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## SCIENTIFIC INTELLIGENCE.

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## NUMBER CXIV.



## SCIENTIFIC INTELLIGENCE.

## ERRATA.

P. 128, bottom line, for "her," read "other."—P. 192, l. 7 from bottom, for "plain," read "plane."—P. 193, l. 13 from top, for "48,000," read "4,800."—P. 291. l. 25 from top, for "Muscovum," read "Muscorum."—P. 299, l. 20 from bottom, for "fattening," read "fertilizing."—P. 427, for "Cotapaxi," read "Cotopaxi."

THE

AMERICAN

# JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

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## ART. I.—*On Certain Harmonies of the Solar System;* by Professor DANIEL KIRKWOOD, Indiana State University.

### I.—THE ROTATIONS OF THE PLANETS.

IN 1849, a very simple formula connecting the rotations of the planets, and harmonizing in a remarkable manner with the elements of the solar system so far as known, was communicated to the American Association for the Advancement of Science. This formula represented the rotation-period of Uranus, which had never been observed, as greater than that of any other planet. Hence a determination of the true rotary velocity would have been regarded as a test of the new harmony. The number of powerful instruments had then been recently increased, and Uranus was approaching a more favorable position for accurate examination. Little doubt was therefore entertained that the claims of this planetary law would soon be decided by telescopic discoveries. Inasmuch, however, as no definite results have yet been obtained, a brief review of the facts may not be destitute of interest.

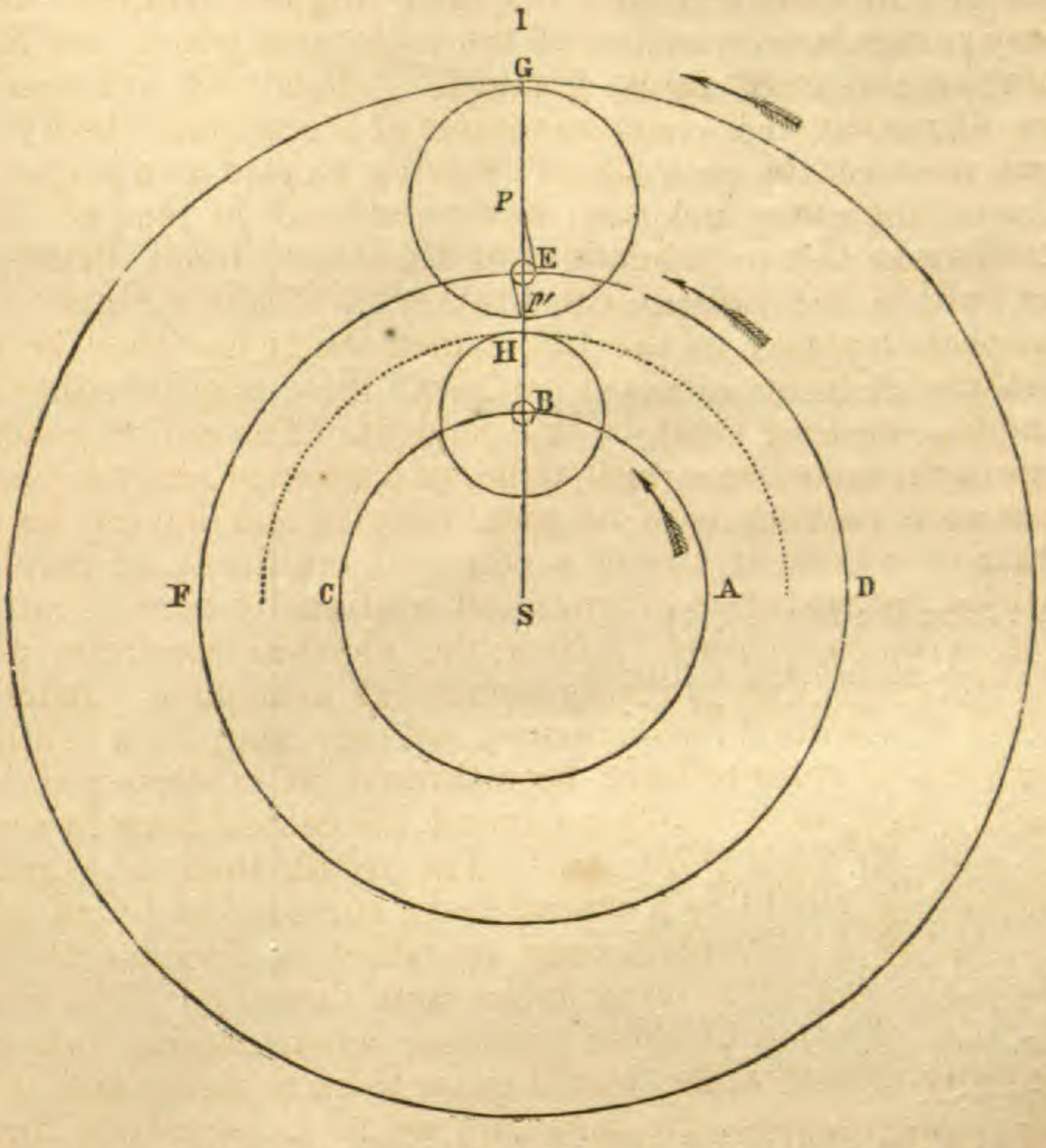
If the solar system has resulted, as was supposed by Laplace, from the gradual contraction of a rotating nebulous spheroid, what was probably the physical constitution of the abandoned equatorial rings in the first stages of their separate existence? The celebrated author of the nebular hypothesis supposed each of the rings in which the several planets originated to have revolved, during an indefinite period before its dissolution, as one continuous mass. "These zones," he remarks, "ought, according to all probability, to form by their condensation and by the mutual attraction of their particles, several concentric rings of vapor circulating about the sun. The mutual friction of the molecules of each ring ought to accelerate some and retard others, until they all had acquired the same angular motion. Consequently, the real velocities of the molecules which are *farther* from the sun ought to be greatest."[1] This view has also been generally adopted by later advocates of the nebular theory. Instead, however, of each planet's having existed as a single, continuous ring, may not each have consisted of many? Such, according to the investigations of Professors Bond, Peirce, and Maxwell, is the present constitution of Saturn's rings.[2] The conclusion reached by the last named writer is "that the rings must consist of disconnected particles: these may be either solid or liquid, but they must be independent. The entire system of rings must, therefore, consist, either of a series of many concentric rings each moving with its own velocity and having its own system of waves, or else of a confused multitude of revolving particles not arranged in rings and continually coming into collision with each other." Now the physical condition of the *primary* rings was probably somewhat analogous. After the process of separation commenced, we may suppose a continued succession of rings to have been thrown off in close proximity to each other, each revolving round the central mass in accordance with Kepler's third law.[3] The result, then, of a gradual condensation would be a central body surrounded by an indefinite number of concentric rings, or, rather, of disconnected planetary molecules, all moving in the same direction. The mutual attraction of some of these particles, when coming into close proximity to each other, would cause them to unite, and in this way we may suppose the planetary nuclei to have been first established. In the subjoined diagram, let S be the center of the solar mass; ABC and DEF the orbits of two adjacent planetary nuclei, B and E; and H the point of equal attraction between them. Let also $p$, $p'$ be particles revolving round the center, S, in approximate accordance with the third law of Kepler. Their motion is disturbed by the attraction of E, and, in consequence, they finally coalesce with it. But the orbital velocity of $p$ is *less* than that of E, while, on the other hand, that of $p'$ is *greater.* It is obvious, therefore, that they would not approach the nucleus in lines normal to its surface. The point of contact of the *outer* particle would be *behind* the center, that of the *inner* one in *advance* of it. These particles, then, would act as oblique

[1] Syst. of the World, Harte's Translation, vol. ii, p. 359.

[2] Gould's Astr. Jour., vol. ii, No. 1, May 2d, 1851; vol. ii, No. 3, June 16th, 1851; and vol. iv, No. 14, Sept. 5th, 1855. Also, Maxwell's Essay on the Stability of the Motions of Saturn's Rings, p. 67.

[3] This law, we are aware, would but imperfectly represent the motions of the individual particles of the rings.

forces; their tendency being to produce a rotation in a direction *contrary* to that of the orbital motion. When the planetary mass, however, had gained considerable magnitude, the solar




attraction would produce a tidal elevation on the hemisphere toward the sun. The gravitating force of this protuberant matter would maintain the greatest axis, during an indefinite period, in the direction of the central body; thus causing an equality between the angular velocities of rotation and orbital revolution, such as is now found to obtain in the case of the secondary planets.

Let us now consider the consequences of further condensation.

Let S be the center of the solar mass;

$AC = D =$ the diameter of a planet's sphere of attraction;

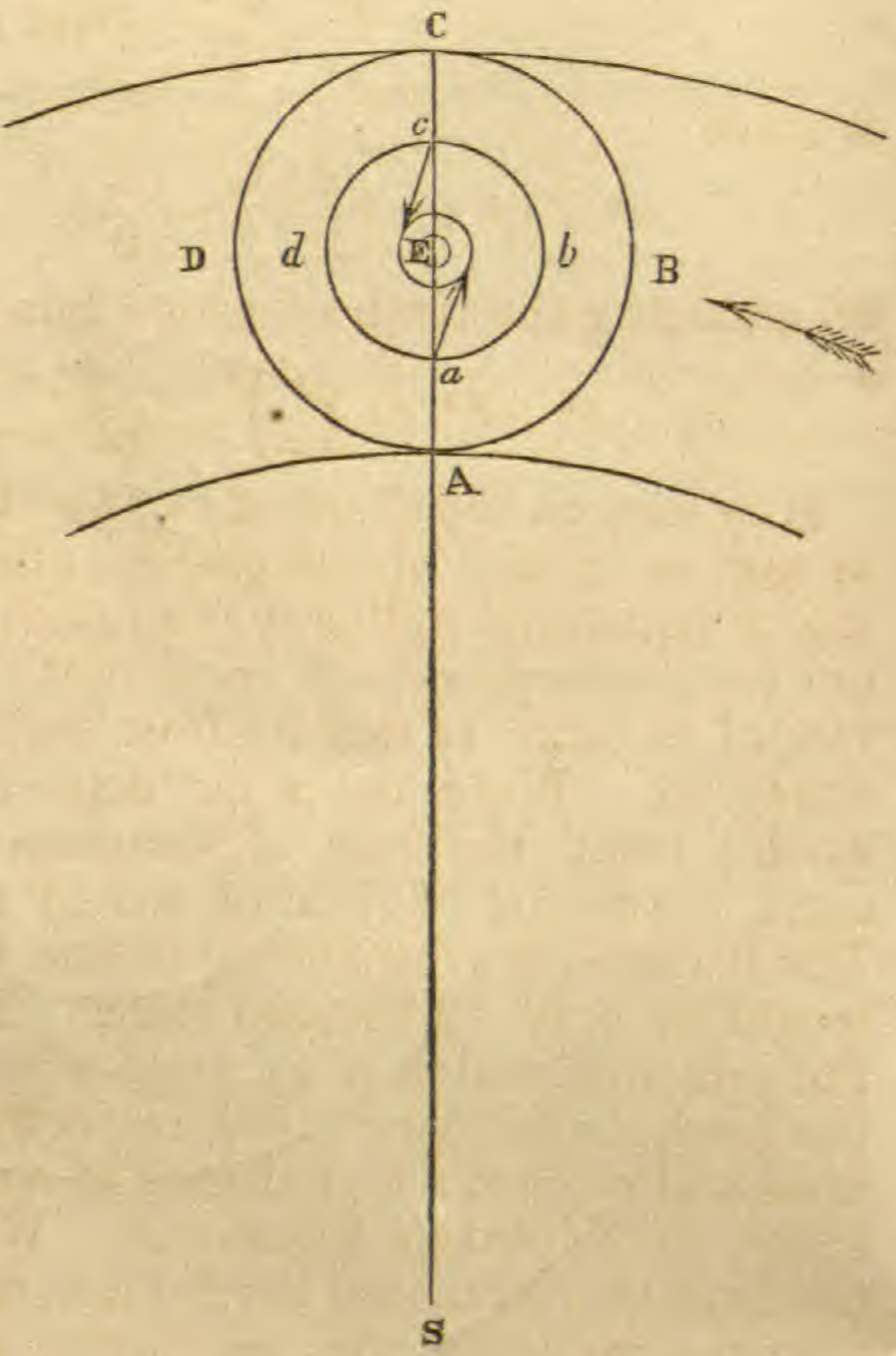
and $ac =$ the diameter of the vaporiform planet at the close of the epoch of equality between the angular velocities of the rotary and progressive motions.

