p. 263, Observationes Lichenologicæ; *E. Tuckerman.*—p. 288, Investigation on the best approximate representation of all the mutual ratios of *k* quantities by those of simple integers; *Oliver.*

Proceedings of the Acad. Nat. Sci. Philadelphia. No. 4, Sept. and Oct. 1864.—Page 181, Limits and relations of the Raniformes; *E. D. Cope.*—p. 183, Description of a Gar-pike supposed to be new; *A. Winchell.*—p. 186, New genera and species of N. A. Myriapoda; *H. C. Wood, Jr.*—p. 187, Note on the Paralepidoids and Microstomatoids, and on some peculiarities of Arctic Ichthyology; *T. Gill.*—p. 189, Synopsis of the Cyclopteroids of eastern N. A.; *id.*—p. 194 and p. 214, Synopsis of the Pleuronectoids of California and Northwestern America; *id.*—p. 199, on the affinities of several doubtful fishes; *id.*—p. 208, Note on the family of Stichæoids; *id.*—p. 211, Notes on shells, with descriptions of new fossil species and genera; *T. A. Conrad.*—p. 224, On the characters of the higher groups of Reptilia squamata, and especially of the Diploglossa; *E. D. Cope.*—p. 231, On a blind Silurid, from Pennsylvania; *E. D. Cope.*—p. 234, Fasti Ornithologiæ; Obituary of P. L. S. Müller; *J. Cassin.*

Proceedings of the Essex Institute, Salem, Mass.—Vol. III, includes years 1860 to 1863. Vol. IV commences with the year 1864. No. 3 of Vol. IV, (July, August and September,) contains the conclusion of *J. A. Allen's* Catalogue of the Birds of Massachusetts, and a paper on the habits of some species of Humble Bees, by *F. W. Putnam.*

Journal of the Portland Society of Natural History, Vol. I, No. 1. Portland, Maine.—This number is occupied by E. S. Morse's paper on the Terrestrial Pulmonifera of Maine, noticed in the last volume of this Journal (p. 303).

THE

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JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XVI.—*On Terrestrial Magnetism, as a mode of Motion;* by PLINY EARLE CHASE, M.A., S.P.A.S.[1]

IN a note to a former communication, I expressed my belief that the British Astronomer Royal would find in the mechanical action of the sun's rays, the precise "occasional currents" for which he was seeking, as the probable cause of magnetic storms. Mr. Airy has recently sent me a copy of his very interesting paper (*Trans. Roy. Soc.*, 1863, Art. XXIX), and its perusal has greatly strengthened this belief.

All of my meteorological views rest upon the hypothesis, that the atmospheric changes, whether of humidity, temperature, pressure, electricity, or magnetism, are purely mechanical; and that, being controlled by the laws of motions, their proper explanation does not require the assumption of any peculiar magnetic or electric fluid, but that a single homogeneous, elastic, and all-pervading æther may be both the source and the receptacle of all the various forms of force. In its principal features, this theory harmonizes with the now generally accepted belief in the mechanical origin of light and heat, but in its details it involves some new and interesting special applications, which I have endeavored partially to develop.

It will be readily seen, by a reference to my communication of April 15 (*Proc. A. P. S.*, ix, 367, *et seq.*), that the *mechanical* action of the currents to whose *electric* action Ampère ascribed the origin of terrestrial magnetism, produces two opposite spirals in the air and æther,—the lower moving from the

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[1] From the Proceedings of the American Philosophical Society, Oct. 21, 1864.

poles to the equator, and against the earth's rotation; the upper from the equator to the poles, and in the same direction as the earth's rotation; the two being connected by innumerable currents of convection, or threads of ascending and descending particles. It will also be evident that at every place there are two principal sets of such double spirals, one with an axis perpendicular to the earth's radius vector, producing a maximum disturbance in the early afternoon, and the other more stable and uniform, with an axis passing through the nearest poles of greatest cold. In addition to the mutual perturbations of these two principal polarizing currents, the rolling of the luni-tidal attraction-wave produces at every instant a greater or less derangement,[2] and I find that the ratio of the lunar-barometric to the lunar-magnetic disturbance, (4·384), is nearly identical with Mr. Welsh's determination of the moment of magnetic inertia (4·4696; *Phil. Trans.*, cliii, 297). From a variety of considerations, it appears that the mechanical polarity or magnetic force thus engendered is a third proportional to two other forces, which may be called, respectively, central and tangential.

The communication which was presented at the meeting of Oct. 7, in its exhibition of the first numerical relationship that has even been pointed out between the barometric and magnetic fluctuations, showed that A : B : : B : M, a proportion in which A represents a central, B a tangential, and M a magnetic force.

I find a similar proportionality in each of Mr. Airy's summary tables (Op. cit., p. 627, *seq*). Thus, in his "Table II, Algebraic Sums of Magnetic Fluctuations (in terms of Horizontal force) for each Year, from 1841 to 1857, including all Days of Record of Great Magnetical disturbance," the Mean Disturbance is

Westerly force.	Northerly force.	Nadir force.
− ·00023=M.	− ·00146=T.	− ·00057=C.

Here the proportion T : C : : C : M gives for M a

Theoretical value, ... :	− ·000222
Observed "	− ·000228
Probable error, . . .	·000050

"Table III. Algebraic Sums of Magnetic Fluctuations (in terms of Horizontal Force) for each Year, from 1841 to 1857, including only those Days of Great Magnetic Disturbance, in which Records were made by the three Instruments."

Theoretical value of M, ...	− ·000287
Observed " "	− ·000257
Probable error, . . .	·000068

[2] Besides the great disturbing agencies, whose effects may perhaps be determinable by mathematical prediction, every transient local accumulation of heat or cold will exert an influence. Everythiag that can produce currents or eddies in the atmosphere, may also be presumed to affect the æther, and the inconceivable rapidity of the æthereal motions, as manifested in the velocity of the waves of light and heat, will account for the extreme sensitiveness of the magnetic needle.

Tables V and VI exhibit an approximation to the proportion, C : T : : T : M, but the approximation does not come within the limits of probable error. As no attention is paid in these two Tables to the positive and negative signs, we could not reasonably expect so satisfactory results as in Tables II and III.

"Table VIII. Sums, without regard of sign, of Coefficients of Magnetic Irregularity (in terms of Horizontal Force) for each Year, from 1841 to 1857, including all Days of Record of Great Magnetical Disturbance." The proportion C : T : : T : M, gives for M a

Theoretical value,	·001218
Observed "	·001203
Probable error,	·000066

"Table IX. Sums, without regard of sign, of Coefficients of Magnetic Irregularity (in terms of Horizontal Force) for each Year, from 1841 to 1857, including only those Days of Great Magnetic Disturbance, in which records were made by the three Instruments."

Theoretical value of M,	·001137
Observed " "	·001150
Probable error,	·000081

In addition to these numerical coincidences, the following points in Mr. Airy's paper appear to me to be specially noteworthy.

1. "The Aggregate for the Westerly Force . . . . (taken in comparison with that for the Northerly Force), appears to show that, on the whole, the direction for the Disturbing Force is 10° to the east of south;" p. 628. This indicates a line of mean disturbance about midway between the magnetic meridian (which at London, is about N. 24° W.), and the solar meridian, or midway between the meridians of decussation in the two sets of principal spirals, to which I have referred.

2. "Sometimes two waves in one direction correspond nearly with one in the other direction. . . . . A more frequent relation appears to be, that the evanescence of one wave corresponds with the maximum of the other;" p. 635.

3. "The most striking particulars in the last line (of Tables VIII and IX) are the following:

"First, the almost exact equality of the Mean Coefficients of Irregularity in the three elements. . . . With reference to their physical import, I think it likely that the equality of Coefficients of Irregularity may hereafter prove to be one of the most important of the facts of observation.[1]

"Second, the near agreement in the number of Irregularities for Westerly and for Northerly Force.

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[1] This approach to equality appears to be still more important, in view of the proportionality—C : T : : T : M—P. E. C.

"Third, the near agreement in the number of Irregularities for Nadir Force with half the number of Irregularities for Westerly or for Northerly Force;" pp. 641–2.

4. Tables X and XI (pp. 643–4) show that the disturbances are greatest in the winter months and in the night hours. Table X also appears to indicate minima of fluctuations and inequalities in months when there is the greatest uniformity of temperature, and maxima when the changes of temperature are greatest and most frequent.

5. Tables XI and XII furnish the materials for the following synopsis:

Forces.		Sums of Wave disturbance.		Sums of Irregularities.	Mean Wave disturbances.	Average departure from mean $\mp$	Mean Irregularity $\mp$	Frequency of Storms.
		+	−					
Westerly force.	Time of max.,	20 *h.*	10 *h.*	10 *h.*	20 *h.*	13 *h.*	15 *h.*	9 *h.*
	Time of min.,	10 *h.*	21 *h.*	23 *h.*	10 *h.*	2 *h.*	1 *h.*	23 *h.*
	Amt. of max.,	·1170	·2191	·1976	+·00142	·00104	·00162	126
	Amt. of min.,	·0165	·0083	·0346	−·00165	·00056	·00074	51
Northerly force.	Time of max.,	5 *h.*	12 *h.*	8 *h.*	5 *h.*	20 *h.*	15 *h.*	8 *h.*
	Time of min.,	22 *h.*	1 *h.*	23 *h.*	22 *h.*	1–2 *h.*	23 *h.*	23–1 *h.*
	Amt. of max.,	·1407	·2917	·1754	+·00038	·00168	·00144	136
	Amt. of min.,	·0038	·0674	·0441	−·00319	·00093	·00077	57
Nadir force.	Time of max.,	7 *h.*	14 *h.*	10 *h.*	0 *h.*	3 *h.*	0 *h.*	10 *h.*
	Time of min.,	22 *h.*	1 *h.*	23 *h.*	17 *h.*	22 *h.*	23 *h.*	1 *h.*
	Amt. of max.,	·3976	3133	·1241	+·00570	·00363	·00180	86
	Amt. of min.,	·0355	·0306	·0177	−·00380	·00157	·00074	19

"The Soli-tidal character of the principal characteristics of the occasional Magnetic Storms, as to frequency, magnitude, inequalities of wave disturbance, and Irregularities, is seen clearly in this Table." (Table XII) p. 645. There are subordinate maxima and minima, the consideration of which will become interesting, when the laws of the principals have been well ascertained and defined.

6. "In regard to the Wave-disturbance: for Westerly Force, the aggregate is + from 17^h to 6^h, − from 7^h to 16^h; for Northerly Force, the aggregate is + from 3^h to 5^h, − from 6^h to 2^h; and for Nadir Force, the aggregate is + from 23^h to 10^h, −from 11^h to 22^h;" p. 644.

7. Mr. Airy presents some conclusive considerations, "showing that the observed disturbances cannot be produced by the forces of any suddenly created galvanic current or polar magnet," and remarks as follows, respecting his theory: "Its fundamental idea is, that there may be in proximity to the earth something which (to avoid unnecessary words) I shall call a Magnetic Ether; that under circumstances generally, but not always, having reference to the solar hour, and therefore, probably, depending on the sun's radiation or on its suppression, a

current from N.N.W. to S.S.E., approximately, or from S.S.E. to N.N.W. (according to the boreal or austral nature of the ether), is formed in this Ether; that this current is liable to interruptions or perversions of the same kind as those which we are able to observe in currents of air and water; and that their effect is generally similar, producing eddies and whirls, of violence sometimes far exceeding that of the general current from which they are derived;" p. 646.

8. "And in the relation between E. and W. disturbances and vertical disturbances, there is a point which well deserves attention. When a water-funnel passed nearly over the observer, travelling (suppose) in a N. direction, he would first experience a strong current to the E., afterward a strong current to the W. (or *vice versa*), and between these there would be a very strong vertical pressure in one direction, not accompanied by one in the opposite direction; thus he would have half as many vertical as horizontal impulses. This state of things corresponds to the proportion which we have found throughout for the magnetic disturbances, and to the relation found in Article 18. I may also add that the rule at which we have arrived, that the waves of vertical force are few, but that their power, when they do occur, is very great, seems to correspond with what is reported of the whirlwinds of great atmospheric storms; which, violent and even frequent as they may be, occur very rarely at any assigned place;" p. 647.

I add a few considerations from Maj. Gen. Sabine's discussions. (*Phil. Trans.*, cliii, Art. XII.)

9. "The westerly deflections at Kew . . . have a decided double maximum, with an intervening interval of about eight or nine hours.

. . . The conical form and single maximum which characterizes the *easterly* deflections at Kew, belong also to the easterly deflections in all localities in North America, where the laws of the disturbances have been investigated. But . . . at Nertschinsk and Pekin . . . the conical form and single maximum characterize the *westerly* deflections, whilst the easterly have the double maximum. . . . At the two Asiatic stations, the aggregate values of the *westerly* deflections decidedly predominate, whilst in America the *easterly* deflections are no less decidedly predominant; and at Kew, . . . the amount of deflection in the two directions may be said to be balanced;" p. 282.

10. The differences of the weekly from the annual means of declination, indicate "with a very high degree of probability, an *annual variation*, whereby the north end of the magnet points more toward the east when the sun is north, and toward the west when the sun is south, of the equator;" p. 291.

11. The residual errors in the monthly determinations of the Horizontal Force and of the Dip "are thoroughly confirmatory of a semi-annual inequality, having its epochs coincident, or nearly so, with the sun's passage of the equator;" p. 303.

12. There appears to be "an increase of the Dip and of the Total Force, and a deflection of the north end of the Declination magnet toward the West, in both hemispheres, in the months of October to March, as compared with those from April to September. . . . The greater proximity of the earth to the sun in the December compared with the June Solstice most naturally presents itself as a not improbable cause; but we are as yet too little acquainted with the mode of the sun's action on the magnetism of the earth, to enter more deeply into the question at present;" p. 307.

I have neither the leisure nor the ability to undertake an exhaustive analysis of the results thus brought together; but I present them as well worthy of a profound mathematical investigation, as confirmatory in very striking and minute particulars of my mechanical hypotheses, and as furnishing new and strong presumptive evidence of that marvellous simplicity of force to which many independent branches of modern physical research so strongly point. This evidence is strengthened by the existence, as I have shown elsewhere, of the tidal law of sines in the solar-diurnal variation of the magnetic needle, by the magnetic effect of the daily barometric rotation tide, as exhibited in the convergence of lines of equal barometric disturbance toward the hours of high barometer and their divergence from the hours of low barometer, by many points of resemblance between the daily and yearly variation curves, both of temperature and of magnetism, and by certain considerations confirmatory of the reasonable presumption that there may be lunar monthly magnetic tides, somewhat analogous to those which I have pointed out in the barometer. (*Proceedings of the Roy. Soc.*, June 16, and of the *Am. Phil. Soc.*, June 17, 1864.)

Besides the differential or tidal action of the moon, there is a slight tendency to diminish the weight of the air that is nearest the moon, and to increase the weight of that which is most remote. In proportion as this tendency is exerted in conjunction with, or in opposition to, that of the sun, the mean solar-diurnal magnetic currents should be increased or diminished. Slight as the disturbing influence is, and modified as it must be by various causes, both occasional and periodic (*e. g.* the earth's rotation, the cyclical revolution and consequent varying latitude of the moon at the commencement of each new month, the oscillations in the aerial rotation-spheroid produced by lunar attraction, the changes in the average temperature of day and night at different seasons and different years, &c.), it may yet, perhaps, be discernible in comparing the results of a long series of careful and delicate observations. The accompanying tables are deduced from such a comparison of the St. Helena records.

TABLE I.

Solar and Lunar Daily Magnetic Tides, in parts of Force.[4]

Solar and Lunar Hours.	Horizontal force.		Vertical force.		Total force.	
	Solar.	Lunar.	Solar.	Lunar.	Solar.	Lunar.
	·00	·000	·000	·000	·000	·000
0	+1099	+006	−022	−005	+95	+005
1	+0911	−003	+229	+027	+82	−001
2	+0623	−011	+446	+031	+60	−005
3	+0368	−014	+593	+044	+40	−006
4	+0133	−020	+638	+072	+20	−007
5	−0080	−014	+608	+041	+01	−006
6	−0270	−007	+611	+050	−15	+001
7	−0394	−004	+545	+028	−26	+001
8	−0465	000	+300	−012	−36	−002
9	−0511	+022	+219	−011	−41	+018
10	−0530	+032	+074	−017	−45	+025
11	−0522	+031	−011	−037	−45	+022
12	−0481	+019	−100	−003	−43	+016
13	−0449	+017	−165	+005	−41	+015
14	−0405	+013	−224	+019	−38	+014
15	−0376	−009	−289	+048	−36	−001
16	−0352	−011	−345	+051	−35	−002
17	−0329	−008	−398	+029	−34	−003
18	−0298	−009	−465	+013	−32	−006
19	−0154	−001	−513	−011	−20	−005
20	+0130	+003	−582	−053	+03	−005
21	+0470	+006	−491	−050	+34	−002
22	+0803	+013	−427	−054	+63	+004
23	+1019	+009	−214	−067	+85	−001

TABLE II.

Lunar-Monthly Magnetic Tide of Horizontal Force.

Moon's Position.	Mean Daily Fluctuations of Horizontal Force at St. Helena.[5]				Moon's Position.	Mean Daily Fluctuations of Horizontal Force at St. Helena			
°	1844.	1845.	1846.	1844-6. Average.	°	1844.	1845.	1846.	1844-6. Average.
0	61·31	53·75	40·88	51·98	180	58·81	53·91	40·86	51·19
15	61·66	53·29	41·24	52·06	195	59·39	53·64	40·27	51·10
30	62·20	52·85	41·73	52·26	210	59·35	53·57	40·70	51·20
45	62·32	53·61	41·81	52·58	225	58·88	52·91	40·74	50·85
60	62·56	52·40	40·66	51·87	240	59·66	52·80	39·79	50·75
75	62·76	52·36	41·34	52·15	255	60·35	52·92	40·70	51·32
90	61·82	52·80	41·54	52·05	270	60·50	53·43	40·65	51·51
105	61·37	53·11	40·42	51·63	285	60·89	53·81	41·10	51·94
120	60·47	53·43	39·97	51·29	300	61·64	52·82	41·41	51·96
135	59·42	53·60	40·46	51·16	315	61·80	53·10	41·34	52·08
150	60·23	53·01	40·14	51·13	330	62·14	53·45	42·39	52·66
165	60·54	53·46	39·72	51·24	345	62·16	52·97	41·85	52·32

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[4] The first decimal figures are placed, for convenience, in an upper line.

[5] The value of one scale division is ·00019 of the horizontal force, in 1844 and 1845, and ·00021 in 1846.

Table I is compiled from Maj. Gen. Sabine's Tables 36, 37, 50, 51, 52, 53 (*St. Helena Observations*, ii). It is especially interesting as showing the influence of the opposition of attraction to rotation in producing low solar tides at 10 or 11 P. M., the prompt and direct influence of the sun upon the æthereal currents in the production of a high tide at noon, the double maxima and minima in each of the lunar tides, the additional confirmation of the analogies that I have heretofore pointed out between the spheroids of attraction and rotation, the opposition of the solar and the resemblance of the lunar zenith and nadir effects, and the evidence in the partial "establishment" of the moon's tides that most of her magnetic influence is exerted indirectly on the æther, through the intervention of atmospheric attraction-currents.

Tables II and III were formed by taking the mean of the hourly averages, on the twenty-four days in each lunar month which are most nearly indicated by the angular positions given in the first column. Each of the tabular numbers for 1844 and 1845 represents the average of two hundred and eighty-eight hourly observations; each of the numbers for 1846, the average of two hundred and sixty-four observations, with the few exceptions of holidays and other omitted days, for which the missing numbers were interpolated. Table II indicates a tendency to mean lunar influence between 90° and 105°, and between 270° and 285°, the influence increasing when the moon acts either in conjunction with the sun, or directly upon condensed air and *vice versâ*. It also shows the existence of disturbances, which may be accounted for by some of the causes to which I have already referred. Table III exhibits apparent tendencies to diminution of force near the syzygies, and to increase of force a day or two after the quadratures.

TABLE III.

Lunar-Monthly Magnetic Tide of Vertical Force.

Moon's Position.	Mean Daily Fluctuations of Vertical Force at St. Helena.*				Moon's Position.	Mean Daily Fluctuations of Vertical Force at St. Helena.			
°	1844.	1845.	1846.	1844-6 Average.	°	1844.	1845.	1846.	1844-6. Average.
0	48·42	48·51	43·56	46·83	180	47·06	47·77	46·54	47·12
15	48·21	48·55	43·90	46·89	195	47·96	48·02	46·29	47·42
30	47·33	48·53	44·55	46·80	210	48·14	48·26	45·88	47·43
45	47·33	48·25	43·96	46·51	225	47·49	48·26	46·39	47·38
60	47·45	48·47	43·69	46·54	240	48·40	48·60	45·32	47·44
75	47·62	47·88	44·22	46·57	255	48·16	48·52	44·64	47·11
90	47·65	47·43	44·77	46·62	270	48·00	48·08	44·54	46·87
105	47·62	46·92	45·31	46·62	285	47·93	47·70	44·61	46·75
120	47·53	47·42	45·65	46·87	300	48·06	48·26	44·91	47·08
135	47·76	47·40	47·30	47·49	315	48·17	48·56	44·95	47·23
150	47·55	47·50	47·23	47·43	330	48·49	47·91	43·78	46·73
165	47·52	47·70	47·02	47·41	345	48·18	47·44	43·99	46·54

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* The value of one scale division varies from ·00051 to ·00091 of the vertical force.

Table IV is a compendium of the tidal differences in the two preceding tables. It shows the effect of temperature in producing maxima and minima when the coolest and warmest portions of the earth are submitted to the direct action of the moon (at or near 240° and 45°), low temperature producing a minimum of horizontal force, with a maximum of vertical force, and *vice versâ.* From the variations of horizontal force $\left(\frac{\Delta X}{X}\right)$ and vertical force $\left(\frac{\Delta Y}{Y}\right)$ given in this table, table V is formed, the mean variations of total force $\left(\frac{\Delta\varphi}{\varphi}\right)$ being

TABLE IV.

Lunar-Monthly Magnetic Tide. Differences from Monthly Means.

Moon's Position.	Horizontal Force.			Vertical Force.			Means.	
°	1844.	1845.	1846.	1844.	1845.	1846.	H. F.	V. F.
0	+·38	+·54	−·02	+·59	+·51	−1·56	+·30	−·16
15	+·73	+·08	+·34	+38	+·55	−1·22	+·38	−·10
30	+1·27	−·36	+·83	−·50	+·53	−·57	+·58	−·19
45	+1·39	+·40	+·91	−·50	+·25	−1·16	+·90	−·48
60	+1 63	−·81	−·24	−·38	+·47	−1·43	+·19	−·45
75	+1·83	−·85	+·44	−·21	−·12	−·90	+·47	−·42
90	+·89	−·41	+·64	−·18	−·57	−·35	+37	−·37
105	+·44	−·10	−·48	−·21	−1·08	+·19	−·05	−·37
120	−·46	+·22	−·93	−·30	−·58	+·53	−·39	−·12
135	−1·51	+39	−·44	−·07	−·60	+2·18	−·52	+·50
150	−·70	−20	−·76	−·28	−·50	+2·11	−·55	+·44
165	−·39	+25	−1·18	−·31	−·30	+1·90	−44	+·42
180	−2·12	+·70	−·04	−·77	−·23	+1·42	−·49	+·13
195	−1·54	+·43	−·63	+·15	+·02	+1·17	−·58	+·43
210	−1·58	+·36	−·20	+·31	+·26	+·76	−·48	+·44
225	−2·05	−·30	−·16	−·34	+·26	+1 27	−·83	+·39
240	−1·27	−·41	−1·11	+57	+·60	+·20	−·93	+·45
255	−·58	−·29	−·20	+33	+·52	−·48	−·36	+·12
270	−·43	+·22	−·25	+·17	+·08	−·58	−·17	−·12
285	−·04	+·60	+·20	+·10	−·30	−·51	+·26	−24
300	+·71	−·39	+·51	+·23	+·26	−·21	+28	+09
315	+·87	−·11	+·44	+·34	+·56	−·17	+·40	+·24
330	+1 21	+·24	+1·49	+·66	−·09	−1·34	+98	−·26
345	+1·23	−·24	+·95	+·35	−·56	−1·13	+64	−·45

obtained by the formula $\frac{\Delta\varphi}{\varphi} = \cos^2\theta\frac{\Delta X}{X} + \sin^2\theta\frac{\Delta Y}{Y}$. I have taken $\theta = -22°$; one scale division of horizontal force $=·000194$; one division of vertical force $=·000792$; which are almost identical with the values employed by Gen. Sabine in the computation of his tables of hourly variation in solar and lunar total force.

In a similar manner I have computed Table VI, showing the average hourly variations, both in solar and lunar total force, in each of the three years which have furnished the data for most of my deductions. The first decimal figures are placed in an upper line, as in Table I. Perhaps the principal utility of this table may be found in some future extension of these investigations, but even now it is interesting, inasmuch as it exhibits the probable influence of periodic causes in shifting the hours of the daily maxima and minima, and as it lends added weight to the preceding tables, by showing that the monthly tide is more regular than the daily tide.

TABLE V.

Lunar-Monthly Magnetic Tide of Total Force.

Moon's Position.	Mean Daily Fluctuations at St. Helena, in parts of Total Force.			
°	1844.	1845.	1846.	1844-46. Average.
0	+·00013	+·00015	−·00018	+·00003
15	+·00017	+·00007	−·00008	+·00005
30	+·00016	·00000	+·00007	+·00008
45	+·00018	+·00009	+·00002	+·00010
60	+·00023	−·00008	−·00020	−·00002
75	+·00028	−·00015	−·00003	+·00003
90	+·00013	−·00012	+·00007	+·00002
105	+·00005	−·00014	−·00006	−·00005
120	−·00011	−·00003	−·00010	−·00008
135	−·00026	·00000	+·00017	−·00003
150	−·00015	−·00009	+·00011	−·00004
165	−·00010	+·00001	+·00001	−·00003
180	−·00044	+·00009	+·00015	−·00007
195	−·00024	+·00007	+·00003	−·00005
210	−·00023	+·00009	+·00005	−·00003
225	−·00038	−·00002	+·00012	−·00009
240	−·00015	·00000	−·00016	−·00010
255	−·00006	+·00001	−·00009	−·00005
270	−·00005	+·00005	−·00011	−·00004
285	·00000	+·00007	−·00002	+·00002
300	+·00014	−·00004	+·00006	+·00006
315	+·00018	+·00004	+·00005	+·00009
330	+·00027	+·00003	+·00010	+·00013
345	+·00024	−·00010	+·00003	+·00006

It seems not improbable that the mutual planetary perturbations which are sufficiently powerful to affect their orbital revolution, may also exert an appreciable influence on their æthereal spheroids, and that numerous cyclical magnetic variations may be thus produced. The disturbance of Jupiter is by far more important than that of any other planet, its mean attractive energy being nearly a third proportional to those of the sun and moon.[7] The annual fluctuations are very great, the intensity being about $\frac{1}{114}$ when Jupiter is nearest the earth, and less than half as great, or only about $\frac{1}{247}$, when most remote. The combined operation of the tropical revolutions of Jupiter, the moon's apsides, and the moon's nodes, should produce a series of disturbances corresponding very nearly in duration with General

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[7] If we take as our unit the moon's attraction for the earth $\frac{M}{D^2}$, the sun's will be about 177, and Jupiter's $\frac{1}{174}$.

Sabine's magnetic "decennial period," and Schwabe's period of solar spots.

The law of varying attraction suggests a plausible explanation for the approximate mean proportionality of the barometric to the tidal and magnetic variations. For the ratios of attraction of any planet when in solar conjunction, at quadrature, and in opposition, vary as $(n+1)^2$, n^2, and $(n-1)^2$, respectively, the attraction at the mean distance being nearly a mean proportional between the maximum and minimum attractions. The barometrical fluctuations are occasioned by variations in the gravitation of the air toward the earth's center,—the tidal motions, by the influence of distant heavenly bodies,—and the magnetic, according to my hypothesis, by the oscillations of the air and æther in their efforts to restore the unsettled equilibrium. The three disturbances, therefore, must evidently have nearly the same mutual relations as if they were produced by three forces, one centripetal, and the other two centrifugal, the two latter being nearly equal in amount but diametrically opposed in direction. This leads us at once, theoretically, to the general formula with which we started empirically,

$$A : B :: B : M,$$

and strengthens the conviction that there are none of the phe-

TABLE VI.

Solar and Lunar-Daily Tides of Total Force.

Solar and Lunar Hours.	1844.		1845.		1846.	
	Solar.	Lunar.	Solar.	Lunar.	Solar.	Lunar.
	·000	·000	·000	·000	·00	·000
0	+83	+009	+88	-009	+066	+004
1	+83	+011	+97	-014	+104	+015
2	+67	-005	+85	-016	+097	-004
3	+49	-004	+63	-008	+076	+018
4	+28	-024	+41	-012	+054	+017
5	+15	-032	+20	-001	+031	+024
6	+02	-027	+02	+002	+008	+028
7	-11	-016	-11	+010	-014	+022
8	-22	-006	-24	+003	-026	+022
9	-31	+001	-35	+034	-036	+022
10	-35	+008	-39	+041	-043	+024
11	-37	+029	-46	+031	-047	+021
12	-37	+017	-48	+021	-053	+008
13	-38	+021	-43	+012	-048	+011
14	-39	+045	-44	+012	-040	-001
15	-32	+021	-42	+006	-038	-029
16	-30	+001	-39	+006	-043	-033
17	-29	-004	-39	+006	-041	-027
18	-22	-012	-37	-008	-042	-020
19	-24	-006	-36	-016	-041	-019
20	-16	-014	-24	-008	-027	-011
21	+07	+017	+03	-006	-008	-021
22	+38	+022	+32	-002	+029	-008
23	+64	+016	+63	-008	+063	-010

nomena of terrestrial magnetism which cannot be explained, either by the instantaneously received and instantaneously transmitted impressions which are made directly upon the æther by attraction, heat, or rotation,—by the more sluggish oscillations of the air, which originate from the same sources,—or by the combination of the two.

Every particle is exposed to the influence of these several impressions, the tidal waves of the solid earth having a range, according to Prof. Thomson's calculations (*Phil. Trans.*, cliii, 574), at least two-fifths as great as if the globe were entirely fluid. There is, therefore, good reason to hope, that by the application of mechanical laws to the several phases of the æthereal undulations which produce the phenomena of light, heat, electricity, polarity, aggregation, and diffusion, we may obtain a clearer understanding, not only of all the meteorological changes, but also of seismic tremors, crystallization,[*] stratification, chemical action, and general morphology.

If density is a functional product of superficial magnetism and central attraction, the resemblance of the formula $B=\sqrt{AM}$, to the expression of Mr. Graham's experimental law of molecular diffusion, $\text{Time}=F\sqrt{\text{Density}}$ (*Proc. Roy. Soc.*, No. 56, p. 616-7), and to the general theoretical formula of which Mr. Graham's is a corollary,

$$\text{Velocity}=F\sqrt{\frac{\text{Elasticity}}{\text{Density}}}$$

may be something more than accidental.

If we assume the atmospheric density as our unit, $D'=1$, and represent the aerial and æthereal elasticities by E', E'', respectively, the proportion

$$\tfrac{1}{5} : 192{,}000 :: \sqrt{\frac{E'}{D'}} : \sqrt{\frac{E''}{D''}}$$

gives an approximate value for the density of the kinetic æther, $D''=\cdot00000000000108\,\frac{E'}{E''}$. The magnetic and barometric fluctuations may perhaps furnish the necessary data for determining the unknown ratio $\frac{E'}{E''}$.

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[*] The phosphorescence that is often observed during the process of crystallization, and the auroral displays in frosty air, are perhaps owing to analogous vibrations. Every chemical, as well as every physical, action produces a ponderable disturbance of equilibrium, which must give rise to æthereal oscillations, to which, in their simplest form, we give the name of electric, magnetic, or galvanic currents.

ART. XVI.—*On the construction of the Spectroscope;* by LEWIS M. RUTHERFURD.

I KNOW of no good substitute for bisulphid of carbon as the dispersive agent in the spectroscope. Flint glass, besides being expensive, when in large masses and of good quality, possesses but half the dispersive power, and the specimens of the denser glass which I have seen tarnish so rapidly, and have so high an index of refraction as to be practically useless. Having devoted much time to the construction and management of bisulphid of carbon prisms, it is quite possible that the results of my experience may be useful to those who may wish to fit up a spectroscope with such prisms, and perhaps I shall best attain the object by describing my own instrument.

The two principal telescopes are provided with objectives of 1·6 inches aperture and 19 inches focal length. The slit or collecting telescope has but one motion about a vertical axis at the side of the platform and just in front of the objective, enabling it to command all parts of the platform. The observing telescope has two motions, one about the central axis of the instrument, and the other about a second vertical axis, which by means of a slide, capable of being clamped, can be placed under the last surface of any prism on the platform; thus commanding by one motion the whole spectrum.

Before the slit is a prism for the comparison of different spectra, and the observing telescope is provided with eye-pieces of various powers. The first circuit consists of six prisms which are of brass faced with plates of glass, cemented with glue and molasses. These are each of about the angle of 60° and present an aperture of 2·9×1·8 inches. The faces to receive the glass are carefully ground to a flat surface and the glass quite thick and free from veins has been selected with reference to the flatness and parallelism of the sides.

Since however it is scarcely possible to find glass with parallel surfaces, care has been taken so to place the glass that the inclination of its faces is perpendicular to the axis of the prism. After grinding the prisms, the bases were so adjusted by filing that the refracting surfaces are rigidly perpendicular to the plane of the platform. This once done removes all necessity for foot screws, which complicate the prisms and add to the expense. The surface to be glazed was washed with an alkaline solution to remove all grease, and then in dilute nitric acid, finally washed in pure water and allowed to dry spontaneously. After being warmed, the prisms were so placed that the surface to be glazed was uppermost and in a horizontal position: the glass, having been cleaned, after the manner of a plate for photographic purposes, also warmed and both surfaces to be in contact dusted with a fine camel's hair brush, was placed in position upon the prism, a hot and fluid mixture of glue and molasses was then applied with a fine brush around the edges of the glass, whereupon a uniform and very thin film of the cement was introduced between the glass and the prism by capilarity. The prism was left untouched, until the cement had hardened so as to admit of its being removed and placed glass downward, for at least a day, when the next surface was treated in like manner. After the expiration of another day, I generally put on another coat of cement, much thicker in consistency than the first. In five days more the prism was ready to be filled through an orifice in the top, to which is fitted a ground stopper which is rendered perfectly tight by a little molasses. It may be thought that my description is needlessly particular, but I have mentioned nothing which experience has not shewn to be necessary to the permanence or performance of the prism.

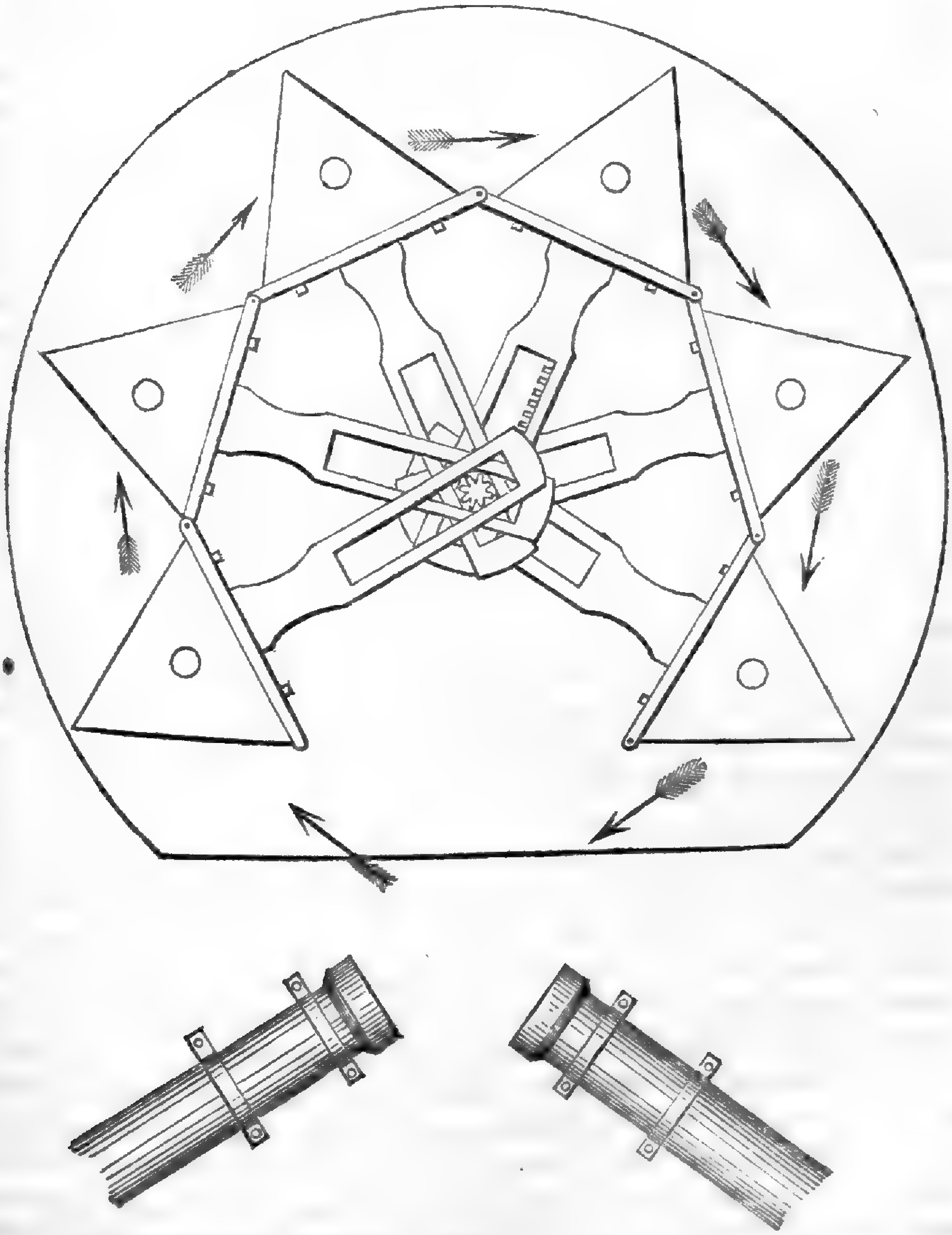
I soon discovered that, after I had made a good prism, its performance would be uncertain, and I finally traced the difficulty to a want of equal density in the bisulphid of carbon, and this peculiarity I have observed not only in the fluid of commerce, but quite as much so in that specially redistilled for the purpose. The fact of this unevenness of density is found in two ways. If a good prism which, with a high power, refuses to define the soda line, (a more stringent test than solar lines,) is violently shaken and then placed in position, it will for a few minutes define beautifully, but gradually settle into its former condition. By covering the aperture of the prism, except a small portion of the upper part, and bisecting the soda line with a spider's web in the eye-piece, all parts of the instrument being clamped, then covering all but the lower portion of the prism, it will be found that the soda line has been carried to a notable extent toward the violet end of the spectrum.

This want of homogeneity in the bisulphid of carbon is entirely different from the disturbance of density by thermal variations. It is a permanent feature of some specimens of the fluid, and is most observable when the prism has been longest at rest in equable temperature. I have one such prism, filled nearly two years since, which defines beautifully for a short time after being well shaken, but soon returns to a poor condition. The difference between the indices of refraction of the upper and lower strata is quite a measurable quantity. My mode of overcoming this obstacle is to filter several pound bottles of the bisulphid of carbon into a long glass jar, having a faucet at the bottom and a ground stopper at the top. After remaining undisturbed two days, the liquid arranges itself according to its density, and I fill the prisms from the faucet, being careful not to shake the jar.

Careful and repeated measures give for the index of refraction of the soda line with the prism first filled from the bottom, 1·62376, and with the ninth prism, filled with the fluid near the upper portion of the jar, 1·62137.

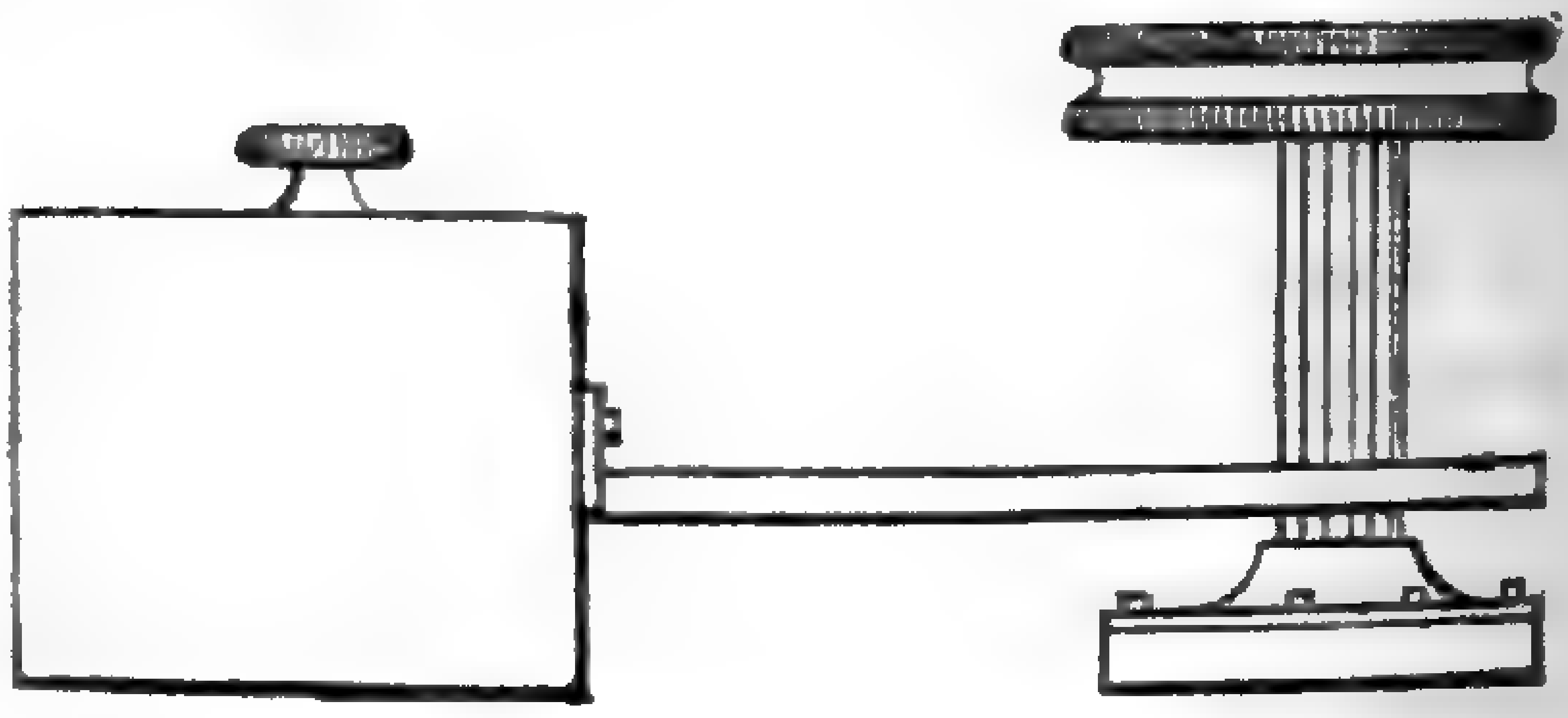
In order to obtain fine definition, it is necessary that the prisms should be placed at the angle of least deviation for the ray under observation. To make the adjustment with several prisms, or to change it when made, is so laborious and troublesome a task as almost to amount to a prohibition of the use of a powerful battery for practical and extended investigations. To remedy this evil I have devised and executed a mode by which I effect the adjustment of all the prisms by one motion of a milled head. An inspection of the accompanying Fig. 1, which represents the

1.



system of prisms without the button, as seen from above, will shew the manner in which this adjustment is accomplished. Upon the glass plate which forms the platform of the instrument, and in the center of the system, is cemented a brass plate, in a cavity of which revolves without shake a pinion provided at the top with a milled head, as seen in figure 2. The prisms are all hinged together at the corners: and from the back of each projects at right angles a brass bar provided with a slot, which, embracing the revolving standard accurately, retains each prism in such a position that, whether nearer or more distant from the center, its back is always perpendicular to the radius connecting it with the center of the standard. The slot of the third prism is provided with teeth which gear into the pinion, so that by turning the milled head this prism is forced to approach or depart from the center: but, from the construction, this cannot take place without imparting a similar motion to each of the other prisms, and thus, at will, their backs are made tangents to a larger or smaller circle, which is the adjustment sought.

2.



This mechanism is capable of adjusting six, or any smaller number, of equi-angled prisms. The outer spiral, when more than six are used, must be adjusted by hand.

New York, Dec. 10, 1864.

Art. XVII.—*Contributions from the Sheffield Laboratory of Yale College.* No. VIII.—*On crystallized Diopside as a furnace product;* by George J. Brush.

Some months since, Mr. Joseph C. Kent, Director of the Cooper Iron Works at Philipsburg, New Jersey, sent to me an exceedingly beautiful crystallized furnace product, which he had observed in one of his furnaces. It consisted of a group of colorless to grayish-white prismatic crystals, associated with a grayish vitreous mass of the same substance, intermingled with fragments of anthracite and traces of graphite and metallic iron. Many of the isolated crystals were transparent and colorless, while others were grayish white and transculent; some of the larger ones were over half an inch in length by one sixteenth of an inch in diameter. Mr. John M. Blake has determined them to have the form of rhombic prisms with brilliant reflecting surfaces. He obtained for the acute angle, in four measurements, with the reflecting goniometer, 86° 50′, 86° 52′, 87°, and 87° 12′. The prism was truncated at each of its obtuse angles by a plane of nearly the same width as the planes of the rhombic prism. The terminal planes were observed on only a few of the smaller crystals, and as they were microscopic were not measured. There is a tendency to cleave apparently in the direction of the rhombic prism, but no reflected image could be obtained from the more or less conchoidal surfaces thus produced.

The hardness of the crystals was about 5·5, and the specific gravity was found to be 3·16 (determined on 138 milligrams of substance). Lustre, vitreous and brilliant. Before the blowpipe in the forceps the substance fuses easily with intumescence to a colorless glass, giving at the same time an intense soda flame.

It is partially attacked by chlorhydric acid, emitting the odor of sulphuretted hydrogen; and entirely decomposed by fusion with carbonate of soda. Analyses made by Mr. Peter Collier, assistant in this Laboratory, gave the following results:

	I.	II.	III.	Mean.	Oxygen.	
Silica,	49·95	49·86		49·91	26·61	28·95
Alumina,	5·03	5·00		5·01	2·34	
Lime, - -	23·71	23·55		23·63	6·75	14·57
Magnesia, -	17·25	17·42		17·33	6·93	
Ferrous oxyd, -	0·39	0·41		0·40	·09	
Potash, -	....	....	1·42	1·42	·24	
Soda, - -	....	....	2·16	2·16	·56	
Calcium, -	0·30	0·33		0·31		
Sulphur, -	0·24	0.26		0·25		
Manganous oxyd,	tr.	tr.		tr.		
				100·42		

Analysis I and II were made by fusion with carbonate of soda, with addition of nitre to oxydize the sulphur to sulphuric acid. The alkalies (III) were determined by Smith's method. The small amount of sulphur was calculated as sulphid of calcium. The oxygen ratio of the mean shows the relation of the protoxyd bases to that of the silica and alumina, considering the latter to replace silica, to be 14·57 to 28·95 or 1 : 2, giving the formula $\dot{R}^3(\dddot{\text{Al}},\ddot{\text{Si}})^2$. This is the composition of aluminous diopside, and corresponds very nearly with that described by Hunt from Bathurst, in Canada.[1] The crystalline form, as determined by the observations and measurements of Mr. Blake, shows further its relation to pyroxene, and, taken with the chemical composition, leaves no doubt as to the identity of the crystals with the diopside variety of that mineral. Diopside has been previously observed as a furnace product by v. Kobell,[2] and Hausmann,[3] from iron furnaces at Jenbach, near Schwatz, in the Tyrol, and at Gammelbo in Sweden.

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[1] Rept. of Geol. Canada, 1863, p. 467. [2] Münchener gelehrte Anzeigen, xix, 97
[3] Liebig & Kopp, Jahresbericht. 1851, 767.

In regard to the occurrence and formation of this interesting product, Mr. Kent has kindly communicated to me the following facts: "I had one of our large furnaces (21 feet diameter and 55 feet high) in blast for over three years. At the end of the third year the fire brick under the flues commenced burning away, but, unwilling to put the furnace out when it was working well below, I kept on for some weeks, and then took off the blast without 'blowing out,'—as by 'blowing out' the top of the furnace would have been damaged. I then drew the stock out from below, and when the greater part of it had been withdrawn, threw water in to cool the interior and quench the fire. This, of course, left a solid mass in the hearth and exactly in the centre of the furnace, just above the tuyeres, say three and a half feet from the bottom, I found a mass of these crystals clustered within a space of about four cubic feet. I did not find them in any other part of the furnace, and had never before seen or found anything like them."

New Haven, Dec. 9, 1864.

ART. XVIII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology*; by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

(Continued from p. 58.)

§ 8. *The condition of the primitive Nebula.*

Whatever may be the distribution of matter in the nebula, and however the particles may move, the beautiful theorem—the magna charta of the nebular theory—obtains, i. e. *the nebula possesses the invariable plane of maximum areas*, or if ω represents the angular velocity, r the projection on the invariable plane of the radius vector drawn out from the center of gravity, m the mass, and A the projection of the area swept over by r in the unit of time, we have, C being a constant,

$$\Sigma m A = C \; ; \qquad . \quad . \quad . \quad . \quad (5)$$

or, as the *centrifugal force* γ of the particle m with respect to the principal axis is

$$\gamma = \omega^2 r \qquad . \quad . \quad . \quad . \quad . \quad (6)$$

while, at least for a very small unit of time,

$$A = \tfrac{1}{2}\omega\, r^2 , \qquad . \quad . \quad . \quad . \quad (7)$$

we have also

$$\Sigma\, m\, r^{\frac{3}{2}} \gamma^{\frac{1}{2}} = 2C . \qquad . \quad . \quad . \quad (8)$$

On account of the resistance of the ether, C will not be quite constant, but decrease in time; still it is apparent that, as a first approximation, we may neglect this resistance by considering C strictly constant.

Now, by the mutual attraction of the particles, the nebula is continually becoming more dense, or r is continually decreasing; hence, by (8), *the centrifugal force of any particle in the nebula is continually increasing.*

But the force of gravity at the surface is likewise constantly increasing; for we may without materially erring conceive the mass below the particle to remain constant, but then *gravity* is inversely as the square of the radius, or *rapidly increasing with the progress of condensation.*

But *these two forces determine the figure of the Nebula.* However irregular the figure may be at first, we see that the moulding forces, by constantly increasing, will at length shape the nebula accordingly. From Plateau's Experiments (see above, § 6, result 1, 2) we know this shape to be a flat ellipsoid. Laplace[1] has demonstrated that but *one single oblate ellipsoid of revolution* will be produced by these forces, *i. e.*

$$z^2 + m\,(x^2 + y^2) = a_1^{\,2}\,, \quad . \quad . \quad . \quad (9)$$

the plane x, y, coinciding with the *invariable plane*, being the equator, z the *axis of rotation* $= 2a_1$ and

$$m = \frac{1}{\sqrt{1+\lambda^2}}, \qquad \lambda = tg\,\theta, \qquad \sin\theta = e, \quad . \quad (10)$$

e being the *eccentricity* of the meridian; hence the equatorial semi-axis

$$a = a_1\sqrt{1+\lambda^2}. \quad . \quad . \quad . \quad (11)$$

Assuming, for a moment, the nebula to be homogeneous, we can determine the eccentricity by the *density* δ, and the *angular velocity* ω (i. e. by (6) proportional to the square root of the centrifugal force g at a unit of distance). Laplace found, if the mass of the whole nebula be M, and its moment of inertia E, that

$$\mathrm{E} = \tfrac{4}{15}\,\pi\,\delta\,a_1^{\,5}(1+\lambda^2)\sqrt{g} = \frac{\sqrt{g}}{5}\,a^2\mathrm{M}\,; \quad . \quad . \quad (12)$$

$$\mathrm{M} = \tfrac{4}{3}\,\pi\,\delta\,a_1^{\,3}(1+\lambda^2) = \tfrac{4}{3}\,\pi\,\delta\,\frac{a^3}{\sqrt{1+\lambda^2}}\,; \quad . \quad . \quad (13)$$

$$q' = \frac{25\,\mathrm{E}^2}{\mathrm{M}^{\frac{10}{3}}}\left(\tfrac{4}{3}\,\pi\,\delta\right)^{\frac{1}{3}}; \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$\text{arc}\,(tg = \lambda) = \frac{9\lambda + 2q'\,\lambda^3\,(1+\lambda^2)^{-\frac{2}{3}}}{9 + 3\lambda^2}\,; \quad . \quad . \quad (15)$$

which last equation he shows to have *but one* single positive real root, so that λ or e has but one value, if g, the centrifugal force, and δ, the density, are given.

But the density is certainly *not* constant throughout the whole

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[1] Mécanique Céleste, Liv. iii, chap. III, § 21.

nebula; but as this nebula is a gaseous body, δ will be determined by γ and gravity and, just as in the case of our earth, become uniform in the successive homothetic ellipsoidal shells included between any two successive surfaces. Hence we may consider δ as a function of the equatorial axis of such surface, and, as the density is increasing toward the center, we may, instead of the general law

$$\delta = f(a), \quad . \quad . \quad . \quad . \qquad (16)$$

take the law assumed by Laplace for the interior of our earth,

$$\delta = \Delta - c\,a. \quad . \quad . \quad . \qquad (17)$$

Plana has shown[2] that this law is the most probable in the case of the earth. Prof. Forchhammer of Copenhagen has lately shown[3] how this law accounts for one of the principal circumstances relating to the succession of geological strata.

It must finally be borne in mind that the nebula may have the various elements at the same place, because the laws of diffusion of gases will apply to the gaseous nebula. Thus far the chemical analyses of meteorites and the spectral analysis of the sun, moon and planets have corroborated this conclusion; still we must not hastily conclude that *some* differences may not obtain,

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[2] Note sur la densité moyenne de l'écorce superficielle de la terre. *Astronomische Nachrichten*, 1851, vol. xxv, No. 828. For the density at the center of the earth, he finds 16·3010.

[3] Inledning til en Række af Forelæsninger Stoffernes Kredslöb i Naturen (on the circulation of the elements in nature.) *Nordisk Universitets-Tidskrift*, viii, 1 hefte. Copenhagen, 1862. p. 68-81.

As an instance, *Forchhammer* describes the circulation of *Lime*: first the smaller diurnal orbits between plants and animals and the soil; next the greater annual circulation between land (washed by rain) and the sea; and, being here deposited in large strata through the agency of marine animals, these are again, after long geological periods, put into circulation, especially by the inorganic powers of nature, for these again to recommence a new cycle.—Now, where did the lime at first come from? Forchhammer thinks that as granitic rocks (specific gravity 2·7) are less dense than the dark trap-rocks, (on Bornholm, sp. gr. up to 2·93,) they would during the first period of the igneous earth, float upon the trap; thus the first solid shell would be formed of granitic rocks, i. e. *free from lime*—consequently unfit to support life, not fossiliferous. By the next revolutions this rock was dislocated and broken through by the underlying trap rocks containing lime and iron as silicates; the atmosphere being so rich in carbonic acid and being dissolved in the then hot waters would decompose these silicates, and thus bring lime into circulation. Organic life can now first commence—and the first fossiliferous rocks appear. This beautiful idea is further substantiated by the fact that volcanos, after a longer period of rest, commence their eruption with emitting trachytic, i. e. granite-like, masses almost free from lime—which are later succeeded by the *heavy* black lava, containing both lime and iron.

The richest deposit of gypsum, the Triassic period, is succeeded by the extraordinary limestone formation of the Jurassic period, thus giving another link in this chain of inductions, for gypsum, being more soluble, will more rapidly circulate, and thus occasion a greater deposit of limestone.

These views of our illustrious teacher show us what patient investigation yet may accomplish; we see the cause for the succession of granite and trap—see why organic life could not commence earlier than it does—see the cause for abundance of limestone during the Jurassic period, etc.—in the simple circumstance that granitic masses, being lighter than trap, were exterior to the latter in the igneous globe.

as the diffusion certainly is limited by the sinking of the denser particles. In a nebula from which a whole *cluster* of solar systems has been formed, we may therefore expect to find considerably different elements. We thus decline the imputation of Rutherfurd that homogeneity of original diffuse matter "is almost a logical necessity of the nebular hypothesis," and cannot see any real objection to this hypothesis, if, as he says, "we have now the strongest evidence that they (the stars) also differ in constituent materials" (this Journal, 1863, vol. xxxv, p. 77).

In regard to the signification of δ we must remark that, in the following, we use the letter δ to represent *the mean density of the nebula* from the centre to the distance r, while in (17) δ indicates the density of the shell at the very distance r. As (17) is only adduced to serve for a comparison, this course is legitimate. But it is easily demonstrated, that, at least for a spherical nebula, this law (17), if true for the individual shell, will also be true for the mean density of all shells inside of it. For, the actual density varying according to (17), the mean density of the interior body from $r=0$ to r is found to be

$$d=\Delta-c'.r, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where $c'=\frac{3}{4}c$. This law[4] is evidently the same as (17).

§ 9. *Attraction in the Nebula.*

As the nebula now may be considered made up of homothetic oblate ellipsoidal shells, individually of constant density, and as we know (from *Méc. Cél.*, liv. iii, chap. I, § 2,) that such a shell does not exert any attraction on a point within, we find *the attraction at any point in the nebula determined by the attraction of the ellipsoid whose surface passes through that point.*

This force, at the point x, y, z, is given by the following formulæ (from *Méc. Cél.*, liv. iii, ch. I, § 4), independent of the law of the density (17), and merely depending on the proved uniformity of the density in each separate shell.

If we put

$$Q=\frac{3m}{2\lambda^3}\left[\tan^{-1}\lambda-\frac{\lambda}{1+\lambda^2}\right], \quad . \quad . \quad . \quad . \quad . \quad (19)$$

then the components X, Y, Z, of the attraction (positive toward the origin) are

$$X=Q.\frac{x}{a_1^3}; \qquad Y=Q.\frac{y}{a_1^3}; \quad . \quad . \quad . \quad (20)$$

$$Z=\left(Q-\frac{\lambda^3}{1+\lambda^2}\right).\frac{z}{a_1^3}. \quad . \quad . \quad . \quad . \quad . \quad (21)$$

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[4] The "Density" in Trowbridge's article (this Journal, xxxviii, 354, 1864), is different, because referring to the density at different periods of time.

All of these three components act to *condense* the nebula; but X and Y also determine the revolution of the particles, while Z has no such influence, all motions in the direction of the axis of z mutually destroying each other, because x, y is the invariable plane. Composing X and Y we get

$$R = Q.\frac{r}{a_1^3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

and *directed toward the axis of rotation ;* $r^2 = x^2 + y^2$.

Substituting the first (13) ($m = M$) in (22) we obtain

$$\left.\begin{aligned} R &= \mu . \delta r, \\ \text{where} \qquad & \\ \mu &= \frac{2\pi}{\lambda^3}\left[(1+\lambda^2) \operatorname{arc}(tg=\lambda) - \lambda\right]. \end{aligned}\right\} \quad . \quad (23)$$

As now μ only depends on λ, i. e. on the eccentricity (10) which is constant, the shells being homothetic, we see that μ is at any given moment for all parts of the nebula the same, hence: *the radial force R in the nebula is proportional to the density δ and the distance r from the axis of rotation.*

This simple result is of very great importance, as we shall see in the sequel.

§ 10. *The orbit of the Planets.*

The particles of the nebula had originally motions in all directions; but as we assumed the existence of a momentum of rotation (§ 6), the principle of the invariable plane will keep up this momentum (§ 8), while all motions at variance therewith will in time mutually destroy themselves. Therefore, *all particles describe circles around the axis of rotation.*

Such as the orbit of the single particles that formed a planet will also be the orbit of the latter; hence the eccentricity and inclination of all planetary orbits ought to be zero. This may also be seen from Plateau's Experiment, and agrees well with the smallness of both the eccentricity and the inclination (see §1, 9° and 11°, § 3, 4° and 5°). Still, neither of these two quantities is actually zero. Are, then, these small deviations from this value accounted for by some accessory conditions of the problem?

We think so, for there are *two* modifying circumstances, the *rupture of the ring*—which it is beyond our power as yet to take into consideration—and the perturbating influence of already separated masses. The latter we may estimate. Representing the eccentricity by e, the inclination of the orbit to the ecliptic by i, to the invariable plane by I, we have from observation [*Humboldt's Cosmos*] :*

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* The numbers in the last column of the following table are not quite exact.—Eds. Jour. Sci.

	e.	*i.*	I.
Mercury, - - -	·2056	7° 0′	5° 19′
Venus, - - - -	·0068	3° 23′	1° 42′
Earth, - - -	·0168	0° 0′	1° 41′
Mars, - - - -	·0932	1° 51′	10′
Asteroids,[5] - - -	·160	7° 55′	6° 14′
Jupiter, - - - -	·0482	1° 19′	13′
Saturn, - - -	·0561	2° 30′	48′
Uranus, - - - -	·0466	0° 46′	55′
Neptune, - - -	·0087	1° 47′	6′
Invar. plane, - - -	....	1° 41′	0° 0′

We see how clearly the principal members of the system *move in one plane*, and that this plane is the *invariable plane* of the system; the great planets deviate less than one degree, the principal of the interior planets, Earth and Venus, only 1⅔ degrees—and even the inclination of the smallest planet, Mercury, amounts to but 5⅓ degrees! So also in relation to the eccentricity, this being less than one-twentieth for the principal bodies.

As to the deviations, we see that Neptune, which if not the most distant planet, certainly is (or *was*) separated from the next by a very large distance, so that if either could not at all, or but slightly, be disturbed, has indeed the smallest inclination (only 6 minutes!) and about the smallest eccentricity (less than one-hundredth!). Jupiter, which, on account of its enormous mass, could not be much disturbed by other bodies, has an inclination of only 13 minutes, while Saturn and Uranus have—corresponding to their smaller mass—about four times as considerable an inclination (48 and 55 minutes). The eccentricities of these three orbits are about equal; perhaps that of Jupiter is near its maximum, or the eccentricity of Saturn and Uranus near their minimum.

The inclination of the Earth and Venus is greater than that of the exterior planets, for the mass of the former is small as compared to that of the latter; but as Venus and the Earth are the great planets among the interior, we see that the inclination and eccentricity of Mercury's orbit are much more considerable than either, and that Mars has less inclination and eccentricity than Mercury. Is it because Jupiter, the only planet that would exert considerable perturbation on its development, was so far distant?

The orbit of the asteroids is explained in § 5, V.[6] We gave publicity to these views in an address delivered before the physical section, at the meeting of the Scandinavian philosophers, July, 1860.

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[5] Mean of *the first* 72 *Asteroids*, elements given in Table of Smithsonian Report, 1861, p. 218–219.

[6] We intended in this place to give a fuller account of our views concerning the development of the asteroids; but learning from a letter of Mr. Trowbridge that the continuation of his article will contain a solution of this problem, I abstain for the present from publishing my details.

The same principles will apply to the satellites; but we have too few data to make a comparison of this principle with observation profitable.

§ 11. *The periodic time of the Planets; Kepler's third law.*

Since every particle in the same shell revolves around the axis under the influence of a force R proportional to the distance r from the axis (§ 9), we know from mechanics that the periodic time T of such a particle is

$$T=\frac{2\pi}{\sqrt{\mu\delta}}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (24)$$

where, it will be remembered (23), μ is the same for the whole nebula, and δ constant for the same shell, so that *the time of revolution is the same for all particles of the same homothetic shell, but for the different shells inversely proportional to the square root of the density.* Thus every shell rotates as if it were solid; and if the whole nebula had the same density throughout it would rotate like one solid. But if the density be different in different parts, some shells will rotate faster than others (§ 12).

Eliminating δ by means of (13) we get

$$\left.\begin{aligned} T^2 &= \mu' . \frac{a^3}{M} \\ \mu' &= \frac{8}{3} \frac{\lambda^3}{\sqrt{1+\lambda^2}} \frac{1}{(1+\lambda^2)\tan^{-1}\lambda - \lambda} \end{aligned}\right\} \quad . \qquad (25)$$

We know that the ellipticity of the nebula is determined by the centrifugal force, and the latter by the state of condensation (§ 8); and even in case an ellipsoid becomes impossible, we can not but conclude that the figure continues to be determined in the same manner. But the condensation continues—the increase of the centrifugal force depending thereon will also continue and produce a series of rings in a certain succession, just as *one* ring was formed in the experiments of Plateau (§ 7). We see now how the continued increase of the condensation occasions a periodical change in the figure of the nebula. Granting the variation of the figure beyond the possible ellipsoid to be determined by the same circumstances as the ellipsoid itself, we may compare the corresponding stages of the nebula by referring to the same ellipticity e or the same μ' in (25); at any rate, we know that this can be done if we only compare the nebulæ, when within the limits of the possible ellipsoid. But then μ' will be the same for all rings, and as the mass of the planets is but very small as compared to that of the sun, M remains *almost* constant. Then (25) becomes

$$\frac{T^2}{a^3} = \text{constant}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (26)$$

or *the squares of the times of rotation of the different rings are as the cubes of their radii.*

If we remember that the possible ellipsoids reach to a proportion of 1 to about 3 between polar and equatorial diameters of the nebula, we can be sure that *this* covers the principal part of the metamorphosis; hence, (26) is rigorously proved for the greatest part of the condensation intervening between the formation of two successive rings; the nebula acquires its principal dimensions while changing in accordance with the ellipsoidic figure, and when abandoning this it quickly passes to the form of a slightly oblate spheroid and a ring. The interruption in our strictly mathematical demonstration cannot, therefore, seriously interfere with (26). But then this or *Kepler's third law is a consequence of the nebular hypothesis*, or *the observations embodied in this law sustain equally the nebular hypothesis and gravitation.*

Again, inductively, we may conclude from Kepler's third law that the interruption in our analytical deductions occasioned by our ignorance of the exact mechanical laws of the metamorphosis of the ellipsoid into the *globe ring* (we might in reference to Saturn find the expression *Kronion-form* convenient) is not of serious consequences.

Thus we may at least conclude from the third of Kepler's great laws that the development of the planets was periodical; for, this law being a fact, and (25) being rigorously true, we *must* have

$$\frac{\dot{\mu}'}{M} = \text{constant}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

but, as remarked before, M remains essentially constant, hence μ or what is the same λ, i. e. the ellipticity ϵ of the nebula corresponding to the different planets, must have been the same at corresponding epochs, just as we assumed above.

But *if* the metamorphosis of the nebula has been periodic, and not simultaneous, we must ascertain *whether the successive intervals of time were equal or not.* We shall find that *they were equal*, just as it would be the most natural or the simplest to assume.

§ 12. *Spiral Nebulæ.*

In the preceding paragraph we considered the density of the nebula sensibly equal throughout, so that the nebula always rotated like a solid, all particles having sensibly the same period of revolution. This might be done, because the dimensions of such nebula—however immense in reality—are not sufficiently great to produce a very large change in δ (17) in the space allotted to each planet.

But there may be bodies of dimensions so vast as to render it utterly impossible to consider the density approximatively uniform throughout the nebulous mass. Then the nebula will not rotate like a solid, but the angular velocity ω of any particle will be

$$\omega = \frac{2\pi}{T}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

or, by (24),

$$\omega = \sqrt{\mu\delta}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

As μ (23) is constant for the whole nebula, we see that *the angular velocity is proportional to the square root of the density*, or, according to (17), greatest near the center of the nebula.

If θ be the angle of position of those particles which are originally (i. e. when $t = 0$) in one and the same straight line, we have at the time t,

$$\theta = \omega t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

or by (29)

$$\theta^2 = \mu . \delta . t^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

Remembering that the density is a function of the distance (16) and also of the time on account of the progressing condensation, we see that (31) may be written,

$$\theta^2 = \mu t^2 f(a, t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

At any given moment of time (t constant), all the particles that originally were situated in the same straight line given by

$$\theta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

will now form the curve

$$\theta^2 = \varphi(a),$$

i. e. *a spiral.* This contains the fundamental principles of a mechanical theory of the *spiral nebulæ.*

Substituting Laplace's law of the density (17) in (31) or (32), we obtain as the equation of the spiral

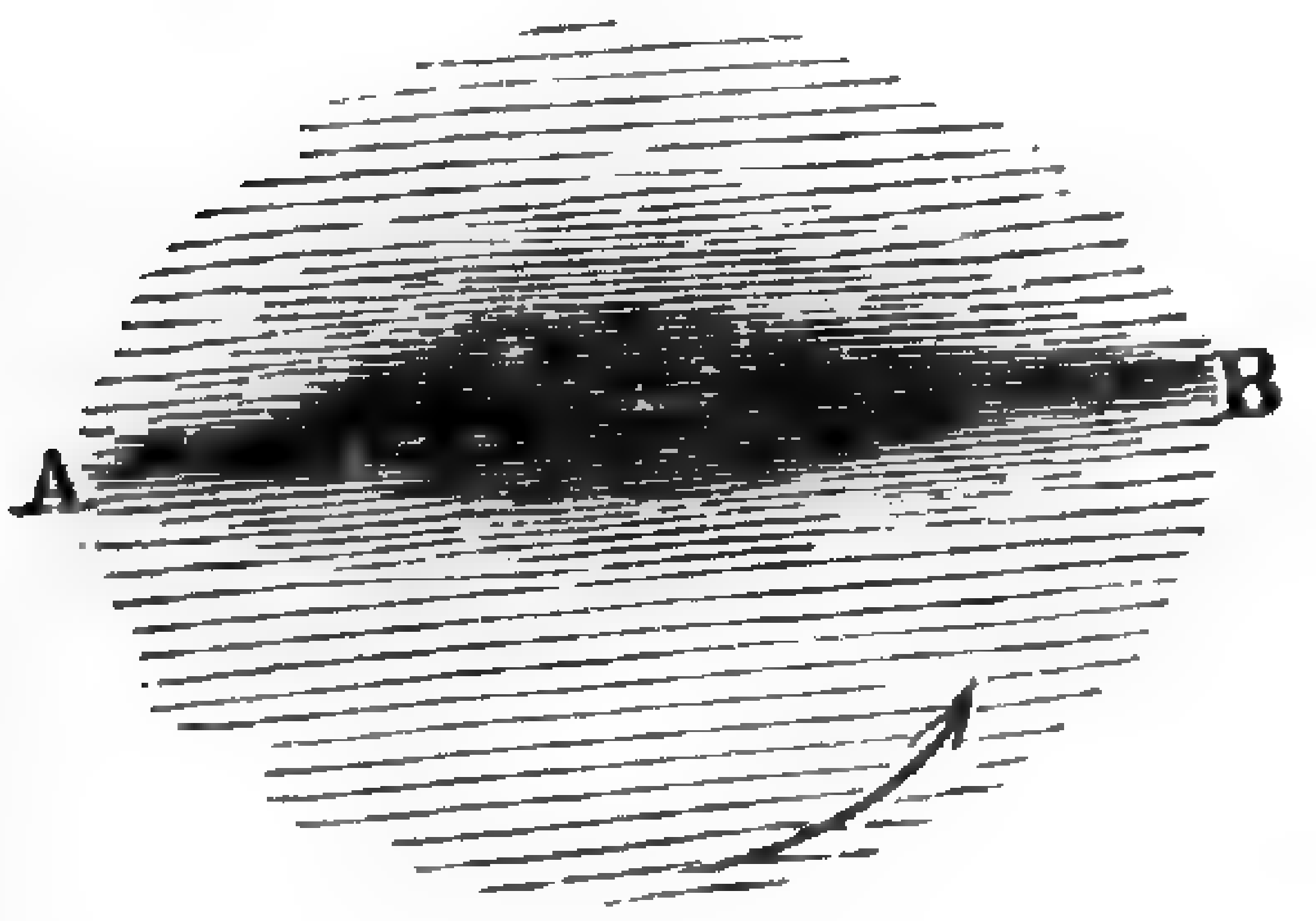
$$a = \frac{\alpha^2 - \theta^2}{C}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

wherein $\alpha = \mu . \Delta . t^2$ depends upon the ellipticity (μ), the *density* Δ *at the center*, and the time t, whilst $C = c\mu t^2$ depends upon the same μ and t and the *rate of variation of the density.* We see that these spires are limited, for $0 < \theta < \alpha$; and that the sweep α of the spire increases with the *age* of the nebula, the *density* at its center, and the *ellipticity.*

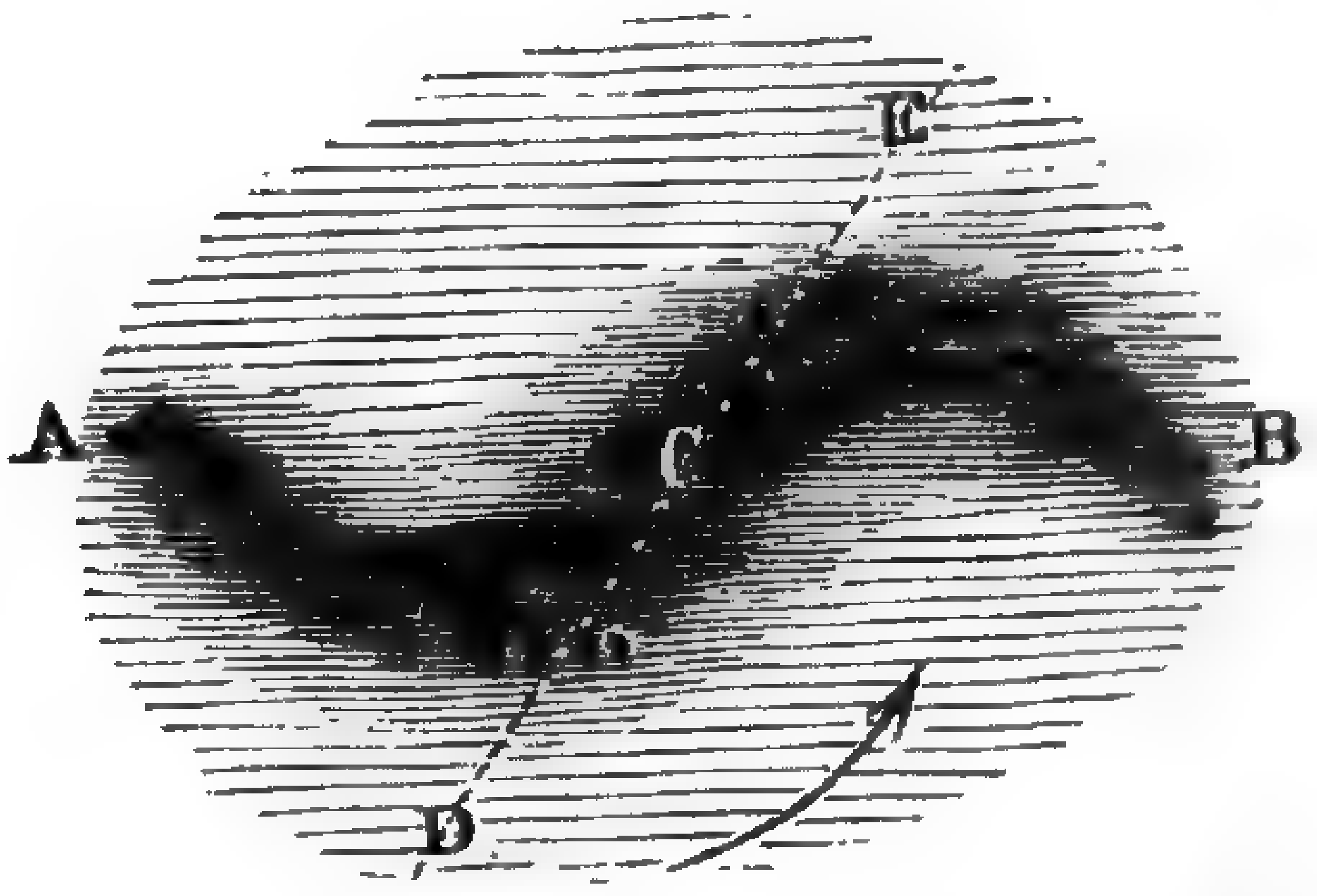
In order that such spiral structure may become apparent in a regular ellipsoidal nebula, the brightness must originally have been different in different meridians, though the density was constant in the same shell, i. e. the same in all meridians. Thus, if the brightness in the nebula was originally greatest in the opposite meridians AC and BC, (Fig. 1.) and it rotates in the direction of the arrow around the axis C, the *spiral nebula*, (fig. 2,) would result. As the age increases, the sweep, or the angle

BCE = α, would increase, whilst A and B remain nearly at the same distance from C: so that an annular nebula with a central core might in time result from a spiral nebula; even several concentric rings might be formed.

1.



2.



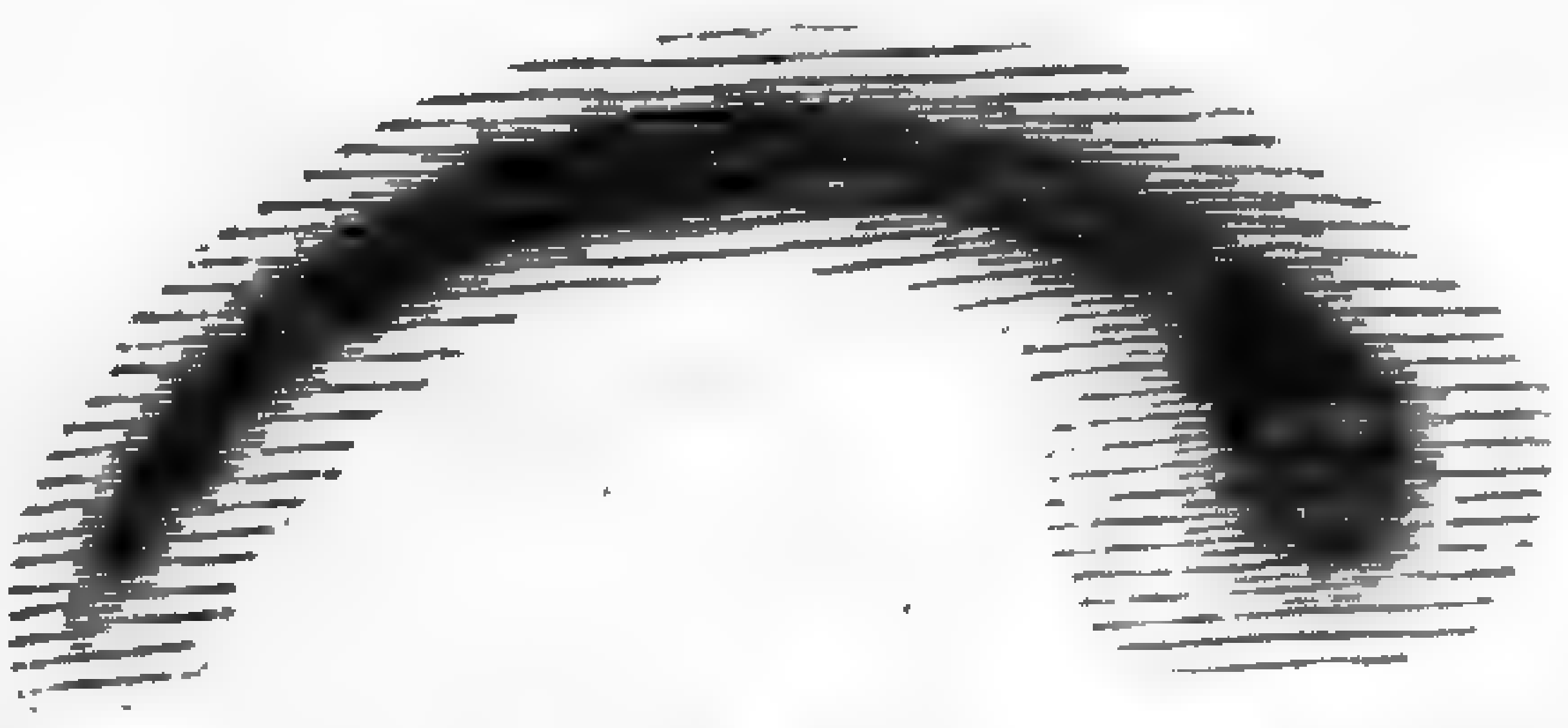
We cannot suppose any nebula to have different brightness in parts of the same density; and neither is it reasonable to assume such vast masses to be already shaped to a regular ellipsoid by the influence of the central forces (see 7).

It is much more reasonable to think that the nebulous masses at first were of *any shape*—such as might result from a predominating attraction of those portions where the heaviest elements were formed or collected in greater abundance. Then the formulæ deduced in the preceding paragraphs, though no longer representing the exact conditions of the nebula, still would continue to be approximate; the angular velocity would still be greatest near the central parts, as can also easily be shown directly, by considering the motion of each particle as subject to the attractions of all the others. Then the particles originally in a straight line would still in time form a spiral.

So we see that a nebula originally in the shape of a light rectilinear cloud with a condensation near the middle, like the part AB in fig. 1, would after some time exhibit a spiral like the dark part in fig. 2. The nebulæ, Herschel 1061, and H. 1337, as seen by Lord Rosse,[7] have exactly such a form. If, instead of having the nucleus in the middle, the original nebula had been denser near one extremity, like fig. 3, a simple spire like fig. 4 would

3.