It is obvious that the orbital velocity of a particle at $c$ must be *greater* than that of the center of the mass, while that of a particle at $a$ must be *less*. Any further contraction of the spheroid must tend therefore, to accelerate the rotation *in the direction* *of the orbital motion.* It must also be manifest that the rotation-period at the epoch of solidification will depend upon the ratio of *ac* to SE. Now, it may be impossible to determine in any particular case what was the diameter of a gaseous planet at the commencement of the acceleration of the rotary velocity. We may assume, however, with a high degree of probability, that its ratio to the diameter of the planet's sphere of attraction was the same or nearly the same in each instance; or that

$$\Delta : \Delta' :: D : D'; \qquad (1)$$

where D, D′ are the diameters of the spheres of attraction, and $\Delta$, $\Delta'$, the diameters of the planets immediately before passing the limit of equality between the periods of rotation and revolution.

2




The law of rotation as originally announced is as follows:—

"Let P be the point of equal attraction between any planet and the one next interior, the two being in conjuction: P′ that between the same and the one next exterior.

Let also D= the sum of the distances of the points P, P′ from the orbit of the planet; which I shall call the diameter of the sphere of the planet's attraction;

D′= the diameter of any other planet's sphere of attraction found in like manner;

$n$ = the number of sidereal rotations performed by the former during one sidereal revolution round the sun;

$n'$ = the number performed by the other; then it will be found that

$$n^2 : n'^2 :: D^3 : D'^3.\text{"*} \qquad (2)$$

Let, now, T, T′= the periodic times of two planets;

$t$, $t'$= their periods of rotation;

and $d$, $d'$= their mean distances from the sun;

* Proc. Am. Assoc. for the Adv. of Sci., 2d Meeting, held at Cambridge, 1849, p. 207.

then (2) becomes

$$\left(\frac{T}{t}\right)^2 : \left(\frac{T'}{t'}\right)^2 :: D^3 : D'^3; \text{ or, by Kepler's third law,}$$

$$\frac{d^3}{t^2} : \frac{d'^3}{t'^2} :: D^3 : D'^3;$$

whence

$$t^2 : t'^2 :: \left(\frac{d}{D}\right)^3 : \left(\frac{d'}{D'}\right)^3 \qquad (3)$$

or, assuming the truth of proportion (1),

$$t^2 : t'^2 :: \left(\frac{d}{\Delta}\right)^3 : \left(\frac{d'}{\Delta'}\right)^3 \qquad (4)$$

It is seen on the slightest examination that in the solar nebula, as well as in each of the gaseous planets, the ratio of the revolving or equatorial radius to the radius of gyration varied throughout the entire process of condensation: in other words, that the rate of variation of density from surface to center was constantly changing. Were the solar mass expanded so as to fill the earth's orbit, the rate of variation of density remaining the same, the period of rotation would not be 365 days, but 5457. The diameter corresponding to one revolution in a sidereal year would be only 12,720,000 miles. If we compute the values of the principal radius of gyration when the spheroid extended to the present planetary orbits respectively, we find that the solar mass had reached a high degree of central condensation before the epoch of Neptune's separation. We find, moreover, that the condensation advanced much more rapidly about the center than near the surface of the contracting mass. Probably it became so great as to produce chemical action, thus forming a nucleus in a state of igneous fluidity by the precipitation of the denser portion of the nebula, long before the exterior parts had passed from their original gaseous condition.

It is not necessary to suppose, as has been generally done, that if the nebular hypothesis be true the outer planets of the system must have an immensely greater antiquity than the interior. The formative processes in the different cases may have been contemporaneous.[4] In this view of the case, it seems probable that the remoter planets are less advanced in their physical history than some nearer the sun. The formation of a single planet from matter diffused around the circumference of a larger circle, would undoubtedly require more time than the aggregation of a ring of smaller dimensions. Possibly there may be

[4] Since writing the above, we have been favored with the reading of some highly interesting researches on the nebular hypothesis, by David Trowbridge, Esq., of Perry City, N. Y. These mathematical investigations sustain the probability of the synchronous formation of different members of the solar system. We are gratified to know that Mr. T. is preparing a treatise on this subject for the press, and trust it may soon be given to the public.

rings exterior to Neptune still in the nebular state, or at least not yet collected about a single nucleus.

It has been shown that, according to the nebular theory, a planet's time of rotation ought to be *some* function of the ratio of the radius of its orbit to the diameter of its sphere of attraction. Those ratios are very nearly equal in the case of Jupiter and Saturn; the periods of rotation are also nearly identical: the ratio, however, is somewhat greater in the case of Saturn; so also is the time of rotation. The ratios again are not very different in the cases of Mercury, Venus, the Earth and Mars; and again *in each instance* a greater ratio corresponds to a slower rotation. The *form* of the function as expressed by the equation,

$$\frac{T}{tD^{\frac{3}{2}}} = \text{a constant},$$

was found by a tentative process.

This analogy indicates, as we have stated, a longer period of rotation for Uranus than had been *conjectured* by some astronomers. It assigns, however, a physical *cause* for this slow revolution; while the short period of nine hours and a half, assumed by some writers, has no such basis. The best observers have failed to detect any such polar compression of the planet as would indicate a rapid revolution. "Professor Mädler," says Mr. Hind, "thinks he has detected a very considerable ellipticity in the form of the planet, and makes the ratio of the equatorial to the polar diameter as ten to nine, the axis being inclined at an angle of 15° 26′ to the circle of declination (1843, September 28). Other astronomers, with more powerful telescopes, have not succeeded in gaining any certain evidence of an appreciable difference in the diameters. Mr. O. Struve has informed me orally that the grand refractor at Pulkova affords no indications of ellipticity."[5] The Rev. Robert Main, of the Royal Observatory, Greenwich, states that at his request "Professor Challis obligingly measured the planet, some years ago, with a double image micrometer attached to the telescope of the great Northumberland equatorial, for the purpose chiefly of discovering whether it had any sensible ellipticity, which the author suspected from some measures of his own made with a far inferior telescope. The result was that the ellipticity is too small to be measurable."[6] These measures were made at nearly the same time with those of Mädler. The disk of Neptune, also, even when viewed through the most powerful telescopes, appears perfectly circular. Now the degree of ellipticity correspoding to a short period of rotation, nearly equal to that of Jupiter or Saturn, would undoubtedly be so great, especially in the case of Uranus, as to be easily recognized. The preponderance of evi-

[5] Hind's Solar System, p. 121. [6] Main's Rudimentary Astronomy, p. 130.

dence, therefore, apart from the reason assigned by my analogy, is unquestionably in favor of a long period of rotation.

It is easily shown that the equality between the angular velocities of rotation and orbital revolution, which obtains in the secondary systems, is not incompatible with the law of rotation. When the volumes of the primary planets had the same ratio to their spheres of attraction as those of the satellites now have to theirs, the former were still in a state of vapor, their masses extending beyond the present orbits of the secondaries, and not having reached, in all probability, the limits of equality between the two angular velocities in their respective cases. Had the satellites, at the corresponding epoch in *their* history, been equally rare, so that any increase in rotary velocity would not have been prevented or arrested by solidification, the same law would doubtless have obtained in the secondary systems.

I have believed, however, almost from the time of its first announcement, that the statement of my analogy requires some modification. If it be the expression of a physical law, it must depend on the relation between the primitive momentum of rotation and that of orbital revolution. Now the time of rotation of any planet having satellites is evidently greater than it would have been had the entire mass condensed in a single body. In order, therefore, to find the proper constant of rotation for the earth, we must determine the time in which its axial revolution would be performed were the matter of the moon diffused over its surface; the momentum of the satellite's orbital motion being converted into momentum of rotation.

The earth's momentum of rotation cannot be known with accuracy, because the rate of variation of density from surface to center has not been determined. It is probable, however, that the mean density is nearly attained not far from the surface, and that no very important error would result from considering the radius of gyration to be that of a homogeneous sphere. Adopting this hypothesis, we obtain, by an easy calculation in mechanics, about 22h. 48m. as the rotation period of the united mass. The corresponding number of days in a year would be 384·4.

The sum of the masses of Jupiter's satellites is only about $\frac{1}{6000}$th, the mass of Jupiter being 1. The sum of the masses of the Saturnian satellites is still less as compared with their primary. In a calculation of this nature, therefore, the acceleration of the rotary velocities of these planets from the precipitation of their secondaries may be wholly neglected. The mass of Saturn's ring, however, according to Bessel, is $\frac{1}{118}$th, that of the primary being 1. A calculation similar to that which we have made for the earth and moon shows that the precipitation of even this mass upon the planet would shorten the period of rotation only about 17 minutes. But with Bond's estimate of the thickness of the ring, this value of the mass would indicate a density more than three times that of Saturn—a greater density than has been found for any planet, primary or secondary, exterior to Mars. This result seems too improbable to be admitted without confirmation. We have thought it best, therefore, in this state of the case, to adopt, without alteration, the received value of Saturn's period of axial revolution, viz: 10h. 29m. 17s.

If we use the masses of Jupiter, Saturn and Uranus, adopted in the *American Nautical Almanac*, we find the diameter of Saturn's sphere of attraction = 8·5478. Hence the constant of rotation

$$C = \frac{n}{D^{\frac{3}{2}}} = 985{\cdot}161\,;\ \log C = 2{\cdot}993507.$$

The diameters of the spheres of attraction for the other planets are then found from the formula

$$\log D' = \tfrac{2}{3}\,(\log n' - \log C).$$

The results, tabulated, are as follows:—

| | | | | | |
|---|---|---|---|---|---|
| Diameter of | the sphere | of attraction | of | Mercury, | 0·1992 |
| " | " | " | " | Venus, | 0·3801 |
| " | " | " | " | Earth, | 0·5340 |
| " | " | " | " | Mars, | 0 7731 |
| " | " | " | " | Jupiter, | 4·8342 |
| " | " | " | " | Saturn, | 8·5478 |

Let us now find the corresponding masses of Mercury and Mars, together with the mass and distance of the asteroid planet. We will employ the mass of the earth given in the *American Ephemeris.* Leverrier's value of the mass of Venus there adopted is probably somewhat too large, as seems to have been more recently admitted by Leverrier himself. We will, therefore, take Encke's value, $\frac{1}{401837}$. The calculation is obvious and need not be repeated. We find

The mass of Mercury = $\frac{1}{2997700}$.
" " Mars = $\frac{1}{3097100}$.
" " Asteroid Planet = $\frac{1}{1255868}$.
Distance of the Asteroid Planet = 3·1116.

*Remarks.*

1. This value of Mercury's mass is considerably greater than that found by Encke, but is very nearly identical with Leverrier's second value.

2. Our mass of Mars is somewhat less than Burckhardt's. Mr. Airy's researches on the theory of the sun have led him to the conclusion that a considerable diminution of that value is actually demanded.[7]

[7] Grant's Hist. of Phys. Astr., p. 129.

3. The resulting mass of the asteroid planet is less than one-third that of the earth; about the limit assigned by Leverrier for the mass of the zone of asteroids.

*Researches of Professor Hinrichs.*—The learned and interesting paper on the density, rotation, and relative age of the planets, in the American Journal of Science and Arts, for January, 1864, by Professor Hinrichs, would seem to enhance the difficulty of developing any order either in the *present* arrangement of the members of the solar system, or their revolutions on their axes. The conclusions of Professor H., however, depend upon the existence of a resisting medium, the only indication of which is found in the shortening of the period of Encke's comet. Encke himself regards this resistance as insensible exterior to the orbit of Venus. Now if the zodiacal light consists of solar rings, the sensible resistance of the comet is limited to the time of its passage through them. May we not thus find a sufficient cause for the diminution of the period?—and ought not all speculations in regard to the effect of the medium on the motions of the remoter members of the planetary system, to be received with caution, until we have some further evidence of its existence?

With respect to the want of harmony in the present arrangement of the planetary orbits, Prof. Hinrichs remarks as follows:

"The present configuration of the planetary system is without that harmony and order everywhere else observed where matter is aggregating (e. g. in crystals, etc.); we must therefore suppose that the original harmonious configuration has been altered by the action of some general cause, displacing the celestial strata (orbs) according to the individual mass, size, and position of each body; the same we know to have occurred in the case of the earth's figure, being at first ellipsoidal, but now to some extent irregular—or the terrestrial strata of rocks, which were at first continuous, but are now greatly dislocated. This cause has been, and is, *the resistance of the ether* filling the heavenly space in which the celestial globes are moving; for the mathematical investigation of the effects of such a resistance agrees perfectly with the phenomena observed." [a]

Without entering upon any special discussion of this interesting subject, we respectfully submit the following considerations:

1. The hypothesis adopted by Encke in regard to the medium which causes the acceleration of the mean motion of his comet, is that the density varies inversely as the square of the distance from the sun, and that the resistance is proportional to the density of the medium and the square of the velocity of the moving body.

[a] Am. Jour. of Sci. and Arts, Jan., 1864, p. 55.

2. Granting the existence of an ethereal medium, it would seem unphilosophical to ascribe to it one of the properties of a material fluid—the power of resisting the motion of all bodies moving through it—and to deny it such properties in other respects. Its condensation, therefore, about the sun and other large bodies must be a necessary consequence.

3. This condensation existed in the primitive solar spheroid, before the formation of the planets: the rotation of the spheroid would be communicated to the ether co-existing with it: *hence, during the entire history of the planetary system, the ether has revolved around the sun in the same direction with the planets.*

4. This condensed ether must participate in the progressive motion of the solar system.

5. Even if we reject the doctrine of the development of the planetary system from a rotating nebula, we must still regard the density of the ether as increasing to the center of the system. The sun's rotation, therefore, would communicate motion to the first and denser portions; this motion would be transmitted outward through successive strata, with a constantly diminishing angular velocity. The motion of the planets themselves through the medium in nearly circular orbits would concur in imparting to it a revolution in the same direction.

6. Whether, therefore, we receive or reject the nebular hypothesis, the resistance of the ethereal medium to bodies moving in orbits of small eccentricity and in the direction of the sun's rotation, becomes an infinitesimal quantity.

7. The doctrine of a resisting medium is not generally accepted by astronomers as an established fact. "It is manifest," says an eminent writer, "that more extensive indications of such a medium must be discovered before the problem of its existence can be considered as having received a definitive solution. It has not yet affected to a sensible extent any of the other celestial bodies, and, until such is found to take place, the question relative to it must remain in abeyance." [9]

## II.—The Planetary Distances.

As long ago as the commencement of the seventeenth century, the celebrated Kepler observed that the respective distances of the planets from the sun formed nearly a regular progression. The series, however, by which those distances were expressed, required the interpolation of a term between Mars and Jupiter—a fact which led the illustrious German to predict the detection of a planet in that interval. This conjecture attracted but little attention till after the discovery of Uranus, whose distance was found to harmonize in a remarkable manner with Kepler's order of progression. Such a coincidence was of course regarded

[9] Grant's Hist. of Phys. Astr., p. 135.

with considerable interest. Toward the close of the last century, Professor Bode, who had given the subject much attention, published the law of distances which bears his name, but which, as he acknowledged, is due to Professor Titius. According to this formula, the distances of the planets from Mercury's orbit form a geometrical series of which the ratio is two. In other words, if we reckon the distances of Venus, the earth, &c., *from the orbit of Mercury*, instead of from the sun, we find that—interpolating a term between Mars and Jupiter—the distance of any member of the system is very nearly half that of the next exterior. The series is usually expressed as follows:—

$$4 \qquad\quad = 4$$
$$4 + 3 \times 2^0 = 7$$
$$4 + 3 \times 2^1 = 10$$
$$4 + 3 \times 2^2 = 16$$
$$4 + 3 \times 2^3 = 28$$
$$\&c. \qquad \&c.$$

The numbers 4, 7, 10, &c., represent approximately the relative distances of Mercury, Venus, the earth, &c., from the sun. The ninth term, however, which corresponds to Neptune, is 388, instead of 300. It was, moreover, remarked by Gauss that "the member which precedes $4 + 3$ should not be 4; i. e. $4 + 0$, but $4 + 1\frac{1}{2}$. Therefore, between 4 and $4 + 3$, there should be an infinite number; or, as Wurm expresses it, for $n = 1$, there is obtained from $4 + 2^{n-2} \times 3$, not 4, but $5\frac{1}{2}$."

Professor Challis has applied a modification of Bode's empirical formula to the secondary systems of Jupiter, Saturn and Uranus;[10] his coincidences, however, are far from exact. The formula itself, as well as the modifications by Wurm and Challis, may be expressed—commencing with the greatest distance and proceeding toward the center—by the series,

$$a + br^n,\ a + br^{n-1},\ a + br^{n-2},\ \&c.$$

In the scheme of Bode and Titius, $a = 4$, $b = 3$, and $r = 2$; in that of Wurm, $a = 387$, $b = 293$, and $r = 2$. Both fail to represent, even approximately, the relative distances of Mercury and Neptune.

I have never doubted that the planetary distances were arranged in *some* discoverable order. These failures, however, in the series of Titius have seemed a sufficient cause for its rejection, or, at least, some considerable modification. I have, for many years, been devoting such thought and attention to the subject as circumstances would permit, and I now propose to submit my results to the public.

In the American Journal of Science and Arts, for September, 1852, the fact was noticed that, "if we commence with Neptune, the most remote planet known, we shall find that the primary

[10] Cambridge Philosophical Trans., vol. viii, p. 171.

planets are arranged in *pairs*, the members of which are nearly equal in diameter." Neptune and Uranus constitute the first pair; Saturn and Jupiter, the second; the asteroids and Mars, the third; the earth and Venus, the fourth; finally, Mercury is without a *known* companion. It was also remarked that in each of the three complete pairs, the first, second and fourth, the densities of the members are to each other very nearly as their volumes; and that the facts seemed to indicate "a similarity in the original constitution of the members of each pair, and an intimate mutual dependence or connection in their primitive condition." It appeared not improbable that in the first stages of their history, Neptune and Uranus constituted a system of closely associated rings; Saturn and Jupiter, another, &c., and that the law of planetary distances might be found in *the relative situations of the centers of gyration of those binary rings.* In short, my researches on the subject led to the hypothesis that *the differences of the radii of gyration of the primitive rings form a geometrical series;* or that

$$d_{(1)} = d_{(2)}k = d_{(3)}k^2 = d_{(4)}k^3 = \ldots = d_{(n)}k^{n-1};$$

where $k$ = a constant;

$d_{(1)}, d_{(2)}, d_{(3)} \ldots$ &c., $= r_{(1)} - r_{(2)}, r_{(2)} - r_{(3)}, r_{(3)} - r_{(4)}$, &c., respectively;

$r_{(1)} = \left(\frac{D_{(1)}^2 + D_{(2)}^2}{2}\right)^{\frac{1}{2}}$ = the radius of gyration of the first pair, Neptune and Uranus;

$D_{(1)}, D_{(2)}$, &c. = the distances of Neptune, Uranus, &c., from the sun;

$r_{(2)}, r_{(3)}$, &c., = the radii of gyration of the successive binary rings;

*Examination of this Hypothesis.*

The values of $r_{(3)}$, $k$, and $D_{(5)}$ are thus computed: Having found

$$r_{(1)} = 25{\cdot}20061$$
$$r_{(2)} = 7{\cdot}68305$$
$$r_{(4)} = 0{\cdot}87270$$
$$d_{(1)} = 17{\cdot}51756$$

we have the equations

$$r_{(3)} = \left(\frac{D_{(5)}^2 + (1{\cdot}5236923)^2}{2}\right)^{\frac{1}{2}} \qquad (1)$$

$$(7{\cdot}68305 - r_{(3)})\, k = 17{\cdot}51756, \qquad (2)$$

$$(r_{(3)} - 0{\cdot}87270)\, k = 7{\cdot}68305 - r_{(3)}, \qquad (3)$$

whence

$k = 3{\cdot}34189$ $\qquad$ log = 0·5239916

$r_{(3)} = 2{\cdot}44123$ $\qquad$ log = 0·3876087

$D_{(5)} = 3{\cdot}09800$

*This value of the distance of the asteroid planet is almost exactly identical with that found by my analogy.*

The radii of gyration of the primitive rings, together with their differences, are as follows:—

| | | |
|---|---|---|
| $r_{(1)} = 25{\cdot}20061$ | $d_{(1)} = 17{\cdot}51756$ | $\log = 1{\cdot}2434736$ |
| $r_{(2)} = 7{\cdot}68305$ | $d_{(2)} = 5{\cdot}24182$ | $\log = 0{\cdot}7194821$ |
| $r_{(3)} = 2{\cdot}44123$ | $d_{(3)} = 1{\cdot}56852$ | $\log = 0{\cdot}1954906$ |
| $r_{(4)} = 0{\cdot}87270$ | $d_{(4)} = 0{\cdot}469350$ | $\log = \bar{1}{\cdot}6714991$ |
| $r_{(5)} = 0{\cdot}40335$ | $d_{(5)} = 0{\cdot}140455$ | $\log = \bar{1}{\cdot}1475076$ |
| $r_{(6)} = 0{\cdot}26289$ | $d_{(6)} = 0{\cdot}042026$ | $\log = \bar{2}{\cdot}6235161$ |
| $r_{(7)} = 0{\cdot}22086$ | $d_{(7)} = 0{\cdot}012575$ | $\log = \bar{2}{\cdot}0995246$ |
| $r_{(8)} = 0{\cdot}20828$ | $d_{(8)} = 0{\cdot}003763$ | $\log = \bar{3}{\cdot}5755331$ |
| $r_{(9)} = 0{\cdot}20451$ | $d_{(9)} = 0{\cdot}001126$ | $\log = \bar{3}{\cdot}0515416$ |
| $r_{(10)} = 0{\cdot}20338$ | $d_{(10)} = 0{\cdot}0003369$ | $\log = \bar{4}{\cdot}5275501$ |
| &c. &c. | &c. &c. | &c. |
| $r_{(\infty)} = 0{\cdot}20299$ | $d_{(\infty)} = 0{\cdot}0000000$ | $\log = -\infty$ |

*Remarks.*

1. The radius of gyration ($r_{(5)} = 0{\cdot}40335$) of the fifth primitive ring corresponds very closely with the mean distance of Mercury. This planet, therefore, according to the hypothesis, ought to be an exception to the binary arrangement. Such, in fact, appears to be the case. Or, if the planet originally existed as a binary ring, the mean distances of the members having the same ratio to each other as those of Venus and the earth, both must have been included between the present limits of Mercury's orbit. The union of the two rings and the formation of a single planet may thus have resulted from the eccentricity of the primitive annuli.