4.



be the resulting spiral nebula, as we see it in H. 327, H. 1946,

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[7] Prof. G. P. Bond, Director of Harvard College Observatory, kindly sent me copies of a number of Rosse's latest figures of spiral nebulæ—for which important service I here repeat my sincere thanks.

etc. A nucleus with four branches of different density and magnitude would give a spiral nebula like the beautiful object, Messier 99. If each of the two arms in figure 1 had been subdivided into two branches, H. 2084 would result.

These few remarks must be sufficient at this place. We have already deduced forms as fanciful as H. 1196, H. 131, H. 1744, and others, from simple rectilinear forms, and we hope before long to discuss this theory more at length. We here merely intended to show that the forms revealed to us by the great telescope of Lord Rosse appear to be simple mechanical consequences of the nebular theory, if applied to very large nebulæ.

§ 13. *The Law of the Planetary Distances.*

The law of the planetary distances has not as yet been discovered, though it has been most diligently sought for as the principal element of the "Harmony of the Spheres." The endeavors of Plato were in vain, and Kepler at last ascended to the truth that the present distances are not exactly the original ones. Titius, and after him Bode, came near it [§ 2, (1)]; but the *deviations* from this law remained unaccounted for, thus not giving the conformation most essential to any law.

To find the true law of the planetary distances has been our aim for nearly ten years; we hope the sequel will prove that we at length have found the solution of this problem in the following law:

The mutual distances of the planets correspond to equal intervals of time.

That this is a *fact* we will demonstrate; but *why* these intervals were equal we are not yet able fully to see—still we know that this is the simplest way in which the periodicity in the development of the nebula as found in § 10 can obtain.

Deferring a thorough discussion of the earlier attempts, (some of which are almost contemporaneous with our own solution,) to some future opportunity, we will now give the *inductive reasoning* which leads to our law above stated.

There are a few well known laws in the evolution of the nebula which embody the solution of the problem. We know that the planetary masses are insignificant as compared with the solar mass; hence we see that the orbits of the planets simply mark the equatorial band of the condensing nebula at those definite periods when the radius of the nebula had diminished to the distance of the planet. Thus we see that *the planetary distances must be functions of time.*

Or, if it be more plain, we may say that the original nebula, in contracting, left at certain intervals a few particles behind to mark the limit of the nebula at those instants. But while condensing, the uttermost particle of the nebula describes a spiral curve; and if we can find the relation between the distance *a* of this particle and the time t, we need only to substitute the different intervals corresponding to the formation of the different planets in order to obtain their distances. But as the evolution is regularly periodical (§ 10), it is most probable that these intervals *are equal;* comparison with observation shows this to be the case.

But the motion of such a particle in so rare a nebula is regulated by the attraction of the whole nebula and the resistance of the ether. The first of these forces is inversely proportional to the square of the distance a, since the mass remains sensibly the same, and the particle is considered as on the equatorial *surface* of the nebula. In other words, the force of attraction on the particle is the same as the force of gravitation acting upon a planet. Resistance of the ether will necessarily follow the same law, whether a single particle or a planet be subject to it. But then our analysis[*] of the motion of a planet toward the sun is directly applicable to the motion of the superficial particle in its fall toward the center of the nebula. Formula (10) of that article shows the distance a (radius of the nebula) to be

$$a = \mathrm{A}.\varepsilon^{-2\nu t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

where A is the original distance (or radius of the nebula) and t the time of falling from A to a. Or, if a_t represent the distance of the planet that separated from the principal nebula at a time t *earlier* than the now nearest planet—i. e. the *age* of the planet as counted from Mercury,—the above (35) becomes

$$a_t = \beta.\gamma^t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

where β and γ are constants. But in the analysis leading to (35) the coëfficient ν of resistance

$$\nu = \frac{3}{8}\frac{\delta}{\Delta\varrho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

has been considered constant; here we cannot do so, for though the density δ of the ether and the radius ϱ of the particle may be considered constant, the density Δ of the particle varies very much, about inversely as the cube of the radius of the (homogeneous) nebula. If, therefore, ν' be the value of ν corresponding to the particle at the distance 30·0 of Neptune, ν'' the same at the distance 0·4 of Mercury, we have for a homogeneous nebula

$$\nu' : \nu'' = (\cdot 4)^3 : (30\cdot 0)^3 = 1 : 422000$$

nearly. If δ increases toward the center this proportion would be diminished; but still we see that ν decreases toward the interior. The formula (36) can therefore only express the principal part of the law; how (36) has to be amended in order to take

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[*] On the Density, Rotation and relative Age of the Planets. This Journal, 1864, [2], xxxvii, 36.

the variation of ν into account has to be separately investigated. Before we attempt this we will compare (36) with observation.

Erecting at equal distances (§ 10) ordinates proportional to the actual planetary distances, and interpolating by connecting these points by a curve, we see that this curve has the appearance of a *logarithmic curve* (like those given on the plate appended to our former article, this Journal, 1864, vol. xxxvii); and if drawn with sufficient care, we find that the constancy of the subnormal, characteristic of the logarithmic curve, holds good in the present instance—thus proving that (36) really is applicable to the planetary distances. But it is not the exact law, for the axis in the diagram is evidently too far below the curve, or the distances are too great by a constant α, so that the diagram of the planetary distances will be expressed not by (36) but by

$$a_t = \alpha + \beta . \gamma^t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

and it remains to be seen whether this additional constant α can be accounted for by the variation of ν (37). Before we investigate this, we will see how far (38) represents observation.

We see that it is almost the same as the law of Titius, but while in the latter t *is a mere index*, it is in (38) a variable, the great independent variable of mechanics, *time* or *age!* Besides, (38) deviates from Titius in the case of Mercury. Adapting the constants of Bode to (38), it becomes

$$a_t = 4 + (1·5) . 2^t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

Representing by a the actual distance, we have, for comparison with observation,

Planet.	age, t.	Distance calc. a_t.	Distance obs. a.	Difference.
Mercury, - - - -	0	55	38·7	+ 16·3
Venus, - - - -	1	70	92·3	− 2·3
Earth, - - - -	2	100	100·0	0·0
Mars, - - - -	3	160	152·4	+ 7·6
Asteroids ①–(72), - -	4	280	262·3[8]	+ 17·7
Jupiter, - - - -	5	520	520·3	− ·3
Saturn, - - - -	6	1000	953·9	+ 46·1
Uranus, - - - -	7	1960	1918·2	+ 41·8
Neptune, - - -	8	3880	3003·6	+ 876·4

We see that the *present distances* a agree with the *original* a_t for the principal planet of both groups, for the Earth and Jupiter. Mars, Saturn, and Uranus are about $\frac{1}{20}$th of their distance too near the sun, having approached the latter so much more on account of their mass being smaller. Mercury and Neptune have even approached still more, the former because of the smallness

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[8] Calculated from the table in *Smithsonian Report* for 1861, p. 218-219. We found the following interesting fact: mean distance of (1) to (31) = 2·599; of (31) to (56) = 2·679; of (57) to (72) = 2·752, showing that in general *the more distant members of the group of asteroids have been later discovered.*

of its mass, the latter on account of its high age (see this Journal, vol. xxxvii, p. 41). Before the precise influence of resistance was known, these deviations were considered sufficient cause to reject the law of Titius-Bode; but now these very deviations have become essential supports of the truth of that law.

Another and better test of our law (38), and of the constants of Bode (39), is obtained by directly solving (39) for the age t

$$t = \frac{\log(a_t - 40) - \log 15}{\log 2} \quad . \quad . \quad . \quad . \quad . \quad (40)$$

and seeing how far t is given by the series 0, 1, 2 ... We thus find

Planet.	Age.	too small.
Mercury, - - - - -	imag.	
Venus, - - - - - -	1·1066	−·1066
Earth, - - - - -	2·0000	·0000
Mars, - - - - - -	2·9056	+·094
Asteroids ①–(72), - - -	3·890	+·11
Jupiter, - - - - - -	5·0001	·000
Saturn, - - - - -	5·9264	+·073
Uranus, - - - - - -	6·9683	+·031
Neptune, - - - - -	7·6230	+·377

From this table we see that the age of the planets above that of Mercury is as the series of natural numbers, *the deviations not only being but small*, but just such as influence of the mass would make them. This may be easily proved by the formula contained in the article on the age of the planets before referred to.

If the present age of Mercury be m, then the age of the interior planets will be to that of the exterior ones as $m + \frac{6}{4}$ is to $m + \frac{26}{4}$, or as $2m + 3$ to $2m + 13$. We found this ratio as 1 to 3 (this Journal, vol. xxxvii, p. 43); if true it would follow that $m = 1$, or the *total age of any planet would be* $t + 1$, *the unit being the age of Mercury.*

After having seen that (38), the modified form of (36), is applicable to the planetary distances, we will demonstrate that this modification is consistent with the signification of t, the time.

If the resistance R be proportional to the velocity v, or

$$\mathrm{R} = \nu v \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

we have the tangential force (this Journal, vol. xxxvii, p. 40)

$$\frac{1}{r}\,\frac{d\left(r^2\frac{d\theta}{dt}\right)}{dt} = -\mathrm{R}\cos\eta = -\nu r\frac{d\theta}{dt}, \quad . \quad . \quad (42)$$

where r is the radius vector, and θ the anomaly; but Kepler's second law gives

$$r^2\frac{d\theta}{dt} = c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

so that (43) becomes

$$\frac{1}{r}\left[\frac{dc}{dt}+\nu c\right]=o, \quad \ldots \ldots \quad (44)$$

giving *for ν constant,*

$$c=C.e^{-\nu t}, \quad \ldots \ldots \quad (45)$$

or, since by Kepler's third law, $c^2=a\mu$,

$$a=\frac{C^2}{\mu}e^{-2\nu t}, \quad \ldots \ldots \quad (46)$$

all of which formulæ are demonstrated in our article referred to above.

Instead of solving the problem directly, we may indirectly try to find how ν must vary that (36) may become (38), i. e. to add a constant term to (46). In other words, C instead of being constant must be considered a function of t, i. e. (44) must be

$$\frac{1}{r}\left[\frac{dc}{dt}+\nu c\right]=\varphi(t), \quad \ldots \ldots \quad (47)$$

so that the resistance now becomes, see (42),

$$R=\nu v-\frac{\varphi(t)}{\cos\eta}=\nu v-\frac{ds}{d\theta}\cdot\frac{\varphi(t)}{r}, \quad \ldots \ldots \quad (48)$$

instead of (41), where $\cos\eta=\frac{rd\theta}{ds}$, and ds is the element of the orbit. The function $\varphi(t)$ can now, by the method of the variation of the arbitrary constants, be so determined that (46) or (36) coincides with (38). Since r is a function of t, we may make

$$f(t)=r\,\varphi(t), \quad \ldots \ldots \quad (49)$$

hence (47) becomes

$$\frac{dc}{dt}+\nu c=f(t). \quad \ldots \ldots \quad (50)$$

Taking the complete differential of (45), i. e. also considering C variable, substituting in (50) and reducing by (45), we obtain for the determination of C,

$$e^{-\nu t}\cdot\frac{dC}{dt}=f(t). \quad \ldots \ldots \quad (51)$$

This gives, by making K an arbitrary constant,

$$C=K+\int e^{\nu t}.f(t).dt, \quad \ldots \ldots \quad (52)$$

which, substituted in (46), gives,

$$a=\frac{e^{-2\nu t}}{\mu}\left[K+\int e^{\nu t}.f(t).dt\right]^2. \quad \ldots \quad (53)$$

This should be identical with (38), i. e. (remembering that t here is counted from the most distant, in (38) from the nearest planet and that γ in (36) is $e^{-2\nu}$ in (35))

$$a=\alpha+\beta.e^{-2\nu t}. \quad \ldots \ldots \quad (54)$$

Equating (53) and (54), and solving for $f(t)$, we find,

$$f(t) = \nu \frac{\alpha\sqrt{\mu}}{\sqrt{\alpha+\beta e^{-2\nu t}}}, \quad . \quad . \quad . \quad . \quad (55)$$

or, by (54),

$$f(t) = \nu\, a \sqrt{\frac{\mu}{a}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (56)$$

But Kepler's third law gives $\mu = a.v^2$ (this Journal, vol. xxxvii, p. 38, note); hence

$$f(t) = \nu\, a\, v, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

consequently, by (49), r and a being now the same again,

$$\varphi(t) = \nu\, a \frac{v}{a}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

or (48), cos η being almost equal to one, the orbit being nearly circular,

$$\mathrm{R} = \nu\, v\left(1 - \frac{v}{a}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

Thus we see that (36) becomes (38) if the resistance R, instead of being simply proportional to the velocity (41), is varying according to (59), which may be comprehended in (41) by taking the factor ν to decrease from $\nu\,(a=\infty)$ to $0\,(a=\alpha)$ according to

$$\nu' = \nu\left(1 - \frac{\alpha}{a}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (60)$$

This variation of the coefficient of resistance is conformable to (37), since Δ, according to (16) (then δ), increases as a decreases.

The law $a_t = \alpha + \beta.e^{-2\nu t} = \alpha + \beta.\gamma^t$

is, therefore, but an amplification of

$$a_t = \beta.e^{-2\nu t} = \beta.\gamma^t:$$

in the latter the coefficient of resistance *is constant*, in the former it *varies according to* (60). As now (60) is real, (54) or what is the same (38) is the real law of the planetary distances, t continuing to represent the age, and not, as in Bode's law, a mere index. *And as now finally* (38), *applied to the actual distances, gives values for t that are very nearly as the natural numbers, our law, announced above, holds true, that the planetary distances correspond to equal intervals of time;* or *the consecutive planets were abandoned at equal intervals of time.*

There remain yet two remarkable consequences to be drawn from this exponential law of the planetary distances. If in (38) *it is sufficiently great* (i. e. the corresponding planet far from the center) to make the first term insignificant as compared to the second, we have approximatively

$$a = \beta.\gamma^t,$$

hence

$$a^{t+1} = \beta.\gamma^{t+1},$$

consequently $$\frac{a_{t+1}}{a_t}=\gamma, \quad . \; . \; . \; . \; . \; . \; . \; . \quad (61)$$

or *for the most distant planets their distances approach to a simple geometrical series whose quotient is the base* γ. But this law will again in part be interfered with on account of the action of resistance on the completed system, which, on account of the higher age, has drawn the most distant planets comparatively nearer to the sun than the less distant ones, so as to *diminish* the above quotient γ.

Again, if t is sufficiently small—or the planet sufficiently near the center—the exponential series contained in (38) is highly convergent, so that perhaps the approximation may be sufficient if only the term of the first order is taken, so that (38) becomes, A and B representing constants,

$$a_t=\mathrm{A}+\mathrm{B}.t, \quad . \; . \; . \; . \; . \; . \; . \; . \quad (62)$$

hence $$a_{t+1}=\mathrm{A}+\mathrm{B}(t+1),$$
$$a_{t+2}=\mathrm{A}+\mathrm{B}.(t+2), \text{ etc.}$$

or, $$a_{t+1}-a_t=a_{t+2}-a_{t+1}=\mathrm{B}, \quad . \; . \quad (63)$$

i. e. *the innermost planets have a tendency to become equidistant.*

Both of these consequences are very plainly marked in the solar system, especially in the lunar, but also in the planetary orbs. For, as regards (61), we have for the distances of

Saturn	to	Jupiter	as	1·85 to 1.
Uranus	"	Saturn	"	2·01 " 1.
Neptune	"	Uranus	"	1·57 " 1.

For Saturn–Jupiter this proportion is still less than $\gamma=2$—also because Jupiter both on account of its age and mass has fallen less toward the sun than Saturn; but for Uranus–Saturn the ratio is almost equal to $\gamma=2$, while for Neptune–Uranus it is less again, on account of the higher age of the first.

The second circumstance, expressed in (63), seems to be exemplified in the orbits of Mercury, Venus, Earth, the three planets that are nearest to the sun, or for which t is the smallest. Their distances are

	Distance.	Difference.
Mars,	152·4	52·4
Earth,	100·0	26·7
Venus,	72·3	33·6
Mercury,	38·7	

We see how Mars, Earth and Venus follow Bode's law exactly, for one-half of 52·4 is 26·2, or very nearly 26·7—but the distance between Venus and Mercury is 33·6 instead of ½ of 26·7 or 13·4. This difference might be considered as a consequence of (63); but we know that it is principally due to the small mass of Mercury.

[To be concluded.]

ART. XIX.—*Periodic action of Water;* by LOUIS NICKERSON.

IN reading, some weeks ago, the article by Prof. Loomis, on the vibrations of water flowing over a dam, I was somewhat surprised at the idea of deriving the peculiar motion from a foreign source, as a column of air; surprised, because, however much the air might effect, by reaction, after the action had commenced, the perturbations of a liquid, in whatever state of motion it may exist, have always been so connected with periodic action as to have given use to the name of its most common attribute "the wave," as the characteristic title of nearly all periodic action. Without an attempt to discuss the question with the distinguished gentleman engaged, I shall endeavor to point out the manner in which the vibrations may be considered simply as the result of a wave peculiarly circumstanced.

I was sitting one day upon the bank of a large river in the West. Before me was a strong ripple, supposed by the people around to have been caused by the lodgment of snags upon the bottom. The sound from it was much louder than the roar of the stream—then in a state of freshet, and itself uproarious. But there was a cadence in it, an easily distinguished division into regular periods, which induced an inclination to pause and watch. My position was just upon the middle of an arc, which a late "caving in" of the bank had indented, each point of the arc running past the average bank of the river toward the center. The one down the river being in the greatest projection gave to the arc the appearance of a crescent, quartering into the bank and with its back down-stream-ward. Now a portion of the main current of the river, striking this lower horn, rounded inward as though to make a whirl within the crescent, but, thrown off, shot over toward the center and up stream. Above, another partial current, cut off from the main stream by a shoal or otherwise, came down along shore, and passing the upper horn bore directly down upon the face of the first eddy. Now they attempt to bear each other back, as though striving for the mastery of the crescent. For an instant there is an equilibrium. Both currents at the place of meeting rise rampantly into waves; both seem to receive reinforcements. They might be supposed to be equally matched, but the upper current receives the most water. For another instant they stand poised and opposed, and then the upper rushes, broken, but conquering, down over the surface of the other. Carrying off, however, not only its superabundance, but dragging along a little more water, so that the lower current quickly regains its ascendency, driving the upper back to be again checked and again overpowered, as before.

Afterward, I watched this place for hours at a time, unfortunately without timing, but yet with so distinct and definite a feeling of the regularity of the periods, that it was easy to estimate in the mind the exact period when, equilibrium having been attained, the lower water would start suddenly back and the accumulated waves from above rush over it, always dragging a sufficient extra quantity of water from the upper current to give for a time the ascendency to the lower. The space thus fought over was three or four feet.

But mill-dams are certainly not in just this situation. The permanent weir opposes no such active resistance as the elastic and moving weir just described. Its characteristic is passive resistance. Must we therefore look to it for continuous action? I take it to be thus:

1st. That a certain quantity of water arrives at the pool, and is all passed over the weir in the end, but in periods.

2d. That the quantity passed between these points, is, at the lesser points of velocity, of greater transverse section. This is obvious.

3d. That the decrement of velocity, and corresponding increment of section, is greater at a point nearly under the point of greatest depression of the curve of amplitude.

4th. That outside of this stream the pool is made up of water in a state of slow motion, at rest, or even in some cases of reaction, whirling or flowing backward.

Now we may examine the curve of hydraulic amplitude and its changes. For this we take the formulæ of permanent motion; which, though not exact, is sufficiently characteristic, as derived from Weisbach,

$$a-a'_1=\left(\frac{\sin\alpha-z\dfrac{\mathrm{P}}{ab}\cdot\dfrac{v^2}{2g}}{1-\dfrac{2}{a}\cdot\dfrac{v^2}{2g},}\right)l,$$

when

$\sin\alpha$ = slope of original stream
P = whetted perimeter,
al = transverse section,
v = velocity,
l = distance between a and a_1.
a = depth of dam or known point.
a_1 = required depth.
z = coefficient of resistance,
g = 32·2, or a gravitation.

The form of this curve is represented in the works of almost all hydraulic authors, and its equation shows it to be asymptotic to the original surface. It is easily seen above that when $\sin\alpha$ of the original surface becomes equal to $z\dfrac{\mathrm{P}}{ab}\cdot\dfrac{v^2}{2g}$ and therefore equal to $\sin\alpha'$ of a transverse section of the pool; $a-a_1=0$, or the pool is simply a continuation of the stream, and if $\frac{v^2}{2g}$ becomes equal to $\frac{a}{2}$, or the height due to velocity becomes equal to one-half the depth, both of the original stream, then $a-a_1=\infty$, a case which we shall examine more hereafter. We know that when the pool is first filled and the water is just on the point of flowing over the dam, the surface is horizontal, and we call it the hydrostatic amplitude. It is moreover true, that when the flowage commences and the hydraulic amplitude obtains, there is a stream of water passing through the pool, various in its velocities, and with a sheath of water, differently circumstanced, around it, which it in some way affects. We have also the admitted law, (Weisbach, vol. 1, art. 307; D'Aubinson, art. 54), that when any stream of liquid is in motion in any direction, its pressure in all other directions is equal to its hydrostatic pressure, less the pressure in the direction of its motion, and generally, that when a liquid in motion is made to pass through a liquid of less velocity, a part of the latter is dragged along by its greater lateral pressure, and passes off with the stream. In 1797 the engineer Venturi applied this principle successfully to the drainage of public lands. (Ewbank's Hydraulics, p. 478.)

In a pool fed at one point and yielding up the water at another, such as a mill-pond, this state of affairs practically obtains: that there is first a stream of water running some distance into the remou, and another passing out; in long dams only near the ends, perhaps, but in those of ordinary size throughout the whole length. This stream comes in contact with and passes through walls of partly quiescent water, not only that of the dead angles, but also of superimposition. For, says the engineer D'Aubinson, "moreover, the water of flowage seem only to be superimposed above the current, and not to participate wholly in its motion. The engineers who took the levels upon the Weser have observed, at a distance of 3884 feet from the dam, that the velocity of the surface was nearly insensible, whilst that at the bottom was quite strong."

We have now the fact that a stream is running through water much more nearly at rest, and that owing to the difference of pressure, some of the slower water must be dragged along in the course of the faster, in quantity and force varying as the difference of lateral pressure. We must remember that as in the end the weir can only pass over the same amount of water as it has received from the upper end, i. e., the water of the current, there must, then, be a periodic lull until the deficiency caused by this dragging action has been replaced.

It might readily appear that, as the velocity may become less from the interior of the stream to the outside, this might occur and yet a continuous action be kept up. But a little thought will show that as normally the stream passes out exactly the quantity received through its own continuation outside the pool, and as the water which I have shown to be dragged out is an extra quantity, there must be a pause after its exit until the surface, which has been lowered a very small increment by its departure, attains its own proper regimen.

You will notice that this reasoning requires that there should be a rise and fall of the surface of the curve of amplitude. I have seen no such rise myself, nor been able to obtain a confirmation of its occurrence from others. But these causes of its existence appear to me too plain to be disputed. It must by necessity be extremely small,—too small perhaps for observation. For the formula $h = \frac{v^2}{2g}$ when applied to a 4 foot velocity gives us $h = 0{\cdot}25$, and a velocity of 4·3 yields for h only $h = 0{\cdot}263$. By which we see that a rise which would only add a quantity $= 0{\cdot}013$ to the depth would increase the velocity full $\frac{3}{40}$. Then for the ordinate of the parabola of theoretic fall, we should have for the *vis viva* of the horizontal component a quantity varying as the square of velocity, or, for velocities varying from 4 ft. to 4·3 ft., a mechanical effect varying as 16 to 18·5 against the resistance of the atmosphere. That the atmosphere both outside of the falling water and the column inclosed between it and the dam, would assist by its elastic reaction in the vibrated distance, there can be no manner of doubt. But a cursory thought strikes me as I write, that were the action of the air truly isochronous with these vibrations, then the vibrations would have a constant tendency to increase. Partly it assists, partly deadens.

Again let us recur to our formula. Although made by Weisbach only to measure the curve of amplitude, and for the ordinary case of remou, it still contains the very elements which we need, and its changes may at least mark corresponding changes in the law which we discuss. In

$$a_1 - a_0 = \left(\frac{\sin \alpha - z \frac{P}{a\,b} \cdot \frac{v^2}{2g}}{1 - \frac{2}{a} \cdot \frac{v^2}{2g}}, \right) l$$

we see that $\sin \alpha$ is the measure of the slope of the original current, $z \frac{P}{ab} \cdot \frac{v^2}{2g}$ of the resistances of the whetted perimeter of the pool, whilst the denominator marks the changes which occur in the condition of the stream. To use it for our present purpose we must find the value of a_1 for some finite point on the axis of the stream, and then placing $l =$ to an infinitesimal distance from that point, the difference $a_1 - a_0$ should show the fluctuation of height due to a periodic change in the discharge. Now when $\sin a = z\frac{P}{ab}\cdot\frac{v^2}{2g}$, it is also by the law of the formula $= \sin\alpha'$ of that transverse section of which the second member shows the resistance; therefore

$$\sin\alpha = \sin\alpha',$$

therefore the velocity of the pool = the velocity of the stream, and the surface line of the pool is a parallel line with the bed; for $a_1 - a_0 = 0$, therefore it is circumstanced as in the original stream. There is no backwater, no difference of pressure and no vibration.

When $$\sin\alpha = 0$$ the surface becomes level, for there is no velocity, no flowage, therefore no resistance, $z\frac{P}{ab}\cdot\frac{v^2}{2g} = 0$; again $a_1 - a_0 = 0$; and there is no action, periodic or otherwise.

So we see that there are two points at which the vibrations cease: namely, when the water is sufficiently high to flow over the dam without much remou, as with a stream undammed, and with its surface a line nearly corresponding with the surface of the original stream; and again when the water is so low as to make the difference between the hydrostatic and hydraulic pressures very small. Of course these limits are much circumscribed by the inertia of a large body of water which has constantly a tendency to absorb and soften these vibrations. The most violent palpitation should then occur when

$$\sin\alpha = \left(z\frac{P}{ab}\cdot\frac{v^2}{2g}\right) m,$$

m being a new quantity to be found by a knowledge of the stream.

Again, if we put $\frac{2}{a}\cdot\frac{v^2}{2g} = 1$, or $\frac{v^2}{2g} = \frac{a}{2}$, or when the height due the velocity of the original stream becomes equal to one-half the depth of the same, we have

$$a_1 - a_0 = \infty.$$

Certainly nature admits no such differences as this. Yet, the fact beautifully follows from this, that when the height due the velocity of the original stream is equal to one half the depth of the same, the back water no longer retains the concave form, but, tending to rise infinitely, is checked by the action of gravity, falls backward upon the original stream, and tends to form a convex wave, with a nearly horizontal surface at a height above the bed of about one and one-half times the hydrostatic height. After this height becomes greater than half the depth, the wave is actually formed, and the water of amplitude flows to and rebounds from the foot of the stream. Bidone discovered this law, and Belanger has applied to it a formula.

The general formula which relates to this action we may gather from what proceeds. If h be the height of the remou, just before greatest action, $h-h_2 =$ height at the beginning of the lull that succeeds. The velocities are then

$$v = v_{,} - v_{,,}$$

And the times $= t = \sqrt{\frac{s_1 - s_2}{g}}$, s_1 and s_2 being spaces due to the two velocities combined with the time, t, of the vibration. Or the time of one vibration is equal to the time in which water falls through the height upon the dam, minus a space due to the velocity of different heights of remou, combined with the time observed. In other words, it is the time required for the quick water to draw a certain portion of nearly quiescent water into its own mean velocity—the quantity of quiescent water so drawn, and consequently the time, depending upon the regime of the remou. To this must be added the time of recuperation.

The action when $a-a_1 = \infty$ would be the formation of a remou, similar to the one described as seen by the writer of this in the first part of this paper, just at the point where the stream runs into the pool, which would cause one set of vibrations there, and perhaps another set similar to those on ordinary dams at the weir.

For a practical solution of this question, to prevent the vibrations, I am only prepared to recapitulate the foregoing examination into the nature of the action.

1st, We may consider that the vibrations become practically small when the inertia of the pool becomes sufficient to absorb them; therefore they are small when the quantity of water running over the dam is small in relation to the pool, and $\sin \alpha = 0$ or nearly.

2d, The same would be true if there were no backwater, or the stream retained its mean velocity unretarded, obtaining when the $\sin \alpha = \sin \alpha'$, as before, or when the surface becomes parallel with the bed; and again, should the pool be so filled as to make the bed become parallel with the surface.[1]

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[1] After the horizontal line (or line of hydrostatic amplitude) which bounds the surface of a pool just on the point of running over its dam is found, and the water begins to flow over, the longitudinal outline of the surface changes from its straight and horizontal form, and becomes a curve, which joins the water of flowage at some distance from the dam, and proceeding backward, after the law of the equation given in this paper, becomes asymptotic to the surface of the original stream. It is called the hydraulic amplitude. This curve marks by its changes every alteration or irregularity in the flowage, either of quantity or velocity or resistance, and having been carefully subjected to mathematical laws, answered as a touchstone to the deductions of this paper. And the law developed, not being dependent upon the form or nature of the weir, is a law for the periodic action of all fluids in a state of motion past an obstacle.

ART. XX.—*Remarks on the Carboniferous and Cretaceous Rocks of Eastern Kansas and Nebraska, and their relations to those of the adjacent States, and other localities farther eastward; in connection with a review of a paper recently published on this subject by M. Jules Marcou,*[1] *in the Bulletin of the Geological Society of France:* by F. B. MEEK.

IT is doubtless known to most of the readers of this Journal, that other explorers have long differed from Mr. Marcou, in regard to several important points in the geology of the Western States and Territories. During the autumn of 1863, it seems that he made an excursion to the West for the purpose of examining some of the localities in Nebraska respecting which he and others could not agree. While making these examinations, he was accompanied by Professor Capellini, an able Italian geologist from Bologna; but, as Mr. Marcou distinctly states that the views set forth in his paper are entirely his own, and that Prof. Capellini may have arrived at very different conclusions, it is due to the latter gentleman, that the geologists and amateur collectors familiar with western geology, who may read this review, should be informed that he is in no way responsible for any of the opinions expressed by Mr. Marcou.

Geological observations made by rail-road or steamboat travel being necessarily disconnected, Mr. Marcou's remarks, in the paper under review, so far as based upon personal examinations, relate to few isolated points along the shores of that part of the Missouri river ascended by him, and lying between St. Joseph, Missouri, and Sioux City, Iowa. By a glance at a map of the West, it will be seen that the distance between these two points, by an air line, is about two hundred miles; and that this part of the river forms the eastern boundary line of the new Territory of Nebraska, consisting of a portion of the southeast corner of a vast area formerly known by the general name of Nebraska Territory.

The first points at which Mr. Marcou seems to have touched, after taking steamboat at St. Joseph, were near Savannah, and in the vicinity of Iowa-point, where he saw outcrops of rock some sixty feet in thickness, consisting of bluish-gray clays, with intercalated layers of gray limestone, all showing a slight inclination of 3° or 4° in a W.N.W. direction. He says that he made no collections here, but that he saw *Productus* and *Terebratula*, showing it to be Carboniferous. (To this formation it had been referred by all others.)[2]

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[1] Une reconnoissance geologique du Nebraska; par M. Jules Marcou. Bulletin Geol. Soc. France; xxi, 132-147, January, 1864.

[2] It is worthy of note here that Dr. Owen collected at this locality species which he referred to *Nautilus tuberculatus*, *Productus Cora*, *Spirifer fasciger*, *Terebratula plano-sulcata*, and *Orthis Umbraculum*. (Report Iowa, Wiscon., and Minn., 135.) The

At another locality farther up the river, in the region of Nebraska City, he saw various exposures of rock, from which he constructed a section embracing some seventy feet of strata, consisting of reddish, gray, greenish and black clays, with a six inch seam of cannel-coal, and intercalated layers of light-colored dolomitic limestone, and toward the top a thin bed of sandstone containing numerous fragments of plants, some of which he thought like *Zamites* and *Walchia*;[3] the whole, according to him, showing a general inclination of only 5° to 7° to the N.N.W. These beds, he says, agree in color and other lithological characters with the Lower Trias of France, (*Dyas* of him,) to which horizon he refers them, and differ from the subjacent Carboniferous strata upon which they repose, according to him, unconformably, (sur lequel elles reposent en discordance de stratification). He does not, however, state that he anywhere saw the junction of these supposed two systems of strata, nor does he explain exactly the nature of the discordance of stratification. But we infer that he alludes to the fact of the Carboniferous beds showing at one place a slight inclination of 3° to 4° to the W.N.W.; and similarly slight inclination of the so-called "Dyassic" rocks, of 5° to 7° to the N.N.W., at another.

Geologists familiar with the numerous local undulations of strata in the west, where they often lie so nearly horizontal that their general inclination can only be distinguished from these local undulations by careful observations over considerable areas, will know how to appreciate such evidence as this. The improbability too (though within the range of possibilities) of a newer rock dipping at a higher angle than that upon which it reposes, and in a different direction, when the inferior rock is so nearly in its original horizontal position, will be apparent.

But from these rocks Mr. Marcou collected a number of fossils, which should clear up all doubts in regard to their age. These consist of the following forms, as identified by him, viz:—*Nautilus*, *Pleurotomaria*, *Murchisonia*, *Bellerophon*, *Panopæa*, *Edmondia*, *Avicula*, *Monotis*, *Ancella*, *Myalina*, *Bakevellia*, *Pecten*, *Lima*, *Orthis*, *Productus Prattenianus*, *Productus*, (undt.); *Chonetes mucronata*, *Spirifer* (*Martinia*) *Clannyanus*, *Spirifer* (undt.), *Terebratula*

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shell he always referred to *Spirifer fasciger*, is now well known to be *S. cameratus* Morton, and that which he called *Productus Cora*, is the *P. æquicostatus* Shumard, two of the most common and characteristic species of our Western Coal-measures.

The species of so-called *Terebratula*, mentioned by Mr. Marcou, is most probably *Athyris* (or *Spirigera*) *subtilita* Hall, as that shell is known to occur there, and at nearly all other places in the same rock, while Mr. Marcou habitually calls it *T. subtilita.*

[3] It should be remembered that these identifications are not given on the authority of Prof. Capellini, whose opinion on such a question would have been worthy of consideration. I am also gratified to see that, since this gentleman's return to Europe, he has published a work at Bologna, in which he says that he dissents from Mr. Marcou in regard to the age of the rocks at this locality, and thinks that the fossils he saw are more like Carboniferous forms.

near *T. subtilita*, some small Corals, or Bryozoa, an *Apiocrinus*, and a Crinoid near *Encrinus moniliformis.*

That Mr. Marcou may have collected or seen, at Nebraska City, specimens he honestly believes to belong to all the genera and species mentioned by him, and that he did find some of these forms there, the writer is fully prepared to understand from a careful personal study of the same beds and their characteristic fossils at numerous localities in Kansas and Nebraska, as well as from familiarity with collections from that and other localities in the immediate vicinity. For instance, he knows that *Chonetes mucronata* Meek and Hayden, *Productus Prattenianus* Norwood, *Spirigera subtilita* Hall, *Spirifer* (*Martinia*) *planoconvexus* Shumard, *Spirifer cameratus* Morton, *Myalina perattenuata*, *Pleurophorus occidentalis* and *Sedgwickia? concava* M. & H.,[4] together with species of *Aviculopecten*, and one of those forms belonging to the same group as the so-called *Monotis speluncaria* of authors, (genus *Eumicrotis* Meek,) occur there. Dr. Owen also found at this locality (called Fort Kearney in his Report, there being no town there at the time of his visit), in the highest bed of limestone, *Productus costatus*, *P. Flemingii*, and great numbers of *Fusulina cylindrica.* The shell referred by Mr. Marcou to *Spirifer Clannyanus* is beyond any reasonable doubt the same called *S. planoconvexus* by Dr. Shumard, since it is known to occur there and at numerous other localities in the same beds and far below; while it is scarcely distinguishable from *S. Clannyanus.* It is, however, quite as nearly allied to, if not really identical with, the well-known Carboniferous species *S. Urii.* Indeed, specimens of this shell sent to Mr. Davidson from the Coal-measures of Illinois (where it has received from Mr. McChesney the name *Ambocœlia gemmula*) were pronounced by him undistinguishable from British specimens of *S. Urii.* Good wood-cuts of the same shell, from Kansas, sent to Mr. Salter of London for comparison, within the past year, were also referred to *S. Urii.*

Now these species, that is, *Fusulina cylindrica*, *Chonetes mucronata*, *Productus Prattenianus*, *Productus costatus*, *P. Flemingii*, *Spirigera subtilita*, *Spirifer planoconvexus* (or *Urii*), and *Spirifer cameratus* are the most common and characteristic forms of the Coal-measures of Kansas and Nebraska, Northern Missouri and Western Iowa, in the very beds which, it will be seen, Mr. Marcou refers to the Subcarboniferous. They are also there found in the Coal-measures associated with species of *Nautilus*, *Bellerophon*, *Pleurotomaria*, *Murchisonia*, *Edmondia*, *Aviculopecten*, *Monotis*, (so-called), and, especially in Kansas, Nebraska, and northwestern Missouri, with a form so nearly like *Panopæa*

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[4] In describing the latter three species, the writer and Dr. H. thought the bed from which they were obtained might be Permian; but on afterward ascertaining that these shells are there and elsewhere associated with numerous well marked Coal-measure forms, they were satisfied that it does not belong to the Permian.

as to readily deceive more skillful paleontologists than Mr. Marcou professes to be.[*]

The name *Ancella*, in Mr. Marcou's list of Nebraska City fossils, is doubtless a mis-print of *Aucella*, there being no such name as *Ancella* known to the writer in Paleontology or recent Zoology. As some authors refer such forms as the so-called *Monotis speluncaria* to *Aucella*, it is probably one of these, which are common in the Coal-measures and Permian rocks of Kansas and Nebraska, to which he alludes. That he found here associated with all the Carboniferous fossils known to occur at this locality, the Jurassic genera *Aucella* (as illustrated and defined by Count Keyserling) and *Apiocrinites*, paleontologists may be pardoned for hesitating to admit.

In regard to *Encrinus*, the writer would remark that he has now before him from the Coal-measure outcrop at Bellevue, Nebraska, (referred to the Subcarboniferous by Mr. Marcou, as will be seen farther on,) the cup of a Crinoid, which in form and the arrangement of its plates, up to the summit of the first radials, as well as in the articulating surfaces for the reception of the succeeding range of pieces, seems to agree exactly with the corresponding parts of the genus *Encrinus*. None of the other parts, excepting as detached pieces, have ever been seen by the writer. The cup of the same or a closely allied species, beyond all doubt the same genus, occurs in the Coal-measures of Illinois; and separate plates of the same are not uncommon through nearly all the Coal-measures of Kansas and Nebraska. That a Crinoid with such a cup may be expected to be found nearly related to *Encrinus*, it is quite reasonable to suppose; but even if it should be found in all respects undistinguishable from that genus, would it therefore be philosophical to refer these beds at Nebraska City, and the Coal-measures of Illinois (placed by Mr. Marcou in the Subcarboniferous) with their great numbers of Carboniferous fossils, in the Permian, or the Dyas as he prefers to call it? If not, then all the arguments based upon the presence of this Crinoid, at Nebraska City, fall to the ground.[†]

Mr. Marcou lays great stress upon the fact that the Crinoids found by him at Nebraska City differ entirely from the numerous American Carboniferous forms hitherto made known. He perhaps forgets that nearly all our Carboniferous species, yet described or illustrated, have come from the Subcarboniferous deposits far below the Coal-measures. Some species, however, are known from our Coal-measures, and the fragments of a number of

[*] The type here alluded to is a widely gaping, edentulous, very thin shell, truncated behind, and, when well preserved, covered with minute granules. In short, it is the type of a new genus related to *Allorisma*, to which the writer has applied the name *Chænomya*, in a work now in the press. The typical species, *Allorisma ? Leavenworthensis* Meek and Hayden, occurs in the Coal-measures at Leavenworth City, Kansas, and others range much higher in the same series.

[†] See note at the end of this paper.

others have been seen, all of which differ widely from those known from the great Subcarboniferous limestones below the horizon of the Millstone grit.

The next locality examined by Mr. Marcou is at the village of Plattesmouth, some fifty miles farther up the Missouri by an air-line. Here he saw another exposure of rocks, some forty-five to fifty feet in thickness, composed of grayish and dark colored clays, in places streaked with red, together with a six-foot stratum of yellowish dolomitic limestone; all of which he says agree lithologically with the Lower Trias of France and Germany—Permian of authors, (= *Dyas* of him), to which horizon he refers them. As these beds, however, differ somewhat in color and composition from those seen at Nebraska City, he thinks they belong to another and lower division of the so-called Dyas, which, as its name implies, consists of two divisions in Europe, and consequently must be expcetde to present the same feature in this country.

Here too he collected a number of fossils, which, according to him, eminently represent a Lower New Red, or Dyassic, Fauna, though he at the same time admits that these fossils are very like Carboniferous types. But, to let the fossils speak for themselves, through Mr. Marcou, they were *Chonetes mucronata*, *Productus Calhounianus*, *Spirifer Clannyanus*, *Terebratula* [*Spirigera*] *subtilita*, *T. Mormonii*, *Spirifer* (undt.), *Fusulina cylindrica*, together with (from an upper bed) *Monotis*, *Avicula* and *Pecten*.

We may also add that Dr. Owen collected here from these beds specimens he referred to *Fusulina cylindrica*, *Productus semireticulatus*, *P. carbonarius*, *P. longispinus*, *Orthis Umbraculum*, *Spirifer fasciger* (?), *Chonetes semiovalis*, *Allorisma sulcata*, some Corals, and a small undetermined *Spirifer*. (Report Geol., Iowa, Wisc., and Minn., 133.)

Now *Chonetes mucronata* of Meek and Hayden was found in Kansas, ranging through a great thickness of Coal-measures, and the type specimens upon which this species was founded were collected at Fort Riley nearly, two hundred feet below the lowest well marked Permian beds. This was the highest position at which they met with this fossil, after careful examinations of hundreds of exposures; and at this place it was found directly associated with a shark tooth which Prof. Leidy at once recognized as the same he had described from the Coal-measures of the Alleghany Mountains, Pennsylvania, under the name *Petalodus Alleghaniensis*. (See Meek & Hayden's paper, *Proceed. Acad. Sci. Philad.*, Jan., 1859, p. 17.)

As already explained, the species referred by Mr. Marcou to *Spirifer Clannyanus* is the *S. plano-convexus* Shumard, which was originally described from this very locality. *Spirigera subtilita* all know to be a common characteristic Coal-measure species, from Western Pennsylvania to the Rocky Mountains, and from Nebraska and New Mexico. Mr. Marcou figures it himself, under the name *Terebratula subtilita*, in his *Geology of North America*, even as a Mountain Limestone species, from Utah and New Mexico. His so-called *Terebratula Mormonii*, is a *Retzia*, dedicated by him to the Latter Day Saints, from the fact that he first found it at their Capital City. It is worthy of note, however, that he figures and describes it as a Mountain Limestone species in the work just mentioned. So it would seem this little shell, in migrating eastward, obtained a long lease of life, since it here turns up, according to the same authority, in the so-called Dyas. The identity of the fossil is not questioned; indeed the writer and Dr. Hayden long since identified it, ranging through a great thickness of Coal-measures in Kansas, and northwestern Missouri. The notable point is, that it should be in Utah a Mountain Limestone species, and here at Plattesmouth, part of an eminently characteristic Lower Triassic Fauna!

The *Productus Calhounianus* of Swallow, mentioned by Mr. Marcou, is the same referred by Dr. Owen to *P. semireticulatus*, from which, if separable at all, it is certainly with difficulty distinguished. Whether distinct or not, however, it is known, as stated by Prof. Swallow in describing it, to range far down through all those very Coal-measures in Kansas and Missouri which Mr. Marcou refers to the Mountain Limestone. The geological position of *Fusulina cylindrica*, in this country has already been explained. *Productus longispinus*, *P. carbonaria* and *Orthis Umbraculum*, or at any rate species so referred by Dr. Owen, and sometimes by others, are common to the Coal-measures and the Subcarboniferous beds in the West. The species always called *Spirifer fasciger?* by Dr. Owen, as explained in another place, is *S. cameratus* Morton;—the very specimen of it figured by Owen was from the Plattesmouth locality. It is known to be everywhere characteristic of the Coal-measures, from New Mexico to Nebraska, and from Western Pennsylvania[7] to the Rocky Mountains.

The group of shells to which the name *Monotis* is often applied in this country and England, and by some continental writers, (though generically distinct from the Triassic *Monotis salinarius*, the type of *Monotis* Bronn,) is usually regarded in Europe as a Permian type. It is well known, however, to range through a great thickness of upper Coal-measures in Kansas and Northwestern Missouri, referred by Mr. Marcou to the Subcarboniferous. The names *Avicula* and *Pecten* are used so loosely by paleontologists, that they may be said, as generally understood, to range from the Silurian to our existing seas.

Now, how any geologist, having even a limited knowledge of American Carboniferous rocks and fossils, could regard a group

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[7] **See Prof. Rogers' Report Pa., ii, 833, fig. 694.**

of forms such as those mentioned above from Plattesmouth, as eminently a Lower Triassic (or Dyassic) fauna, seems inconceivable, excepting upon the supposition that he labors under some kind of a hard mental twist or bias on the subject of determining the age of rocks by their lithological characters.

After disposing of the so-called Lower Dyassic rocks at Plattesmouth, Mr. Marcou takes boat again, and ascends the Missouri some fifteen or more miles to Bellevue, north of the broad alluvial valley of Platte river. Here he saw a small exposure of rocks, some fifteen feet in height above the river, composed of whitish and yellowish limestone, and pale blue clays, altogether presenting different lithological characters from the outcrops seen below the Platte, and according to this favorite test of his, belonging to a very different epoch, or in other words to the Subcarboniferous.

Here he collected from the limestones, according to his identifications, the following fossils, viz:—*Productus Flemingii*, *P. semireticulatus*, *P. Cora*, *P. punctatus*, *P. scabriculus*, *P. pustulosus*, *P. pyxidiformis*, *Spirifer striatus*, var. *triplicatus* Hall, *S. Rocky-Montanus*, *S. lineatus*, *Terebratula subtilita*, *T. plano-sulcata*, *T. Royssii*, *T. Utah*, *Myalina*, *Nautilus*, and spines of *Archæocidaris*.

It may be as well to add just here, that Dr. Owen gives the following list of fossils collected by him at this locality, viz: *Fusulina cylindrica*, *Productus punctatus*, *P. Cora*, *P. costatus?*, *P. Flemingii*, *P. Humboldtii?*, *Spirifer fasciger?*, *Orthis Umbraculum*, *Terebratula plano-sulcata*, and a *Bellerophon* near *B. hiulcus*. (Rept. Iowa, Wisc., and Minn., p. 133).

From the same outcrop, the writer has now before him (collected by Dr. Hayden) *Productus costatus*, or a common form of the western Coal-measures generally referred to that species, *Productus Rogersii*, together with the Coal-measure form usually called *Productus punctatus*, *Spirigera subtilita*, *Rhynchonella Utah* (= *Terebratula* Marcou), *Terebratula bovidens* Morton, (= *T. millipunctata* Hall), *Spirifer Kentuckensis*, *S. cameratus*, an *Allorisma*, and the peculiar *Encrinus*-like Crinoid already mentioned.

As has been explained in another place, the shells from these rocks, referred by Owen and Mr. Marcou to *Productus semireticulatus* and *P. Cora*, are the *P. Calhounianus* Swallow, and *P. æquicostatus* Shumard; and, whether distinct or not from the species first named, they are very common in our Western Coal-measures. *P. punctatus* of their lists is *P. tubulospinus* of McChesney; which is scarcely distinguishable from the *punctatus*. At any rate, it is, as remarked by McChesney, very common in the Coal-measures "throughout the Western States." The same shell also occurs in the same position twenty-eight miles below Wheeling, in Ohio. *P. scabriculus* of Marcou's list is, beyond reasonable doubt, the widely distributed Coal-measure species, *P. Rog-*

ersii of Norwood and Pratten, as it is known to occur there, and is figured by Mr. Marcou under the name *P. scabriculus*, in his *N. Am. Geology*. *P. pustulosus* of his list, judging from the figure formerly given by him under that name, may be a variety of the *Rogersii*. *Spirifer striatus*, var. *triplicatus* Hall, (= *S. fasciger* of Owen's list), is *S. cameratus* Morton, everywhere common in the Coal-measures, and unknown from any lower position. *S. Rocky-Montanus* Marcou, is unknown to the writer from any locality east of the Black Hills. *S. lineatus* of Marcou's list is undoubtedly the same shell called *S. perplexa* by McChesney. (*Trans. Chicago Acad.*, 1). It is common in the upper Coal-measures of the West, being, as McChesney correctly states, found "in the Upper Coal-measures, in almost every part of the country where rocks of that age exist." It seems to differ from the Subcarboniferous species generally referred to *lineatus* mainly in being uniformly smaller. *Terebratula plano-sulcata* of Owen's and Marcou's lists is almost beyond doubt the *Athyris orbicularis* of McChesney; at any rate, that is the species figured by Mr. Marcou in his *North American Geology* under the name *Terebratula plano-sulcata* Phillips. It may or may not be identical with Phillips' species; but, as McChesney correctly states, it occurs in "the Coal-measures, particularly the upper portion, extensively distributed in the West." *Terebratula Royssii* is doubtless the *Spirigera* generally referred to that species in the West, but described by McChesney as *Athyris differentis*. Whether identical or not with the *S. Royssii* it is a common Upper Coal-measure form, as remarked by McChesney. *Terebratula Utah* of Marcou is a *Rhynchonella*, and, as long since shown by the writer and Dr. Hayden, is a common companion of the *Retzia Mormonii*, through all the Upper Coal-measures of Kansas and Northwestern Missouri. *Orthis Umbraculum*, more properly *Streptorhynchus Umbraculum*, or at least the shell figured under that name by Owen, is very common in the Coal-measures of Kansas, and ranges up to, if not into, the Permian.

Terebratula bovidens Morton (= *T. millepunctata* Hall) first described from the Coal-measures of Ohio, is a very common species in rocks of that age in the West, and unknown in any lower position. The same may also be said of *Spiriferina Kentuckensis* Shumard.

So we have here a group of fossils which any geologist, or mere amateur collector, acquainted with the forms characterizing our Carboniferous and Subcarboniferous rocks of the Mississippi valley, would at once, and without a moment's hesitation or doubt, refer to the Coal-measures. Some few of them are such as appear to be common to the Coal-measures and Subcarboniferous rocks, or are supposed to be, but all the others are wholly unknown below the horizon of the Millstone grit.

Again, it should be remembered that, of the foregoing lists, *Fusulina cylindrica*, the *Encrinus*-like Crinoid, *Spirigera subtilita*, *Productus semireticulatus*, (or *Calhounianus*), *Productus costatus*, *P. Flemingii*, (= *longispinus*), *Spirifer cameratus*, and *Orthis Umbraculum*, all likewise occur at the very localities, and in the very beds, referred by Mr. Marcou to the so-called Upper and Lower Dyas. It may also be added that, in the interior of Kansas, as well as at some places in Nebraska and Iowa, all the other Bellevue species occur in the very beds called Lower Dyas at Plattesmouth, and some of them, particularly in Kansas, in much higher positions.

In short, all the rocks seen by Mr. Marcou on the Missouri, from St. Joseph to the Cretaceous above Bellevue, belong to one unbroken series of Upper Coal-measures, as was first shown by Prof. Swallow; with possibly the exception of some of the highest out-crops near Nebraska City, where there is a downward undulation, that may have left portions of the Permian on the higher parts of the country. The few little isolated sections seen by him constitute but a mere fractional portion of this series; while his reference of these several outcrops to such widely different epochs, and his supposition that the beds he calls Mountain Limestone, form island-like masses, between those he refers to the Permian or so-called Dyas, were deposited unconformably, however honestly believed by him, may be all set down as purely imaginary. If he had gone out into the interior, where this series is much more extensively developed, and followed carefully up the smaller streams, he would have seen exactly the same beds he at one place calls Subcarboniferous, and at others Upper and Lower Dyas, with intermediate strata, all following each other in regular succession without the slightest physical or paleontological break. He might there also, by the same method of examining isolated sections, and applying the same lithological tests, and loose interpretation of fossil evidence, have found material enough to divide the so-called Dyas into twenty, or as many more, subdivisions as he pleased.

In 1859, the writer and Dr. Hayden, who were directly interested in the Permian discovery, and naturally desired, and confidently expected, to find somewhere a break between the Permian and Carboniferous rocks of that region, spent more than a month in traversing hundreds of miles of the districts in Kansas where these rocks are best developed. They did not hurry from point to point, but followed up the valleys of the streams on horseback, with a camping party, provided with a wagon and team for the transportation of supplies, specimens, &c. Yet, after carefully examining the various beds and seams, inch by inch, collecting all the fossils they could find, and carefully keeping separate those from the different strata and seams, they completely satisfied themselves that there is no where in the whole series any break indicating a marked change of physical conditions. They found, it is true, alternations of clays, limestones, shales, sandstones, &c., but all resting conformably one upon another, and inseparably linked together by their organic remains. Starting from Leavenworth City on the Missouri, where the same Coal-measure rocks which Mr. Marcou will insist belong, in Iowa, Missouri and Illinois, to the Mountain Limestone, occur characterized by such fossils as *Fusulina cylindrica*, *Spirifer cameratus*, *S. plano-convexus*, (= *Clannyanus*, of Mr. Marcou's lists), *Productus costatus*, *P. semireticulatus*, *P. punctatus*, *P. æquicostatus*, (or at any rate the forms usually so-called in the West), *P. Rogersii*, *Spirigera subtilita*, *Terebratula bovidens*, *Retzia Mormonii*, *Rhynchonella Utah*, &c., (but even here also containing a species belonging to the same genus, and near the so-called *Monotis speluncaria*),—they continued their researches through the succeeding strata to the Permian in the interior. Through a great thickness of these rocks, they found the Coal-measure fossils persistent and abundant, but occasionally associated with a Permian type.[8] The higher they ascended in the series, the greater was the proportion of Permian types observed, while the Carboniferous types gradually disappeared, until at last, above a certain horizon, and near the upper part of the series, only Permian forms were met with, with the exception of the ubiquitous *Spirigera subtilita*, or a shell that could not be distinguished from it. (See this Journal, [2], xxvii, 424, 1859; *Proceed. Acad. Nat. Sci. Philad.*, Jan. 1859, p. 8; *Dr. Newberry's Rept. Geol.*, *Ives' Colorado Exped.*, p. 112, &c.)

One of the most remarkable opinions, however, set forth in the paper under review, respecting these rocks is, that all the Coal-measures of Northern Missouri, and of Iowa and Illinois, belong, not as supposed by all others, to the horizon of the true Coal-measures of Europe, but to the Mountain Limestone series. He also thinks that possibly the Coal-measures of Indiana, Ohio and Michigan, and a part of those of Pennsylvania, Virginia and Kentucky, may belong to this lower horizon.

That beds of coal occur in the lower part of the Millstone grit in Pennsylvania, Western Virginia, portions of Kentucky, Tennessee and Arkansas, precisely as in England (see a paper by Messrs. Hull & Green, *Quart. Jour. Geol. Soc.*, Lond., Mar., 1864, p. 246)—while in the western part of Arkansas a great thickness of shale, sandstone, &c., above the conglomerate, is believed

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[8] A considerable thickness of these intermediate rocks were included by Prof Swallow in the Permian. The writer and Dr. Hayden, although satisfied that this intermediate series was not separable by any visible break from the Carboniferous below, or the Permian above, thought that it might be convenient to designate it as Permo-Carboniferous.

to be barren of coal—are well known facts. But to maintain that the Coal-measures of Northern Missouri, Iowa and Illinois belong to this or any lower horizon, to say nothing of those of the other States mentioned, beneath which the Millstone grit is so well developed, is to state a proposition the fallacy of which is manifest to all who have studied these rocks with even a moderate degree of care. In the first place, the whole physical structure of our Carboniferous system, and its relations to the Devonian below, and the Permian above, show at once that the Coal-measures of the Mississippi valley occupy precisely the same horizon as those of Europe. Here, as there, we have first, above the Devonian, the Subcarboniferous or Mountain Limestone group; then the Millstone grit, and above the latter the regular Coal-measures, which in Kansas, shade gradually upward into the Permian. The existence of the extensively developed regular Coal-measures above the Millstone grit in Pennsylvania, Western Virginia, Ohio, Kentucky, Tennessee, Indiana, and Southern Illinois, are facts so well known as scarcely to need mentioning here. It is true that the Millstone grit, which is found from four to five hundred feet in thickness beneath the Coal-measures of Southern Illinois,[*] thins out in a north-westerly direction, so that, farther north in Illinois, in Iowa and Missouri, the Coal-measures are found reposing directly upon the Subcarboniferous rocks. If we take this as an evidence, however, that the Coal-measures there belong to the Mountain Limestone series, we might upon the same principles argue that at other places in these states the coal-bearing strata belong to the Devonian, or even to the lower part of the Lower Silurian; for it is well known that, at some localities there, all the intermediate rocks are wanting, and the Coal-measures are found in direct contact with these older rocks.

If we had no stratigraphical evidence, however, in regard to the parallelism of our Coal-measures with those of Europe, their flora alone would be sufficient to settle this question. Lesquereux, who was especially commissioned to study the fossil plants of the Coal-measures in connection with the State surveys of most of the Western States, containing Upper Carboniferous rocks, including Illinois, says, "if we admit the generic distribution of the fossil plants of the coal, as it has been established by Brongniart in his *Tableau des Genres* (certainly the best that has been attempted, either before or after him), all the European genera, even the undefined genus *Aphlebia* Sternb., have repre-

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[*] Mr. H. Engelmann, an assistant in the Illinois State Geographical Survey, gives its thickness in the southern part of that State at five hundred feet, and gives a section showing it to rest upon the upper Archimedes Limestone group, everywhere regarded as the upper member of the Subcarboniferous or Mountain Limestone series. (*Trans. Acad. Sci., St. Louis*, Nov. 1862, p. 188.)

sentative species in the Coal-fields of America."[10] He also shows by tables in the same article, that out of a list of about 350 known species of our Coal-measure plants, 150, or approaching one-half, are identical with European Coal-measure species. Dr. J. S. Newberry, an equally good authority in this department of paleontology, had previously arrived at very nearly the same conclusion, from a careful study of the Carboniferous flora of Ohio and some of the neighboring states.[11]

In regard to the identity of the Coal-measures of Illinois, with the regular Coal-measures (overlying the Millstone grit) of Ohio, Pennsylvania, Kentucky, and other neighboring states, it is only necessary to again quote Lesquereux, who has given especial attention to tracing out the parallelism of the subordinate beds at distantly separated localities in the coal fields of the Middle and Western States. On this point he says, (this Jour., [2], xxx, 367,) "Such is, nevertheless, the uniformity of the distribution of the strata of our coal-basins, that a section made in Western Illinois or Western Kentucky, or in any part of the coal-fields in these States, will prove comparatively similar (that is with some differences in the thickness of the strata,) to any section made in the coal-fields of Pennsylvania or Ohio." Now

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[10] This Journal, [2], xxx, 65.

[11] The fauna of our Coal-measures has not been compared with that of the equivalent rocks of Europe, in the same detail as its flora. Indeed, judging from the publications on the Mollusks, and other invertebrate remains of the European Coal-measures, our rocks of this age, particularly the upper members in the West, are far richer in animal remains than those of the Old World. Another fact that gives to our Coal-measure fauna a more Subcarboniferous aspect is, that during the deposition of these rocks in this country the sea seems to have much more frequently re-occupied the area of these formations than in the Old World. For, whatever theory we adopt in regard to the formation of coal, to meet all the requirements of the phenomena here presented, we must admit the frequent presence of the sea, since we find Corals, Brachiopods, Crinoids, marine types of Gasteropods, Acephals, &c., occurring through hundreds of feet of Coal-measure rocks in the West. Indeed, during more than twenty years' familiarity with the fossils of the Western Coal-measures, the writer has never met with any fresh or brackish-water types or terrestrial remains, other than plants. The presence of these marine remains is not the exception, but the rule, particularly in the upper and middle members of the western Coal-measures; they are found over wide areas, and through great thicknesses of strata, and in a profusion and state of preservation that precludes the possibility of accounting for their presence by the supposition that they were transported there by currents or earthquake waves. They occur not only in the limestones, shales, sandstones, &c., alternating with the numerous seams and beds of coal, but in some rare instances directly in the coal itself. The writer has a specimen of impure coal from Illinois, in which there is a very thin *Aviculopecten* entirely replaced by pyrites, which, when the block of coal was first broken open, presented almost the brilliancy of burnished gold. In Ohio also, numerous shark teeth and entire specimens of other marine fishes have been found by Dr. Newberry embedded directly in cannel coal. (See this Journal, [2], xxiii, 212.) Dr. Newberry also found, at another locality in Ohio, specimens of *Solemya* (a marine shell), flattened between the laminæ of good coal. In other instances, beds of coal are known to shade upward into a black, more or less richly bituminous mass, containing great numbers of the shells of Brachiopoda, the delicate, unbroken spines of which can be seen piercing like needles the black matrix.

this conclusion, it should be remembered, is not based upon structure alone, or other lithological characters, but also upon a careful and thoroughly scientific investigation and comparison of the fossil plants characterizing each bed or subordinate stratum.

But it is not alone upon the evidence of structure, and their fossil flora, that the Illinois Coal-measures are known to belong to the same horizon as the regular Coal-measures of Indiana, Kentucky, Ohio, Pennsylvania, &c., for we have also the unmistakable evidence of its group of animal remains. It is true, as in all other formations, species sometimes occur in these rocks in one State that do not in another, but a miscellaneous collection of shells, Corals, Crinoids, Bryozoa, &c., from the Coal-measures of any part of Illinois, would be at once referred to that horizon by any person familiar with the forms characterizing rocks of the same age in any of the States above mentioned. Long lists of species might be cited to illustrate this point, but it is wholly unnecessary; we may remark, however, that *Pleurotomaria sphærulata*, *P. tabulata*, *Euomphalus catilloides*, and *Macrocheilus primigenius*, of Conrad, which are among the most characteristic fossils of the Coal-measures, not only in Illinois, but in Iowa, Missouri, and in part in Kansas, were first described by Mr. Conrad from the regular Coal-measures of Western Pennsylvania.

In relation to the Coal-measures of Illinois belonging to the same horizon as those of Missouri and Iowa, it is scarcely necessary to say anything, as all in this country who have given any attention to the subject know this to be the case, and even Mr. Marcou admits it. Indeed, we have the clearest evidence that the Illinois Coal-field was once continuous with that of Missouri and Iowa, from which it is only separated by the broad valley of denudation scooped out by the Mississippi river. Following the Missouri and Iowa Coal-field westward, we find that it passes uninterruptedly into Kansas and the southeast corner of Nebraska —the valley of the Missouri, owing to the northwestward inclination of the strata, not going deep enough to cut it into two distinct fields.

Throughout all this area, these rocks are characterized by essentially the same fauna. As in all other formations, some species are local in their geographical range, but the majority are not, and, as elsewhere stated, most of them range into the Coal-measures of Kentucky, Indiana, Ohio, and some even into Pennsylvania, while comparatively very few of them have even been suspected of being identical with forms occurring in any lower position.

As much of the evidence relied upon in this review in discussing the relations of the Western Carboniferous and Permian rocks, is derived from the presence or absence of certain types of Brachiopoda, it is but fair that we should not dismiss this part of the subject without letting it be known that Mr. Marcou does not admit the validity of the testimony of this class of Mollusks. His remarks on this point, however, are so extraordinary, that, for fear of being suspected of having done him an injustice in the translation, they are here given in his own words, and in his own language. They are as follows:—"Je suis arrivé à la conviction qu' après les foraminiféres, les brachiopodes sont les plus mauvaises fossiles dont on puisse se servir comme fossiles caractéristiques des formations, et qu'en réalité ils ne sont meme pas du tout les *Leitmuschel.* J' ignore ou les zoologistes placent les brachiopodes, ou meme s'ils sont d' accord entre eux sur la place á leur assigner; mais ces sont certainement des etres tres-inferieurs et plus bas méme dans la série que les coraux, si j'en juge du moins d'après leur utilité pour la geologie pratique."

This is certainly very hard on the Brachiopoda; it was bad enough to place them in any sense of the word below the Corals but to deny them the right, next to the Foraminifera, to testify in regard to the age of the very rocks to which their shells have so largely contributed is still worse. Whatever zoologists may think of this view respecting the rank of the Brachiopoda, they will certainly accord to it the merit of originality. Geologists and paleontologists, however, are not likely to agree with him in regard to the value of this class of Mollusks as a means of distinguishing strata, as their almost united testimony goes directly to the contrary. Various quotations might be made to show this; but one from Mr. Thos. Davidson of London, the highest authority on the Brachiopoda, particularly the fossil forms, will be sufficient. After speaking of their almost universal distribution in marine strata, he says, "their value to the geologist is consequently very great; and, as they so commonly fall under his hammer, where other classes are often but sparingly represented, they must, therefore, be looked upon as excellent data for the age of deposits; for, although some few individual forms pass from one stage to another, the generality are limited to definite horizons."[13]

That in the hands of one who first makes up his mind in regard to the age of a formation from its color and other lithological characters, and then sets to work to interpret its fossils accordingly, without the ability or inclination to discriminate between the genera *Terebratula*, *Spirigera*, *Retzia*, and *Rhynchonella*, the Brachiopoda will be found of little use, and their testimony discordant, is quite natural to suppose. Under other circumstances, however, the result is always very different. Yet after all, the Brachiopoda have less reason to complain of Mr. Marcou's practical, than of his theoretical dealings with them, for

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[13] British Foss. Brach. Genl. Introduction, p. 1.

it will be remembered that, so far as he relied at all upon paleontological evidence, he identifies the rocks at Bellevue, with a particular formation in New Mexico, and both with the Mountain Limestone of Europe, almost *solely* upon the testimony of certain *Brachiopoda.*

After completing his researches in the region of Bellevue, Mr. Marcou continued his progress up the Missouri to examine a sandstone formation extending along that stream for some distance below Sioux City, located at the mouth of Sioux River. In regard to the age of this formation, the readers of this Journal will remember that Mr. Marcou and some geologists in this country have differed widely. In 1853, the writer and Dr. Hayden examined it while returning from an expedition to the Bad Lands for Prof. Hall. They found but a few badly preserved fossils in it, but left the locality under the impression that it was probably Cretaceous, as it was immediately overlaid by rock undoubtedly of that age; which opinion was adopted in a paper published by Prof. Hall and the writer in *Mem. Am. Acad. Arts and Sci.*, 1856.

In his Geological Map, however, of N. America, published in 1855, (*Ann. des Mines,* [2], vii), Mr. Marcou had colored this rock here on the east side of the Missouri, *Mountain Limestone*, and along the west side, *New Red Sandstone.*

In papers published in 1856–7, Dr. Hayden and the writer still maintained that it was of Cretaceous age.

Again, in 1858, in reissuing his Geological map, along with his *Geology of North America,* Mr. Marcou left this rock colored as before on both sides of the Missouri, and on page 143 of the *Geology*, says, "the red formation of the vicinity of Sioux City, lying upon the Carboniferous rocks, is of the age of the New Red Sandstone."

About the same time, Dr. Hayden and the writer maintained that the numerous modern types of dicotyledonous leaves found in this rock could not belong to an older epoch than Cretaceous. But in a paper published in the *Archives des Sci. Bibliothéque Universelle,* June, 1858, Mr. Marcou unhesitatingly referred it, from our later description, to the *Jurassic.* Being perfectly satisfied that even this could not be the case, and in order to convince Mr. Marcou and others of the fact, the writer made sketches of some of the leaves found in this rock, and sent them to Prof. Oswald Heer, of Zurich, Switzerland, one of the most eminent botanical paleontologists of Europe, Dr. Newberry, one of our best authorities in this department of paleontology, being then in New Mexico. Before Prof. Heer's reply was received, Dr. Newberry returned, and the specimens of fossil leaves were submitted to him, and he fully agreed with us that the rock must be Cretaceous. The modern affinities of the leaves showed it could not be older, and its position beneath unquestionable Cretaceous strata, in a district where the undisturbed condition of all the rocks precluded the supposition that there might have been an overthrow, settled the question that it could not be newer.

Soon after Dr. Newberry's return, a letter was received from Professor Heer, who was evidently deceived by the unusually modern affinities of the leaves (and doubtless, though unconsciously to himself, in some degree by the theoretical views of Mr. Marcou, who was then in Zurich), stating that he could not regard these leaves of Cretaceous age, but that they appeared most nearly allied to Miocene types. Almost simultaneously, a printed pamphlet was issued by Mr. Marcou at Zurich, addressed to Meek and Hayden, in which he took the ground that they had included in this leaf-bearing formation all sorts of rocks, *excepting* Cretaceous, and endorsing Prof. Heer's suggestion that the beds containing these leaves are of Miocene age.

Notwithstanding the fact that Dr. Hayden and the writer had against them as weighty an authority as Prof. Heer, they continued to maintain that the rock is, nevertheless, Cretaceous.

After having expressed so many and such widely different opinions in regard to the age of this formation, we may readily understand that it must have been with no ordinary degree of interest that Mr. Marcou caught the first glimpse of these sandstone escarpments as he ascended the Missouri; especially as he had with him a skilful botanical paleontologist, to assist in unraveling the knotty question. After a thorough exploration, however, of the various out-crops in the vicinity of Sioux City, and along the Missouri below there, both of these gentlemen completely satisfied themselves that this formation *is* Cretaceous, and *only* Cretaceous, as had been from the first maintained by all in this country. This they both stated in conversation at Washington City, and Mr. Marcou frankly admits it in the paper under review. And thus ends one long mooted question in the geology of the West.

There are a few points, however, in regard to this formation, upon which the writer cannot agree with Mr. Marcou. In the first place, he (Mr. Marcou) considers it a fresh-water formation, because he found in it shells of a *Cyrena;* (for which he proposes the name *C. Nova-Mexicana*, from the supposition that it is identical with a species found by him in New Mexico). This is undoubtedly the *Cyrena arenaria* of Meek & Hayden,[13] which was long since described from the same locality and position. Its presence there, however, by no means proves this to be a fresh-water deposit, since the writer and Dr. Hayden found directly

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[13] We first described it as a *Cyprina*, from imperfect specimens, but afterward referred it to the genus *Cyrena.*

associated with it casts of an *Axinæa* (= *Pectunculus*), figured and described by Hall and Meek in the *Memoirs Am. Acad. Arts and Sci.* Dr. Hayden and the writer have also described from there a *Mactra*, under the name *M. Siouxensis*, and a *Pharella*. As *Pectunculus* and *Mactra* are marine genera, and *Pharella* and *Cyrena* are brackish-water types, it is manifest that this rock was deposited in a bay or estuary, which must have been alternately brackish, and salt enough to sustain marine Mollusks. The nature of the sediment composing it, as well as the numerous leaves and even trunks of trees, at some places found in it, attest the fact of its being a shore deposit.

Again, while admitting this to be a Cretaceous rock, Mr. Marcou seems to think he is differing from Meek & Hayden, in not referring it to the oldest Cretaceous, and in placing it on a parallel with the New Jersey Cretaceous. In this, however, he is mistaken. We placed it at the base of our section of the *Cretaceous rocks of the Upper Missouri Country,* which position it undoubtedly holds. But we have not, in our more recent papers, especially referred it, even provisionally, to the horizon of the oldest Cretaceous of Europe. By looking at our paper published in the *Proceed. Acad. Nat. Sci. Philad.*, Dec., 1861, page 418, it will be seen that we there refer this rock to the position of the Gray Chalk of English geologists. It will also be seen there, and in our other papers, that we have, almost from the first, maintained that this formation is the exact equivalent of the leaf-bearing beds on Raritan River, New Jersey, forming the inferior member of the Cretaceous rocks of that State.

Mr. Marcou also has some remarks on the question of the parallelism of this sandstone and the overlying *Inoceramus* beds, with the upper part of his Pyramid Mountain section in New Mexico. We have not the space to enter upon the discussion of this question here, nor is it necessary. The able review of this whole subject in Dr. Newberry's report as Geologist of Ives's Colorado Expedition, and Dr. Shumard's several papers on the geology of Texas, place this question in so clear a light that probably no one but Mr. Marcou has any doubts on the subject.

Note on a new Crinoid, referred to on page 164.—Since the remarks on page 164 were in type, Mr. Worthen has sent me specimens of the Crinoid there alluded to, from the Coal-measures of Illinois, which show that, however much it may resemble *Encrinus*, it still presents differences of structure believed to be of generic importance. These differences consist in the presence of *a series of true subradial pieces* ; while it differs from the structure (at least the normal) of *Encrinus*, in having but *two primary radials*, instead of *three*, in each ray. A European specimen of *Encrinus* in the private collection of Smithson, deposited at the Smithsonian Institution, shows five very minute pieces within those generally considered the basal plates. If these minute pieces were developed so as to assume the character of true basal plates, then those generally regarded as such would become subradials, as in our Crinoid. As this, however, seems never to have been the case in true *Encrinus*, it is reasonable to infer that our Carboniferous form presents other differences entitling it to rank as the type of a distinct genus. The genus may be named and characterized as follows:

ERISOCRINUS, Meek & Worthen. Basal plates 5; subradials 5; radials 2 × 5; anal and interradial pieces none. Column and arms unknown. We know the following species:—

1. *E. typus* M. & W.—Body, below the summit of the first radial pieces, basin-shaped, rounded below, and subpentagonal in outline above; composed of thick, smooth, scarcely convex plates; breadth 0·60 inch, height 0·24 inch. Basal pieces small, about half hidden by the column; subradials three times as large as the basal pieces, equally hexagonal; first radial pieces much larger, of equal size and form, being all wider than long, pentagonal in outline, and broadly truncated above for the reception of the succeeding radials, which are nearly as large, equally pentagonal, and support upon their superior sloping sides either the first brachial pieces, or a series of secondary radials.—From the Coal-measures, near Springfield, Ill.

2. *E. Nebrascensis* M. & W.—Differs from the foregoing in having the cup below the summit of the first radials proportionally deeper, in consequence of the larger size of the subradial pieces, but may be only a variety of the same. Breadth, 0·69; height to top of first radials, 0·35 inch.—From the Coal-measures, Bellevue, Nebraska.

ART. XXI.—*Analysis of a Carbonate of Lime and Manganese, (Spartaite of Breithaupt,) from Sterling, Sussex County, New Jersey;* by S. W. TYLER, A.B. Communicated, with remarks, by C. U. SHEPARD.

MORE than a year since I supplied Mr. Tyler (a graduate of this College) with very fresh specimens of this mineral, to be analyzed during his residence at the Göttingen Laboratory. He has just forwarded to me, under date of Nov. 10th, the results he has obtained.

"Sp. gr. =2·815. Before the blowpipe, decrepitates and becomes black without fusing. With soda and microcosmic salt, gives the well-known reaction of manganese. Not acted on by acetic acid, but dissolves easily with effervescence in cold chlorhydric acid.

Analysis gives,

Mn O,	13·79	as 1 : 4 : 5,
Ca O,	43·65	
CO^2,	42·01	

which leads to ($\frac{1}{5}$Mn O+$\frac{4}{5}$Ca O) CO^2 as the formula of the species.

	Calculated.	Obtained.
Mn O,	13·79	13·79
4Ca O,	43·49	43·65
$5CO^2$,	42·72	42·01
	100·00	99·45 "

This is the same mineral as that referred to by Rammelsberg, page 209 of his *Handbuch der Mineralchemie*, as analyzed by Jenzsch and Richter with the following results:

	By Jenzsch.	By Richter.
Carbonic acid,	40·77	44·04
Lime,	48·75	47·92
Magnesia,	0·92	1·21
Protox. iron	0·38	7·13
Protox. manganese,	6·83	
Oxyd of zinc,	0·38	....
Water,	0·32	....
	98·35	100·30

Rammelsberg gives the following as an approximative formula, based on the foregoing:

$$6\dot{C}a\ddot{C}+\left.\begin{matrix}\dot{M}n\\ \dot{M}g\end{matrix}\right\}\ddot{C}.$$

Regarding the magnesia, iron and zinc as accidental, I have only to add, respecting the species, that in color it is generally a very pale reddish white, and where associated with dysluite and jeffersonite (hedenbergite) of a bluish gray tint, sometimes (from partial decomposition) yellowish or reddish brown. It cleaves with facility; and the rhombohedrons thus obtained afford, both by the common and the reflective goniometer, angles rather over than under 106°. The planes are striated parallel to their longer diagonals. With borax before the blowpipe the characteristic manganese reaction is speedily produced.

This mineral had for years been recognized in my cabinet as a double carbonate of lime and manganese, and called by me *calcimangite*, a denomination which I have only recently learned has been anticipated by Breithaupt's *Spartaite*. But as this last name is so nearly synonymous with Spartalite, (given in 1852 by Brooke and Miller, to the red oxyd of zinc), I would suggest the substitution of calcimangite for it, as an appellation that may serve to prevent confusion, the more especially as the two minerals occur together.

Amherst College, Dec. 14, 1864.

ART. XXII.—*Contributions to the Chemistry of Natural Waters*; by T. STERRY HUNT, A.M., F.R.S.

IT is proposed to divide this essay into three parts, in the first of which will be considered some general principles which must form the basis of a correct chemical history of natural waters. The second part will embrace a series of chemical analyses of mineral waters from the Paleozoic rocks of the Champlain and St. Lawrence basins, together with some river-waters; and the third part will consist chiefly of deductions and generalizations from these analyses.

I.

CONTENTS OF SECTIONS.—1, atmospheric waters; 2, 3, results of vegetable decay; 4–7, action on rocky sediments; 8, action on iron-oxyd; 9, solution of alumina; 10, reduction of sulphates; 11, kaolinization; 12, decay of silicates; 13, origin of carbonate of soda; 14, Bischof's view rejected; 15, 16, porosity of rocks, and their contained saline waters; 17, saliferous strata; 18, action of carbonate of soda on saline waters; 19, origin of sulphate of magnesia; 20, 21, Mitscherlich's view rejected; 22, 23, salts from evaporating sea-water. composition of ancient seas, origin of carbonate of lime; 25–27, origin of gypsum, carbonate of magnesia, and dolomite; 28, waters from oxydized sulphurets; 29, origin of free sulphuric and chlorhydric acids; 30, of sulphhydric and boric acids; 31, of carbonic acid gas; 32, of ammoniacal salts; 33–35, classification of mineral waters.

§ 1. The solvent powers of water are such that this liquid is never met with in nature in a perfectly pure state; even meteoric waters hold in solution, besides nitrogen, oxygen, carbonic acid, ammonia, and nitrous compounds, small quantities of solid matters which were previously suspended in the form of dust in the atmosphere. After falling to the earth, these same waters become still farther impregnated with foreign elements of very variable nature, according to the conditions of the surface on which they fall.

§ 2. Atmospheric waters, coming in contact with decaying vegetable matters at the earth's surface, take from them two classes of soluble ingredients, organic and inorganic. The waters of many streams and rivers are colored brown with dissolved organic matter, and yield, when evaporated to dryness, colored residues which carbonize by heat. This organic substance, in some cases at least, is azotized, and similar if not identical in composition and properties with the apocrenic acid of Berzelius. The decaying vegetation, at the same time that it yields a portion of its organic matter in a soluble form, parts with the mineral or cinereal elements which it had removed from the soil during life. The salts of potassium, calcium, and magnesium, the silica and phosphates which are so essential to the growing plant, are liberated during the process of decay, and hence we find these elements almost wanting in peat and coal. See, on this point, the analyses by Vohl of peat, peat-moss, and the soluble matters set free during its decay. *Ann. der Chem. und Pharm.*, cix, 185, cited in *Rép. Chim. Appliquée*, i, 289. Also Liebig, analysis of bog-water; *Letters on Modern Agriculture*, p. 44; and, in the second part of this paper, the analysis of the waters of the Ottawa river.

§ 3. At the same time, an important change is effected in the gaseous contents of the atmospheric waters. The oxygen which they hold in solution is absorbed by the decaying organic matter, and replaced by carbonic acid, while any nitrates or nitrites which may be present are by the same means reduced to the state of ammonia (Kuhlmann). By thus losing oxygen, and taking up a readily oxydizable organic matter, these waters become reducing instead of oxydizing media in their farther progress.

§ 4. We have thus far considered the precipitated atmospheric waters as remaining at the earth's surface, but a great portion of them sooner or later in their course come upon permeable strata by which they are absorbed, and in their subterranean circulation undergo important changes. The effect of ordinary argillaceous strata destitute of neutral soluble salts may be first examined. Between such sedimentary strata and the waters charged with organic and mineral matters from decaying vegetation, there are important reactions. The composition of these waters is peculiar. They contain, relatively to the sodium, a large amount of potassium salts, besides notable quantities of silica and phosphates, in addition to the dissolved organic matters and the earthy carbonates, and in some cases ammoniacal salts and nitrates or nitrites. The sulphuric acid and chlorine are moreover not sufficient to neutralize the alkalies, which are perhaps in part combined with silica or with an organic acid.

§ 5. The experiments of Way, Voelcker, and others have shown that when such waters are brought into contact with argillaceous sediments, they part with their potash, ammonia, silica, and phosphoric acid and organic matter, which remain in combination with the soil; while, under ordinary conditions at least, neither soda, lime, magnesia, sulphuric acid nor chlorine are retained. This power of the soil appears from the experiments of Eichhorn to be in part due to the action of hydrated double aluminous silicates; and the process is one of double exchange, an equivalent of lime or soda being given up for the potash and ammonia retained. The phosphates are probably retained in combination with alumina or peroxyd of iron; and the silica and organic matters also enter into insoluble combinations. It follows from these reactions that the surface-waters charged with the products of vegetable decay, after having been brought in contact with argillaceous sediments, retain little else than sulphates, chlorids, or carbonates of soda, lime and magnesia. In this way the mineral matters required for the growth of plants, and by them removed from the soil, are again restored to it; and from this reaction results the small proportion of potash salts in the waters of ordinary springs and wells as compared with river-waters. From the waters of rivers, lakes, and seas, aquatic plants again take up the dissolved potash, phosphates, and silica; and the subsequent decay of these plants in contact with the ooze of the bottom, or on the shores, again restores these elements to the earth. See a remarkable essay by Forchhammer, on the composition of fucoids, and their geological relations, *Jour. für Prakt. Chem.*, xxxvi, 388.

§ 6. The observations of Eichhorn upon the reaction between solutions of chlorids and pulverized chabazite, which, as a hydrated silicate of alumina and lime, may perhaps be taken as a representative of the hydrous double silicates in the soil, show that these substitutions of protoxyd bases are neither complete nor absolute. It would appear, on the contrary, that there takes place a partial exchange or a partition of bases according to their respective affinities. Thus the normal chabazite in presence of a solution of chlorid of sodium exchanges a large portion of its lime for soda; but if the resulting soda compound be placed in a solution of chlorid of calcium, an inverse substitution takes place, and a portion of lime enters again into the silicate, replacing an equivalent of soda; while, by the action of a solution of chlorid of potassium, both lime and soda are, to a large extent, replaced by potash. In like manner, chabazite in which by the action of a solution of sal-ammoniac, a part of the lime has been replaced by ammonia, will give up a portion of the ammonia, not only to solutions of chlorids of potassium and sodium, but even to chlorid of calcium. It results from these mutual decompositions, that there is a point where a chabazite containing both lime and soda, or lime and ammonia, would remain unchanged in mixed solutions of the corresponding chlorids, the affinities of the rival bases being balanced.[1] Inasmuch, however, as the proportions of ammonia and potash in natural waters are usually small, when compared with the amounts of lime and soda existing in the form of hydro-silicates in the soil, the result of these affinities is an almost complete elimination of the ammonia and potash from infiltrating waters.

§ 7. That the replacement of one base by another in this way is not complete is shown, moreover, by the experiments of Liebig, Dehérain and others, who have observed that a solution of gypsum removes from soils a certain amount of potash-salt, which was insoluble in pure water. In this way gypseous waters may also acquire portions of sulphate of soda, and perhaps of sulphate of magnesia, from silicates.

[1] This Journal, [2] xxviii, 72.

It is not certain that all the above reactions observed for chabazite are applicable without modification to the double hydro-aluminous silicates of sedimentary strata. Were such the case, important changes might, in certain conditions, be effected in the composition of saline waters. Thus, in presence of a great amount of a hydrous silicate of lime and alumina, solutions of chlorid of sodium might acquire a considerable amount of chlorid of calcium; but it is probable that these reactions, however important they may be in relation to the soil, and to surface-waters with their feeble saline impregnation, have at present but little influence on the composition of the stronger saline waters. It is, however, not impossible that the action of the ancient sea-waters, holding a large amount of chlorid of calcium, upon the hydrated and half-decomposed feldspars which constituted the clays of the period, may have given rise to those double silicates which formed the lime-soda feldspars so abundant in the Labrador series.

§ 8. The reactions just described assume an importance in the case of waters impregnated with soluble matters from vegetable decay, and in this event another and not less important class of phenomena intervenes, which are due to the deoxydizing power of the dissolved organic matter. By the action of this upon the insoluble peroxyd of iron set free from the decomposition of ferruginous minerals and disseminated in the sediments, protoxyd of iron is formed, which is soluble both in carbonic acid, and in the excess of the organic (acid) matter. By this means, not only are great quantities of iron dissolved, but masses of sediments are sometimes entirely deprived of iron-oxyd, and thus beds of white clay and sand are formed. The waters thus charged with proto-salts of iron absorb oxygen when exposed to the air, and then deposit the metal as hydrated peroxyd, which when the organic matter is in excess, carry down a greater or less proportion of it in combination. Such organic matters are rarely absent from limonite, and in some specimens of ochre amount to as much as fifteen per cent.[2] The conditions under which hydrous peroxyd of manganese is often found are very similar to those of hydrous peroxyd of iron, with which it is so frequently associated; and there is little doubt that oxyd of manganese may be dissolved by a process like that just pointed out. A portion of manganese has been observed in the soluble matters from decaying peat-moss; and it seems to be generally present in small quantities with iron in surface-waters.

§ 9. There is reason to believe that alumina is also, under certain conditions, dissolved by waters holding organic acids. The existence of pigotite, a native compound of alumina with an organic acid, and the occasional association of gibbsite with

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[2] Geology of Canada, p. 512.

limonite point to such a reaction. That it is not more abundant in solution, is due to the fact that, unlike most other metallic oxyds, alumina, instead of being separated in a free state by the slow decomposition of its silicious compounds, remains in combination with silica. The formation of bauxite, a mixture of hydrate of alumina with variable proportions of hydrous peroxyd of iron, which forms extensive beds in the Tertiary sediments of the great Mediterranean basin, indicates a solution of alumina on a grand scale, and perhaps owes its origin to the decomposition of solutions of native alum by alkaline or earthy carbonates. Emery, a crystalline anhydrous form of alumina, has doubtless been formed in a similar manner. See this Journal, [2] xxxii, 287. The existence in many localities of an insoluble sub-sulphate of alumina, websterite, in layers and concretionary masses in Tertiary clays, evidently points to such a process. Compounds consisting chiefly of hydrated alumina are frequently found in fissures of the chalk in England. On the absence of free hydrated alumina from soils, see Muller, cited in this Journal, [2] xxxv, 292.

§ 10. The organic matter dissolved by the surface-waters serves to reduce to the condition of sulphurets the various soluble sulphates which it takes up at the same time or meets with in its course. These sulphurets, decomposed by carbonic acid, which is in part derived from the atmosphere, and in part from the oxydation of the carbon of the organic matter, give rise to alkaline and earthy carbonates on the one hand, and to sulphuretted hydrogen on the other. In this way, under the influence of a somewhat elevated temperature, are generated sulphurous waters, whether of subterranean springs, or of tropical sea-marshes and lagoons. The reaction between the sulphurets thus formed and the salts or oxyds of iron, copper and similar metals which may be present, gives rise to metallic sulphurets. The decomposition of sulphuretted hydrogen by the oxygen of the air, produces native sulphur, with which are generally found associated sulphates of lime and strontia. By virtue of these reactions, soluble sulphates of lime and magnesia may be completely eliminated from waters, the bases as insoluble carbonates, and the sulphur as sulphuretted hydrogen, free sulphur, or a metallic sulphuret. Moreover, as Forchhammer has pointed out in the paper already cited, sulphuret of potassium in the presence of ferruginous clays is also completely separated from solution, the sulphur as sulphuret of iron, and the alkali as a double aluminous silicate.

§ 11. We have thus far considered the composition of surface-waters as modified by the decay of vegetation, or by the reactions between the matters derived from this source and the permeated sediments. Not less important, however, than the elements thus removed by substitution from sedimentary strata are those which are liberated by the slow decomposition of the minerals composing these sediments.

It has long been known that in the transformation of a feldspar into kaolin the double silicate of alumina and alkali takes up a portion of water, and is resolved into a hydrous silicate of alumina; while the alkali, together with a definite portion of silica, is separated in a soluble state. The feldspar, an anhydrous double salt formed at an elevated temperature, has a tendency under certain conditions to combine at a lower temperature with a portion of water, and break up into two simpler silicates. Daubrée has moreover shown that when kaolin is exposed to a heat of 400° C. in presence of a soluble silicate of potash the two silicates unite and regenerate feldspar. These reactions are completely analogous to those presented by very many other double salts, ethers, amids, and similar compounds. The preliminary conditions of this conversion of feldspar into kaolin and a soluble alkaline silicate, however, still require investigation. It is known that while some feldspathic rocks appear almost unalterable, others containing the same species of feldspar are found converted into kaolin to a depth of many feet from the surface. This chemical alteration, according to Fournet, is always preceded by a mechanical change of the feldspar, which first becomes opaque and friable, and is thus rendered permeable to water. He conceives this alteration to be molecular and to be connected with the passage of the silicate into a dimorphous or allotropic condition.[3]

§ 12. The researches of Ebelman in the alterations of various rocks and minerals have thrown considerable light on the relations of sediments and natural waters.[4] From the analyses of basaltic and similar rocks, which include silicates of lime, magnesia, iron and manganese in the forms of pyroxene, hornblende and olivine, and which undergo a slow and superficial decomposition under atmospheric influences, it appears that during the process of decay the greater part of the lime and magnesia is removed, together with a large proportion of silica. It was found moreover that in the case of a rock apparently composed of labradorite and pyroxene, the removal of the lime and magnesia from the decomposed portion was much more complete than that of the alkalies; showing thus the comparatively greater stability of the feldspathic element. The decomposition of the feldspar in these mixed rocks is, however, at length effected, and the final result approximates to a hydrous silicate of alumina, or clay. This slow decomposition of silicates of protoxyd-bases appears to be due to the action of carbonic acid, which, remov-

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[3] Ann. de Chimie, [2], lv, 225.

[4] Ebelman, Recueil des Travaux, ii, 1-79.

ing the lime and magnesia as carbonates, liberates the silica in a soluble form; while the iron and manganese, passing to a state of higher oxydation, remain behind, unless the action of organic matters intervenes to give them solubility.

§ 13. It is to be remarked that, apart from the peculiar and complete decomposition resulting in the production of kaolin, to which orthoclase, oligoclase, and some other feldspathides, as leucite, beryl, and perhaps also the scapolites and albite are occasionally subject, orthoclase is less liable to change than the soda-feldspars, albite, oligoclase and labradorite. Weathered surfaces of these become covered with a thin, soft, white and opaque crust, from decomposition, while the surfaces of orthoclase under similar conditions still preserve their hardness and translucency. The decomposition of feldspathides, and other aluminous double silicates, whether rapid and complete, or slow and partial, apparently yields the same results. A gradual process of this kind is constantly going on in the feldspathic matters which form a large proportion of the mechanical sediments of all formations; and in deeply buried strata is not improbably accelerated by the elevation of temperature. The soluble alkaline silicate resulting from this process is in most cases decomposed by carbonates of lime and magnesia in the sediments, giving rise to silicates of these bases, (which are for the greater part separated in an insoluble state,) and to carbonate of soda. Only in rare cases does potash appear in large proportion among the soluble salts thus liberated from sediments, partly because soda-feldspars are more subject to change, and partly from the fact that potash salts would be separated from the percolating waters in virtue of the reactions mentioned in § 5. Hence it happens that apart from the neutral soda salts of extraneous origin, waters permeating sediments containing alkaliferous silicates generally bring to the surface little more than soda combined with carbonic and sometimes with boric acid, and carbonates of lime and magnesia with small portions of silica.

§ 14. This explanation of the decomposition of alkaliferous silicates and of the origin of carbonate of soda is opposed to the view of Bischof, who conceives that carbonic acid is the chief agent in decomposing feldspathic minerals.[5] The solvent action of waters charged with carbonic acid is undoubted, as shown by various experimenters, especially by the Messrs. Rogers;[6] but this acid is not always present in the quantities required. The proportion of it in atmospheric waters is so inadequate that it becomes necessary to suppose some subterranean source of the gas, which is by no means a constant accompaniment of natron springs. A copious evolution of carbonic acid is observed in

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[5] Bischof, Chem. Geol., ii, 181.

[6] This Journal, [2], v, 401.

the vicinity of the lake of Laach, where the alkaline waters studied by Bischof occur.[7] The same thing is met with in many other localities of such springs, among which may be mentioned the region around Saratoga, where saline waters containing carbonate of soda, and highly charged with carbonic acid, rise in abundance from the Lower Silurian strata; but farther northward, along the valleys of Lake Champlain and the St. Lawrence, similar alkaline saline waters, which abound in the continuation of the same geological formations, are not at all acidulous. From this the conclusion seems justifiable that the production of carbonate of soda is a process, in some cases at least, independent of the presence of free carbonic acid. In this connection, it is well to recall the solvent power of pure water on alkaliferous silicates, as shown more especially by Bunsen, and also by Damour, who found that distilled water at temperatures much below 212° takes up from silicates like palagonite and calcined mesotype, comparatively large amounts both of silica and alkalies. (Damour, *Ann. Chem. et Phys.*, [3], xix, 481.)

§ 15 Another and an important source of mineral impregnation to waters exists in the soluble salts enclosed in sedimentary strata, both in the solid state and in aqueous solution, and for the most part of marine origin. In order to form some conception of the amount of saline matters which may be contained in a dissolved state in the rocky strata of the earth, we have made numerous experiments to determine the porosity of various rocks; some few of the results of which may here be noticed. Fragments of the rocks were dried at a heat of 150° to 200° F., in a current of dry air until they ceased to lose weight. They were then soaked in distilled water, and kept under it for many hours beneath an exhausted receiver. When thus saturated they were wiped from adhering water, and weighed; first in air to determine the augmentation of weight from absorption, and secondly, in water to give, by the loss in weight, the volume of the specimens. These data furnish the means of determining the volume of water absorbed, which is given below for 100.00 parts of different rocks from the Paleozoic strata of the St. Lawrence basin.

Potsdam	formation,	(sandstone)	3	specimens	2·26—2·71
"	"		3	"	6·94—9·35
Calciferous	"	(cryst. dolomite)	4	"	1·89—2·53
"	"	" "	2	"	5·90—7·22
Chazy	"	(argill. limestone)	4	"	6·45-13·55
Trenton	"	(gray crys. ")	4	"	1·18—1·70
"	"	(black impalp. ")	2	"	0·30—0·32
Utica	"	(black shale)	3	"	0·75—2·10
Hudson River	"	(arenaceous shale)			—7·94
Medina	"	(argill. sandstone)	2	specimens	8·37-10·06
Guelph	"	(cryst. dolomite)	3	"	9·34-10·60
Niagara	"	(impalpable ")	2	"	9·69-10·92

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[7] Bischof, Lehrbuch, i, 357-363.

The above data might be much more extended, but sufficient have been given to show the porosity of the principal Paleozoic rocks of the basin.[*]

§ 16. If we take from the Potsdam sandstone the mean of the first three trials, giving 2·5 per cent for the volume of water which it is capable of holding in its pores, we find that a thickness of 100 feet of it would contain in every square mile, in round numbers, 70,000,000 cubic feet of water; an amount which would supply a cubic foot (over seven gallons) a minute for more than thirteen years. The observed thickness of the Potsdam sandstone in the district of Montreal, varies from 200 to 700 feet, and the mean of 500 feet may be taken. To this are to be added 300 feet for the Calciferous formation, whose capacity for water may be taken, like the Potsdam sandstone, at 2·5 per cent. We have thus in each square mile of these formations, wherever they lie below the water-level, a volume of 490,000,000 cubic feet of water, equal to a supply of a cubic foot per minute for 106 years. The capacity of the 800 feet of Chazy and Trenton limestones which succeed these lower formations, may be fairly taken at one half that of those just named. But it is unnecessary to multiply such calculations—enough has been said to show that these sedimentary strata include in their pores great quantities of water, which was originally that of the ocean of the Paleozoic age. These strata throughout the great Silurian basin of the St. Lawrence, are now for the greater part beneath the sea-level; nor is there any good reason for supposing them to have ever been elevated much above their present horizon. Wells and borings sunk in various places in these rocks show them to be still filled with bitter saline waters; but in regions where these rocks are inclined and dislocated, surface-waters gradually replace these saline waters, which in a mixed and diluted state appear as mineral springs. These saline solutions, other things being equal, will be better preserved in limestones or argillaceous rocks than in the more porous and permeable sandstones.

§ 17. But besides the saline matters thus disseminated in a dissolved state in ordinary sedimentary rocks, there are great volumes of saliferous strata, properly so-called, charged with the results of the evaporation of ancient sea-basins. These strata enclose not only gypsum and rock-salt, but in some regions large quantities of the double chlorid of potassium and magnesium, carnallite; and in others sulphate of soda, sulphate of magnesia, and complex sulphates like blödite and polyhallite. Besides these crystalline salts, the mother-liquors containing the

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[*] A great many similar determinations will be found in a Report on Building Stones to the British House of Commons in 1839, by Barry, Dela Beche and Smith See also Delesse. Bull. Soc. Geol., [2], xix, 64.

more soluble and uncrystallizable compounds, may also be supposed to impregnate, in some cases, the sediments of these saliferous formations. The conditions under which these various salts are deposited from sea-water, and their relations to the composition of the ocean in earlier geological periods, are reserved for consideration in § 22. Infiltrating waters remove from these saliferous strata their soluble ingredients; which, together with the ancient sea-waters of other sedimentary rocks, give rise to the various neutral saline waters; while the mingling of these in various proportions with the alkaline waters whose origin has been described in § 13, produces intermediate classes of waters of much interest.

§ 18. I have elsewhere described the results of a series of experiments on the mutual action of the waters of these two classes.[9] When a dilute solution of bicarbonate of soda is gradually added to a solution which, like sea-water, contains besides chlorid of sodium, the chlorids and sulphates of calcium and magnesium, the greater part of the lime separates as carbonate, carrying down with it only from one to three-hundredths of carbonate of magnesia; a portion of lime however remaining in solution as bicarbonate. When the chlorid of calcium is wholly decomposed, the magnesian salt is attacked in its turn, and there finally results a solution in which the whole of the earthly chlorids are replaced by chlorid of sodium. A farther addition of the solution of carbonate of soda gives them the character of alkaline-saline waters; which moreover contain abundance of earthy carbonates.

The substitution of neutral carbonate for bicarbonate of soda in the above experiment does not affect the result, except in causing a somewhat larger proportion of magnesia to be thrown down with the carbonate of lime. The resulting liquid still retains large quantities of earthy carbonates in solution.[10]

§ 19. In the saline waters just considered, chlorids generally predominate, the sulphates being small in amount, and often altogether wanting. Some exceptions to this are however met with; for apart from waters impregnated with gypsum, whose origin is readily understood, there are others in which sulphate of soda or sulphate of magnesia enter largely. The soda-salt may sometimes be formed by the reaction between solution of gypsum and natriferous silicates referred to in § 7; or by the decomposition of gypsum by a solution of carbonate of soda; while in other cases its origin will probably be found in the natural deposits of sulphates, such as glauberite, thenardite, and glauber-salt, which occur in saliferous rocks; a similar origin is probable for many of those springs in which sulphate of mag-

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[9] This Journal, [2], xxviii, 170.

[10] Geol. Survey of Canada, Report, 1853-56, p. 468.

nesia predominates. This salt also effloresces abundantly in a nearly pure form upon certain limestones, and is in some cases due to the action of sulphates from decomposing pyrites upon magnesian carbonate or silicate. In by far the greater number of cases, however, its appearance is unconnected with any such process; and is, according to Mitscherlich, due to a reaction between dolomite and dissolved gypsum.

§ 20. In support of this view, it was found by the chemist just named that when a solution of sulphate of lime was made to filter for some time through pulverized magnesian limestone, it was decomposed with the formation of carbonate of lime and sulphate of magnesia. This reaction I have been unable to verify. A solution of gypsum in distilled water was made to percolate slowly through a column of several inches of finely-powered dolomite; and after ten filtrations, occupying as many days, no perceptible amount of sulphate of magnesia had been formed. Solutions of gypsum were then digested for many months with pulverized dolomite, and also with crystalline carbonate of magnesia, but with similar negative results; nor did the substitution of a solution of chlorid of calcium lead to the formation of any soluble magnesian salt. Solutions of gypsum were then impregnated with carbonic acid, and allowed to remain in contact with pulverized dolomite and with magnesite as before, during six months of the warm season, when only inappreciable traces of magnesia were taken into solution. These experiments show that no decomposition of dissolved gypsum is effected by native carbonate of magnesia or by the double carbonate of lime and magnesia at ordinary temperatures.

§ 21. I find however that hydrated carbonate of magnesia readily completely decomposes a solution of gypsum when agitated with it, with formation of carbonate of lime and sulphate of magnesia, and the same result is produced with the native hydrate of magnesia when mingled with a solution of gypsum in presence of carbonic acid. Now there may be dolomites which contain an admixture of hydro-carbonate of magnesia, as there certainly are others which like predazzite, are penetrated with hydrate of magnesia. The reaction between solutions of gypsum and such magnesian limestones, (with the intervention in the case of predazzite, of atmospheric carbonic acid), would suffice to explain the results obtained by Mitscherlich, and the appearance in certain cases of sulphate of magnesia as an efflorescence on dolomites. In the experiments above described, the nearly pure crystalline dolomites from the Guelph and Niagara formations were made use of.

§ 22. When sea-water is exposed to spontaneous evaporation, the lime which it contains separates in the form of sulphate, gypsum being but sparingly soluble in a concentrated brine, and the greater portion of the chlorid of sodium crystallizes out in a nearly pure state. The mother-liquor of specific gravity 1·24, having lost about four-fifths of its chlorid of sodium, still contains dissolved a large proportion of sulphate of magnesia. If the evaporation is continued at the ordinary temperature, till a density of 1·32 is attained, about one-half of the magnesian sulphate separates, mixed with common salt; and by reducing the temperature to 6° C., a large portion of pure sulphate of magnesia now crystallizes out. The further evaporation of the remaining liquor by the heat of summer causes the potassium salts to separate in the form of a hydrous double chlorid of potassium and magnesium, an artificial carnallite.[11]

By varying somewhat the conditions of temperature, the sulphate of magnesia and chlorid of sodium of the mother-liquor undergo mutual decomposition, with the production of sulphate of soda and chlorid of magnesium. Hydrated sulphate of soda crystallized out from such a mixed solution at 0° C., and by reducing the temperature to $-18°$ C. the greater part of the sulphates may be separated in this form from the mother-liquor of 1·24, previously diluted with one-tenth of water; without which addition a mixture of hydrated chlorid of sodium would separate at the same time. If, on the other hand, the temperature of the mixed solution be raised above 50° C., the sulphate of soda crystallizes out in the anhydrous form, as thenardite. By the spontaneous evaporation during the heats of summer of the mother-liquors of density 1·35, a double sulphate of potassium and magnesium separates. These reactions are taken advantage of on a great scale in Balard's process, as modified by Merle,[12] for extracting salts from sea-water.

§ 23. The results of the evaporation of sea-water would however be widely different if an excess of lime-salts were present. In this case the whole of the sulphates present would be deposited in the form of gypsum at an early stage of the evaporation, and the mother-liquor after the separation of the greater part of the common salt, would contain little else than the chlorids of sodium, potassium, calcium, and magnesium.

§ 24. A consideration of the conditions of the ocean in earlier

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[11] The hydrous double chlorid of potassium and magnesium (carnallite of H. Rose), occurs in large quantities in a stratum of clay overlying a great bed of rock-salt 100 feet thick, at Stassfurth in Prussia. It is associated with considerable quantities of sulphate of magnesia. According to Clemm this sulphate of magnesia, to which the name of *kieserite* has been given, and which occurs also in Anhalt, contains but one equivalent of water, (MgO,SO_3+HO). It is not more soluble than gypsum, and unlike the ordinary sulphate of magnesia, loses the whole of its acid at a red heat in a current of steam, the acid passing off undecomposed. This salt is found in such large quantities as to be of economic importance. (Bull. Soc. Chim. de Paris, 1864, p. 297.)

[12] See my paper in this Journal, [2], xxv, 361; also Report of Juries of the Exhibition of 1862, class II, p. 48.

geological periods will show that it must have contained a much larger quantity of lime-salts than at present. The alkaline carbonates, whose origin has been described in § 13, and which from the earliest times have been flowing into the sea, have gradually modified the composition of its waters, separating the lime as carbonate, and thus replacing the chlorid of calcium by chlorid of sodium, as I have long since pointed out.[13] This reaction has doubtless been the source of all the carbonate of lime in the earth's crust, if we except that derived from the decomposition of calcareous silicates. (§ 12). In this decomposition by carbonate of soda, as already described in § 18, it results from the incompatibility of chlorid of calcium with hydrous carbonate of magnesia, that the lime is first precipitated with a little adhering carbonate of magnesia, and it is only when the chlorid of calcium is all decomposed that the magnesian chlorid is transformed into carbonate of magnesia. This latter reaction can consequently take place only in limited basins, or in portions cut off from the oceanic circulation.

§ 25. It follows from what has been said that the lime-salt may be eliminated from sea-water either as sulphate or as carbonate. In the latter case no concentration is required; while in the former the conditions are two,—a sufficient proportion of sulphates to convert the whole of the lime into gypsum, and such a degree of concentration of the water as to render this insoluble. These conditions meet in the evaporation of modern sea-water; but the evaporated sea water of earlier periods, with its great predominance of lime-salts, would still contain large amounts of chlorid of calcium, the insolubility of gypsum in this case serving to eliminate all the sulphates from the mother-liquor. Evaporation alone would not suffice to remove the whole of the lime-salts from waters in which the calcium present was more than equivalent to the sulphuric acid; but the intervention of carbonate of soda would be required.

§ 26. In concentrated and evaporating waters freed from lime-salts by either of the reactions just mentioned, but still holding sulphate of magnesia, another process, which I have elsewhere described, may intervene.[14] The addition of a solution of bicarbonate of lime to such a solution gives rise, by double decomposition, to sulphate of lime and bicarbonate of magnesia. The former being much the less soluble salt, especially in a strongly saline liquid, is deposited as gypsum; and subsequently the magnesian carbonate is precipitated in a hydrous form. The effect of this reaction is to eliminate from the sea-water both the sulphuric acid and the magnesia, without the permanent addition to it of any foreign element.

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[13] Canadian Journal for 1858, p. 202; this Journal, [2], xxv, 102, and *Comptes Rendus*, June 9, 1862, p. 1191.

[14] This Journal, [2], xxviii, 174.

§ 27. Gypsum may thus be separated from sea-water by two distinct processes,—the one a reaction between sulphate of magnesia and chlorid of calcium, and the other between the same sulphate and carbonate of lime. The latter, involving a separation of bicarbonate of magnesia, can as we have seen, only take place when the whole of the chlorid of calcium has been eliminated; and if we suppose the ancient ocean, unlike the present, to have contained more than an equivalent of lime for each equivalent of sulphuric acid, it is evident that a lake or basin of sea-water free from lime salts could only have been produced by the intervention of carbonate of soda. The action of this must have eliminated the whole of the lime as carbonate, or at least have so far reduced the amount of this base that the sulphates present would be sufficient to separate the remainder by evaporation, in the form of gypsum, and still leave in the mother-liquor a quantity of sulphate of magnesia for reaction with bicarbonate of lime.

The source of the magnesian carbonate, whose union, under certain conditions, with the carbonate of lime gives rise to the dolomite,[15] may thus be due either to the reaction just described between bicarbonate of lime and solutions holding sulphate of magnesia, or to the direct action of carbonate of soda upon waters containing magnesian salts; but in either case the previous elimination of the incompatible chlorid of calcium must be considered an indispensable preliminary to the production of the magnesian carbonate.

§ 28. To the three principal sources of mineral matters in mineral waters already enumerated, viz., decaying organic matters, decomposing silicates, and the soluble saline matters in rocks, a few other minor ones must be added. One of these is the oxydation of metallic sulphurets, chiefly iron-pyrites, giving rise to sulphate of iron, and more rarely to sulphates of copper, zinc, cobalt and nickel; and by secondary reactions to sulphates of alumina, lime, magnesia, and alkalies. This process of oxydation is necessarily superficial and local, but the soluble sulphates thus formed have probably played a not unimportant part. (§ 9.)

§ 29. Besides these last, which contain chiefly neutral and acid salts, there is another class of waters characterized by the presence of free sulphuric or chlorhydric acid, or both together. These acid waters sometimes occur as products of volcanic action, during which both chlorhydric acid and sulphur are often evolved in large quantities. This latter element generally comes to the surface as sulphuretted hydrogen, which by the

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[15] This Journal, [2], xxviii, 180–186, and further, Geol. Survey of Canada, Report for 1859, 214–218.

oxydation of the hydrogen may deposit its sulphur in craters and fissures. In other cases, as shown by Dumas, the sulphur and hydrogen may be slowly and simultaneously oxydized at a low temperature, giving rise directly to sulphuric acid. Not less frequent, however, is probably the direct conversion, by combustion of the sulphuretted hydrogen into water and sulphurous acid, which afterward absorbing oxygen from the air is converted into sulphuric acid.

§ 30. The source of the hydrochloric acid and the sulphur of volcanos is probably the decomposition of chlorids and sulphates at high temperatures. It is known that for the decomposition of earthy chlorids, water and an elevated temperature are sufficient, and at a higher temperature, chlorid of sodium is readily decomposed in presence of siliceous and aluminous minerals, with the intervention of water. Another agency which probably comes into play in volcanic phenomena is that of organic matters, which, reducing the sulphates to sulphurets, enable the sulphur to be subsequently disengaged as sulphuretted hydrogen by the operation of water, either with or without the intervention of carbonic acid or of siliceous and argillaceous matters. Even in cases where this reducing action is excluded, the ignition of sulphates in contact with earthy matters must liberate the sulphuric acid as a mixture of sulphurous acid and oxygen; and these uniting in their distillation upward through the strata, may give rise to springs of sulphuric acid.[18] The reactions similar to those just noticed, involving borates like stassfurthite and hayesine, or boric silicates like tourmaline, etc., are to be ascribed the large amounts of boric acid which are sublimed in some volcanos, or volatilized with the watery vapor of the Tuscan *suffioni*.

§ 31. The action of subterranean heat upon buried strata containing sulphates and chlorids is then sufficient to explain the appearance of hydrochloric and sulphurous acids and sulphur, even without the intervention of organic matters, which are, however, seldom or never wanting; whether as coal, lignite, bitumen, and pyroschists, or in a more divided condition. The presence of hydrogen and of marsh-gas, as observed by Deville among volcanic products, is an evidence of this. The generation of marsh-gas, is, however, in most cases clearly unconnected with volcanic action or subterranean heat.

To the decomposition of carbonates in buried strata by siliceous matters, with the aid of heat, is to be ascribed the great amounts of carbonic acid gas which are in many places evolved from the earth, and impregnating the infiltrating waters, give rise to acidulous springs. The principal sources of this gas in Europe are in regions adjoining volcanos, either active or recently extinct;

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[18] See the note to § 22, on kieserite.

but their occurrence in the Paleozoic strata of the United States, far remote from any evidence of volcanic phenomena other than slightly thermal springs, shows that an action too gentle or too deeply-seated to manifest itself in igneous eruptions, may evolve carbonic acid abundantly. The sulphuric-acid springs of Western New York and Canada, to be described further on, are not less remarkable illustrations of the same fact.

§ 32. The frequent presence of ammoniacal salts in volcanic exhalations is here worthy of notice, especially when considered in connection with the rarity of nitric and ammoniacal compounds in natural waters, except in some local conditions, as in the wells of cities, etc., where they are sometimes observed in comparatively large amounts. The explanation of this is evident: for although nitrates themselves are not directly removed from the water, they are by the reducing action of organic matters converted into ammonia, which is retained by the soil. In consequence of this affinity, the argillaceous strata, whether of the present period or of older formations, hold in a very fixed form a considerable quantity of nitrogen. This, from the slowness with which it is eliminated in the form of ammonia under the influence of alkaline solutions, probably exists as an ammoniacal silicate, (§ 6). The action of acids, however, as well as alkalies may be supposed to liberate it from its combination, and thus generate the ammoniacal salts which are such frequent accompaniments of volcanic phenomena. The numerous experiments of Delesse show that ammonia, or at least nitrogen capable of being evolved by heat and alkalies in the form of ammonia, is present in the limestones, marls, argillites, and sandstones of former geological periods, in quantities scarcely inferior to those in similar deposits of modern times, amounting for most of the ancient sedimentary strata to from one to five thousands of nitrogen;[17] from which it will be seen that the amount of this element thus retained in the rocky strata of the earth's crust is very great.[18]

§ 33. If we attempt a chemical classification of natural waters in accordance with the principles laid down in the preceding sections they may be considered under the following heads:

A. Atmospheric waters.
B. Waters impregnated with the soluble products of vegetable decay.
C. Waters impregnated with the salts from decomposing feldspathic rocks, and holding a portion of carbonate of soda as a characteristic ingredient.
D. Waters holding neutral salts of sodium, calcium or magnesium from strata where they existed as solid salts, or as impregnating brines.

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[17] Ann. des Mines, [5], xviii, 151–523.

[18] For an exposition of the views put forward in the four preceding sections, see my paper in the Canadian Journal for 1858, p. 206.

E. Waters holding chiefly sulphates from decomposing pyrites; copperas and alum waters.
F. Waters holding free sulphuric or chlorhydric acid.

§ 34. The name of mineral waters is popularly applied only to such as contain sufficient foreign matters to give them a decided taste; and hence the waters of the divisions A and B, and many of the feebler ones of C and D, are excluded. Those of E and F have peculiar local sources; but those of C and D are often associated in adjacent geological formations, and their commingling in various proportions gives rise to mineral waters intermediate in composition. In accordance of these considerations, a classification of mineral waters for technical purposes was adopted by me in *The Geology of Canada*, p. 531, including only those of C, D, and F, which were arranged in six classes.

I. Saline waters containing chlorid of sodium, often with large portions of chlorids of calcium and magnesium, with or without sulphates. The carbonates of lime and magnesia are either wanting, or present only in small quantities. These waters are generally bitter to the taste, and may be designated as brines or bitterns.

II. Saline waters which differ from the last in containing besides the chlorids just mentioned, considerable quantities of carbonates of lime and magnesia. These waters generally contain much smaller proportions of earthy chlorids than the first class, and are hence less bitter to the taste.

III. Saline waters which contain, besides chlorid of sodium and the carbonates of lime and magnesia, a portion of carbonate of soda.

IV. Waters which differ from the last in containing but a small proportion of chlorid of sodium, and in which the carbonate of soda predominates. The waters of this class generally contain much less solid matter than the three previous classes, and have not a very marked taste until evaporated to a small volume, when they will be found, like the last, to be strongly alkaline.

Of these four classes, I corresponds to the division D, and IV to C, while II and III are regarded as resulting from the admixture of these in varying proportions. Sulphates are sometimes present in these waters, but never predominate; in their absence, salts of barium and strontium are often met with. The chlorids are generally, if not always, associated with bromids and iodids. Small quantities of potassium salts are also present, while borates, phosphates, silicates, and small portions of iron, manganese and alumina are generally present. These various waters are occasionally sulphurous, and those of the last three classes may be impregnated with carbonic acid.

V. The fifth class includes acid waters remarkable for containing a large proportion of free sulphuric acid, with sulphates of lime, magnesia, portions of iron, and alumina. These waters, which are characterized by their sour and styptic taste, generally contain some sulphuretted hydrogen.

VI. The sixth class includes some neutral saline waters, in which the sulphates of lime, magnesia, and the alkalies predominate, chlorids being present only in small quantities. These waters, like the last, are often impregnated with sulphuretted hydrogen.

The above classification, although adopted originally for the convenient description of the mineral waters of Canada, will, it is thought, be found to embrace all known classes of natural waters, with the exception of those included under E, and some waters from volcanic sources holding muriatic acid. These may constitute two additional classes. In the first three of the classes above described, chlorids predominate; in the fourth, carbonates; and in the fifth and sixth, sulphates. The waters of the first, second, and sixth classes are neutral; those of the third and fourth, alkaline; and those of the fifth acid.

The results of the chemical analysis of various waters of these classes it is proposed to give in the second part of this paper.

ART. XXIII.—*Abstract of a Memoir on Shooting Stars;* by H. A. NEWTON.

(Read before the National Academy of Sciences, August, 6th, 1864.)[1]

1. THE periodical shooting stars, particularly those of August and November, have hitherto very naturally attracted more attention than the sporadic ones, those seen on every clear night throughout the year. Yet these latter are objects of no small interest. There are methods of observing, and of computation, by which much can be learned about them, and observations already made are of use in determining something of their numbers, and of their place in the Solar System. I propose to combine these existing materials, and see to what they lead us. If it shall appear that I use rude processes and inexact data, and reject terms of considerable importance, I must plead that it is a step forward to do anything in this direction, and express the hope that better data will soon warrant the use of more refined processes. It will be necessary to assume certain propositions which are not probably strictly true, and others which may not be universally conceded. Such assumptions are however very distinctly set forth, that future observations may correct them if erroneous, and verify them if true. The details of many of the arguments are necessarily omitted in this abstract.

The table of altitudes of the paths of shooting stars published in the last volume of this Journal, p. 135, is the basis of the computations. Singly, these altitudes are liable to large probable

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[1] From the Proceedings of the Academy: published by permission.

errors; yet, taken together and used with caution, it is believed that they may be safely employed in a first approximation.

2. *Distribution vertically of the middle points of the luminous portions of the meteor-paths.*—From the table named, by rejecting the manifestly unreliable cases, and giving weight to the others, the following numbers are obtained to express the relative numbers of the shooting stars at different altitudes above the earth's surface. By altitude is meant the altitude of the middle point of the visible part of the path expressed in kilometers.

Between	0	and	30 kil.	39	sh. stars,
"	30	"	60	114	"
"	60	"	90	243	"
"	90	"	120	277	"
"	120	"	150	106	"
Between	150	and	180 kil.	57	sh. stars.
"	180	"	210	20	"
"	210	"	240	20	"
"	240	"	270	8	"
Above	270 kil.			12	"

There is evidently a definite upper limit to the region of meteor-paths. Again, the best observations give few if any very low altitudes. The altitudes less than 30 kilometers, and those greater than 180, are therefore disregarded.

I assume that these observed paths are fair examples as to altitude of all visible paths, and hence that the frequency of the middle points of all visible paths at different altitudes above the earth's surface is proportional to, and may be expressed by, these numbers, viz:

From	30	kilometers	to	60	by	114
"	60	"	"	90	"	243
"	90	"	"	120	"	277
"	120	"	"	150	"	106
"	150	"	"	180	"	57

Representing these numbers by ϱ, and the mean altitude of the paths by d, we have approximately,

$$d=\frac{\Sigma\varrho x}{\Sigma\varrho},$$

where in the finite summation indicated by Σ the successive values of ϱ are to be taken, and x is to be successively $\frac{1}{2}(30+60)$, $\frac{1}{2}(60+90)$, $\frac{1}{2}(90+120)$, &c. The value of d, that is, the mean altitude of meteor paths above the earth's surface, is thus found to be 95·55 kilometers, or not quite 60 miles.

3. *Distribution of the paths of Shooting Stars over the apparent heavens in azimuth.*—Theoretical considerations seem to require that the meteors should be nearly uniformly distributed in azimuth. Observations confirm this. Collating the observations of Mr. Herrick and others, as published in this Journal, and preserving those only in which the heavens were divided into four parts, we have of 6,598 observed paths, 800 in N., 733 in N.E., 965 in E., 852 in S.E., 847 in S., 833 in S.W., 889 in W., and 679 in N.W. These numbers imply a small predominance toward the southeast. Since the average zenith distance of the

middle points of paths is about 48°·3, the above numbers give as a center of gravity, or center of distribution, a point about 2° from the zenith in the direction of S. 28° E. We therefore conclude that the relative frequency of occurrence of meteor-paths in different parts of the visible heavens is in the main *a function of zenith distance only.*

4. *Distribution of meteor-paths over the sky in altitude.*—The law distribution of the paths in apparent altitude may be obtained directly by observations properly devised for that object. But such observations would have to be continued for a considerable time, and would involve great labor. I have therefore sought for a method of obtaining the approximate law of distribution from observations already made for other purposes.

If the zenith distances of a large number of paths seen by one observer were measured or computed, we should find them affected by his habits of watching. Thus, one who looked habitually to the zenith would see only those near that point, while one looking low down would see few near the zenith. But, combining the observations of a large number of persons, we might hope that many of these individual habitudes would counteract each other, and that the aggregate results would be affected only by common errors. I have therefore taken for this purpose various sets of observations made by about forty different persons. For a part the distances of the middle points of several paths from the zenith for the instant of appearance were computed. For the remainder the place of the zenith was in each case computed, and the zenith distance carefully measured on a very good sixteen-inch globe. The number of paths thus computed or measured was 1,393. Of these, 30 were within 10° of the zenith, 60 were between 10° and 20° from the zenith, 142 between 20° and 30°, &c., as in the second column of the following table:—

Table illustrating the distribution of meteor-paths over the sky.

Zenith distance.	No. of meteors.	Area of sky.	$\frac{\text{No.}}{\text{Area.}}$	Sec³θ.	$\frac{\text{No.}}{\text{Area sec}^3\theta.}$
0°–10°	30	·01519	1975	1·012	1951
10–20	60	·04512	1330	1·110	1198
20–30	142	·07366	1928	1·343	1436
30–40	197	·09999	1970	1·819	1083
40–50	274	·12325	2223	2·828	786
50–60	304	.14279	2129	5·299	402
60–70	245	·15798	1551	13·25	117
70–80	110	·16837	653	57·68	11
80–90	31	·17365	178	1510	0

If the area of the visible hemisphere is unity, the numbers in the third column give the areas of the corresponding zones. The number of paths divided by the areas gives quotients proportional to the numbers of stars at different altitudes to any unit of surface. These form the fourth column. They increase slowly to about 45°, and then rapidly diminish to the horizon.

The number of paths along an oblique line is greater than that along a perpendicular line. They should be very nearly as the cubes of the lengths of the lines, or, disregarding the curvature of the earth, as $\sec^3\theta : 1$. The cubes of the secants of 5°, 15°, 25°, &c., are given in the fifth column, these angles being taken as the mean zenith distances of the zones. In the sixth column are the quotients of the numbers in the fourth column divided by those in the fifth.

Under any reasonable law the numbers in the last column should diminish from the zenith. The deviations from this uniform diminution that appear in the table are due in part to the small number of paths measured. The first number represents a small area, and should perhaps be combined with the second. Upon the whole, there seems to be a deficiency near the zenith, which may be due to the tendency of observers to look habitually towards points from 30° to 60° high.

From inspection of this column, and from considerations detailed in the memoir, 1,800 is thought to be a reasonable number to assume as that to which the series approaches. The product $1{,}800 \times \sec^3 5° \times \cdot 01519 = 27\cdot 67$, would then give the number of shooting stars out of 1,393 that should be seen within 10° of the zenith. This is equivalent to one in 50·35. That is, *about one in fifty of all shooting stars seen at a place should have the middle points of their apparent paths within* 10° *of the zenith.*

5. *Number of shooting stars that come into the atmosphere each day.*—Shooting stars are seen in all countries, and any differences of number for different countries, thus far detected, may be easily explained by the personal equations of observers, or by differences in the clearness of the atmosphere. It will be assumed that for a given *considerable* period of time the meteors are equally abundant over all parts of the earth's surface.

Their frequency at different altitudes from the surface of the earth varies. If we suppose ϱ_1 to be the number of middle points of visible paths that fall in a given period of time into any cubic unit of the space of the region of meteor-paths, we should have ϱ_1 a function of the altitude above the earth's surface. Let x represent the altitude, and R the earth's radius. Suppose now an inverted cone, whose vertex is at the eye of the observer, whose axis is a vertical line, and whose semi-vertical angle is 10°. In general, shooting stars which have the middle points of luminous parts of their trajectories in this cone will have the middle of their apparent paths within 10° of the zenith. The number in this cone in the given time will be expressed very nearly by the formula,

$$\int_a^b \pi \varrho_1 \tan^2 10^\circ x^2 dx,$$

where a and b are the values of x for the lower and upper surfaces of the region of meteors.

On the other hand, the total number of shooting stars within the given period over the whole earth will be equal to

$$\int_a^b 4\pi\varrho_1 (\mathrm{R}+x)^2 dx.$$

The whole number visible at one place is 50·35 times the number seen within 10° of the zenith, and therefore 50·35 times the number within the above described cone. Hence if m is the number in a given period visible at one place, and N the number that would be visible (except for daylight, clouds, moon, &c.,) through the whole earth in the same period, we should have,

$$\mathrm{N} = \frac{m}{50{\cdot}35} \cdot \frac{\int_a^b 4\pi\varrho_1(\mathrm{R}+x)^2 dx}{\int_a^b \pi\varrho_1 \tan^2 10^\circ x^2 dx}.$$

Now ϱ_1 is a function of x, and, assuming as heretofore that the observed altitudes are fair examples of the real altitudes, we shall have $\varrho_1 = k\varrho$, where k is a constant depending on the period of time assumed, the unit of space assumed, and the abundance of meteors. As we have assumed an equable distribution over the earth's surface, this constant may be removed from the integral sign. Again, without great error finite summation may take the place of integration, in which case the equation easily reduces to the form,

$$\mathrm{N} = 2{\cdot}555\, m \left\{ \frac{\Sigma_a^b \varrho x^2 + 2\mathrm{R}\Sigma_a^b \varrho x + \mathrm{R}^2 \Sigma_a^b \varrho}{\Sigma_a^b \varrho x^2} \right\}.$$

Taking x successively 45, 75, 105, 135, and 165 kilometers, and ϱ also 114, 243, 277, 106, and 57, and R equal to 6370, we find

$$\mathrm{N} = 10{,}460\ m,$$

that is, *the number over the whole earth is to be considered as* 10,460 *times the number visible at one place.*

To obtain this result, it was assumed that the shooting stars were uniformly distributed over the earth's surface, and that the conditions of visibility are uniform. If, however, we regard the actual instead of the theoretical case, we find that the numbers seen vary through the hours of the night. The rapid diminution toward the horizon, already shown, indicates the great influence of mists, &c., in the air, in absorbing the light of these bodies. But for this cause, many more could be seen within ten degrees of the horizon than in the whole of the rest of the heavens; whereas of 1393 only 31 were actually seen in this part of the sky. These mists must of course vary in different climates. Hence the numbers visible in different places may reasonably be expected to differ. Let, however, a locality have an atmosphere of mean purity, let it in other respects be one of medium character with respect to the number of visible meteor-paths, and let m be the mean hourly number of shooting stars seen in a clear sky at that place: then we may consider that the whole number that under clear skies could be seen over the earth in one hour would be 10,460 m.

The value of m is of course to be determined by observation. The hourly number varies through the night. The value of m may be found from observations extending through the night, or from those made near midnight.

Mr. E. Bouvard, in the year between Oct., 1840, and Oct., 1841, observed at Paris on every clear moonless night. (*Comptes Rendus*, xiii, 1029). He always watched between 11 and 1 o'clock. During 74 hours and 22 minutes he saw 572 shooting stars. Allowing one-fourth of a minute for recording each meteor (which is the time estimated by him), and we have an average of 8 meteors per hour.

By what factor we must multiply the number seen by one observer to obtain the whole number visible at the place, we have no observations that I know of to determine. It is probable that this multiplier is as large as four, and that thirty is therefore not too large for the mean value of m.

This would give the average number of meteors that traverse the atmosphere daily, and that are large enough to be visible by the naked eye, if the sun, moon and clouds would permit, equal to 30 × 24 × 10,460, or *more than seven and a half millions.*

6. It will now be assumed that the phenomenon called a shooting or falling star is caused by a small body (probably a solid) which originally was moving in its own orbit in the solar system, or in space; that this body coming into the atmosphere of the earth elicits light by the loss of velocity, and is usually itself dissipated before reaching the earth's surface. The term *meteoroid* will be used to designate such a body before it enters the earth's atmosphere.

7. *Number of meteoroids in the space which the earth traverses.*—Suppose many small bodies to be distributed through an indefinite space, so that there shall be n bodies in a cubic unit. Suppose that these bodies have all an uniform velocity of v units per second in the same direction. Suppose a larger sphere whose radius is R, and which is without attraction, to be at rest in this space. The sphere intercepts in each second as many small bodies as are contained in a right cylinder, whose length is v, and whose radius is R, that is $\pi R^2 nv$ bodies.

If the sphere attracts the small bodies, it may be shown that instead of $\pi R^2 nv$ we should have $\pi R^2 nv\left(1+\frac{v_0^2}{v^2}\right)$, when v_0 is the velocity of a particle falling by the attraction of the sphere from rest at infinity, to the surface of the sphere. If the sphere has an uniform motion the same formula applies by making v the velocity relative to the sphere.

This result may be extended to several systems of small bodies. Let there be distributed through the indefinite space, in each unit of it, n' bodies of one system, n'' bodies of a second system, n''' bodies of a third system, and so forth, and let the bodies of the first system move in one direction with a velocity v' relative to the sphere, let the bodies of the second system move in another direction with a velocity v'' relative to the sphere, and so forth, then will the number of bodies which the sphere intercepts in each second be equal to

$$\pi R^2 n'v'\left(1+\frac{v_0^2}{v'^2}\right)+\pi R^2 n''v''\left(1+\frac{v_0^2}{v''^2}\right)+\pi R^2 n'''v'''\left(1+\frac{v_0^2}{v'''^2}\right)+\text{ etc.}$$

Call this N′ and we may write,

$$N'=\pi R^2\left(\Sigma n'v'+v_0^2\Sigma\frac{n'}{v'}\right),$$

where the summation indicated by Σ extends to all the systems of bodies.

If v is the mean value of v', v'', &c., for all the bodies and n is the sum of $n'+n''+n'''+$&c., then

$$\Sigma n'v'=n\text{v}.$$

The remaining term is the sum of fractions whose denominators vary. We may, however, write,

$$v_0^2\Sigma\frac{n'}{v'}=\frac{nv_0^2}{\text{v}}(1+\theta),$$

where θ is a number and is evidently positive. If the values of v', v'', v''', &c., do not vary widely, θ will be small. Making these substitutions, we have

$$N'=\frac{\pi n R^2}{\text{v}}(\text{v}^2+v_0^2+\theta v_0^2).$$

This formula expresses the number of meteoroids which the earth intercepts by considering the earth with its atmosphere as the supposed sphere, R its radius measured from the upper part of the region of meteor-paths, v the average velocity of the meteoroids when they come into the region of the earth's sensible attraction, v_0 the velocity of a body falling by the earth's attraction from infinity to a distance R from the centre of the earth, N′ the average number of meteoroids coming into the earth's atmosphere in a second, and n the average number in a cubic unit of the space the earth is traversing in a given period.

If m be the average number visible at one place in a unit of time we have found above that $N=10{,}460\,m$. The volume of a sphere whose radius is R is $\frac{4}{3}\pi R^3$. Let M be the number of meteoroids in a space equal to such a sphere, then $M=\frac{4}{3}\pi n R^3$, and

$$10460\,m=\tfrac{3}{4}\frac{M}{vR}(v^2+v_0^2+\theta v_0^2),$$

or

$$M=\tfrac{4}{3}\cdot\frac{10460\,m\,Rv}{v^2+v_0^2+\theta v_0^2},$$

where m denotes the average number (or fraction of a number) seen in one place per second. If the hourly number is, as before assumed, equal to 30, then $m=\frac{1}{120}$, and $M=116{\cdot}2\,\frac{Rv}{v^2+v_0^2(1+\theta)}$.

8. *The mean length of apparent paths.*—I have computed or measured the lengths of 1016 paths, observed by a large number of persons. The mean length is found to be 12°·6.

9. *Numbers of Telescopic shooting stars.*—Shooting stars are of all degrees of brilliancy, and there are many very faint ones. Almost every hour that a person watches, he sees, or thinks he sees, flights that are yet so faint as to leave him in doubt whether they are shooting stars, or only illusions. We may therefore reasonably conclude that large numbers of shooting-stars are entirely invisible to the naked eye, which yet might be seen by the telescope.

This conclusion is verified by observation. In 1854, Messrs. Pape and Winnecke observed[2] together at Göttingen for 32 hours, on nights between the 24th of July and the 3d of August. Pape saw with the naked eye 312 shooting stars, and Winnecke saw 45 in the same time with a comet seeker. The diameter of the field of view is not given, but in observations at the same time diameters of 53′ and 36′, with powers of 30 and 60, were used.

If the apparent length of a meteor-path is l and the breadth of the field of view of a telescope is b, and if the axis of the telescope is directed towards any part of the area whose length is l, and whose breadth is b, the meteor-path would cross the field of view. If all paths were of the same length, b, and were equally distributed over the heavens, a telescope would then command a portion of the whole heavens expressed by the fraction $lb \div$ *surface of sky*. Meteor-paths diminish somewhat in length with apparent brightness. On the other hand a path may perhaps be longer when viewed by aid of the telescope than when seen by the naked eye. Hence for the approximate mean value of l may be taken 12°·6, the mean value of the length of the apparent meteor-paths visible to the naked eye. Let b be 53′

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[2] Astron. Nachrichten, xxxix, 113.

and the ratio of those actually seen through one telescope to those which are bright enough to be visible in it is $53 \times 12^\circ{\cdot}6 : 360 \times 60 \times 180 \div \pi$, or, $1:1853$. I have selected the larger diameter of the telescope that the ratio may not be too large. For the same reason I prefer to reject in the divisor that part of the surface of the heavens within 15° of the horizon. This makes the ratio $1 \div 1371$.

We have seen that according to Bouvard's observations one person should see an average of eight shooting stars per hour. Hence if $\frac{45}{312}$ is taken as the ratio of the number seen in a comet seeker to those seen by one person with the naked eye, there should be in each hour $\frac{8 \times 45 \times 1371}{312}$, or 1582 shooting stars hourly that might be visible through a comet seeker if the whole heavens could be watched. The ratio between those visible at one place and those visible somewhere over the whole earth is $1 \div 10{,}460$ for common meteors. If the same ratio applies to telescopic shooting stars (doubtless not a very reasonable supposition), we have for the whole number of meteoroids coming daily into the air, at least, $1582 \times 24 \times 10460$, or more than 400,000,000. There is, moreover, no reason to doubt that a farther increase of optical power would reveal still larger numbers of these small bodies.

10. *Mean distance of the meteor-paths from an observer.*—Although the data in our possession are inadequate to furnish the mean distance of the meteor-paths from an observer, yet limits to this mean distance can be obtained. Not so large a portion of the meteor-paths along an oblique line can be seen as along a vertical line. Now from the distribution of the paths in absolute altitude, and their distribution over the apparent heavens, can be computed the mean distance on either of two suppositions:

1st, That those paths along the oblique line that become invisible are always the most distant ones;

2d, That the paths which become invisible are distributed along the oblique line in proportion to the numbers along the line.

It is evident that the first supposition affords a mean distance less than the truth, and that the second affords one that is too great. These two limits are found to be about 232 and 140 kilometers. It must, however, be admitted that in this computation the unavoidable errors arising from using finite summation for integration, and from other sources, are considerable.

11. *Mean foreshortening of the meteor-paths by perspective.*—To determine the effect of perspective in shortening the apparent paths we have need to use the following geometrical proposition:

If a sphere whose radius is a be supposed to have an indefinite number of diameters, and the extremities of these diameters are distributed uniformly over the surface of the sphere, and if O be a point without the sphere whose distance from the center of the sphere is b, then will the mean value of the angles at O, subtended by all these diameters, be equal to $\frac{\pi}{2}\frac{a}{b}$.

The mean effect of the foreshortening by perspective may therefore be thus expressed. Let a diameter of the sphere be bent into the arc of a circle whose radius is b; then the angle it thus measures, is to the mean value of the angles at O, as any square, is to its inscribed circle.

This effect is independent of the ratio $a : b$, except that it must be less than unity. Hence the proposition may be applied to any number of short lines viewed by an observer, if the directions of the lines be properly distributed.

If shooting stars came directly downward we should see all that were coming toward us, since they would be near the zenith. We should see few, if any, whose paths are at right angles to the line of vision, for those would be down near the horizon, and would be concealed by mists and smoke. It seems probable that in this case the mean effect of foreshortening would be a little greater than that of the diameters of a sphere.

Again, if the paths were all parallel to the horizon we should see an undue number moving nearly at right angles to the line of vision. For those which are diminished most by the foreshortening would be near the horizon, and hence mostly hidden.

But the directions of the meteor-paths are from all parts of the heavens from horizon to zenith. It seems reasonable to conclude that the mean effect of foreshortening is intermediate also between that for paths coming from points in the horizon, and that for paths coming from the zenith. Hence it ought to be nearly represented by the diameters of the sphere.

The conclusion may be thus expressed. The mean length of the observed paths of shooting stars is found to be $12^\circ{\cdot}6$. If every path was supposed to be turned about its middle point, and bent into an arc of a circle of which the observer's eye was the center, the mean apparent length would be increased, in the ratio of a square to its inscribed circle; that is, it would be equal to $12^\circ{\cdot}6 \times \frac{4}{\pi}$, or about $16^\circ{\cdot}04$.

12. *Mean length of the visible part of meteor-paths.*—From the mean distance and mean angular length of the meteor-paths can be computed approximately their mean lengths. For if l be the mean length, and b the mean distance, we have

$$\frac{l}{b} = \frac{\pi \times 16^\circ{\cdot}04}{180^\circ}, \quad \text{or} \quad l = 0{\cdot}28b.$$

The two limits for the mean distance given above indicate that the mean length is less than 65, and greater than 39, kilometers.

These results should, however, be diminished a little, since the *mean* distance and *mean* angular length give too large a *mean* length. The smaller of the two limits given is doubtless much nearer the truth.

13. *Mean duration of flight, and the mean velocity of the shooting stars.*—Mr. Wartmann of Geneva gives the duration of the flights of 368 shooting stars observed at Geneva during one night by six observers. The aggregate is $180^{s}\cdot33$, which gives a mean of $0^{s}\cdot49$ for each flight. The mean of 499 estimates made in August and November last is $0^{s}\cdot418$. The mean duration of the 867 flights is $0^{s}\cdot45$.

If the estimates of those observers who were accustomed to astronomical observation, and hence to judging of small intervals of time, had been alone taken, the result would have been very nearly the same. Almost all observers agree that the mean duration of flight is not greater than half a second.

A mean duration of half a second, and a mean length of path between 39 and 65 kilometers seem to imply a mean velocity between 78 and 130 kilometers per second. The smallest of these (more than forty-eight miles,) is twice and a half the velocity of the earth in its orbit about the sun. This cannot consist with the supposition that most of the meteoroids move in closed orbits about the sun. Hence, we must accept as highly probable one or more of these three conclusions, viz:—

1st, That the length of track is too long; which seems to involve that the altitudes on which our computation is based are, on the whole, too large. All the altitudes greater than 150 kilometers, might, I think, have been safely rejected.

2d, That the estimates of time are, in general, too small. This is quite probable. The mind may not make proper allowance for the time that elapses after the shooting star is seen, before the eye is directed to the place of the path.

3d, That very many of the meteors move in hyperbolic orbits about the sun. Whatever may be said of the sporadic meteors, this cannot be true of the members of the August and November groups.

14. *The sporadic shooting stars cannot all belong to one narrow ring which has a diameter nearly equal to that of the earth's orbit.*—Such a ring would have to be but little inclined to the ecliptic in order to furnish meteors throughout the year. The bodies could not have a retrograde motion, else shooting stars would be seen only in the morning hours, and would, moreover, have a very distinctly marked radiant.

They cannot have a direct motion. For their velocity must then be nearly equal to, and yet a little less than, that of the earth, in order that more be seen in the morning than in the evening. Their relative velocity on entering the atmosphere would be not much greater than that of a body falling to the earth from an infinite distance—that is, not much greater than 11 kilometers per second. So small a mean velocity is entirely inconsistent with direct observations, as well as the conclusions given above.

We might, it is true, suppose the ring to have a considerable breadth, in which case the meteors would have a larger mean relative velocity. But if the breadth be such as to furnish a velocity at all consistent with observation, we have no longer a ring lying between the orbits of Mars and Venus, but a disk extending much beyond these planets.

15. *A large portion of the meteoroids must, when they meet the earth, have absolute velocities greater than the earth's velocity in its orbit; or else, the sporadic meteors have a series of radiants at some distance from the ecliptic, and hence come from a series of rings considerably inclined to the earth's orbit.*—For shooting stars cannot have relative motions upward from the earth's surface when they enter the atmosphere. If then the absolute velocities of the meteoroids were all less than those of the earth, their relative motions (neglecting the earth's attractions,) would all be from points of the heavens less than 90° from that point toward which the earth is moving. To an observer early in the evening but a small part of this hemisphere is above the horizon, while in the morning almost all of the hemisphere is visible. Hence, either the number of those seen in the morning should be *very much greater* than that of those seen in the evening, or else there should be a radiant in that part of the sky which is above the horizon in the earlier hours of the night. If the earth's attraction be considered the disproportion between the morning and evening hours would be slightly diminished. But even with this allowance the increase through the night would be greater than the observed increase, unless one of the two suppositions above given is true.

16. *Distribution of the orbits of the meteoroids in the solar system.*—There are at least three suppositions respecting the distribution of the orbits of the meteoroids in the solar system which are naturally suggested. Either of them may be considered as plausible, and one does not exclude another.

1st. They may form a number of rings, like the August group, cutting or passing near the earth's orbit at many points along its circuit. The sporadic shooting stars may be outliers of such rings.

2d, They may form a disk in or near the plane of the orbits of the planets.

3d. They may be distributed at random, like the orbits of the comets.

According to the first of these suppositions there should be a succession of radiants corresponding to the several rings. Dr.

Heis and Mr. R. P. Greg believe that they have detected such a series. Continued observation directed to this end will probably decide whether the meteoroids belong entirely, or mostly, to rings.

According to the second supposition the apparent paths of the sporadic meteors should, if produced in the direction of the motion, all cut the ecliptic below the horizon. For the absolute motion of a meteoroid and the earth's motion being both in, or near, the ecliptic, the resultant relative motion should be from some point in the ecliptic. That point should be above the horizon, since the body must move downward to enter the atmosphere. And as the apparent path produced backward cuts this point, it will, if produced forward, cut the ecliptic below the horizon.

We have thus a simple means of determining whether the sporadic meteors come exclusively, or even largely, from a disk or a lenticular shaped group about the sun, like that which the zodiacal light is often supposed to indicate.

If the third supposition is true, the mean velocity of the meteors is a function of the numbers of shooting stars in the different hours of the night. For, if the velocity is very small, few would be seen in the evening, while if it is very large, there should be nearly as many in the evening as in the morning.

Consider a small spherical space in the upper region of the earth's atmosphere as receiving the meteors.

By supposition the absolute motions of the meteoroids are directed in equal numbers from all parts of the celestial sphere. If their velocities are all equal and represented by v', the earth's velocity being v, if N_0 represents the numbers of those whose absolute motions in a given period are from the visible celestial hemisphere, n the whole number that should come from all parts of the celestial sphere, and θ the distance from the zenith to that point of the heavens to which the earth is moving, then we find between these quantities the equation,[*]

$$N_0 = \frac{n}{2}\left(1 + \frac{v}{v'}\cos\alpha\right).$$

Let l be the latitude of an observer, and h the hour-angle counting from the time when the point toward which the earth is moving is on the meridian, then when that point is on the equator we have $\cos\alpha = \cos l \cos h$. This will correspond to the mean of the year. Computing now the values of $N_0 \div n$, on the several suppositions $v' = v\sqrt{2}$, $v' = v$, and $v' = \frac{4}{5}v$, for the latitudes of New Haven, and of Paris we have the following table. When $v' = v$ we have orbits whose major axes equal those of the earth. When $v' = v\sqrt{2}$ we have parabolic orbits.

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[*] After reading this memoir, I found that Mr. A. S. Herschel had obtained the same formula.

Table showing the hypothetical distribution of the shooting stars through the hours of the night.

Hour of the night.	New Haven.			Paris.		
	$v'=v\sqrt{2}$	$v'=v$	$v'=\cdot8v$	$v'=v\sqrt{2}$	$v'=v$	$v'=\cdot8v$
6	·765	·875	·970	·732	·829	·911
7	·756	·862	·954	·725	·818	·897
8	·730	·825	·907	·701	·785	·856
9	·687	·765	·832	·664	·732	·791
10	·633	·688	·735	·616	·665	·706
11	·569	·597	·622	·560	·585	·606
12	·500	·500	·500	·500	·500	·500
1	·431	·403	·378	·440	·415	·394
2	·367	·312	·265	·384	·335	·294
3	·313	·235	·168	·336	·268	·209
4	·270	·175	·093	·299	·215	·144
5	·244	·138	·046	·275	·182	.103
6	·235	·125	·030	·268	·171	·089

The velocities have been considered as uniform. But it is evident that if the shooting stars have various velocities, the hourly distribution should correspond approximately to that for the mean velocity.

The number entering a spherical space has been considered. In fact we have to consider the number entering the region of the atmosphere commanded by observers at one place. The two are not identical. For if so, we ought, on the nights of 9–12th of August to have the conformable meteors equally distributed through those hours of the night in which the radiant is above the horizon. But there is in fact an increase of numbers till dawn. Hence the difference between the numbers seen in morning hours, and those seen in the evening hours, should be greater than is given by the preceding table.

On the other hand, the effect of the earth's attraction is to render the numbers a little more uniform through the night. Again, there should be less difference between the morning and evening during the half year from July to December, than in the other half year, for the point to which the earth is then moving is north of the equator.

Mr. Herrick estimated that there were about three times as many shooting stars in the morning as in the evening. Mr. Coulvier-Gravier gives the following[4] as the mean hourly numbers of shooting stars at Paris :—

5^h—6^h	7·2	10^h—11^h	8·0	3^h—4^h	15·6
6 —7	6·5	11 —12	9·5	4 —5	13·8
7 —8	7·0	12 — 1	10·7	5 —6	13·7
8 —9	6·3	1 — 2	13·1	6 —7	13·0
9 —10	7·9	2 — 3	16·8		

How much reliance is to be placed on these numbers of the Paris observer we know not.

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[4] Recherches sur les météores, p. 220.

This estimate, and these numbers, taken strictly imply a mean velocity greater than that of a parabolic orbit. The character of the data does not allow the argument to be pressed. Yet we must regard as almost certain (on the hypothesis of an equable distribution of the directions of absolute motions), that the mean velocity of the meteoroids exceeds considerably that of the earth; that the orbits are not approximately circular, but resemble more the orbits of the comets.

17. *Number of meteoroids in the space which the earth is traversing.*—We have found that, of the space through which the earth is moving, a volume equal to that of the earth (atmosphere included), contains a mean number of meteoroids expressed by the equation

$$\mathrm{M} = 116{\cdot}2\,\frac{\mathrm{R}\,\mathrm{v}}{\mathrm{v}^2 + v_0^{\,2}(1+\theta)}.$$

In this equation v is the mean relative velocity of the meteoroids. If their absolute velocities were equal, and the points from which they come uniformly distributed over the heavens, we should evidently have

$$\mathrm{v} = \frac{1}{2}\int_0^{\pi} (v^2 + v'^2 - 2vv'\cos\omega)^{\frac{1}{2}} \sin\omega\, d\omega = v + \frac{v'^2}{3v}.$$

For v' we may use as an approximation the mean absolute velocity. If $v = v'$, then $\mathrm{v} = \frac{4}{3}v$. If $v' = v\sqrt{2}$, then $\mathrm{v} = \frac{5}{3}v$. As the velocity seems to be greater than that of the earth, and more nearly equal to that of bodies moving in parabolic orbits, the latter value is preferred for v. On this supposition, and by neglecting θ, we find M greater than 14,000. If allowance be made for the space occupied by the earth's atmosphere we find that, in the mean, *in each volume of the size of the earth, of the space which the earth is traversing in its orbit about the sun, there are as many as* 13,000 *small bodies, each body such as would furnish a shooting star visible under favorable circumstances to the naked eye.* If telescopic meteors be counted, this number should be increased at least forty-fold.

There seems to be little reason for supposing that the space near the earth's orbit is very much more thickly strewn with these bodies than other parts of the solar system. That they are grouped according to some law, is altogether probable. But a velocity different from that of the earth implies, of necessity, that they are not grouped closely about the earth's orbit.

These bodies cannot be regarded as the fragments of former worlds. They are rather the materials from which the worlds are forming. If astronomy furnishes any measure of their total mass we may therefrom obtain some idea, rude though it be, of the mean mass of the individuals.

ART. XXIV.—*Action of Binoxyd of Lead and Sulphuric Acid on Hippuric Acid;* by DR. JULIUS MAIER, Chemical Assistant, School of Mines, Columbia College.

Hipparaffin, $\text{Є}_8H_7N\Theta$.

HIPPURIC acid, at a moderate temperature, is treated with binoxyd of lead and sulphuric acid, care being taken to avoid an excess of either. A strong evolution of carbonic acid ensues, and the mass congeals to a thick paste. This is placed on a filter, and washed with a dilute soda solution till the wash-water shows no further reaction of sulphuric acid. This mass is treated with alcohol, which dissolves the hipparaffin, and evaporated in a water-bath to one-half its volume. On cooling, the entire liquid congeals to a crystalline mass. The separated hipparaffin is collected on a filter, and washed with boiling water, until all acid reaction has disappeared, and is then redissolved in hot alcohol, and allowed to crystallize. Hipparaffin forms a network of white silk-like needles, which melt at 210° C., subliming previously at 100° C. They are entirely insoluble in water, that reagent not even causing them to become moist; they are readily soluble in alcohol and ether. Hipparaffin owes its name to the fact of its not being perceptibly decomposed by acids or alkalies. It is not soluble in solutions of potassa, and develops ammonia only after continued fusing with caustic potassa; it is easily dissolved by a mixture of sulphuric and nitric acids, and is not separated from this solution by water. Fuming nitric acid dissolves it with liberation of gas. This solution, after neutralization with carbonate of soda and evaporation, yields benzoic acid on addition of chlorhydric acid. Upon being heated in a stream of chlorhydric acid gas, a colorless oil passes over, which congeals to a crystalline mass. Schwarz,[1] who first described hipparaffin gave it the incorrect formula $C_{16}H_8NO_2$. I found its composition to be $\text{Є}_8H_7N\Theta$, the formation of which is explained as follows:

$$\overset{\text{Hippuric acid.}}{\text{Є}_8H_9N\Theta_3}+\overset{\text{Binoxyd lead.}}{Pb_2\Theta_2}+\overset{\text{Sulphuric acid.}}{S_2H_2\Theta_4}=\overset{\text{Hipparaffin.}}{\text{Є}_8H_7N\Theta}+\overset{\text{Carbonic acid.}}{\text{Є}H_2\Theta_3}+\overset{\text{Sulphate of lead.}}{S_2Pb_2\Theta_4}+\overset{\text{Water.}}{H_2\Theta}$$

Hipparin, $\text{Є}_8H_9N\Theta_2$.

When an excess of sulphuric acid is used in the preparation of hipparaffin, an oily liquid is separated which rises to the surface of the alcoholic solution, and on cooling congeals to a crystalline mass, on the addition of water. This mass dissolves in hot water, and crystallizes in long, white, glassy needles, grouped in the shape of fans. These needles melt at 49° C., and become

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[1] Schwarz Annal., lxxv, 201.

solid at 20° C. They are soluble in hot water, alcohol and ether. Their formation may be explained as follows:

$$\overset{\text{Hippuric acid.}}{\text{Є}_9H_9N\Theta_3}+\overset{\text{Binoxyd of lead.}}{Pb_2\Theta_2}+\overset{\text{Sulphuric acid.}}{S_2H_2\Theta_4}=\overset{\text{Hipparin.}}{\text{Є}_8H_9N\Theta_2}+\overset{\text{Carbonic acid.}}{\text{Є}H_2\Theta_3}+\overset{\text{Sulphate of lead.}}{S_2Pb_2\Theta_4}$$

Hippuric and Benzoic acids are entirely decomposed by heating them with an excess of superoxyd of lead and sulphuric acid, all carbon being driven off in the form of carbonic acid.

The following are the products of the decomposition of hippuric acid by binoxyd of lead and sulphuric acid:

1. $\text{Є}_9H_9N\Theta_3+Pb_2\Theta_2+S_2H_2\Theta_4=\text{Є}_8H_9N\Theta_2+\text{Є}H_2\Theta_3+S_2Pb_2\Theta_4$
2. $\text{Є}_9H_9N\Theta_3+Pb_2\Theta_2+S_2H_2\Theta_4=\text{Є}_8H_7N\Theta+\text{Є}H_2\Theta_3+S_2Pb_2\Theta_4+H_2\Theta$
3. $\underset{\text{Hippuric acid.}}{\text{Є}_9H_9N\Theta_3}+\underset{\text{Binoxyd lead.}}{Pb_6\Theta_6}+\underset{\text{Sulphuric acid.}}{S_6H_6\Theta_{12}}=\underset{\text{Benzamid.}}{\text{Є}_7H_7N\Theta}+\underset{\text{Carbonic acid.}}{\text{Є}_2H_4\Theta_6}+\underset{\text{Sulphate of lead.}}{S_6Pb_6\Theta_{12}}+\underset{\text{Water.}}{H_4\Theta_2}$

Action of Bromine and Iodine on Hippuric Acid.

Bromohippuric acid, $\text{Є}_9H_8BrN\Theta_3$.

An alcoholic solution of hippuric acid, at a boiling heat, is treated with bromine; the solution is allowed to boil a few minutes and water added, and then evaporated on a water-bath to one-half its volume. The yellowish brown color of the solution gradually disappears, and, on cooling, a white crystalline deposit is formed, which consists of fine needles plainly visible under the microscope. These needles are difficultly soluble in water, alcohol or ether, when cold, but readily soluble in all of these media upon boiling. They are colorless; when moistened and allowed to stand for a short time at an ordinary temperature, they undergo decomposition and turn brown, owing to the separation of bromine. The aqueous solution has a strongly acid reaction, and is not precipitated by lime, baryta, or the salts of copper. Upon being heated with caustic lime, the crystals emit a distinct odor of benzonitril, and a violet colored oily liquid was formed, previous to which benzoic acid having a red color passed over. The crystals dried in vacuo and burned with oxyd of copper, and finally in a stream of oxygen, gave the following results:

0·2735 grms. gave 0·4160 grms. carbonic acid and 0·083 grms. water.
0·2760 grms. burned with caustic lime, gave 0·1995 grms. bromid of silver.
0·6855 grms. burned with soda-lime, gave 0·0405 grms. ammonia.

		Calculated.	Found.		
			I.	II.	III.
9. Carbon,	108	41·86	41·50	....	....
8. Hydrogen,	8	3·10	3·40	....	....
1. Bromine,	80	31·00	....	30·91	....
1. Nitrogen,	14	5·43	....	....	4·86
3. Oxygen,	48	18·61	....	....	....
		100·00			

The salts of potash and soda could not be obtained in crystals. The salt of lime, $\text{Є}_9H_7CaBrN\Theta_3$, crystallizes in fine white needles, is difficultly soluble in cold water, readily so in hot water; dried between 90° and 100° C. it was not decomposed. The analysis gave the following result: 0·3835 grms. of the dried crystals gave 0·0665 grms. carbonate of lime, the formula as calculated requiring 0·0692 grams.

Iodohippuric acid, $\text{Є}_9H_8IN\Theta_3$.

This acid was obtained in the same manner as the bromine compound, employing iodine instead of bromine. To the alcoholic solution water was added, and it was filtered from the separated iodine; the filtrate was allowed to stand over night. The solution which was at first brown became colorless, and fine needles were deposited. These are white, odorless, difficultly soluble in water, alcohol and ether, in the cold, but readily so in all of them upon boiling. They are decomposed at 90° C., turning yellow, owing to the separation of iodine. All the salts with the exception of that of silver are soluble in water.

The crystals, when burned with oxyd of copper, and finally in a stream of oxygen, gave the following results:

0·2430 grms. gave 0·3080 grms. carbonic acid and 0·0605 grms. water.
0·2080 grms. gave 0·1590 grms. iodid of silver, when burned with caustic lime.
0·4760 grms. burned with soda-lime gave 0·0240 grms. ammonia.

		Calculated.	Found.		
			I.	II.	III.
9. Carbon,	108	35·41	34·57	....	....
8. Hydrogen,	8	2·62	2·76	....	....
1. Iodine,	127	41·64	....	41·30	....
1. Nitrogen,	14	4·59	....	....	4·16
3. Oxygen,	48	15·74	....	....	....
		100·00			

New York, Dec. 1st, 1864.

ART. XXV.—*On the Preparation of Oxalate of Ethyl*; by M. CAREY LEA, Philadelphia.

THE processes described in the text books for the preparation of oxalate of ethyl are liable to objection. If we heat binoxalate of potash with sulphuric acid and alcohol, we introduce into our distillate the decomposition-products of sulphuric acid in addition to those of oxalic acid and alcohol. It has also been recommended[1] to heat oxalic acid in a retort until it begins to

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[1] Gmelin Handbook, Cav. ed., ix, 179.

emit white fumes, and then allow alcohol to drop in through the tubulure. As the temperature at which the acid begins to sublime is considerably below that at which it melts, this process is apt to result in breaking the retort.

I was therefore led to seek for a better process, and have used the following repeatedly, with very satisfactory results.

A tube of an inch or more in diameter, and of a proper length, is drawn out at one extremity, and at the same time bent to an angle, somewhat as in the ordinary form of a combustion tube, except that the bent part is made less slender, and shorter ($\frac{1}{4}$ inch diameter at extremity). The tube is then completely filled with dehydrated oxalic acid, except for the last two inches at the open end. This is closed with a cork penetrated by a tube. For this purpose the cork is not bored parallel with its axis, but in an inclined direction, so that the smaller tube when inserted two or three inches may touch the side of the larger. This last is then supported in a horizontal position over a gas combustion furnace, so that the bent end may be directed upwards, and the cork turned so that the small tube may touch the upper side of the interior of the large one. The tube should be supported about six inches above the wire gauze of the furnace. The bent end of the tube is now connected with a Liebig's condenser, and the small tube with a flask containing alcohol of 42 B. The furnace is then lighted, and as soon as the acid melts, a stream of alcohol vapor is sent over it by heating the flask. The gas-furnace is maintained at a heat such that a very gentle ebullition shall be kept up in the tube. At the end of an hour to an hour and a quarter the operation is terminated, and the oxalic acid in the tube is converted into oxalic ether. The distillate also contains a little oxalic ether which cannot be separated by water, but which may be used for preparing oxamid. Or the distillate may be gently evaporated to one-tenth its bulk, and then may be mixed with a little water.

This process gives an abundant product, and requires no attention after the proper temperature is once adjusted. In filling the tube with the oxalic acid, it is not necessary to leave an air passage; the material settles in fusing sufficiently without. The oxalate of ethyl, which is a little brownish, and contains the usual impurities of the crude product, may then be purified in the ordinary manner.

ART. XXVI.—*Method of applying the binocular principle to the eye-piece of a Microscope or Telescope;* by ROBERT B. TOLLES.

To apply the binocular principle to the eye-piece of a microscope or telescope, it is only necessary to make use of the erecting form of eye-piece, and to place the dividing prism at the point where the pencils composing the whole bundle of rays proceeding from the object cross in the eye-piece, which is the point where, in any erecting eye-piece, the diaphragm proper is correctly placed.

If the theory of the erecting eye-piece of common form were generally understood, no demonstration that binocularity can be given to such an eye-piece would be necessary.

Suffice it to say that, since any one pencil of light proceeding from any point of the object through the *whole area* of the object-glass *does* at this point equally fill the whole area of the diaphragm (that being of proper aperture) substantially in the same manner, therefore the division for binocular vision, if made here by the appropriate prism, must be a very nearly equal division of every particular pencil, and give a similar and satisfactory image of the entire field in each eye-tube. This is a sufficient expression of the whole theory of the binocular eye-piece.

It is, however, important in order to avoid pseudoscopic effects, to adopt the proper form of dividing prism; and this form is precisely that best suited to that kind of binocular microscope in which the dividing prism is placed immediately above the objective. The natural presumption has been—contrary to this—that prisms of rectangular form would give the proper effects in the eye-piece, because of the pseudoscopic effects produced by their use in the microscope of binocular body. But this is an error, inasmuch as the pencils proceeding to form the second image in the erecting eye-piece reach the small dividing prism under conditions suitable for correct vision of the object *were the eye placed there,* and, accordingly, the same false appearances obtain with the eye-piece of rectangular prisms, *having oculars above,* as if such division were made immediately above the objective; the effect being, that the order in which the rays proceeding from the sides of the object or image viewed reach the eyes, as to right and left, is reversed from that which exists in natural vision; the left eye receiving a preponderating portion from the right side, and the right from the left side of the object.