2. The sum of the infinite series

$$d_{(1)} + d_{(2)} + d_{(3)} + \&c., = \frac{d_{(1)}k}{k-1} = \frac{17{\cdot}51756 \times 3{\cdot}34189}{2{\cdot}34189} = 24{\cdot}99762;$$

and $25{\cdot}20061 - 24{\cdot}99762 = 0{\cdot}20299$ = the distance from the sun's center to the limit at which the separation of solar rings must have ceased. This is immediately exterior to the present limit of equilibrium between the centripetal and centrifugal forces.

3. The fifth radius of gyration, according to this hypothesis, exceeds the present mean distance of Mercury by the quantity 0·01626, or about 1½ millions of miles. The corresponding difference in the period is about 5 days, or twice the amount by which the period of Encke's comet has been shortened in the last half century. Have the period and mean distance of Mercury been diminished, during the immensity of past time, from the same cause? Or may this slight and only exception to the strict accuracy of the law be referable to zones or groups of asteroids in the vicinity of Mercury's orbit, the existence of which has been indicated by the researches of Leverrier? [11]

[11] Runkle's Math. Monthly, vol. ii, p. 240.

4. The radius of gyration of the sixth primitive ring is 0·26289. This distance is nearly equal to that of Mercury's perihelion. Between this and the limit, 0·20299, the formula indicates the abandonment at the solar equator of an indefinite number of rings in close proximity to each other. The appearance of such zones or rings of nebular matter would be similar to that of the *zodiacal light.* This phenomenon was ascribed by Cassini to the blended light of an innumerable multitude of extremely minute asteroids revolving round the center of our system. Recent observations, it is true, have suggested the hypothesis that the appearance is produced by a *terrestrial* ring; but in opposition to this view, Prof. Barnard has adduced a number of weighty, if not insuperable objections. The greatest elongation of these rings of nebular or meteoric matter, when abandoned at the solar equator, would certainly be much less than that of the vertex of the zodiacal light; but is it not possible that, as in the case of several comets, the orbits of some may have been greatly changed by planetary perturbations?

## *Application to the Secondary Systems.*

### I.—THE SATELLITES OF SATURN.

The distances of the satellites of Saturn, in radii of the primary, are as follows:—

| | | |
|---|---|---|
| I. | Mimas, | 3·1408 |
| | Enceladus, | 4·0319 |
| II. | Tethys, | 4·9926 |
| | Dione, | 6·399 |
| III. | Rhea, | 8·332 |
| | ——— | |
| IV. | Titan, | 20·706 |
| | Hyperion, | 25·029 |
| V. | ——— | |
| | Iapetus, | 64·359 |

This table is taken from *Loomis's Practical Astronomy.* The distances, we are informed by the author, "were derived chiefly from Mädler, modified in some instances by comparison with Herschel's Astronomy and Hind's Solar System." A chasm in the order of distances is observed between Rhea and Titan, and another between Hyperion and Iapetus—that is, immediately interior to the largest two members of the system. Now, as in the primary system the zone of asteroids occurs just within the powerful mass of Jupiter, supplying the missing term noticed by Kepler and Bode, is it not probable that similar *secondary* zones exist in those intervals? By interpolating these two satellites or asteroid zones, we obtain a series of ten terms, in which the eight known distances are represented *with perfect accuracy.* It is also remarkable that the limit of the ring-forming process, according to this series, is precisely where Bond's ring is situated, between the body of the planet and the inner bright ring. The interpolated distances are 12·43 and 35·32 respectively. The radii of gyration of the pairs, together with their differences, are as follows:—

| Names of Satellites. | Rad. of Gyr | Differences. |
|---|---|---|
| I. Mimas and Enceladus, | 3·61 | 2·13 |
| II. Tethys and Dione, | 5·74 | 5·08 |
| III. Rhea and —— | 10·82 | 12·13 |
| IV. Titan and Hyperion, | 22·95 | 28·96 |
| V. —— and Iapetus, | 51·91 | |

These differences form a geometrical series whose ratio is 2·385. The sum of the series = 50·59, which substracted from 51·91 gives 1·32 as the limit. The series indicates the abandonment of an indefinite number of satellites or rings in close proximity to each other, between Mimas and this limit. Now Professor Vaughan has shown that neither a gaseous nor a liquid satellite, of considerable magnitude, would be stable so near the primary.[12] Hence the probable origin of Saturn's rings.

### II.—THE SATELLITES OF JUPITER.

If we suppose the third and fourth satellites of Jupiter to have originally constituted one ring, or system of associated rings, the first and second, another, and that the ratio of the differences of the radii of gyration was the same as in the primary system, we shall find

$$r_{(1)} = 21{\cdot}960 \text{ equatorial radii of the planet,}$$
$$r_{(2)} = 8{\cdot}036 \quad \text{"} \quad \text{"} \quad \text{"}$$
$$d_{(1)} = 13{\cdot}924 \quad \text{"} \quad \text{"} \quad \text{"}$$
$$d_{(2)} = 4{\cdot}166 \quad \text{"} \quad \text{"} \quad \text{"}$$
$$\frac{d_{(1)}k}{k-1} = 19{\cdot}869 \quad \text{"} \quad \text{"} \quad \text{"}$$

and 21·960 − 19·869 = 2·091 = the distance from the center of the primary, within which no rings could have been formed.

This again is nearly equal to the distance (2·299) of the point of equilibrium between the centripetal and centrifugal forces.

But we discover no decided indications of the binary arrangement in the Jovian system. Is a similar relationship then to be found between the respective distances of the satellites themselves? "The four satellites of Jupiter," says Humboldt, "present a certain regularity in their distances, forming nearly the series, 3, 6, 12. The distance of the second from the first, expressed in diameters of Jupiter, is 3·6; the distance of the third from the second, 5·7; and that of the fourth from the third,

[12] Proc. Am. Assoc. for the Adv. of Sci., 1856, p. 111.

11·6." This would indicate a value of $k$ for the Jovian system equal to 2; $r_{(\infty)} = 3$; and the possible separation of secondary asteroids between the limit 3 and the distance 4·5.

Our knowledge of the Uranian system is perhaps too imperfect to justify any conclusion in regard to the prevalence of a similar order.

In researches of this nature, the want of *exact* numerical verification ought not to be regarded as decisive evidence against the truth of an hypothesis. A partial deviation may be produced by the interference of some other law or arrangement. Of this we have innumerable instances in nature. Even the celebrated laws of Kepler, as commonly stated, are not strictly true; the mutual attractions of the planets producing endless perturbations.[13]

## III.—The Mean Distances of the Periodic Comets, and their Relation to the Solar System.

The celebrated Laplace remarked that, according to the nebular hypothesis, "the comets do not belong to the solar system." He regarded them as small nebulæ which, wandering through space till they come within the sphere of the solar influence, enter our system from *without*, pass around the sun, and, unless influenced by the attraction of the planets, or the resistance of the ethereal medium, again pass off in parabolas or hyperbolas. Other astronomers believe them to have originated within the solar system. Perhaps each view may be partially correct. Several comets, among which we may instance that of June, 1861, have moved in hyperbolic orbits. These, together with many whose orbits seem to be parabolas, have probably entered the system *ab extra*. On the other hand, a large majority of *periodic* comets are believed to have originated *in* the system, and to belong properly to it. The author several years since called attention to the fact that there is an approximate coincidence between the planetary and cometary periods.[14] There are 13 known

[13] The idea of employing the radii of gyration of the planetary pairs, occurred to the writer in 1852. Various hypotheses involving this element were tested in the search for the law of distances; but the fact that the preceding formula gives the radius of gyration of the fifth planetary pair *greater* than the mean distance of Mercury, seemed, it was thought, sufficient ground for its rejection. At the Albany Meeting of the American Association for the Advancement of Science, in 1856, a paper was read by Professor Stephen Alexander, "On some Special Arrangements of the Solar System, which seem to confirm the Nebular Hypothesis." In this memoir, Professor A. likewise employed the radii of gyration of the planetary pairs; his method of using them, however, was different from the author's. This paper induced the writer to resume the subject. The applicability of a similar formula to the Saturnian system was regarded as confirmatory, and, finally, as it seemed possible to account for the slight inaccuracy in the case of Mercury, the striking conformity of the theory with facts has determined the author to submit his results to the public.

[14] Proc. Am. Assoc. for the Adv. of Sci., 1858, p. 10.

comets whose periods are included between those of Mars and Jupiter. Their motions are all direct; their orbits are less eccentric than those of other comets; and the mean of their inclinations is about the same as that of the asteroids. The perihelia of 5 are exterior to the earth's orbit, and the nearest approach of Faye's to the sun is several million miles beyond the orbit of Mars. In fact, there is less difference between the eccentricity of the orbit of Faye's comet and that of some of the asteroids, than between the latter and that of some of the old planets; so that this body may be regarded as a connecting link between planets and comets. These facts appear to indicate some connection in their origin with the zone of asteroids.

Since the commencement of the present century, five comets have been discovered, which form, with Halley's, an interesting and remarkable group. The first of these was detected by Pons, on the 20th of July, 1812; the second by Olbers, on the 6th of March, 1815; the third by DeVico, on the 28th of February, 1846; the fourth by Brorsen, on the 20th of July, 1847; and the last by Westphal, on the 27th of June, 1852. The periods of these bodies are all nearly equal, ranging from 68 to 76 years; their eccentricities are not greatly different; and the motions of all, except that of Halley, are direct. The existence of these two cometary groups was noticed several years since both by Hind and Alexander. The latter supposes the cluster whose times of revolution are nearly equal to the period of Uranus, to have had a common origin. He infers from various facts that in the early part of the fourteenth century a large comet approached very near to Mars, if indeed there was not an actual collision between the two bodies. This ancient comet he supposes was thus separated into fragments. That most, if not all, of this cometary group have had a common origin, we regard as highly probable: we doubt, however, whether the true explanation of that origin has yet been proposed.