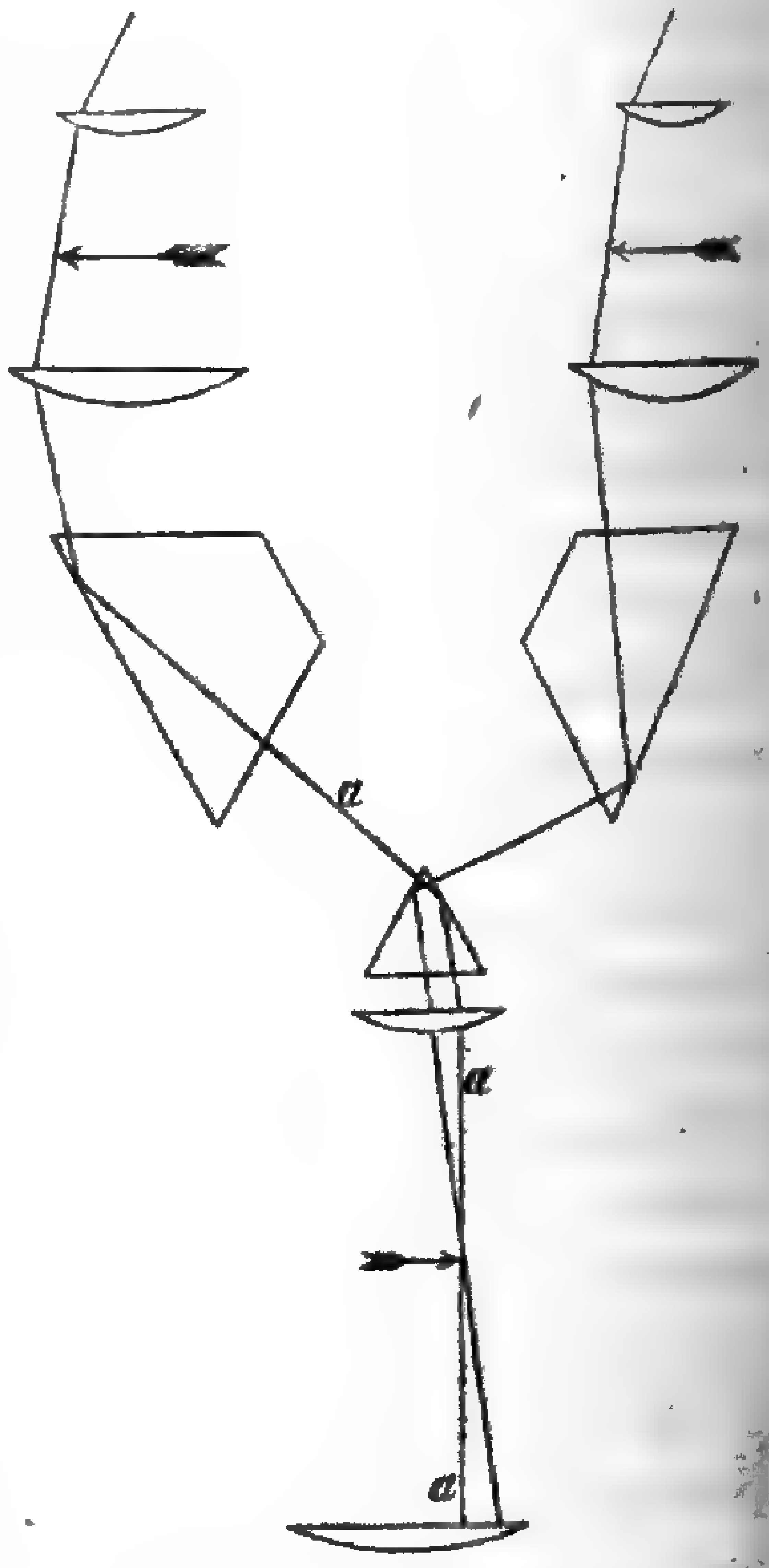
It is to be noted, however, that the eye-piece with rectangular prisms, arranged after the first method of Prof. Riddell, does not uniformly produce conversion of relief, or that inversion of perspective which obtained in that first experimental arrangement for a binocular microscope. Such a *binocular eye-piece* used in the microscope upon transparent objects only occasionally gives the view in depth thus inverted. With low powers, and considerable thickness of the transparent object, the view is usually pseudoscopic. With medium and higher powers, it is otherwise; and the effect is much controlled in this respect by the direction of the light upon the object.

When the binocular eye-piece formed with rectangular prisms is used in the telescope to view a landscape, the perspective is not *throughout* inverted, but portions of the field appear interposed between the eye and nearer objects in a singular and somewhat startling manner.

By arranging the compound rectangular dividing prism so that the optical pencil is divided in the *plane of vision*, instead of vertically, the pseudoscopic effect is almost entirely obviated.

In constructing the binocular eye-piece, the prisms and arrangement of Nachet have been found to answer every condition and requisite of binocular vision. The dividing prism being placed, as before stated, at the point of crossing of pencils in the erecting eye-piece, each pencil of light will enter the small dividing prism and impinge upon its reflecting surfaces in a manner similar to that illustrated in the Nachet binocular microscope. The binocular eye-piece has greatly the advantage over the other arrangement. For when the prisms are placed *in the binocular body immediately above the objective*, their position, in order to secure a *proper division* of each transmitted pencil, should change with every change of objective used—which can be easily provided for in the case of low powers but is rather impracticable with the higher numbers, it being very difficult to bring the prisms sufficiently near to the posterior combination of the objective. On the contrary, when the binocular arrangement is embodied *in the eye-piece*, the prism being once fixed in proper position, as before described, is correctly placed for every power of objective, and the eye-piece, thus binocular in form, is as applicable through the whole range of powers as if it were monocular. Applied to high powers, only one condition would be distinguishingly critical in the case of the eye-piece,—that of the centricity of the central prism. The form of *erecting eye-piece* found most advantageous in this binocular adaptation is a duplication of the ordinary Huyghenian negative eye-piece, wherein the small dividing prism is very nearly at the eye-hole point of such a negative eye-piece as is ordinarily applied in the monocular microscope. At a proper distance above this is placed another negative eye-piece, in which is formed a second image of the object viewed.

This form of erecting eye-piece gives less extension above the body of the microscope than the positive form, and for that reason is preferred.



The annexed diagram illustrates the division of one lateral pencil proceeding from one point of the first image formed in the eye-piece, and its general course to emergence at the two eye surfaces of the eye-piece. When the eye-piece is constructed of the form outlined in the annexed diagram, (which is about half size in the superior portion), the field is produced very satisfactorily, and of tolerable expansion; and does not necessitate more than 4·50 inches extension beyond the microscope body. The draw-tube can be as well withdrawn, and the eye-piece occupy its place, thus diminishing somewhat the total extent of the instrument. With proper modifications of the system of lenses placed before the prisms in the eye-piece, the whole binocular arrangement can be brought still nearer the objective, and retain also all the characteristics of the binocular eye-piece as contradistinguished from the binocular microscope known and in use.

The objection that loss of light must occur on account of the additional front system of lenses pertaining to an erecting eye-piece, (the lower system in the diagram,) of course militates against this arrangement; but there are, on the other hand, incidental advantages in the use of the erecting form. And moreover, if desirable, the "eye-lens" of the lower system can be successfully united with the dividing prism, thus eliminating two surfaces; and it is practicable to make the field-lens of each upper system solid with the opposite prism. The binocular erecting microscope would thus have no more refracting surfaces than the instrument of negative form and binocular body. As the object is, for the sake of efficiency with a high power objective, to give as large area to the transmitted pencil as possible at the point where it undergoes division in the small prism, therefore the power of the front system should be kept down, and amplification, as far as necessary in the eye-piece, be produced after the division has taken place. The accompanying cut is in this respect not in strict proportion—the lower system of lenses being represented not more than half the distance apart that the most favorable arrangement for use with high powers (and equally with low) would call for, and the standard length of microscope body admit of. But the aperture of the lenses might remain about the same.

Having obtained by this means a pencil (or beam rather) transmitted through the eye-piece of the greatest possible dimension or area, at the point of binocular division, greater amplification in the eye-piece, as to its total power, might be advantageously effected by means of lenticular immergent and emergent surfaces of the upper prisms; the lower face of each prism to be *convex*, the upper emergent surfaces concave, giving achromatized refraction in each case. By this means, a larger field, together with a minimum length of tubes above the prisms, would be secured.

By thus appropriating every surface of all the prisms not a reflecting surface, for the purpose of lenticular refraction, the greatest aggregate advantage appears to be secured.

Applied to the telescope, the binocular eye-piece should of course have, and practically has, the same characteristics as when used in the microscope. In this case, however, the front lens of the eye-piece can be dispensed with. Its utility, as thus applied to those telescopes too large to be conveniently in the double form, is too obvious and striking to need remark. The view thus obtained is truly stereoscopic.

Canastota, N. Y., Dec. 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the wave lengths of Fraunhofer's lines.*—ANGSTRÖM has communicated to the Royal Swedish Academy of Sciences a memoir on the wave-lengths of the principal fixed lines in the solar spectrum, which possesses great value not merely as a revision of Fraunhofer's determinations, but also as containing data not hitherto to be obtained. The measurements which form the basis of the work were executed by means of an optical theodolite by Pistor and Martins, reading by means of two microscopes to 2″·1 of arc. The eye-piece was also provided with a micrometer, the head of the screw being divided into 100 equal parts, each division of the scale corresponding to 1″·308 for parallel rays. The ruled glass was by Nobert, and contained in a space of 9·0155 Paris lines, 4501 divisions, drawn with a diamond. The error of the division according to Nobert's measures, was less than 0·00002 of a Paris line. With this ruled glass Fraunhofer's lines could be seen in the third and fourth spectra, the lines being themselves more numerous and distinct than in the spectrum produced by a flint glass prism. The observations were calculated according to the well known formula

$$e \sin \theta = m \lambda,$$

in which e is the distance between two successive lines $= 0{\cdot}000166954$ of a Paris inch; λ the wave length sought, θ the observed angles of diffraction, and m the number of the spectrum. The influence of variations of temperature and pressure of the atmosphere was found to be insensible; that of the temperature of the ruled glass, which varied from 13° C. to 18° C., was also found too small to require the application of a correction. The same statement applies to a variation in the angle θ, arising from the absolute motion of the instrument in the direction of the path of the incident ray. As means of several determinations the following results were obtained for the lines measured by Fraunhofer, the unit of measure being $\frac{1}{1{\cdot}00000000}$ of a Paris inch.

B	2539·73	b	1916·50	H	1467·18
C	2426·29	F	1797·27	H_1	1453·98
D	2178·59	G	1592·34	—	......
E	1948·24				

The difference in wave lengths for the two D lines was 2·226; between the two E lines only 0·395. (The numbers given above doubtless refer to the least refrangible lines in the cases of D and E.) The author gives a careful discussion of the results of the two series of measurements made by Fraunhofer, and arrives at the conclusion that the measurements made with Nobert's ruled glass deserve the greater degree of confidence. The following table gives the results of Angström's determinations of the wave lengths of other lines in the spectrum, the unit being the same as above.

A	2812	
B	2539·7	
C	2426·26	
a	2312·2,	a strong atmospheric line.
	2287·3	A group of strong lines produced by iron and calcium.
	79·6	
	76·8	
	69·4	
	67·7	
	62·1	
	55·1	
D	2179·70	sodium lines.
	2177·48	
1.	2076·1	Iron.
2.	71·3	Iron.
3.	69·7	
4.	2068·3	Iron.
5.	65·4	
6.	60·1	“
7.	16·9	“
8.	13·6	“
	13·1	
9.	07·3	“
10.	05·3	“

11.	1998·4	Iron.	
	97·9		
12.	85·8	“	strong.
	85·3	“	faint.
	84·2	“	
	83·5	“	
13.	1974·2	“	double.
14.	69·6	“	
15.	68·1	“	
16.	65·3	“	
17.	1953·2	“	
E	1948·44	Iron and calcium.	
	48·04		
	46·8	“	“ double.
	34·6	Iron.	
	36·4	“	
	19·6	“	double.
b	1916·50	Magnesium.	
$b_{\prime}$	12·39	“	
$b_{\prime\prime}$	1911·10	Iron and magnesium.	
	10·49	Iron.	
	03·4	“	
c	1832·70	“	
	19·1	“	
	18·4	“	

	08·3	Iron, double.		71·2	Iron.
	01·1	"	*g*	62·4	Calcium, double.
F	1797·27	Hydrogen.		32·0	Iron, double.
f	1632·2	Iron.	*h*	15·9	No metallic line.
	28·5	"		05·3	Iron, strong.
	20·4	"		02·0	"
	04·3	Hydrogen.		1495·2	"
	1598·8	Iron.		80·4	Strong, no corresponding metallic line.
G	1592·34	Iron.			
	79·1	"	H	67·2	Calcium.
	74·7	"	H_1	54·0	"

In the concluding section of his memoir the author points out a method of determining the motion of the solar system by observing the interference bands produced by a ruled surface of glass. The principle of this method was laid before the Swedish Academy, Oct. 6, 1860, and was proposed by Babinet two years ago in the French Institute. It depends simply upon the fact that if we assume that the rays which pass through the openings in the ruled surface are propagated independently of the motion of the instrument, the same must be true of the interference bands, and consequently, in observing with the telescope, an aberration must ensue proportional to the relation between the motion of the telescope perpendicular to its axis and the velocity of the light in the direction of the axis. The results obtained in testing this method appear to confirm the correctness of the theory, but are not sufficiently numerous to be decisive. The author proposes to resume the subject under more favorable circumstances of weather.—*Pogg. Ann.*, cxxiii, 489. W. G.

[*Note.*—The annexed table of wave-lengths, taken in connection with that of Angström, will present to the reader, it is believed, all, or almost all, the exact knowledge which we have at present of the subject. The unit is here the $\frac{1}{10,000,000}$ of a millimeter.

		Wave-lengths.				Wave-lengths.	
1.	A	= 7680	Mascart.	18.	α' Hg	5759	Plücker.
2.	B	6897	Fraunhofer.	19.	$\gamma\, CO_2$	5599	"
3.	α L	6763	Müller.	20.	$\gamma\, SnCl_2$	5584	"
4.	β N	6610	"	21.	β Hg	5461	"
5.	C	6559	Fraunhofer.	22.	α Cl	5451	"
6.	α H	6533	Plücker.	23.	δ I	5337	"
6*b*.	$\alpha\, PCl_3$	6493	"	24.	$\delta\, SnCl_2$	5333	"
7.	$\alpha\, SnCl_2$	6445	"	25.	β O	5328	"
8.	$\alpha\, SiCl_2$	6329	"	26.	E	5265	Fraunhofer.
9.	α O	6150	"	27.	β Cl	5216	Plücker.
10.	11 N	6089	"	28.	$\delta\, CO_2$	5190	"
11.	$\beta\, PCl_3$	6024	"	28*b*.	γ O	5185	"
12.	$\beta\, SiCl_2$	5978	"	29.	α Br	= 5169	"
13.	β I	5947	"	30.	ε I	5167	"
14.	D	5888	Fraunhofer.	31.	$\gamma\, SiCl_2$	5050	"
15.	$\beta\, SnCl_2$	5794	Plücker.	32.	F	4856	Fraunhofer.
16.	α Hg	5782	"	33.	β H	4843	Plücker.
17.	17 N	5762	"	34.	β Br	4793	"

35.	$\gamma\,Cl$	4792	Plücker.
36.	$\gamma\,Br$	4766	"
37.	$\delta\,Br$	4691	"
38.	$\zeta\,I$	4661	"
38*b*.	$\delta\,S_2$	4631	Müller.
39.	$\eta\,I$	4629	Plücker.
40.	$\gamma\,PCl_3$	4591	"
41.	$\varepsilon\,SnCl_2$	4524	"
42.	$\zeta\,CO_2$	4501	"
43.	$\theta\,I$	4446	"
44.	$\eta\,CO_2$	4382	"
45.	$\delta\,O$	4367	"
46.	$\gamma\,Hg$	4359	Plücker.
47.	$\gamma\,H$	4339	"
48.	G	4296	Fraunhofer.
49.	$i\,I$	4215	Plücker.
50.	H	3963	Fraunhofer.
51.	L	3791	Esselbach.
52.	M	3657	"
53.	N	3498	"
54.	O	3360	"
55.	P	3290	"
56.	Q	3232	"
57.	R	3091	"

W. G.

2. *On the phenomenon of interference in the prismatic and diffraction spectra.*—STEFAN has communicated to the Viennese Academy an interesting paper on the interference bands discovered by Talbot and employed by Esselbach,[1] and subsequently by Bernard,[2] for the measurement of the wave-lengths of the ultra-violet rays. Talbot's method of obtaining these bands consists in placing a thin plate of mica before the eye in such a way as to cover one-half of the pupil, leaving the other half free. On looking at a prism so placed that the spectrum is distinctly seen, dark bands will be seen parallel to the edge of the prism, provided that the plate of mica covers the half of the pupil which is upon the same side with the edge of the prism producing the spectrum. Stefan remarks in the first place, that the mica may be placed anywhere between the eye and the prism, and may even be cemented to the prism, provided that it cover the side next the edge. No bands are seen when the rays pass either through the covered or uncovered part of the prism above, but only when the eye receives rays which have passed through both these portions. The plate of mica may be cemented to the farther surface of the prism, or placed at any point between the prism and the slit, provided that the part of the bundle of rays which traverses the plate also passes near the edge of the prism. The same bands may be seen in the diffraction spectra produced by a plate of ruled glass: when the mica covers the left half of the pupil the bands are seen in the spectra lying to the right, and *vice versâ.* When two plates of mica are cemented to the same ruled glass, one to the right and the other to the left, with a narrow space between them which occupies the center of the pupil, bands are seen on both sides in all the spectra. These bands are similar when the plates are of equal thickness; they disappear when the plates are brought together, unless one plate is thicker than the other, when the bands are seen toward the side where the plate is thinnest. The plate of mica must not be too thick in these experiments, as otherwise the number and fineness of the lines become so great that they can no longer be distinguished by the eye. For a prism of 45° the author uses a plate of mica 0·1 millimeter in thickness; with a prism of 60° glass plates up to 0·15 mm. may be used, and a plate of this thickness gives about 120 lines between B and H. The lines produced by thicker plates are best seen by looking at the spectrum with a telescope and placing the inter-

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[1] Pogg. Ann., xcviii, 513.

[2] This Journal, Sept., 1864.

ference plate, as Brewster and Airy have done, between the eye and the eye-piece of the telescope, only, when the astronomical or inverting eye-piece is used, the plate of glass or mica must be placed upon the side which is away from the edge of the prism. Glass plates of the kind used as covering glasses for microscopic preparations are more even than plates of mica and give much sharper lines. The interference plate may be placed anywhere between the eye and the slit of the collimator. By attaching it to the objective on the side toward the refracting edge of the prism the lines are seen at once over the whole spectrum. It is important, in the case of thick plates, that the surfaces should be parallel. A glass plate about three millimeters in thickness with a flint glass prism of 60°, gave about 2500 very sharp and distinct bands between B and H. The bands near B are produced by the interference of rays whose difference of path amounts to over 2700 wave-lengths, while the interval between those near H amounts to more than 5000 wave-lengths. By inclining the interference plate its optical thickness is of course changed; an increase of the inclination causes the lines to run from the violet to the red end of the spectrum, and *vice versâ.* The lines have their maximum deviations when the plate is perpendicular to the direction of the ray, and this affords an easy method of adjustment. As the plate may be placed anywhere between the objective and the prism or between the prism and the collimator, we have the means of determining the influence of physical changes produced in the plate by heat or pressure. The same statements apply in general to the phenomena as seen by the diffraction spectrum. Finally it is possible to produce the interference bands by placing the plate in front of the slit so as to cover one-half of the opening. In this case it is a matter of indifference upon which side of the slit the plate is placed.—*Pogg. Ann.*, cxxii; from *Sitzungs Ber. der kais. Akad. der Wiss.*, xlix. W. G.

3. *On thermo-electric batteries of remarkable power.*—BUNSEN has found that kupferkies possesses a far higher place in the thermo-electric series than bismuth, and that pyrolusite is also superior to the metal though inferior to kupferkies. When kupferkies is combined with an alloy of two parts of antimony and one of tin, or, in order to employ high temperatures, with copper, a couple is produced which, under equal circumstances, gives far more powerful currents than any combination hitherto known. In four experiments the ratio between the electro-motive forces in a kupferkies-copper element and in Daniell's battery was found to be as 1 : 14·7, 1 : 11·9, 1 : 10·9, and 1 : 9·7, while the essential resistances to conduction were 0·74, 0·79, 0·72, and 0·72. During the experiments the temperature of one end of the element, which was plunged into water, was about 60° C., while that of the other end was above that of melting tin. The battery gives a ten times more powerful action than a bismuth-antimony element of the same resistance heated to 0° and 100°. Ten pairs of the kupferkies-copper elements fully replace a Daniell's pair of fourteen square centimeters of active copper surface. Kupferkies is easily fused at a high temperature and may be cast in a mould, but its place in the thermo-electric series is then far below that of bismuth. The native mineral, which is easily worked, must be employed. Pyrolusite combined with platinum gives a battery, the electro-motive force of

which may be easily brought to $\frac{1}{10}$ of that of Daniell's battery without decomposing the mineral. In one experiment Bunsen found the electromotive force of pyrolusite and platinum not less than $\frac{1}{9 \cdot 8}$ of that of Daniell's battery, but the resistance to conduction was 18·4 times as great as that of the Daniell's cell used for comparison.—*Pogg. Ann.*, cxxii, 505.

W. G.

4. *On the thallic alcohols.*—Under this term Lamy has described the compounds which are formed by replacing an equivalent of hydrogen in wood-spirit, common alcohol, &c., by an equivalent of thallium, and which are represented respectively by the formulas $\left.\begin{matrix}C_2H_3\\ Tl\end{matrix}\right\}O_2$, $\left.\begin{matrix}C_4H_5\\ Tl\end{matrix}\right\}O_2$, $\left.\begin{matrix}C_{10}H_{11}\\ Tl\end{matrix}\right\}O_2$, &c. Methyl-thallic alcohol is solid and crystalline. The ethyl compound is a heavy colorless oily liquid; its density at 0° is 3·550; its indices of refraction for B and H are respectively 1·661 and 1·759, the difference between these numbers which measures the dispersive power is 0·098, while the corresponding difference for bisulphid of carbon is 0·079. The amyl-thallic alcohol is also a colorless oil, but its refractive and dispersive powers are much less than those of the corresponding ethyl compound, which is at once the heaviest, the most refractive and the most dispersive of organic compounds. The thallic alcohols are decomposed by boiling; they give by distillation pure hydrogen, alcohol, the corresponding acid and a large residue of metallic thallium. All three compounds burn in the air with a green, more or less brilliant flame, and all are soluble in the corresponding alcohol or in ether. Chloroform also dissolves thallic alcohols, but the solution is soon decomposed with deposition of protochlorid of thallium. Water decomposes these compounds, producing a hydrate of the oxyd of thallium and regenerating the alcohol proper. Acids also decompose them, but carbonic acid forms a solid and definite combination. The analogy between this class of compounds and the corresponding bodies containing potassium and sodium is striking, and confirms the correctness of the view which associates thallium with the alkaline metals and separates it from lead.—*Comptes Rendus*, lix, 780.

W. G.

5. *The Correlation and the Conservation of Forces.*[1]—Under this title Dr. Youmans has presented a collection of memoirs by Grove, Helmholtz, Mayer, Faraday, Liebig, and Carpenter, together with a well written introduction and brief biographical notices of the various writers whose works he has collected. The work is a very welcome addition to our scientific literature, and will be particularly acceptable to those who wish to obtain a popular, but at the same time precise and clear view of what Faraday justly calls the highest law in physical science—the principle of the conservation of force. To scientific men the book will also be most useful for convenience of reference. Sufficient attention has not been paid to the publication of collected monographs or memoirs upon special subjects. Dr. Youmans's work exhibits the value of such collections in a

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[1] The Correlation and Conservation of Forces. A Series of Expositions, by Prof. Grove, Prof. Helmholtz, Dr. Mayer, Dr. Faraday, Prof. Liebig, and Dr. Carpenter. With an Introduction, and Brief Biographical Notices of the Chief Promoters of the New Views. By Edward L. Youmans, M.D. 12mo, pp. xlii, 438. N. Y.: D. Appleton & Co.

very striking manner, and we earnestly hope that his excellent example may be followed in other branches of science. Meantime we will permit ourselves to beg that the faithful laborer in the cause of science and the liberal publisher to whom we owe this volume will still further add to the obligations under which they have laid men of science by giving a complete collection of the mathematical papers which have appeared upon the subject of the mechanical equivalent of heat, and which are now scattered through many volumes of scientific journals and transactions of learned societies, so as to be accessible to few and inconvenient for all.

6. *A new developing solution.*—Some experiments recently undertaken in company with Mr. A. B. Crockett for the purpose of ascertaining the quality of different developing solutions, have led us to the conclusion that a mixture of double sulphate of potash and iron with double sulphate of ammonia and iron, produces better results than any hitherto used. The negatives made by this combination are remarkable for clearness of the shadows, and beauty of half tones, without an excess of intensity in the high lights. The solution acts with remarkable regularity and uniformity, and has little or no tendency to produce fogginess, or a thickening of the fine lines, even when kept upon the plate for a long time.

In practice the following formula was found most convenient. Although not theoretically exact, it produces excellent results.

Pure water, - - - - - -	32 oz.
Neutral sulphate of potash, - - - - -	$\frac{1}{2}$ "
Proto-sulphate of iron, - - - - - -	$\frac{3}{4}$ "
Double sulphate of ammonia and iron, - -	1 "

To this solution add two drops of ammonia, two ounces of acetic acid, and alcohol sufficient to make it flow evenly over the plate. A. E. V.

II. GEOLOGY.

1. *Note on the Geological age of the New Jersey Highlands as held by Prof. H. D. Rogers;* by J. P. Lesley.—The last sentence of an article in the January number of the American Journal of Science, (at p. 97,) extracted from the Proceedings of the Natural History Society of Montreal, seems to me to do great injustice to Professor H. D. Rogers, and other geologists, whose views of the structure and relationships of the rocks of the Highlands have long been published, and are in this article both ignored and misrepresented. The sentence to which I refer reads as follows:—

"This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region [the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs] as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestone of Orange county and of New Jersey underlie unconformably the Lower Silurian strata."

Now Prof. Rogers, to my certain knowledge, not only never expressed such an opinion, at any time, but has always maintained just the reverse; and proved, thirty years ago, what he published on page 43, (Chap. 2,) of his New Jersey Report, in 1840, viz: that the "sedimentary rocks [of the Lower Silurian system] repose in immediate contact with the gneiss, presenting from the attitude of their beds, abundant evidence that they were precipitated upon it [that is, upon the system of the Highlands in New Jersey] while it was yet only in part elevated above the waters." The Canadian geologists will find in these words a clear statement of precisely the view which they themselves have learned to entertain by their week's excursion last summer.

Whatever errors Rogers may have made—and we all make errors—no one can charge him justly with so tremendous a blunder as that of regarding the Highland range as a metamorphic repetition of the Silurian sediments. He mistook indeed the crystalline limestones engaged among the Highlands for metamorphosed synclinal outliers of No. II, as at Franklin (see page 62, New Jersey Report); but this is an exceptional case, as any one can see by studying the true outlying Silurian synclinals represented as sunk among the gneiss hills on Rogers's long sections in the New Jersey Report; or by referring to the many sections (made very nearly at the same time) of the synclinal Silurian valleys among the Durham hills of Pennsylvania. Professor Cook has seen horizontal Potsdam or Calciferous beds overlying these upturned Franklin limestones; (see this Journal, [2], xxxii, 208); and there are exposures of similar crystalline limestones, west of New Jersey, where the evidence is rather in favor of, than against, their subsilurian age. But Prof. Rogers's synthesis of Appalachian geology was on too grand a scale to permit him to overlook *as a whole* and *in the main* the mutual relationship of the Silurian and Azoic Systems. The words which I have quoted above are immediately followed by others showing how clear and large a view he took of the subject, a view which the Canadian geologists are only illustrating:—

"As the same strata [the Silurian], moreover, hold a similar relation to our primary rocks throughout their entire range, from Vermont to Alabama, separated from them by no other group of strata yet discovered, claiming an earlier origin, I have deemed it expedient, for the sake of classification, to confer upon them the title of the *Older Secondary strata* of the United States . . . . with the synonyme of the *Appalachian System of Strata*." (p. 44.)

On page 12, Rogers designates the Highland rocks as the "*Gneiss system*," and, on page 13, carefully distinguishes them from the Staten Island, Trenton, Philadelphia rocks, on the south, as he subsequently distinguishes them from the Potsdam, Trenton, and Hudson river Silurian on the North. On page 11, he gives his column of Formations, and places the "*Primary Rocks*" of the Highlands, underneath No. 6, of the "*Lower Secondary Rocks*;" "a white quartzose sandstone, somewhat coarse and friable: occurs only in a few localities"; and pages 12 to 48 are devoted to a Chapter on these rocks, under the heading "*Primary Rocks of the State: Geology of the Highlands.*"

In fine, no geologist, even of small capacity, could study the New Jersey Highlands, the Durham Hills of Eastern Pennsylvania, and their continuations in the South Mountain and Blue Ridge, and even conceive the suspicion, much less publish it as a conviction, that their strata were metamorphic representatives of the Silurians. We know very well that to say that Rogers thought or said such a bêtisme is to do him gross injustice.

East of the Hudson, there seems to have been made by Rogers, Hall, Logan and many others, all, in fact, except Emmons, a gross mistake of this kind, owing to peculiar circumstances, principally the presence of a great fault of unknown size and location, secondly, by the obliteration of the usual fossils from the top-slates ("Hudson River") of the Lower Silurian System, and thirdly, by a serious change in the Lower Silurian group itself, viz: its expansion in thickness toward the northeast, which made it at first to be confounded with the true Highland gneiss-range running behind it. But in Southern New York, New Jersey, Pennsylvania, and farther south, no such mistake was possible, and at least was never made by Rogers. In fact he had enjoyed such advantages for studying the phenomena of this very belt that he was just the last man in the world who could have made it.

I think the above reclamation is due, in his absence, to Professor Rogers, whose untiring energy and devotion to Appalachian geology did so much to lay fundamental views, which our Canadian friends will find it beyond their power to do more than illustrate. I wish they would try their hand at bearing the veritable *crux criticorum* of our geology, the age and relationships of the Philadelphia-Baltimore belt. There is also something to do about *our* Potsdam sandstone. For as to the patches of so-called Potsdam on the North face of the Highlands at Reading—the long ridge of Potsdam crossing the Schuylkill, 15 miles above Philadelphia—the cliffs of so-called Potsdam at Chicques rock, on the Susquehanna, two miles above Columbia—the supposed Potsdam at the anticlinal of the Nittany Valley, crossing the Juniata near the foot of the Alleghanies—I think it is very doubtful, in spite of the *Scolithus linearis* which they contain, whether these correspond *exactly* to the New York bottom layer of the Silurian. The structure of the Chester county valley, of white marble, with a Potsdam barrier on the north and a mica slate and serpentine country on the south, is a great puzzle. If Sir William Logan and Prof. Hall will take *these* in hand they will run small risk of doing injustice to anybody.

At the same time, it is very satisfactory to have a well trained Canadian eye, familiar with Laurentian lithology, after taking a good look at our gneissic mountains, pronounce them certainly Laurentian and not Huronian, and to receive from the same high authority the announcement that the Sillery sandstones and other prominent members of the Quebec group disappear, coming this way, at about the latitude of the Hudson Gap. For we have been looking for their representatives in Pennsylvania, in vain; although Emmons is understood as identifying his Taconic system with the rocks of the southeast side of the Great Valley.

Philadelphia, Jan. 13, 1865.

2. *Skulls of the Reindeer Period from a Belgian bone-cave, indicating a superior, as well as an inferior, race of primitive men in Europe.*—Prof. Van Beneden, the distinguished zoologist of Belgium, in recent explorations in caverns, has found, as he writes to John Lubbock, Esq., of Chiselhirst, Kent, crania of two distinct pre-historic races, of the Reindeer period, and one of them, that least well preserved, "est franchement brachy-

cephale et prognathe, mais avec une belle boite cranienne." The cavern is in the Carboniferous limestone, 30 or 40 meters above the level of the Lesse. A large number of human remains were found.—*Reader, Jan.* 7.

3. *Casts of fossils.*—Prof. H. A. WARD, of Rochester, New York, is contributing greatly to the diffusion of a knowledge of geological science through the country, by furnishing casts, made under his direction, of fossils of various kinds, from those of the Megatherium, Dinotherium, Glyptodon, and others of gigantic dimensions, to those of Ammonites, Trilobites, eggs of Æpiornis, Gold nuggets, etc. The casts are remarkably well made, firmly secured against breaking, and of high finish. All institutions in which geology is taught, would find it greatly to their advantage to supply themselves with some of the Rochester casts.

4. *Geological Survey of Canada*, Sir W. E. LOGAN, *Director. Figures and Descriptions of Canadian Organic Remains*, Decade II: *Graptolites of the Quebec Group*, by JAMES HALL. 152 pages, 8vo, with 21 plates and many wood-cuts. Montreal, 1865: *Dawson Brothers;* London, New York, and Paris: *Bailliere.*—The first, third and fourth Decades of the figures and descriptions of Canadian Organic Remains, published as a part of the Reports of the Geological Survey of Canada, were issued in the years 1858, 1859, as announced in this Journal in vol. xxvi, at p. 299, and vol. xxviii, at p. 148. The second Decade, just now published, contains an elaborate and extended memoir on the Canadian Graptolites by James Hall. The wonderful variety of these fossils afforded by the Quebec group has enabled Prof. Hall to throw great light on the growth, development and structure of Graptolites. The numerous plates contain full and excellent illustrations of the species. The number of species recognized in the Quebec group is over *fifty*, and none of these are known higher in the series. In the Trenton and Hudson River groups the whole number known is about *thirty*, and in the Upper Silurian there are but *two* (and these occur in the Clinton group), excepting the species of the genus *Dictyonema*, here referred by Hall, of which there are *three* species in the Quebec group, *one* in the Trenton, *three* in the Niagara, *one* in the upper Helderberg and *two* in the Hamilton.

5. *Alger's Cabinet of Minerals.*—The excellent Cabinet of minerals made by the late Francis Alger, of Boston, has been purchased for Alleghany College, Meadville, Pennsylvania.

III. BOTANY AND ZOOLOGY.

1. *Harvard University Herbarium.*—This establishment is noticed in the Annual Report of the President of the University to the Board of Overseers, made in January last, as follows:—

"Dr. Asa Gray has presented to the University his invaluable Herbarium and his Botanical Library; which have been safely transferred to the fire-proof building furnished, at a cost of over twelve thousand dollars, by the generosity of Nathaniel Thayer, Esq., of Boston. A fund has also been raised by subscription, for the support and increase of the collection. . . . The gift of Dr. Gray cannot be estimated in money, but it embraces the results of many years' labor faithfully given by that distinguished botanist, aided by the generosity of his collaborators and correspondents in various parts of the world."

The collections were formally presented by the following letter:—

"*Botanic Garden, Cambridge, November* 30, 1864.

"To the Rev. Dr. Hill, President of Harvard University,

"*My Dear Sir*:—I have the pleasure to inform you that the Herbarium and Botanical Library, which a year ago I offered to present to the University, are now safely deposited in the building erected for their reception by Mr. Thayer. I have regarded them as belonging to the University from the beginning of the present year: but I wish more formally to make them over to the President and Fellows, as the foundation of the Harvard University Herbarium.

"The Herbarium is estimated to contain at least 200,000 specimens, and is constantly increasing. From the very large number of typical specimens it comprises, its safe preservation is very important.

"The Library, from the rough catalogue which has been made out, contains about 2200 botanical works—perhaps 1600 volumes, and nearly as many separate memoirs, tracts, &c.

"The current expenses of the establishment for the first half of the year now drawing to a close have been defrayed by Dr. Jacob Bigelow, who placed in my hands a special donation of two hundred dollars for this purpose.

"I had stated that the income of a capital sum of $10,000 would be required to defray the current expenses of the Herbarium, i. e. for the purchase of certain collections and books not obtainable by exchange, for freights and charges, paper, alcohol, fuel, &c. I am informed that this sum, which Mr. George B. Emerson undertook to raise by subscription, is substantially secured. It is desirable, but probably not at this time practicable, that this endowment should be so far extended as to provide for the services of a Curator, so that I could myself devote valuable time to the prosecution of important botanical works for which I am prepared, and to which I am pledged.

"I have the honor to be, with great respect, very truly yours,

Asa Gray."

We understand that extensive collections of botanical specimens, to be added to the herbarium, have recently accrued. Among them are—

A full suite of Mr. Charles Wright's collections, (about 2500 specimens,) made in Cuba during the past four years, and just now arranged and distributed among botanists.

A very interesting set of plants recently collected, chiefly by Professor Brewer, in the Geological Survey of California under Professor Whitney.

The numerous and important duplicate *Carices*, (and other *Cyperaceæ*,) of the late Dr. Boott, presented by Mrs. Boott; the proper herbarium, set of *Carices* having been bequeathed to the herbarium of the Royal Gardens at Kew.

A large collection of plants of Mauritius and Madagascar, and a continuation of the distribution of the British East Indian herbaria of Griffith, Helfer, &c., presented by the directors of the Royal Botanical Gardens and Herbaria at Kew.

A similar distribution (in continuation) of plants of the Dutch East Indies and Japan, from the Royal Netherlands Herbarium, Leyden, now under the charge of Professor Miquel.

A selection from the Mexican collections of the late Professor Liebmann (Oaks, Ferns, *Cyperaceæ*, &c.); from the Royal Danish Botanic Garden, Copenhagen.

An extensive set of authentically determined plants of Persia, Siberia, and Northern China, from Professor Bunge of Dorpat; and of Algerian plants, &c., from Dr. Cosson.

A set of Mandon's plants of the Andes of Bolivia; acquired by purchase.

A fine general collection of *Algæ*, from Professor Agardh of Lund, authentically named, according to his new *Species Algarum.*

But the most notable accession is the munificent gift which has just been made by John A. Lowell, Esq., of all the botanical books of his own library which the new establishment did not already possess, being chiefly very large, choice, and costly works, such as the Flora Danica, Sibthorp's Flora Græca, Bateman's Orchidaceæ of Guatemala, the Botanical Register, Botanical Cabinet, Botanical Repository, Richenbach's Icones, the large edition of Duhamel, the great works of Jacquin, and others of the same character,—in all 335 volumes, the pecuniary value of which must be reckoned at several thousand dollars.

The building, to which these treasures are consigned, is of brick, upon a granite foundation, with freestone facings, is 32 feet in front and 56 feet deep, a story and a half high, the walls all hollow and ventilated, for greater security from dampness. On the side next the residence of the Professor (with whose private study it is connected by a cold conservatory, 18 feet in length), the wall is entire, except at the private entrance, which is guarded by a double set of wrought iron doors. The room for the herbarium (for greater security against fire) is also separated from the small working-rooms and library and entry in front, by a hollow brick partition, through which passage is had only by heavy iron doors. In the half-story over the front rooms are two other commodious working or store rooms. The principal room, for the herbarium, is about 30½ by 35 feet, and 19 feet high to the vaulted ceiling. It is lighted by a domed skylight, and by a large double window in the north-western end. At the height of about 8 feet, an iron gallery, four feet deep, surrounds the apartment, interrupted only by the large window. The space between the floor and the gallery is completely filled by the herbarium-cabinets, except upon one side of the entrance, where a sort of furnace or stove, of soapstone, chiefly in the front room, comes through the partition wall, and supplies warm air by registers. The walls above the gallery are reserved for a second and similar tier of cabinets, to be constructed when needed. The cabinets, casings, and all the woodwork except the floors (which are of hard pine, bedded in mortar) are of chestnut wood. The building was constructed under the superintendence of the architect, Mr. Edward D. Harris, in a thorough and durable manner, and it is hoped will form a safe and permanent place of deposit for the collections which have been and may hereafter be consigned to it.

2. *Story about a Cedar of Lebanon.*—In the *Edinburgh Review* for October last, turning over the pages of an article on Coniferous Trees, we read, with more of amusement than instruction, a detailed account of the famous Cedar of Lebanon, in the Jardin des Plantes, which, the Review-

er informs us, is "well-known from an anecdote connected with its arrival there."

"It appears that about one hundred and thirty years ago, viz. in 1737, M. Bernard de Jussieu, the celebrated botanist, *when travelling in the Holy Land*, had brought away with him, from among the cedars of Mount Lebanon, a little seedling. Being unprovided with better means of conveyance, *he made a flower pot of his hat, in which he planted it.* He got it safely on board the vessel in which he sailed for France; but tempestuous weather and contrary winds drove the ship out of her course, and prolonged the voyage so much that the water began to fail," .... And the narrative goes on, at great length, to say how the passengers were reduced to the allowance of half a glassful of water a day,—how Jussieu, "all through the lengthened voyage, under the bright sun of the Mediterranean, shared his half glassful of water with his little plant,"—how "his own strength began to sink under the prolonged privation, but he never flinched, and arrived at Marseilles with his own health damaged, but with that of his little plant uninjured,"—how it is said that he came near losing his treasure through the incredulity of the custom house officers, who suspected a contrivance for smuggling jewels or prohibited articles under the roots of the seedling;—how his entreaties and eloquent appeals at length prevailed, and he was allowed to carry his seedling undisturbed to the *Jardin des Plantes*, where it was planted, and became a great and famous tree;—how it grew and flourished until it reached one hundred years of age and 80 feet in height; and, finally, how "in its hundredth year (1837) *it was cut down to make room for a railway, and now the hissing steam-engine passes over the place where it stood.*" (The italics are our own.)

Of course, it is almost unnecessary to tell our readers that Bernard Jussieu never visited the Holy Land, and was not likely, if he had, to come home bare-headed, using his hat the while for a pot; that the fact, or at least the accepted tradition, is merely this, that he brought the seedling Cedar from *England* to Paris in his hat. The story of the voyage from the Levant to Marseilles appears to be an adaptation of one about the three Coffee-plants, which *Antoine* de Jussieu, in the year 1720, sent from the *Jardin des Plantes* to the vessel commanded by Capt. Declieux, who was charged by the French Government with the duty of transporting them to Martinique. The voyage being unusually long, the water is said to have given out, two of the precious plants died, and the remaining one is said to have been kept alive by the devotion of the Captain, who bestowed upon it his own scanty ration of water, and so preserved the ancestor of all the coffee-plantations of the Antilles. For this devotion, we presume, his name is commemorated in the genus *Declieuxia*, of the Coffee Family.

What are the other ingredients of this *pot-pourri* we are unable to conjecture. But the naturalists of the *Jardin des Plantes* may be somewhat astonished to learn that a railway traverses their peaceful grounds, and that a hissing steam-engine runs over the steep little hill upon which flourished, and as they fondly imagine still flourishes, Bernard de Jussieu's Cedar of Lebanon.

We learn from this same article, farther on, that "poor Douglas," the botanical explorer of Oregon and California, "perished at last in a pitfall set for *bears or buffalos in a North American* forest; and that the *average height* of full-grown trees of the Mountain Redwood, (*Sequoia gigantea*, which they still call *Wellingtonia* in Scotland) is full three hundred feet!

3. *Calluna vulgaris in Newfoundland.*—Mr. Murray, late of the Geological Survey of Canada, and now engaged in a survey of Newfoundland, has brought to Montreal specimens of this plant, which were collected by Judge Robinson, on the east coast of Newfoundland, near Ferryland (lat. 47°, long. 52° 50′), and which are stated to be from a small patch of the plant not more than three yards square.

4. *Preservation of Starfishes with natural colors;* by A. E. VERRILL.—Starfishes may be dried, so as to retain their natural colors almost unimpaired, by immersing them in alcohol of moderate strength for about a minute, or just long enough to destroy the life and produce contraction of the tissues, and afterward drying them rapidly by artificial heat. The drying is best effected by placing them upon an open cloth stretched tightly upon a frame and supported a few feet above a stove. Care should be taken not to raise the heat too high, as the green shades change to red at a temperature near that of boiling water. By this process I have succeeded in preserving the delicate shades of red, purple and orange of the species found on the coast of New England, including *Solaster papposus*, *S. endeca*, *Cribella*, *Asteracanthion pallida*, *A. littoralis*, and various other species, specimens of which are in the Museum of Yale College.

The same process is equally applicable to Echini and Crustacea.

5. *A new American Silkworm.*—After numerous experiments, Mr. L. Trouvelot, of Medford, Mass., has succeeded in rearing successfully, and in great numbers, *Attacus Polyphemus* Linn., and in preparing from its cocoon an excellent quality of silk, possessing great lustre and strength, and pronounced superior to Japanese and all other silks, except the best Chinese, by competent judges.

The silk is unwound by a simple process perfected by Mr. Trouvelot, each cocoon yielding about 1500 yards. This insect is very hardy, being found throughout the Northern States and Canada; and, as it feeds upon the leaves of oak, maple, willow, and other common forest trees, may be reared easily in any part of the country.

Mr. Trouvelot has gradually increased his stock from year to year, by raising young from the eggs of the few individuals first captured, until he has at present seven wagon-loads of cocoons, the entire progeny of which he proposes to raise during the coming season.

The thanks of the country are due to the ingenious and persevering author of this successful attempt to introduce a new and interesting field for industry and enterprise, which cannot fail to be a source of profit to those who intelligently engage in it, and of increased wealth and prosperity to the people, should it be developed to the extent that now seems possible.

The first public notice of his experiments with this insect was given by Mr. Trouvelot at a meeting of the Institute of Technology, at Boston, about a year ago, when he exhibited specimens of silk manufactured from it, both natural-colored and dyed. A. E. V.

IV. ASTRONOMY AND METEOROLOGY.

1. *Shooting Stars of Nov.* 11–14, 1864.—Since the last No. of the Journal of Science was issued the Committee on Meteors of the Connecticut Academy of Arts and Sciences have received, by letter from Prof. B. Silliman, dated in California, the following observations obtained by him, while in that part of the country:—

First; Observations at Virginia, in Nevada, made by Mr. Richard H. Stretch:—*Nov. 12th.* One observer. Three hours watch, from $1^h\ 15^m$ A.M. to $4^h\ 15^m$ A.M. Eighteen shooting stars were observed. Twelve were conformable to the well-known radiant in Leo, although four of the number exhibit large deviations. But the deviations are about equal, on either side of the radiant. Of the number that were not conformable, five had their courses directed from an area, $3\frac{1}{2}^\circ$ in diameter, in the same general quarter of the heavens, but far to the north,—in fact, centered about 1° N.W. of β in *U. Major.* Only one—and that of small magnitude—exhibited no tendency from the common radiant. All were yellow, except one that exploded with a beautiful green light. Nearly all appeared as brilliant points of light,—and six of the number were as conspicuous as stars of the first magnitude; also four of the latter were remarkable either for scintillations, or explosion, or broad trains attending them. These four were of the twelve first above mentioned.

Nov. 13th.—The same observer. His watch was begun at $1^h\ 45^m$ A.M. In one hour and eight minutes twenty-two falling stars were seen through the moonlight. Afterward twenty-four more were observed, during a time not stated, but ending, not improbably, about 4^h A.M.—as on the morning preceding. Excepting No. 22, at $2^h\ 53^m$, all appeared as points of yellow light. Mr. Stretch has illustrated the phases of this No. 22, by three sketches. It exploded like a rocket, its tail dropped, and grew shaggy and broad, it turned from yellow to red, shaping itself, at first, like the horn of an ox, and finally like a corona; or, more nearly like a blunt crescent occupying three-fourths of a circle, with the left hand branch exceeding the other one-half in length, but of much less curvature. The duration was three-quarters of a minute.

It is remarkable that the entire group was conformable to a common radiant; although eight of the forty-six—and only eight—show a large deviation—a part to the east, but more to the west. This radiant, however, was in A. R. 156° and N. Dec. 24°,—or nearly seven degrees of arc eastward by northward of its ordinary situation. This position, and the same of the 12th, are derived from the very neat charts which accompany the observations of Mr. Stretch, and which have every meteor's flight traced clearly and numbered upon them.

Second; Observations at Shasta, Nevada, made by Mr. G. K. Godfrey. *Nov. 14th.*—Time, from midnight until 2^h A.M., certainly, and probably until later. At midnight Mr. Godfrey had his attention attracted, "while riding into Shasta, by the falling of innumerable stars." Of these he counted "some fifty," but the time of counting is not stated. Occasionally a star "shot off to the east with great velocity, leaving a brilliant trail behind. There were some which "cast a bright scintillation of a violet color," another group "were of a deep crimson, and much larger, apparently, than the fixed stars, and described a small arc in the sky, and were confined to a small space in the heavens." These last expressions agree (color excepted) with the striking appearances, in 1833, of a limited space around the radiant; but Mr. Godfrey has not stated, even in general terms, the position of the "small space" which he has described. All that can be certainly inferred from his account is the simple fact that shooting stars appeared, at Shasta, on the morning of Nov. 14th last, in such numbers, and of such brilliancy, as to fix Mr. Godfrey's attention upon them for, at least, two hours without any previous notice or expectation of such an occurrence. A. C. T.

New Haven, Jan. 5.

2. *Meteor and Meteorites of Orgueil.*—On the evening of the 14th of May, 1864, a very bright fireball was seen in France throughout the whole region from Paris to the Pyrenees. Loud detonations were heard in the neighborhood of Montauban, and a large number of stones came down near the villages of Orgueil and Nohic. The passage of the meteor was witnessed by a large number of intelligent observers since it occurred early in the evening. Numerous accounts of its appearance have been published in the *Comptes Rendus.*

This fall of meteorites is of peculiar interest. Whilst we have over a hundred large fireballs and detonating meteors whose paths through the atmosphere have been computed, with more or less precision, there are only four or five of them from which stones have been known to come. Of these four or five only one, the Weston meteor, has been so well observed that we can speak with confidence of its path.

The published accounts show that the Orgueil meteor was first seen at an altitude greater than 55 miles, that it exploded at an altitude of about 20 miles, and that it was descending in a line inclined at the least 20° or 25° to the horizon. The velocity must have been not less than 15 or 20 miles per second. This example affords the strongest proof that the stone-producing meteors and the detonating meteors are phenomena not essentially unlike. H. A. N.

3. *Chemical and mineralogical characters of the meteorite of Orgueil.*—Daubrée, Cloëz, Pisani and Des Cloizeaux have communicated to the French Academy papers on the physical and chemical characters of the Orgueil meteorite. In outward appearance it resembles an earthy lignite. The dark mass contains minute grains of a bronze yellow substance having a metallic lustre and a high density, which permits its easy separation from the main portion of the meteorite by levigation; observed under the microscope, these particles are seen to be distinct hexagonal tables; they are strongly attracted by the magnet and have all the physical and chemical properties of magnetic pyrites. A very marked characteristic of the meteorite is, that when placed in water it falls to powder, and a portion of it is in such an extreme state of mechanical division that it remains a long time in suspension in the water, and passes through the closest filter paper. The density of the meteorite taken in pure benzine gave Cloëz 2·567. An analysis by this chemist shows it to contain a considerable amount of magnetic oxyd of iron, besides silicates, protosulphid of iron, traces of nickel and chromium, 5·92 per cent of carbon

(probably of the graphitic form), 9·06 p. c. of water and 5·30 p. c. of matter soluble in water, consisting of chlorids of ammonium, sodium and potassium, and sulphates of magnesia, soda, etc. It is decomposed by chlorhydric acid with evolution of sulphuretted hydrogen giving a greenish yellow solution and leaving a black residue amounting to 7·6 p. c.; this residue when heated with excess of air burns and leaves a gray substance amounting to 2·2 p. c.

Pisani confirms the observations of Cloëz as to the peculiar comportment of this stone when treated with water, and calls attention to its porosity, and to the fact that this accounts for the facility with which the sulphids have become oxydized to sulphates and hyposulphites, and for the avidity which a dried specimen of it has for water. An experiment showed that a specimen dried at 110° C. absorbed 7 p. c. of water in a few hours when simply exposed to the air. Pisani found 3·35 p. c. of matters soluble in water in operating on about 18 grams of the undried substance. This contained hyposulphurous acid 0·48, sulphuric acid 1·40, chlorine 0·08, magnesia 0·30, lime 0·16, potash 0·60, soda, ammonia, etc., and loss 0·77. Alcohol took up 0·37 p. c. which proved to consist chiefly of sulphur. Pisani's tabulated results give for the composition of the whole meteorite:

$\dddot{\mathrm{Si}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{Fe}}$	$\dot{\mathrm{Mn}}$	$\dot{\mathrm{Ca}}$	$\dot{\mathrm{Na}}$	$\dot{\mathrm{K}}$	$\dddot{\overline{\mathrm{Al}}}$
26·08	17·00	6·96	0·36	1·85	2·26	0·19	0·90,

together with chromic iron 0·49, magnetic iron 12·03, nickeliferous sulphid of iron 16·97, water and supposed organic substances 14·91=100. This gives for the oxygen ratio of the oxyds and silica 9·98 : 13·90=3 : 4.

Pisani proved the presence of magnetic iron by dissolving the mineral in hot nitric acid which decomposed the silicate and the sulphids, and left a black magnetic residue. The nickel was proved to exist as sulphid by treating the substance with sulphid of ammonium, which dissolved out sulphid of nickel. The stone contains 55·60 p. c. of silicates, and taking into account the water determined by Cloëz, the oxygen ratio of the silicate is that of serpentine. If the alumina is due to anorthite it will give 2·42 p. c. of this feldspar contained in the meteorite. Subsequent observations by Des Cloizeaux, Pisani, Daubrée and Cloëz prove that this remarkable meteorite contains minute rhombohedral crystals of a double carbonate of magnesia and iron, and Cloëz obtained a little more than half of one per cent of carbonic acid from a portion of the meteorite operated upon.—*Comptes Rendus*, May 30, July 18, and Nov. 14, 1864.

G. J. B.

4. *Shooting Stars of Jan. 2d.*—On the morning of Jan. 2d, 1865, shooting stars were sufficiently numerous to attract the attention, at New Haven, of those who were not aware that unusual numbers were looked for on that morning.

5. *Discovery of another Asteroid, Alcmene,* (82).—Another small planet was discovered by Luther, at Bilk, on the 27th of November, 1864. It has been named Alcmene. Oppolzer of Vienna gives the following elements of its orbit, computed from observations of Nov. 27th, Dec. 3d, and Dec. 8th.

Epoch Dec. 0·0, Berlin m. t.
$M = 308° \ 40' \ 1''$
$\pi = 136 \ 47 \ 50$
$\Omega = 28 \ 49 \ 28$

$i = 3° \ 3' \ 14''$
$\varphi = 11 \ 25 \ 41$
$\mu = 781''{\cdot}152$
$\log a = 0{\cdot}438181.$

6. *Comet IV*, 1864.—On the 11th of December, 1864, Mr. Bäker at Nauen, near Berlin, discovered a telescopic comet.

7. *Spectrum of a shooting star.*—Herschel has recently observed the spectrum of a shooting star. It appeared near Capella, and was almost as brilliant as that star. He followed it for more than a second in its rather slow motion, and ascertained that its spectrum was as continuous a spectrum as that of Capella, and a little more extended, and, therefore, that it consisted of a solid or liquid substance and not of a gas or incandescent vapor, as Mr. Huggins has suggested with regard to some nebulæ.

V. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *Patent regenerative Gas-furnaces, of* C. W. & F. Siemens.—Professor Faraday, in his lecture at the Royal Institution, on the 20th June, 1862, describes these furnaces in the following terms:—

"The gaseous fuel is obtained by the mutual action of coal, air, and water at a moderate red heat. A brick chamber, perhaps 6 ft. by 12 ft., and about 10 ft. high, has one of its end walls converted into a fire-grate, *i. e.*, about half way down it is a solid plate, and for the rest of the distance consists of strong horizontal plate bars where air enters, the whole being at an inclination such as that which the side of a heap of coals would naturally take. Coals are poured, through openings above, upon this combination of wall and grate, and being fired at the under surface, they burn at the place where the air enters; but as the layer of coal is from 2 to 3 feet thick, various operations go on in those parts of the fuel which cannot burn for want of air. Thus the upper and cooler part of the coal produces a large body of hydrocarbons; the cinders or coke which are not volatilized, approach, in descending, toward the grate; that part which is nearest the grate, burns with the entering air into carbonic acid, and the heat evolved ignites the mass above it; the carbonic acid, passing slowly through the ignited carbon, becomes converted into carbonic oxyd, and mingles in the upper part of the chamber (or gas producer) with the former hydro-carbons. The water, which is purposely introduced at the bottom of the arrangement, is first vaporized by the heat, and then decomposed by the ignited fuel, and re-arranged as hydrogen and carbonic oxyd; and only the ashes of the coal are removed as solid matter from the chamber at the bottom of the fire-bars.

These mixed gases form the gaseous fuel. The nitrogen which entered with the air at the grate is mingled with them, constituting about a third of the whole volume. The gas rises up a large vertical tube for 12 or 15 feet, after which it proceeds horizontally for any required distance, and then descends to the heat-regenerator, through which it passes before it enters the furnaces. A regenerator is a chamber packed with fire-bricks, separated so as to allow of the free passage of air or gas between them. There are four placed under a furnace. The gas ascends through one of these chambers, whilst air ascends through the neighboring chamber, and both are conducted through passage outlets at one end of the furnace, where mingling, they burn, producing the heat due to their chemical action. Passing onward to the other end of the furnace, they (*i. e.*, the combined gases) find precisely similar outlets down which they pass; and traversing the two remaining regenerators from above downward, heat them intensely, especially the upper part, and so travel on in their cooled state to the shaft or chimney. Now the passages between the four regenerators and the gas and air are supplied with valves and deflecting plates, which are like four way-cocks in their action; so that by the use of a lever these regenerators and air-ways, which were carrying off the expended fuel, can in a moment be used for conducting air and gas into the furnace; and those which just before had served to carry air and gas into the furnace now take the burnt fuel away to the stack. It is to be observed, that the intensely heated flame which leaves the furnace for the stack always proceeds downward through the regenerators, so that the upper part of them is most intensely ignited, keeping back, as it does, the intense heat: and so effectual are they in this action, that the gases which enter the stack to be cast into the air are not usually above 300° F. of heat. On the other hand, the entering gas and air always pass upward through the regenerators, so that they attain a temperature equal to a white heat before they meet in the furnace, and there add to the carried heat that due to their mutual chemical action. It is considered that when the furnace is in full order, the heat carried forward to be evolved by the chemical action of combustion is about 4000°, whilst that carried back by the regenerator is about 3000°, making an intensity of power which, unless moderated on purpose, would fuse furnace and all exposed to its action.

Thus the regenerators are alternately heated and cooled by the outgoing and entering gas and air, and the time for alternation is from half an hour to an hour, as observation may indicate. The motive power on the gas is of two kinds—a slight excess of pressure within is kept up from the gas-producer to the bottom of the regenerator to prevent air entering and mingling with the fuel before it is burnt; but from the furnace, downward through the regenerators, the advance of the heated medium is governed mainly by the draught in the tall stack, or chimney.

Great facility is afforded in the management of these furnaces. If, whilst glass is in the course of manufacture, an intense heat is required, an abundant supply of gas and air is given; when the glass is made, and the condition has to be reduced to working temperature, the quantity of fuel and air is reduced. If the combustion in the furnace is required to be gradual from end to end, the inlets of air and gas are placed more or less apart the one from the other. The gas is lighter than the air; and if a rapid evolution of heat is required, as in a short puddling furnace, the mouth of the gas inlet is placed below that of the air inlet; if the reverse is required, as in the long tube-welding furnace, the contrary arrangement is used. Sometimes, as in the enameller's furnace, which is a long muffle, it is requisite that the heat be greater at the door end of the muffle and furnace, because the goods, being put in and taken out at the same end, those which enter last, and are withdrawn first, remain, of course, for a shorter time in the heat at that end; and though the fuel and air enters first at one end and then at the other, alternately, still the necessary difference of temperature is preserved by the adjustment of the apertures at those ends.

Not merely can the supply of gas and air to the furnace be governed by valves in the passages, but the very manufacture of the gas-fuel itself can be diminished or even stopped, by cutting off the supply of air to the grate of the gas-producer; and this is important, inasmuch as there is no gasometer to receive and preserve the aeriform fuel, for it proceeds at once to the furnaces.

Some of the furnaces have their contents open to the fuel and combustion, as in the puddling and metal-melting arrangements; other are enclosed, as in the muffle furnaces and flint-glass furnaces.

The economy in the fuel is esteemed practically as one-half, even when the same kind of coal is used either directly for the furnace or for the gas-producer; but, as in the latter case, the most worthless kind can be employed—such as slack, &c., which can be converted into a clean gaseous fuel at a distance from the place of the furnace, so, many advantages seem to present themselves in this part of the arrangement."

Faraday concludes his lecture with the following conclusive figures:—

"Carbon, burnt perfectly into carbonic acid in a gas-producer, would evolve about 4000° of heat; but, if burnt into carbonic oxyd, it would only evolve 1200°. The carbonic oxyd, in its fuel form, carries on with it the 2800° in chemical force, which it evolves when burning in the real furnace with a sufficient supply of air. The remaining 1200° are employed in the gas-producer in distilling hydro-carbons, decomposing water, &c. The whole mixed gaseous fuel can evolve about 4000° in the furnace, to which the regenerator can return about 3000° more."

2. *National Academy of Sciences.*—The fourth session of the National Academy of Sciences was held at the Capitol in Washington on the 3d, 4th, 5th, and 6th of January of the present year. The three vacancies in the Academy were filled by the election of Prof. O. N. Rood and Gen. M. C. Meigs to the Class of Mathematics and Physics, and Prof. J. P. Kirtland to the Class of Natural History. The following papers were read:

1. On a chronograph for measuring the velocities of projectiles; by J. E. HILGARD.

2. Homologies and classification of the Cephalopods; L. AGASSIZ.

3. Geographical distribution of North American Birds; S. F. BAIRD.

4. On the tables of the Moon; BENJ. PEIRCE.

5. Metamorphoses of some Malacopterygians; L. AGASSIZ.

6. On chemical classification; W. GIBBS.

7. Progress of the Geological survey of California; J. D. WHITNEY.

8. On a method of exhibiting certain statistics of hospitals; J. L. LECONTE.

9. Note on the changes which have taken place in the bar of Charleston harbor since the sinking of obstructions in the main channel, as developed by the U. S. Coast Survey; J. E. HILGARD.

10. Glacial phenomena and present configuration of the State of Maine; L. AGASSIZ.

11. Dimensions and proportions of American soldiers; B. A. Gould.
12. On a regulator for maintaining uniform motion, and an apparatus for recording time-observations in type; J. E. Hilgard.
13. Mineral lands of the United States, and the relation of the government to their management; J. D. Whitney.
14. Origin and formation of sedimentary rocks; J. S. Newberry.
15. Origin and distribution of petroleum in the U. S.; J. S. Newberry.

Alex. Braun, G. B. Airy, Astronomer Royal of England, R. Owen, Prof. F. Wöhler, Sir R. I. Murchison, and M. V. Regnault, were elected Foreign associates. The next session of the Academy will be held at Northampton, Mass., on Wednesday the 23d of August, 1865.

3. *Lawrence Scientific School, at Cambridge, Mass.*—The Lawrence Scientific School has received, as a New Year's gift, (Jan. 1, 1865) fifty-two thousand five hundred dollars, twenty-five hundred to be expended at once for the equipment of the laboratory, and the balance to endow equally the Chemical and Engineering departments.

Obituary.—4. Capt. James M. Gilliss, U. S. N.—Captain Gilliss, the Superintendent of the Washington Observatory, died suddenly, at Washington, of apoplexy, on Thursday, the 9th of February. The Naval Observatory, under his charge at the time of his death, was constructed from his plans, and equipped with its original instruments by him, during the years 1843, 1844, Congress having authorized its establishment by an Act passed in 1842; but only since 1861, when Maury, faithless to his country, left his post of duty, has it been under his abler direction. It would have been better for the scientific reputation of the country had it continued in his hands. An earlier Observatory at Washington, fitted up mainly by him, had been the scene of his labors from 1838 to 1842, and in the volume containing the results—the *first* volume of American Astronomical Observations—Mr. Gillis expresses in his Preface, his pleasure that "the prosecution of these observations should have resulted in the foundation of a permanent Naval Observatory."

During the three years, 1849 to 1851, Capt. Gilliss was in Chili, in charge of the U. S. Expedition for determining the Solar Parallax, and if his observations failed of all that was expected of them, it was from the want of that coöperation in the northern hemisphere which was reasonably looked for by him. The National Intelligencer, (Washington) of the day before his death, (Feb. 8,) contains his last astronomical communication—one relating to the Planet Mars—dated Feb. 7.

Capt. Gilliss was an observer of great skill and accuracy, a man of noble personal character, and a patriot in the highest sense of the word. Three of his sons have been in the recent armies of his country, and the eldest—a Captain—reached home from the Libby prison, after four months imprisonment, only the day before his father died.

5. George P. Bond.—It is seldom that astronomical science has received a more severe blow than that occasioned by the death of George Philips Bond, of Harvard College, Philips Professor of Astronomy, and Director of the observatory connected with that institution. After a lingering illness of more than a year, during which his ardor in the study of the heavens led him oftentimes to exposures entirely incompatible

with the state of his health, he closed a useful and an unblemished life on the 17th of February,—eight days after his compeer, Captain Gilliss.

As an accurate and truthful observer of astronomical phenomena, he was, without question, unequalled by any one in this country, and among the first in the world. In his short career he contributed many valuable papers of original discoveries and calculations to various periodicals and institutions in this and other countries. His greatest work, and that which gave him honor the world over, is his account of the Donati comet which constitutes the third volume of the Annals of the Observatory. To this, the palm of unrivalled excellence has been freely awarded by the best astronomical observers of Europe.[1] Well trained by his lamented and distinguished father, and taking advantage of the best telescope mounted in so high a southern latitude, he explored with searching scrutiny the great nebula of Orion, a work which he pursued with untiring zeal and anxiety in his latter days; and while we fear his waning strength may have left it incomplete in form, we are assured, and rejoice in the assurance, that abundant ability remains in the observatory to prepare it for publication.

We might dwell much longer on his astronomical history, but the necessary brevity of this notice requires that we should turn to his private life. It is rare indeed, that so many virtues are blended in any man. His innocent unpretending manners, the perfect absence of every air of vanity or pretension, crowned with an unwavering christian faith and deep sense of religious obligation, secured for him, not the mere respect, but the kindest regard of all that had the happiness of his acquaintance.

W. M.

6. Dr. HUGH FALCONER.—Dr. Falconer, Vice-President of the Royal Society, died at London, on the 31st of January.

7. *The Differential Calculus;* by JOHN SPARE, M.D. Boston, 1865. Bradley, Dayton & Co. 8vo. pp. xx, and 244.—The plan of this treatise is different from that of most works upon the same subject. The aim seems to have been to treat the calculus as peculiarly the continuation of Algebra. Hence most of the applications are to problems which resemble as much as possible the ordinary problems of Algebra. The author's style is somewhat involved, and his logic not entirely free from criticism.

H. A. N.

8. *History of Delaware County, Pennsylvania, from the Discovery of the Territory included within its limits to the present time, with a notice of the Geology of the County, and Catalogues of its Minerals, Plants, Quadrupeds and Birds;* written under the direction and appointment of the Delaware Co. Institute of Science, by GEORGE SMITH, M.D. 582 pp. 8vo.—This history is the work of great care and research. The chapter on the Geology of Delaware County is illustrated by a colored map showing the distribution of its rocks, and it closes with a catalogue of mineral localities. The work is illustrated by several plates, wood-cuts and maps.

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[1] We are informed that, a month since, Mr. Bond received word from President De La Rue, of the Royal Astronomical Society, that the Society, at its last annual meeting, in January, had voted him a gold medal for his work on this comet.

THE

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[SECOND SERIES.]

ART. XXVII.—*On Molecular Physics*; by Prof. W. A. NORTON.

[Continued from vol. xxxviii, page 223.]

IN a former part of this paper a succinct exposition has been given of a consistent general theory of Molecular Forces, and the Molecular Constitution of bodies, and special theories of the different states of aggregation of matter, and the processes of transformation from one state to another, as well as of the essential nature and modes of excitation and propagation of the two agencies of light and heat.[1] The conclusions arrived at were all deduced from two fundamental principles, viz:

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[1] In a paper on heat-vibrations, by Mr. James Croll, published in the Philosophical Magazine, May, 1864, it is maintained that the heat-vibration does not consist in a motion of an aggregate mass of molecules, but in a motion of the individual molecules; also that it does not consist in excursions of the molecule or atom across centres of equilibrium, but in alternate expansions and contractions of the atom itself. It will be seen that these ideas are in accordance with the conception of the constitution of a molecule adopted at the beginning of the present memoir (p. 62), and with the theory of heat-vibrations, or heat-pulses, deduced therefrom (p. 64). The author remarks that his conclusion that "the ultimate atom itself is essentially elastic, is opposed to the ordinary idea, that the atom is essentially solid and impenetrable, and favors the modern idea that matter consists of a force of resistance acting from a center." But in the present communication the ground is taken that it is the elastic ethereal atmosphere condensed upon the atom that invests it with its panoply of power. Thus armed it becomes an efficient molecule—when associated with a kindred molecule an epitome, in fact, of the universe. In the contractions and expansions that result from the action of the central atom upon its atmosphere is to be discerned the origin not only of heat-vibrations, but of all the molecular forces; and in the varied possible movements and changes of molecular atmospheres, dependent upon their elasticity and mutual action, are to be found the essential causes of physical phenomena.

(1.) That matter exists in the three different forms of *ordinary* or *gross matter*, an *electric ether*, and a more subtile *universal ether;* and that each of these is made up of spherical atoms.

(2.) That there are two primary forces: *attraction*, and *repulsion.*

The primary force of attraction is exerted between the atoms of ordinary matter and the two ethers, and between the atoms of the electric ether and those of the universal ether; while a mutual repulsion subsists between the constituent atoms of the two ethers.

From these two postulates the conclusion was derived that each atom of ordinary matter must be surrounded by two atmospheres, one consisting of electric ether, and another of the universal ether, pervading the former; and that the atoms of the electric ether must also be surrounded by atmospheres of the more subtile universal ether which pervades the space between them. Such being the condition of things, it was assumed that the attraction of the central atom of matter for the atoms of electric ether exterior to it was propagated by the intervening universal ether, and that the same was true of the mutual repulsion exerted between the individual atoms of the electric ether.

The primary force of heat, as one of the molecular forces, was deduced from these principles, and found to have its origin in the force of molecular attraction.

We propose now to show that the characteristic phenomena of electricity, comprised under the several heads of *frictional electricity, voltaic-electricity, thermo-electricity, magnetism, electro-magnetism, magneto-electricity, induced currents*, and *dia-magnetism*, may be derived as mechanical deductions from the same fundamental conceptions.

We have thus far had no occasion to introduce arbitrary hypotheses, but have in fact discarded those hitherto in vogue, as that of the permanent polarity of atoms, and various hypotheses as to the mode by which such polarity is constituted and maintained. It will be seen that in the wide field we are now entering, the same fundamental ideas will suffice, and that the same two forces, attraction and repulsion, operating on the same three forms of matter, are alone concerned in evolving the phenomena. In every different province that we enter we but recognize new results, achieved by the same agencies, working by the same essential processes. As transcendently wonderful as is the infinite variety of Nature, no less so is the all-comprehensive unity of its origin, and the grand simplicity of its evolution. From the point of view now taken, this truth, which has long been discerned with more or less distinctness, stands forth in its full proportions. In the discussion of specific properties of different substances, we have admitted, and shall continue to admit, only differences in degree, not in kind; and differences too, that fall within the scope of the general theory. No higher requirement than this can reasonably be exacted of any fundamental conception.[1]

Electricity.

Preparatory to the discussion of this topic it is important to inquire more minutely than has hitherto been done into the electric condition of molecules, both simple and compound. Each simple atom, as we have seen, is surrounded by an electric atmosphere; but has this atmosphere a definite limit? and if so what is the condition of the electric ether exterior to it, and in the interstices between molecules? The equilibrium of such an atmosphere is determined by the operation of two antagonistic forces, the attraction of the central atom for each atomette of the atmosphere, and the repulsion experienced by such atomettes from all the others of which the atmosphere is composed. If we regard these forces as in no degree intercepted in their propagation, the atmosphere will extend with decreasing density to that height at which the attraction exerted by the central atom upon an atomette is just neutralized by the repulsion exerted upon the same by the entire atmosphere. The centers of repulsion and attraction will also be coincident, and the law of variation of the two forces will be the same; hence beyond the limits of the atmosphere there will be no effective action exerted by the molecule upon any electric ether that may be posited there. But the notion that there is no interception of force does not accord with the fundamental idea of the propagation of force by the universal ether (p. 238). It is impossible that a propagated force of repulsion should take effect upon an atomette of electric ether, unless the universal ether, which is the medium of propagation, be in some degree condensed upon the surface upon which the wave-force falls. Such condensation must give rise to a reflex wave, and the dispersion of a certain amount of force into the surrounding ether. Upon the principle of the conservation of force the amount of force thus dispersed must be abstracted from the original wave-force. Now if the atomettes of the electric atmosphere intercept a certain portion of the repulsion propagated from other atomettes, it follows that the center of repulsion of the whole atmosphere cannot be coincident with the center of attraction, or center of the atom of mat-

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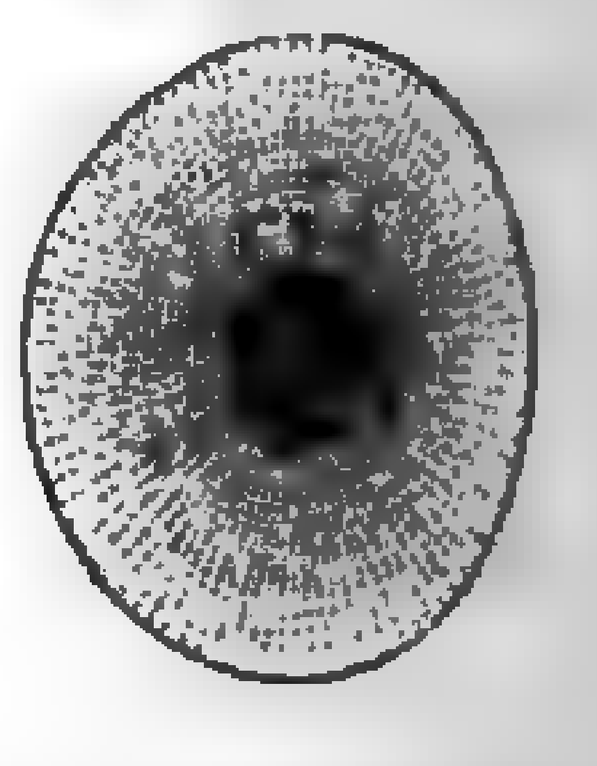
[1] The author may seem to have adopted, in the scheme of molecular forces presented on pp. 63, 64, an arbitrary hypothesis, in assuming the existence of a force of molecular repulsion between the surfaces of contiguous electric atmospheres; as the grounds upon which the theoretical inference was drawn were not stated in the course of the discussion. There will soon be occasion to offer these in another connection.

ter. It would seem also that the repulsive pulses propagated from the atomettes should be more or less intercepted by the central atom of the molecule, which should tend to displace the center of repulsion still more. For each side of the molecule this center must lie somewhere between the center of the atom and the surface of the atmosphere, as at r, fig. 4. In this state of things the atmosphere will have a definite limit, as before; but beyond its limits, since its repulsion must decrease more rapidly than the attraction, an effective force of attraction will be exerted by the entire molecule. As a consequence, the external electric ether will be retained in contact with the atmosphere, and press upon its surface with a certain force. Its density will decrease outward and doubtless become insensible at sensible distances. If then we confine our attention to a single molecule, we perceive that it must consist of an atom of ordinary matter surrounded with two envelopes of electric ether; of which the outer presses upon the inner, and extends indefinitely, but becomes evanescent at a distance a certain number of times greater than the diameter of the inner envelope.[3] Hitherto in speaking of the electric atmosphere of a molecule, we have alluded only to the inner and principal envelope, and have regarded the diameter of a molecule the same as the diameter of this envelope; and we shall use these terms in the same sense hereafter, unless the exception is distinctly specified.

4.



The outer electric envelopes of molecules serve to establish an electric communication between them, and play an important part in all electric phenomena. By their pressure upon the inner envelopes, or the atmospheres so-called, of the molecules, they develop a force of electric repulsion exerted outward at the surface of each atmosphere. This is the force already recognized as one of the molecular forces (p. 64). It has its immediate origin in the compression of the atmosphere at or near its surface, by the outer envelope, which increases the repulsive action of the upper portion of the atmosphere, and so brings into operation an effective repulsion at this surface, where otherwise the effective force would be zero. It is to be borne in mind that the attraction of the central atom is dynamical in its effects, and that attendant upon its exertion the outer envelopes will have an inward and outward movement; but such alternate movements will, when propagated outward, neutralize each others' effects, unless a secondary force is developed in the process. Now it is precisely such a force that is developed in the manner just explained, and this must be propagated by the electric ether

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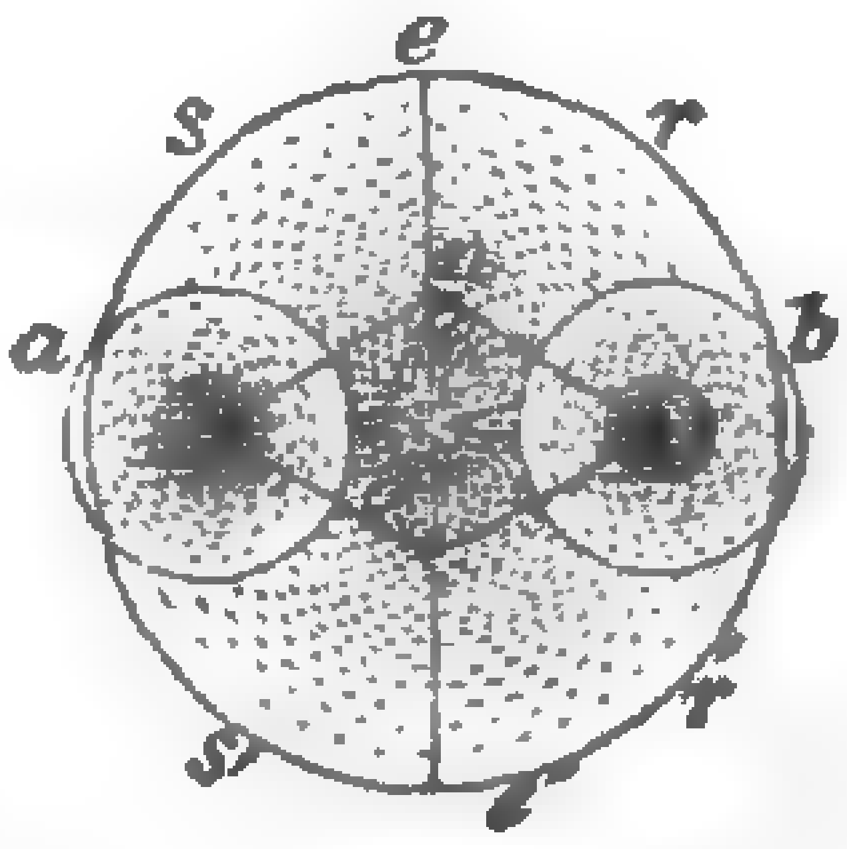
[3] It is possible that in some cases the outer atmospheric envelope may be made up of several spherical layers, separated by surfaces of no effective molecular action.

of the outer envelope, or the interstitial electric ether of bodies, as a wave-force.[4]

We have seen that the attraction exerted by the atom upon its atmosphere, proper, by forcing outward a portion of the universal ether near the surface of the atom, develops at the same time an effective molecular attractive force and what we have called the force of heat-repulsion. From the difference that obtains in the circumstances under which these forces and the above mentioned force of electric repulsion originate, it is highly probable that their constants (*n* and *m*, p. 68,) would be different.

5.



To pass now to the case of a *compound molecule:* let *a* and *b* (fig. 5) be two similar molecules, held in equilibrium by their mutual actions, and let us enquire into the condition of the electric ether on the line crossing perpendicularly the line of their centers, at its middle point, *m*. At *m* the attractions of the two molecules for an atom of electric ether there will neutralize each other, but at all other points of this line, the resultant of the attractions will have a finite value and be directed inward toward the line of the centers. This resultant will increase in value as the distance from the line of the centers increases, to a certain point, *n*, beyond which it will decrease. The sum of all these resultants will act as a compressing force upon the electric ether about *m*, and determine its density at that point. At other points, as *e*, *r*, *s*, variously situated in the vicinity of the molecules, the joint attraction of the two will generally exceed the attraction of either molecule alone. It follows, therefore, that the two united molecules will be surrounded by an electric atmosphere of their own, spherical or spheroidal in form, and at the same time that the two individual atmospheres will be materially modified. This atmosphere will have an outer envelope, as in the case of a simple molecule.

If we suppose several molecules thus united, the entire group will have its own proper atmosphere. The extent of this atmosphere will depend upon the normal attraction at the surface of the group; and this will also determine the density of the electric ether in the interstices between the molecules.[5]

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[4] The pulses of heat that are received from extraneous sources, and pass from molecule to molecule, augment this force of atmospheric repulsion. Accordingly, the value of the "constant," *m*, of this force (p. 67.) depends not only upon the attractive force, size, and perhaps other peculiarities of the central atom, but also upon the amount of heat received from all extraneous sources. External forces, of compression or extension, applied to a body, also tend to augment or diminish the value of *m*.

[5] In treating briefly of solidification (p. 209, &c.), our attention was confined chiefly to the union of simple molecules in one homogeneous mass, but there is good reason to believe that in most cases of solidification, with the exception, perhaps, of that of perfect crystallization, compound molecules are first formed, and that these combine, in various modes, to form the solid. In every such instance the formation

Let us next inquire into the true nature of the *electric polarization* of molecules.