Again: the comet discovered by Peters on the 26th of June, 1846, has a period, according to the discoverer, of about 13 years; and Tuttle's comet (1858, I.) completes its revolution in 13·6 years. The perihelion of each is exterior to the earth's orbit, and their motions are direct. The periods of these bodies are a little greater than that of Jupiter. It may also be remarked that the comet which passed its perihelion on the 28th of November, 1793, has, according to Burckhardt, a period of 12 years. The period of the great comet of 1843 is probably nearly the same with that of Neptune. Other coincidences might be pointed out, but the periods in most cases are too doubtful to be relied upon. Those which we have adduced seem to point to an approximate coincidence between the mean distances of the planets and those of the periodic comets.

May not the exterior secondary rings, thrown off by the planets, have been at too great a distance to form *stable satellites?* and in such case would not the detached portions of matter revolve round the sun in very eccentric orbits, the degree of eccentricity depending on the direction of their motion at the epochs of separation from the secondary system? If so, the approximate coincidence between the periods of planets and comets would follow as a consequence.[15]

Indiana University, Bloomington, Indiana, March 29, 1864.

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## ART. II.—*Abstract of Prof. Meissner's Researches on Oxygen, Ozone, and Antozone;* by S. W. JOHNSON.

[Concluded from vol. xxxvii, p. 335.]

THE 2d Section, entitled *The Polarization of Oxygen in the Act of Combustion*, opens with an examination of the products of the slow oxydation of phosphorus. The white fumes which are always formed in this process have been the subject of much speculation. Schönbein observed that the fumes appear only in *moist* air. He once (1848) held them to consist merely of phosphorous acid, formed by the contact of vapor of phosphorus with oxygen. Afterward, he noticed that they do not readily disappear upon agitating with water, and since dry $PO_3$ absorbs water with great avidity, he assumed in the fumes the existence of an insoluble $PO_3$, isomeric with the ordinary acid. Williamson thought the cloud to consist of $PO_5$, the last result of the action of ozone on vapor of phosphorus. Osann, at first, denied the existence of any of the oxyds of phosphorus in the fumes on account of the permanence of the latter, as they may be passed through water, potassa lye, oil of vitriol, nitric acid, and solutions of nitrate of silver, arsenious acid, protosulphate of iron, and iodid of potassium, without perceptible change. Finding, however, evidence of the presence of $PO_3$ in the water over which the cloud had been allowed to stand until it disap-

[15] The perturbation of such portions of nebulous matter was the "general cause" to which the writer referred in his paper on the mean distances of the periodic comets, read before the American Association in 1858. Nearly the same idea was suggested in a letter dated May, 1863, by DAVID TROWBRIDGE, Esq., of Perry City, N. Y. At that time, Mr. T. knew nothing of the above-mentioned paper on the subject, so that the hypothesis was with him entirely original. He remarks:

"The breaking up of the ring might detach small portions that would not unite with the parent mass. The eccentricity would depend on the angle of projection. See *Math. Monthly*, vol. ii, p. 160, Art. 18, Equation (58).

"This being true, the mean distances of the comets should coincide approximately with the mean distances of the planets. I think we should look for the larger comets to have the longer periods of revolution, because larger amounts would be thrown from the large rings in a less condensed state."

peared, Osann adopted Schönbein's idea of the existence of two modifications of phosphorous acid.

Quite recently, as our readers are aware, Schönbein has given up his former opinions and now maintains that the cloud consists essentially of *nitrite of ammonia.* The objection with which he now argues the impossibility of its being constituted of phosphorous acid, viz: its insolubility in water, he does not appear to notice is equally fatal to this new idea.

Meissner, on subjecting a stream of air that had passed over moist phosphorus to the tests already detailed, obtained with it all the phenomena which characterize antozone. Thus, when the air is washed with solution of iodid of potassium, whereby it is deozonized and thereupon is made to pass through water, it emerges from the latter as a thick cloud. The cloud vanishes of itself after the lapse of about half an hour and cannot then be reproduced, though it is scarcely diminished by agitation for a short time with water; when subjected to drying agents, it disappears, but is formed again on renewed contact with water, if too much time is not allowed to transpire.

In the air which is acted on by phosphorus there thus appear both ozone and antozone, as in the case of electrized air; but the relative proportions of the two allotropic oxygens, in the air which has been conducted over moist phosphorus, are very different from what exist in electrized air. In the case of air exposed to moist phosphorus, the quantity of ozone is much less than in electrized air. This is due to the fact that the phosphorus itself consumes a large share of the ozone. Meissner obtained in his experiments much more antozone by phosphorus than by electricity, as measured by the density and ready appearance of the cloud.

While passage of the electrized air through strong solution of KI removed the cloud, by absorbing its moisture, it was found, as Osann had observed, that the phosphorus cloud was scarcely affected by bubbling through oil-of-vitriol. It is only needful, however, to prolong or increase contact between these drying agents and the cloud in order to cause its disappearance.

When the phosphorus cloud traverses a strongly heated glass tube, it disappears. The same is true when it is brought into intimate contact with $PbO_2$ or $MnO_2$.

By diminishing to a certain point the quantity of oxygen in a gaseous mixture, we may have the result, that when it streams over moist phosphorus, all the ozone produced is again consumed, and only antozone (of the two active forms of oxygen) remains in the air.

The complete identity of the cloud that forms when phosphorus slowly oxydizes in moist air, with that produced when electrized air emerges from water, is thus established.

Long ago, Schönbein observed that $HO_2$ appears when air is acted upon by moist phosphorus. This substance Meissner finds to be more largely and easily obtainable from air exposed to phosphorus than from electrized air. This he concludes to depend upon the corresponding difference in the relative development of antozone in the two instances. On the other hand, when the air from phosphorus passes through solution of KI, it yields to subsequent wash-waters but minute traces of iodic acid, while from electrized air this substance may be collected in considerable quantity. This fact is again to be ascribed to the small amount of ozone in the former as compared with the latter case.

In water which had been traversed by the air from moist phosphorus, previously deozonized by means of KI, no nitrous acid and no ammonia could be detected. Of the latter, at least only those minute traces everywhere recognizable with potassio-iodid of mercury, were observed.

The water, thus containing $HO_2$ but free from $NO_3$ and $NH_3$, contained $PO_5$. When the antozone cloud produced by phosphorus is received in a perfectly dry and clean vessel, and there allowed to resolve into ordinary oxygen and water, the latter, which deposits as a dew on the walls of the vessel, has an acid reaction, which is not attributable to the minute trace of $IO_5$ it contains, but proceeds from $PO_5$, whose presence is readily made out by the usual tests.

Even when the antozone cloud is made to bubble through potash-lye, it still retains a trace of $PO_5$. It is, in fact, the property of the antozone cloud to transport suspended matters, which accounts for the finding of $PO_3$ and $PO_5$ by other observers. It is of course only needful to procure sufficient contact between the antozone cloud and water, or an alkali, to arrest these substances entirely.

When the air clouded by contact with moist phosphorus is made to pass direct through water for a long time, the latter acquires an acid reaction from $PO_5$. After the $PO_5$ is removed, or neutralized, $HO_2$ may be detected by aid of KI, starch and $FeO\,SO_3$. Nitric acid Meissner found but very rarely and then in but very minute traces.[1] It appears that while in electrized air nitrogen is oxydized by the ozone to a considerable extent, in air streaming over phosphorus the phosphorus appropriates the ozone in great measure.

The results of the mutual action of phosphorus, air, and water are somewhat different, when, as in Schönbein's experiments, the air is allowed to stagnate over the phosphorus. In the phosphatic acid, as we may designate the solution which forms about the phosphorus in the ordinary ozone bottle, there are found

[1] So also Pugh failed to find $NO_5$ in similar experiments. "*On the Sources of Nitrogen to Vegetation.*" *Phil. Tr.*, 1861, Pt. II, p. 496.

$PO_5$ and $PO_3$, as has long been known; $HO_2$, as Schönbein discovered,[1] and likewise $NO_5$. As regards the last named substance, Meissner states that its quantity is but small, more of it being produced in one hour by his electrizing apparatus, than in 24 hours by phosphorus.

Schönbein has lately asserted the presence of nitrous acid and ammonia in the so-called phosphatic acid. The reactions on which Schönbein based the discovery of $NO_3$ are all, indeed, as he described them; but they all admit of a different explanation. The decomposition of KI by the phosphatic acid may be attributed to $HO_2$; and a more direct reaction must be employed in order to demonstrate the existence of $NO_3$. For this purpose Meissner freed the liquid of $PO_5$ and $PO_3$ by BaO, made it alkaline with KO, and, after suitably concentrating, examined it for $NO_3$ by means of FeO $SO_3$ and dilute $SO_3$. In a few instances he found minute traces of $NO_3$ by this method; but in no case was $NO_3$ present in the quantity that would be indicated by the KI and starch test. As regards $NH_3$, Meissner declares his inability to discover in the phosphatic acid more than the traces, which, as Faraday, Boussingault and others have taught us, are universally distributed on the surfaces of porous bodies and throughout the atmosphere.

Schönbein appears to place most reliance upon his experiments wherein a clean moistened sponge was used to absorb the vapors or cloud that is formed over phosphorus. He was perfectly right in asserting that the water obtained by squeezing the sponge after prolonged exposure, is neutral to test papers, but, nevertheless, strikes a deep blue color with acidulated solution of KI mixed with starch. Meissner assures us that this reaction is entirely attributable to a product of the action of ozone upon an ingredient of the organic matter of the sponge, viz: *iodine;* and that, on concentrating the water pressed from the sponge and adding to it sulphurous acid, a copious separation of iodine occurs. This reaction demonstrates that iodic acid, which decomposes KI with ease, gave the reactions from which Schönbein deduced the formation of $NO_3$ in the slow combustion of phosphorus.

In the examination of the so-called phosphatic acid, Meissner found evidences of the presence in it of another substance, possessed of reducing properties, opposed to the oxydizing quality of antozone. As previously observed, solution of pure KI, free from $IO_5$, when acidified, after some time suffers decomposition with separation of I, and the rapidity as well as the extent of

[1] And in very large quantity, compared with what is produced when the air is made to traverse water placed in another vessel. In the latter case, antozone which with water forms $HO_2$, is too far advanced in its reversion to the inactive state to produce its highest effect.

the decomposition are the greater the larger the quantity and the stronger the quality of the acid added. If, now, two equal portions of the same solution of KI, each acidified with a drop of the same dilute $SO_3$, are mixed, one with pure water and the other with the same volume of what remains of the phosphatic acid solution after it has been precipitated by CaO, it is seen that the separation of iodine is greatly hindered or entirely prevented in the latter case.

Further study of this liquid conducted to the result that it owes this reducing quality to ozone. In fact, when air ozonized by phosphorus or by electricity is allowed to stand at rest in a flask until all antozone has vanished, and then the flask washed repeatedly to remove all phosphatic acid or deposited matters, the ozone that remains communicates this property to pure water. *Ozone water*, as the liquid may be called, gradually loses its reducing quality when exposed to air or evaporated on the water-bath. Its properties are the opposite of those of $HO_2$, which we may term *antozone water*. Ozone water and $HO_2$ may exist together in the same liquid, and, under certain circumstances, the former prevents the oxydations which the latter, if alone, would accomplish. This reducing power of ozone water is only relative, and not an absolute and invariably exhibited quality. Meissner has found indeed no means of directly effecting oxydations by means of ozone water; but he has learned that in some cases it does not limit or counteract oxydizing influences. Solution of KI, as is known, is decomposed by $PbO_2$ with liberation of I. The oxygen of $PbO_2$ acts accordingly like ozone. This oxydation, so far from being hindered, appears to be promoted by ozone water. Meissner's observations on this so-called ozone water are, however, confessedly incomplete.[3]

He recalls, however, a fact noticed twenty years ago by Schönbein, who found that water which had been agitated for a long time with a large volume of air ozonized by electricity, was electro-negative compared with pure water. Schönbein remarks that ozone is taken up by water to only a very slight extent, and its voltaic activity is extremely small. Antozone-water, i. e. $HO_2$, is electro-positive compared to water. Meissner has experimentally confirmed these observations.

The action of phosphorus, according to the author's view, consists "in polarizing the inactive oxygen. It operates like an electrically excited body, and since it combines with ozone, which is negative-active oxygen, it operates like a positively electrized body, or has the effect of electro positive tension. Phosphorus must produce this polarizing effect by virtue of what we designate its great chemical affinity."—p. 256.

[3] May not the reducing action which $HO_2$, as ordinarily prepared, is well known to exert upon many oxyds, be due to the same cause as was operative in Meissner's experiments? S. W. J.

"What we call the chemical affinity of phosphorus for oxygen is the same thing that is exerted in a less degree by zinc when it slowly oxydizes in moist air: the latter is, however, due to the same cause which results in the exhibition of negative electricity when zinc is immersed in water; the chemical affinity of phosphorus to oxygen is the *difference* in its attraction toward positive and negative electricities: affinity is strong because the difference in electrical attractions is great, because the attraction for negative electricity largely preponderates."—p. 256.

"If phosphorus polarizes the molecule of inactive oxygen (into the ozone and antozone atoms), it must exert a polarizing and separating influence upon the electricity of other bodies. This is really the case, as shown by Schönbein, whose observation I have found to be entirely correct. Schönbein discovered that when a plate of gold or platinum is immersed in vapor of phosphorus, or is rubbed over its surface with a stick of phosphorus, and dipped into water in contact with a clean plate of the same metal, it exhibits positive electricity. When phosphorus is placed in a bottle with moist air, it emits, at first, a garlic odor; and only afterwards, when some time has elapsed, does the odor of ozone become manifest. The garlic odor is evidence that unoxydized vapors of phosphorus occupy the bottle. If a platinum plate be suspended in these vapors, it is positively electrized; when, however, later, ozone predominates, the plate becomes negative."—pp. 266–7.

We have not space to give, in full, Meissner's interesting theoretical discussion of this topic, which occupies a number of pages of his book, but must confine ourselves chiefly to recounting the facts he adduces.

In the rapid combustion of phosphorus, Meissner remarks that ozone does not appear, being entirely consumed; but antozone is much more copiously produced than in the slow combustion. The fume yielded by phosphorus burning in moist air, though containing phosphoric acid, is the antozone cloud. Dry phosphorus, burning rapidly in dry air, gives a cloud consisting of phosphoric acid alone; but it is very different in aspect and permanency from that produced when the burning phosphorus floats on water within a bell-glass. If a bottle full of the cloud thus formed be briskly agitated after a little water has been introduced within it, the $PO_5$ is shortly absorbed; but the mist caused by antozone remains. When a current of air, charged with the products of the rapid combustion of phosphorus, is transmitted through strong potash-lye and then through water, the phosphoric acid is mostly retained; but the cloud is increased in quantity and density. Meissner detected no formation of $HO_2$ in this case.

Since all the ozone produced when phosphorus burns rapidly is consumed by the phosphorus, no oxydation of nitrogen can occur, and, in fact, none of the oxyds of nitrogen are discoverable in the products.

It is known that phosphorus, burning with flame in a limited volume of air, does not wholly exhaust the latter of oxygen. This is due to the fact that phosphorus cannot combine with antozone, but only with ozone: when, therefore, the former alone remains, the combustion must cease. That nearly all the oxygen is nevertheless taken up by the phosphorus, results from the rapid reversion of antozone to ordinary oxygen at a high temperature, so that the same oxygen is probably polarized and depolarized over and over again, until the temperature so far falls that antozone acquires a considerable degree of permanency, when depolarization ceases.

At ordinary temperatures, phosphorus is capable of removing oxygen completely from the air, if *sufficient time* be allowed; i. e. if the antozone first formed be allowed to revert to common oxygen, which is again polarized, whereby half (?) of it is converted into ozone, which unites to the phosphorus, and this process repeats itself until all oxygen passes into combination, or until only a minute, and to our tests inappreciable, residue remains uncombined.

In the slow oxydation of turpentine and other volatile oils, it appears that oxygen is polarized, the ozone being completely absorbed by the oil, resin and other products resulting: while antozone, which does not oxydize the oil, remains free or is dissolved in it as $HO_2$. This view is sustained by the following experiments. When a stream of air is passed through freshly-distilled and pure turpentine, the latter is oxydized, as when exposed to air, but more rapidly; but no ozone can be detected. Antozone, however, is contained in the oxydized oil: at least, the latter gives, as Schönbein has shown, the reactions of $HO_2$. When electrized but deozonized air is transmitted through the oil, it is oxydized precisely as by a stream of ordinary air; the antozone produced by electrization being without effect. Ozone, however, oxydizes and resinifies the oil with extraordinary rapidity.

The same process doubtless occurs in the slow combustion of ether.

In case of zinc, both the ozone and antozone unite with the metal.

The combustion of hydrogen is a process which Meissner has likewise studied experimentally. Burning a jet of this gas under a bell-glass which is connected with an air-pump having intervening receivers, it is observed that when, by pumping, a rapid stream of air is carried along the flame and afterward through water, the antozone cloud is produced without difficulty.

There is no doubt, adds Meissner, that hydrogen, when exploded with oxygen by the electric spark, polarizes the latter substance; and Priestly has observed, in such an experiment, the formation of a dense white mist—the antozone cloud.

The formation of antozone in the combustion of hydrogen is proved, also, by the fact that $HO_2$ appears as a product. Böttger made the observation that the perfectly neutral water, obtained by burning pure hydrogen in the air, liberates iodine from slightly acidulated solution of KI, and likewise reduces an acidulated solution of $KO, Mn_2O_7$. Böttger (and Schönbein, also, on learning this fact,) was inclined to attribute these reactions to $HO_2$; but since the liquid retained these oxydizing and reducing qualities even after concentrating considerably at a boiling heat, and, moreover, gave Schönbein a negative result on applying his test (KI with $FeO, SO_3$), it was concluded by both these chemists that *nitrite of ammonia* is produced in this combustion; a conclusion which they both have since extended to all instances of combustion.

Meissner finds that in some cases the reactions described by Böttger are not obtainable with the water yielded by the combustion of pure hydrogen; but that when they are, the water, also, gives the reaction of $HO_2$ with Schönbein's test, provided too much $FeO, SO_3$ be not employed. Meissner further finds, by trial, that a dilute solution of $HO_2$ may be boiled a long time without losing its characteristic qualities; while $NH_4O, NO_3$ in solution is completely and very rapidly decomposed. *There remains, then, no ground whatever for the extremely improbable assumption that nitrite of ammonia is a universal product of combustion.*

The formation of the antozone mist and of $HO_2$ may be observed with any flame the same as with burning hydrogen, care being taken that the air which surrounds the flame, or is made to stream over it for the purpose of transporting these products into a suitable receiver, be not heated too strongly. The experiment succeeds easily with the alcohol-flame; but is difficult when a smokeless gas-flame is employed, on account of the high temperature of the latter.

The antozone produced in the vicinity of a flame is, in great part, destroyed again by the high temperature to which it is exposed, even when the flame is situated in the midst of a powerful current of air.

When the combustion is slow or smouldering, antozone appears in large quantities, and in presence of moisture forms the characteristic mist or cloud. Tobacco-smoke, according to Meissner, is a genuine antozone mist, though various products of combustion are suspended in it. When a cigar is "smoked" by an air-pump or aspirator, the larger share of these products of combustion may be removed by means of suitable absorbents; but the mist remains undiminished, and by passing it through water is actually increased in density. Cold tobacco-smoke, when collected in a bottle, slowly disappears in a manner corresponding to that noticed in case of the antozone cloud obtained from electrized air. The water, which is an essential part of tobacco-smoke, comes partly from the tobacco itself, partly from the mouth of the smoker. It is a fact of common observation that the smoke which is blown from the mouth is much whiter and more opaque than that which curls from the end of the cigar, and by retaining smoke in the mouth for a time, its density is increased. The fact that tobacco-smoke may be passed through water, as in the nargile, without losing its odor or narcotic effect, is due to the property of the antozone cloud to suspend and transport solid matters, which has already been noticed. Such are Meissner's views, which certainly have very many probabilities in their favor, although they are not altogether without further need of experimental confirmation.

In the third and last division of this work, the author discusses the *Ozone and Antozone of the Atmosphere.* He first reviews the observations of Halley, Kratzenstein, Saussure, and Forbes, regarding the physical structure of mist, and confirms the fact of its vesicular nature. He next discovers by experiment that while it is easy to condense moisture from any moist gas or gaseous mixture by cold or rarefaction, *it is impossible to produce a mist unless the gas is oxygen, or contains this element.* The water condensed by artificial means from pure oxygen or from the atmospheric air always exhibits the character of a cloud; that, separated from other gases or mixtures free of oxygen, always assumes directly the form of rain. Where oxygen is present, the water condenses in *vesicles;* in other cases, in *solid drops.* Meissner states, further, that air saturated with moisture gives a cloud on sudden rarefaction until the pressure is reduced to about 8 inches of barometric pressure. At this levity the cloud is, however, extremely delicate and transitory, and under a less pressure no cloud could be produced. This stand of the barometer corresponds to a height in the atmosphere, above tide-level, of 27,000 feet. According to Kämtz, the lightest and highest clouds, *cirrhi*, are formed at an average altitude of 20,000, and a greatest altitude of 24,000 feet. The densest artificial cloud is formed in the densest air, and the heaviest *cumuli* are formed within 5,000 feet from the sea-level.

From these facts, Meissner proceeds to adduce arguments in favor of the view that all atmospheric clouds are really due to antozone, and consist of antozone in its union with water. We do not propose to follow the author through the details of his discussion of this physical part of his subject. We shall confine ourselves to pointing out concisely the train of conclusions he has arrived at.

In examining the analogies that exist between the natural atmospheric clouds and the ozone-cloud of his experiments, Meissner finds that the same molecule of oxygen may exist in all of a series of conditions ranging from highly (positively) electrized oxygen (antozone), to ordinary inactive oxygen, in all of which conditions it is characterized by its hygroscopic quality. From the higher to the lower stages of antozonization it passes progressively and spontaneously, and the decline of activity, chemically or electrically speaking, is commensurate with its loss of hygroscopic capacity. It would appear, further, that the hygroscopic property of antozone is specially characterized by its producing the vesicular arrangement of the molecules of water.

That the atmospheric oxygen is always more or less mixed with antozone, or is in an antozone state, is shown, so Meissner reasons, by the results of observations on atmospheric electricity, which nearly always indicate the air to be somewhat positively excited.

Meissner here recounts the views formerly maintained by Saussure, Hube, Parrot, and others, but now abandoned, which attributed important offices to electricity in the formation of clouds. He further reminds us that fogs are always highly electro-positive, and the more so the denser they are, as Volta, Read and Schübler have observed. As regards clouds, which are nothing but fogs at an elevation, he finds that the mass of testimony leads to the conclusion that they, too, are positive.