A molecule becomes polarized whenever, from any cause, a portion of its electric atmosphere is urged around from one side to the opposite side; where there is an excess, the polarity being *positive*, and where there is a deficiency, *negative*. But it is especially important to observe that the act of polarization is distinct from the result. The one is dynamical, the other statical. The act of polarization, too, does not consist simply in a flow of a certain amount of electric ether from the one side to the other of the atom; in addition to this, a certain quantity of the ether is detached, or expelled from the atmosphere on the positive side, and a corresponding amount absorbed into it on the negative side. Accordingly, *when a molecule is being polarized, or when its polarization is increasing, there is a flow of electric ether toward its negative side, and away from its positive side. On the other hand, when a polarization once acquired is falling off, there is a flow, or an electric current, in the opposite direction.* To make this more evident, let S (fig. 6) be a surface receiving a charge of positive electricity, and *a*, *b*, *c*, &c., a series of particles of a dielectric medium. The repulsion of the positive charge on S will polarize the nearer side of *a* negatively, and the farther side positively; but the same force will urge the interstitial ether toward *a*, and the augmented attraction of the negative side of *a* will add a certain portion of it to the atmosphere of *a*. At the same time, the excess of electric ether accumulated on the farther side of *a* will act repulsively upon the upper portion of its atmosphere and expel a certain portion of it. It will also urge toward *b* the interstitial electric ether between *a* and *b*, and polarize *b*, just as S has polarized *a*. The chain of particles will therefore become polarized in succession; and at the same time there will be an electric movement, a dis-

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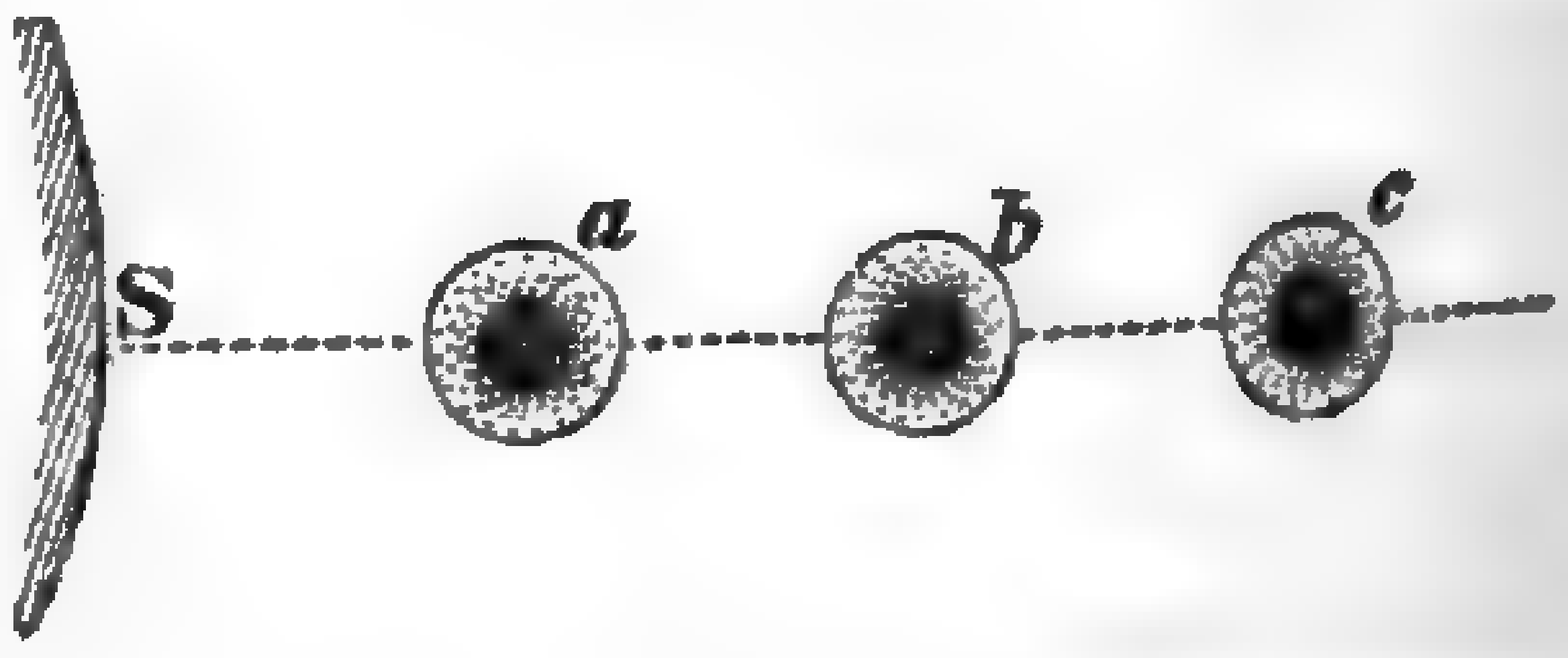

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of such groups of molecules should be attended with the evolution of heat. This will be the inevitable result of the compression of the electric ether between the combining molecules (fig. 5); whether the individual atmospheres be compressed or not.

The heat of combustion and of chemical union generally, has, in all probability, a similar origin; that is in a compression of the electric ether between the molecules, or in such a compression combined with a contraction of the individual atmospheres. This topic, and the probable cause of the different effects observed in different instances of combination, will be briefly considered under the head of chemical union.

The varied forms of crystallization assumed by different substances, under similar circumstances, are probably due, in a great degree, to the diversity that obtains in the number and grouping of the individual molecules of which the ultimate compound molecules consist.

charge from one to another. To take a more complete view of the matter, the molecular polarization resulting from the electricity received by any single point, S, will not be confined to the normal line *a*, *b*, *c*, but will extend, though with diminished intensity, along lines radiating outward from S. A similar remark may be made of the flow of electricity that accompanies the polarization.

If we take account of the entire spherical surface, S, and suppose it to be surrounded by a dielectric medium, as the air, extending to an indefinite distance, each new addition to its positive charge will develop a wave of increasing molecular polarization, which will be propagated in all directions outward through the surrounding medium. This wave, if it may be so termed, will be accompanied by a spherical electric "wave of translation" that will spread indefinitely through the same medium. This latter wave will consist of a series of discharges from one spherical layer of particles to the next; while the former will consist of propagated movements confined to the atmospheres of the particles;—these movements being produced by the repulsive action of the electric ether accumulating on the farther sides of the particles, transmitted primarily through the universal ether. The quantity of electricity that moves forward from one layer of particles to the next, should be equal to the quantity received by the surface, for the determining cause of the electric wave of translation lies in the fact that the force of tension of the propagated electricity urges this quantity between the surface and the first layer of the medium.

The tension of the electricity received results from the compression produced by the expansive action of the charge, which is ultimately in equilibrium with the force by which the disturbed atmospheres of the medium tend to return to their original condition. The tendency of the free electricity to escape between and around the polarized molecules of the medium is counteracted, as will soon be seen, by a resistance developed by the polarization.

This we conceive to be the true process of *induction.* It will be readily perceived that it is in entire conformity with the experimental results obtained by Faraday, and in accordance with his theoretical ideas of the general nature of the process.

We have supposed the electrised surface to be surrounded by a single dielectric medium, extending indefinitely; but the result would be essentially the same if the waves were propagated through several such media, as air, glass, &c. If we suppose the first medium to be replaced by another, while the entire quantity of electricity propagated by it, as the polarization of the medium goes on, should remain the same, as Faraday has shown that it actually does, it does not follow that the final degree of polarization received will be the same as before. In this respect substances may differ, or they may have different "specific inductive capacities," as maintained by Faraday. It is, in fact, just this difference of property that constitutes the difference between conduction and non-conduction, or between the different degrees of conduction or of non-conduction.

So long as the charge of electricity on the surface S, fig. 6, remains the same, the degree of polarization of the surrounding medium, or media, will continue the same; but if the charge be drawn off, waves of decreasing polarization will be propagated outward from S, which will be accompanied by electric waves of translation flowing inward toward S. The entire process that went on within the medium while the body was receiving its charge, will now be exactly reversed.

If we suppose a body to receive a charge of negative electricity, the process will also be reversed; as when a charge of positive electricity is withdrawn. The final result will be that the particles of the surrounding dielectric medium will be positively polarized on the side toward the electrised surface, and there will be a deficiency of electric ether between the surface and the first layer of the particles of the medium.

Conduction.—We have seen that the act of polarization comprises two different processes: a movement of a certain portion of the ether of the atmosphere of a molecule from one side to another, and cotemporaneously with this a discharge of ether from the one side, and an absorption of a corresponding quantity upon the other side. Now the electric constitution of two different bodies may be such that, with the one the same amount of electric movement, from particle to particle, may be accompanied by a feeble degree of polarization, and with the other by a much greater degree. The polarization is a change determined originally by the forces propagated by the universal ether, while the outward flow of the electricity is partly the result of the mutual repulsion between contiguous atoms of the electric ether.

Let us suppose two extreme cases, compatible with our general theory. (1.) Let there be an indefinite line of molecules; such that the density of the ether between them is the same as at the surface of the molecular atmospheres, and suppose a quantity of free electricity to arrive at one end of this line. It is plain that the repulsive energy of this electricity will urge before it a certain quantity of electricity throughout the whole length of the line, and that if the flow be unchecked it can exert little or no disturbing action upon the molecular atmospheres except very near their surface, and cannot polarize them. To do this the moving electricity must have a certain decided tension; that is, be present in a certain sufficient quantity at each point of the line. This would be the case of perfect, or nearly perfect conduction, and there would be little or no polarization of the molecules. (2.) Let us suppose that the density of the electric ether is very feeble between the molecules, there being now no continuous mass of ether to give way before the electricity received at the end, the latter will, as a first effect, exert its repulsive action through the universal ether upon the atmospheres of the nearer molecules and polarize them, and this will extend through the line, and the polarization will determine the discharge in the manner already explained. This would be the case of a non-conducting dielectric medium. In all actual instances of good conduction the case lies between these two extremes. The greater part of the electric movement is confined to the outer envelope of the atmosphere, and the outer portion of the inner envelope; and the polarization resulting from the disturbance of the lower envelope, or the atmosphere proper, is slight. With non-conductors, or poor conductors, the flow near the surface of the lower envelope, around the molecule, resulting from the direct repulsive action (or elastic tension) of the electric ether, is comparatively slight; and the polarization of the lower envelope, with the attendant discharge on the farther side and absorption on the nearer side, is much greater.[6]

To obtain a more distinct idea of the entire process of imperfect conduction, we must take account of a certain resistance which a polarized molecule offers to the flow of ether around it from the negative to the positive side.[7] Let *a*, fig. 7, be a polarized molecule. The center of repulsion of its electric atmosphere will fall at some point, *o*, on the plus side of the center. If we enquire into the effective action of the molecule upon a mass of electric ether posited at *s*, midway between *p* and *n*, if the atmosphere were not disturbed the center of

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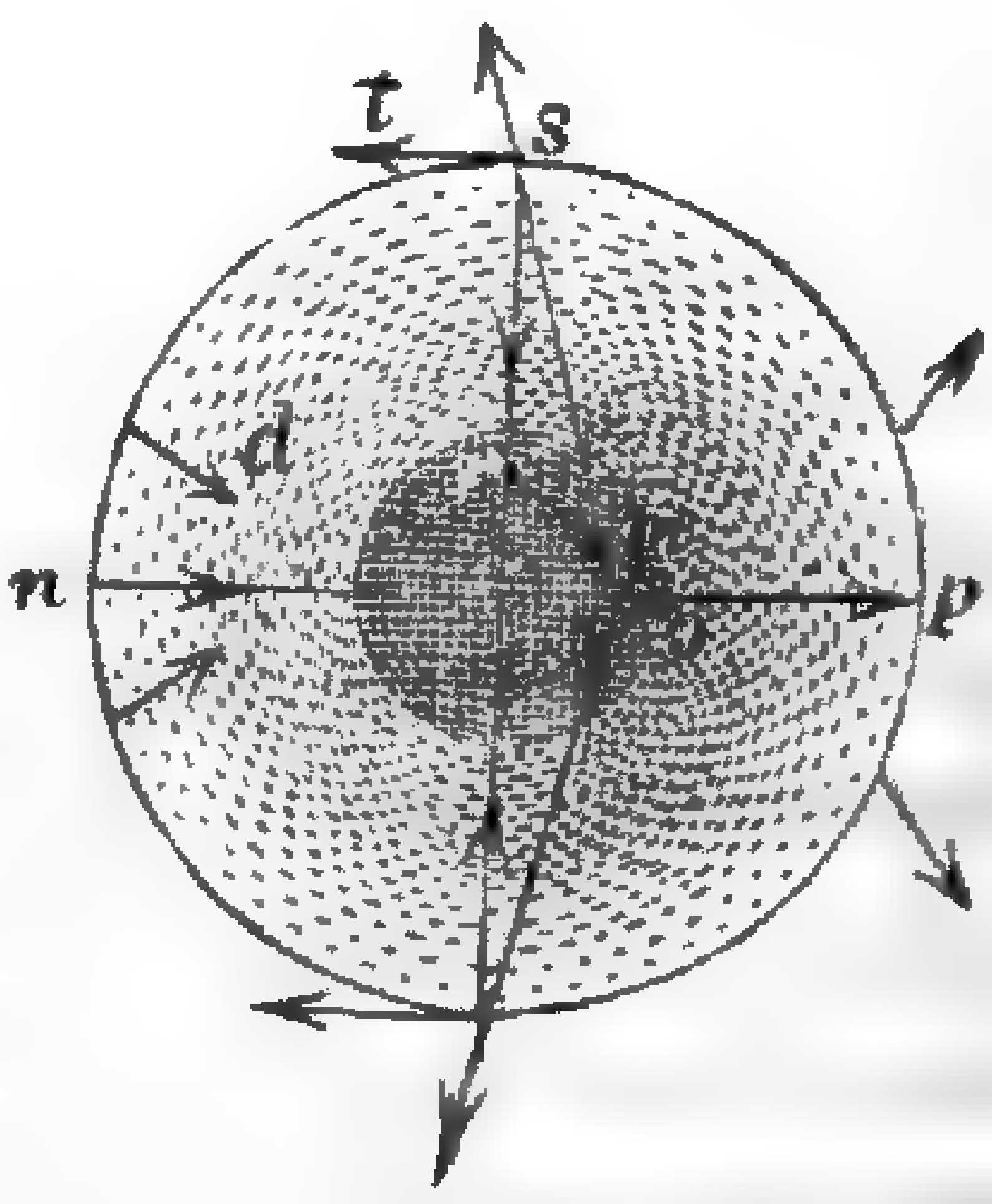

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[6] In the comparison made on p. 216, between the propagation of heat and the propagation of electricity, it is intimated that each is promoted by a polarization of the molecules. This is true only in a limited sense. The circumstances most favorable in general to a conduction of either electricity or heat, are a uniformly dense condition of the interstitial electric ether, which may serve as a medium for the direct propagation of the waves, or pulses, and such a condition of the electric atmospheres of the molecules that the resistance developed by any polarization that may ensue, shall be comparatively feeble. (See p. 246). When, however, the direct conductive action, by the intervention of the interstitial electric ether, is feeble, a greater facility of polarization on the part of the individual molecules would promote the discharge from molecule to molecule.

[7] The terms negative and positive, as here used, have reference to the polarization simply, which is a change in the distribution of the electric ether in the lower portion of the atmosphere. It will be seen in the sequel that in the upper portions through which the free electricity moves, when a conductive discharge occurs, there will be an excess of electric ether, or a positive state, on the side of the molecule which is negatively polarized, and a deficiency or a negative state on the opposite side.

effective repulsion would fall at *r*; in its supposed polarized condition, therefore, it will lie at some point *m* intermediate between *r* and *o*. The two forces exerted upon *s* would therefore act in the direction shown in the figure, and their resultant would act from *s* toward *t*. It will be readily seen that at all points between *p* and *n*, the resultant will have a tangential component directed toward *n*, and a normal component acting outward on the positive side, and inward on the negative side of the molecule. The density of the electric ether at *n* should therefore be greater than at *p*; and while by its elastic tension it is urged around toward *p* it is resisted by the tangential force just spoken of. And so if the electric ether above *n* is urged by the repulsion of free electricity on that side of the molecule around toward *p*, it has to overcome this resistance. It is to be observed that the attractive or compressing action about *n*, is but the result of the repulsive energy of the free electricity which has determined the polarization.

The outer circle in the figure represents the surface of the undisturbed atmosphere (inner envelope). When the molecule becomes polarized the surface of its atmosphere takes, or tends to take, the spheroidal form, with the center of figure lying on the side negatively polarized. At points near this surface, about midway between *p* and *n*, and indefinitely beyond the surface, there will be a resultant force, as shown in the figure, tending to check the flow of free electricity around or past the molecule, in the direction of *n* toward *p*.

It does not follow because the lines of direction of the attraction and repulsion meet at *s* under a small angle that the resultant *s t* may not be equal to, or greater than the force of tension of the free electricity that may reach the molecule, since the absolute intensities of these forces must be very great. Besides, whatever may be the intensity of this resultant, it must be of a corresponding order of magnitude with the tension of free electricity, since it is precisely the resistance exerted by the polarized molecules of air contiguous to an electrised surface, that determines the tension of the statical electricity collected upon it.

When a discharge takes place through an imperfect conductor, as rarefied air, the contiguous particles of air are first polarized by the repulsion of the electrised surface, propagated by the universal ether. This determines the escaping electricity to fall upon their nearer negative sides; the molecular resistance considered above then comes into play, and if the elastic tension of the electricity is sufficient to overcome this resistance, a conducting discharge takes place; otherwise the electric movement can only be after the manner explained on p. 243, that is, as in the process of induction. According to these views, the greater resistance offered by dense than rarefied air, is to be explained by the smaller number of resisting particles encountered, together with the diminished resistance of each particle. For it will be evident, on refering to fig. 7, that if the molecular atmosphere expands, as it will if the air be rarefied, the resistance at *s* will be diminished. Thus air rarefied to a certain degree should become a conductor of electricity. For a similar reason the surfaces of solid, or liquid bodies, should conduct electricity better than the internal portions, since the molecular atmospheres are more expanded there.[8] It will also be observed, in view of the results obtained, that imperfect conduction and non-conduction depend upon certain effects resulting from the polarization of molecules, and that non-conductors may become imperfect conductors if the tension of the charge presented to them be sufficient. But it should not be overlooked that the amount of polarization induced, and therefore of the resistance resulting from it, must depend upon the condition of the interstitial electric ether (p. 243), and the distance between the molecules; since the interval of time in which the repulsive impulses propagated by the universal ether, may come into action before the electric ether impelled from the one molecule to the next shall begin to take decided effect, must depend upon these two particulars. The quality of conduction, or non-conduction of a substance, should therefore depend to a certain degree upon the mode of aggregation of the particles; more especially as different groups of particles, or compound molecules (p. 241), may offer different degrees of resistance.

If an insulated metallic ball, *a*, be placed in good conducting communication with the ground and afterward with the charged prime conductor of an electrical machine, the electricity will flow freely along the route thus opened, and the tension of the ether passing over *a* being very feeble there will be no perceptible outward movement from *a* through the surrounding air, and no sensible polarization of the particles of air; but this will no longer be the case if the charge be too large to be conducted off with facility. The effect of connecting the prime conductor with the ground when the machine is worked is to allow a free escape to the ether, which would otherwise pass toward the first layer of air particles, and displace an equal quantity from each layer in succession and transfer it to the next.

Electric Spark.—If we suppose the insulated metallic ball, *a*, still connected with the prime conductor, to be placed near another ball, *b*, which is in communication with the ground, on working the machine the ball *a*, in receiving its charge, will give rise to electric waves proceeding outward from it (p. 243), and at the same time polarize the particles of the surrounding

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[8] Surfaces of non-conductors resist the flow of free electricity after the same manner as the internal mass, that is, by their molecules becoming polarized.

air. As a consequence, a certain portion of the electric fluid on the nearer side of *b*, will be urged away and pass into the ground. This side will thus be electrised negatively by induction, and react upon the air particles between the two balls, increasing their polarization, and upon the nearer parts of *a*, drawing more electricity to them. All these changes will be attended with increasing wave movements, and increasing discharges from one particle of air to another along the line between the nearest points of the balls. The electric ether in transitu between the air particles on this line, may thus come to have sufficient density to establish a certain degree of conducting communication between the two bodies, and so to convey a sudden conductive discharge along this line. This result will be partially due to the lateral expansion, which the free electricity received by the air particles on the line will occasion in their electric atmospheres. There will be two causes in operation to produce this effect, the pressure of the stream of ether passing from one particle to the next, against the atmosphere upon which it falls, and the mutual repulsion of the particles that will thus become momentarily overcharged. We have seen (p. 247) that such lateral expansion would give rise to a diminution in the resisting force of the polarized molecules.

As the positive electricity which thus passes over to *b* spreads over its surface, it partially neutralizes its negative state, and thus tends to check the flow and interrupt the passage of the spark. There is also a sudden diminution in the tension of the electricity received by *a* from the prime conductor, which is another cause of this interruption. Apparently another cause conspiring with these two is a reaction to the sudden lateral expansion above mentioned. If *a* were previously charged and insulated, and *b* brought continually nearer to it, the mutual inductive action of the two balls upon each other would initiate the electric movement above alluded to through the intervening air, which would finally result in the passage of a spark.

The light of the spark results in part from the vibratory movements in the atmospheres of the air molecules, attendant upon the discharge. (See p. 219.) Experiments by Riess, Masson, and other physicists, have conclusively established that the electric light is partially due to the passage of highly luminous metallic particles from the positive to the negative surface. The detachment of such particles, as one result of the discharge, may be explained by the discharges, or flow of electricity that must take place directly through the ball *a*, when the conductive discharge from *a* to *b* occurs. This follows from the fact that the electricity within the ball is no longer in equilibrium under the action of the electrical shell that surrounds it, and the impulses conveyed by the electric current should tend to detach the par ticles at the end of the metallic line through the ball. The luminosity of the detached particles is to be ascribed to the vibratory movements imparted by the discharge to the electric atmospheres of the particles.

Excitation of Electricity.—There are various special modes of exciting electricity, but they are all only so many different modes of polarizing contiguous molecules; or, more comprehensively, of effecting certain disturbances in the equilibrium of electric atmospheres. The different special causes of disturbance in these cases may all be traced to some action of the molecular forces, or of heat; and, as we have seen (p. 64), heat is also, in its primary origin, one of the molecular forces.

Frictional Electricity.—The surface of one body should not in its natural condition exercise any sensible direct electric action upon that of another, unless they are brought into close proximity; certainly much nearer than in any ordinary case of mere contact. (See p. 240 and Table I.) When, however, two dissimilar surfaces are *pressed* together, especially if they are brought within the limits at which a force of adhesion would come into operation, the unequal molecular forces of the dissimilar particles, might, as will be seen in another connection (p. 250), give rise to a polarization of the particles. The development of such polarization should establish a wave movement entirely through the two bodies (p. 242), and as the result, the surface toward which the flow occurred might have an excess of free electricity while it would be negatively polarized (p. 246); that is, the outer envelopes would, on the outer side of the molecules, have an excess of electricity, and on the inner a deficiency. The reverse would be true of the other surface. This is the probable explanation of the electrical excitement which may be obtained in a feeble degree, by simple *pressure*, as in the experiment by Haüy, of pressing the smooth surfaces of fragments of calcareous spar, quartz, &c., between the fingers. But the process of excitation by friction seems to be different. The most notable distinction to be perceived between the act of rubbing and that of pressure, is, that in the former alone the protuberent parts of the two surfaces are pressed against each other laterally. The probability, then, is that this lateral pressure of the surface particles is the immediate cause of the development of electricity by the rubbing of one surface over another. Now it is easy to see that this pressure should tend to compress the electric atmospheres of the particles laterally, and so to produce an accumulation of electric ether on their outer sides, and a discharge from thence toward the other surface. If the surfaces be dissimilar in nature, or condition, the effects of this sort of action should be unequal, and the result should be that upon the one surface there would be an excess of electricity, and upon the other a deficiency.

The particles of each body, if non-conductors, should also become polarized, after the same manner essentially as already explained (p. 242), and in this condition would serve to retain the surfaces in their disturbed state (p. 245–6). The explanation here given is sustained by the effect of heat when applied to one of the surfaces, which is to dispose that surface to take the negative state, and in fact the heat-pulses should expand the molecular atmospheres (p. 73) and tend to produce a discharge of electric ether from their outer sides. The like tendency of roughness of surface may be explained in a similar manner.

The tension of the electricity obtained should depend upon the degree of polarization which the surface molecules are enabled to retain, for upon this will depend the limit of the resistance which they are capable of offering to the free flow of the electricity from molecule to molecule (p. 246). It should therefore be independent of the velocity, the pressure, and the extent of the rubbing surfaces, as established by Peclet. The electrical state of the surface, whether positive or negative, must depend upon the mutual relations of the surfaces, and the comparative energy of the disturbing force in operation upon each (p. 249).

Voltaic Electricity.—In entering upon this topic, we must first endeavor to obtain, from our present point of view, an insight into the nature of *chemical union.* When two dissimilar molecules, *a* and *b*, are brought into close proximity, the effective action of *a* upon the atmosphere of *b* may be quite different in amount from that exerted by *b* upon the atmosphere of *a.*[*] As a consequence, the relative condition of these atmospheres may be disturbed; in other words, they may become polarized. As the act of polarization proceeds, an electric current, or series of waves (p. 243), will pass through the two molecules, and at the same time they will approach each other. As this approximation continues the density of the electric ether between them will increase (p. 241, fig. 5,) and a true electric or conducting union may thus be established between them. Under these circumstances, it would seem that finally the non-conducting resistance offered by the polarized molecules (p. 245) should be in a great degree overcome, a comparative equilibrium of tension be established between the outer envelopes of the two atmospheres, and, as a consequence, the polarization materially decrease if not ultimately disappear. In this event the final union would be due chiefly, or entirely, to the molecular attraction proper, as in the case of similar molecules. It is in this final

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[*] A disturbing action from the molecular attraction, may even come into operation when the molecules are beyond the range of effective attraction, that is beyond Oc, Fig. 1. For it is to be observed that in the action of each molecule upon the atmosphere of the next, the attractive impulses prevail over the repulsive, beyond Oc as well as between Oa and Oc.

condition, essentially, that we suppose a particle of water to exist. The two constituent molecules, oxygen and hydrogen, are not polarized, or but feebly so, and they are, to a certain degree, *in conducting communication with each other.*[10]

In developing the theory of the voltaic current, let us cônfine our attention to the case of a single cell, consisting of water, or acidulated water, between a plate of zinc and a plate of copper. We must suppose that the first effect is a mutual polarizing action between a molecule of zinc and one of oxygen, the former being brought into the positive state and the latter into the negative state, upon their contiguous surfaces. The attraction thus developed will arrange the oxygen, with its associate hydrogen molecule, on a line normal to the surface of the zinc plate. The farther surface of the molecule of hydrogen will be brought to the same positively polarized state as the zinc plate, and will act in a similar manner upon the next particle of water; and so on from one particle to another until a complete chain of polarized molecules extends to the copper plate.

In this chain, as first established, we regard each particle of oxygen as in the negative state on the side nearest to the zinc, and each associate particle of hydrogen as in the positive state on its farther side; or at least that they are brought essentially into this condition, and that the true polarization of the contiguous sides is comparatively feeble, by reason of the conducting communication between them, resulting from the condensed state of the electric ether by which they are electrically connected (p. 241). Not only does the positive repulsion that originates at the zinc plate establish, by induction, a chain of polarized water particles, in which the farther, or hydrogen side, is in the same positive state as the zinc, but it also tends to increase the density of the electric ether posited between the oxygen and hydrogen of the individual water particles of this chain, and so to urge them asunder.[11] Before the closing of the circuit, while the mutual

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[10] It is not absolutely essential to the explanation of the voltaic current that the two molecules, when combined, should be regarded as devoid of polarization.

According to the received theory of the constitution of a molecule of water, we must regard the molecule of hydrogen that combines with a molecule of oxygen as compound, and composed of two simple molecules (p. 241, fig. 5); but this in no degree effects the explanation to be given, for the compound molecule, as it is not decomposed, comports itself throughout essentially as a simple molecule would under like circumstances. It is to be observed that the process of polarization above considered does not occasion an excess of electric ether upon the entire molecule of the one substance, and a deficiency on the entire molecule of the other; since when a molecule becomes polarized it absorbs upon the one side the same amount of electric ether that it gives off from the other side (p. 242).

In all these remarks the term molecule is used in the same sense as heretofore.

[11] If the constituent molecules of each water particle were not in conducting communication, then the action transmitted along the line would serve to polarize these molecules and thus to bind them more closely rather than to separate them. This objection seems to hold against Schönbein's theory.

polarizing action between the zinc and oxygen is in continual operation, waves of positive electricity spread in an indefinite series, from the zinc plate through the cell and all the media on that side of the plate (p. 243). A corresponding series of negative waves spread in every direction through the zinc plate and the media lying without it, the electric movement in these being toward the zinc plate. When the two plates are provided with wires leading away from them, we have evidence of this wave movement, and of the polarization that has attended it, in the positive and negative states of the ends of the wires. Now if the ends of these two wires be brought together, the entire series of waves which pass through the copper and zinc plates are condensed, so to speak, upon the wires, and pass through the circuit. The entire quantity of electricity that would be dissipated from the copper plates is thus brought around to the zinc plate again. The arrival of this electricity intensifies the polarizing action going on at the zinc plate, and hastens the union of the zinc and oxygen molecule. It also determines, if not before established, the completion of the line of polarized water-particles traversing the cell. As soon as this takes place, the waves that before spread through the cell are converted into linear currents. At the same time, the electricity discharged from the zinc to the oxygen passes over by conduction to its associate hydrogen molecule, and by its impulsive and repulsive action urges the latter over to the next particle of oxygen in the chain. This particle of hydrogen, with its charge of positive electricity thus received, acts upon the second particle of oxygen in the same manner that the zinc acted upon the first, and so on throughout the chain. As the detached hydrogen particle is made, by the same force which detached it, to attract the next oxygen particle more energetically, there may be no material movement of the common center of gravity of any of the pairs of particles that are separating or uniting.

The explanation of the voltaic current that has now been given seems to accord with the established laws and phenomena of the current. The primary *electro-motive force* must consist in the energy of the natural polarizing, or chemical action,[12] exerted between the zinc and the oxygen molecule, diminished by any opposing action of the same nature that may be in operation at the copper plate. It follows from the principles of induction laid down on p. 243, that the quantity of electricity in circulation, or the *intensity* of the current, must be the same at all points of the circuit. The period of time which the zinc and oxygen particles occupy in combining should be proportional to the length of the entire circuit, supposing it to be of the same

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[12] This chemical action is intensified by the coöperative polarizing action of the sulphuric acid.

material and cross section throughout; actually should be proportional to the "*reduced*" length of the circuit. During this period all the electricity set in motion by the union of the two particles should pass through the circuit; or more strictly, be urged forward past each point of the circuit, in electric "waves of translation." The quantity of electricity that moves forward in a given time should then be *inversely proportional to the length of the circuit*, other things being the same. The reason that the quantity of electricity, or the intensity of the current, is *proportional to the area of the cross section of the wire* is, probably, that the number of points of the zinc plate which are contemporaneously in action, with the same degree of energy, would be proportional to this cross section. The *tension* of the electricity circulating in the current should be the greatest where the velocity of the individual particles of the ether is the least. Possible retardations result from the electric relations of contiguous molecules in the line being such that they become more or less polarized, and so offer a resistance to the free flow of the electricity (p. 245); besides that the process of polarization is attended with a retardation. The degree of polarization that exists at any point of the current serves as a measure of the "*resistance*" experienced by the current there.

If an *electrolyte* be disposed between the ends of the wires, the theory of its electrolysis is similar to that of the decomposition of the water in the cell. The only difference is that the ends of the wires are brought by the electro-motive force into the same positive and negative states, which the natural chemical action in the cell determines upon the zinc and copper plates.

When two or more cells are employed, the natural polarizing action at each zinc plate should be enhanced, and the tension of the free electricity at the ends of the wires of the broken circuit should be augmented. Hence there should be a more energetic force to polarize and decompose an electrolyte interposed between the ends of the wires. But it does not follow that when only good conducting wires are employed to complete the circuit the intensity of the current will be augmented by increasing the number of cells; since the principal retardation of the flow occurs in the cells, and this increases in the same proportion with the number of cells. (See Pouillet, *Elements de Physique*, ii, 732.)

The *heat* developed in the voltaic current is to be ascribed to the impulsive action of the electric ether moving in it upon the universal ether. Currents, or waves of translation, are thus developed in this ether, which fall upon the central atoms of the material molecules in the circuit, or the dense ether surrounding these atoms. The impulses thus received are given off, or pass into the molecular atmospheres, one after another, and are finally radiated off as heat-pulses. The explanation is the same as that of the molecular absorption, and subsequent radiation of the ethereal pulses of radiant heat, already given (p. 215). When the condition of things is such that the particles in the circuit become polarized, a greater amount of heat should be developed, because a part of the electric movement within the molecular atmospheres, which was before confined to their upper portions, now occurs at greater depths, where the universal ether is more dense. Thus, when the resistance to the passage of the current becomes greater, more heat is developed. Heat may also be evolved, under special circumstances, as a consequence of a compression of the molecular atmospheres, produced by the current.

We shall see, in the remaining portion of this memoir, that it is to these same *impulses of the moving electric upon the universal ether* that are to be ascribed all the external actions of the current; as attracting or repelling wires conveying currents in the same direction with the given current, or in the opposite direction,—giving motion to the magnetic needle,—developing magnetic, or diamagnetic currents, in the compound molecules of adjacent masses,—and inducing currents in wires, or metallic bodies, in the vicinity.

[To be continued.]

ART. XXVIII.—*On the combination which takes place when Light of different tints is presented to the right and left eye;* by Prof. OGDEN N. ROOD, of Columbia College.

IN 1806, de Haldat stated that when differently colored glasses were held before the two eyes, a combination of the two tints took place in the brain, and that the resultant impression was the same that would have been produced by mixing the two tints together, and presenting the compound color to a single eye. These experiments were repeated by many good observers without success. In 1841, with the aid of a stereoscope and polariscope, Dove confirmed the general correctness of de Haldat's conclusion. In 1846, Seebeck, and, in 1849, Foucault and Regnault arrived at the same result.

The testimony of these observers has then proved that a combination of the two sensations does take place in the brain, and that the resultant impression is, in the cases they examined, generally similar to that which would be produced by the presentation of the two tints to a single retina: but with what exactitude the resultant tint obtained by the binocular method agrees with that produced in the ordinary way by rapid rotation, or by

Helmholtz's method, or how far it is in point of fact possible to predict, by the binocular method, what the result of the true mixture of different tints willl be, is a question which, so far as I know, has never been studied.

The following set of experiments was undertaken for the examination of this matter.

The first method pursued was as follows: Light of different tints, complementary or not, was presented to the right and left eye, and the resultant tint gained by binocular vision was noted: afterward, by the method of rapid rotation the two tints were mixed and presented to a single retina. In many cases the results obtained by the two methods were nearly identical, but in others there was found to be a considerable variation, so that often it was impossible to predict by the binocular method what the exact resultant by rotation would be; it sometimes being of a tint which was not at all present to the eye or mind during the binocular union.

An open lenticular stereoscope was provided with a white card board, in which were cut two square apertures one half an inch in diameter, they being placed at such a distance apart that their binocular union readily took place. The ground glass was removed, and the stereoscope held horizontally in front of a rotation apparatus to which was attached a circular card-board disc, with its two halves painted in different colors. The disc remaining at rest, the dividing line of the colors being vertical, light of different colors entered the two apertures in the card-board, and after their binocular union had been effected, the resultant was noted: the disc was then set in rotation, the stereoscope remaining in its position, and a true mixture of the two tints was obtained and compared with the first result.

Helmholtz has shown that vermilion represents the red of the solar spectrum up to the line C, that red lead answers to a portion between C and D, but not reaching up to D; also, that the pure yellow portion of the spectrum is imitated by the chromate of lead, the less refrangible blue by Prussian blue, and the more refrangible blue by ultramarine. Accordingly discs were prepared with these substances, and, in addition, some others were used: viz.: a disc colored with a bright and pure yellow, a little more refrangible than chrome yellow, but still not at all greenish yellow—its tint was about that of 107·7, in Kirchhoff's chart: for the production of a greenish yellow disc, gamboge, with a minute portion of Prussian blue, was used—its tint was that of 113·2; more of the latter color, with gamboge, gave a good greenish-blue disc. Red discs, slightly purplish, were used; also discs colored with emerald green, 170· in the chart. In addition discs of a golden yellow, being some of the same paper I had previously used in the stereoscopic imitation of the luster of gold—its tint was 102·7; and finally purple discs were made by the mixture of ultramarine and crimson lake.

Comparison of the results obtained by the binocular and actual combination of different tints.

1. When one-half of the disc was covered with vermilion, the other with emerald green, the binocular combination was rather difficult, and the result was sometimes a red gray, sometimes a green gray; but when the disc was set in rotation, its true resultant tint was found to be a decided green, which could not have been predicted from the binocular union. That red and green should give a decided green by rotation may seem singular, but it must be remembered that I was not able to vary the *proportion* of these two tints, *each half* of the disc necessarily being of the same tint. In this case then, it merely happened that the intensity of the green was such that after a portion of it had been neutralized by the red, enough remained to give a strong green coloration to the disc. Similar results, given below, will be passed over without notice: they really furnished a delicate test for deciding whether the binocular resultant agreed with that produced by rotation, *preconceived notions* of complementary colors being thus avoided, as it was impossible to know beforehand what the result would be, when the rotation experiment was tried with *equal* surfaces of the colored papers. In several cases the tints on equal portions of some of the discs nearly exactly balanced each other, so that the resultant tint by rotation approximated to a white or neutral gray.

2. Vermilion and ultramarine gave with some ease the *same* tint by the two methods of combination; viz., a red purple: the tint was however rather more red by rotation than would have been expected from the binocular combination.

3. Vermilion and yellow (107·7) gave by rotation a tint containing much more yellow than would have been expected from the binocular examination.

4. Vermilion and purple gave the same tint in both cases, viz., a red purple.

5. Vermilion and black gave approximately the same result in the two cases; the disc was however less red by binocular vision than by rotation.

6. Vermilion and white gave the same result in both cases.

7. Orange, (red lead,) and purple gave a good approximation to the same result in the two cases; the tint was more red by rotation than was expected.

8. Orange and black gave a more neutral tint by binocular vision than by rotation.

9. Orange and white gave the same tint.

10. Chrome yellow and emerald green (170·) gave the same result, viz., a yellowish green.

11. Chrome yellow when combined with black by rotation gave a more decided yellow than would have been expected from the stereoscopic union of the two components.

12. With white the tint was the same in each case.

13. Emerald green with white gave the same tint in both cases; with black the tint was not nearly green enough in the binocular union.

14. Ultramarine with white or black gave about the same result in each case.

15. Purple and yellow (107·7) by rotation gave a decided yellow: the resultant impression from binocular vision was much more neutral.

It will be noticed that in all these experiments the colors of the discs were such either in intensity or nature that by rotation a neutralization was not to be expected; I now pass to those cases where the tints were complementary or approximately so.

1. Golden yellow and Prussian blue gave about the same tint in the two cases, viz., a nearly pure gray.

2. Vermilion and greenish blue gave approximately the same tint in both cases, viz., a gray slightly purplish: the tint by binocular vision was a little more neutral.

3. Greenish yellow and purple gave sometimes the same tint in both cases, viz., a gray with a tinge of green.

4. Emerald green and purplish red gave by rotation a good neutral gray, but I could not, by binocular vision, get exactly the same result, the tint of the resultant being either too red or too green. Both of the original colors used were very pure and bright.

Lastly, I give two combinations on which many experiments were made, viz., first, chrome yellow and ultramarine. By the stereoscopic union of the components a pure gray was obtained, or at least a gray without any tinge of red, but by rotation the color was of reddish copper hue, which could not have been anticipated in using the other method of combination. Also, yellow (107·7) and ultramarine often gave in the binocular method a neutral gray, while the true union of the colors gave a flesh tint.

It will be seen from these experiments, that while a combination does in reality take place, yet that the resultants in the two cases may very considerably differ, and farther, that there is a tendency in the binocular method to consider the resultant impression a neutral one, when the method by rotation shows that this is not truly the case. This is illustrated in a striking manner by an experiment previously published by me, which I have lately repeated a number of times with the same result. A yellow glass is held before one of the eyes, a blue glass before the other, when, as Brücke rightly states, a landscape thus viewed, seems of a neutral gray if the attention be equally directed to the two impressions. When, however, the two glasses are attached to opposite openings in a blackened card disc, and set in rapid rotation so that the tints are truly mixed and a true resultant obtained, this is found (with the glasses I employed), to be a strong purple, though nothing of this kind is seen or suggested in the binocular use of the glasses. It is probably this tendency to consider the resultant neutral, that makes it so easy to combine, in binocular vision, tints produced by polarized light, and to perceive that the resultant is white.

It may be remarked in passing, that when the stereoscope is held as described before the two halves of the colored disc, and the latter is caused to revolve slowly, that the aperture in the card board appears extraordinarily lustrous, so much so that it would be possible by a little artistic arrangement of the accessories to deceive persons with the belief that they were looking at the surface of a polished metallic mirror.

Binocular union of complementary tints produced by polarized light.

For the purpose of experimenting on this point, I arranged a binocular polariscope, consisting of a large plate of polished glass resting on a black cloth, two Nicols' prisms being fastened before the two eyes in the right position, the principle section of one of them making an angle of 90° with that of the other. As objects, plates of selenite were used, fastened behind apertures in black card board. Films of common mica cannot well be employed on account of the rapid change of the tint by the least inclination. As the colors were exactly complementary, the resultant by binocular vision should always be pure white.

Results with polarized light.

1st, Red with greenish blue of 2nd order combined with some readiness to make a white. The same was true of these tints in the 3rd order; here the resultant white was *very easily* obtained. The same tints in the 4th order gave an excellent result; the components were of course pale, but the resultant white was proportionally steady. So also with the same tints in the 5th order.

2nd, Golden yellow with its complementary blue of the 2nd order, gave a good result. The same tints of the 3d order also readily neutralized each other.

3rd, Yellow and indigo blue of 1st order gave easily a very good white. The same tints in the 2nd order united with more difficulty. Union was about the same in 3rd order.

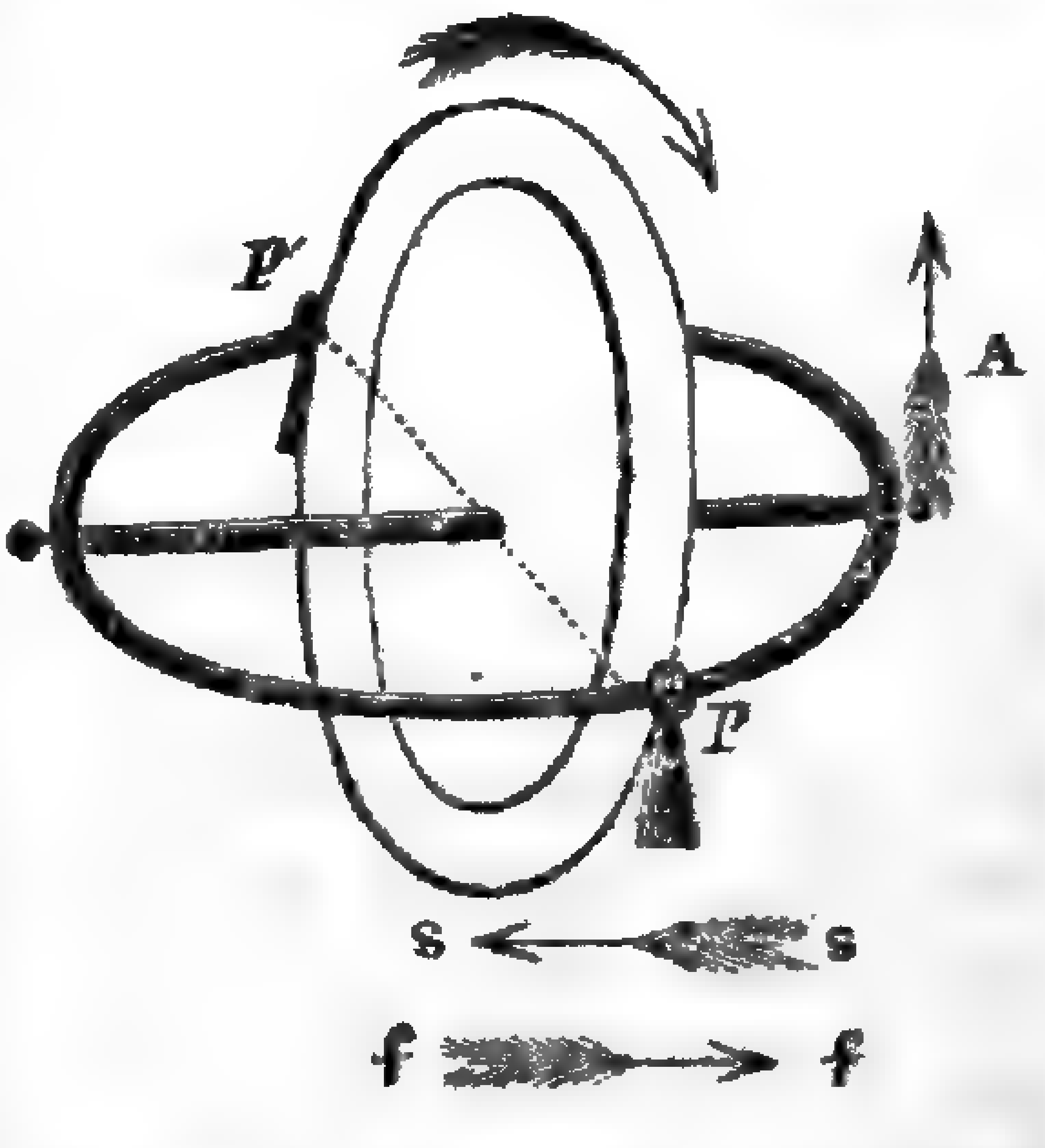
4th, Greenish yellow and violet of 2nd order combine to a white with some difficulty. The same is true of these tints belonging to the 3rd order.

5th, Green and purplish red of the 2nd order, united with moderate ease. The same tints in the 3d order united with great ease, and the resultant white could be retained very steadily.

From the above it will be seen that the union most easily takes place when the complementary tints have a low intensity. The same result can also be produced with the colored discs by shading them, when the two impressions much more readily fuse into a neutral single one.

ART. XXIX.—*On an experiment with the Gyroscope;* by Prof. O. N. ROOD, of Columbia College.

IF a gyroscope be suspended in equilibrium by pivots at *p* and *p'*, and the disc be set in rotation in the direction indicated, and an attempt be made to cause it to rotate about a new axis *p' p*, in the direction shown at A, the disc will tend to turn in the direction *s s*, and if the pivots *p p'* are immovable, pressure against them will be generated in the direction *s s*, which will continue till the disc has been rotated through an arc of 90°. If the rotation around the new axis *p p'*, be still continued, the pressure on the pivots *p p'* becomes reversed, and the disc tends to move in the direction *f f*, and retains this tendency till it has passed over an arc of 180°, when this tendency is again reversed and becomes the same as in the first case, its sign not being changed till the disc has passed through an arc of 180°.

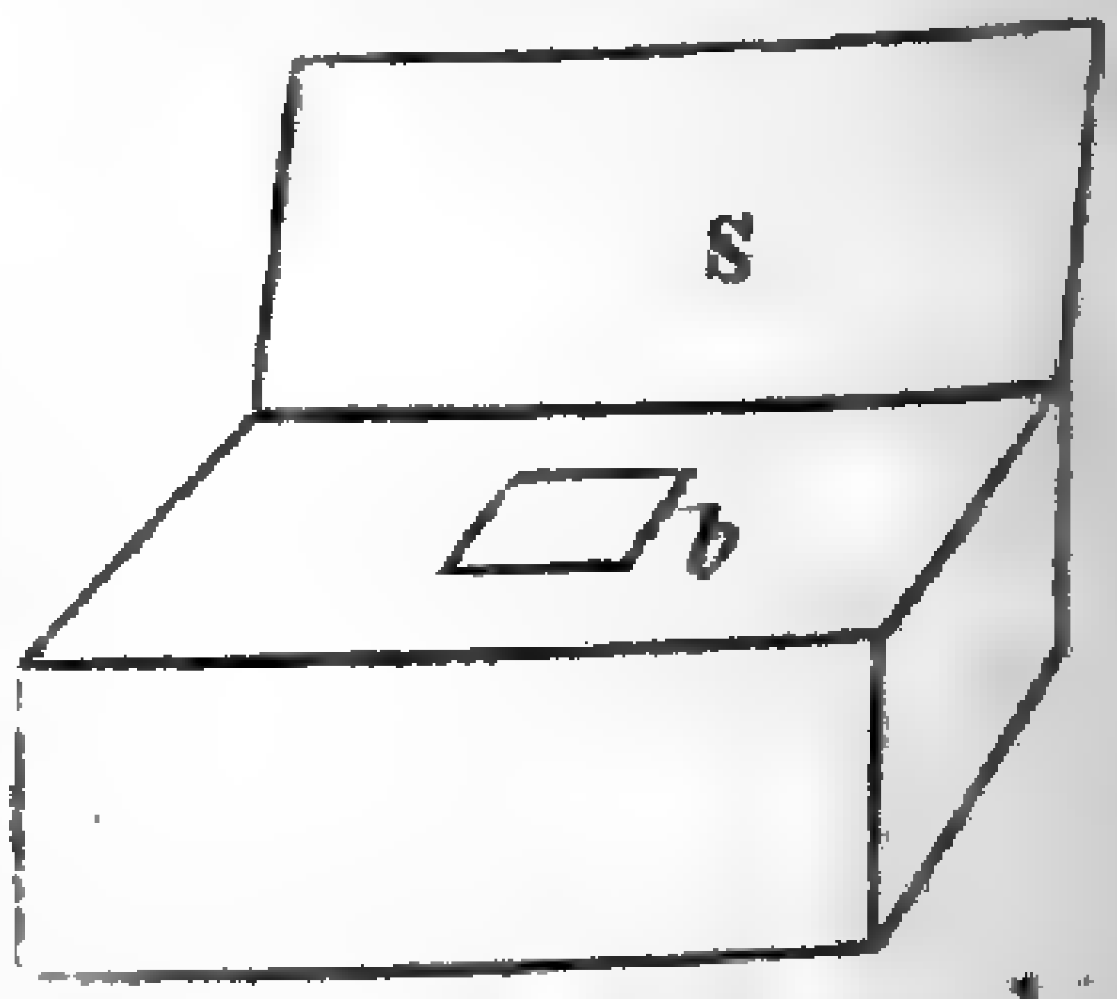


Hence, during the entire revolution around the new axis *p p'*, a varying pressure is exerted on the fixed pivots, alternately to the right and left hand.

It occurred to me that by causing a gyroscope, suspended in this way, to rotate *rapidly* about a new axis as *p p'*, its original motion might thus be destroyed in this indirect manner. A gyroscope was selected, which, when set in rotation by a certain force and allowed to remain undisturbed, continued in motion for 16 minutes. It was connected with a set of multiplying wheels, and the instant after it had been started in rapid rotation, it was forcibly caused to revolve about the new axis *p p'*, at a rate of forty revolutions per second. The resistance experienced was considerable, such as in two cases to eject the disc from its mounting, and the motion of the disc was found to be arrested in about twenty seconds, or in the $\frac{1}{48}$th portion of the time it otherwise would have occupied.

ART. XXX.—*Description of a simple apparatus for producing lustre without the use of Lustrous surfaces or of the Stereoscope;* by Prof. OGDEN N. ROOD, of Columbia College.

IN attentively looking at plane polished surfaces of aventurine glass, I have often been for a moment unable to determine by binocular vision exactly where some of the imbedded crystals were placed, they seeming for an instant to be suspended in the air above the glass. This uncertainty depends on the fact that it frequently happens minute crystals will reflect bright beams of light to one eye but none to the other, so that one of the binocular components is wanting. The apparatus described below was contrived so as to produce the same effect even to a much greater degree, as well as to show the production of simple lustre.



A box was constructed three inches long, one and a half inches broad and deep; this was blackened, and provided with a black screen at S; the side directly under S was left entirely open, and allowed the light from a window or candle to enter. At *b* was an aperture $\frac{1}{2}$ inch square, which was covered by a black card-board or piece of blackened brass foil, in which a hundred or more pin-holes, or better, minute triangular apertures, were punched. Directly below this on the blackened surface of the floor of the box, were sprinkled about a hundred small pieces of white, red, and green paper, each being about $\frac{1}{15}$th of an inch square. When thus arranged and properly illuminated, many of the apertures appeared lustrous, like small brilliant grains of sand, from the reason that, owing to the construction, light of different intensity or color was presented to the two eyes.

If now a second blackened diaphragm of brass foil, perforated with many minute triangular apertures, carefully prepared so as not to have an indented surface, be placed above the first set of apertures, at a distance of $\frac{1}{2}$ or $\frac{1}{4}$ of an inch, brilliant points of light are seen either by one eye or the other, light from the same opening rarely reaching both eyes. It now becomes impossible to decide on the location of these points, and they often seem suspended in space, somewhere in the interior of the box, but exactly where the observer cannot determine.

ART. XXXI.—*Remarks on the Beatriceæ, a new Division of Mollusca;* by ALPHEUS HYATT, JR.

DURING an expedition to the island of Anticosti in the summer of eighteen hundred and sixty-one, undertaken by Mr. A. E. Verrill, N. S. Shaler and myself, and prosecuted under the auspices of the Museum of Comparative Zoology, in Cambridge, numerous specimens of Beatriceæ were collected at English Head, West Point, near Junction Cliff, on the shores of Ellis Bay, and at several points between Salmon River and Fox Bay. I have satisfied myself by a careful inspection of this large collection during the past two years, that these remarkable remains are not tree-like fossils, as originally described by Mr. Billings of the Canada Geological Survey, but a new and interesting order among Mollusca closely allied to the Orthoceratites.

I have in course of preparation a monograph upon this new order, but, as unforseen difficulties will probably delay its completion, I hope that even so slight a sketch as is here given of the most prominent characters may not prove uninteresting.[1]

The Beatriceæ are long cone-like bodies, composed of three distinct parts, or layers, in the following order: (1) A central chain of small hollow chambers; (2) a succession of concentric coniform layers; (3) an external or sub-epidermal layer.

(1.) The central chambers are imperforate, generally deeply concave, and set one upon another like a pile of Chinese teacups. The tops, as also the bases of the coniform layers, are turned toward the larger end of the fossil, and completely closed by the bottoms of the succeeding cavities. They invariably occupy the axis of the encircling coniform layers, and have thin and sharply defined walls, resembling the fossilized nacreous septa of an Ammonite or Nautilus, and are not continuous with each other or with the adjoining coniform layers.

These cavities present many variations of form in the same individual, especially in *B. undulata*, sometimes in this species even becoming reversed, convex instead of concave, or broken up into several minor chambers. Such irregularities, however, are merely local, arising from mutilation or other disturbing

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[1] Although any adequate acknowledgment of the kindness shown to the members of the expedition by Prof. Louis Agassiz, Sir Edmund Head, then Governor-General of Canada, and Mr. George Ticknor of Boston, all of whom did their utmost to aid us in accomplishing our designs, would be more appropriate in the monograph alluded to above, I cannot refrain from availing myself of the present opportunity to express our sincerest thanks for the sympathy and assistance we received from them. My personal thanks are also due to my father, Mr. Alpheus Hyatt, Sr., of Baltimore, Md., to whose generosity I owe the privilege of attending the expedition.

causes, not affecting any individual as a whole. In *B. nodulosa* they remain, with but few exceptions, constantly concave, and have regular forms.

(2.) The coniform layers are separated in fully grown specimens by intervening spaces near the central line of cup-like cavities, which become gradually shallower upward and outward, and finally disappear between the closely appressed bases of the overlapping layers.

In consequence of the great length of the cones or layers, the upper or narrower portions of the intervening spaces in *B. nodulosa* are parallel with the external surface, and appear as narrow streaks of blue limestone. In the young of *B. undulata* and *B. nodulosa* the intervening spaces are not present, but the layers are easily distinguished and have the same abrupt cone-like terminations as in the adult. The cones are formed of numerous thin laminæ, whose surfaces being but partially in contact, form oblong, angular, interstitial spaces or cells, with two concave, and one convex side, the latter always turned outward. They are largest near the center, giving a porous, spongy aspect to that part of the cone immediately around the axial cavities, but become gradually smaller outward until they are not distinguishable by the naked eye between the closely packed laminæ.

(3.) The sub-epidermal or outer layer is of a dark brown color, and in well preserved specimens is ornamented and continuous without annular marks or furrows.

It was probably the structure of the overlapping partitions or cones, and the parallelism of their upper parts, which in a transverse section closely resemble annular rings of growth, that led Mr. Billings to class Beatriceæ with plants. The cellular structure is, however, not due to the presence of true cells pervading the substance of the cones, but to the regularly formed interstices, described above, between the thin laminæ of which they are composed; and the annular appearance is consequent to the overlapping of the cone-like concentric partitions, which were deposited from within, and cannot, therefore, be considered plant-like exogenous layers.

We were fortunate enough to obtain a specimen of *B. undulata* at English Head which demonstrates that the cone-like partitions are the walls of chambers that were successively occupied by a part, if not by the whole body of an animal.

It is a fragment of the larger end, broken across the terminal chamber, exhibiting the outline of the animal parts, contained within the cavity at the time of the burial of the Beatricean in its Silurian grave. The centre is filled with a core of carbonate of lime from which a variable number of lobe-like longitudinal folds impinge against the sides of the vesicular cone, corresponding to angular external ridges projecting from the outer surface. The spaces between the folds are acutely angular, and answer to an equal number of broad shallow channels between the external ridges. The correspondence of this core with the exterior renders the supposition of its being the fossilized form of those parts of the animal that secreted the shell extremely probable. The differences, also, which exist between the lobe-like folds and the corresponding, angular, external ridges between the vacant, deeply cut, angulated interspaces, and the shallow, external channels, show that the inclosed body was surrounded by a mantle. If the shell had not been secreted by an enveloping mantle, the outline of the exterior would have corresponded more closely to the internal irregularities of the surface of the body, and there would have resulted a shell with prominent lobe-like folds, instead of depressed, angular ridges, and deeply cut angulated interspaces, instead of broad, shallow channels.

In attempting to refer the Beatriceæ to their proper place in the animal kingdom, we were at first much impressed by their great likeness to Cystophyllum; Mr. Verrill, however, after having made extensive comparisons between them and all the seemingly allied forms of Radiata in the Museum of Comparative Zoology in Cambridge, became convinced that their apparent affinity to the Radiate branch was nothing more than an analogical resemblance; extending, however, in a most extraordinary manner throughout the internal parts.

Mr. Verrill separates them from the Radiata principally because of the absence of any radiated structure, and the presence of the external, subepidermal, ornamented layer; features which he considers as irreconcilable with the structure of that branch.

An additional objection of considerable weight, may be founded upon the internal organization of the specimen, from English Head, above described.

This, with the fossilized remains of its former occupant still preserved within the chamber, the last of a series of other cone-like chambers, when considered in connection with the continuity of the sub-epidermal layer, and the absence of all annular furrows, shows conclusively that the shell was secreted by a Mollusk.

The Beatriceæ are very like the Hippuritidæ, both in general form and the arrangement of the component parts of the shell; but here again it may be demonstrated that the resemblance is not so close as it at first appears to be.

The geological horizon in which they occur, without other evidence, would alone be sufficient to render their affinity with the Hippuritidæ exceedingly doubtful; but, besides this, the structure, evidently, is not so closely allied to that of the Hippurite as to the Cephalopod. The shell of the Hippurite is composed of three parts: first, the inner septa, second, the outer layers, which frequently form a porous mass,[2] and third, an external sub-epidermal layer.[3]

The inner septa, which supported the principal part of the body of the Mollusk, form large cavities, while the second part is made up of laminæ laid on by the mantle margin, or at least that part corresponding to the mantle margin of the Lamellibranchiates.

In Beatriceæ, on the contrary, the inner septa did not contain the body of the animal, and there are no marks whatever of a mantle margin. This objection could not be urged against their affinity with Caprinella and the like, in which the central cavities are small; but from these they may be separated by the absence of all ligamental or muscular impressions and the mode of forming annular, cellular partitions, composed of numerous laminæ, instead of a continuous series of porous or tubular laminæ.

The Hippurites, Caprina, and the like, were, with few exceptions, attached to the surfaces upon which they lived or to each other, and had short, thick, cone-like forms, affording broad bases of attachment, whereas the Beatriceæ were long, thin, almost tubular bodies, resembling the Orthoceratites, and entirely unfitted to support themselves in fixed positions.

We saw hundreds of *B. nodulosa* and *B. undulata* "in situ," but nowhere any indications of attachment, either to the rocks or to each other.

After close comparison with all the types to which these singular fossils appeared to have any resemblance, I have at length considered myself warranted in considering them as Cephalopods more closely allied to the genus Endoceras, than to any other group of that class.

They differ greatly from all the Tetrabranchiates, in the open structure of the partitions or septa between the chambers, and this character, together with the absence of a siphon, and the cone-like form of the septa, demands that they should be separated as a distinct order, for which I propose the name of Ceriolites.[4] Although distinct as an order because of the differences in the form and structure of the septa, arising from their great length, and the loose way in which the laminæ are arranged, the parts may be compared point for point with similar parts of Endoceras.

We may imagine the cone-like septa of a Beatricean to be spread apart, until their surfaces should be parallel throughout the shell; they would then be entirely separated by hollow

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[2] Woodward. Hippuritidæ. Quar. Jour. Geol. Soc.; London. v. 16, part 1, page 41.

[3] Woodward. Quar. Jour. Geol. Soc., p. 46.

[4] κηριον, a honeycomb, λιθος, a stone.

chambers, as the septa are in Endoceras, and if, at the same time, the central cup-like cavities were supposed to be prolonged into cones, we should, without violence to the typical idea of the organization, have transformed the Beatricea into a shell separable from the Endoceras by only one character, the vesicularity of the septa.

The analogies which the Beatriceæ have with plants in their general aspect, with Radiates in their internal vesicular structure, and with Hippurites in the arrangement of the parts, are so close as to entirely bury, as it were, their true affinity with Cephalopoda, which only becomes obvious after diligent comparisons.

This complex nature establishes the Ceriolite as one of those strange types that not only combines, as does the "*Phoronis hippocrepia,*[5] the Annelidan homomorph of the Hippocrepian Polyzoa," the characteristic features of other groups belonging to different branches of the animal kingdom, but also of groups in other classes within its own sub-kingdom.

All these analogical relations are, so to speak, retrospective; they refer to lower ranks of life than the one in which Beatricea itself is found.

There still remains a question which I have not been able to solve in a satisfactory manner with the specimens at my command. Are the Beatriceæ internal or external shells? This problem, so difficult to settle conclusively with regard to the Orthoceratites, is equally puzzling in the structure of Beatriceæ. Their extreme length, cellular structure, and the irregularity of the ornamentation, would be almost determinative in favor of their being internal shells, were it not for the aspect of the cast of the animal in the terminal chamber of the specimen from English Head, which proves that a large portion, if not the whole of the body, was contained within the shell.

If, however, upon further examination, they should prove to be internal, which I consider doubtful, the cycle of their analogical characteristics would be completed, and they would refer not only to lower types, but have certain features in common with the more highly organized dibranchiate Cephalopods.

There are but two known species of the order Ceriolites, both occurring in the Silurian strata of Anticosti.[6]

Mr. Billings, in describing the species, states that they differ only in the external ornaments or markings.[7]

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[5] Allman's fresh-water Polyzoa. Ray Soc., 1856, p. 55.

[6] Since writing the above I have been indebted to Prof. J. D. Dana for the loan of some fragments of a Beatricea, resembling *B. undulata*, from Kentucky. The remoteness of this locality affords ground for hoping that the remains of other species may be brought to light in some of the intermediate Silurian basins.

[7] Billings's Report, Canada Geol. Survey. 1853–56. p. 343.

He, however, probably did not possess so fine a series of natural sections as it was our good fortune to collect, or he would have discovered the very considerable differences which exist in their internal structure, by which they may be as readily distinguished as by the external characters. I think, therefore, it may not be amiss to redescribe the species, giving such peculiarities as have not already been mentioned.

ORDER CERIOLITES, Hyatt.

FAMILY CERIOLIDÆ, Hyatt.

Genus BEATRICEA, Billings.

Beatricea nodulosa, Billings, is a long cone, tapering very gradually, the central chambers occupying from a fourth to a third of the transverse diameter in adults; in the young they are larger proportionally, varying from one-half to two-thirds of the breadth of the whole shell. The coniform layers are nearly parallel; the inclination at the lower part, as they trend outward to the circumference, being decreased very slowly. The external shell is closely set with tubercles and covered with granular points. The size, as nearly as could be inferred from fragments, is not over four feet long, by from three to five inches in diameter at the larger end.

B. undulata is a much larger species, one fragment found by the expedition being thirteen and a half feet long, by eight and a half inches in diameter at the larger end, and judging by the inclination of the sides, the length of the entire shell, when living, was certainly not less than twenty feet. The chambers are very small, frequently in adults not occupying more than one-tenth of the transverse diameter. The coniform layers are more widely separated in the adults than in the young; their inclination as they trend outward to the periphery is more decided, and they nowhere assume the parallel appearance of the same parts in Beatricea nodulosa.

The exterior is granulated and ornamented by ten or more prominent longitudinal ridges and intervening broad, shallow channels.

Cambridge, Mass., Dec. 6, 1864. (Residence, Baltimore, Md.)

Art. XXXII.—*The Albert Coal, or Albertite, of New Brunswick;*

by Charles H. Hitchcock.

The nature of the Albert coal and the mode of its occurrence in the strata have been vexed questions in geology. Its beautiful appearance attracts the eye, while its pecuniary value gave rise to the litigation which occasioned the delivery of the diverse opinions. In this as in so many other difficult subjects time has developed much truth, and shown us that we must not insist too strongly upon seemingly well-established theories.

In this communication I propose to describe briefly the geological features of the Albert coal vein and the accompanying rocks. I shall, whenever necessary, refer to the facts observed by others in this locality, but rely chiefly upon my own observations made in 1861 and 1864, as well as upon hints derived from persons of intelligence living in the vicinity. For a knowledge of many facts relating to the distribution of the different strata, I am indebted to Mr. James Blight, of Hillsborough, N. B. It is necessary to be dependent upon others for some knowledge of the internal structure of the Albert Mine, because the Company will not allow any scientists to examine their property below the surface. I use the word *coal* as a matter of convenience, not necessarily in strict propriety.

There are four different mining properties in Hillsborough, situated upon veins of Albertite:—the Albert, (the only one worked extensively and thoroughly proved), the East Albert, the Prince of Wales, and the Princess Alexandra. The second lies east of the first, and the others north and south of the same. Hillsborough is situated upon the west bank of the Petitcodiac river, near its confluence with the Bay of Fundy.

The rocks are of Lower Carboniferous age, and belong to the Acadian coal series. Several species of *Palæoniscus, Lipidodendron, Lepidostrobus, Spheræedra, and Stigmaria* occur in the shales and sandstones. Two or three miles southwest from the Albert shaft there appear older crystalline rocks, such as syenite and metamorphic slates; constituting the easternmost point of the extension of these rocks from the vicinity of St. John. The lowest rock in the Carboniferous series is the Albert shale; but I cannot state whether it crops out near the syenite. It probably does not appear anywhere on the edges of the coal basin. Nothing similar to it occurs in the Joggins' section. It should be expected to occur near and below the numerous deposits of gypsum in the eastern provinces.

This shale contains a large amount of hydro-carbonaceous matter. Certain layers of it at the "Caledonia Oil Works," by a rude process, have yielded thirty gallons of refined illuminating oil to the ton. The greater portion of the shale will sustain a fire without the aid of other fuel. Other layers are more bony, and others still highly ferruginous. It contains immense numbers of fossil fish, almost enough to make one imagine they gave the shale its inflammable character. The surfaces of many layers are glazed. The rock is very weak and abounds in small contortions of the strata. It appears in three localities. The largest has the Albert shaft in its centre; being exposed a mile or so in length, and showing best in the low ground. Small patches of shale may be seen on Peck's and Stinking Creeks, besides an unknown amount farther west at the oil-works. This series cannot be less than one thousand feet thick, as it has not yet been cut through by the shaft, and the general inclination of the bedding is very small.

The second group of strata is a conglomerate, separated from the first by a narrow bed of sandstone. Bits of Albert coal and shale constitute component parts of certain coarse sedimentary strata of this group, and render them oleaginous. The thickness is unknown, probably from 100 to 200 feet. Between the Albert shaft and the Petitcodiac, from three to four miles, this group prevails at the surface, except it be a small area of shale in a deep valley.

The next layers in ascending order are red marl and a bluish gray siliceous limestone. Above these are immense deposits of gypsum, from which most excellent plaster-of-paris is made for exportation. The highest rock in the series in this vicinity is another great mass of reddish conglomerate and sandstone.

There appears to be an anticlinal axis passing through this region, trending nearly ten degrees north of east. I have traced it from the Albert shaft to the Petitcodiac river, and have reason to believe it extends much farther, both west and east. The anticlinal structure is shown in three ways. *First*, the testimony is unanimous that there is an anticlinal in the Albert Mine. *Second*, the conglomerate dips in opposite directions in seven equi-distant localities examined over this area, but the dip is small. *Third*, the rocks succeed one another on both sides of the supposed anticlinal line in the ascending order mentioned above. On the north, we find above the shale, conglomerate, red marl, limestone, gypsum and sandstone. On the south, the order is the same with an opposite dip.

There appears to be a fault along or near this axis, displaying the usual phenomena of anticlinal fissures, and its location may easily be accounted for. The great plicating force acting from the direction of the ocean crowded the Hillsborough rocks northwesterly from the Bay of Fundy, crushing them up into a fold. The shale was not strong enough to sustain the bending; hence its layers were much twisted and fissured along a central line.

It is not likely that any of the shale rose above the surface at the time of flexure; and now that a portion of it has been laid bare by the removal of the upper rocks, the fissure and contortions show more plainly than in the overlying tough conglomerate to the east, which being narrower will naturally contain less of any foreign matter that has subsequently been injected into it.[1]

Some relics of this great force are now perceptible at the Albert mine, showing that the pressure is still exerted, perhaps as strongly as at any time of its manifestation. This phenomenon is much more noticeable than the "swellings of the walls," so common in deep mines. As soon as the coal is removed, strong timbers are put in to keep open the drifts, but in a short time these cross-pieces are split and crushed by the powerful force pushing the walls together. And when the timber is destroyed, the walls shut in, closing with a great noise as loud as thunder for hours, but not so near the workmen as to interfere with their progress. Not merely do the walls close, but frequently large fissures are produced behind the vein, so that the miners can clamber up and down new crevices. Large masses of rock are sometimes detached from either wall, in consequence, filling up the drift. We might explain the falling of fragments by gravity, but not so easily the crushing of the timbers.

The coal shows the effects of the crushing process no less plainly. It is much broken, even to grains, and needs no pick for its removal from the vein. It will flow as easily as heaps of corn, and therefore pains are taken to tap the vein in the right place, and at the proper time. If by oversight the main shaft is not walled up very tight, the coal will stream through the crevices between the beams, to the great inconvenience of the workmen.

The first outcrop of Albert coal was discovered by John Duffy, fifteen or sixteen years ago, in a deep ravine on Frederick's brook. The vein was about four feet wide, but by working upwards twenty-five feet into the bank, it thinned out to two or three inches. Duffy drifted about 300 feet on the course of the vein above the water-level, and sunk a shaft sixty feet, where the coal is said to have attained a width of ten feet. He then disposed of the property to Cairns, Allison & Co., who held it at the time of the litigation, but have now mostly transferred their shares to other parties, holding them under the same charter.

To describe the numerous variations in the course, thickness and shifts in the Albert workings, so far as known, would be unnecessarily tedious. Percival describes them for the first 200

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[1] The fact of the existence of the coal in a vein occupying an anticlinal dislocation was maintained by Messrs. Robb and R. C. Taylor in their Joint Report upon the Albert Mine in 1851. See *Proc. Am. Phil. Soc.*, vol. v, p. 242. Their report was accompanied by chemical analyses by Dr. C. M. Wetherill, who made the material a variety of asphaltum and named it *melan-asphalt*.

or 300 feet of the descent, occupying more than three pages of the size of this Journal in a little more than catalogue style of enumeration. I am assured by the manager, Capt. Byers, that, from that depth to the bottom of the shaft, 950 feet, the character of the irregularities has not changed. In brief, the peculiarities of the mine are the following.

The general course of the vein is N. 65° E., but the coal is repeatedly heaved southward by small faults. Its inclination is northwestward from 75° to 80°, often vertical. The body of the vein is extremely irregular, constantly expanding and contracting, both laterally and vertically. What is too narrow to be worked in one level enlarges to six and twelve feet, a hundred feet lower, or the reverse; but in general the width increases in following down the vein. At the time of Percival's examination, the vein was not considered workable 170 feet west from the old shaft. At lower levels the yield is renumerative 700 feet west and 2300 feet east of the new shaft, which lies several rods west of the first. In consequence of the uncertainties in the character of the vein, it is found necessary to accumulate a large supply of the coal during the suspension of navigation, so that there will always be enough stock on hand in the warmer months to load the vessels without delay. Whenever a displacement is met with the vein is not lost, because a film of the coal remains in the slip to indicate the location of the heaved portion. The widest part of the vein is said to be twenty-eight feet.

The narrow portions of the coal are invariably contained in a harder rock; where the rock is softer the vein is larger. "Horses" are common. In such cases the cavity above, out of which the horse fell, is found to be filled with coal; so that the width of the coal at that level is equal to the usual width *plus* the width of the horse. Numerous small branches run off into the shales from the main vein. These are short and might be described as irregular and branching spines from a main stem. Many of the fragments of rock taken from the mine show these small injections. The most striking proof of the proper character of the mass is afforded by the edges of the strata in contact with the coal; they are coated with Albertite, while the surfaces are covered only when enclosing one of the small lateral branches, a few inches long.

With the facts now presenting themselves to the explorer, I think no one would call the Albertite mass a bed. It occupies an irregular fissure along an anticlinal line, and the deep workings have failed to develop the lower anticlinal branching of the coal anticipated by the advocates of the bed theory. The numerous branches are unlike any phenomena connected with beds. There is no fire-clay to form the floor of a bed; and, in addition, the common adherence of the coal to the edges of the strata, rather than slickensides—I do not mean those in the horizontal slips—seems to complete the evidence that the coal does not occur between stratified planes. To disclaim a bedded character casts no reflections upon the observations of the distinguished geologists who have decided otherwise; because they started with erroneous premises. To them the idea of coal in a vein was preposterous. It appeared as great an anomaly as it would be now to find Niagara fossils in the Potsdam group.

The vein-character of the deposit is seen more distinctly in the smaller openings. On the East Albert property two shafts have been commenced near the anticlinal line in the conglomerate over the shale. These reveal, at the depth of thirty feet, nearly six inches width of a richer and more beautiful coal than the Albert, gradually thinning out to the width of coarse paper at the surface, and most unequivocally cutting vertically across nearly horizontal layers of sandstone. As before, we have here the phenomena of shifts constantly working the vein southward, and a slight leaning in the same direction. Following the line to the Petitcodiac, there are seen other openings upon the vein of less extent.

The two veins crossing the anticlinal are very interesting. Upon parallel lines about a mile apart, their course is N.E. and S.W. One appears to intersect the principal vein very near the Albert shaft. The intersection of the other is concealed by a great depth of alluvium. It cannot be said that the coal is likely to prove more abundant at these intersections, as is the case at the union of metallic lodes, yet the similarity of the two classes of veins is such as to warrant the exploration.

These two side veins cut the strata nearly at right angles to the dip. The following is in general their nature, as observed in half a dozen openings. The conglomerate with a gentle dip is traversed by vertical joints, two of which parallel to each other, and from two to seven feet apart, are filled with threads of Albertite, occasionally enlarging to bunches an inch thick. Between them are branching threads of the same material, joining the lateral seams at various angles. I think there are no branches upon the outside walls. The whole field reminds one of an area of tin veins. Like the others, this vein-field leans slightly southward. It will be interesting to watch the development of these veins to see whether they will develop like the Albert. Their persistency and ability to cut through the strata render them worthy of attention. But no one ought to expect to discover a large vein till the threads have been followed down to the subjacent shale. The surface at the Albert mine is more than a hundred feet lower geologically than the bottom of the East Albert shaft where the coal is nearly six inches wide.

I think the following conclusions may be drawn legitimately from the foregoing and kindred facts.

1. *The Albert coal occurs in true cutting veins, not in sedimentary beds like ordinary coal.*

2. *The Albert coal was originally in a liquid state, was injected into vertical fissures, and subsequently hardened into a substance resembling jet.* The liquid may have been derived from vegetable accumulations, or possibly in part from the abundant ichthyic remains in the shales. Whether the shales were originally oily, as now, and the fissures subsequently filled with a viscid fluid derived from them, or whether the charging of the fissures imparted an inflammable character to the rock, I will not conjecture, though it is easy to satisfy one's own mind. The cavities of the Albert coal occasionally hold liquid petroleum, and those in the adjacent shales more often. A few quarts of petroleum have been brought up from borings along the line of both the Albertite veins on the east side of the Petitcodiac. With the hardening, the hydro-carbonaceous liquid received oxygen into its composition.

3. *The Albert coal must be compared with the asphaltic and bituminous veins found in the Quebec group in Canada.* It there "fills veins and fissures in the limestones, shales, sandstones, and even in the trap rocks which traverse these." "In other cases, it fills fissures several inches in diameter, so that it has been mistaken for coal, and attempts have been made to work it at Quebec and elsewhere. The mineral is never, however, in true beds like coal, but is always confined to veins and fissures which cut the strata." "The matter is of a shining black color, very brittle, breaking into irregular fragments with a conchoidal fracture." (*Geol. Canada.*) The Quebec coal is like the Albert in the small amount of the ashes, but contains more carbon.

4. *These carbonaceous veins are analogous to veins of petroleum.* The borings for petroleum in Ohio and Western Virginia are most successful along lines of fracture, particularly an anticlinal axis. The description of the chasm filled with oil would undoubtedly be given in words similar to those used respecting the Albert vein, if we could sink shafts and drive on the course. The views of Prof. Andrews in this Journal, ([2] xxxii, 85,) respecting the location of petroleum, are very just, and show that it often occurs along anticlinal faults. The immense yield of many oil-wells certainly suggests the presence of more than the "horse-cavities" filled with the liquid.[2]

5. The carbonaceous veins, such as the Albert coal, Canadian asphalt and liquid petroleum, while possessing many characteristics of metallic lodes, will be found to differ from them in some

[2] A valuable paper, by T. S. Hunt, in this Journal, [2] xxxv, 157, 1863, upon Bitumens, etc., presents the general conclusions stated above. His data were derived both from analysis of mineral combustibles and explorations in petroleum districts. Sir William Logan mentions the occurrence of petroleum springs for twenty miles along a fold in the stratification in Gaspé in 1844. J. P. Lesley has described a vertical vein of asphaltic coal, precisely like the Albert, in Ritchie Co., W. Va.

respects. These particulars will be ascertained fully by the immense enterprise now manifested in sinking for petroleum. We can anticipate differences in respect to the limited depth, little variations of thickness at intersections, irregular yield, and origin of the carbonaceous veins. A proper knowledge of them may lead to some modification of terms in our definitions.

37 Park Row, New York, Jan. 23, 1865.

ART. XXXIII.—*Detection of the adulteration of Essential Oils with Oil of Turpentine by the Saccharimeter;* by Dr. JULIUS MAIER, Assistant in the School of Mines, Columbia Coll., N. Y.

THE essential oils, especially the expensive ones, are mostly adulterated with oil of turpentine. It is often difficult to detect this adulteration, especially when the adulterated oil gives similar reactions with oil of turpentine.

With the saccharimeter it is possible not only to detect the adulteration but even to find out the quantity of oil of turpentine mixed with the other essential oils. A large number of oils, particularly those belonging to the camphenes, the carbon of which is in the proportion of 5 to 8 to their hydrogen, have an action on the polarized light, deviating the light either to the right or the left hand side. These optical researches have been made by Biot, Soubeiran, Capitaine, Gladstone and Berthelot, in order to establish the constitution of the camphenes. I made some researches to detect the adulteration of the essential oil with the oil of turpentine.

For that purpose, a chemically pure oil of lemon which I had prepared myself, was tested in a saccharimeter, the tube of which was 200mm long.