The electrical state of the atmosphere thus favoring the assumption that it may contain antozone, we should expect to find direct evidence of the presence of the latter. Meissner has not indeed directly examined the rain of thunder storms for $HO_2$, but he reasons with much cogency that the reactions from which Schönbein has deduced the presence of nitrite of ammonia in rain-water may be attributable to $HO_2$, as we have already seen.

Evidence that antozone is produced by discharges of lightning, is not, however, wanting. The formation of a dense cloud that has been compared to tobacco-smoke, in the path or vicinity of a thunderbolt, is a matter of oft-repeated observation; and Schönbein himself has recorded two instances of this kind that fell under his notice: in one case his own house, and in the other a neighboring church, having been filled with an irritating vapor or cloud.

Meissner reviews the attempts to account for the positive electricity of the atmosphere, and in pronouncing them all failures, he puts in place of the question, "What is the source of atmospheric electricity?"—the inquiry, "What is the origin of antozone—positive-electrized oxygen—in the atmosphere?" We see at once that the former question may be regarded as answered when the latter finds a satisfactory reply. Meissner considers himself warranted in assuming that what he has shown to be true in the combustion of phosphorus and hydrogen is typical of all processes of oxydation by atmospheric oxygen, and that, accordingly, this element suffers polarization in every instance where its affinities are exerted. "A bit of phosphorus with its immediate surroundings of air and water, in which it slowly burns, is a picture of the earth with its atmosphere and the oxydable substances, together with water, upon its surface: the white fumes which rise from the phosphorus (the solid matters held by them in suspension being disregarded) are not only chemically the same, as the fogs and clouds of the atmosphere, but the mode of origin of both is identical."—pp. 345–6.

Here we finish our imperfect analysis of this book; a book which we have studied with great satisfaction, both from the importance of the topics it discusses, and the philosophic spirit everywhere exhibited by the author. We are bound to say that the assumption of the formation of *nitrite of ammonia* from nitrogen and water is refuted by Meissner, and the true origin of the oxyds of nitrogen that occur in nature is satisfactorily explained. The ozone question by these researches acquires a broader basis and more consistent aspect than it has hitherto possessed, and the new fields of investigation that are displayed through these pages are full of invitation and of promise.

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ART. III.—*On the Transparency of the Earth's Atmosphere;* by CLEVELAND ABBE.

BOUGUER in his *Traité d'Optique* and Laplace in his *Mécanique Céleste* have investigated the effect of the earth's atmosphere in absorbing the light of celestial luminaries. The latter has shown that, approximately, the logarithm of the intensity of their light varies inversely as the cosine of their zenith distance, and, more nearly, as the quotient of the refraction divided by the sine of the zenith distance.

If, then, assuming any unit of comparison, we observe the intensity $i$ at the zenith distance $\theta$, and the intensity I at the zenith distance zero, the intensity just before entrance into the atmosphere being put at $i_0$, we have,

$$\log\frac{i}{i_0}=\frac{\log\frac{I}{i_0}}{\cos\theta}=\frac{-Q.(\varrho).l}{\cos\theta},\quad\text{or}\log E=\frac{\log E_0}{\cos\theta},\qquad(1)$$

where $-Q$ is a constant to be determined, $(\varrho)$ is the density of the atmosphere at the time and place of observation, and $l$ is the height of a homogeneous atmosphere of the density ($\varrho$).

Therefore we see that $\log E_0$ varies with the barometer and thermometer. For the more accurate formula we have,

$$\log \frac{i}{i_0} = \frac{\delta\theta}{\sin\theta}.\log\frac{I}{i_0} = -H.\frac{\delta\theta}{\sin\theta}, \text{ or } \log E = \frac{\delta\theta}{\sin\theta}.\log E_0, \quad (2)$$

where $\delta\theta$ is the observed atmospheric refraction, and $-H$ a constant equal to $-\frac{n^2}{2K}.Q.l$.

This last formula depends on the assumption of a uniform temperature throughout the atmosphere, which assumption, says Laplace, can produce no great error. Therefore for the same place and standard height of the barometer and thermometer we should for all celestial luminaries have $E_0$ constant.

The numerical values of I and $i_0$ in terms of our assumed standard can be determined from two observations of $i_1$ and $i_2$ at the zenith distances $\theta_1$ and $\theta_2$.

To this end we have, adopting the approximate formula (1),

$$\log I = \frac{\cos\theta_1 \log i_1(\cos\theta_2 - 1) - \cos\theta_2 \log i_2(\cos\theta_1 - 1)}{\cos\theta_2 - \cos\theta_1}, \quad (3)$$

$$\log i_0 = \frac{\cos\theta_1 \log i_1 - \log I}{\cos\theta_1 - 1} = \frac{\cos\theta_2 \log i_2 - \log I}{\cos\theta_2 - 1}. \quad (4)$$

Bouguer has given, on pp. 79–81 of his *Traité*, the result of two observations on the brightness of the full moon, which he made at Croisic in Bretagne, as follows:

| | | | | |
|---|---|---|---|---|
| 1725, Nov. 23d, | 10h 30m | $\theta_1$=70° 44′ | $i_1$=1681 | times that of |
| " " " | 15 0 | $\theta_2$=23 49 | $i_2$=2500 | 4 standard candles. |

Whence he deduces, p. 84, that this ratio $\frac{1681}{2500}$ is the loss due to the absorptive power of 7469 toises of air having the density of that at his place and time of observation. On the result of these two observations he bases the table on page 332, where we find that

$$E_0 = +\ 0{\cdot}8123, \quad (A)$$

which number is adopted by Laplace in the tenth book of his *Mécanique Céleste.*

As Bouguer's serial formula is not equivalent to our equation (1), I have repeated the computation by (3) and (4), and find

$$E_0 = \frac{I}{i_0} = \frac{2548{\cdot}1}{3127{\cdot}0} = +0{\cdot}8149, \quad (B)$$

assuming of course the barometer to have been at 30 in. and thermometer 50° F. during the day.

On page 81, vol. xxxvi, of this Journal, will be found some observations which Mr. Alvan Clark has made, which will give us a second determination of this constant. I was in hopes of securing a longer series of observations accompanied with a record of the barometer and thermometer, but these seem to be the only ones available for our present purpose.

Mr. Clark, by diminishing the apparent diameter of the sun until it is barely visible to the eye, thus determines its brilliancy in units of the brightness of a faint sixth magnitude star. However much his results may depend on the peculiarities of his method and the delicacy of his eye, yet we may fairly consider them as comparable among themselves.

We have then

1863, April 27, Boston M. T.

| | | | |
|---|---|---|---|
| $18^h\ 40^m$ | $\theta_1=73°\ 11'$ | $i_1=783100$ | times that of the |
| 24 0 | $\theta_2=28\ 14$ | $i_2=1308000$ | faintest visible star, |

Whence we derive, assuming the barometer and thermometer to have been at standard heights,

$$E_0=\frac{I}{i_0}=\frac{1347632}{1680904}=+0{\cdot}8017, \qquad (C)$$

The difference between the values B and C may be considered as due to the diurnal and other changes in the heights of the barometer and thermometer at Croisic and Cambridgeport; to the differences in the transparency of the atmosphere and the annual and diurnal changes in the same; and to the fact that Bouguer observed the moon at night, and Clark the sun by day, therefore, the different laws of variation of temperature in the strata of the atmosphere and the brilliancy of the atmosphere as illumined by these luminaries will introduce discordances. This latter is the most important source of error; the light which we must measure in observations of this nature being the sum of those rays which penetrate the atmosphere *plus* the light of the atmosphere as illumined by those rays which it absorbs. In respect to this point it is very important that observations be made on the stars, comets and moon, as well as the sun. Mr. Clark's two observations at $18^h\ 30^m$ and $10^m$ P. M. are interesting in this relation. They are as follows. Admitting a large circle of the illuminated air which surrounds the sun he finds,

| | | | | |
|---|---|---|---|---|
| April 27th | $18^h\ 30^m$ | $\theta_1=75°\ 2'$ | $i_1=1055360$ | times that of faint- |
| 28th | 0 10 | $\theta_2=28\ 20$ | $i_2=1574400$ | est visible star. |

Treating these observations by equations (3) and (4) we find:

$$E_0=\frac{I}{i_0}=\frac{1606041}{1858843}=+0{\cdot}8640, \qquad (D)$$

the rough agreement of which, with the values A, B, C, is due to the preponderating influence of those rays of the sunlight which penetrate the atmosphere.

It will certainly be interesting to make a more elaborate investigation of this subject and to examine also the transparency of our atmosphere to the rays of heat and the chemical rays as well as to those of light. The importance of this matter in certain astronomical studies is not to be underrated; since we often notice the tendency to an erroneous estimate of the relative briliancy of a comet's nucleus and its tail as seen through the evening twilight in the most interesting part of its orbit; also in investigations upon the form of our Milky Way based upon the number of stars visible to Herschel in his guages, or observed by Bessel and Argelander in their zones.

For the present we shall merely append a table of the computed values of E for the two values of $E_0$ given in (B) and (C), together with the corresponding values abstracted from the table given by Bouguer, p. 332. The 2d, 3d, and 4th columns give the E which results from assuming $i_0=1$. The 5th column contains the quotient $\frac{E}{E_0}$ for the value (C) of $E_0$.

| $\theta$ | Bouguer's table. | Bouguer's observations. | Clark's observations. | Clark's observations. |
|---|---|---|---|---|
| 0° | 0·8123 | 0·8149 | 0·8017 | 1·0000 |
| 10 | 0 8098 | 0·8123 | 0·7990 | 0·9966 |
| 20 | 0·8016 | 0·8042 | 0·7904 | 0 9859 |
| 30 | 0·7866 | 0·7894 | 0·7746 | 0·9664 |
| 40 | 0·7624 | 0 7654 | 0·7494 | 0·9347 |
| 50 | 0·7237 | 0·7272 | 0·7091 | 0 8844 |
| 60 | 0·6613 | 0·6639 | 0 6428 | 0·8017 |
| 70 | 0 5474 | 0·5496 | 0·5241 | 0·6537 |
| 80 | 0·3149 | 0·3076 | 0·2801 | 0·3494 |
| 90 | 0·0006 | 0·0000 | 0·0000 | 0·0000 |

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ART. IV.—*On the Distribution of the Dark Lines in the Spectra of the Elements;* by Prof. GUSTAVUS HINRICHS, Iowa State University.

As soon as I heard of the great discovery of Kirchhoff and Bunsen, I felt sure that the dark lines of the elements would prove to be distributed according to simple laws, and that these laws might lead us to a knowledge of *the relative dimensions of the atoms.* But it was only quite recently that I was enabled to study the distribution of these lines in its detail, Prof. Silliman Jr. having kindly sent me Kirchhoff's two memoirs. Yet I limited my investigation to the group of the alkaline earths and iron; for whatsoever is true for some elements will also be found applicable to the remainder of them; and besides Kirchhoff's measurements are not at all to be considered as final, neither does he give all lines that may be observed: so that the material at hand can justify only a *preliminary investigation*, to be completed and modified by more complete and more accurate observations.

As it is well known that some distinguished American experimenters are engaged in a more accurate and complete survey of the spectra, I thought it proper to give the results of this preliminary research in order that the remarkable laws here found as the essence of Kirchhoff's determinations might as soon as possible be further developed.