The deviation was +137°·296 for the middle yellow ray. The oil of turpentine, used for the research, prepared by myself, had a specific gravity of 0·865 and gave a deviation of −73°·135. A mixture of equal volumes of both these oils showed a deviation of +30°·65. The calculation gives a deviation of +32°·081 in the following manner:

$\frac{1}{2}$ vol. oil of lemon = + 68·648
$\frac{1}{2}$ vol. oil of turpentine = − 36·567

1 vol. mixture = +32°·081

A mixture of 2 vol. oil of lemon with one vol. oil of turpentine gave a deviation of +65°·34; from the calculation results a deviation of +67°·152, as follows:

$\frac{2}{3}$ vol. oil of lemon = +91°·531
$\frac{1}{3}$ vol. oil of turpentine = − 24°·379

1 vol. mixture = +67°·152

I made the same researches with pure oil of juniper, which I had prepared myself, and arrived at the following results:

The oil used for the experiment had a specific gravity of 0·858 and showed a deviation of −5°·970. The oil of turpentine employed was the same as in the above mentioned experiment. A mixture of equal volumes of oil of juniper and oil of turpentine showed a deviation of −40°·84; the calculation gives a deviation of −39°·553 as follows:

$$\begin{array}{lr} \tfrac{1}{2}\text{ vol. oil of juniper} & = -\ \ 2^\circ{\cdot}985 \\ \tfrac{1}{2}\text{ vol. oil of turpentine} & = -36^\circ{\cdot}568 \\ \hline \text{1 vol. mixture} & = -39^\circ{\cdot}553 \end{array}$$

From this it is proved that the quantity of the adulterating oil of turpentine can be detected through the medium of the saccharimeter. But if the essential oil is adulterated not only with oil of turpentine but also with another optically active oil, the saccharimeter test is of no value. In order to find out the quantity of the adulterating oil of turpentine from the deviation showed by the oil of turpentine, by the adulterated oil, and by the mixture, the numbers of deviation have to be brought in reference to a common distinct starting point. This starting point generally is the power of rotation, that is the deviation of the respective oil as calculated for a tube of 100mm length, and a specific gravity of 1. This power of rotation, the worth of which is generally expressed by $[\alpha]$, is the following for:

$$\begin{array}{ll} \text{Oil of turpentine} & [\alpha] = -42^\circ{\cdot}275. \\ \text{Oil of lemon} & [\alpha] = +80^\circ{\cdot}573. \\ \text{Oil of juniper} & [\alpha] = -\ \ 3^\circ{\cdot}479. \end{array}$$

The quantity of oil of turpentine employed for the adulteration, is calculated as follows:

a the power of rotation of the pure oil.
b " " " " " " oil of turpentine.
c " " " " " " mixture.
m " quantity " " "
x " " " " adulterating oil of turpentine.

The quantity of the pure oil as contained in the mixture is $=m-a$, and the power of rotation of this quantity is $=(m-x)a$; the power of rotation of the oil of turpentine $=-b\,x$, and the power of rotation of the whole quantity of mixture $=mc$; hence results the following equation:

$$\begin{gathered} (m-x)a-b\,x=m\,c \\ m\,a-a\,x-bx=m\,c \\ m\,a-m\,c=a\,x+b\,x \\ \frac{m(a-c)}{a+b}=x \end{gathered}$$

To show this calculation by an example, the power of rotation is supposed to be—

of the pure oil of lemon $= +80^\circ{\cdot}573$
" " " " turpentine $= -40^\circ{\cdot}275$
" " mixture $= +18^\circ{\cdot}70$
the quantity of the mixture $= 20$ c. cm.

$$\begin{aligned}(20-x)\,80{\cdot}573 - 40{\cdot}275x &= 20 \times 18{\cdot}70\\ 7611{\cdot}46 - 80{\cdot}573x - 70{\cdot}275x &= 374\\ 1237{\cdot}46 &= 122{\cdot}848x\\ 10{\cdot}0 &= x.\end{aligned}$$

The mixture contains equal parts of the pure oil and the adulterating oil of turpentine.

Optical behavior of several essential oils.

Tested oil.	Specific gravity.	Power of rotation.	Observer.
Oil of absinth,	0·973	+ 20°·67	Soubeiran & Capitaine.
Oil of orange blossoms, first product,	0·835	+127°.43	
Oil of orange blossoms, second product,	0·837	+125°·59	
Oil of bergamot,	0·850(?)	+ 29°·28	Biot.
Oil of bergamot, first product,	0·850	+ 49°·396	Soubeiran & Capitaine.
Oil of bergamot, last product,	0·877	− 6°·573	
Oil of caraway seed,	0·897	− 11°·7	Maier.
Oil of lemon,	0·848	+ 80°·484	Biot.
Oil of lemon,	0·852	+ 80°·573	Maier.
Oil of lemon, (Grasse,) first product,	0·844	+ 79°·749	Soubeiran and Capitaine.
Oil of lemon, (Grasse,) last product,	0·853	+ 78°·156	
Oil of lemon, rectified,	0·854	+ 80°·916	
Oil of copaiva balsam,	0·881	− 34°·18	
Oil of copaiva balsam, (Para,)	0·898	− 28°·553	
Oil of cubebs,	0·929	− 40°·159	
Oil of cubebs, free from water,	0·914	− 39°·40	
Oil of elemi,	0·852	− 90°·30	Deville.
Oil of juniper,	0·855	− 3°·521	Soubeiran & Capitaine.
Oil of juniper,	0·858	− 3°·479	Maier.
Oil of turpentine,	0·8722(?)	− 39°·950	Biot.
Oil of turpentine,	0·860	− 43°·38	Soubeiran & Capitaine.
Oil of turpentine,	0·865	− 42°·25	Maier.

Gladstone (Chem. Soc. Jour., [2], ii, 1,) has given a more complete table on the power of rotation of most of the essential oils. My researches were restricted to the few above mentioned.

ART. XXXIV.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

(Concluded from p. 150.)

§ 14. *The Lunar Distances.*

As Kepler's third law was deduced from the planetary orbits alone, so was the law of Titius. But it was shown to be a consequence of the law of universal gravitation, and therefore itself universal and applicable to any system—hence, also to the lunar systems. Now the law of Titius, as modified above, has been found to be identical with the equality of the intervals of time in the history of any system. Therefore, also, this law (38) must apply to the lunar systems. This we now will show.

A. *The Lunar System of Jupiter.*

The *Jovial World* is the youngest of those great lunar systems that adorn the exterior planets. (This Journal, xxxvii, p. 45.) Therefore, it is the most regular yet of any, and our law (38) must very closely harmonize with the actual distances of Jupiter's moons. It is easily found that $\gamma=2$, again, as for the planetary distances; and that $\alpha=4$ and $\beta=3$ radii of Jupiter. Thus (38) is for the Jovial World,

$$a_t=4+3\times2^t. \quad \ldots\ldots\ldots \quad (64)$$

		t.	Distance. Calculated.	Distance. Observed.	Fall.
Moon	I.	0	7	6·049	·951
"	II.	1	10	9·623	·377
"	III.	2	16	15·350	·650
"	IV.	3	28	26·998	1·002

The "fall" of a moon is the distance it has fallen toward the planet in virtue of the resisting ether. That this fall corresponds to the age, mass and density of the different moons has been shown in our previous article. (This Journal, xxxvii, 45.)

The calculation of t from the observed distances gives for the 2d, 3d and 4th, respectively, ·907, 1·92, and 2·94, which only deviate by ·09, ·08 and ·06 from the theoretical values 1, 2 and 3; and all values being *too small* shows that these moons are correspondingly nearer the primary, having approached so much on account of the etherial resistance.

B. *The lunar world of Saturn*

is next in age, hence not quite so regular as that of Jupiter. We find that (38) represents the distances of the eight moons if the constants are

$$a_t=4+0{\cdot}35\times2^t, \quad \ldots\ldots\ldots \quad (65)$$

as will be seen from the following table:

Moon.	Distance. Calculated.	Distance. Observed.	Difference.
I. Mimas,	4·35	3·4	+·95
II. Enceladus,	4·70	4·3	+·40
III. Tethys,	5·4	5·4	·0
IV. Dione,	6·8	6·8	·0
V. Rhea,	9·6	9·6	·0
VI. Titan,	15·2	22.2	−7·0
VII. Hyperion,	26·4	28·0?	−1·6?
VIII. Japetus,	48·8	64·0	−15·2

Excepting for a moment the 6th and 8th moon, we see but small differences; Mimas and Enceladus being too near Saturn, appear to have but very small mass, which conclusion is strengthened by the fact that it required Herschel's great telescope to discover them (1789). The next three almost exactly harmonize with this law; they are, therefore, not only larger than the first two, but also much alike. They were discovered by Cassini, first the fifth (Rhea) in 1672, and later (1684) Tethys and Dione. As the latter were discovered by the same observer, the difference in date is, perhaps, alone due to the greater nearness to the disk of the primary. Hyperion is even lower than any, and, therefore, smaller than even the interior ones. This is confirmed by its discovery, which was not made till 1848, by Bond and Lassell. But the sixth, Titan, and the eighth, Japetus, are much farther distant than (65) gives; thus proving them to have much more considerable mass (or rather *v* (37) is less, which in general will be the case if the mass is greater). This is fully confirmed by the date of discovery: Titan being the first discovered of all, (by Huyghens, 1655), and Japetus the second, (by Cassini, 1671). These estimates of the masses are further corroborated by Humboldt,[1] who calls Titan "the largest of all known secondary planets." Compare another theoretical estimate, (this Journal, xxxvii, 46), leading to the same results.

C. *The lunar system of Uranus*

is exceedingly important on account of the plane and direction of its motions. We have tried to show that this very position affords one of the most conclusive confirmations of the nebular theory. (This Journal, xxxvii, 50.) Here we will consider the arrangement of the individual members of the system.

We know it to be the oldest, because it is the most distant system of which we have definite knowledge. The original distances and the original harmony of these distances is therefore here most deranged. We cannot even with any degree of certainty consider the moons to be now in the same order of suc-

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[1] *Cosmos*, i, Harper's edit., p. 95.

cession as at first. At the same time, observation has as yet hardly determined the number, much less the exact distance of the different moons. Therefore, we give the following more for the sake of completeness than with the view of adding any important confirmations of our law.

We have seen that the nearest luminaries may be equi-distant, and that the farthest may succeed at distances that form a geometrical progression--see (61) and (63). If the distances, as given by Herschel, and the times of revolution, as given by Lassell,[2] are exact, we may represent the distance of the first six moons by

$$a_t = 7{\cdot}5 + 3t, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (66)$$

corresponding to (62), and the distance of the sixth, seventh and eighth by

$$a_t = a_6 \,.\, 2^{t-6}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (67)$$

corresponding to (61).

	Distance.		
	Calculated.	Observed.	Difference.
Moon I	=7·5+0× 3 = 7·5	7·5	·0
II	=7·5+1× 3 =10·5	10·5	·0
III	=7·5+2× 3 =13·5	13·1	+ ·4
IV	=7·5+3× 3 =16·5	17·0	− ·5
V	=7·5+4× 3 =19·5	19·8	− ·3
VI	=7·5+5× 3 =22·5	22·7	− ·2
VII	= 2×22·5=45·0	45·5	− ·5
VIII	= 4×22·5=90·0	91·0	−1·0

If these observed distances really are correct, then this remarkable discontinuity will enable us to determine the lunar masses long before observation can ascertain them.

D. *Conclusion.*

The lunar system of the *Earth*, consisting of but *one* moon and that of *Neptune*, which comprehends one or two, cannot, or do not afford any chance to test our law. But we have seen that the systems of Jupiter and Saturn fully confirm our law (38), if due regard is had to the individual mass and volume—or density and radius—of the several moons. Even the system of Uranus, as far as known, does not deviate from it except in so far as it offers the two extreme limits of the law, probably on account of the high age and a close similarity between the masses of the first six moons.

Therefore we may say that as far as observation on the lunar systems goes it is embodied in our law (38), or *in every lunar system the consecutive moons were formed at equal intervals of time.*

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[2] See Schweigger in Astronomische Nachrichten, No. 832, Beilage.

§ 15. *The incommensurability of the periodic times.*

By the third law of Kepler we have, if T_t and $T_{t'}$ are the periodic times of two planets,

$$\frac{T_{t'}}{T_t} = \left(\frac{a_t{}'}{a_t}\right)^{\frac{3}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (68)$$

or by (38),

$$\frac{T_{t'}}{T_t} = \left[\frac{\alpha+\beta\gamma^{t'}}{\alpha+\beta\gamma^{t}}\right]^{\frac{3}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (69)$$

which expression will not generally make $T_{t'}$ and T_t commensurable. Thus we see that our law accounts for another important condition of stability of the system, (see § 3, 1).

But as the distances are continually decreasing, and at different rates, (this Journal, xxxvii, 41, gives the numerical values of these rates), we perceive that *in time such commensurability may take place* between any two planets.[3] Such is actually the case between Jupiter and Saturn, as discovered by Laplace.

The distances were (see § 13) for Jupiter $a_5 = 520$, for Saturn $a_6 = 1000$, giving for (68) the continued fraction 2(1, 1, 2 . .) having the approximations,

$$\frac{2}{1}, \ \frac{3}{1}, \ \frac{5}{2}, \ \frac{13}{5} \quad . \quad . \quad .$$

or $T_6 : T_5$ approached originally to 5 : 2; now it is very nearly so.

For Venus and the Earth the original distances 70 and 100 give the approximations,

$$\frac{1}{1}, \ \frac{2}{1}, \ \frac{5}{2}, \ \frac{12}{7}, \ \frac{29}{17} \quad . \quad . \quad .$$

whilst Airy has found the commensurability 13 : 8 or nearly our 29 : 17 [13 : 8=29 : 17·8].

In the *lunar systems* such commensurability is common; and it is for the satellites of Jupiter that Laplace demonstrated[4] the great proposition, *if such commensurability exists but approximatively it will become exact in time.*

Having seen that the change in distance produced by resistance will make the ratio approach commensurability, it therefore, as we stated before, will become rigorously so.

From (68) we find easily that the ratio will be 2 if the distances are in the ratio of $2^{\frac{2}{3}} : 1$, or (by continued fractions) as the approximative fractions,

$$\frac{1}{1}, \ \frac{2}{1}, \ \frac{3}{2}, \ \frac{8}{5}, \ \frac{19}{12}, \ \frac{27}{17}, \quad . \quad . \quad .$$

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[3] Grant (*History of Physical Astronomy*, London, 1852, p. 92.) states that the libration of the jovial moons is "independent of the effects of a resisting medium," meaning that it will be preserved notwithstanding such medium. This is probably a mistake, for it would depend upon the relative magnitude of the resistance and the perturbation.

[4] Méc. Cél., vol. viii, Ch. vi, § 15. We express the proposition in more general terms.

For Jupiter's satellites we have $a_2 = 16$, $a_1 = 10$, or $a_2 : a_1 = 8 : 5$; and $a_1 : a_0 = 10 : 7 = 8 : 5{\cdot}6$, therefore we find the periodic time of the second moon twice that of the first, $T_1 = 2T_0$, and the periodic time of the third twice that of the second, $T_2 = 2T_1$; hence Laplace's famous relation between the mean motions,

$$n_0 + 2n_2 = 3n_1.$$

In the system of Saturn similar relations obtain. At first we had (see § 14, B) for the distance of Tethys (III) and Mimas (1) the ratio 54 : 44=8 : 6·6, while the duplication of the periodic times requires the ratio 8 : 5. But Mimas has approached Saturn the most, and thus this proportion (now 5·4 : 3·4=8 : 5·04) has been brought about.

For the fourth and second we had originally, Dione : Enceladus=6·8 : 4·7=8 : 5·5, or likewise sufficiently near 8 : 5 that the duplication of the periodic time should become almost rigorous.[5]

The lunar world of Uranus is particularly noted for such duplications, from the fact that Schweigger, as early as 1814, on such grounds predicted the existence and gave the orbits of the two innermost moons of Uranus, which were discovered by Lassell in 1851. The coincidence is very remarkable, as will be seen from the following:[6]

	Schweigger, 1814.	Lassell, 1851.
Uranus, I moon, - - -	2·1767 days,	2·5117 days.
II - -	4·3534 "	4·1445 "

and the IV (or II of Herschel) having a period of 8·7068 days approximates to the further duplication of the periodic times. Also the period of III is about half the IV period, the former being 5·8926 days, the latter 10·9611.

Taking only the first two decimals we find by means of continued fractions the following approximations:

$$\text{II to I or } \frac{441}{251} = \frac{1}{1}, \frac{2}{1}, \frac{3}{2}, \frac{5}{3}, \frac{28}{17}, \frac{33}{20}, \text{ etc.}$$

$$\text{IV `` II `` } \frac{871}{441} = \frac{2}{1}, \frac{19}{9}, \frac{21}{10}, \frac{40}{19}, \frac{61}{29}, \text{ etc.}$$

$$\text{V `` III `` } \frac{1096}{589} = \frac{1}{1}, \frac{2}{1}, \frac{13}{7}, \frac{93}{50}, \frac{199}{107}, \text{ etc.}$$

thus proving that only the fourth (Herschel II) and second (Lassell II) have periodic times nearly in the ratio of 2 to 1.

The other instances adduced by Schweigger, and especially the first, do not seem to have any claim to be considered as real duplications. Still it is evident that the configuration of the Uranian-system is such as approaches to simple ratios between the periodic times; and if the perturbating force arising here-

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[5] Herschel, *Outlines of Astronomy*, § 550.

[6] Schweigger; Ueber die Auffindung der ersten Uranustrabanten durch Lassell. Astronomische Nachrichten. 1852, No. 832.

from is greater than the effect of resistance, these ratios and the corresponding configuration would become permanent. It is not improbable that an analysis of the lunar system of Herschel's planet will throw much light on the future configuration of the solar world by ascertaining the exact relation between perturbation in commensurable revolutions brought about by resistance and the continued influence of the latter force on such commensurable motions.

Though this latter question cannot at present be fully answered, we have proved in this paragraph that not only the general incommensurability of the periodic times ensuring the stability of the system, but also the deviations therefrom are accounted for by our law (38).

§ 16. *History of the Solar System.*

Believing that we have, in the preceding pages, brought forth some further arguments in favor of the nebular hypothesis, we may be permitted in a very few words to sketch the grand history of the material universe as it is seen in the light of this theory. The philosophers of old called Man a Microcosmos—we compare the Universe, the Macrocosmos, to man, thereby intimating that as Man has a parentage, growth and decay, i. e., a *history*, so has the Macrocosmos.

The history of the *material* world may be divided into four periods or ages, corresponding to those given in a note to §6. (Compare Guyot's views in Dana's Geology—chapter on "Cosmogony").

In the beginning *God created the heavens and the earth.* And the earth was *without form and void,* and *darkness* was upon the face of the deep. And the spirit of God moved upon the face of the waters. (*Genesis*, I, 1, 2).

The material universe was created *not* in its present form, but without form; it was *void* and *dark*; but the spirit of God pervaded it, and planned it such that his All-Foresight, or Providence, might also be manifest in the material world. *This* is really *the Creation*—it is merely stated, not described, for it is inconceivable to mortal understanding. It is too awful, our mind is lost in reflecting thereon; hence the divine writer merely mentions it at the beginning, and, to give fullness to his picture and adapt himself to our understanding, describes the first three great ages as real creative acts, though mere consequences of the unfathomable word given in the *first* verse of Genesis. We believe that the first five verses of Genesis have never before been fully understood in their deepest sense. We shall in the sequel keep constantly before our eyes both this, the *revealed History of the Cosmos*, and *science*, deduced from the revelation we have in the present form of nature.

Since Genesis merely states that the universe (i. e. heaven and earth) was *formless*, *void* of any organized being and *dark*, it is science alone that can give us any idea of the constitution of the universe as it came from the hands of the Creator. But as science is progressive, our ideas of the primeval condition of the Cosmos must progress correspondingly, or rather with advancing science our eye pierces farther and farther back into the dark past, approaches more and more to the mysterious and almighty "Fiat." As these approaching steps represent greater and greater series of ages, we infer that the *Fiat* lies infinitely far behind us, and can never be reached by human thought. We experience in regard to the age of Cosmos by penetrating farther and farther into the dark past with our spiritual eye, the same that we feel in piercing, by means of more and more powerful telescopes, farther and farther into the world-filled abyss of space. Here, if looking through a giant telescope we find ourselves surrounded by a boundless space filled with the wonders of the Creator; and if ardently searching in the existing documents of nature for records of her past, we behold infinity also here, the infinity of time, *eternity*, teeming with wonders no less astounding. The beautiful poem of Schiller, "*die Grösse der Welt*," is true both as to the extension and the duration of the World.

The ancients most frequently thought that the world left the hands of the Creator in the shape it now is. Even Newton himself was unable to see farther back. To him the Creator was but a tinker, forming his wheeling globes and wheeling them around their axis, putting them one by one and one by one to their very place in his clockwork—to him an unorganized machine to run on and on forever in the same shape. But Huyghens, and Newton himself, by discovering the generic cause of the figure of the earth aimed the first blow at this base idea, which nevertheless has found its advocates even to the present hour, especially among theologians. The corner-stone being broken out of the system it has been crumbling down. Geology has restored the lost history of the earth, and the nebular theory has traced this earth to the sun as her mother. Thus creation was now identical with the productions of the rotating mass of matter, i. e., of the chemical elements.

We have attempted to show that both rotation and the elements come from the forces wherewith the ONE matter (Urstoff) was endowed (see § 6). It is highly interesting to see how the first verse of Genesis has been understood by scientific men. It will at the same time more clearly set forth what we implied above when saying that science is approaching to the true original condition of Cosmos by making steps representing longer and longer periods of time.

"In the beginning God created the heaven and the earth" means according to

Newton, 1686: a direct, immediate creation of every globe as it is now.

Huyghens, Hutton, and modern Geologists: a direct creation of the heavenly globes as *fiery masses*, circulating in the system as they do now.

Kant, 1755, Laplace (later): a direct creation of a rotating mass of chemical elements; giving rise to the planetary system.

We, in 1854, conceived this rotating mass of elements to be the product of a created nebula consisting of but *one single element.*

We will now contemplate the different ages manifest in the development of this Urstoff.

First Day or Age.—The atoms of "Urstoff" combine—light (and heat) and the *chemical elements* result. The mere production of light would not entitle it to be considered one of the days of creation; but light is by the divine writer taken as a type to represent itself, and the less obvious, though much more important, chemical elements. It was not so much the light as the formation of the elements, the basis of modern physical science, which characterized the first day. We think that a rotation was also produced hereby. (This Journal, 1864, vol. xxxvii, p. 52.)

Second Day or Age.—Formation of the planetary orbs with their satellites.—The nebula developed itself into a great number of similar planetary nebulæ, which again gave birth to similar lunar nebulæ. Thus we see here the simplest kind of "life," reproduction by division, as exhibited by many plants, and even animals, which to distinguish them as such from inanimate matter, have another mode of reproduction *besides.* The planets represent the children, the moons the grandchildren of the sun.

Third Day or Age—The fiery balls resulting from this subdivision cool down and are shaped, as Geology has ascertained in relation to our own earth.

The Fourth Age of the *inorganic era* is the present. We have shown that the further characteristic of life, namely, death, is not restricted to the organic but is participated in by inorganic nature (this Journal, [2], xxxvii, 56). As every breath of our lungs is a differential of decay—so every rotation of the earth giving us the enjoyments of another day, and every revolution charming us with the succession of the seasons, brings our own mother earth nearer to her grave.[1]

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[1] We beg the scientific reader's pardon for these paragraphs, which do not belong to this place. But we felt it urgent to say at least this much, as some, even to-day, are apt to base the cry of "heretic," "infidel," etc., on any such deviation from the beaten path in their dogmas. The nebular hypothesis has richly participated in the abuse heaped in its day on the Copernican system, and on some leading doctrines of geology. Even yesterday, I found, in one of the leading religious quarterlies, Laplace called an "atheistic dreamer"! We wrote this paragraph as a protest against such imputations.

§ 17. *Conclusion.*

The principal results arrived at in this paper are

1st, A simple mechanical *theory of spiral nebulæ.*

2d, A more accurate determination of the orbits; and above all,

3d, *The discovery of the true law of the planetary and lunar distances.*

4th, The determination of the periodic times as a function of the distances—or borrowing this third Law of Kepler from the theory of gravitation, we have therein almost a theoretical demonstration of *the equality of the intervals.*

As (38) what we have repeatedly called "our law" is very much like Bode's, or rather Titius's, law, we apprehend that the propriety of thus naming (38) will be doubted. To set this point in clear light we refer to a similar, though undoubtedly grander, case in the history of science.

The law of Titius was exclusively derived from observation. It is empirical, as is the third law of Kepler. It is, moreover, not exact, neither in its general form nor in its numerical results. But neither is the famous law of Kepler exact, though, on account of the different circumstances connected herewith, this latter law agrees better with the numerical data of observation than Titius's law.

Newton discovered the true form of Kepler's law by deducing it from a higher law, that of universal gravitation. Instead of Kepler's form, C being the same constant for all planets,

$$\frac{T_2}{a^3}=C, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (70)$$

Newton found that the true law is

$$\frac{T^2}{a^3}=\frac{\mu}{4\pi^2}(M+m), \quad . \quad . \quad . \quad . \qquad (71)$$

μ being the constant of gravitation, hence the same for all planets; hence,

$$C \propto (M+m). \quad . \quad . \quad . \qquad (72)$$

That is, Kepler's constant C is proportional to the sum of the mass M of the sun and the mass m of the planet. By farther analysis it is found that C even is dependent on *all* the masses and distances in the system.

So also in our case. We have given the true expression of Titius's law by extending it to Mercury and have accounted for the deviations of nature from the law, by demonstrating that it is a necessary consequence of the higher law, viz: *the intervals between the abandonment of the different orbs of the same system are equal* (see § 13). *Now this is what we claim as our law.* As Newton deduced and corrected Kepler's law by his law of *equal gravitation*, so we have deduced and corrected the law of Titius by our law of *equal intervals.*

We referred to (38) as our "law" because it is a *consequence* of our law, and certainly our formula; we did not intend to obliterate the merit of Titius, as will be seen wherever we have mentioned his name.

There is yet another circumstance which makes our demonstration of the law of planetary distances so important. It is the touchstone of the nebular theory; for as this ascribes the formation of the planets to the slow descent of cosmical matter to its center, it has to be proved that such descent will give exactly the actual system. Already Plato held that[7] "the motion of the planets is such as if they had been all created by God in some region very remote from our system, and let fall from thence toward the sun, their falling motion being turned aside into a transverse one whenever they arrived at their several orbits." Galileo was the first who subjected this "concetto platonico" as he calls it to a numerical calculation based upon the laws of falling bodies as discovered by him. He finds an admirable harmony between his calculations and the actual velocities and distances as they were known at his time.[8] Next after him, Newton took the matter in hand, and in his third letter to Dr. Bentley he gives as his result, that it is impossible to account for the configuration of the system in the manner of Plato and Galileo. This result is based upon his assumption of a vacuum. By taking the influence of a resisting medium into account, we have proved that the Platonic idea as embodied in the nebular hypothesis *does* lead to the present configuration of the solar world. We make these remarks to show that the idea we advocate is old and venerable; we hope, at some other time, to give the highly interesting history of the law of planetary distances, including the application of the Phyllotaxis, (Pierce, Agassiz,) the radius of gyration, (Kirkwood,) the regular polyhedra, (Kepler, Plato,) etc.

How grand and beautiful is the harmony of the planetary world! What an admirable *unity of plan* is manifested therein! As *now* the planets are slowly sinking to the sun, so they have *always* been sinking since the moment of their creation as a nebulous mass; the same motion that now brings them nearer to their death has caused their formation, has brought them to life! And how sublime is the plan of creation! To call forth the harmonious system of the solar world with all its multiform aspects and dependencies fit to support life throughout almost endless ages—nothing but a collection of matter endowed with its

[7] Brewster, *Life of Newton*, Ch. 16.

[8] *Dialogo intorno ai due massimi Sistemi del Mondo, Tolemeico e Copernicano.* Giornata I, (ed. *Opere*, Firenze, 1842. Vol. i, p. 34–35.) He finds: le grandezze dei cerchj, e le velocità dei moti s'accostano tanto prossimamente a quel che ne danno i computi, che è cosa maravigliosa.

molecular forces was placed in a little spot of the house that contains many mansions besides. This matter slowly collected together. In thus following the force of attraction planted in it by eternal love, the whole great life of the solar world was awaked; and as the pulsation of the heart in man indicates the fleeting moments of his life, so the pulsations of that great whole, succeeding each other at equal intervals, gave each one birth to a new world to mark the historic epochs of the Universe by its position and to roll on for ages, a revelation of the Great Author, until, always following the same attractive force, it in death finds rest at the bosom of the planet-mother, the sun. And then—this grand system remains as a mere lump, a *Cosmic Fossil*, suspended in space, where perchance some higher being may meet with it, touch it, investigate it, and construct its whole past history, as the geologist in our days studies the history of a fossil shell!

Iowa City, Iowa, July—November, 1864.

ART. XXXV.—*The determination of the height of Auroral Arches from observations at one place;* by H. A. NEWTON.

IN the displays of the Aurora Borealis the luminous cloud often takes the form of an arch. Sometimes the lower boundary of the auroral light is arch-shaped. Beneath is a dark segment, while perhaps streamers run upward from the mass of light. Again, there is sometimes a bank of light in the north, resting apparently on the horizon. The upper boundary of this bank forms a more or less regular arch. Again, there is sometimes a narrow band or bow of light, spanning the heavens, coming down to within two or three degrees of the horizon at each extremity, having one or both of its edges sharply defined, and being often only two or three degrees in breadth.

The arch in each of these three cases may be incomplete, or broken, or otherwise irregular. But there is a manifest tendency to form a regular curve. This curve, that is, the boundary line of the arch, or the axis of the bow, is rarely, if ever, an arc of a great circle. It cuts the horizon at points notably less than 180° from each other. It has apparently the same law of formation in each of the three cases. Its peculiar shape is therefore probably due to a single cause.

There is no reason to believe that each observer sees a different arch, just as each sees his own rainbow. There is no center of light beneath the arch, and moreover a decided parallax is very frequently found. The curve of the auroral arch has then a definite locus in the atmosphere.

This curve is not caused by mists in the atmosphere obscuring and revealing parts of an indefinite cloud. For the arch has little or no relation to the horizon, and cuts it at all angles.

It is not a straight line, for the arch does not cut the horizon at points 180° from each other.

The arch resembles the projection of a portion of a circle, or a sphero-conic. The venerable Hansteen has in two instances seen at Christiana nearly the whole ellipse. Prof. Twining has observed, at Middlebury, Vt., in one instance at least, an arch in which the extremities of the major axis of the ellipse were visible above the horizon.

It is reasonable to infer that, in general, the locus of the light is parallel to the earth's surface. For the arch has the same general form at all places, as will be seen in the diagrams of Mairan and others.

This leads naturally to the hypothesis of Hansteen,[1] that the auroral arch is a real ring, which in its normal form is parallel to the earth's surface, and is symmetrically placed about the magnetic pole. The dark segment is seen when we look beneath the ring into space beyond. The bank of auroral light is a similar broader or more distant ring.

The results of Prof. Loomis's investigations respecting the geographical distribution of the aurora[2] confirm and modify this conclusion. He shows that there is a narrow belt of an elliptical form surrounding the magnetic and astronomical poles of the earth, and at a considerable distance from them, which is the region of the greatest and most frequent displays of the aurora. It is reasonable to infer that an aurora of considerable intensity would naturally take a form symmetrical with this narrow belt of the earth's surface. The portion of the curve which we see at any instant should be regarded as part of a circle whose center is the center of curvature of the nearest portion of this belt.

To obtain the parallax of the auroral cloud, observations at two distant stations have been necessary. These have to be made upon a moving object, the time of whose appearance cannot be predicted. It is only by a happy chance that good observations can be secured. If the height can be computed from measures made at a single station, a great advantage is gained. A second observer is not essential, if the position and shape of the auroral cloud is assumed to be as described above.

The distance on the earth's surface from the observer to the center of curvature of the nearest portion of the belt of frequent auroral displays can be measured. Represent this by d. Let the apparent altitude of the auroral arch be h, and its amplitude on the horizon be $2a$. Let x be the height of the auroral cloud

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[1] Mémoires de l'Académie Royale de Bruxelles, tome xx, p. 118. See also B. V. Marsh, this Journal [2], xxxi, 311.

[2] This Journal [2], xxx, 89.

from the earth, R the earth's radius, b the distance from the observer to the point on the earth directly under that part of the cloud which forms the vertex of the arch, and c the distance in like manner from the observer to the point underneath that part of the arch seen in the horizon. We have then these equations,

$$R+x=R\sec c=R\cos h\sec(b+h); \quad . \quad . \quad . \quad . \quad (1)$$

and since d, $d-b$, and c, are the three sides of a spherical triangle, and a the angle,

$$\cos(d-b)=\cos d\cos c+\sin d\sin c\cos a. \quad . \quad . \quad . \quad . \quad (2)$$

From these equations b and c may be eliminated, and x found in terms of a, d, h, and R.

From (1) $\cos(b+h)=\cos h\cos c$, and hence

$$\begin{aligned}\cos(d-b)&=\cos\{d+h-(b+h)\}\\ &=\cos(d+h)\cos(b+h)+\sin(d+h)\sin(b+h)\\ &=\cos(d+h)\cos h\cos c+\sin(d+h)(1-\cos^2 h\cos^2 c)^{\frac{1}{2}}. \quad (3)\end{aligned}$$

Equating (2) and (3) and dividing by $\cos c$,

$$\cos d+\sin d\tan c\cos a=\cos(d+h)\cos h+\sin(d+h)(\sec^2 c-\cos^2 h)^{\frac{1}{2}}. \quad (4)$$

But $\cos d=\cos(d+h)\cos h+\sin(d+h)\sin h$. Substituting, dividing by $\sin(d+h)$ and placing $\sin\varphi$ for $\sin d\cos a\operatorname{cosec}(d+h)$ we have

$$\sin h+\tan c\sin\varphi=(\sec^2 c-\cos^2 h)^{\frac{1}{2}},$$

Reducing, $\tan c=2\sin h\sin\varphi\sec^2\varphi$.

Hence, to compute the altitude of the auroral arch above the earth's surface, we have the three equations,

$$\sin\varphi=\sin d\cos a\operatorname{cosec}(d+h),$$

$$\tan c=2\sin h\sin\varphi\sec^2\varphi,$$

and

$$x=R(\sec c-1).$$

To apply these observations to particular cases I have selected 25 or 30 auroral arches, observed by Pres. Stiles, Prof. Olmsted, Mr. Herrick, and Mr. Bradley. They were all observed at New Haven, except those in the year 1860, which were seen by Mr. Bradley at Chicago. Most of these observations are from the Auroral Registers of Mr. Herrick and Mr. Bradley. These Registers will form part of a volume of memoirs about to be published by the Conn. Acad. of Arts and Sciences. The value of d is assumed to be 32°, which is also very nearly the distance from New Haven to the magnetic pole of the earth. In the following table are given the dates of the auroras, the observed apparent altitudes and amplitudes of the arches, and the computed values of x in miles, and in kilometers.

In selecting from the Registers the arches for this table, I have omitted those which were low in the north, as the horizontal extent is then concealed by the mists. I have also omitted those of which the observations were indefinite, or seemed imperfect.

Table of the observed altitudes and amplitudes of auroral arches, with the computed heights above the earth's surface.

Date.	Altitude.	Amplitude.	Height in miles.	Height in kilometers.	Observer.
March 27, 1781	66½°	165°	33	53	Stiles.
Sept. 21, 1840	10–12	120	52–68	84–109	Herrick.
March 6, 1843	5–8	100	42–83	67–133	"
April 13, 1845	8	100	83	133	"
April 27, "	10	90±	155	250	"
Oct. 9, "	8	100	83	133	Bradley.
Oct. 19, 1846	8–10	80±	165–214	266–345	"
Dec. 9, "	5	50	281	452	Herrick.
May 15, 1847	7–8	80	142–165	228–266	"
June 12, "	5	50	281	452	"
Aug. 4, "	4	60	154	248	Bradley.
" " "	7	90	98	158	"
Sept. 29, "	10	70–80	290–214	467–345	Herrick.
Nov. 25, "	10–15	100	111–183	179–295	"
May 18, 1848	7–9	75±	168–222	270–358	"
Oct. 23, "	5–6	70	134–165	215–266	Bradley.
March 18, 1849	10–15	100	111–183	179–295	Herrick.
April 6, 1850	10–12	90	155–193	250–310	Bradley.
Feb. 18, 1851	23	120–130	154–104	248–168	"
March 18, "	12	120	68	109	Olmsted.
Sept. 29, "	8	90	118	190	"
Sept. 29, 1852	10–15	100	111–183	179–295	Herrick.
March 26, 1860	6±	90±	80	129	Bradley.
March 27, "	10	90–100	155–111	250–179	"
July 4, "	10–12	100+	111–140	179–226	"
Aug. 12, "	5	90±	62	100	"
" " "	7	90±	98	158	"
Aug. 17, "	8	100±	83	133	"

The average height as indicated by the table is 134 miles, or 215 kilometers. The observations were not taken with reference to the use here made of them, and the results can therefore be regarded as only approximately correct. Mr. Bradley informs me that his method of determining the amplitude of the auroral arch was to place himself with his face directly towards one end, and then compare the amplitude with the arc of 90°, which was easily estimated. He was accustomed to consider the curve of the arch as continued down until it cut the horizon.

This method of determining the height of the auroral arch is imperfect in that it supposes for it a given regular form. In fact the auroral cloud is usually more or less irregular. Yet in view of the very great difficulties in securing good observations for parallax at two stations, the method is believed to possess very decided value, both independently, and as a check upon other measurements. It furnishes moreover the means of determining not only the magnitude of the auroral cloud, but also the breadth and height of the streamers which often rise from the arch-shaped mass of light.

Yale College, March 18th, 1865.

ART. XXXVI.—*On the Iron Ores of Marquette, Michigan;* by J. P. KIMBALL, Ph.D.

IT is proposed in the present article to record a few observations made, in the month of May last, in the Iron region of Marquette county, Michigan, with the view of elucidating certain obscure points in the geology of the immense beds of earthy red hematite, specular and magnetic iron ores, with which this district abounds to an unparalleled extent, constituting it beyond any doubt the most opulent source of iron in the world yet discovered. It was not my privilege carefully to explore this region beyond a limited district near the middle. I am therefore unable to add any information as to the geographical extent of the iron deposits to the published results of Foster & Whitney's survey. Indeed, the following contribution to the knowledge of this region is chiefly by way of observation of the instructive rock-cuttings in the iron mines, or rather quarries, and in one particular cutting of the Peninsula Railroad, all of which have been presented to geological study since the close of the government survey; and, inasmuch as neither population nor industry has advanced beyond these quarries, no other advantage to the geologist at present exists over the means of exploration employed by the United States geologists at that time. It is appropriate to remark in this connection that it must certainly be a cause of great regret to all friends of natural science and mineral industry in this country, that the federal government has omitted to publish the special report upon the Marquette Iron Region by Mr. J. D. Whitney, the results of a second geological exploration.

In their survey of that portion of the large area of the Northwest exclusively occupied by schistose, metamorphic and crystalline rocks, which is included in the northern peninsula of Michigan, Messrs. Foster & Whitney in 1851 marked out the outlines of two distinct systems of crystalline rocks, one of which was defined as metamorphic, their Azoic proper; while the other, distinguished as a great development of granitic rocks outstretching in separated expanses, was described under the name of granite belts or ranges. The relation to each other of these two systems was hardly traced to conclusiveness owing to the concealment of their conditions of contact; but the latter was assigned to an origin later than the Azoic series upon the testimony of disturbances of the upper metamorphic strata by intrusive masses of the granite, and earlier than the Silurian, as the lower beds of the Potsdam sandstone are seen to rest undisturbed around them.[1] The distribution of the Azoic and gran-

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[1] Report on the Geol. of the Lake Superior Land District, Part II, 1851, 44-48.

ite rocks, as laid down by the federal geologists, is presented in their map of the district between Keweenaw Bay and Chocolate River, to which reference can readily be had without encumbering these pages with a verbal recital.

The metamorphic or Azoic rocks, consisting of gneiss, hornblendic, talcose and chloritic slates, and beds of argillite, novaculite, quartzite, conglomerate, saccharoidal marble and crystalline limestone, were described as existing under conditions of great displacement, and displaying evidences of metamorphism, particularly in vicinity to the line of contact with the granite or igneous outburst. The occurrence of granite within the area of their distribution was recognized in the form of intrusive masses, while the greenstones and some of the schists, both following the general stratification, were regarded as trappean overflows, the former as an igneous product, the latter as pulverulent greenstone in the form of volcanic mud. The iron-ores of this series of rocks were described as alternating with the trappean ridges in the form of intrusive masses, and the remaining phenomena of their dissemination in many rocks of the series, were ascribed to processes of elimination and mechanical admixture prevalent during the accumulation of the original sediments. This explanation was believed to meet the mode of occurrence of the larger masses, which, at the time it was proposed, had not been extensively uncovered, and manifestly, as appears in the concluding paragraph of the chapter on iron-ores,[2] was not meant to be extended to beds of specular and magnetic oxyds of iron included within metamorphic strata, with a conformable range and dip, which indeed the federal geologists explicitly state their disposition to regard as the result of aqueous deposition.

The granite belts were defined in general terms to occupy an area of more than 2000 square miles, and, within the boundaries of Michigan, to be developed in two distinct ranges or spurs, of which the northern, including the Huron Mountains, forms the coast between Presqu'isle and Granite Point, and expands westward to a width of 25 miles. Along its southern intersection with the metamorphic series—its longest direction—it is laid down as continuous for sixty miles. It is separated by a zone of metamorphic rocks of some fifteen miles in width, which embraces the present iron industry of the region, from the southern belt, which is of very irregular outline, though nearly parallel for a distance of 36 miles along its northern intersection with the metamorphic belt. The large expanse of crystalline rocks occupying the undeveloped tract of the northern part of Wisconsin, stretching across the head waters of the Mississippi to

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[2] Foster & Whitney, ibid, 68.

the north and west of Lake Superior, was considered by the federal geologists to be the continuation of these granite belts. The granite was described as forming numerous parallel ridges bearing east and west, and in both belts to indicate leading axes of elevation. Gneiss was observed to flank the granite both on the north and south with intercalated beds of quartzite, potash, feldspar and hornblendic schists, the whole dipping uniformly from the axes of elevation. Hornblende sometimes was seen to replace mica, forming a light grey syenite. The granite south of the metamorphic belt is generally made up of feldspar and quartz, mica being almost always wanting, or very sparingly present. Feldspar is the most predominating mineral, large bodies of flesh-red orthoclase being of common occurrence. Two systems of dykes, and extensive intrusive masses of greenstone —the larger of a bearing uniform with the axes of elevation, the smaller being N.E. and S.W.—were indicated as occurring throughout the distribution of both belts. These are intersected by veins of quartz.[3]

A most engaging, and, at the same time, a very wide field of inquiry, is presented to geologists in addressing their attention to the ancient crystalline rocks of the Northwest, with the object of resolving the distinctive distribution of the Laurentian and Huronian series.

Although no detailed description of the crystalline rocks of the northern peninsula of Michigan has been given, so far as I am aware, since the close of the survey of the Lake Superior Land District, a noticeable disposition prevails among geologists to assign to the metamorphic strata, distinguished by Messrs. Foster & Whitney as Azoic, an equivalency with the Huronian series of Canada.[4] Such a tendency appears to be due to the investigations upon the south shore of Lake Superior of Mr. Alexander Murray, of the Canada Geological Survey,[5] the results of which have been employed by the provincial geologists in their comparative study of the crystalline rocks of Canada and its bordering districts.

The lines of demarcation between the Azoic strata and the granite, as laid down by the Land District Survey, appear to have been determined by the succession of beds rather than upon any evidence of unconformability. Orthoclasic gneiss, succeeded by alternating beds of quartzite and hornblendic slates, flanks the granite as it falls away from its axes of elevation, and the points at which these have been observed to be succeeded by argillaceous, chlorite, and talcose schists, and beds of conglomerate, afford the only clue to the boundaries of the

[3] Foster & Whitney; ibid, chap. iii.

[4] Geol. of Canada. 66, 596; Dana's Manual of Geology, 143.

[5] This Journal, [2], xxxi, 394.

two systems of rock, thus far presented. It has not been claimed for the information upon the geological structure of the region in question, that the relation of the Azoic series with the granite ranges, has been established by sufficient or definite data. This must continue to remain an open question until it is decided by patient and careful observations, facilitated by the development of the district, which, up to within a short period, was almost unvisited by white men, and still presents formidable physical obstructions to the work of geological exploration, scarcely less in degree to those experienced by the Land District Survey, and explained in the report of Messrs. Foster & Whitney. The recent direction of attention to the Silver-lead region, so-called, or the Huron Mountains, has served somewhat to open this obscure region during the past summer, by the laying out of roads, and the commencement of mining operations. Although the district has been pretty thoroughly explored during the past season in a spirit of mining adventure, it has engaged no particular study with a view to scientific results. It is proper to state, however, that upon the evidence of explorers and collections, obtained at Marquette, the metamorphic character of the Northern range is demonstrated by numerous instances brought to my notice, of the extensive development in that region of orthoclasic gneiss—often granitoid, quartzite, massive feldspars, talcose, hornblende and chlorite slates, seams of graphite and magnetic iron ore. My own observations in the Iron region impressing me with the indigenous character of the larger masses of diorite and granite represented within the defined area of the metamorphic strata, and their entire distinctness from intrusive dykes or erupted masses, and concurring in the recognition of these strata by Mr. Murray as Huronian, I am disposed to regard the entire region as of metamorphic character, all of whose larger masses of crystalline rocks are indigenous, and to be divisible into the two formations, Laurentian and Huronian: the former formation probably forming the surface of the areas known as the granite ranges, while the latter probably occupies, with minor deviations, the limits laid down for the crystalline schists comprehended under the name of Azoic.

This view is fortified by the fact that the Huronian or Azoic series is interposed between the granite or Laurentian rocks and the Potsdam sandstone, thus bringing the Laurentian in its proper place below the Huronian; while the only argument militating against it is the observation by Mr. S. W. Hill, of intrusive granite at one point cutting the slates near their southern line of intersection with the main granite, whence is derived the inference that the slates antedate the granite. The indigenous character of the greater part of the granite, however, throughout its general distribution, connected with the fact of its coëxtensive association with metamorphic sedimentary strata, tends in this instance to invest the intrusive granite with characters distinct from those of its greater development, and to impart to it the special phenomena of an independent dyke. Apart from the evidence of superposition, the lithological features of the Granite Belts, so far as they have been indicated, bear a marked analogy with the standard characters of the Laurentian series as determined in Canada and upon the north shore of Lake Superior.

In a private letter from Mr. T. Sterry Hunt, acquainting me with some of the unpublished leading results of Mr. Murray's examinations upon the south shore, I am informed that this geologist, whose opportunities for comparison cannot be too highly appreciated, describes the islands and some points on the main land north and northwest of Presqu'isle as of Laurentian gneiss with hornblende, epidote, and pyroxene, arriving at this conclusion through the observation to the south of these points of the greenish slates, greenstone, dolomitic limestones and conglomerates, associated with the beds of iron ore, and their relations to the gneiss. This latter series he judges to be the equivalent of the Lower slate conglomerate, or chloritic slates, at the base of the Huronian system, regarding them here as retaining nearly the same relative conditions as upon Lake Huron where the dolomite marks the division between the Upper and Lower slate conglomerates.

The gneiss which marks the boundary of the granite belts, and accordingly characterizes the top of the Laurentian series in this region, the same as it is elsewhere represented, is succeeded by dark colored hornblendic schists, which consequently represent the base of the Azoic or Huronian series. These schists are followed by a series of augitic rocks and schists, interstratified with magnesian hydrous rocks and slates, the two kinds of rocks being represented on the one hand by hypersthene, pyroxene, and bedded diorite passing into dioritic slates, and on the other by talcose and chloritic schists. The former character of rocks prevails to such an extent as to impart to the lower members of the Huronian series a distinctive augitic aspect. The several rocks composing this augitic zone are commonly of a greenish color, and vary in this respect chiefly as to shade, resembling in this particular the Lower slate conglomerate which marks the base of the series in Canada, and from which they seem to differ only in the absence of pebbles and boulders from the subjacent Laurentian rocks, which there form a distinguishing feature. Messrs. Foster & Whitney, however, describe as occurring upon Little Presqu'isle a pure white feldspar containing acicular crystals of hornblende, passing into separate beds of each of these component minerals, and enclosing angular fragments of hornblende and chlorite slates, jasper and a green magnesian mineral.[6] It seems not unlikely that this rock in its further distribution may possess more decidedly the characters of a conglomerate, and accordingly the more closely bear out the lithological analogy between the lower members of the Huronian series near Marquette, and their well ascertained characters north of the great lakes.

The hornblendic beds are succeeded by thick beds of pure quartzite displaying ripple marks, passing into quartziferous conglomerate on the one hand and siliceous slates on the other. The more massive beds of this description form a prominent feature of the country through their outcrop in an east and west ridge traversing the middle of the Azoic or Huronian area. They dip at high angles to the south, though numerous less elevated beds dip to the north. They are associated with greenish hornblendic slates and more or less crystalline diorite, and at their base with bands of dolomite somewhat siliceous and highly altered. Overlying this quartz zone, the probable equivalent of the Upper slate conglomerate of the North Shore, occur talcose, argillaceous and siliceous schists interstratified with specular iron ore, earthy red hematite and specular conglomerates, the latter resting immediately upon thick masses of hornblende rock blending into bedded diorite and hyperite on the one hand, and on the other into chlorite schists, and siliceous serpentines. At some points a dark colored quartzite resembling greisen except in the presence of hornblende instead of mica, and clearly of detrital origin, next underlies the ore beds.

Possessing the same stratigraphical conditions as the schistose rocks, while many varieties of them are represented in the schists by their exact counterpart as to composition, the crystalline Huronian rocks must be regarded as essentially metamorphic, while in a comprehensive view of the whole series it is seen that together with variable conditions of deposit, it indicates variable degrees of local metamorphism.[7] Plentiful evidence exists of the blending of a rock of one character into that of the other, or the continuity between crystallized and schistose beds. The diorites and hyperites of this region are never porphyritic, but on the contrary are so fine-grained as rarely to admit of the mechanical separation, or even the identification of their ingredients.

Besides the indigenous crystalline rocks distributed throughout the Huronian series, exotic or intrusive crystalline rocks are met with, but only in the form of dykes, and limited to a narrow distribution. The numerous elevated protuberances and ridges of granitoid gneiss, rising above the uppermost Huronian

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[6] Foster & Whitney's Report, ii, p. 18.

[7] Compare Foster & Whitney's Report, ii, 16.

schists which rest upon their flanks apparently unaffected except by the regular convolutions of the entire series, are undoubtedly of Laurentian age. Two systems of greenstone dykes, according to Mr. Hill, as cited by Messrs. Foster & Whitney, traverse the gneiss—the one bearing N. from 70° to 80° W.—the other N. 10° E., and the same authorities have noticed many instances of the intersection of the schists by intrusive granite, especially near the borders of the granite ranges.[8] Not having enjoyed the opportunity of following Mr. Hill's notes, I am unable to confirm these observations; but it may not be inappropriate to suggest the probability that the larger and more persistent bodies of greenstone bearing approximately east and west—that is conformably with the axes of the folds which constitute a regular system of flexures coëxtensive with the distribution of the Huronian series in the vicinity of Marquette, are in reality indigenous greenstone, and a portion of the development of the diorite upon which repose the upper members of the series, and which, as will hereafter be shown, is uncovered along most of the ridges of the region. No instances of trappean overflows in this region have been brought to notice. Quartz veins, sometimes carrying the sulphurets of copper, lead, and iron, are of frequent occurrence, occupying fissures evidently of an extensive vertical range. Workable veins of copper pyrites have recently been discovered. This is another point of agreement between the features of the Huronian series as developed in this district, and upon the north shore of Lake Huron, where copper-bearing veins are frequent, of which those of the Bruce, Wellington and Wallace mines are well known examples.

It is urged by Prof. Rivot of Paris, that the crystalline and schistose rocks south of Keweenaw Bay entirely correspond with the rocks of Keweenaw Point; for he asserts the syenite of the Bohemian Mts. to be equivalent to the Granite Ranges, or to the granite of the Huron Mts.; and the metamorphic schists of the Marquette district to be the stratigrapical equivalents of the intercalated greenstones, amygdaloids, sandstones and conglomerates, known as the Upper copper-bearing series, or the Quebec Group. Mr. Rivot thus distinguishes between the general characters of the rocks composing the Bohemian Mts. and the overlying series of beds, and believes to have discovered in the former, which he regards as erupted, the source of the elevation and displacement of the latter, which he regards as more or less completely metamorphosed sediments.[9]

Not to discuss at present the peculiar notions of Mr. Rivot, it will be well to advert very briefly to certain remarkable misconceptions upon which they are based. First: There seems to be

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[8] Foster & Whitney's Report, ii, 44.

[9] Rivot, Notice sur le lac Supérieur—Extrait des Annales des Mines, x, p. 56.

no evidence in support of Mr. Rivot's assumption that the greenstones associated with the hornblendic schists of the Marquette district are, like the copper-bearing rocks of Keweenaw Point, "traversed by veins containing copper and native silver." On the contrary, the vein phenomena of the crystalline schists forming the Azoic belt of Marquette county, are decidedly characteristic of the Huronian series, the lodes of which carry the sulphurets of copper unaccompanied by silver, and are altogether without the distinctive properties of the native copper lodes of Keweenaw Point. Second: The Paleozoic strata among which the greenstones of Keweenaw Point are intercalated conformably, and within the sectional development of which they are included, rest unconformably upon the crystalline schists of the Marquette district, and hence the stratigraphical identity between the rocks of these two districts, inferred by Mr. Rivot, is clearly out of the question. Third: The difference between the rocks composing the Bohemian Mts. and the superincumbent series is one merely of special lithological characters, and not one of general structure as assumed by Mr. Rivot. The syenite of Mt. Houghton has recently been found by Mr. W. H. Stevens, of Copper Harbor, to be intercalated upon the Reliance location with several massive beds of sandstone, conglomerate and amygdaloid, thus establishing uniform conditions of composition and structure to prevail throughout the entire series of the north-dipping copper-bearing rocks of the South shore.

Mr. Rivot was one of the first to assert the metamorphic character of the greenstones of the Marquette district, and, upon grounds of structural analogy alone, opposed the generally received opinion that the bedded syenites, diorites, dolerites, chloritic, zeolitic and epidotic amygdaloids, and so-called traps of the copper-bearing series, are erupted masses, having originated from successive overflows during the accumulation of the Paleozoic sediments in a horizontal position. Notwithstanding the stratigraphical arrangement of this series upon the South shore entirely bears out the theory of their metamorphic origin, certain observations of the Geological Survey of Canada upon the North shore tend to strengthen the view advanced by Messrs. Foster & Whitney.[10] If the explanation of trappean overflows on Thunder Bay, resting upon the authority of Sir William Logan, be confirmed by future investigation, the North shore must be regarded as the geological type of the Lake Superior country, and the absence of similar phenomena upon the South shore attributed to effects of denudation. If, on the other hand, the trappean overflows of Thunder Bay be found disconnected with the lower traps of the Upper copper-bearing series, but be ascertained really to belong to the system of dykes which traverse

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[10] Geol. Survey of Canada, p. 70.

this series on the North shore, and which have no counterparts in Michigan, it will remain for special lithological investigation to complete the evidence of the metamorphic origin of the Upper copper-bearing rocks of Lake Superior on the grounds of analogy with the conditions of their development in Canada. That the question has been left open by the Canadian geologists, is apparent from the fact that the stratified greenstones of this series, in a chapter by Mr. T. Sterry Hunt, are described as the stratigraphical and lithological equivalents of the diorites of the Quebec Group examined by him in Eastern Canada, which he asserts to be altered sediments, and to pass into chloritic, epidotic, and hornblendic slates.[11]

Allusion has already been made to the regularity of the flexures of the Huronian strata in the Marquette district, and to the general parallelism, and approximate east and west bearings of their axes. This regularity prevails throughout the Iron region proper, of which the hills and valleys conform to the resulting system of undulations—thus corresponding to anticlinal and synclinal arches and basins. The distance from one axis to the next of the same character varies from one half of a mile to one mile. Departures from the uniformity of direction occur at different points, but they may generally be directly traced to local causes of displacement. The plications in the strata which thus impart to the district its topographical configuration, have been subjected to powerful agencies of denudation, and, consequently, by the unequal operation of these agencies, are more or less imperfectly preserved. Hence their regularity is less superficial than stratigraphical; for while through the general denudation of the district, in common with the larger portion of the continent, by glacial action, the original surface of the rocks has been removed in a comparatively even manner, local agencies of erosion and excavation have served to break the continuity of ridges, and entirely to obliterate prominent outcrops over considerable areas.

Of the various indications of drift agencies with which this region abounds, the rounded and polished rocks or "*roches moutonnées*," and the glacial furrows which these surfaces retain, are the most frequent and instructive. It is almost impossible to meet with an outcropping rock whose character is such as to resist weathering influences, the surface of which does not exhibit one or the other, or both of these appearances. The course of the glacial grooves is invariably N. from 50° to 70° E., quartering with the direction of the valleys. Two, and not seldom three, systems of furrows can be traced, varying however but few degrees and quite alike as to distinctness. The rounded, polished and grooved surfaces are seen upon the flanks of the

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[11] Geol. Survey of Canada, pp. 612, 614.

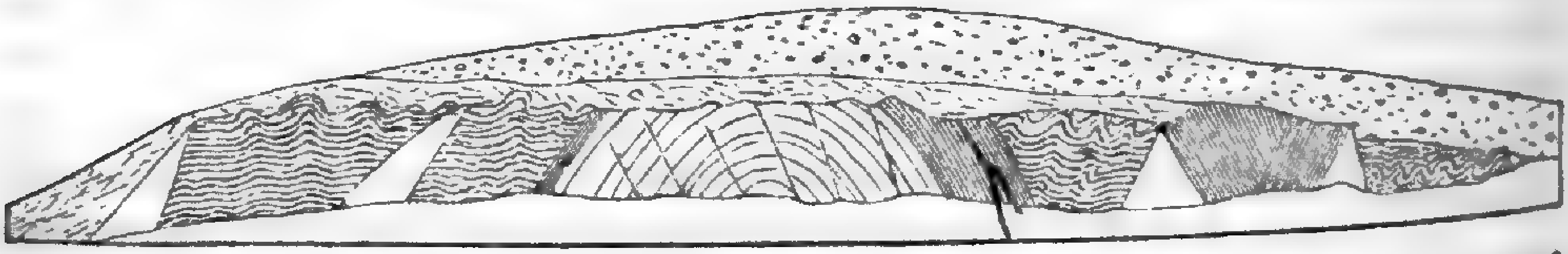
ridges, as well as upon their summits, and are invariably revealed by the removal from the surface of a rock of the alluvium and glacial drift by which it has been effectually protected. It is only in the outcrop of fissile schists that these appearances are wanting. The dense vegetation and the undisturbed covering of drift, aided by the deep snows of winter, almost entirely prevent the weathering of the rocks, so that the phenomena of glacial action are exhibited in this region in uncommon perfection. Fragments of rock occurring near by *in situ*, and boulders of specular iron are scattered throughout the drift, which here generally in fact appears to be made up of material derived from the immediate region, as seen especially in the circumstance that it is exceedingly ferruginous, while the source of the iron as a local detritus is evidenced by its exclusive presence as anhydrous sesquioxyd. Foreign boulders of gneiss occur however, considerable in size and number within limited areas of distribution.

The position of the beds of specular iron ore has already been stated to be at the top of the Huronian series as developed in the Marquette region, and it has also been remarked that they are interstratified with talcose and argillaceous schists. Sharing the plications of the entire series, these specular schists, as they may properly be called, are accordingly folded into synclinal basins and anticlinal crests—of which the axes in the case of the former lie below drainage, in the bottom of the valleys, and in the case of the latter are commonly obliterated through the erosion of elevated outcrops. Hence their outliers have been chiefly preserved along the flanks of the ridges where they have been the least exposed to the agencies of denudation, protected as they were against a drift current coursing not with the valleys but obliquely across them, by the elevated outcrops of their underlying rocks from the summits of which however they have been abraded. The denudation has been most extensive upon the purer specular schists and the earthy red hematites, while the only exceptions to these conditions of erosion and preservation as already given, occur with specular schists which acquire from their composition a refractory structure, or from their mode of deposit, the property of resisting the effects of a sweeping denudation.

The bosses of specular-iron—the iron-knobs or mountains, as they are called—are the most striking examples of exception to the general effects of denudation already noticed. They are instances of the preservation of the anticlinal crest, and owe their permanence to the fact that they are made up, not of pure and soft specular schists, but of specular iron ore closely interlaminated with quartz or jasper—a structure capable of resisting denuding action to a far greater degree than the homogeneous schists. The laminæ of jasper alternating with pure specular iron ore, vary in thickness from a hair's breadth to an inch, producing a banded appearance. The fact that the entire mass is highly contorted into minute as well as sizeable folds, furnishes an explanation of a power of resistance to vicissitudes of erosion, superadded to the same property which it derives from the mere presence of intermingled jaspery bands. The conglomeratic beds of specular iron ore and jasper, are generally preserved in elevated outliers, acquiring a refractory character from their coarse and tenacious structure, but I have met with no instance of the preservation of the anticlinal crest in beds of this description.

In Section 26, T. 47 N., R. 27 W., an example of a preserved anticlinal crest occurs in a ridge of half a mile in length and 170 feet in height. The specular iron-ore at the surface displays a laminated and contorted structure into which enter thin laminæ of quartz, and although the mass in itself is not so substantial as the exterior of the Cleveland Knob, its attending conditions of deposit impart to it similar refractory properties. In this region, as elsewhere, denudation may be noticed to have had less effect upon those stratified rocks which present to its agencies their planes of stratification, than upon those which are highly inclined. Hence it wiil be observed that the more extensive the sweep of the undulations the more complete is the preservation of the specular iron-ore which in the form of schists enters into them.

The most conclusive indications of the stratigraphical conditions which prevail among the hematite schists in Marquette county up to this time exhibited, are disclosed by the cutting of the Peninsula Railroad in the N.E. quarter of Section 8, T. 47 N., R. 26 W., to which allusion was made in the first part of this article. The grade of the railroad has been laid in an excavation 600 feet long and sunk 25 feet below the highest point upon the surface of a hill which derives its configuration from a boss of hematite schists intercalated with argillite. Thus the cutting presents an anticlinal section, as seen in the accompanying woodcut. The lower strata penetrated are preserved in an unimpaired



anticlinal crest, while the arch of the upper, sharing the general denudation of the district, has been abraded with the preservation only of the deflected dips of these strata. The schists are alternations of earthy red hematite and chloritic argillite, or the peculiar greenish slates, already described, both becoming gradually reduced in an ascending order from comparatively heavy beds of two feet to thin laminæ of a quarter of an inch. The power of resistance in the arch is strikingly illustrated; for while the crest itself remains perfect, the same strata have been much disrupted and displaced as they pass out of the arch and change their curvature. The upper laminated portions are exceedingly wrinkled, and the heavy beds broken and doubled. Under these circumstances of displacement but one or two of the heavier beds of argillite are exhibited above the grade of the railroad; and while these, through scales or plates retain marks of stratification so as to expose their true character, they have been sharply folded and collapsed, and by their nearly vertical position have the appearance of intrusive masses. The central portion of the cutting—that is, the undisrupted strata—is traversed by a system of parallel joints which the disrupted portions are without. Several segregated veins of quartz with limonite occupy the divisional planes of stratification.

Other sectional cuttings in the several mines or quarries display more or less completely the stratification and undulations of the hematites in common with the Huronian schists of this region, and show conclusively, if further testimony were requisite, the existence of the iron ore under the same conditions of deposit and secondary modification as the schists with which they are associated. In the Jackson mine, for instance, occur two bosses or short anticlinal folds, while in the Lake Superior mine a perfect synclinal flexure is exhibited.

Beds of specular conglomerate are of frequent occurrence throughout the Iron region of Northern Michigan, consisting of a paste of specular peroxyd of iron, through which are disseminated fragments of jasper, and rounded pebbles of specular iron ore which usually differ from the paste in texture, a difference very perceptible among ores of any one class even within narrow limits of distribution. These conglomerates not unfrequently resemble breccia in the angularity of the jasper fragments which they contain; but the pebbles of specular peroxyd, although sometimes obscure in a matrix of the same material, commonly serve to indicate the detrital origin of these beds. That they are derived from local detritus is evident from the fact that the jasper fragments are not rounded, while the particles of softer specular iron ore are worn but slightly. They seem to be of littoral formation and to have been derived from dismembered and crumbled deposits of successive laminæ of jasper and iron ore—similar to those deposits distinguishable in the bosses of the region. The specular conglomerate invariably exists under circumstances of true bedding, and is traversed by parallel joints splitting the imbedded pebbles. It occurs interstratified with talcose and argillaceous schists quite as regularly as the homogeneous ores. As would naturally be expected, the specular conglomerates, owing to their enduring composition, have resisted erosion more effectually than the purer schists, and are preserved in elevated outliers, as seen particularly at the New England mine, and in the N.W. quarter of Section 21, T. 47 N., R. 27 W. A specular conglomerate uncontaminated with any considerable portion of jasper, forms the bulk of the schists at present wrought by the Lake Superior mine. These are jointed obliquely, and are cleavable at right angles, to the stratification.

Specular schist often occurs charged with detrital quartz, or sand, thus differing from the conglomerates only in its external properties, while it is analogous to them in attitude and actual composition. Schists of this description are intercalated with the conglomerates in Section 21 above noticed, and in Section 26 of the same township are found underlying the laminated beds which crown the anticlinal arch.

An estimation of the thickness of the ferriferous series is attended with no little difficulty at present owing to the absence of any entire sectional cutting. The Jackson mine presents the best data for an opinion upon this point, although its workings expose neither the base, nor uppermost members of the series. It is upon the northern dip of a steep anticlinal, and the ground of the quarry is some 500 feet wide, the excavation having been entirely within the series. At this mine the range of thickness exceeds one thousand feet. Individual thicknesses of specular schist without the intervention of other schists, are upwards of 150 feet. Some idea of the massiveness of a homogeneous bed of specular schist may be had from the fact that in the month of September last 3500 tons of workable ore were thrown down at this mine in one blast, for which 11 kegs of powder were used.

It will be observed that while the smaller plications furnish the most available and complete evidences of the stratigraphical conditions of the ferriferous schists, every exposure of them in quarries or natural outcrops, conveys the same character of evidence, but upon a scale far more extended, and generally requiring allowance for superficial vicissitudes, and a large degree of denudation. Even if space permitted, I conceive it to be unnecessary to multiply instances of this evidence. It has been shown that the iron ores of the Huronian series in Michigan are essentially schists and heavy-bedded strata in which none of the phenomena of aqueous deposits formed by precipitation from water on the one hand, or by detrital accumulation on the other, are wanting. They exhibit not only stratification, anticlinal and synclinal folds, but are invariably traversed by systems of joints, and at many points exhibit a perfect slaty cleavage.

The intimate connection between the greenstones, hornblende rocks, and aluminous and magnesian silicated schists of the ferriferous series, has already been indicated in general terms, these rocks not only alternating with, but passing into, each other—a conjunction commonly existing wherever bedded greenstones, and isolated silicates occur together as sedimentary products of the decomposition of compound silicated rocks. The peculiar green slates which have a large development in this series, are intermediate in composition between clay slate and hornblende slate, and together with the talcose and chloritic slates, with which they are interstratified, are probably products of such a decomposition in the wet way of the same crystalline sediments which entirely or less undecomposed have gone to form those greenstones which constitute members of the same series. Effervescence with acids of some of the green slates in common with many of the greenstones of the series, together with the presence of a large amount of lime and magnesia, was pointed out by Messrs. Foster & Whitney, as indicating the characters of a pulverulent greenstone—the schalstein of German geologists.[12] The presence of carbonate of lime in many of the schistose rocks of this region must be regarded as generally due to the decomposition of silicate of lime in the silicates of the primary crystalline sediments whence they are derived, although in a few special instances it may have been derived by the subsequent conversion of greenstones into schalstones, or by infiltration from superincumbent calcareous strata. Chemical reactions in crystalline sediments resulting from the disintegration of crystalline silicated rocks, and operated upon by carbonated waters, are amply capable to have produced the lithological conditions of augitic rocks, clay-slates, schalstone, and other schists, together with the oxydized ores of iron intercalated with greenstone among the ancient crystalline rocks of North America as well as Europe—as shown by Bischof[13] in pronouncing the Neptunian characters of these rocks. From a stratigraphical point of view, while evidence is elsewhere often obscure, the Huronian greenstone, schists and iron ores of Northern Michigan, in the absence of close attention to their special chemical conditions, exhibit sedimentary and metamorphic phenomena adequate to render quite untenable, it is believed, the theory of the exotic character of any portion of them.

New York, Dec. 19th, 1864.

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[12] Report of the Geology of the Lake Superior Land District, ii, 17, 93.

[13] Bischof, Elements of Chemical and Physical Geology, London, 1854–59, vol. ii, chap. 55. Volger, Studien zur Entwicklungsgeschichte der Mineralien. Zurich, 1854.

ART. XXXVII.—*Astronomical Photograyhy;* by LEWIS M. RUTHERFURD.

MY present observatory is a circular brick building of twenty feet internal diameter, with a light revolving roof supported on twelve wheels which are fixed to the stone coping of the walls. The opening, two feet wide, extends from side to side with simple shutters, which, when elevated on the weather side, serve to prevent the wind from blowing into the observatory and shaking the telescope. Opening from the west side of the Equatorial dome is a small transit apartment with computing room attached. This observatory is in the garden of the house where I reside. The transit is 189 feet N.W. from the Second Avenue, and 76·3 feet N.E. from Eleventh street. It was erected in the summer and autumn of the year 1856. The equatorial, by Fitz, is a very substantial instrument, having circles divided on silver 18 and 20 inches in diameter.

The objective is of $11\frac{1}{4}$ inches aperture, and fourteen feet focal length, and was corrected for figure by myself after the methods and directions of Mr. Fitz. It is a fine glass, capable of showing any object which should be seen by a well corrected objective of those dimensions.

The observatory is low and therefore cannot reach any object near the horizon, but I prefer losing such observations to the tremors and expense of a high structure.

The transit room has been used on several occasions by the U. S. Coast Survey in their telegraphic operations for longitude. It is $0^{h}. 12^{m}. 15{\cdot}47^{s}$ E. of Washington, and in latitude $40°, 43', 48''{\cdot}53$; the latitude being the result of observations with the zenith telescope upon twenty-four pairs of stars by the observers of the Coast Survey.

During the winter of 1857–58, Messrs. Alvan Clark & Sons constructed, and in the spring attached to the equatorial, a driving clock of the highest merit. It has a remontoir escapement similar to that of Bond's spring governor.

Having seen with great interest the photographic experiments conducted at the observatory of Harvard College, I determined, as soon as the clock should be in working order, to prosecute the subject of celestial photography. After many experiments it was ascertained that the best photographic focus of the objective was about $\frac{7}{10}$ of an inch outside the visual focus. I continued making photographs of the moon and such stars as could be obtained, and although when compared with what had been done by others the results gave reason for satisfaction, yet in view of what was desirable and apparently attainable, astronomical photography with me was a failure. By reducing the aperture of the telescope to five inches for the full moon, I was enabled to produce negatives which would bear an enlargement to five inches or fifty diameters. An impression of a sixth magnitude star was never obtained, γ Virginis, then 3″ distant, was the closest pair the duplicity of which could be measured on the collodion plate. The ring of Saturn and the belts of Jupiter were plainly visible, but entirely unsatisfactory. An image of Jupiter could be obtained in from 5 to 10 seconds exposure, but the satellites failed to impress the plate in any length of time. This was due to the uncorrected condition of the objective which diffused the violet rays over a large space, so that in the case of the planet each point of the picture was influenced not only by the ray due to that point, but by the stray beams from adjoining portions of the object, and thus nearly the whole actinic force of the objective was gathered within the dimensions of the image. In the case of the satellite the lost rays were not replaced by the wanderers from any adjacent point.

During the summer of 1858 I combined my first stereograph of the moon, producing quite a satisfactory result with the low power of the stereoscope. I do not know when this was first done in England by Mr. De La Rue, but with me the idea was an original one.

My greatest success with an uncorrected objective was in the pictures of the sun taken with about one-fiftieth of a second exposure, with the aperture reduced to one inch. The negatives were four inches in diameter and exhibited the spots with reasonable sharpness, the manifest difference in light between the center and the edge, and under favorable circumstances the faculæ. Some of the negatives verify the observation of M. Dawes, that the faculæ are elevations.

In June, 1860, the sun's disk was remarkably rich in spots, and I combined the pictures of two days to produce a stereograph, but the result was a failure and did not give the impression of a sphere, but presented the appearance of a flat uniform disk spanned by a spherical net-work which seemed entirely detached from the disk. This is attributable to a want of sufficient detail on the surface of the sun.

During the year 1859 and for a long time I worked with combinations of lenses to be inserted in the tube between the objective and the plate with the view of correcting the photographic ray. This attempt succeeded well so far as the center of the field was concerned, but it was impossible to produce a good correction over a space equal to the area of the image of the moon, without using a corrector of inconvenient size.

In 1860 I prepared a telescope with camera and instantaneous apparatus mounted equatorially to send by the U. S. Coast Survey Expedition to Labrador for the observation of the eclipse.

The objective in this case was a fine one, by Alvan Clark of 4¼ inches aperture. A ring was placed between the crown and flint lenses of such a width that the best visual and photographic foci were united. For this purpose it was necessary to shorten the combined focus about one-twentieth of its former value.

The pictures of the sun taken with this instrument were better than those made by my large telescope, in which no attempt had been made to correct the photographic rays.

Being unable to accompany the expedition, I made a series of pictures of the eclipse at home, upon which are seen the nuclei and penumbræ of the spots, the gradation of light of the sun's disk, and the serrated edge of the moon projected upon the sun. They show, however, none of the fogging of the moon's surface, commented upon by other observers, nor a greater intensity of light at the points of contact between the sun and the moon, both these results are, when they occur, due, in my opinion, to photographic or optical causes, and not to any true astronomical phenomena of that nature.

On examining the first negative of the eclipse I was struck by the difference of sharpness between the edge of the sun and that of the moon projected upon its disk. At first I was inclined to think that it was caused by a falling off in definition near the edge of the eye-piece used. In the next picture the edge of the sun was placed near the center of the field and the moon removed to a remote part of the plate, yet still the result was the same; the sun's edge was soft and indefinite while that of the moon was hard and sharp, showing that the light from the two objects comes to us under different conditions; in one case traversing the sun's atmosphere, in the other unaffected by this disturbing cause.

In the autumn of 1861 I began to experiment with a reflecting telescope with silvered mirror, which recommended itself both by the simplicity and ease of its construction and the entire freedom from dispersion. One was mounted of thirteen inches aperture and eight feet focus, of the Cassegranean form. It was ground and approximately figured by Mr. Fitz, and in its frame, as strapped to my large tube and carried by the equatorial clock, weighed less than fifteen pounds. Many modes were tried of silvering but the best results were obtained by Liebig's process, wherein the silver is deposited from an ammonia nitrate solution by sugar of milk. After three months trial I abandoned this instrument as unfit for use in my observatory. First, the tremors of the city, quite imperceptible in the achromatic, were, by the double reflection, increased about 36 times, an insurmountable obstacle to good work. Secondly, the silver deposit is so easily attacked, both by moisture and the gases which abound in the city, as to make it necessary to re-silver the specuhim at least every ten days, a labor not to be contemplated with equanimity. Dr. Draper has found the silver surface very much more durable in the dry, pure air of the country. I regard the Cassegranean form as the best adapted to lunar photography, since the dimensions of the image can be varied at will, as circumstances dictate, by simply changing the small mirror, a number of which might be kept at hand.

Having thus failed in astronomical photography with an ordinary achromatic, with a correcting lens and with a reflector, I began, in the autumn of 1863, the construction of an objective, to be corrected solely with reference to the photographic rays.

In a former communication to this Journal, Jan. 1863, I drew attention to the peculiar adaptation of the spectroscope as a means of examining the achromatic condition of an objective, and since it was principally by the aid of this instrument that I have been enabled to procure a fine photographic correction, I may be pardoned for touching again upon this application.

The image of a star at the focus of a perfectly corrected objective would be a point, the apex of all conceivable cones having the object glass, or parts of it, as the bases. This point falling upon a prism would be converted in a line red at one end and violet at the other with the intermediate colors in their proper places. If, however, the different colored rays are not all brought to the same focus, the spectrum will no longer be a line, but in the uncorrected colors will be expanded to a brush the width of which will be the diameter of the cone where intercepted by the prism. It will thus be seen that a simple glance at a star spectrum will indicate at once what parts of the spectrum are bounded by parallel lines and consequently converged to one focal point, and what parts do not conform to this condition, and also the amount of divergence.

On applying this test I found that an objective of flint and crown in which the visual was united with the photographic focus, (in other words, where the instrument could be focalized on a plate of ground glass by the eye, as in ordinary cameras, and in the heliographs constructed by Dalmayer for the Kew observatory and for the Russian government), is a mere compromise to convenience in which both the visual and actinic qualities are sacrificed.

In order to bring the actinic portion of the spectrum between parallel borders, i. e. to one focus, it is necessary that a given crown lens should be combined with a flint which will produce a combined focal length about one-tenth shorter than would be required to satisfy the conditions of achromatism for the eye, and in this condition the objective is entirely worthless for vision.

Having obtained the achromatic correction, I had a most delicate task to produce the correction for figure, since the judgment of the eye was useless unless entirely protected from the influence of all but the actinic rays. A cell of glass inclosing a sufficient thickness of the cupro-sulphate of ammonia, held between the eye and the eye-piece, enabled me to work for coarse corrections upon α Lyræ and Sirius, but so darkened the expanded disk of a star in and out of focus that all the final corrections were made upon tests by photography, which gave permanent record of all the irregularities of surface to be combated. Still, however the process was long and tedious, dependent upon but three stars as tests, and they too often obscured by bad weather. My mode of correction was almost entirely of a local nature, such as practiced by the late Mr. Fitz and Mr. Clark for many years.

This objective was completed about the first of December last; it has the same aperture, 11¼ inches, as the achromatic, with a few inches shorter focal length, and can be substituted for it in the tube with great ease. The corrections of this objective are such that I think it capable of picturing any object as seen, provided there be sufficient light and no atmospheric obstacles.

As respects the light, I have obtained images of stars designated by Smythe as of the 8½ magnitude, and other stars on the same plate of full a magnitude lower. In the cluster Prœsepe, within the space of one degree square twenty-three stars are taken, many of which are of the ninth magnitude, with an exposure of three minutes. An exposure of one second gives a strong impression of Castor, and the smaller star is quite visible with half a second. With the achromatic objective it was necessary to expose Castor ten seconds to obtain a satisfactory result.

The great obstacle which prevents the results of photography from realizing the achievements of vision is atmospheric disturbance. In looking at an object the impression is formed from the revelations of the best moments, and it is often the case that the eye can clearly detect the duplicity of a star, although the whole object is dancing and oscillating over a space greater than its distance. The photograph possesses no such power of accommodation, and the image is a mean of all the conditions during exposure. It is, therefore, only on rare nights in our climate that the picture will approach the revelations of the eye.

Since the completion of the photographic objective, but one night has occurred, (the 6th of March), with a fine atmosphere, and on that occasion the instrument was occupied with the moon; so that as yet I have not tested its powers upon the close double stars, 2″ being the nearest pair it has been tried upon. This distance is quite manageable provided the stars are of nearly equal magnitude. The power to obtain images of the 9th magnitude stars with so moderate an aperture promises to develop and increase the application of photography to the mapping of the sidereal heavens, and in some measure to realize the hopes which have so long been deferred and disappointed.

It would not be difficult to arrange a camera box capable of exposing a surface sufficient to obtain a map of two degrees square, and with instruments of large aperture we may hope to reach much smaller stars than I have yet taken. There is also every probability that the chemistry of photography will be very much improved, and more sensitive methods devised.

On the 6th of March, the negatives of the moon were remarkably fine, being superior in sharpness to any I have yet seen. The exposure for that phase, three days after the first quarter, is from two to three seconds, and for the full moon about one-quarter of a second.

The success of this telescopic objective has encouraged me to hope that an almost equal improvement may be made for photography in the microscope, which instrument is more favorably situated for definition than the telescope, since it is independent of atmospheric conditions. Its achromatic status is easily examined by the spectroscope, using as a star the solar image reflected from a minute globule of mercury. Mr. Wales is now constructing for me a one-tenth objective, which, upon his new plan, is to be provided with a tube so arranged as to admit of the removal of the rear combination, and, in place of the one ordinarily used, one is to be substituted at will which shall bring to one focus the actinic rays.

Art. XXXVIII.—*Notes on Coal and Iron Ore in the State of Guerero, Mexico;* by the late N. S. Manross. (From an unpublished Report dated May, 1857).

The region here described is situated near the Pacific coast, just west of the meridian of the city of Mexico, extending 170 miles west of this line, and about 75 miles from the coast.

We have not succeeded in proving that true coal exists in this part of Mexico. Of the reported localities which we examined, the *first*, that of Chilpaucingo, proved to be a bed of soft and peaty brown coal, which might answer perhaps for local purposes, but would not bear transportation. The *second*, near Huetamo, was an irregular vein of asphaltum resembling the "Albert Coal" of Nova Scotia. The slates here bore a strong resemblance to those of the Coal-measures. Other rumored deposits in Tempantitlan and Los Nuevos turned out to be in one case pitchstone, and in the other black tourmaline. The people of the country have so little idea of what coal really is, that they are liable to mistake any black mineral for it. Their information is therefore very unreliable, and it is always best to conduct any examinations for coal by sections without regard to their reports. I am still of the opinion that true coal may be found in this country. The Carboniferous or Mountain limestone is largely developed here; and we have seen in several places shales and sandstones which in their lithological character strongly resemble those of the Coal-measures in other countries. What is still lacking is the evidence of fossils, to prove that a coal vegetation has once existed in these latitudes.

Our examinations for the metals have been highly successful. Iron ores of the very best quality, and inexhaustible in quantity, have been found in six different localities. The *first* is situated at a distance of four leagues from the town of Mescala. It is a vein 25 feet wide, and nearly vertical, and consists of solid magnetic ore. We have specimens from other similar veins in the vicinity, which we had not time to visit. There is no doubt of an abundant supply of ore in this region, and it could be readily transported to the river. It derives importance from the fact that it is upon the present direct road from Mexico to Acapulco.

The *second* great deposit is the Cerro Yman, or Magnet mountain, of Las Anonas Grandes, near Coyuca. This is also about four leagues from the river. It is a mountain of iron ore, fully equal to the celebrated ore of Missouri. It is about 300 feet high, half a mile long, and fully one-third of its bulk is pure magnetic ore. It also contains a bed of limestone in its summit. It is owned by a company who have some small furnaces near it, and are producing small quantities both of iron and steel from it.

Two other deposits, those of San Francisco and Singungao [Singangeo?], situated near each other, may be classed together as a *third* great locality. They are about two miles apart and about the same distance from the Mescala river. That of San Francisco is a conical hill about 150 feet high, consisting principally of pure magnetic ore. Its surface is covered with loose blocks of the same, from one to five feet in diameter. That of Singungao covers several acres near the summit of a mountain six or eight hundred feet above the river. Besides paving the ground over this space with large blocks of ore, the vein throws up several piles of ore in masses of ten or twelve feet diameter. Taken together or singly, these deposits are capable of yielding an unlimited supply of the very best ore. They are not worked at all at present. A *fourth* great deposit was met with near Villadero, within a day's ride of Zacatula. Here a bed of ore four hundred yards long and one hundred wide occupies the side and crosses the summit of a low hill. The surface is made up of blocks of pure ore from three to five feet in diameter.

The country around is well wooded and nearly level from this place to the Pacific.

The *fifth*, and, all things considered, the finest deposit we have seen, is situated near Chutla, on the road from Zacatula to Acapulco. It is not more than three miles in a direct line from the sea, and not more than six from a bay which affords a good and safe anchorage and landing. This deposit extends along the side of a mountain for more than a quarter of a mile, is several hundred feet wide and affords masses of ore of all sizes from ledges down to pebbles. The highest point of the bed is about 150 feet above the adjoining plain. On the upper side the deposit is bounded by a thick bed of soft limestone, affording an easy supply of flux. The ore is pure magnetic, and is the finest we have seen. The country around for many leagues has a fertile soil and is covered with dense forests of hard woods, which would produce an unfailing supply of the best charcoal. This location has special advantages in the quality and quantity of the ores, the convenience of fluxing materials and fuel, and the nearness of a good landing, with the whole of the Pacific coast for a market. I see no reason why the best qualities of charcoal iron could not be produced here as cheaply as elsewhere, with the advantage of freight and duties over any foreign competition in the supply of the Pacific coast of Mexico, and to a considerable extent also of the interior.

One other large deposit of iron was passed over, on the shore between Petatlan and Coyuguilla. It is a hill of large blocks of ore, situated immediately upon the shore, and beaten by a heavy surf. The ore was not seen in place, but the quantity was sufficient to show that a very large bed of it exists in the hill. The beach for several miles was blackened by sand derived from the disintegration of the ores.

We have information of another large deposit of iron ore, near the Hacienda of General Alvarez, which we may perhaps see on our way to Mexico.

The occurrence of so many and so rich deposits of iron, so widely distributed over the country, is interesting, from the assurance it gives that, whenever coal shall be discovered, there will be no want of iron ore within accessible distance of it.

Next to iron, copper appears to be the most abundant metal in this portion of Mexico. I shall defer to another occasion a description of the many localities of this metal which we have seen. One mine, that of Inguaran, has been worked since the time of the conquest, and is still producing well, though worked without the aid of machinery. Others in the same neighborhood, said to be equally rich, are entirely idle. The specimens and information which we have collected are sufficient to convince me that this portion of Mexico is one of the richest copper districts in the world. Silver and lead ores occur in abundance, but I will not undertake to describe here the various localities of them which we have seen, or the specimens which we have collected.

I am fully convinced that this portion of Mexico, when properly opened by the application of intelligent enterprise, will prove one of the richest mining regions of America.

ART. XXXIX.--*Numerical Relations of Gravity and Magnetism;* by PLINY EARLE CHASE, M.A., S.P.A.S.[1]

IN the fifth century before the Christian era, Leucippus and his disciple Democritus taught that heat is the soul of the world, the principle of life and intelligence, and that space is an infinite plenum, pervaded by material atoms too minute to be perceptible to the senses, which, by their constant motions, unions, and separations, form the beginnings and ends of things. In this theory, which is said to have been borrowed from the priests of Isis and Osiris, we may trace the origin of the modern belief in a universal kinetic æther, and of the attempts to resolve all forces into "modes of motion," which were practically inaugurated by our own countryman, Benjamin Thompson, Count Rumford, and which have been so successfully prosecuted by Carnot, Seguin, Mayer, Colding, Joule, Grove, and their collaborators.

The mutual convertibility of Light, Heat, Electricity, Magnetism, Chemical Affinity, and Vital Energy, may be now regarded as one of the most probable physical hypotheses. Faraday has endeavored also to connect gravitation and magnetism or electric action by experimental results, but in vain. Still, the conviction of such a connection is almost irresistible, and various physicists have given us incidental pointings in that direction. Ampère discovered the magnetic effect of electric currents circulating around iron bars; Arago, whose experiments were repeated and extended by Babbage, Herschel, Barlow, Christie, and others, showed that simple rotation produces magnetic disturbances which are governed by fixed laws; the distribution of induced magnetism in masses of iron, as determined by Barlow and Lecount, is the same as would follow from the relative centrifugal motions of different portions of the earth, provided the magnetic axis corresponded with the axis of rotation;[2] Hansteen suspected, and Sabine practically demon-

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[1] From the Proceedings of the American Philosophical Society, Dec. 16, 1864. The Magellanic Gold Medal was awarded the author for this memoir.

[2] This fact was first announced by me, at the Society's meeting, April 15, 1864. See Proc. A. P. S., ix, 367.

strated, the influence of the sun upon terrestrial magnetism; Secchi ascertained that "the diurnal excursion of the needle is the sum of two distinct excursions, of which the first depends solely on a horary angle, and the second depends, besides, on the sun's declination,"[3] and that "all the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance."[4]

This hypothesis has been objected to, on the ground that it is difficult to understand how any conceivable intensity of solar magnetism, by its simple induction, could produce so great a disturbance as is daily observed. Therefore it will probably follow the fate of the earlier ones, which attributed terrestrial magnetism to one or more powerful magnets lying nearly in the line of the earth's axis, while Barlow's idea that the magnetism is superficial, and in some manner induced,[5] will still remain in the ascendant. Secchi's conclusions are, however, none the less interesting, and from the fact that magnetism is, like gravity, a central force, varying inversely as the square of the distance, they lend encouragement to those who are endeavoring to find new evidences of the unity of force.

My own experiments and researches have led me to the belief that all magnetism is a simple reaction against a force which disturbs molecular equilibrium, that the numerical equivalent of the magnetic force is therefore equal and opposite to that of the disturbing force, ($\pm M = \mp D$), and that all the phenomena of terrestrial magnetism result from tidal and thermal changes in terrestrial gravitation.

Sullivan[6] and Reinsch[7] have pointed out the effect of musical vibrations upon the magnetic needle, and I have shown the controlling influence of a purely mechanical polarity.[8] A careful examination of the polarizing thermal and rotation currents,[9] will show that the spirals, which they have a tendency to produce, are quasi horizontal cyclones, one set flowing in a nearly constant direction along the magnetic meridian, and the other toward the momentarily shifting solar meridian. From an investigation of these currents and a comparison of various observations, I have deduced the following theses:

I. The daily magnetic variations, though subject to great disturbances, at different hours, show an average approximation to the differences of the gravitation-tidal currents.

Hours from Mean, - - - -	1h.	2h.	3h.
Means of Theoretical Ratios, - - -	·500	·866	1
" " Observed " - - -	·563	·865	1

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[3] Phil. Mag. [4], viii, 396.

[4] Ibid. ix, 452.

[5] See De la Rive's Electricity, ii, 635.

[6] Phil. Trans., 1831.

[7] Phil. Mag. [4], xiii, 228.

[8] Ibid., p. 367 seq.

[9] Proc. A. P. S., ix, 359.

II. Marked indications of an accelerating force are discoverable in the magnetic fluctuations, especially during the hours when the sun is above the horizon.

Hours from Mean,	1h.	2h.	3h.
Mean Ratios of Hourly Tidal Differences,	100	73	27
" " " Squares of Hourly Magnetic Differences,	100	74	26

See also Thesis V.

III. There are lunar-monthly barometric and magnetic tides, which may be explained by differences of weight or momentum,[19] occasioned by the combined influences of solar and lunar attraction, and terrestrial rotation.

IV. The solar diurnal variations of magnetism between noon and midnight are nearly identical in amount with the variations of weight produced by solar attraction at the same hours.

The ratio of the solar to the terrestrial attraction for any particle at the earth's surface, being directly as the mass, and inversely as the square of the distance ($M \div R^2 = 354,936 \div 23,000^2$), is ·00067. The weight of any particle is therefore increased by this proportionate amount at midnight, and diminished in the same proportion at noon, making a total half-daily variation of ·00134 in the atmospheric weight, and consequently, according to my theory, in the terrestrial magnetism.

Theoretical variation, ·00134. Observed variation, ·00138.

V. The magnetic variations at intermediate hours, between noon and midnight, indicate the influences of an accelerating force, like that of gravity, modified by fluctuations of temperature, and by atmospheric or æthereal currents.

Every particle of air may be regarded as a planet revolving about the sun, in an orbit that is disturbed by terrestrial attraction and other causes. In consequence of these disturbances, there is an alternate half-daily fall toward the sun, and rise from the sun. By the laws of uniformly accelerated and retarded motions, the mean fall and the consequent mean magnetic disturbances should occur at $12^h \div \sqrt{2} = 8^h\, 29'$ from midnight.

Theoretical mean, $8^h\, 29'$. Observed mean, $8^h\, 31'$.

VI. Some of the magnetic influences appear to be transmitted instantaneously, through the rapid pulsations of the kinetic æther; others gradually, through the comparatively sluggish vibrations of the air.

VII. The comparative barometric disturbances of the sun and moon exhibit an approximate mean proportionality between their comparative differential-tidal and magnetic disturbances.

Let the solar differential-tidal force be represented by A′, and

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[19] I believe there can be no weight without some degree of momentum. See Proc. A. P. S., ix, 357.

the lunar by A″, the respective barometric disturbances by B′ and B″, and the magnetic disturbances by M′ and M″. If M′ and B″ are required, we have

	A′÷A″	B′	B″	M′	M″
Theoretical values,			·00012	·00144	
Observed "	2·55	·00057	·00013	·00140	·0000255

VIII. The theoretical gravitation-variation of magnetism (Prop. IV) is slightly less, while the theoretical barometric variation (Prop. VII) is slightly greater, than the corresponding observed variation. The excess in one case exactly counterbalances the deficiency in the other, the sum of the theoretical being precisely equal to the sum of the observed variations.

IX. The total daily magnetic variations, like the barometric, can be resolved into a variety of special tides, which may be severally explained by well-known constant or variable current-producing and weight-disturbing forces.

Hours from Midnight.	A Theoretical Gravitation Tide.	B Theoretical Differential Solar Tide.	A+B Theoretical Mean Tide.	Observed Mean Tide.
0	−·00067	+·00024	−·00043	−·00043
6	·00000	−·00024	−·00024	−·00023½
12	+·00067	+·00024	+·00091	+·00095

The hours are counted from midnight, in each half-day.

Column A contains the hourly differences from mean weight, attributable to solar gravitation, with changed signs; diminution of weight being accompanied by increase of magnetism, and *vice versa.*

The form of the tide in column B is evidently such as should be determined by solar action. The magnitude of the tide is estimated by comparing the relative amounts of motion down the diagonal and down the arc of a quadrant ($\cdot 00067 \times [1 - (\frac{\pi}{4} - \frac{1}{2})] = \cdot 00048$). The mean-tidal difference $[(\cdot 00067 - \cdot 00048) \div 2]$ is very nearly equivalent to the average theoretical inertia-disturbance of weight. The atmospheric inertia at St. Helena, (regarding the fluctuations as uniform between successive hourly observations,) produces retardations of 59′, 85′, 26′, and 31′, at 0^h, 6^h, 12^h, and 18^h, respectively. The mean retardation is 50′, or $\frac{5}{72}$ of a half-day. The theoretical daily gravity-variation being ·00134, the average variation in $\frac{5}{72}$ of a half-day is $\cdot 00009\frac{11}{36}$, the mean tidal difference being $\cdot 00009\frac{1}{2}$.

The consideration of the moon's disturbance of the atmospheric gravitation is complicated by the magnitude of its differential attraction, the position of the center of gravity of the terrestrial system, the varying centrifugal force, and other circumstances involved in the lunar theory. Still there are indications, in the following synopsis, of the influence of gravity, sufficiently striking to encourage a hope that our knowledge of the moon's perturbations may be improved by a thorough comparative study of the lunar astronomical, atmospheric, and magnetic tables.

Lunar-daily disturbances of Magnetic Force at St. Helena, in millionths of the total force.

Hours.	0	1	2	3	4	5	6	7	8	9	10	11	12
Before Lunar M.	+5	—1	+4	−2	−5	−5	—6	−3	−2	- 1	+14	+15	+16
After " "	+5	−1	−5	−6	−7	−6	+1	+1	−2	+18	+25	+22	+16
Mean,	+5	−1	−0·5	−4	−6	−5·5	−2·5	−1	−2	+ 8·5	+19·5	+18 5	+16
Rotation-Tide.	0	0	±4·5	±2	±1	±5	∓3·5	∓2	0	∓ 9·5	∓ 5·5	∓ 3·5	0

The above table shows, that

1. The moon's attractive force ($M \div R^2 = ·016 \div 60^2 = ·000004$) multiplied by the coëfficient of its differential attraction (2·55) gives ·0000113, which is nearly the same as the mean meridional magnetic disturbance [(·000005+·000016)÷2=·0000105].

2. The increase of magnetism at 12^h is nearly equivalent to the attractive force, multiplied by the square of the distance from the center of gravity of the system, and divided by the square of the earth's radius ($·000004 \times 7707^2 \div 3963^2 = ·0000168$).

3. There is a tendency to equality of disturbances on each side of the meridian at 1^h and 8^h, as in the solar magnetic tide.

4. The greatest disturbance occurs at the hours of 10^h and 11^h P. M., both in the solar and in the lunar tide.

5. There are some indications of an increase of gravity, and decrease of magnetic force, when the tidal flow is toward the center of gravity of the terrestrial system, and *vice versa.*

6. The rotation-tide has the customary quarter-daily phases of alternate increase and diminution.

X. The phenomena of magnetic storms indicate the existence of controlling laws, analogous to those which regulate the normal fluctuations. See Proceedings Amer. Phil. Soc., Oct. 21, 1864.

The foregoing comparisons have been based on Gen. Sabine's discussions of the St. Helena records. It would be desirable, if it were possible, to confirm them by observations at other stations near the equator; but the need of such confirmation is in great measure obviated, by the variety of ways in which I have shown the probable connection of gravity and magnetism. At extra-tropical stations, the rotation tide becomes so preponderating that it is difficult to trace the diminished gravitation- and differential-tides, still I shall look confidently to a fuller development of the theory of tidal action, for future additional support to my views.

ART. XL.—*On the Origin and Formation of Prairies;* by LEO LESQUEREUX.

THIS paper is intended as a review of Prof. Winchell's new theory on the origin of the prairies of the Mississippi Valley,[1] and as a defense of my own views on the same subject.[2] I shall therefore omit all details relative to the surface of prairies, their conformation and appearance, and their geological and geographical distribution, since these details do not directly concern the elucidation of the question.[3]

From the brief mention made of my opinion concerning the formation of prairies, it appears to have been misunderstood by those who have quoted it, or rather it was entirely unknown to them. For this reason it is advisable to first expose, as they were originally given, the essential points of a simple explanation, which does not merit to be spoken of as a theory. I merely translate from my letter to Professor Desor:

"You well know that prairies are at our time in process of formation along the shores of the Lakes:—Lake Michigan, Lake Erie, etc. and also along the Mississippi and some of its affluents, especially the Minnesota river. The formation of these recent prairies, whose extent is not comparable to that of the primitive ones, is peculiar, and has a great analogy to that of peat bogs. Where the waves of the lakes, or their currents, strike the shores or the low grounds, and heap materials, such as sand, pebbles, mud, etc., they build up more or less elevated dams or islands, which become covered with trees as soon as they are raised above water. These dams are not always built upon the shores, and do not always even follow their outlines, but often enclose wide shallow basins, whose water is thus sheltered against any movement. There the aquatic plants, sedges, rushes, grasses, soon appear, and these basins become swamps, as may be seen near the borders of Lake Michigan. Though the forests may surround them, the trees do not invade them, even when the swamps become drained by some natural or artificial cause.

"Along the Mississippi and Minnesota rivers, the same phenomenon is observable, with a difference only in the process of operation. During a flood, the heaviest particles of mud are deposited on both sides of the principal current, along the line of slack water, and by repeated deposits, dams are slowly formed

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[1] This Journal [2], xxxviii, 332.

[2] Letter to Prof. Desor in Bull. de la Soc. des Sci. Nat. de Neuchatel, Dec. 1856.

[3] Prof. Whitney in the 1st Chap. of the Report of the Geological Survey of the State of Iowa has given a clear and very accurate descriptive account of the prairies.

and upraised above the general surface of the bottom land. Thus, after a time, the water thrown on the bottoms by a flood, is, at its subsidence, shut out from the river, and both sides of it are converted into swamps, sometimes of great extent. Seen from the high bluffs bordering its bottom lands, the bed of the Minnesota river, in the spring, is marked for miles by two narrow strips of timbered land bordering the true channel of the river and emerging like fringe in the middle of a long continuous lake. In the summer, and viewed from the same point, the same bottoms appear transformed into a green plain, whose undulating surface resembles immense fields of unripe wheat. In reality, they are impassable swamps covered with sedges, rushes, and coarse grasses. By successive inundations, they become, by and by, elevated above the level of the river. They then dry up in summer, mostly by infiltration and evaporation, and when out of the reach of the floods, they become at first wet, and afterward dry, prairies.[4] In that way, admirable locations for river towns have been made. On the Mississippi, Prairie du Chien, Prairie la Fourche, Prairie la Crosse, etc., are, as indicated by their names, towns located on formations of this kind.

"These splendid patches of praries, though of a far more recent origin than the immense plains above them, are nevertheless true prairies. Although bordered on one side by the high-timbered banks of the bottoms, and still separated by a fringe of trees from the actual bed of the river, yet the trees do not enter them. This peculiarity of formation explains first the peculiar nature of the soil of the prairies. It is neither peat nor humus. It is a black, soft mould, impregnated with a large proportion of ulmic acid, produced by the slow decomposition, mostly under water, of aquatic plants, and thus partaking as much of the nature of peat as of that of true humus. In all the depressions of the prairies, where water is permanent and unmixed with particles of mineral matter, the ground is true peat.

"It is easy to understand why trees cannot grow on such kind of ground. The germination of the seeds of arborescent plants needs the free access of oxygen; and the trees, especially when they are young, absorb by the roots a great amount of air, and demand a solid point of attachment to fix themselves. Moreover, the acid of this kind of soil, by its particularly antiseptic property, promotes the vegetation of a peculiar group of plants mostly herbaceous. Of all the trees, the tamarac is the only species which, in our northern climate, can grow on a peaty ground, and this even happens only under rare and favorable circumstances,

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[4] The lowest part of these fluvial prairies is of course farthest from the rivers along the bluffs. Here, generally, the percolation of water through the banks forms springs and deeper swamps, which are often transformed into peat bogs.

that is, when stagnant water, remaining at a constant level, has been invaded by the *Sphagnum*. By their absorbing power, their continuous growth and the rapid accumulation of their remains, these mosses slowly raise the surface of the bogs above water, and it is then, in this loose ground, constantly humid but accessible to atmospheric action, that the tamarac appears.[5]

"Now, let us examine the prairies according to this idea of their formation, and see if, from the first trace of their origin to their perfect completeness, there is any thing in their local or general appearance which is not explained by it or does not agree with it.

"The bay of Sandusky is now in process of transformation into prairies, and is already sheltered against the violent action of the lake by a chain of low islands and sand-banks, most of them covered with trees since a long time, at least, judging from the size of the trees. All these islands are built up with the same kind of material, that is, with lacustrine deposits, either moulded into low ridges under water, or brought up and heaped by waves and currents. Around the bay, especially to the southwest, there are extensive plains covered with shallow water. The bottom, in the depression toward the lake and where the aquatic vegetation is only at its origin, is sandy clay. But, in the more shallow places, the clay is already muddy and blackened at its surface by the detritus of the herbaceous vegetation which has grown upon it. Farther toward the borders, and in proportion to the shallowness of the water, the detritus thickens, and still farther, we have wet prairies, with exactly the same vegetation as that of the lake swamps, and a black soil with a substratum of clay, the same materials also as those of both the deeper and more shallow swamps of the lakes. In receding from the borders of the lake toward the high prairies, the transition from wet to dry prairies is by so insensible degrees that it would be impossible to fix a point of separation between them. All the surface appearances are the same. Vegetation is here and there modified by the presence of some peculiar species of herbaceous plants, but nothing else. The homogeneousness of the soil is still more striking. There is the same kind of clay as subsoil, and this is overlaid by the same kind of black spongy mould. And if here and there you see knolls covered with trees, the cuts of the railroad show that the materials of which they are formed are a different compound from the ground of the prairies, even if they are scarcely elevated above the general level, and that they are of the same nature and of the same formation with those of the low wooded islands of the lake.

On the borders of the numerous lakes which dot the high

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[5] In my letter to Prof. Desor, this question is discussed at length. I have omitted here the details, as they are unnecessary for the elucidation of the subject.

rolling prairies, especially in Minnesota, the process of formation of the prairies is repeated in the very same way. These lakes are of every size, sometimes small and circular, true ponds; sometimes thirty to forty miles in circumference, and in this case shaping the outlines of their shores according to the undulations of the prairies; divided into innumerable shallow branches, mere swamps, covered with water plants, and emptying themselves from one to another; passing thus by slow degrees toward rivers, not by well marked channels, but by a succession of extensive swamps. These are the sloughs which separate the knolls of the prairies—so to speak, the low grounds of the rolling prairies. They are nearly dry in summer, but in the spring are covered by one to three feet of water. Their vegetation is merely sedges and coarse grasses. I have never seen fishes in these sloughs, but plenty of crawfishes and a great quantity of freshwater shells.[6] Wherever the borders of the lakes are well shaped, not confounded with or passing into swamps, they rise from five to six feet above the level of the water and are timbered, mostly with oak and hickory. This elevated margin is more generally marked on the eastern sides of the lakes, a record of the action of the waves under the dominant winds. Or it is shaped as a low range of hills surrounding the lakes, and is due then to original irregularities of the surface. The materials are the same as those of the dams, or low islands, of the great lakes; indeed, the same as those of the under bottoms of the swamps, or those over which the prairies have been formed. But they have been removed from the influence of stagnant water: this is the only difference."

"From all these remarks, what other conclusion can we deduce but that all the prairies of the Mississippi valley have been formed by the slow recession of sheets of water of various extent, first transformed into swamps, and, by and by, drained and dried. The high and rolling prairies, the prairies around the lakes, those of the bottoms along the rivers, are the results of the same cause, and form a whole, an indivisible system."[7]

"To this assertion you will object, I suppose, and say: How is it that the prairies are not everywhere perfectly horizontal; and as there is some unevenness of surface, have these undulations not been formed like the low islands or high borders of the lakes, and why then are they not timbered?"—"I believe that, although the surface of the prairies may be now undulated, it was originally horizontal enough to form shallow lakes, and

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[6] Especially *Planorbis trivolvis*, *P. lentus*, *Lymnea appressa*, *L. emarginata*, *L. decidiosa* Say, etc. The lakes have the same species, with many bivalves, and a great abundance of fishes, especially catfishes (*Pimelodes*).

[7] Atwater, this Journal, i, 116, 1819, and Bourne, ibid., ii, 30, 1820, have both considered the prairies as originating from swamps, without, however, giving an explanation of the phenomenon.

then swamps, like those which at the present time cover some parts along the shores of Lake Erie, Lake Michigan, etc. Where this horizontality has disappeared, it is only by very slow degrees, under the eroding action of water, which, in its slow movements, tends to follow every change of level, seek an outlet, and so establish channels of drainage. I have followed for whole days the sloughs of the prairies, and have seen them constantly passing to lower and well marked channels, or to the beds of the rivers, by the most tortuous circuits, in a manner comparable to the meanderings of some creeks in nearly horizontal valleys. Indeed, the only difference is, that, in the high prairies, there is not a definite bed, but a series of swamps, extending, narrowing, and bending in many ways. The explanation appears to me so natural, that I cannot understand how high prairies could ever be perfectly horizontal. Along the lakes and in their vicinity, the horizontality is a necessary consequence of the primitive evenness of the bottom and of the proximity to water. The level of the low prairies being scarcely above that of the lakes, their surface, after an overflow, becomes dry, rather by percolation and evaporation than by true drainage. But wherever the rivers have cut deeper channels,—as is the case in the north part of the Mississippi basin, where they run sometimes from one to three hundred feet lower than the surface of the high prairies—the drainage has constantly taken place toward those deep channels, and the water, though its movements may be very slow, furrows the surface in its tortuous meanderings. From this, results that irregular conformation of surface generally and appropriately called *rolling*. In Indiana and Illinois, in the vicinity of the Wabash river, for example, there are some high prairies whose surface is apparently horizontal. But these prairies, as at Terre Haute, are surrounded by a margin of low wooded hills, and have originally been shallow lakes of difficult and slow drainage. Moreover, their horizontality is rather apparent than absolute; some parts of them are already dry enough to be cultivated and ploughed in the spring; some parts are used as wet meadows, and still others are covered with water and inaccessible. This apparent horizontality results from the great width of what we may call already channels of drainage. These will, by and by, contract and deepen, and thus the prairie become undulating. This opinion is in contradiction to what you say in your excellent paper on the drift of Lake Superior: that the irregularities of surface of the prairies have been caused by currents at the time when they were under water. If the ground of the high rolling prairies had been prepared in advance, as you suppose, the prominent parts or the knolls would have been timbered like the low islands of the lakes."

Next follows, in my letter, an examination of the evidence that the prairies have been covered with water to their highest points; that the water has not been drained by any violent action but by slow upheaval of the surface; that the deepening of the great channels, and the formation of low prairies, around the lakes and along the rivers, are merely a continuation of the same phenomenon or a result of the slow and continuous upheaval of the great Mississippi basin.

The letter ends in refuting opinions, admitted by some authors, that the absence of trees on the prairies is caused by atmospheric dryness, and by others that it results from fires, etc.

After reading this exposition of my views, Prof. Desor remarked that what he calls my theory has the merit of combining in the same explanation two phenomena, the formation of the peat bogs, and the formation of the prairies, whose relation to one another is too natural to be without foundation. But he objects to some of my ideas, and especially thinks that my explanation of the formation of the *rolling* of the high prairies cannot be admitted, because the spaces between the knolls are not deep and narrow sloughs or simple trenches, but broad depressions, broader than the knolls themselves; that this could not be the case, if they had been formed by the erosion of water.

This objection, I think, is groundless. As we have seen, in considering the surface of the low and of the flat prairies, wherever the drainage is insensible, water can scarcely have any action in digging trenches. In the spring, or after heavy rains, its slow movements extend over the whole breadth of the low grounds, scarcely displacing or carrying away the finest material. This can hardly be considered as *erosion.* Nevertheless, it is certain that all the sloughs of the rolling prairies find their way to lower and deeper channels, where they definitively shed their water. It is certain, also, that in reaching the Mississippi, or the Minnesota, etc., these sloughs are deeper and all nearly perpendicular to the direction of the rivers. For this reason the *rolling* of the prairies along the great rivers resembles a succession of fronting abutments. Prof. Whitney, in the first chapter of the geological report of Iowa, makes a corresponding remark, when he says, (p. 17,) "The streams of the prairies usually take their rise in small depressions of the high prairie, scarcely to be noticed as being below the general level of the region. As their course continues, the beds generally sink, etc." Prof. Desor holds that the knolls of the prairies were formed under water, and in deep water, and, to confirm his assertion, he says that the bottoms of our great lakes, and of the ocean also along the shores, are marked by swells and deep furrows. In fact, the knolls of the prairies are of quite a different form from the long continuous parallel undulations of the bottom of the seas. Moreover, there are such traces of marine action in Iowa and Minnesota in the timbered *Coteaux*, as the Coteau des Bois, Les Bois-rouges, etc., which cut the nakedness of the prairies and resemble those narrow strips of land bordering the ocean and soon becoming long peninsulas covered with a luxuriant arborescent vegetation. The first origin of these undulations may have been in deep water, or they may have resulted merely from the action of the waves. In any case, they cannot be compared with the barren knolls of the prairies.

Prof. Whitney,[8] admitting that the prairies have been covered with water, even to their highest point, considers the absence of trees as caused by the fineness of the soil, which he attributes in part to the nature of the rocks underlying it, and in part to *accumulation in the bottom of immense lakes of a sediment of almost impalpable fineness, under conditions which will be discussed hereafter.*[9] This unfinished explanation cannot fully satisfy the mind. Prairies cover every kind of geological formation, even granitic rocks, as in Minnesota, between St. Peter and Fort Ridgely. Most generally they overlie the Drift. It is evident that the black soil of their surface, as well as the clayey subsoil, though the thickness of these strata may be great, has been formed, in place, by the agency and growth of a peculiar vegetation. In stagnant water, whenever water is low enough to admit the transmission of light and air in sufficient quantity to sustain vegetable life, the bottom is first invaded by Confervas, especially by *Characeæ* and a *peculiar* kind of floating moss (*Hypnum aduncum* Hedw.). These plants contain in their tissue a great proportion of silica, lime, and even oxyd of iron.[10] Moreover, they feed a prodigious quantity of small mollusks, whose shells add to the detritus of the plants, and the final result of the decomposition of the matter is that fine clay of the subsoil of the prairies, truly impalpable when dried and pulverized. This formation has been observed and described long time ago, especially by Chrome, who attributes to the decomposition of Confervas, Characeæ, etc., the clay over which peat bogs generally rest. I have seen it in process of prodigious activity in a large pond of the King's Garden of Fredericksburg in Denmark, which every year is filled with about one foot of clayey matter, by the decomposition of Characeæ, small mollusks and infusoria. This kind of formation is so general that it can be observed in nearly every open swamp. In the lakes of the high prairies it has sometimes a peculiar character. At the depth of from one to three feet, the plants

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[8] Geological Survey of Iowa, chapter i.

[9] Geological Report of Iowa, i, 25.

[10] DeCandolle *Physiologie Vegetale*, p. 183 and 188. When exposed to atmospheric influence, the Charas become covered with an efflorescence of scarcely carbonated or pure lime.

above named, Mosses, Confervas, and Charas, form a thick carpet, which hardens, becomes consistent like a kind of felt, and, floating about six inches from the bottom, is nearly strong enough to sustain the weight of a man. The carpet is pierced with holes, where fishes pass in and out, and the bottom under it is that fine impalpable clay, evidently a residue of the decomposition of its plants. At the depth of three and a half to four feet, this vegetation suddenly ceases and the bottom of the lakes is pure sand and pebbles with shells. Nearer to the borders, on the contrary, at the depth of one foot, the carpet of mosses, etc., begins to be intermixed with some plants of sedges, becoming more and more abundant in proportion as the depth decreases. As soon as the leaves of these plants rise out of the water, they absorb and decompose carbonic acid, transform it into woody matter, under atmospheric influence, and their detritus is at first clayey mould, and then pure black mould, the upper soil of the prairies. Of course, near the borders of the rivers, or under peculiar circumstances, the formation is somewhat modified by the addition of transported matter, or of foreign elements. The clay may thus take a different color, and have a somewhat different composition; but the process of formation does not materially change.

Considering the whole explanation of the formations of the prairies, as it is exposed in this paper, I think that it covers the whole ground and can apply to most of the cases (if not to all) where the ground is naturally naked or without trees. It gives the reason of the formation of the prairies from the base of the Rocky Mountains to the borders of the Mississippi River; of the prairies around the lakes, and of those of the broad flat bottoms of our southern rivers; of the Platas of the Madeyra river; of those of the Paraguay; of the pampas of Brazil; and even of the desert plains of our western Salt Lakes. For this formation is produced, in the same manner, in the salt marshes of the sea, as in the freshwater swamps of our lakes. And if, passing to other continents, we examine, in Europe, the natural meadows of Holland, the barrens or heaths of Oldenbourg, all the plains on the shores of the North and of the Baltic Sea, and, in Asia, the vast steppes of the Caspian, etc., etc., we find everywhere the same appearances, and the same results of a general identical action, modified only by local and mostly climatic circumstances.

The glades on the slopes of some mountains of Arkansas, of the Alleghanies, etc., have been quoted as a phenomenon contradicting the theory of formation of prairies by water.[11] I have carefully examined those glades in connection with the geologi-

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[11] R. W. Wells, this Journal, i, 334, 1819.

cal survey of Arkansas, and found them always the result of water. When the springs descending the slopes are stopped by some obstacle, the waters slowly overflow the surrounding surface, and generally favor the growth of a thin stratum of peat, where only grasses and prairie flowers vegetate. Sometimes the peat of these glades is one to two feet thick. They are generally dry in the fall, but always true swamps or wet prairies in the spring.[12]

Prof. Winchell observes that *a theory* [for the absence of trees] *often urged is the considerable humidity of the soil of certain prairies and especially the wetness of the subsoil*, &c., and refutes it by this mere assertion: *it is singular that such an opinion could be entertained when it is so well known that there is no situation so wet but certain trees will flourish on it. The willow, cottonwood, tupelo, water oak, tamarac, American arbor vitæ*, etc.[13]. And, when considering Prof. Whitney's supposition that the extreme fineness of the prairie soil is the cause of the absence of trees, he sets it aside, in the same way, by another assertion: that *the fatal objection to this theory, and all theories which look to the physical or chemical condition of the soil for an explanation of the treeless character of the prairies, is discovered in the fact that trees will grow on them when once introduced.*[14]

As it is not proper to refute an assertion by a contrary one, let us examine under what circumstances trees may grow in some swamps, and what the highest scientific authorities have to say on the subject.

It is a well known fact of botanical physiology that trees absorb by their roots a certain amount of oxygen necessary to their life. It is in accordance with this principle that trees, to thrive well, ought not to be planted too deep; that most species of trees perish when their roots are buried in a stratum of clay, impermeable to air; that whenever the water of a creek is dammed, to make a pond, all the trees are killed on the whole flooded space. Running water furnishes a sufficient amount of air and oxygen for their life to certain species of trees (most of the species quoted by Prof. Winchell) whose roots, when immersed, have the property of dividing themselves into innumerable filaments. Hence, such trees grow, indeed, in those swamps inundated by the water of adjacent rivers, or periodically in-

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[12] The relation of glades to peat bogs is beautifully exemplified on the slopes of Mt. Marcy and other peaks of the Adirondac Mts. of New York. Here, in the middle of deep and nearly impenetrable woods, such openings, half prairies, half bogs, are suddenly entered without transition whatever, and their surfaces entirely barren of trees, appear like clearings and meadows produced by human agency. They are of every size, cover slopes of various degrees, and ascend as high as to 5000 feet above the sea.

[13] A. Winchell on the prairies. This Journal, [2], xxxviii, 343.

[14] A. Winchell, ibid., 344.

vaded by the tides. The water of such swamps is not permanent, and its whole mass is subjected to some kind of movement. It is thus that in the south the bald-cypress and the tupelo grow even in the middle of creeks and bayous. But look anywhere else, along the rivers, or on the shores of seas or lakes, and wherever a sheet of shallow water is sheltered against the waves, the tides or the currents, you find invariably treeless swamps, passing to prairies.[15] The only fact, to my knowledge, which could be mentioned as sustaining the assertion of Prof. Winchell is, that certain kinds of shrubs, as the button bush, the swamp rose, etc., grow and form thickets around some true swamps of our forests. But, examining the process of germination of the seeds, it is easy to observe that the germs are not developed in water, but in dried decayed vegetable matter of rotten prostrated trees. In summer, and only when the margins of the swamps are dry, the roots penetrate the ground. Thus the swamp is surrounded by a belt of shrubs, but its central part is open and occupied by herbaceous plants only.

Now, what says DeCandolle in his classical book on Vegetable Physiology? He remarks that the constant irrigation required for the rice culture in Lombardy is a serious inconvenience, because the water penetrates the ground of the neighboring estates and *kills the trees*:—That water, left stagnant for a time on the ground, rots the trees at the collum, prevents the access of oxygen to the roots, *and kills them*:—That in the low grounds of Holland they dig, for planting trees, deep holes, and fill the bottoms with bundles of bushes, as a kind of drainage for surplus water as long as the tree is young enough to be *killed by humidity*:—That true swamps and marshes have no trees, and cannot have any, because *stagnant water always kills them.*[16] Authorities to the same effect could be quoted by volumes.

The second assertion, *that trees will grow on the prairies when once introduced* (or planted, I suppose), is certainly true. But we should take care to make a distinction between the results of an artificial process and those of a natural one. When trees are planted on the prairies, the soil is conveniently prepared. The clayey subsoil mixed with the black mould forms a compound which combines density of certain parts with lightness of others, and contains a great proportion of nutritive elements. If the clay of the subsoil is not too thick to be impermeable to water, and thus retain it around the roots, this prepared or artificial ground is indeed very appropriate to the growth of trees. But has any one ever seen oak or hickory, or any other kind of trees, grow on the prairies from a handfull, say even from a

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[15] Species of trees like the magnolia grow over the southern peat bogs for the same reason that tamaracks grow in the peat bogs of the north.

[16] Physiologie Vegetale, pp. 1206–1212.

bushel, of acorns or nuts thrown upon their surface? Why, then, if trees *will grow* on prairies, do not isolated or widely separated clusters of trees insensibly cover a wider area? Some of these trees have lived there for ages; their trunks are strong and thick; their branches widely expanded, and their fruits are swept far away by the impetuosity of autumnal storms; nevertheless, their domain is restricted, by the nature of the ground, to their own narrow limits; these they never pass.

[*To be continued.*]

ART. XLI.—*On a Process of Fractional Condensation: applicable to the Separation of Bodies having small differences between their Boiling-points*; by C. M. WARREN.[1]

IT is well known that the process in general use for the proximate analysis of mixtures of volatile liquids,—viz: that of simple fractional distillation, either from a tubulated retort or from a flask with bulbs, as proposed by Wurtz,[2]—affords but very imperfect and unsatisfactory results, and not unfrequently leads to gross errors and misconceptions, except in those cases in which the boiling-points of the constituents are widely different, or in which some auxiliary method can be advantageously employed.

The want of a more efficient process for effecting such separations has long been recognized. There are numerous natural and artificial products, of the highest scientific interest,—such as petroleums, essential oils, tars, and other mixtures of oils obtained by the distillation, under varied circumstances, of bituminous, vegetable, and animal substances,—of which it may at least be said that we have but very imperfect knowledge,—I might almost say no knowledge, except such as could be derived from the study of very impure materials,—still mixtures of different bodies,—with which, instead of the pure substances sought for, chemists have felt compelled to content themselves, as the best results which they were able to obtain by the means at their command.

In repeated instances, apparently after persevering and protracted efforts, investigators have been forced to assert either the impossibility, or their inability, to obtain, from such mixtures, bodies of constant boiling-point,—a property which is generally received as a test of purity for liquid bodies.

I may here specify a few recent instances of this kind.

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[1] From the Journal of the Acad. Arts and Sciences, Boston, May 10th, 1864.

[2] Annales de Chimie et de Physique, [3], xlii, 132.

1. Warren de la Rue and Hugo Müller,[3] in their paper entitled "Chemical Examination of Burmese Naphtha and Rangoon Tar," after detailing the preliminary treatment by distillation in a current of steam, add that "A further separation of the various products was effected by repeated fractional distillations; but no absolutely constant boiling-points could be obtained, notwithstanding the great number of distillations and the large quantity of material at command. It is true that considerable portions of distillates could be collected between certain ranges of temperature, tending to indicate a constant boiling-point; nevertheless, it soon became evident that distillation alone could not effect the separations of the various constituents, and that recourse must be had to other processes." The other processes resorted to were, treatment with sulphuric and nitric acids, either separately or mixed; but still with very imperfect results. This acid treatment, which was first proposed by De la Rue, and subsequently employed by C. Greville Williams,[4] Schorlemmer, and others, will be further noticed below.

2. Frankland,[5] in speaking of a mixture of the hydrocarbons of the formulæ C_nH_n and C_nH_{n+1} (now generally considered as C_nH_{n+2}), which have a difference of 6° to 7° C. between their boiling-points, says, "The separation of two such bodies by distillation alone is impossible;" and suggests that the employment of anhydrous sulphuric acid may accomplish the object by dissolving out the body of the formula C_nH_n.

3. And so recently as 1862, Schorlemmer,[6] in his first paper "On the Hydrids of the Alcohol-Radicals existing in the Products of the Destructive Distillation of Cannel Coal," remarks that "it was, however, found impossible to obtain a product of constant boiling-point by repeated fractional distillations;" and he also had recourse to the acid-treatment above referred to.

4. Pebal,[7] after an elaborate research on the petroleum from Galicia, in which Wurtz's bulbs were employed, and also Eisenstuck,[8] who made an extended investigation of the petroleum from Sehnde, near Hannover, also with the use of Wurtz's bulbs, both assert in the most positive manner the impossibility

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[3] Proceedings of the Royal Society, viii, 221.

[4] Philosophical Transactions, 1857, 447.

[5] Quarterly Journal of the Chemical Society, 1851, 3, 43.

[6] Journal of the Chemical Society, xv, 419.

[7] Annalen der Chemie und Pharmacie, cxv, 20, asserts the "Unmöglichkeit, das Gemenge durch fractionirte Destillationen zu entwirren."

[8] Annalen der Chemie und Pharmacie, cxiii, 169, says as follows: "Mit den 5° zu 5° aufgesammelten Destillaten wurde die fractionirte Destillation wieder von Neuem vorgenommen, aber nachdem diese Operation sieben Wochen mit etwas 50 Pfund Steinöl fortgesetzt worden war, erhielt ich doch kein Product von irgend constantem Siedepunkt. Nach diesen Versuchen halte ich es für Unmöglich, das Steinöl durch fractionirte Destillationen allein in Producte mit constantem Siedepunkt, zu scheiden."

of separating from petroleum, by fractional distillation, products of constant boiling-point.

Such is the general character of the results obtained in the attempts which have been made to separate the constituents of such mixtures by fractional distillation.

The treatment with strong acids, etc., as an auxiliary to the common method of fractional distillation, which is claimed to have given good results in some cases, is open to serious objections in its application to mixtures of unknown substances, as must be readily apparent. The further consideration of this subject is reserved for another occasion, when I shall submit the results which I have obtained by my process in the study of mixtures almost identical with some of those in the investigation of which the acid process has been employed. I shall then be able to show that the results obtained by that process are, to a considerable extent, inaccurate and by no means exhaustive; and that it is still of the highest importance to have a process which shall be generally applicable in all such cases, without resort to any harsh or uncertain treatment.

With regard to the value of constancy of boiling-point above referred to, as a test of purity of a liquid substance, I may here say that, without scarcely lessening the importance of obtaining constancy of boiling-point, before resorting to harsher treatment, in the study of mixtures of unknown substances, I think I shall be able to show, on another occasion, that this property is not necessarily indicative of so high a degree of purity as has generally been supposed; and that a body may have a constant boiling-point, and yet contain enough of a foreign substance to appreciably—and, in delicate cases, seriously—affect the determination of its constitution and of some of its other properties. But in no such case have I yet found that the removal of the impurity by chemical means has essentially changed the boiling-point,—i. e., never to the extent of 1° C. of temperature. I propose, at a future time, to study this question synthetically, operating with pure liquid substances, with the view to determine, in a few cases, how much of a foreign substance may be present,—which would probably be variable in different cases,—without sensibly affecting the boiling-point. A solution of this question would, I think, be of considerable practical value in some instances.[1]

Of the New Process.—The chief distinctive feature of my process, as compared with the common one, consists in this,—that the operator has complete and easy control of the temperature of the vapors given off in distillation; and consequently can

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[1] Since this was prepared for the press I notice that late experiments by Berthelot go to show the correctness of my conception of the value of constancy of boiling-point, as above stated.

readily cool these vapors to the lowest limit of temperature which the most volatile portion, under the circumstances, is able to bear and retain its vaporous condition. It will be seen at a glance that, under these conditions, the operator has it in his power to secure in any case the very largest possible amount of condensation of the heavier from the lighter vapors. The liquids resulting from the condensation of the less volatile portions of course fall back into the retort, while the vapors of the more volatile parts continue to go forward to a cold condenser, descending in the opposite direction, from which the condensed product falls into a special receiver. In this manner he is able to obtain, in each successive operation, a series of products which shall contain the minimum quantity of the less volatile constituents, which a single distillation is capable of affording.

Of the common process, on the contrary, nearly the reverse of all this is true: the operator having no control whatever; being forced to receive the vapors at the temperature which they naturally acquire in passing from the retort, and laden with such proportion of the less volatile bodies as may be carried forward with them.[10]

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[10] The only apparatus, of which I have any knowledge, which can be regarded as bearing any analogy to my own, is that employed in the rectification of alcoholic spirits, on a manufacturing scale. In one of the older forms of this apparatus, that of Solimani, to which my attention was first called by a friend, after my process had been in use more than a twelvemonth, the temperature of a dephlegmator is kept within such limits as to give alcohol of any required strength more readily than by the common methods. The mode of construction of this apparatus is, however, only adapted to manufacturing purposes, and it could not be utilized in the more exact experiments required in scientific research. Either on account of its complication, or some other cause, the apparatus of Solimani has, I believe, long since been abandoned.

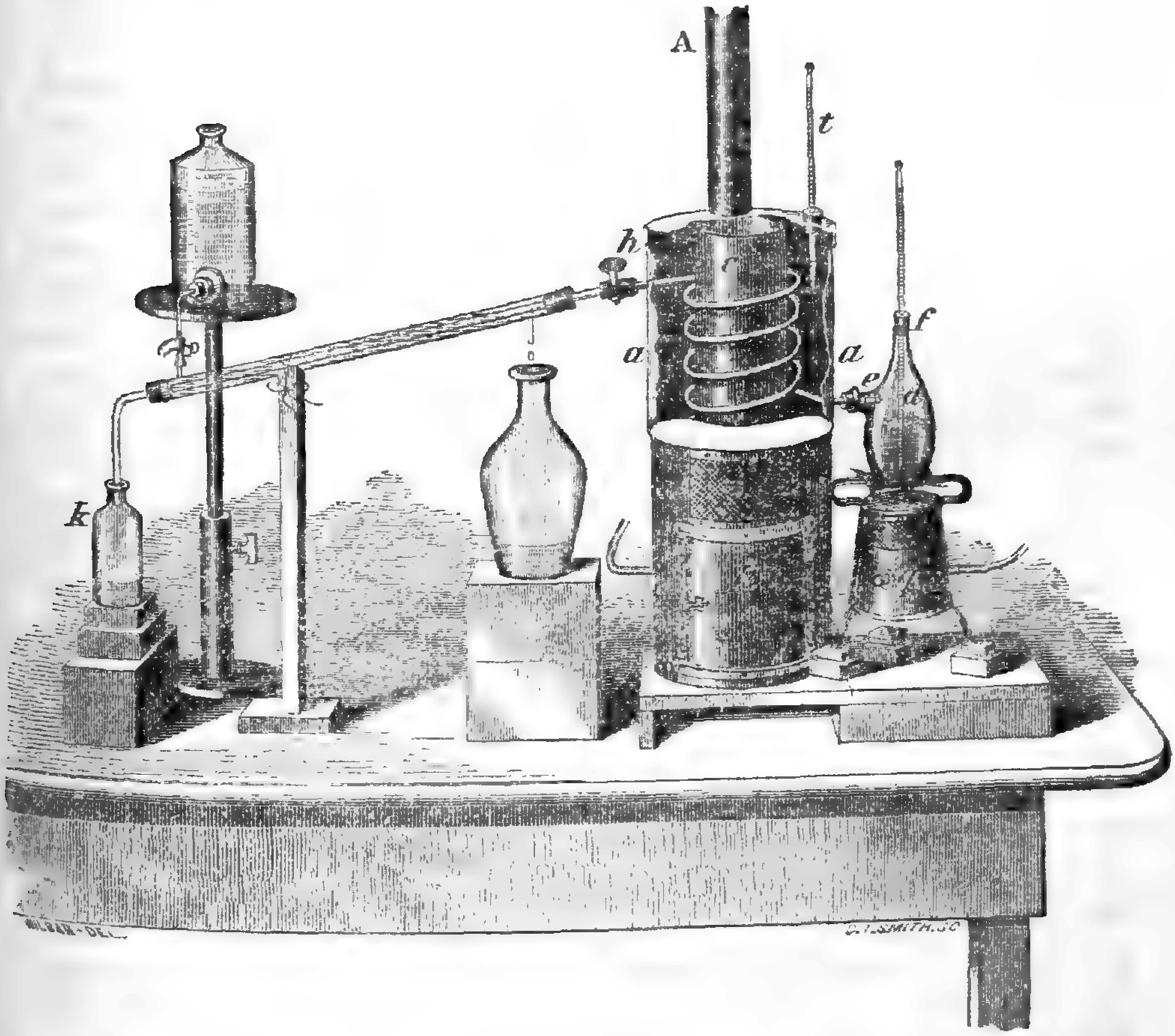
Mansfield (*Quarterly Journal of the Chemical Society*, 1849, i, 264), observing that "the boiling-point of benzole is the same as that of alcohol of sp. gr. 0·825," remarks that "any of the summary processes of rectification which are practised by distillers in the manufacture of alcoholic spirits, are applicable to the separation of benzole from the less volatile fluids of naphtha;" and, appended to his scientific treatise on coal-tar, under the title "*Of a Practical Mode of Preparing Benzole*," goes on to describe a process for that purpose, which, I believe, he had previously patented. It appears that Mansfield did not employ this process in his research, but obtained his benzole, as well as the other less volatile hydrocarbons, in the usual manner,—by simple distillation.

In the belief that no process of fractioning at all analogous to mine has ever been employed in scientific research, and that I am not in any way directly indebted to any of the devices of my predecessors, I have taken no special pains to consider these devices in much detail. I may say, however, that I have found no record of any one's ever having employed the oil bath and a separate fire to regulate a heated condenser, this being the essential feature on which the superiority of my process is based; adapting it at once to both high and low temperatures, and for the most delicate work.

The employment of bulbs, above referred to, as proposed by Wurtz, is simply a modification of the old process. The bulb apparatus furnishes the same, or, at most, but slightly better results than a simple retort; being no more than equivalent to increasing the height of the sides of the retort itself, without introducing any control over the accuracy of the results; the only advantage gained being, that these results are obtained somewhat more quickly.

In the new process, perfect control of the temperature of the vapors is secured by simply conducting these vapors upward through a worm contained in a bath, *aa*, figs. 1 and 2, the temperature of which is regulated by means of a separate lamp, *b*, fig. 2, or by a safety-furnace, *p*, as shown in fig. 1. The bath may be of oil or water, or of metal for very high temperatures, as the case may require, and is furnished with a thermometer, *t*.

1.



That this bath may be equally adapted for the separation of liquids boiling below the common temperature, an empty vessel, *c*, figs. 1 and 2, is permanently secured in the interior of the bath by means of straps of metal across the top, to serve as a convenient receptacle for ice or iced water, by means of which a low temperature may be steadily maintained. The interior vessel also serves a good purpose in economizing time, and fuel in heating the bath, as it diminishes the quantity of oil required to cover the worm. It is made to extend to within about three inches of the bottom of the bath, and large enough to fill the greater part of the space in the center of the coil. The bath and interior vessel are both made of sheet-copper, with joints brazed so that they will bear a high temperature. I generally use, also, copper worms, especially in the earlier distillations, the quantities then operated upon being larger, as such worms are conveniently procured, and not liable to break. In the larger-sized apparatus, the tube of which the worm is made measures ten feet in length and half an inch in diameter. I have tried several lengths of worm and several diameters of tube, but not, as yet, with any special view of determining the precise proportions, in relation to the size of the retort, which would be best adapted to the purpose. There appears, however, to be nothing gained by increasing the length of the worm beyond what is required to reduce the temperature of the vapors to that of the bath. I have in use three sizes of apparatus: the largest has a copper worm 10 feet long and ½ inch bore; the medium size, a worm 5 feet long and ⅜ inch bore; and the smallest size, for very small quantities, a worm 1 foot 6 inches long and ¼ inch bore. Each of these has been found to answer a good purpose. The distillation may be conducted in a glass flask, or more conveniently in a glass retort of the form shown at *d*, figs. 1 and 2. The body of this retort, as appears in the figure, is of the form of the corresponding part of the common retort; but which, in place of a long neck, has only a short tubulure, *e*, in the side, for escape of the vapors, and another tubulure, *f*, in the top, which contains the thermometer, and through which the retort is charged.

In the larger apparatus the retort is connected with the lower end of the elevated worm by means of a glass tube of about the same diameter as the end of the worm. One end of this tube enters the retort at the lateral tubulure through a perforated cork, and the other end is joined to the end of the worm, either by being firmly bound with a strip of cloth thickly covered with vulcanized caoutchouc,—such as is found in commerce,—or by means of a perforated cork, which is made to fit the ends of both tubes as snugly as possible, and then tightly pressed together upon the joint by means of an iron clamp, as shown at *g*, fig. 2. This clamp is figured on a larger scale at E. As it is highly important that all joints in the apparatus should be perfectly tight, inasmuch as the least leakage, when continued a long time, would cause, in the aggregate, a serious loss of material, I would call special attention to the clamp joint as the best which I have tried. Before falling upon this device I had used exclusively the vulcanized caoutchouc joints, which were found to answer a good purpose, in most cases, except that they required too frequent renewal. I have found the cloth covered with vulcanized caoutchouc preferable to the common caoutchouc tubing. In the smaller sizes of apparatus I have the end of the worm itself project far enough from the bath to connect directly with the retort by means of a perforated cork, without the use of an additional connecting tube.


E
B
i
o
n
k
m
m
C
C

The upper end, *h*, of the elevated worm is brought out through the side of the bath at a point about three inches below the top; so that, when working with a low temperature of the bath, the worm may still be completely covered with oil, and also give sufficient space above the worm for the expansion of the oil when higher temperatures are employed. To avoid contaminating the atmosphere of the laboratory with the disagreeable fumes which are given off, in large quantity, from such a mass of heated oil, the top of the bath is tightly closed with a sheet-iron cover, from which a small funnel, A, fig. 1, conducts these fumes to a chimney.

In the larger apparatus, the vapors which succeed in passing through the heated worm are conducted downward into a cooled worm contained in a bath of water, *ii*, fig. 2, and the liquid product is collected in the receiver, *k*. The cold bath, *ii*, contains two condensing worms,—one for each apparatus,—and is large enough to condense for both without the necessity of renewing the water. I have represented two apparatuses combined, as it will be found more economical of time to operate with two at once. In the smaller apparatus, for the table, a Liebig condenser may be conveniently substituted for the cold worm, as shown in fig. 1.

For collecting liquids which boil below the common temperature, when such are present, I attach a refrigerator, B, fig. 2, which is provided with two block-tin condensing-tubes,—one for each apparatus. These are bent in a zigzag form, and attached to the inner sides of the refrigerator. The lower ends of the tubes extend through the end of the refrigerator far enough to form a convenient connection with the second receiver, *l*, fig. 2, which communicates with the first receiver, *k*, by means of the glass tube, *m*.

In order to successfully collect and condense the vapors of such extremely volatile liquids as are now under consideration, it is of course indispensable that the apparatus should be constructed with very tight joints; and for greater convenience, but more especially to prevent breakage, such of the joints as require to be frequently taken apart should be made flexible. A very convenient and perfectly tight joint of this kind may be made as follows:—the short stationary tube, *n*, in the cork of the receiver, *k*, fig. 2, is made with the opening somewhat divergent upward; the end, *o*, of the worm is enough smaller than the inside diameter of the upper end of the tube, *n*, to leave room for a piece of caoutchouc tube to be drawn over it, and still admit of its being inserted in the end of the tube, *n*; the flexible tube is drawn on far enough to prevent the drops which form on the end of the worm from coming in contact with the caoutchouc; a perfectly tight and convenient flexible joint is now made by pressing the tube, *n*, over the caoutchouc covering of the end of the worm, *o*. The joints of the receivers, *ll*, are made in the same manner.

The vapors which escape condensation in *ii* pass through the receivers, *kk* and *ll*, to the refrigerator, B, which contains ice, or a mixture of ice and salt, are there condensed and fall back into the receivers, *ll*; which should stand in a wooden vessel also containing ice or a freezing mixture. The refrigerator, B, is made with double bottom and sides, with an inch space between, which is filled with pulverized charcoal. Being tightly covered, a charge of ice and salt will serve for a long day's operations without renewal. In this manner I have been able to collect, in considerable quantity, bodies boiling nearly at 0° C., and this from mixtures in which such bodies had been quite overlooked by previous investigators.

It will be observed, on reference to fig. 2, that the larger distilling apparatus is represented as standing in a brick fire-place, with brick-work, CC, a few inches high, built up in front; and a sheet-iron apron, DD, folded above. This is for security against fire in case of accident, either to the retort or hot bath of oil. As arranged, the contents of either or both of these could run out and burn without danger to the operator or the premises, as the brick-work in front would prevent the liquid from spreading beyond the fire-place, and the dropping of the sheet-iron apron would cause an additional draft, and thus insure the passage of the flames into the chimney. Instead of placing the apparatus in a fire-place, where that is not convenient, equal security against accidents may be attained by the use of my safety heating-lamp,[11] *q*, fig. 1, to heat the retort, and safety-furnace, *p*, containing a Bunsen's burner, for heating the bath. The bottom of this furnace, and also a large part of the sides, is formed of wire gauze, such as described for the safety-lamp.[12] The gauze upon the bottom need not be permanently attached to the furnace, but may be simply laid over an opening cut in the stool or board on which the furnace is to be placed; if the furnace be then set upon it, taking care that the joint shall be tight around the edge, nothing more will be required. A strip of vulcanized caoutchouc, about an eighth of an inch in thickness, is riveted around the edge of the opening for the door; against this the door tightly closes, so that no ignition can take place through the cracks which would otherwise remain under the edges of the door.

For an apparatus to stand upon the table, the safety-lamp and furnace are especially desirable. I have also used them for the larger apparatus, placed upon the floor of the laboratory. As

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[11] This Journal, 1862, [2], xxxiii, 275.

[12] Loc. cit.

a practical test of the security which they afford, I may relate an incident which happened to myself. I had left the laboratory for a short time, with such an apparatus in full operation; the retort containing nearly a quart of light petroleum boiling below 100° C. Having been detained longer than I expected, on returning I found the laboratory filled with the vapors of hydrocarbons; and, on approaching the retort, found that the caoutchouc joint, connecting the retort with the elevated worm, had failed, and that the larger portion of the liquid had distilled into the room, having been mainly condensed in the upper worm, and conducted thence down the outside of the retort into the safety-lamp. This process was still going on, the lamp being highly-heated from the excess of fuel thus added to it, but no ignition took place outside the lamp. Although this experiment was rather injudicious, it furnishes a valuable test of the efficiency of the safety-lamp and furnace.

Having described the apparatus, I now proceed to give such details of the method of conducting the separations as have been found, in my experience, most efficient and economical of time. In commencing with a crude mixture of unknown liquids, I deem it advisable to operate at once on a tolerably large quantity of material, especially if the constituents are supposed to be numerous, and to omit chemical treatment till after the separations have so far progressed as to indicate the number and species of bodies present, and, approximately, their several boiling-points.

Notwithstanding the precautions taken to avoid loss from evaporation and leakage, I have at times been surprised at the large waste of material which has been made apparent after a long series of operations. When it is considered, however, that the time required to make a complete separation of a very complex mixture of liquids must necessarily be very protracted, during which more or less of evaporation is constantly taking place, it will be a matter of no surprise that the loss is so considerable. The quantity of material required must depend also on the proportions in which the various constituents are contained in the crude mixture, and upon their degree of volatility; but as these cannot be known *a priori*, it may suffice to make a single preliminary distillation of a portion of the mixture, from a tubulated retort, to ascertain the range of temperature within which it distills, noting at the same time the proportions which come over between certain temperatures; as, for example, below 50° C.; between 50° and 100°, etc.; from these data one may judge pretty nearly of the quantity which it will be advisable to take. It is evident that, when very volatile bodies are present, even in considerable proportion, a much larger quantity would be required than if the material were but slightly volatile; as the waste in the former case, from evaporation, would be much greater.

But in many cases it will be found that highly volatile bodies are present only in very small proportion,—e. g., in viscid petroleum like Rangoon tar, and in the products of distillation of some species of asphalt. In such cases, the requisite quantity to be operated upon, to obtain the most volatile constituents in sufficient quantity for anything like a complete study of their chemical relations, would be extremely large,—too large to be conducted in the laboratory,—and one would have to resort to the manufactory for the first distillation. I have dwelt at some length on this point, having experienced the disappointment which one feels, after months of labor, on finding the products insufficient for his requirements, when the expenditure of a little more time, comparatively, might have given double the quantities obtained.

In the first series of fractioning, I generally operate on successive portions, of about one gallon each, of the crude material, and take off a fraction for every 20° C. rise of temperature of the retort. These fractions are preserved in well-stoppered bottles, and each carefully labelled with the temperatures between which it was obtained. The fractions for each fresh portion of the crude material, being collected between the same limits of temperature, are added to the corresponding products from the preceding operations, till enough of the crude material has been taken to insure, ultimately, a sufficiency of the pure products.

In the commencement, not only of this but of all subsequent fractionings, when the temperature to which the bath should be raised is unknown, I first bring the liquid in the retort into full ebullition, so that a steady stream of liquid shall flow back from the end of the worm into the retort. I then carefully raise the temperature of the bath until the vapors from the retort pass through the heated worm so freely that the liquid, in condensing from them, shall drop with tolerable rapidity into the cold receiver. In order that this dropping may be continuous, it is necessary that the temperature of the bath should rise *very gradually* as the more volatile constituents of the mixture are taken off; this is easily effected by carefully regulating the flame under the bath.

It is advisable to boil the retort as rapidly as possible without choking the lower end of the heated worm with the returning liquid. As this choking would give rise to additional pressure in the retort, and consequently occasion abnormal elevation of the temperature, and possibly a rush of liquid into the receiver, and thus introduce irregularities in the work, excessive heat under the retort should be avoided. The first indication of choking of the worm is a partial or entire stoppage of the stream of liquid which normally flows steadily from the end of the worm into the retort. Any interruption or unsteadiness of this flow would indicate too rapid ebullition.

As a rule, other things being equal, the greater the difference between the temperature of the bath and that of the retort, the slower the products will come off, and the more effectual will be the separation. I think it possible, however, that the earlier fractionings may be conducted so slowly that the loss of time would more than counterbalance what might be gained by more thorough separation, and that equally good results may be more economically obtained by more frequent operations, somewhat more rapidly conducted.

A striking illustration of the advantage to be gained by this process is presented by the fact that, during the first fractioning of a crude mixture, such as American petroleum or coal-tar naphtha, for example, the difference between the temperature of the bath and that of the retort may sometimes be as much as 35° C., or even more. While, as the products become purer, this difference between the temperatures of the bath and retort proportionally decreases, till finally, in operating on a pure product, the temperature of the bath must be brought to within a few degrees of that of the retort, in order to bring the vapors through. But the amount of this difference is variable for different bodies of equal purity.

The first fractionings must necessarily be quite arbitrary; for, as a general rule, when operating on such mixtures as those just mentioned, neither the thermometer nor the quantities obtained for any given range of temperature will indicate any decided preponderance of any one substance. On the contrary, the temperature rises uniformly, and about the same quantity is generally obtained for the same number of degrees of temperature throughout the operation. In other mixtures, in which certain bodies may seem to be present in much larger proportion than others, or in which there may be a greater difference between the boiling-points of the constituents than in the cases referred to,—facts which would be indicated by the thermometer of the retort, and by the relative quantities of the products obtained,—there might be something gained by exercising discretion in taking off fractions according to these indications.

In the second series of fractioning, the first or lowest fraction of the preceding series, which is large enough to operate upon by itself, is transferred to the retort, and brought into ebullition. The temperature of the bath is then adjusted as above described, and the distillation continued, the fractions obtained being placed in their appropriate bottles until the temperature of the retort shall have risen to, or somewhat above, the point at which the second or next succeeding fraction of the first series may be supposed, or has been found by experiment, to boil. This fraction is then added to the residue in the retort, and the distillation is continued as before. In the same manner, I proceed with the remaining fractions of the first series.

All subsequent fractionings are similarly conducted. As the work progresses, however, the fractions are taken for a gradually decreasing number of degrees of temperature, until finally it becomes necessary, for the attainment of absolute constancy of boiling-point, to take off a fraction of every degree, centigrade; and to continue thus to operate on these fractions, each representing one degree of temperature, until the desired end is attained.

The operator will observe that, in each series of fractions, in which each fraction has been taken for the same range of temperature, the difference between the boiling-points of any two contiguous fractions is nearly the same as the difference between any other two contiguous fractions,—in other words, that the difference referred to approximates to a common difference throughout the same series. Once ascertained, this difference serves as a valuable guide in determining with sufficient accuracy when to add the next fraction to the retort. By observing this systematic course, irregularities, from the improper mixture of products, may be avoided, and time thus economized.

After a few series of fractionings,—sometimes after two or three, variable in number, according to the nature or complication of the mixture,—it will be found that some of the fractions are considerably larger than others for the same range of temperature, indicating approximately the boiling-points of the several constituents. But fractions of constant boiling-point, or those, the boiling-points of which cannot be sensibly changed by further fractional condensation, are not obtained, as already mentioned, till after repeated careful fractioning of every degree of temperature. When fractioning of every degree, it is important to use every precaution to protect the thermometer from external influences, and to carefully apply the corrections for variations in the atmospheric pressure. This may even be desirable earlier; but it is of so much importance in the case specified, that, if omitted, the operator would be liable one day to mix products which he had separated the day previous.

In this way, certain larger fractions are obtained, which are not susceptible of further alteration in their boiling-points; but there are yet considerable quantities of liquid in the intermediate fractions, which still continue to change more or less in each succeeding operation. When the fractions of constant boiling-point have once been obtained, if it were not important to test for other bodies in the intermediate fractions, the operation might here be suspended, provided the pure products already obtained should be large enough for the purposes required.

But, in my investigations, I have undertaken to prove the negative as well as the positive. I have attempted to carry the process of separation so far, that I might assert the absence of other bodies, as well as the presence of those obtained; and this clearing up of the intermediate fractions has generally been the most tedious part of the work. I have continued to operate upon these by themselves, until they also have become distributed in regular course—no new bodies appearing—among the fractions of constant boiling point, or to such an extent that the intermediate quantities have become too small to admit of further continuance of the process.

This process has been in constant use in my laboratory during the last three years. In this time it has been applied in the study of petroleums, coal oils, the more volatile parts of coal- and wood-tars, the essential oil of cumin, commercial fusel oil, from corn whiskey, and even to mixtures more complex than either of these. As the result of this long experience, I can say that, as regards bodies not decomposed by heat in distillation, I have not yet found a mixture so complex that it may not be resolved by this process into its proximate constituents so completely, that these shall have almost absolutely constant boiling-points. In repeated instances, even from petroleums, I have obtained these constituents so pure, that the contents of an ordinary tubulated retort charged with one of them has been completely distilled off without any essential change of temperature; i. e., not to the amount of ½° C., the thermometer frequently remaining absolutely constant for more than half an hour, a constancy of boiling-point not exceeded by that of distilled water. This state of purity, I think I may safely assert, has never before been attained from such mixtures, by any system of fractional distillation.

As I shall soon be prepared to present to the Academy detailed results of the investigations above referred to, I may omit further allusion to them on this occasion.

I would remark, in conclusion, that it seems to me not improbable that this process may ultimately prove to be of great value in the arts. It is not too much to anticipate that, whenever the various constituents of the mixtures referred to shall have been separately and thoroughly studied in a pure state, some of them may be found to possess properties which will give to them great commercial value, sufficient to justify the expenditure necessary to separate them in large quantities.

Art. XLII.—*Examination of Petroleum from California;* by B. Silliman.

The specimen of petroleum here described came from a natural well or spring on the Simi estate in Santa Barbara county, California. It issues from rocks of the Tertiary Age. The discharge from this spring is sufficient to flow nearly two miles down the dry bed of the ravine in which it occurs.

It is of a dark brown color, dichroic, thin and mobile as water, and of a faint naphtha odor, quite without offensive smell. In a thin tube it has a yellowish brown color by transmitted light, and obviously owes its dark color to its holding in solution a portion of the asphaltum with which it is associated.

Its density is 0·861 at 15° C. or about 34·30° B. It burns, in its crude state, in a double current lamp, without smoke, with quite a bright flame and strong light for a few moments. After eight or ten minutes the wick commences to coal, and after about fifteen minutes it smokes and finally dies out.

1000 c. c. of the crude substance were subjected to fractional distillation in a tubulated retort to which was adapted a thermometer and condenser. A condensible vapor appeared at 60° C., the liquid simmered at 90° and boiled at 123°.

Of the 1000 c. c. of crude oil there were distilled from glass up to the boiling point of

Mercury, - - - - - - -	500 c. c.
In iron, - - - - - - -	496 "
Loss and coke, - - - - - -	4 "
	1000 "

This method of practical distillation gives, as is well known, very unsatisfactory results, as compared with the method of Warren.[1] As the experiment was, however, very carefully conducted by Mr. Peter Collier, of the Sheffield Laboratory, under my directions, it furnishes a good illustration of the differences arising solely out of an imperfect method of analysis, when practiced on the same raw material. I therefore append the results in detail.

The temperatures were noted by a mercurial thermometer the bulb of which was continually in the boiling liquid.

20 c. c. had distilled at	153° C.	260 c. c. had distilled at	234° C.
40 " " " "	170 "	300 " " " "	250 "
100 " " " "	177 "	360 " " " "	266 "
140 " " " "	190 "	400 " " " "	280 "
160 " " " "	200 "	450 " " " "	297 "
200 " " " "	213 "	485 " " " "	320 "
240 " " " "	223 "	500 " " " "	370 "

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[1] See this vol., p. 327.

When about 485 c. c. had passed over, the temperature rose very suddenly from 320° to 370° C.

The remaining 460 c. c. were distilled without noting temperature, in an iron retort, leaving a light carbonaceous residue.

The successive fractions gave the following sp. gr. and explosive tests, the latter being the commercial standard as marking the temperature at which the liquid exhales an inflammable vapor.

	Sp. gr.	Beaumé.	Expl. test.
1st, 100 c. c.	755	55·77	58° F.
2nd, 100 "	775	51·80	70° "
3rd, 100 "	793	47·62	100° "
4th, 100 "	815	42·70	156° "
5th, 100 "	838	37·83	196° "
6th, 100 "	867	32·08	190° "
7th, 50 "	872	31·13	190° "
8th, 100 "	890	27·79	181° "
9th, 100 "	900	26·00	171° "
10th, 100 "	900	26·00	140° "

The color of these products varies from Nos. 1 and 2, which are quite colorless, through 3 to 7, which, commencing in 3 as a faint pink, become red in 6 and 7, while 8, 9 and 10 can hardly be distinguished in color from much of the better quality of crude oil flowing from the best wells, having the same change of color (dichroism) by reflected and transmitted light. The unrectified products would arrange themselves thus by color—

I.	1st,	2nd,	3rd,		fractions.
II.	4th,	5th,	6th,	7th,	"
III.	8th,	9th,	10th,		"

But in the actual conduct of the manufacture of this oil, I presume the distiller would make three divisions:

1st,	Light oil, including the	1st	fractions,	about 53·5°	commercial.
2d,	Burning oil, " "	3 to 7	"	" 38·5°	"
3d,	Lubricating oil, "	8 to 10	"	" 28·0°	"

By mingling a portion of the light oil with the heavier, it is probable that a product of about 60 per cent would be obtained suitable for illumination.

The light oil and the burning oil are perfectly decolorized by the usual treatment with sulphuric acid and carbonate of soda. The light oil has a highly agreeable smell, and none of the other products possess any of the heavy and disagreeable odors common in the petroleum products. The burning oil has a very high illuminating power, and fails to char or encrust the lamp-wick after an hour's burning. The explosive test is 155° F.

The densities of various specimens of crude California petroleum which I have determined range from 0·859 to 0·972, the difference being apparently due to the greater or less degree of evaporation and oxydation of the products consequent on exposure to the warm air of a very dry climate. The specimens thus far examined from California are all from surface springs. When the oil is drawn from artesian borings the same change of density will undoubtedly be observed as in Pennsylvania. I found the surface oil of Oil Creek, in 1855, having a density of ·880 to ·885, thick in warm weather as thin molasses, and in cold weather quite viscid. The flow of wells in the same region is now quite thin, and has a density of ·800 to ·850.

Appended are the results of Mr. Warren's examination of the same California oil now under consideration.

"To Prof. SILLIMAN, New Haven.

"*Dear Sir*—The results of my examination of the crude California petroleum which you sent me through Messrs. Spear, Burke & Co., of this city, and which bore the seal of Messrs. Wyeth & Bro., are as follows:

"Specific gravity at 17 c. 0·864=33° Beaumé.
1250 c.c. of the crude substance subjected to
my process of fractional condensation, gave—
41 c.c. of light oil between 93° and 100° C. } Specific grav-
43 " " " " 100° and 140° C. } ity not taken.[1]
525 " Burning oil " 140° and 310° C. sp. gr.=44° B.
From this point the distillation was conducted in the ordinary manner from a common retort, no thermometer being used.
270 " lubricating oil of sp. gr. 29½° Beaumé.
293 " " " " 28° "

1172 c.c. = 93·8 per cent of total product.

"The residue left in the retort was dry coke. After treatment with sulphuric acid and alkali the light oil and the burning oil were nearly or quite colorless. The lubricating oil had a yellowish color. The odor of the burning oil was extremely agreeable, fully equal in this respect to the best kerosene or refined Pennsylvania petroleum. The other products were also entirely free from disagreeable odor, and indeed the same may be said of the crude oil itself. In this respect it is readily distinguishable from the Pennsylvania petroleum.

"The burning properties of the illuminating oil are not surpassed by any oil which I have seen.

"What I have called *light oil* is not very volatile, and would not, I think, rank in the market as naphtha, at least not that taken between 100° and 140° C.; and it is my opinion that the burning oil would take the whole of the light oil, and still bear the commercial fire test. In that case some of the lighter of the lubricating oil probably might be run into the burning oil, so that the yield of the latter would be over 50 per cent."

Boston, March 31, 1865."

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[1] The specific gravity of the mixture determined by me is 0·753=58° Beaumé.—S.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the determination of lime.*—**Fritzsche** has made the very simple yet important discovery that oxalate of lime may be easily and completely converted into caustic lime free from every trace of carbonate by simple ignition over a blast lamp. The tedious processes recommended in works on analytical chemistry, such as ignition with carbonate of ammonia, conversion into sulphate of lime, etc., become unnecessary, and will hardly be employed in future. The author used in his experiments a platinum crucible 33^{mm}. in height and 27^{mm}. in width at the top: the ignition took place over a gas lamp consisting of six Bunsen's burners, (the ordinary Bunsen's blast lamp would doubtless answer even better). In four experiments Fritzsche obtained from known weights of oxalate of lime 99·9, 100·08, 100·08 and 99·98 per cent of the calculated quantity of lime. In all these experiments the ignition was not continued longer than 15 minutes. In a large platinum crucible, quantities of the oxalate not exceeding one gram could be rendered perfectly caustic in 15 minutes, but 30 minutes were necessary for a quantity of more than 3 grams. Precipitated carbonate of lime was also completely converted into caustic lime by ignition, and the method, as giving results which are more accurate and more easily obtained than those of the older processes, will doubtless come at once into general use.—*Journal für prakt. Chemie*, xciii, 335. W. G.

2. *On the sulphur compounds of uranium.*—**Remelè** has found that the brown precipitate which sulphid of ammonium produces in a solution of nitrate of uranium is neither sulphid of uranium nor a mixture of protoxyd of uranium and sulphur as was formerly supposed. The precipitate changes rapidly when formed in an aqueous solution and becomes orange and then yellow. When formed in an alcoholic solution, the precipitate is permanent and has the formula U_2O_2S, so that it is analogous to the well known chlorid U_2O_2Cl. By boiling in presence of sulphid of ammonium the compound is easily decomposed into sulphur and hydrate of protoxyd of uranium.—*Ann. der Chemie und Pharmacie*, III *Supplementband*, p. 196. W. G.

3. *Note on the regenerative gas-furnaces of the brothers Siemens.*—The regenerative gas-furnaces of the Messrs. Siemens are so interesting and suggestive that we make no apology for again calling attention to them, supposing of course that their general features are sufficiently familiar from the description given by Dr. Faraday, and quoted in the last number of this Journal. Two points which appear to us of special importance are, first, the extremely high temperature which can be obtained and which in fact is limited only by the nature of the materials employed in the construction of the furnace, and secondly, the possibility of employing at will either an oxydizing or a reducing atmosphere. The brothers Siemens have already applied their furnaces to puddling and reheating, but we venture to predict for them a very extensive use in metallurgical processes. Thus, it is well worth while to determine by direct experiment

upon a large scale whether the rich iron ores of Lake Champlain, Lake Superior and Missouri, cannot be directly reduced to the metallic state by heating them to a sufficiently high temperature in the chamber of a Siemens furnace, and then changing the gaseous mixture in the furnace to a reducing condition. This would in fact be blooming upon a large scale, and would not improbably avoid the inconvenience and expense of blooming in the small way, which in spite of the superior quality of the iron produced has been almost wholly superseded by the cheaper process of puddling. Experiment only can determine whether fluxes can be used with advantage in blooming in this manner, when poorer ores are employed. Ores of copper could doubtless be roasted and reduced in furnaces of this construction, and with some additions to the original plan, the sulphurous acid formed during the roasting might be directly converted into sulphuric acid in leaden chambers. But it is for the metallurgy of iron that the new furnaces will probably be found most advantageous. As the temperature attainable is extremely high, it may even be found practicable to melt the malleable iron formed by the direct reduction of the ore, the walls of the fire-chamber being lined with lime as more refractory than any fire-clay. But even if this rather bold suggestion should not be realized, it is at least probable that the earthy impurities of the ore would be reduced to a peculiarly fluid condition, so that the blooms could be easily treated under the hammer and brought into the form of malleable iron. For the rest, there is hardly a branch of manufacture in which heat is employed upon the large scale in which furnaces on the regenerative principle would not find an application. We suggest that small gas furnaces could be made upon the same principle for laboratory use and for various processes in the arts, using the ordinary city gas, however, as fuel instead of gas produced by a special furnace. The high temperature obtained in Gore's gas-furnaces appears to be due to the heating of the air and gas before they mix in combustion. W. G.

4. *On some aluminum compounds.*—BUCKTON and ODLING have studied the action of aluminum upon ethyl-mercury and methyl-mercury, and have thus obtained ethyl-aluminum and methyl-aluminum without difficulty and in considerable quantity. Ethyl-mercury, or, as the authors term it, mercuric ethid, with an excess of clippings of aluminum, was heated for some hours in sealed tubes in a water bath, when the mercury was found to be completely displaced by the aluminum. After distillation the aluminic ethid was obtained as a colorless mobile liquid which boiled at 194° C. and did not solidify at $-18°$ C. It evolved dense white fumes on exposure to the air, and when in thin layers took fire spontaneously burning with a bluish-red edged flame, and producing an abundant smoke of alumina. Analysis gave nearly the formula $Al_2(C_4H_5)_3$ or $Al_4(C_4H_5)_6$. The vapor-density at 234° C. was found to be 4·5, the theoretical number for $Al_2(C_4H_5)_3$ being 3·9, whence it would appear that the true molecular formula is $Al_2(C_4H_5)_3$ and not $Al_4(C_4H_5)_6$. Water decomposes aluminic ethid with explosive violence. Iodine forms iodo-derivatives and iodid of ethyl. Oxygen gives a body apparently analogous to boric di-oxyethid. Aluminic methid was obtained as a colorless mobile liquid boiling at 130° C. and solidifying a little above 0° to a beautiful transparent crystalline mass. The liquid takes

fire spontaneously in the air. The formula of this body was found to be $Al_2(C_2H_3)_3$, with which the vapor-density 2·80 closely agrees, the theoretical number being 2·5. But the vapor-density was found to increase very rapidly as the temperature diminished, so that it is probable that at a temperature not far removed from the boiling point the molecular formula would be $Al_4(C_2H_3)_6$. The authors ask whether the observed vapor-density of aluminic chlorid may not correspond to the high density of aluminic methid, so that both may be anomalous and untrustworthy for determining the general formulæ of aluminum compounds. —*Chemical News*, No. 271, p. 61.

5. *On the density of the vapor of salammoniac.*—H. St. Claire Deville has repeated his important experiments on the density of the vapor of sal-ammoniac, and has replied to the objections made by various chemists to the conclusions drawn from his first researches. The author shows conclusively, by an ingenious piece of apparatus, that chlorhydric acid and ammonia, each heated to a temperature of 360° C., combine with a sensible evolution of heat, so that at this temperature the vapor the density of which is taken cannot be regarded as a mixture of HCl and NH_3, but must be admitted to be NH_4Cl, the vapor-density of which corresponds to 8 volumes ($H=2$) and not to 4 volumes as in the case of other organic compounds. Deville suggests that the proof that no simple or compound substance corresponds for one equivalent to 1 or to 8 volumes rests with those chemists who make this assertion, and that until that proof is given the assertion amounts to a pure hypothesis. Among other difficulties, the following is suggested to the partisans of the so-called anomalous densities.

(1.) Sulphydric acid forms with ammonia two distinct crystalline and volatile compounds, the formulæ of which are respectively NH_4S, and NH_4S, HS. Sulphid of ammonium represents 4 volumes of vapor; its condensation is $\frac{1}{3}$: sulphydric acid and ammonia combine and remain combined at the temperature (for example 100° C.) at which we determine the density of the vapor. Sulphydrate of sulphid of ammonium represents 8 volumes of vapor; its condensation is nothing. If we suppose its elements separated at the temperature at which we take the vapor-density (100° for example), we must admit that it separates into ammonia and sulphydric acid, NH_3 and $2HS$, each giving four volumes and having 8 volumes as their sum. Now at this temperature the elements can only separate into sulphid of ammonium and sulphydric acid, giving the one 4 volumes and the other 2 volumes, the sum of which is 6. Hence, if the sulphydrate were decomposed at the temperatnre at which the vapor-density is taken, it ought to furnish 6 volumes, and as it furnishes in reality 8 volumes, it follows that it is not decomposed.

(2.) In the case of bodies, simple and compound, which present vapor-densities variable with the temperature, it cannot be said that these densities would become normal if the temperatures could be sufficiently increased, because, as Deville and Troost have shewn, as the temperatures increase the vapor-densities diminish toward a constant limit, so that the coëfficients of expansion must also become constant and differ insensibly from 0·00367. To admit that the coëfficient of expansion of arsenic and phosphorus vapors may be different from this number, in order to show that these vapors do not correspond to one volume ($H=2$) is to make an assumption which is inadmissible in the present state of science.—*Comptes Rendus*, lix, 1057.

6. *On the dispersion of light produced by the rotation of the plane of polarization in quartz.*—Stefan has given a beautiful method of forming a spectrum by means of the dispersion of the planes of polarization produced by plates of quartz. A bundle of parallel plane-polarized rays of white light is allowed to fall upon a glass cone, the sides of which make with the axis an angle equal to the angle of polarization. The reflected rays are received upon a screen perpendicular to the axis of the cone and produce a circular sector in which the maximum intensity of light corresponds to the rays, the plane of reflection of which is parallel to the plane of polarization. If we introduce a plate of quartz cut perpendicular to the axis into the path of the bundle of incident parallel rays, the position of the maximum is different for differently colored rays, and the sector then exhibits the different colors of the spectrum. In this manner the dispersion of the planes of polarization is rendered visible. In the same manner, if we receive the polarized bundle of rays transmitted by a plate of quartz upon a plate of spar cut perpendicular to the axis which gives in the analyzer a system of rings traversed by a black cross, each of the branches of this cross will be replaced by a sort of fan exhibiting the different colors of the spectrum.

Measurements the details of which are not yet published have led Stefan to represent the rotations due to a plate of quartz 1^{mm}. in thickness by the formula

$$\varrho = \frac{8\cdot1088}{\lambda^2} - 1\cdot697$$

in which the wave length λ is expressed in thousandths of a millimeter. From this formula we should deduce the remarkable consequence that the rotation would be zero for rays of a wave length equal to $0\cdot002186^{mm}$, and would change its sign when this limit is exceeded.—*Ann. de Chimie et de Physique*, [4], iii, 501.

W. G.

7. *On combustion by invisible rays.*—At the Newcastle meeting of the British Association in 1863, Dr. C. R. Akin proposed three experiments by means of which, if successful, rays of heat could be converted into rays of light. One of these experiments consisted in converging the sun's rays by means of a concave mirror, then cutting off the light by "proper absorbents," and then igniting platinum foil in the focus of the invisible rays. Want of means appears to have prevented the execution of the proposed experiments; but Dr. Tyndall, who had devised experiments of a similar character, and who had discovered a liquid possessing precisely the required properties, has recently succeeded in the conversion of rays of heat into rays of light in a very striking and instructive manner. Tyndall employed an electric lamp of Duboscq and a linear thermo-electric pile, the spectrum being formed by lenses and prisms of rock salt. As in the case of the solar spectrum, the heat was found to increase in intensity from the violet to the red, rising to a maximum beyond the red at a distance about equal to the distance of the green on the more refrangible side. The curve of intensities rises beyond the red into a steep and massive peak, quite dwarfing the radiation of the luminous part of the spectrum. Water interposed cuts away the peak in part; the vapor

of water, according to Tyndall, would do the same, and this explains the difference between the heat-spectra of the electric and solar light, the peak being much higher relatively in the former than in the latter case. After several trials the author found that a solution of iodine in bisulphid of carbon, so opake as to cut off the light of the midday sun, was, within the limits of experiment, absolutely transparent to invisible radiant heat. When the rays from the electric lamp were concentrated into a cone by means of a small glass mirror silvered in front, a cell containing the solution of iodine absorbed the light completely, while the invisible rays of heat being brought to a focus set fire to paper, wood, charcoal and other substances. With a sufficient battery power a plate of platinized platinum is rendered white-hot at the focus of the invisible rays, and then yields a complete and brilliant spectrum. The image of the carbon points of the electric lamp formed upon the platinum plate becomes visible as the plate becomes red-hot, and a species of thermograph is obtained, as is well shown by drawing the points apart or causing them to approach each other, the red-hot image following the motion. In this case the invisible rays of heat have their refrangibility raised in becoming visible, while in the case of fluorescence the invisible rays of light have their refrangibility lowered. Tyndall proposes to apply the term *calorescence* to the phenomena of heat in question, a term which is certainly preferable to the word "calcescence" employed by Akin. We regret to see that a bitter personal controversy has been carried on in a prominent English scientific journal between Dr. Tyndall and Dr. Akin as to the proprietary rights of each in the beautiful experiment above described. Into the merits of this controversy we cannot enter; we may, however, be allowed to express the opinion that the experiment in question does not involve much originality of conception, but that it is a very simple and natural deduction from the experiments and results of Melloni upon smoked rock salt and upon the production of light without sensible heat. —*L. & E. Phil. Mag.*, March, 1865, p. 241. W. G.

8. *On the Electrical properties of Pyroxiline-paper and Gun-cotton;* by Prof. J. Johnston. (In a letter to Prof. Silliman, dated Middletown, Conn., January 25, 1865.)—You did me the honor, a year ago, to publish in the Journal a note of mine on the electrical properties of *pyroxiline paper* and *gun-cotton*, adding an approving note of your own. Having lately had opportunity to make some further experiments with the same substances, I herewith send you the results.

It is proper to say that both the paper and the cotton were prepared nearly a year ago, and may have undergone some change, though nothing of the kind was apparent to the eye, except that one sheet of the paper used seemed at one place a little discolored.

The substances experimented with were *amber, sealing wax, sulphur, gum lac, pitch, rosin, caoutchouc* (native rubber), *hard rubber* (stick obtained of Messrs. J. F. Luhme & Co. of New York), *common vulcanized rubber* (as used in forming gas bags), *gutta percha*, and various crystallized mineral substances. But these last, becoming always positive, will not be further alluded to. The sulphur by friction with the gun-cotton always became positive, and also by friction with different sheets of the paper, except in a single instance, when using the paper which was slightly discolored, it appeared to be feebly negative. Rosin, pitch, gum lac, and amber, both with the paper and the cotton, became always positive, as did also the native rubber, by which I mean the rubber as it is imported. Sealing-wax with the cotton became always positive, but with the paper occasionally negative. Vulcanized rubber (the kind used in making gas bags) would sometimes become positive and sometimes negative, and the same was true of gutta percha, two different specimens being used. The hard rubber (from Luhme & Co.) became always negative, both with the paper and the cotton. This being contrary to the results obtained by yourself with this substance, I made very many trials, but always with the same result.

In all cases after friction with other substances, whether the latter became positive or negative, the paper and cotton would be found invariably negative. Sheets of the paper, when handled, especially in cold, dry weather, often become highly excited,—always negatively, so far as has been determined; and my son informs me that sometimes, when handling considerable quantities of the recently prepared paper, he was even fearful that it might become ignited by the sparks produced!

I will just remark in closing, that in making experiments like these, great care is required in order that the results arrived at may be satisfactory. This is particularly the case when it is necessary to rub a substance, as a roll of sulphur or a stick of sealing-wax, successively with different substances, some of which give the positive and others the negative electricity. Occasionally it will be found that the substance will be positive at some points and negative at others; and in such cases the only way is to lay the particular specimens aside until they shall have returned to their natural condition at all points. Sometimes a substance when first rubbed, after having remained undisturbed twenty-four hours or more, will take on one electricity, but, by continuing the friction a very little time, it will take on the other. Thus, a stick of sealing-wax in its natural state, when gently rubbed, *one* or *two strokes*, with a silk handkerchief, will often be found decidedly positive, but by a few strokes more it will become as decidedly negative; and it cannot be made positive again by friction with silk until allowed by repose first to return to its natural state.

II. MINERALOGY AND GEOLOGY.

1. *On Tin Ore at Durango in Mexico;* by Prof. C. F. Chandler.—I have recently examined a sample of 1450 grams of tin ore from Durango, and find it to be a handsome "wood tin" in pebbles and fibrous crusts, some of which are an inch in their longest diameter. The color varies from a very light brown to black.

Associated with the cassiterite, there are brilliant crystals of topaz, some of which are half an inch long. They vary from transparent to opaque, and from colorless to deep brown. I have not been able to examine them very closely, but have noticed the planes O, I, $i\breve{2}$, and 2. Owing to the development of the planes $\widetilde{i2}$ and 2, and their rich brown color, some of the crystals might easily be mistaken for cassiterite.

The sample examined gave—

Tin (by crucible assay), - - - - - -	50·90
Topaz crystals easily separated by the process, - -	3·10
Other topaz crystals, too small to be easily separated, -	1·00 (?)
Oxygen and impurities (by difference), - - -	45·00
	100·00

The material used for the assay was very carefully averaged, by pulverizing about 500 grams of the ore, after the topaz crystals had been separated. A correction was subsequently made in the result of the assay corresponding to the quantity of topaz removed.

School of Mines, Columbia College, N. Y., Dec. 23.

2. *Note by F. B. Meek and A. H. Worthen in relation to a genus of Crinoids from the Coal-measures of Illinois and Nebraska, proposed by them on p. 174 of the last number of this Journal.*—At the time we proposed the name *Erisocrinus* for this genus, we were not aware that M. Koninck had described it under the name *Philocrinus* in 1863, among some Carboniferous fossils from India (see *Memoire sur Foss. Palæozoiques Recueillis dans L'Inde*, p. 21. *Liege*, 1863). Owing to the fact that Mr. Koninck's figure, (probably from the wearing of the stone from which the impressions were taken,) does not show the basal pieces, nor distinctly their connection with the subradials, we had overlooked the generic identity of the American and Indian species. On comparison with his description, however, it becomes evident that our Crinoid agrees exactly in its generic characters with his. This being the case, we cheerfully withdraw our name, and arrange our species, which differ specifically from that described by him, under his name. In doing this it also becomes necessary to change one of our specific names (*E. typus*), since it is not the typical species of Koninck's genus. Hence our species may be called *Philocrinus pelvis* and *P. Nebrascensis*.

It is worthy of note that Mr. Koninck's figure of the arms and secondary radial pieces of this type, together with its other characters, show it to be closely allied to the still older genus *Bursacrinus*, Meek & Worthen, 1861, which only differs in the possession of an anal piece, and the consequent truncation of one of the subradials.

3. *On the Oil Formation in Michigan and elsewhere;* by Prof. A. Winchell, of the Michigan University. From a report on the "Baker Tract," near Lakeport, St. Clair Co., Michigan. 8 pp. 8vo. Detroit, 1864.—In most of the Northwestern States occurs a formation which has been known among American geologists as the *Black Slate* or *Black Shale.* It is well known in Ohio, Indiana, and Kentucky, and extends also into the surrounding States. It was formerly supposed by Prof. Hall, to be the western representative of the "Marcellus Shale" of New York; but the researches of western geologists tend rather to prove its equivalency with the "Genesee Shale" of New York. The color of the rock is due to the presence of a large amount of carbonaceous or bituminous matter, believed to be derived from the vegetation of the period in which the shales were deposited.

Similar black shales were brought to light by Dr. Houghton, many years ago, in the lower peninsula of Michigan; and the opinion was expressed by him and by Mr. Hubbard, his assistant, that these shales were of the same geological age as the black slate of Ohio and Indiana. The same formation was subsequently described by Mr. Murray, of the Canada geological survey, as making its appearance at various points in Canada West; and the geologists of that survey speak of the formation as constituting the principal part of the "Hamilton group" as known in Canada. In the prosecution of the geological survey of Michigan, I enjoyed the opportunity to establish, by direct comparisons, the identity between the Black Slate of Michigan and that of Ohio and Indiana, and, by proving that it lies stratigraphically above the limestones of the Hamilton group, have established its distinction from the Marcellus shale, and co-ordinated it with the Genesee shale of New York. It is locally known as the *Huron Group.*

The geographical distribution of this formation in Michigan, will be seen by a reference to my Report for 1860. The strata along the eastern slope of the lower peninsula of Michigan dip westward beneath the Carboniferous rocks of the interior. This dip, however, is extremely slight, and the Huron group does not terminate at the waters of Lake Huron and the river St. Clair. It prolongs itself eastward, and overlaps the angle of Canada West, finally thinning out along a line extending from the mouth of Maitland river to a point on the Great Western Railway, a few miles east of Bothwell.

The lithological characters of the formation are but little varied. Throughout its entire thicknesss of 600 or 700 feet, it is argillaceous and generally bituminous. The most bituminous and darkest colored portions seem to be situated near the bottom of the group. We know, at least, that they are not near the upper part of the group. Other portions of the series consist of bluish, argillaceous shales and plastic clays, ranging in color from bluish to whitish. Numerous bands of sandstone, and of arenaceous magnesian limestone, from four to eighteen inches in thickness, occur irregularly in the middle and upper portions of the group. Bands of iron pyrites, of local extent, but sometimes from one to six inches through, are found imbedded in the shales and clays. The "black slate" proper, of the series, is freely combustible; and in some cases, where fire has been communicated by accident to outcropping masses, the combustion has survived for months. By distillation, this shale affords petroleum and all the related compounds. Its appearance at the surface is generally regarded as an indication of coal, and many serious disappointments have resulted from ignorance of the fact that the "black slate" is several hundred feet beneath the coal measures.

The bituminous shales of this group of rocks are generally considered to be the source of most of the native petroleum of the Northwestern States. It is the opinion of Dr. T. S. Hunt, of the Geological Commission of Canada, that the product issues from a bituminous limestone—the Corniferous limestone—next underlying these shales; but though this limestone may unite its exhalations with those of the Huron group, it seems more probable that the latter supplies the principal amount of the bituminous flow. It is also asserted that, at least in a few cases, petroleum is eliminated in quantities of commercial importance from the rocks embraced in the coal measures.

Wherever the oil-producing shales are exposed to the air, or covered by a porous medium, the product of the slow spontaneous distillation going on escapes into the atmosphere, and is lost. Where the shales are covered by an impervious layer, as of shale or plastic clay, the oil and gas elaborated are retained in the rocks, filling cavities by driving the water out by elastic pressure, and saturating porous strata embraced in the formation, or intervening between it and the impervious cover above. In the oil region of Ohio and Pennsylvania, an overlying sandstone becomes the reservoir in which these products are accumulated; and the oil wells of that region consequently terminate in that sandstone without penetrating to the original source of the oil. Throughout the oil-producing region of Canada and Michigan, this sandstone is wanting, but the destruction of the outcropping shales by geological agencies has created an immense deposit of argillaceous drift material, which forms an impervious cover for the petroleum beds. In Enniskillen the lower portion of this drift deposit is rendered somewhat porous by the presence of gravel, and this, in consequence, has become saturated with oil in the same manner as the overlying sandstone of Ohio and Pennsylvania. By boring into the rock, however, a lighter and purer oil is found surcharging the beds of sandstone and arenaceous limestone by which the group is intersected. It is found that the quality of the oil improves with the depth from which it is obtained. That drawn from the drift gravel lying upon the surface of the rock, is dark, thick, and impure, and commands but a moderate price for purposes of distillation. It is now principally employed for lubrication.

Though it is not yet actually demonstrated that rock oil exists in Michigan in quantities of economical importance, we possess every favorable indication which has ever been discovered in the prosecution of this new branch of industry. In the first place, the topography of the oil region of Michigan is identical with that of Enniskillen. The surface is level, rather low, and inclined to dampness. The soil is thin, argillaceous and indifferent for agricultural purposes. The forest growth, nevertheless, is dense and luxuriant, consisting of Beech, Hard Maple, Swamp Elm, and Black Ash, with occasional trees of White Ash, White Oak, Hickory, Sycamore, &c., interspersed. The water in the vicinity of the oil manifestations is of a dark color. Black river of Michigan seems to be the counterpart of Black creek of Enniskillen. It would seem evident that the color of the water in both cases must be due to some constituent of the crude oil; and yet a chemical examination of the water of Black river taken at Port Huron, did not sustain my expectations. It should be tested six or eight miles further north.

When we compare the more direct indications, the resemblance is equally complete. The oil region of Michigan abounds in "gum beds," or masses of inspissated oil, some of which have hardened to the character of asphalt. The oil is still issuing, and it shows itself on the surface of standing water everywhere throughout the Baker tract. Moreover, inflammable gas, the almost infallible criterion of an oil district, exists in very great abundance, and, in several instances, has been known to escape with noise and violence from the bottom of wells. In one instance the jet, when lighted, illuminated the country over a radius of four miles, and the sound was like that of steam escaping from a high-pressure engine. Many similar cases have occurred.

Whatever differences exist between the geological indications observed in the two regions are rather in degree than kind; and the advantages are undoubtedly on the side of the Michigan region. Canada West is located upon the thinning outskirts of the formation; Michigan embraces the great mass of it. And, in the abundance of gas emitted from the underlying rocks, the Enniskillen region bears a very unequal comparison.

It is quite possible that no porous stratum will be found in Michigan reposing upon the surface of the rock. Its occurrence is accidental anywhere. Should it be found wanting, and "surface oil" consequently wanting, this fact would not have the slightest bearing on the probability of oil in the rocks below.

I have stated that the Huron group, or Oil formation, extends continuously from the peninsula of Michigan into the peninsula of Canada West. To embrace both regions under one designation, they might be spoken of as the *Peninsula Oil Region.* It is not to be imagined that oil will certainly be obtained at all points within this region. It may be, but probably will not. Slight undulations are liable to exist in all strata so nearly horizontal as those of the two peninsulas. The volatile emanations from the strata would therefore accumulate under the highest arches. Possibly one of these arches runs nearly north and south through the Canadian region, from Bothwell to Petrolea, and beyond, while another underlies the district of petroleum manifestations on the Michigan side of the river. For the present, it would be safest to confine large expenditures to the immediate vicinity of this belt of indications.

In the transverse section across the strata, already referred to, an attempt has been made to show in what way the oil-bearing rocks may rise in two or more gentle undulations, thus inducing the oil and gas to accumulate along geological axes, and become restricted to belts of country of limited width.

4. *Account of some new or little known species of Fossils from rocks of the age of the Niagara Group.* From the 18th Report on the New York State Cabinet; printed in advance of the Report, and issued January 1865, by Prof. James Hall. 48 pp. 8vo, with many illustrations.—These pages contain descriptions of a number of new species of Cystideans, with figures of seven of them, and of several Crinideans, Trilobites, and various Mollusks. The following important remarks on the age of the Galt or Guelph limestone of Canada, and Leclaire beds of Iowa, and their relations to the New York Niagara group, form an introduction to the paper:

In the study of the fossils of the Niagara group and associated strata, previous to the publication of the final report on the Fourth Geological District,[1] and also to the publication of the second volume of the Paleontology of New York, I separated a few species found in the impure drab-colored limestone from Wayne county, New-York.

The limestone containing these fossils became exposed only in the bed of the canal during its excavation; and the low country, or deeply drift-covered surface in the vicinity did not admit of any exposure of the

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[1] The same reference of species had in fact been made during the Geological Survey; and they were thus published in the Report on the Fourth Geological District in 1843.

rock in place. The materials were thrown out of the excavation in connexion with the soft marls of the Onondaga-salt group, and the specimens of rock containing the fossils preserved the peculiar celluliferous structure and characteristic color of the argillaceous limestone of that formation. Differing so especially from any known beds in the Niagara group, I did not hesitate to refer them to the Onondaga-salt group, since there was no evidence of any other formation in the neighborhood.

Throughout the State of New York, the country along the junction of the Niagara and Onondaga-salt formations is low and level, or covered by drift accumulations; and no opportunity offered of discovering any exposure of similar beds along the course of the outcrop. In some places in Monroe county, we have been able to trace the two formations to within a few feet of their contact with each other; but no fossiliferous beds, similar to those of Wayne county, have been found. At a later period,[2] my attention was called to some peculiar fossils collected at Galt, in Canada West; and in visiting the locality, I discovered some pieces identical with those before known, from beds which I had regarded as of the Onondaga-salt group in New York. As this limestone at Galt [and Guelph] was clearly above the great Niagara limestone of the Falls, and contained an almost entirely different set of fossils, I very naturally inferred that it belonged to the next higher formation, or the Onondaga-salt group; and that the Wayne county locality was a feeble representation of the limestone of Galt.[3] For these reasons the two were treated as identical, and referred to the age of the Onondaga-salt group; an opinion at that time sustained by the members of the Canadian Geological survey.

At a later period, during the Geological Survey of Iowa, I recognized, at the Leclaire rapids on the Mississippi river, a limestone holding the same relative position, having the same lithological character, and containing some identical and many similar fossils with the limestone of Galt or Guelph in Western Canada; and I thus announced its apparent relations in the Report on the Geology of Iowa.

"Should the identity of the limestone of these two distant localities be proved, it will afford sufficient ground for separating these beds from the Onondaga-salt group, and for establishing a distinct group. It seems quite probable that the limestones of this period have their eastern extremity in Central New York, where, from their small development, as well as from similarity of lithological character, there seemed no sufficient ground for separating them from the non-fossiliferous beds of the Onondaga-salt group.[4] Since, however, in Canada, these beds attain considerable importance, and (admitting the conclusions above given) acquire a still greater thickness and more distinctive character on the Mississippi river, it seems necessary to elevate them to the same rank as the other groups of the series." *Geology of Iowa*, vol. i, p. 75, 1857.

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[2] After a considerable part of vol. ii had been printed.

[3] The name *Galt* being considered objectionable, on account of a similar term already in use, and the same rock occurring also at Guelph, it has been called the "Guelph formation," in the nomenclature of the Geological Survey of Canada.

[4] My views regarding the presence of the Onondaga-salt group proper in Wisconsin and Iowa have somewhere been called in question, and I have only to remark in this place that I have seen no reasons on my own part, nor facts adduced on the part of others, to change my opinion in reference to the occurrence of this formation in the localities I have heretofore cited.

Some two or three years later, I explored the geology of the central and eastern portions of Wisconsin and the adjacent parts of Illinois. I here found the limestone of Racine, and a part of Waukesha and some other localities, resembling in all respects that of Leclaire, and holding many of the same fossils. It is likewise underlaid by the even-bedded darker-colored limestone, bearing *Halysites catenulatus*, *Pentamerus oblongus*, and many large Orthoceratites, which are everywhere regarded as evidence of the Niagara age. I could not hesitate, therefore, to parallelize the succeeding beds with the limestone of Leclaire, though we had failed to trace it across the country in a continuous outcrop. At the same time, on critical examination of the collection of fossils made at Racine and at some other points, I detected many species known as characteristic of the Niagara formation in the State of New York, requiring its recognition as a member of that group (rather than of the Onondaga-salt group), and uniting with it, as identical in position, the Leclaire limestone.[5]

At the same time, we have recognized, from Racine and adjacent localities, including Leclaire in Iowa and a single locality in Illinois, the following species which are identical with, or are very closely allied to, those from Galt in Canada West: *Pentamerus occidentalis*, an *Obolus*-like fossil, a *Favosites* and a species of *Amplexus* which are identical in several localities, *Cyclonema sulcata*, *Murchisonia Logani*, *Murchisonia* identical with or closely allied to *M. mylitta* (Billings), an undescribed *Murchisonia* from Racine identical with one from Galt, *Subulites ventricosa*, *Pleurotomaria solaroides?* *Loxonema longispira*, besides other forms which are closely allied to species of the Guelph limestone.

An examination of several localities in Wisconsin shows that this peculiar fossiliferous limestone is very unequally distributed. At Racine it has a very considerable thickness;[6] while in other places, either from denudation or other causes, it is very thin, or even absent. In some places in the vicinity of Milwaukee and Waukesha, there are indications of beds of passage from the regularly bedded limestones below to the unequally bedded rock above. There appears, indeed, very good evidence of the irregular or unequal accumulation of this higher rock in many of the localities along a considerable portion of the outcrop; and where the lower part of the formation comes to the surface, the upper rock does not appear to be developed. I am therefore induced to believe that this limestone at Racine, the mass at Leclaire and extending thence into Iowa, as well as the Guelph formation in Canada and the feeble representation of the same in New York, are really lenticular masses of greater or less extent, which have accumulated upon the unequal surface of the ocean bed in a shallow sea during the latter part of the Niagara period. These isolated masses of limestone have close relations with each other, while their relations with the Onondaga-salt group, though very intimate in the single locality in Central New York, become less and less conspicuous in a westerly direction.

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[5] Report on the Geology of Wisconsin, p. 67, 1861.

[6] I am inclined to believe that I have overestimated the thickness of the limestone at Leclaire from the presence of lines of false bedding, but I have had no opportunity for a re-examination of the locality.

5. *Observations on the Geology of Southern New Brunswick*, made principally during the summer of 1864 by Prof. L. W. BAILEY, and Messrs. G. F. MATTHEW and C. F. HARTT, prepared and arranged, with a geological map, by L. W. BAILEY, A.M., Prof. Chem. in the Univ. of New Brunswick, &c. Printed by order of the House of Assembly. 160 pp., 8vo. Fredericton, 1865.—In this Report, the distribution of the formations is given, with many details as to structure, fossils, geological relations, and economical products. The formations included are the Paleozoic, with the Azoic below these, and the Triassic Red Sandstone above, together with Post-tertiary beds. The Report contains a large colored geological map, showing the limits of the out-cropping rocks. The outcrops are nearly parallel with the coast, or about N.E. by E. in direction. The Azoic is in the region of Portland; and at St. Johns, between it and the Bay of Fundy, occur Primordial rocks, affording, according to determinations by Mr. Hartt, *Paradoxides* and other characteristic fossils, besides some Brachiopods. Mr. Hartt observes upon these fossils:—

"Representatives of four genera of Trilobites have been obtained thus far from the Saint John rocks, viz:—*Paradoxides*, *Conocephalites*, *Agnostus*, and *a new genus?* allied to *Conocephalites*.

The number of species in each genus has not yet been satisfactorily made out; but of *Paradoxides* there are at least five, of *Conocephalites* seven, and of *Agnostus* and the *new genus* each one.

All the species appear to be new. One of the *Paradoxides* bears a close resemblance to *P. rugulosus*, Corda, from the *Etage* C of Barrande, in Bohemia, and one of the *Conocephalites* is allied to *C. coronatus*, Barrande, from the same fauna and horizon, though neither is identical with the European species.

There are six species of Brachiopoda, belonging to the genera *Orthisina*, *Discina*, *Obolella*, and *Lingula*. I have not been able to identify any of the forms with described species.

Though all the species from the Saint John group are apparently new, yet the occurrence of *Paradoxides* and *Conocephalites*, genera confined entirely to the so-called *Primordial fauna* of Barrande, and everywhere characteristic of it, together with the strong likeness borne by the Saint John species, in their facies, to those of the same genera of the faunæ of the "Primordial" in Europe and America, enable us unhesitatingly to assign to the Saint John group, or at least to that lower part of it which has afforded Trilobites, a geological position equivalent to Barrande's Etage C, or to the Potsdam proper of America.

The lower part of the Saint John Group, at Coldbrook, has been divided by Mr. Matthew, on lithological grounds, into three Bands, viz:

No. 1. The lower or arenaceous band, with no determinable fossils, and constituting passage beds from the Coldbrook Group.

No. 2. Argillaceous shales, rich in fossils, *Paradoxides*, *Orthisina?*, *Conocephalites*, *Obolella*.

No. 3. Carbonaceous shales, full of fossils, *Paradoxides*, *Conocephalites*, *Orthisina*, *Discina*, &c., all much distorted."

Prof. Bailey, speaking of the *Albertite*, states that in his opinion and that of Mr. Matthew, "it is neither coal nor jet, but an oxydized oil, derived from the decomposition of fish remains, and subsequently changed by chemical action," giving as reasons the position and constitution of the material, the nature of the adjoining rocks, and the fact that "springs containing oil are not uncommon throughout the district in which the Albertite is found."

6. *On Devonian Insects from New Brunswick;* by S. H. Scudder. (From a communication addressed by Mr. Scudder to Dr. C. F. Hartt, of New Brunswick, dated, Boston Society of Natural History, January 11, 1865.)—I have made as careful an examination as my present circumstances will permit of your most interesting collection of the fossil remains of insect-wings from Lancaster. There are ten specimens in all, eight of which are reverses of one another, thus reducing the number to six individuals; of these, one, a mere fragment, belongs, I think, to the same species as another of which the more important parts of the wing are preserved, so that we have five species represented among these Devonian insects, and these remains are all, I suspect, composed of portions of the anterior wing alone. The data being thus fragmentary, the conclusions cannot be quite so satisfactorily determined as we could wish, but we can still discover enough to prove that they are of unwonted interest. Besides the peculiar interest which attaches to them as the *earliest known traces of insect life on the globe*, there is very much in themselves to attract and merit our closest attention.

One of them is a gigantic representative of the family of *Ephemerina* among Neuroptera, some three or four times the size of the largest species now living with which I am acquainted.

Another borrows some striking points of the peculiar wing-structure of the Neuropterous family *Odonata*, and combines with them those of families remote from that, and even belonging to a distinct section of the Neuroptera, exhibiting to our view a synthetic type which combines in one the Pseudoneuroptera and the Neuroptera, and represents a family distinct from any hitherto known.

Other fossil insects, found in Carboniferous concretions in Illinois, and described in this Journal ([2], xxxvii, 34), which Professor Dana has kindly allowed me to evamine,[1] also belong to hitherto unrecognized families, exhibiting similar relations to sections of Neuropterous insects in our day disconnected; and your third species is a member of still another family of Neuroptera, which finds its natural relations between the two described by Professor Dana.

A fourth, of which only an unimportant fragment was found, would seem to belong to the Neuroptera; but by some peculiarities of the minuter cross-veins, thrown off in the middle of the outer edge of the wing in a most irregular and unusual manner, suggests no intimate relations with any known family, but must have belonged to a group of large and weak-winged insects.

The fifth and last to be mentioned is of very striking interest, because, while it exhibits the peculiar venation which forms the well known tympanum or stridulating apparatus of the male in the Orthopterous family *Locustariæ* (though differing somewhat from that), it also most resembles the Neuroptera in all, or nearly all, the other peculiarities of its structure, and suggests the presence in the insect-fauna of those ancient times

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[1] The results of this examination will shortly be communicated to this Journal.

of a synthetic type, which united the characteristics of the Orthoptera and Neuroptera, in themselves closely allied; this point however requires patient and severe investigation, and only my earliest impressions are here recorded, made, however, immediately after a close examination into the relations of other fossil insects.

7. *Note on the Azoic age and metamorphic origin of the Iron Ore of Mexico, described by N. S. Manross, at page* 309 *of this volume;* by J. D. Dana.—The great thickness and extent of the magnetic and specular iron masses of Guerrero leave little doubt that the rocks belong, like those of Northern New York, Michigan and Canada, to Azoic or præsilurian time, and thus they indicate the existence of an Azoic area in this part of the continent. The metamorphic nature of the iron ore is proved by the alternation of the beds with conformable layers or strata of granular or metamorphic limestone, and syenite or granite. In a section of the Chutla "iron mountain," figured by Mr. Manross, the great iron ore bed is represented as lying beneath "beds of limestone," and over granite. In another, of the iron mountain of Las Anonas, the largest bed of iron ore is divided along the middle by a bed of limestone, and on either side there are alternate beds of iron ore and syenite with epidote. In another, the Sochipala region, the iron bed has beds of granular limestone above it; and below, syenite, then another iron ore bed, 30 feet thick, and then syenite again. Thus the evidence is the same in kind as in the Azoic region of the north.

8. *Report on the Geology of Illinois;* by A. H. Worthen.—The Legislature of Illinois has recently appropriated $20,000 for the publication of 3000 copies of the Geological Report of Illinois, made by Mr. Worthen under the authority of the State. The plates will be numerous, and, as we know from an examination of the figures, excellent. In working up the Paleontology, Mr. Worthen has had the assistance of Mr. F. B. Meek for Zoology, and L. Lesquereux for Botany. The Report will yield to none hitherto published in the country in the value of its contributions to science. The State is especially rich in fossil shells, fishes and crustaceans of the Carboniferous age, and affords many new species of coal plants.

9. *Check List of the Invertebrate Fossils of North America: Miocene;* by F. B. Meek. Smithsonian Miscellaneous Collections. 32 pp. 8vo. Washington, Nov. 1864.—This list of Invertebrate Miocene Fossils of North America is complete to the time of publication, both as regards paleontological discovery, and zoological science. Critical remarks are appended to the list.

10. *Ichthyosaurian Skin.*—A specimen of the *Ichthyosaurus tenuirostris,* recently obtained at Barrow-on-Soar, shows a large extension of the dermal covering upon the surface of the slab, seeming to indicate that the animal had a prominent ridge along the dorsal surface similar in appearance to that which the males of the pond-newt, *Triton cristatus,* present in spring.—*Reader, Dec.* 3.

11. *Murchison on the Drift.*—The last annual address of the President of the Royal Geographical Society, Sir R. I. Murchison, contains a discussion at length of the question of the European drift, in which the author sustains the Iceberg theory. Only the want of space prevents our citing his views in this Journal.

12. *Annual Report of the Geological Survey of New Jersey, for the year* 1864, by Prof. GEO. H. COOK, State Geologist. 24 pp. 8vo. Trenton, N. J., 1865.—Prof. Cook presents, in his Report, a brief statement of the progress of his survey, dwelling mostly on economical results. A colored map of the formations is included.

III. BOTANY AND ZOOLOGY.

1. DECANDOLLE: *Prodromus Syst. Nat. Regni Vegetabilis.* Pars 16, Sect. posterior, Fasc. I, *sistens Cupuliferas*, etc. Nov., 1864. pp. 160.—In pursuance of the plan adopted for bringing out the concluding volumes of the Prodromus in parts, as soon as the respective orders are elaborated, we have here the *Cupuliferæ*, upon which Mr. DeCandolle himself has bestowed so much labor,—the leading genus, *Quercus*, extending to 281 species, *Castanopsis* (to which belongs *C. chrysophylla* of California), 14 species, *Castanea*, only two well determined species, and *Fagus*, 15; the *Corylaceæ*, admitted as an order, after Hartig and Doll; also the *Juglandaceæ*, the earliest botanical work of Casimir DeCandolle, whose separate memoir upon the subject we have already noticed with approbation; the *Myricaceæ* by the same promising hand (38 species of *Myrica*, including *Comptonia*, and Chapman's *Leitneria* of Florida); the *Platanaceæ*, and, in a note, *Liquidambar*, by Alphonse DeCandolle. The *Betulaceæ* and *Salicineæ* will follow in this volume, we believe at no distant date. A. G.

2. J. D. HOOKER: *Handbook of the New Zealand Flora.* Part I. London: Lovell Reeve & Co. 1864. pp. lxxviii, 392, 8vo.—This is a new member of the series of British Colonial Floras. The present part includes all the known Phænogamous plants and Ferns, not only of New Zealand, but of the outlying islands which may be regarded as of the New Zealand group, such us the Chatham and Kermadec islands on the one hand, and the Lord Auckland and Campbell's islands on the other. The Cryptogamous plants are to follow. The basis of the present work, of course, is Dr. Hooker's Flora of New Zealand and the Flora of Lord Auckland and Campbell's islands, making three large illustrated volumes of the Botany of the Antarctic Voyage under Capt. Ross,—volumes much too bulky and costly to subserve the main purpose of the Handbook, i. e., to be freely used by the colonists themselves. Moreover, large discoveries have been made since the Flora Novæ-Zelandiæ appeared, mainly in the middle island, adding fully one-third to the number of flowering plants previously known. The genera are here brought up to 303, the species to 935,—still exemplifying the insular paucity. Of these species 677 are peculiar to these islands; 222 are Australian, and 111 American. There are, besides, 51 Australian representative species, and 32 American representative species. Among the American identical species are *Myosurus aristatus*, of the Pacific United States and Chili, and our *Elatine Americana*, which has a wide range in the southern hemisphere, viz: in S. America, New Zealand, Feejee Islands, Australia, and Tasmania. Dr. Hooker remarks that few if any plants appear to have been collected in the Lord Auckland's Group by Commodore Wilkes's expedition. A single officer, Lieut. Holmes we think, passed a few hours on land, collecting, however, among other plants, a single specimen of an apparently new *Ranunculus* (*R. Aucklandicus*), which has been accidentally overlooked in the present work. A. G.

3. MARTIUS: *Flora Brasiliensis;* fasc. 36, 37, 38. fol. Dec. 1864.—The *Gesneraceæ*, by Dr. Hanstein, the curator (succeeding the late Dr. Klotszch) of the Berlin Herbarium; with eleven plates. The *Salsolaceæ* (*Chenopodiaceæ*), by Frof. Fenzl, of small importance in Brazil; with five plates, one of them illustrating *Chenopodium anthelminticum*, and one *Roubieva multifida. Magnoliaceæ, Winteraceæ* (retained, as likewise *Schizandreæ*, as ordinally distinct), *Ranunculaceæ, Menispermaceæ*, and *Berberideæ*, by the acute Dr. Eichler; twenty-six plates, one of them devoted to the anatomy of the wood of *Drimys Winteri*, and two to that of Menispermaceous stems, which the author appears to have discussed with much ability. A. G.

4. *The Journal of the Linnæan Society*, No. 31 (Dec. 1864), is especially rich in articles upon Dimorphism and even Trimorphism in plants, and upon the agency of insects in their fertilization. There is 1. *Notes on the Fecundation of Orchids, and their Morphology*, by the late Dr. Cruger, Director of the Botanical Garden, Trinidad; *Catasetum* and *Stanhopea* being the principal subjects, and the conclusions of Mr. Darwin being fully confirmed. 2. *Dimorphism in the Flowers of Monochoria vaginalis*, by Dr. Kirk. The additional kind of flower would seem to be somewhat after the fashion of that of *Utricularia clandestina*, and arranged for self-fertilization. 3. *On the Individual Sterility and Cross-Impregnation of certain species of Oncidium*, by Mr. John Scott. He shows by experiment "that the male element of *O. microchilum* will fertilize the female element of the two distinct species, *O. ornithorhynchum* and *O. divaricatum cupreum*, and yet be completely impotent upon its own female element; nevertheless, the susceptibility of the latter (female element) to fertilization is shown by its fertile unions with another individual of the same species, and likewise by a fertile union with an individual of a distinct species;" and the same is true of *O. microchilum*. 4. *Notes on the Sterility and Hybridization of certain species of Passiflora, Disemma, and Tacsonia*, by the same author. Species which, in cultivation, are perfectly sterile upon the application of the pollen to the pistil of the same individual, are readily fertilizable by the pollen of other individuals of the same or of an allied species, and their pollen is likewise potent upon such individuals; although in hybridization the influence frequently is not reciprocal. For instance, *Passiflora racemosa* may be fertilized by the pollen of *Tacsonia mollissima*, upon the ovules of which, conversely, the pollen of the *Passiflora* is utterly impotent. There are two Passionflowers, totally impotent when self fertilization is attempted, the pollen of one of which effects the development of the ovaries of the other, but never the seeds, while conversely even the ovary fails to develop. And there are two species of *Tacsonia*, the pollen of one of which causes the ovary and even the seed-coats of the other to develop, but never the embryo, while conversely the effect is sometimes the same, but generally nothing at all. Although general conclusions should be hesitatingly drawn from limited experiments upon cultivated plants, yet the known facts conspire to show that no sharp line is drawn in nature between fertility and sterility in crosses. 5. *On the Sexual Relations of the three forms of Lythrum Salicaria*, by Charles Darwin. Here we have the results of an investigation which Mr. Darwin has before

referred to. A curious case it is, and treated with the wonted sagacity and point of this prince of biological inquirers.

"In *Lythrum Salicaria* three plainly different forms occur: each of these is an hermaphrodite; each is distinct in its female organs from the other two forms; and each is furnished with two sets of stamens or males, differing from each other in appearance and function. Altogether, there are three females and three sets of males, all as distinct from each other as if they belonged to different species; and, if smaller functional differences are considered, there are five distinct sets of males. Two of the three hermaphrodites must co-exist, and the pollen be carried by insects reciprocally from one to the other, in order that either of the two should be fully fertile: but, unless all three forms co-exist, there will be waste of two sets of stamens, and the organization of the species as a whole will be imperfect. On the other hand, when all three hermaphrodites co-exist, and the pollen is carried from the one to the other, the scheme is perfect; there is no waste of pollen and no false co-adaptation. In short, Nature has ordained a most complex marriage-arrangement, namely, a triple union between three hermaphrodites,—each hermaphrodite being in its female organ quite distinct from the other two hermaphrodites, and partially distinct in its male organs, and each furnished with two sets of males."

One must study this instructive paper to see how neatly it is shown, "that only the longest stamens fully fertilize the longest pistil, the middle stamens the middle pistil, and the shortest stamens the shortest pistil. And now we can comprehend the meaning of the almost exact correspondence in length between the pistil of each form and the two half-dozen sets of stamens borne by the two other forms; for the stigma of each form is thus rubbed against the same spot of the insect's body which becomes most charged with the proper pollen." For the use which Mr. Darwin makes of this case, and the theoretical deductions drawn from a genus which presents trimorphic, dimorphic, and monomorphic species, the illustration of the advantage of trimorphism, and of the now established fact that sexual differences,—"thought to be the very touchstone of specific distinction,"—may characterise and keep separate the coexisting individuals of the same species in the same manner as they do those groups of individuals which we denominate species, we must refer to the memoir itself, not having space for a full abstract. Mr. Darwin, on raising from seed some individuals of our *Nesæa verticillata*, ascertained that this plant is also trimorphic. We commend it to the particular attention of any who may be disposed to prosecute farther such investigations, which, though requiring genius to originate, are easy to follow up, and almost inexhaustible in interest.

Of the remaining papers in this number of the Linnæan Journal, one is *On a Peloria and Semi-double Flower of Ophrys arenifera*, by Dr. Masters, to which a note is added, mentioning a nearly similar monstrosity of *Pogonia ophioglossoides*, (known to him from a description by Prof. Gray), collected by Rev. J. A. Paine, in a bog near Utica, N. Y. Several flowers were detected, last year, all sharing more or less of the peculiarity, viz: having three *labella* and the column resolved into small petaloid organs; the two accessory labella and a small petaloid body on the other side of the flower answering to the three suppressed stamens of the outer series, while two little filaments answer in position to the suppressed lateral stamens of the inner series. It is hoped that other specimens may be detected the ensuing summer, and preserved in spirit for more searching examination. In another paper, Dr. Hooker identifies *Pinus Peuce*, Griesb., of the mountains of Macedonia, with *P. excelsa* of the Himalayas. Mr. Mitten describes new *Musci* and *Hepaticæ* of Japan and the coast of China, and Prof. Oliver, new genera of plants of Western Tropical Africa.

The longest paper in No. 32 (February, 1865) is one by Dr. Masters, *On the Morphology and Anatomy of the Genus Restio*, Linn., *together with an Enumeration of the South African Species.* There is also an extract from a letter of Prof. Brewer to Sir Wm. Hooker, *On the Forests of Sequoia* (*Wellingtonia*) *gigantea of California*, correcting the popular impression that this magnificent tree is extremely local and in danger of extinction. A. G.

5. *Further remarks on larve budding;* by Dr. W. C. MINOR.—In a preceding number of this Journal (January, 1865) some notice was taken of a newly observed form of insect larve budding. The first number of the *Zeits. f. wiss. Zoölogie* for this year has a further account by Dr. Wagner, the original observer, which, beyond the interest of a detailed reply to questions from Dr. Siebold, contains some generalizations upon the phenomena of budding, which we give in brief. Dr. Wagner offers the following table of the grades between the simplest asexual and the most perfect sexual multiplication in the articulates.

I. Asexual spontaneous multiplication of the larve-nurse (Amme), with sexual generation of the developed animal. The germ is metamorphosed out of the fat or granular substance of the larve-nurse, and the animal has three or four transformations, in the forms of protoscolex, deuteroscolex, strobila and proglottis.—*Cestodes* and *Trematodes.*

II. Larves with sexual organs. The germs developed in these organs become individuals within the body of the larve-nurse, and are born alive, forming thus two metamorphoses.—*Aphides.*

III. Multiplication only in the perfect sexual animal:—*a.* In both males and females, but without sexual influence.—*Daphnidæ.* *b.* In one sex only, without sexual influence.—*Bees*, and some *Butterflies.* *c.* In one sex only, under the influence of fructification.

The author admits that nothing more than one point of view is attempted here, as deeper embryological studies will, by bringing out homologies and complications of structure, probably separate the higher and lower groups here put together. The case he has discovered in the Diptera seems to him to form a transition from the first to the second group. He refers to Leydig's comparison of the Aphis buds with the summer eggs of Daphnia, and of the true Aphis eggs with the winter eggs or ephippia of Daphnia, and extends the comparison to the larve buds and eggs here.

Upon the distinction between Parthenogenesis and Alternate genesis, he urges, that the former is a germination of buds in *special sexual organs*, though without fructification; while the latter is a *self-transformation*, also unfructified, of tissue into germs or buds, without any special organ for the transformation, and in a degree independent of the nurse, and hence spoken of as *spontaneous.* The first of the groups given above comes under the head of alternate genesis—Generationsweeksel —while the second, and in part the third, are instances of parthenogenesis. From this point of view, also, is the present case intermediate between the first and second groups.

He calls attention to the compensation, by the budding of the larve, for the limited egg-bearing capacity of the mature fly, and regards this compensational balance as usual in these cases. Also, considering the fat or granular substance of the larve-nurse to be directly transformed into these larves—a view confirmed, it will be remembered, by Dr. Meinert—he points to the hypertrophied eggs as a store of material for its production. That this granular substance is of a nutritive nature, is made altogether probable by its transference into the stomach through the Malphigian vessel, in cases of prolonged abstinence, and also by the presence of sugar in it; and he observes that the imperfect tracheal system and sluggish movements, for a time at least, of the larve-nurse tend to leave this store intact for the sole use of the buds.

Both in this, and the first paper of Dr. Wagner's, which we have now received, are numerous points of anatomical and physiological interest; such as the condensation in the mature fly of the separate ganglia of the larve, the valves at the posterior end of the dorsal heart—also figured by Dr. Pagenstecher—and which, as well as the glands along its side, supposed from analogy—in leeches—to form the blood corpuscles, we have ourselves seen.

It is perhaps not premature to state here that the writer has found a number of large oval germs in some minute larves observed lately, and he would urge the attention of American observers, who may be better situated than himself, to this inquiry; since, to judge from the difference in shape of the larve's head, these were not of the same genus.

6. *On the Hymenoptera of Cuba*, by E. T. Cresson. (From the Proceedings of the Entomological Society, Philadelphia, Jan. 1865. 200 pp. 8vo.)—The Entomological Society of Philadelphia has shown great activity since its formation, having issued many long and thoroughly labored memoirs. Mr. Cresson's work is based principally on the collection of Prof. F. Poey of Havana, purchased by Dr. T. B. Wilson of Philadelphia and presented to the Society, and also on specimens in the collection of Dr. J. Gundlach of Cuba. Most of the Chalcididæ, Prototrupidæ and Formicidæ are reserved for future papers.

7. *Synopsis of the Bombycidæ of the United States*, by A. S. Packard, Jr. Part II. 66 pp. 8vo. From the Proceedings of the Entomological Society, Philadelphia. Nov. 1864.

IV. ASTRONOMY AND METEOROLOGY.

1. *Correction of D. Trowbridge's article on the Nebular Hypothesis*; by the Author.—In my article on the Nebular Hypothesis, Art. xlvi, and note 41 (this Jour., [2], xxxix, 31, 32), I have committed an error in my reference to Prof. Ferrel's article on the retarding effect of the sun and moon on the earth's rotation, in Gould's Astronomical Journal. One part was owing to a careless misconception, and the other to a

misprint in his article. The retarding effect is given in his article in angular velocity, and not in absolute velocity as I conceived (though it was so given that one might easily make the mistake). The effect of the earth on the moon is 5625 times as great, and not 562½ times, as printed. My general reasoning, however, as applied to the nebular hypothesis, is not affected by the error which I made. My error was in numbers (I read Prof. F.'s article several years ago when it was published) and not in principles.

Hector, N. Y., March 23d, 1865.

2. *Cambridge Observatory in* 1864. Extract from the Annual Report of the Observatory Committee to the Board of Overseers of Harvard College, (made previous to the decease of the Director, G. P. Bond).—The labors of the year have been chiefly directed to the continuation of the zone observations, the examination of nebulæ, observations on the asteroids, on changeable stars, and on the two telescopic comets of the year. During the last winter and the early part of the spring, observations of minute inquiry were made on Orion on fifty-nine nights, resulting in the discovery of many new features, as well as corroborating characteristics heretofore published. Among the discoveries were six variable stars in the neighborhood of the Trapezium.

In the prosecution of the zone observations, Mr. Safford obtained the position of four thousand seven hundred stars by the great equatorial, besides performing the necessary reductions and computations involved. Very few persons, who have not had the privilege of examining the great mass of manuscripts already accumulated, can form any conception of the enormous labor required in this valuable undertaking.

The director repeats his great obligations to Mr. Safford for his devotedness to all the interests of the Observatory, and the skill and fidelity with which he has executed the several parts assigned him.

3. *Extracts from the Address of* W. De La Rue, *President of the Astronomical Society of London, connected with the awarding of the Society's medal to* Mr. G. P. Bond.—The announcement that the Royal Astronomical Society has awarded its gold medal to Professor Bond will be hailed with universal satisfaction by those interested in the science which he has so ably cultivated. At the anniversary meeting of the Society, held on Friday, Mr. Warren De La Rue, under whose auspices the Society has taken such a large stride in its path of usefulness, according to custom, accompanied the announcement by an address, which should add greatly to the pleasure of the medallist, so warmly does it appreciate the work done by the transatlantic astronomer, who has for so long had the finest telescope in the world to use, and has used it so well. The address is too long to be reproduced *in extenso*, but we give several extracts from it, reserving our report of the other business done at the meeting for a future occasion.

Mr. De La Rue commenced by observing, that 'when father and son have worked zealously together in the same direction, and especially in those cases where the son has succeeded to the position created and held for a number of years by the father, it is extremely difficult to draw any line of demarcation, from which to date the commencement of the son's independent career, or the cessation of the momentum imparted to his course of activity by the father. This is especially the case with respect to the late Professor W. C. Bond and his son, Professor George Phillips Bond, to whom the council have awarded the highest mark of distinction this society can confer.

In the estimate formed of his scientific work, the professional astronomer is generally placed somewhat at a disadvantage in comparison with the amateur. In the case of the latter, the whole of his work is weighed in the balance, while, in that of the former, large deductions are made from his labors as belonging properly to the duties of his official position. An official astronomer, in this age of zealous activity and speedy publication, inaugurated I believe by the example of the present Director of our own Royal Observatory, may, consequently, produce much good and original work without his name coming with due prominence before his peers, so long as that work falls, or rather appears to fall, within the range of his official duties. . . .

Unquestionable evidence that Prof. G. P. Bond has done more for our science than even a scrupulous discharge of his duty demanded, was given by the appearance of the 'Annals of the Astronomical Observatory of Harvard College,' vol. iii, 1862. At first sight, indeed, that volume might appear to be nothing beyond a record of excellent but, nevertheless, official work; when, however, I shall presently come to speak of its details, it will, I doubt not, be conceded that it belongs in a great measure to the category of private labor independent of official duty.'

After stating that Prof. G. P. Bond had been a most active contributor to astronomical discovery and methods for a long series of years, and that, besides his great and indeed unique work on the comet of 1858, an amount of good work had been accomplished by him which fully entitled him to the distinction now conferred upon him, Mr. De La Rue remarked that 'his earlier exertions appear to have been directed to the discovery of comets and the computation of their orbits, and in the Report of the Committee of Harvard College he is mentioned as having detected independently eleven of those bodies. It is not surprising, therefore, that so zealous an observer of these strange visitors, about the nature of which so little is known, should have been one of the most assiduous observers of so splendid a comet as that of Donati. It was also a natural consequence that Professor Bond should have desired to compare his own results with those of other observers, but it was by no means a necessary sequence that he should have entailed upon himself the enormous labor of collecting, systematically arranging, and reducing, the whole of the data resulting from this comparison.

The circumstances attending the apparition of the comet of 1858 were peculiarly favorable; its early discovery by Dr. Donati, while yet a faint telescopic object, and the prediction of its great brilliancy and splendor, having had the effect of directing the attention of a great number of astronomers to this remarkable body. Hence, in the history of cometary observations, there has never been collected so large a mass of observations as in the case of Donati's comet, the absence of moonlight, the short duration of twilight, and the remarkable continuance of clear weather during the most important part of the comet's apparition, largely contributing to this result. The darkness of the sky, which served as a background to the comet, was peculiarly favorable to the delineation of the fainter outlines and peculiar features of this splendid visitor, which was visible to the naked eye from August 19 to December 9, one hundred and twelve days. The whole period of visibility in the telescope extended from June 2, 1858 to March 4, 1859, an interval of two hundred and seventy-five days. One of the most successful workers in recording the phenomena of this comet was Professor G. P. Bond, and the sketches and drawings made at the Observatory of Harvard College form the main contribution to the splendid graphic illustrations which are a remarkable feature of this altogether remarkable production. The pains taken to insure a truthful representation of this comet of 1858, both in its eye- and telescope-features, few are better able than myself to appreciate; and I am able, from my own practical experience, to state that the success which has attended these efforts has been deservedly won by battling with greater difficulties than would probably be imagined by the uninitiated.'

'The comprehensiveness of Professor Bond's work will be at once recognized by an enumeration of the various sections into which it is divided. They are as follows:—

I. Figure and Position of the Tail; II. Observations upon the Secondary Tails; III. Reduction of Observations upon the Figure and Position of the Tail; IV. Probable Errors of Observations of the Tail; V. On the Deflection of the Tail; VI. Columnar Structure of the Tail; VII. Reduction of Observations on the Secondary Tails; VIII. The Nucleus and Envelopes; IX. On the Outline of the Head of the Comet; X. On the Branches and Central Darkness of the Tail; XI. The Nucleus; XII. The Envelopes; XIII. The outer faint Veil; XIV. On the Direction and Initial Axis of the Tail; XV. Summary of the Contents of the Volume.'

'The sections I to VII relate to the figure, dimensions, and positions of the tail, from its first appearance on August 14, 1858, seventy-three days after its first discovery, when it was seen at Copenhagen by D'Arrest and at Vienna by Homstein, until the last recorded observation at Santiago, in Chili, by Moesta, on February 7, 1859. Arranged alphabetically under the same date, are the names of the several observers, sixty-seven in number, whose statements are given verbatim in the language in which they were written; but the value of this record is greatly enhanced by the occasional remarks of the author, who draws the reader's attention to points of special interest, and thus brings under notice the changes which actually occurred, as well as those which did not take place although anticipated from previous hypotheses. The reader is informed, in the introductory chapter, that from June 2 to September 8, the earth was on the north side of the plane of the orbit, and on the latter day crossed the line of nodes, giving an opportunity for observations on the figure of the tail projected on a plane at right angles with the comet's orbit. After the middle of September the tail was presented in nearly its full-length proportions; within a day or two of the perihelion passage on September 30, the axis of the tail was brought to a position at right angles to the line of vision, and, ten or twelve days later, when the comet had reached its least distance from the earth, its profile was almost precisely that of a section in the plane of the orbit. Professor Bond remarks, that where so little is known, *à priori*, respecting the actual figure, there is an obvious advantage to be derived from these accidental circumstances of its position, by which the influence of perspective foreshortening is, in a great measure, eliminated.'

'In order to recognize and explain any errors or ambiguities in the observations themselves, a provisional chart was constructed, showing the path of the comet and the position of the tail among the neighboring constellations. This chart naturally gave confidence and insured accuracy in the subsequent discussion of the observations, which are duly 'weighted' according to the dependence which can be placed upon them.'

'Lastly, the normal outlines of the tails are given in a series of charts which represent the final results deduced from the whole series of observations between September 16th and October 17th, referred to the common epoch for each date of 7^h mean solar time at the Observatory of Harvard College. The figure of the tail is preserved free from any considerable distortion by the method adopted; namely, by projection on a tangent cylinder, the position of which, in regard to the path of the comet, was selected with that view.'

After having given an idea of the comprehensiveness of the first seven sections of Professor Bond's work on the comet of 1858, the speaker referred to those parts which treat of the telescopic observations of the nucleus and envelopes—which are characterized by the same care, skill, and resource, though the difficulties of dealing with the discrepancies must have been enormously greater.

'Professor Bond gives in his work a plate representing the drawings or engravings of the nucleus and envelopes in the form they reached him. A cursory inspection of this collection will suffice to show how great is the diversity of portraiture of the same object, even when made at about the same period of absolute time.'

'The form of the head of the comet received considerable attention; the result was such as to show that its outline did not accord with a parabola, but that its contour was nearly that of a catenary curve. Moreover, advantage was taken by Professor Bond, of the apparition of Comet III, 1860, and more recently, of the great Comet of 1861, to test some of the hypotheses discussed in reference to the Comet of 1858, notably the phenomena of the successive throwing off of a series of envelopes from the nucleus, and the diminution of their elevation velocities as they receded from it. A recent careful revision of these phenomena has, according to the report of Professor Bond for 1864, completely confirmed the results previously announced.

Before dismissing the consideration of the volume on the comet of 1858, Mr. De La Rue added a few words in honor of Mr. Josiah Quincy, of Boston, who defrayed the expenses of the letter-press of the volume, and of the twenty-four gentlemen of Boston, Waterton, Cambridge, and Salem, who subscribed the funds necessary to cover the cost of the unrivalled engravings with which it is illustrated. It is most creditable to these gentlemen that they so fully appreciated the importance of doing ample justice to the labors of the Director of the Observatory of Harvard College, and enabled him to lay before the public such a written and graphic record as could not fail to attract the attention and obtain the confidence of the astronomical world. It should be remembered that the Observatory in question is not an institution endowed with ample funds to meet all its requirements, but that, on the contrary, appeals for aid to patriotic citizens have frequently been necessary, and have always been generously responded to whenever any object of scientific importance could be thus advanced.'

Passing from this splendid addition to our knowledge of the physical phenomena of comets, Mr. De La Rue next referred to a very important step made by Professor Bond in connection with the theory of planetary perturbation. 'It is well known that when the excentricity and inclination of the orbit of the disturbed planet are considerable, the problem of three bodies represents difficulties which have baffled the efforts of the most consummate mathematicians since the time of Newton. Fortunately, when the question relates to the computation of the perturbations of the principal bodies of the planetary system, such difficulties are not found to exist.' 'But for comets, which revolve in orbits of every degree of excentricity, and inclined to the ecliptic at angles of all degrees of magnitude, a special method is necessary. The chief merit of improving and applying this method of computation must, in all fairness, be assigned to Professor Encke. Nevertheless, Encke and other German astronomers were to a very great extent anticipated by Professor G. P. Bond, whose suggestions were made so early as the month of May, 1849, in a paper published in vol. iv. of the 'Memoirs of the American Academy of Arts and Sciences,' whereas Encke's labors bear date from 1851.' 'It is but right, however, to add that at the time when Professor Encke communicated his researches to the Berlin Academy he was totally unaware of Professor Bond's previously published paper on the same subject.'

Mr. De La Rue next adverted to the magnificent monographs of Saturn in 'The Annals of the Astronomical Observatory of Harvard College,' vol. ii, part i. 'Although the chief merit of that elaborate treatise is due to Prof. W. C. Bond, a reference to the initials of the various observers brings out prominently the great part taken by his son, G. P. B. being affixed to 'two very important series of observations connected with the physical history of that interesting planet, namely, in the first place those which in 1848 led to his discovery of the Eighth Satellite, Hyperion, independently of and simultaneously with Mr. Lassell; and, in the second place, observations of a peculiar luminous appearance within the then so-called inner ring, associated with a dusky belt crossing the planet in such a position that it could not be mistaken for the shadow of the bright rings on the ball, which shadow was seen at the same time in its proper place. I need scarcely say that it was the dusky ring of Saturn that was then for the first time noticed, though it was only subsequently recognized as such when, on Nov. 15, 1850, Mr. Tuttle, of the Harvard Observatory, suggested that the existence of a new ring of a dusky character would explain the phenomena. Mr. Dawes subsequently, namely, on the 25th and 29th of the same month, discovered the same ring independently, with a refractor by Merz, of 6½ inches aperture.' The address concluded as follows:—

'I believe I am right in stating that the only orbit of Hyperion yet published was computed by Professor G. P. Bond, from his own observations. He has also published an analytical investigation of the question of the stability of Saturn's rings, in which he arrived at the unexpected conclusion that they are in a fluid state.[1]

Among other titles to our esteem for him as an astronomer must be mentioned his share in the adaptation of the great Refractor of Harvard College to the observations of very small stars in zones of declination, the first-fruits of the plan being a catalogue of 5,500 stars near the Equator, forming vol. i. part ii. of the above named "Annals," the observations having been made by himself and Mr. Tuttle.

This plan, it will be remembered, consists partly in recording, by the electric method, the times of transit of stars as they cross certain vertical lines drawn on a piece of transparent mica attached to the diaphragm of the spider-line micrometer. The electric recording apparatus is that originally contrived by the late W. C. Bond and his two sons, in which the movement of the recording barrel is regulated by their well-known spring-governor. On the piece of transparent mica exhibiting the transit lines, is also drawn a series of horizontal lines, 10″ of declination apart, grouped into threes and sixes, to indicate half minutes and minutes, the former denoted by making every third line somewhat longer than the adjoining ones, and the latter by causing every sixth line to extend quite across the slip of mica. By means of a cross-light illumination the lines are made to appear white on a dark ground. "The observer, with the aid of an assistant, whose office it is to record the declinations, magnitudes, &c., has it in his power to give the elements of position and magnitude of each star at the rate of seven per minute, the average frequency of observations being only two in each minute." Since Professor G. P. Bond's appointment to the direction of Harvard Observatory in 1859, these zone observations have been diligently continued; and in his report for 1864 it is stated that the region between + 1° 00′ and 1° 10″ had been nearly completed, and that great progress had been made in the zones between 1° 10′ and 1° 20′.

It is only necessary to read the Reports of the "Committee of the Overseers" of Harvard College, and the accompanying Reports by the Director of the Observatory, to show that the same zeal animates Professor G. P. Bond that was so strongly evinced by his father. The vast amount of work accomplished in the way of observation, reduction of the results, and their publication, is truly surprising, for we must in forming an estimate always bear in mind that the Observatory of Harvard College has very small means at its disposal, in comparison with the magnitude of its undertakings.

There is one claim to recognition which I, of all persons, must not pass over without notice—namely, the first application of photography to astronomical observations; for it was my seeing in the exhibition of 1851 a lunar photograph, which emanated from this Observatory, that stimulated me to undertake experiments in that direction. The first application of photography to the delineation of our satellite and of some of the fixed stars I had attributed to Prof. W. C. Bond, in connection with Messrs. Whipple and Black, of Boston; but I am informed that it originated with G. P. Bond, to whom, therefore, in conjunction with these last-named gentlemen, the merit of this important step must be assigned.

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[1] Presented to the American Academy of Arts and Sciences, April 15, 1851.

Professor Bond's latest and yet unfinished, work, is that upon the Nebula of *Orion*, which object is also engaging the attention of Lord Rosse. Professor Bond has already sent to England a proof from a steel plate engraved from his drawing; he has catalogued the stars in and around the nebula with more minuteness than the Russian astronomers; and he has been so fortunate as to discover in the nebula a grand feature which has hitherto escaped the attention of astronomers. This new feature is a great re-entering loop of nebulous matter extending around nearly the whole of the previously known portion, and enclosing, as with a nebulous wall, a large space exterior to the well-known figure. It was discovered by employing with the 22-feet refractor of 15 inches aperture, an eye-piece magnifying 90 times, with a field of 30′; by this arrangement it was seen with great distinctness. It is well known how clearly the low powers of comet-seekers bring out the faint details of comets; and the conversion, so to speak, of the Harvard Refractor into a huge comet-seeker has led to this interesting discovery.

In 1851, Prof. G. P. Bond visited Europe, and his reception at the principal astronomical establishments, and especially at the classical Observatory of Pulkowa, as detailed in the Annals of Harvard College Observatory, vol. i. part i.,[2] may perhaps be regarded as evincing something more than the ordinary courteous welcome due to a stranger. In 1863 he again visited Europe, when I had the pleasure of making his acquaintance, and I am convinced that on this, as on the occasion of his previous visit, Professor G. P. Bond not only gained many personal friends, but also the high opinion and respect of those astronomers with whom he came into contact.'—From *The Reader*, *Feb.* 11, 1865; but somewhat altered and extended after comparison with the publication of the Address in the *Monthly Notices of the Astr. Soc.*, vol. xxv, Feb. 10, 1865, p. 125, received after these pages were in type.

4. *Comet* V, 1864.—A comet was discovered by Bruhns, at Leipzic, on the 30th of Dec. 1864. The following elements were computed by Engelmann, from observations of Dec. 30th, Jan. 3d, and Jan 21st.

$T = 1864$, Dec. $27{\cdot}76616$, $i = 17^\circ\ 7'\ 13''{\cdot}7$,
$\pi = 162^\circ\ 21'\ 55''{\cdot}1$, $\log q = 0{\cdot}047095$,
$\Omega = 340^\circ\ 53'\ 52''{\cdot}6$, Motion retrograde.

5. *Duration of the flight of shooting stars.*—Dr. JULES SCHMIDT, director of the Observatory at Athens, has communicated to Mr. Haidinger some of the results of his observations during the last eight years upon shooting stars. He has recorded the estimated duration of flight of 1357 meteors, out of about 16,000 seen. The mean duration of those of different colors was, of 846 white shooting stars, $0^{s}{\cdot}709$; of 361 yellow, $0^{s}{\cdot}947$; of 101 red, $1^{s}{\cdot}787$; and of 49 green ones, $2^{s}{\cdot}685$. The mean of all was $0^{s}{\cdot}925$. This is somewhat larger than the mean duration obtained on page 203 of this volume. Dr. Schmidt having been accustomed to estimate small intervals of time in astronomical observations, his estimates deserve not a little confidence. We trust that he will classify them according to the hour of the night. His earlier observations were made mostly in the evening. The times of the observations of this series are not specified. We believe that there is a decrease in

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[2] Appendix, pp. 158–60, inclusive.

the mean duration in the later hours of the night. Such decrease is distinctly shown by Wartmann's observations in 1838. H. A. N.

6. *Heights of Auroral Arches.*—Mr. B. V. Marsh has obtained data for computing the altitudes of three auroral arches. A fine arch was seen early on the evening of the 16th of January, 1865, and from observations at Germantown, Pa. and Brunswick, Me., the computed altitude is 97 miles. A second arch on the 20th of February was observed at Germantown, Newburyport, Mass., and Brunswick. The Brunswick and Newburyport observations give an altitude of 67 miles; those of Germantown and Newburyport 92 miles. The mean of the two determinations is 79½ miles. A third arch on the 21st of February, observed at Boston and Philadelphia, had the computed altitude of 57 miles. The mean height of the three arches was therefore 78 miles. The data were not very exact in either case, but taken together they are believed to furnish reliable approximations. H. A. N.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Agassiz Expedition to South America.*—On the 29th of March, Prof. Agassiz, with a large corps of assistants, sailed in the steamer Colorado for Rio Janeiro, on an exploring tour in South America. His corps consists of O. H. St.John and C. F. Hartt to collect fossils and to aid in geological exploration, J. G. Anthony to collect mollusks, J. A. Allen to collect birds and mammals, G. Sceva to make skeletons of mammals, birds, the large reptiles and fishes, and Mr. Burkhardt to make drawings. Prof. Agassiz will devote himself, with native and such other assistance as he may obtain, to the collection of marine invertebrates, yet will have, for his main object, the study of the embryology of some of the remarkable fishes of the Amazon, and investigations with regard to the drift phenomena, or ancient glacial action, in the Andes.

The party is accompanied also by Dr. B. E. Cotting as surgeon, with the wives of Prof. Agassiz and Dr. Cotting, a son of Mr. N. Thayer of Boston, and a son of Mr. S. G. Ward of that city.

The expedition goes first to Rio Janeiro, whence the geological assistants will journey by land north to the Amazon, while the rest of the party, after completing investigations there, will take vessel for the same river. Prof. Agassiz and party will then ascend the Amazon, to the Andes, and, finally, after explorations in the mountains, descend to Lima.

Professor Agassiz at first intended only a visit to Brazil for his health, and proposed to take along one or two assistants to aid him in making collections for the Museum of which he is Director at Cambridge. On mentioning his plan to Mr. Nathanael Thayer, this generous patron of science at once said "Agassiz, go home, find six assistants, and I will pay the bill." The Pacific Mail Steamship Co., hearing early of the projected tour of exploration, immediately tendered to Professor Agassiz and his wife free passage to Rio; and, afterward, on learning of Mr. Thayer's munificent proposition, Mr. McLane, in the name of the Director of the Company, offered to the whole party free passage in the new steamer Colorado, about sailing for Panama *via* Cape Horn. The arrangements were soon completed, and within three weeks after Mr.

Thayer's promise was made, the expedition left New York in the Colorado.

The Secretary of the Navy has given Prof. Agassiz a letter addressed to all officers of the navy whom they may meet, in order to secure for them free transportation when desired. He offered also a government vessel to take them to their place of destination in South America, but they were already provided, through the liberality above mentioned. Every facility may be looked for also from the Emperor of Brazil, who has, for some time past, manifested great interest in the welfare of the Museum of Comparative Zoology at Cambridge.

Great results may be expected from an expedition under such a leader, so ably supported and so well equipped. The explorations will be mainly inland, and therefore the richer in novelties to science.

2. *Temperature of the climate near large bodies of water.*—Mr. J. S. Lippincott, of Haddonfield, N. J., in an article published in the Second Annual Report of the West Jersey Fruit-Growers Association (1864–5, Philadelphia), attributes the milder climate near the great lakes and the ocean to the atmospheric humidity occasioned by the proximity of water, referring to the principle established by Tyndall that moisture in the air absorbs the heat received from the earth and retains it about its surface, preventing thus its escape into space, and to his views on the subject. He explains in this way why western New Jersey is much less favorable for vineyards than the vicinity of the great lakes or the lakes of New York, situated farther to the north.

3. *Carinthian Lake-habitations.*—Prof. Hochstetter has found evidences of lake-habitations on piles in four of the lakes of Carinthia; namely, those of Wörd, Keutschach, Rauschelen, and Osseach. From the lake of Keutschach, the only one yet particularly investigated, numerous black potsherds, pieces of half-burnt clay, half-carbonized fragments, a whetstone, a portion of a stag's horn, are among the relics obtained.—*Reader, Dec.* 3.

4. *A new meteorite from Arkansas.*—Prof. J. Lawrence Smith has received a portion of a new meteorite from Arkansas, consisting of mixed iron and stony matter, which he has under investigation.—*Letter to G. J. Brush*, dated *Louisville, Ky., Apr.* 24.

5. *Cancerine.*—A fertilizer, named *Cancerine*, is made on the coast of New Jersey, out of *King-crabs*, a crustacean otherwise called "*Horse-shoe*," and in science *Limulus*. They are dried and ground; and, thus prepared, the material is said to be worth half as much as Peruvian guano.—*Prof. G. H. Cook's Report on the Geol. Survey of N. Jersey*, 1864.

6. *Large mass of Amber from India.*—Sir David Brewster has described a mass of amber from India weighing upward of two pounds. It is traversed by veins of carbonate of lime.—*Proc. Roy. Soc. Edinb.*, Jan. 16.

7. *Earthquake at Buffalo, N. Y.*—On the 29th of January last, at 4 A. M., there was a shock of an earthquake at Buffalo.

8. *Louis Saemann.*—The Comptoir Minéralogique et Paléontologique—or establishment for the Sale of Minerals, Rocks and Fossils—of Louis Saemann, in Paris, has been removed to Rue de Mézières, No. 6, where he stands ready to supply on reasonable terms, all wishing to buy, or complete, mineral, lithological, or paleontological cabinets.

9. *Dr. A. Krantz.*—The 7th edition of the Catalogue of minerals, rocks, fossils, casts and models for sale at the large and well known house of Dr. Krantz, at Bonn, in Prussia on the Rhine, has recently been issued, and exhibits great completeness in his stock in all departments, and favorable prices for purchasers. Besides the various other collections, we observe one of 114 models of occurring crystalline forms of various minerals, in wood, well labelled, with reference to standard works, for 16 thalers; and another of 675 forms, for only 120 thalers (the thaler being equivalent to about 75 cents).

OBITUARY.—THOMAS B. WILSON.—The scientific world has sustained a severe loss in the death of Dr. Thos. B. Wilson, the late President of the Academy of Natural Sciences, in this city, which took place on the 15th of March last, at his late residence in Newark, Delaware. Dr. Wilson, who was a native of Philadelphia, has for many years devoted himself to the encouragement and promotion of zoological science, especially in connection with the Academy of Natural Sciences, an institution which, mainly by his energy, ability and princely liberality, has been raised from comparative mediocrity to an equality with the leading kindred institutions of the Old World. The superb collection of Birds, which ranks as the third in importance in the world, and the invaluable Library of the Academy, are but a partial evidence of Dr. Wilson's unostentatious munificence. Every department of the institution bears his mark and will feel his loss. He has also contributed largely of late to the Entomological Society at Philadelphia. Although his residence had been removed to Newark, Delaware, half of his time was regularly spent in his native city and occupied with his favorite pursuits. It is difficult to estimate the value of such men to the cause of science, or the loss which a community sustains when their labors are cut short.—*Daily Evening Bulletin, Philadelphia, March* 21.

GEORGE H. EMERSON.—Mr. George H. Emerson, a young chemist of much promise, originally from the city of Hartford, Ct., died at Greenfield, Mass., after a long and painful illness, on the 28th of December last, at the age of twenty-seven. Mr. Emerson is the author of a memoir "On crystals and precipitates in Blowpipe Beads," presented to the Boston Society of Natural History, an abstract of which was published in volume xxxvii of this Journal (p. 414). Having discovered that nearly all those earths, or metallic oxyds, which produce an opaque bead on flaming, give rise, with proper manipulations, to definite and characteristic crystals, he examined, with great labor and care, a large variety of substances, and carried his investigations so far as to determine and describe the appearance of the crystals or precipitates in different fluxes. His researches—which he regarded as but just begun,—promised to contribute much to the progress of the science of Blowpipe analysis. But he was compelled to discontinue them by his failing health. He had been a student of Chemistry in Prof. Cooke's laboratory at Cambridge but one year when he obtained his interesting results. But the enthusiasm and activity with which he applied himself would have severely tried a stronger constitution than his. Having sought to improve his health by a journey to Florida without avail, he died at Greenfield a few months after his return.

Mr. Emerson belonged to that rare class of men, who perpetuate their good works, and after death remain useful to humanity. By his will he gave his whole estate to the endowment of four scholarships for the relief of needy and meritorious graduates of the Lawrence Scientific School of Harvard College. Such a grand and simple life of devotion needs no drapery of euphonious words. He loved science, and gave for her advancement all he possessed, even his young life. A. H.

WILLIAM J. WALKER.—Dr. William Johnson Walker died on the 2nd of April last, at Newport, Rhode Island, in his 76th year. He had been eminently skilful in surgery and the treatment of disease, but quitted the practice of his profession about twenty years since. He was a liberal patron of science and of education, and not less munificent in his gifts than in his bequests. During his later years his gifts amount to four hundred thousand dollars, of which the Boston Natural History Society had a share. In his will he bequeaths one million of dollars (out of an estate of about one and a quarter millions) in equal parts to the Boston Society of Natural History, Amherst College, Tufts College and the Institute of Technology at Boston.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Naturalists' Directory.* From the Proceedings of the Essex Institute, Salem, Mass. F. W. PUTNAM, Editor.—Advance sheets of Part I of this Directory have been received. It forms part of the April number of the Proceedings, but has separate paging so as to be bound by itself. This first part contains the names and addresses of the naturalists of North America and the West Indies, together with their special departments of science, and other useful information. The publication of this work is to be continued quarterly until completed. The next number will contain the names of foreign naturalists, with their addresses and special branches of study, so far as they can be ascertained. In the part already printed the names have been arranged alphabetically for greater convenience, but in future numbers it is proposed to arrange those of each country according to their special departments.

The utility and convenience of a work of this kind must be obvious to every one interested in Natural History, and the first number gives assurance, by the great labor and care that has been bestowed upon it, that the entire work will, notwithstanding the inherent difficulties of the task, be rapidly and successfully completed, and thus supply a want that has long been felt. A. E. V.

2. *The Social Science Review: a Quarterly Journal of Political Economy and Statistics;* ALEXANDER DELMAR and SIMON STERN, Editors. Vol. I, No. 1, January, 1865. 96 pp. 8vo. New York.—The subjects discussed in this first number of the Social Science Review are—Government; Mr. Fessenden's Report; Herbert Spencer on Social Statics, &c.; Mr. About's Progrès; The limits of Political Economy.

3. *The Preparation and Mounting of Microscopic Objects;* by THOMAS DAVIES. 144 pp., 12mo. New York: Wm. Wood & Co., 61 Walker street.—In accordance with the author's aim, as expressed in his Preface, this little manual supplies a want long felt, and supplies it well,

giving the methods of preparing and mounting (and to some extent, also, of collecting) specimens for the microscope that are in general use, along with the results of his own experience; and with such explanations of the rudiments of the art as may be required by a beginner, as well as those details that the advanced student might need. The subjects of the chapters are: I. Apparatus; II. Preparation and mounting of objects "dry;" III. Mounting in Canada Balsam; IV. Preservative liquids; V. Sections, and how to cut them, with some remarks on dissection; VI. Injection; VII. Miscellaneous information on objects of interest for the microscope.

4. *Trübner's American and Oriental Record.*—The Record, published in London by Messrs. Trübner & Co. (60 Paternoster Row), of which the first number made its appearance on the 16th of March last, is to be "a monthly register of the most important works published in North and South America, India and China and the British Colonies, with the occasional addition of notes on German, Dutch, Danish, French, Italian, Spanish, Portuguese, and Russian books." Its object is stated to be two-fold: *first*, to form a medium of communication between American and Oriental authors and publishers and the English public; and, *secondly*, to make American and Oriental works better known in Europe. It has therefore a special interest to American authors and publishers, and to the American people generally. Coöperation from America is asked for, and especially early intimation of publications in contemplation, with a mention of such particulars in each case as the trade and the reading public demand. The house of Trübner & Co. has long dealt largely in American works, and done much toward extending their circulation through Great Britain and Europe. The first number of the Record runs to 24 pages small 4to. Price 6*d.*

5. *American Journal of Conchology;* edited by G. W. TRYON, Jr.—The first and second numbers of the American Journal of Conchology have appeared, and by their contents promise much for the progress of the science of both recent and fossil shells. The numbers of this quarterly contain 96 pages each; the first is enriched by 9 plates of shells, and the second by 8, several of them colored. The price per number is three dollars, and per year ten dollars. These numbers contain articles on Tertiary fossils by T. A. Conrad; and on recent shells by S. S. Haldeman, T. Bland, W. G. Binney, W. Stimpson, C. M. Wheatley, A. D. Brown, J. G. Anthony, and the Editor, besides "Reviews and Summary of Conchological Publications" and "Scientific Intelligence."

6. *Woodward's Country Homes;* by G. E. & F. W. WOODWARD, Architects. 166 pp., 12mo. New York: G. E. & F. W. Woodward, 37 Park Row, office of the Horticulturist.—This neat little volume is illustrated by numerous cuts representing dwelling houses of various tasteful styles, and plans for their construction, and is well calculated to distribute through the country, taste in domestic architecture and some knowledge of the ways of exhibiting it.

7. *The New York Medical Journal.* Vol. I, No. 1, April, 1865. 88 pp. 8vo. Miller & Mathews, New York and J. B. Lippincott & Co., Philadelphia. $5 per year.—This new monthly is to be sustained by the highest medical and surgical talent of the country, and promises to be the leading Medical Journal.

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