It might seem that such an investigation could not lead to definite results, since Kirchhoff's scale is entirely arbitrary and even changing. But within a small range equal differences on this scale must correspond to equal differences in wave length, for whatever the curve may be (we have found it to coincide with a parabola), representing the wave-length as a function of Kirchhoff's millimetre-scale, within such narrow limits the curve will coincide with its tangent. We must therefore first see whether the different *groups of lines* exhibit any order.

Commencing with the *Calcium-spectrum*, we find a group of lines near Fraunhofer's G. Representing the intensity by I, the scale-division by K, the successive differences by D, we find from Kirchhoff's table—

*Group* I, $d_1=5{\cdot}0$.

| I. | K. | D. | *i.* | C. | E. |
|---|---|---|---|---|---|
| 5*c* | 2869·7 | | | 2869·7 | ·0 |
| | | 5·0 | 1 | | |
| 4*b* | 64·7 | | | 64·7 | ·0 |
| | | 10·0 | 2 | | |
| 4 | 54·7 | | | 54·7 | ·0 |
| | | 20·5 | 4 | | |
| 5*c* | 34·2 | | | 34·7 | +·5 |

It is plain that the values of D are as $1:2:4$; considering these ratios as intervals, *i*, and calculating from them and the difference the values C, which would correspond to a perfectly regular distribution of the lines, we obtain the error of this theoretical determination $E=C-K$, as given in the last column. We see, the error is almost nothing—the last one might easily be conceived as the result of the increased range. Hence we find that this group is very regular, having a principal difference of 5·0, or a simple multiple (here successive duplication) thereof.

The *second group*, stretching from 2653·2 mm. to 2605·8 mm., has a range of 47·4 mm., which appears to be rather large; yet we find (comparing lines of high and similar intensity, those of lower intensity not being uniformly given):

*Group* II, $d_2=2{\cdot}026$.

| I. | K. | D. | *i.* | C. | E. |
|---|---|---|---|---|---|
| { 5*b* | 2653·2 | | | 53·2 | 0·00 |
| { 1*d* | (52·9) | | | | |
| | | 14·4 | 7 | | |
| { 5*c* | 38·8 | | | 39·02 | +·22 |
| { 4*c* | (38·5) | | | | |
| 3*c* | (7·1) | | | | |
| | | 32·2 | 16 | | |
| 5*c* | 6·6 | | | 6·60 | −·00 |
| 3*b* | (5·8) | | | | |

The intervals are not in any simple ratio, but closely approach to 1 : 2, yet the great range of this group does not authorize us to give great weight to it.

*Group* III, $d_3 = \cdot75$. A very bright group of small range.

| I. | K. | D. | *i.* | C. | E. |
|---|---|---|---|---|---|
| 4*b* | 1533·1 | | | 33·2 | +·10 |
| | | ·6 | 1 | | |
| 4*b* | 32·5 | | | 32·45 | −·05 |
| | | 2·3 | 3 | | |
| 4*c* | 30·2 | | | 30·20 | ·00 |
| | | 1·5 | 2 | | |
| 5*c* | 28·7 | | | 28·70 | ·00 |
| | | 6·0 | 8 | | |
| 6*c* | 22·7 | | | 22·70 | ·00 |

These errors are certainly within the errors of observation. The intervals seem to be rather irregular, but combining the first two we have 4 : 2 : 8, or 2 : 1 : 4. The signification of such combinations will become apparent in the sequel.

*Group* IV, $d_4 = 1\cdot3$.

| I. | K. | D. | *i* | C. | E. |
|---|---|---|---|---|---|
| 3*d* | 1235·0 | | | 34·8 | − ·2 |
| | | 5·4 | 4 | | |
| 4*c* | 29·6 | | | 29·6 | ·0 |
| | | 1·3 | 1 | | |
| 2*d* | 28·3 | | | 28·3 | ·0 |
| | | 3·6 | 3 | | |
| 5*d* | 24·7 | | | 24·4 | − ·3 |
| | | 3·1 | 2 | | |
| 5*d* | 21·6 | | | 21·8 | + ·2 |
| | | 2·4 | 2 | | |
| 3*c* | 19·2 | | | 19·2 | 0·0 |
| | | 1·4 | 1 | | |
| 5*d* | 17·8 | | | 17·9 | + ·1 |

*Group* V, $d_5 = 3\cdot5$.

| I. | K. | D. | *i* | C. | E. |
|---|---|---|---|---|---|
| 2*e* | 894·9 | | | 895·4 | + ·5 |
| | | 10·0 | 3 | | |
| 4*b* | 84·9 | | | 84·9 | ·0 |
| | | 21·0 | 6 | | |
| 5*b* | 63·9 | | | 63·9 | ·0 |
| | | 3·7 | 1 | | |
| 3*d* | 60·2 | | | 60·4 | + ·2 |

only having an error in the extremes.

*Group* VI, $d_6 = 2\cdot3$.

| I. | K. | D. | *i* | C. | E. |
|---|---|---|---|---|---|
| 5*b* | 740·9 | | | 40·8 | − ·1 |
| | | 4·0 | 2 | | |
| 3*b* | 36·9 | | | 36·2 | − ·7 |
| | | 5·2 | 2 | | |
| 5*b* | 31·7 | | | 31·6 | − ·1 |
| | | 2·7 | 1 | | |
| 2*b* | 29·0 | | | 29·3 | + ·3 |
| | | 8·9 | 4 | | |
| 2*c* | 20·1 | | | 20·1 | ·0 |
| | | 2·3 | 1 | | |
| 2*b* | 17·8 | | | 17·8 | ·0 |

The error of ·7 is considerable in regard to $d_6$ and in general; yet it will disappear if we combine the first two intervals, thereby at the same time obtaining the simple series 4 : 1 : 4 : 1, or showing the whole group to consist of two equal parts of 5, each again composed of 4 : 1. Besides, it must be observed that the line 36·9 is faint as compared to the inclosing ones, marking plainly a subdivision as intimated.

These numbers seem to justify our first law:

I. *The mutual distances of the different lines in each separate group are multiples of the smallest distance in such group.*

To proceed further, we must remember that Kirchhoff himself acknowledges that he did not map all the lines he saw; besides, we know that many lines have been discovered which he did not see. Now the intensity of a line is certainly a less important element than its place; hence it may be that some lines are mapped while corresponding ones are not. Thereby we are permitted to group several lines into one group which we will call a *physical combination*, since it consists entirely of actually observed lines. It would, on account of the above, likewise be proper to admit hypothetical lines, thus dividing a given interval into a *virtual combination;* but since such divisions are given by the numbers representing the interval, and since we desire to keep this part of our article entirely free from any hypothesis, it being our purpose to show just what the given facts mean—we will not make any further use of such virtual combinations; what is here said will be sufficient to make the experimenter look to those places of the spectrum which are pointed out by such combinations.

The six groups of the calcium spectrum show the following remarkable physical combinations:

*Group* I, interval: observed 1 : 2 : 4.

*Group* II, range too great, interval approaching the ratio of 1 to 2.

*Group* III, observed 1 : 3 : 2 : 8

physical 4 : 2 : 8, or 2 : 1 : 4

*Group* IV, observed 4 : 1 : 3 : 2 : 2 : 1

physical 4 : 4 : 2 : 2 : 1

or 4 : 4 : 4 : 1

*Group* V, observed 3 : 6 : 1

*Group* VI, observed 2 : 2 : 1 : 4 : 1

physical 4 : 1 : 4 : 1

or 5 : 5

These numbers show:

II. *The intervals in the different groups may be expressed in very simple numbers, as* 1, 2, 3.

Thus there is a very great harmony between the intervals of the same group. If this harmony is to be admitted as a physical fact, it must extend throughout the whole spectrum of an element. And if our notion of a physical group, as containing corresponding lines, is correct, these physical groups must have corresponding intervals throughout. We find from the preceding tables:

| | Physical interval. |
|---|---|
| Group I, $d_1=5{\cdot}0$ | $\Delta_1= 2d_1=10{\cdot}0$ |
| [ " II, $d_2=2{\cdot}026$ | $\Delta_2= 5d_2=10{\cdot}13$] |
| " III, $d_3= {\cdot}75$ | $\Delta_3=14d_3=10{\cdot}5$ |
| " IV, $d_4=1{\cdot}3$ | $\Delta_4= 8d_4=10{\cdot}4$ |
| " V, $d_5=3{\cdot}5$ | $\Delta_5= 3d_5=10{\cdot}5$ |
| " VI, $d_6=2{\cdot}3$ | $\Delta_6= 4d_6= 9{\cdot}2$ |
| | $5d_6=11{\cdot}2$ |

Group II is enclosed in brackets, because its wide range made us exclude it in the preceding; besides $5d_2$ is but a virtual interval, not marked by actually known lines. $\Delta_6$ is somewhat indeterminable; yet it probably is $4d_6$ on account of the differences between Kirchhoff's scale and the natural scale of wave-lengths being for this group considerable. The whole of the brilliant group III has been considered as $\Delta_3$, it forming decidedly a natural group. The great approximation of the values of $\Delta$, distributed over almost the whole range of the spectrum, seems to justify the following conclusion:

III. *The difference in wave-length between the corresponding lines in a group is the same throughout the whole spectrum.*

It will be borne in mind that we did not select the groups to lead to such a law, but that we here—as actually was the case in finding this law—simply took the physical groups before given.

Hitherto we have never compared greater intervals than $\Delta=10$, taken in different parts of the spectrum; for such the corresponding wave-lengths will prove very nearly equal throughout. But now we must farther compare not the range of a group but the interval separating the different groups. Evidently the single lines interspersed between the groups will have the same physical signification; they may prove to be the only yet known members of less intense groups.

Comparing the values of Kirchhoff's scale with the well known wave-lengths of Fraunhofer's principal lines, we found by a graphical interpolation the following reduction to wave-length for the various single lines or groups:

| I. | K. | Wave length $G.=0^{mm}{\cdot}00042{\cdot}91+$ | Differences if $\delta={\cdot}8$. |
|---|---|---|---|
| Group I, | $28\frac{35}{70}$ | $+$ 0·0 | |
| " II, | $26\frac{53}{06}$ | 1·5 to 1·9 | $= 2\delta-{\cdot}1$ or $+{\cdot}3$ |
| 6*c*, | 2058·0 | 5·7 | $7\delta+{\cdot}1$ |
| 2*c*, | 1832·8 | 7·4 | $9\delta+{\cdot}2$ |
| 5*b*, | 1627·2 | 8·9 | $11\delta+{\cdot}1$ |
| " III, | $15\frac{33}{23}$ | 9·6 to 9·8 | $12\delta$ |
| 2*b*, | 1443·5 | 10·5 | $13\delta+{\cdot}1$ |
| " IV, | $12\frac{35}{18}$ | 12·8 | $16\delta$ |
| 3*c*, | 1029·3 | 15·4 | $19\delta+{\cdot}2$ |
| " V, | $8\frac{95}{60}$ | 19·7 | $25\delta-{\cdot}3$ |
| " VI, | $7\frac{41}{18}$ | 22·3 | $28\delta-{\cdot}1$ |
| 2*b*, | 641 | 23·0 | $29\delta-{\cdot}2$ |

Greater degree of accuracy can not be attained before we have direct measurements of these wave-lengths like those recently made by Eisenlohr. The existing facts give these differences as multiples of $\delta = \cdot 8 = 0^{mm} \cdot 000008$ between the different *groups* of lines.

The intervals appear not very regular, but smallest (equal to $\delta$) near the brilliant group III. Extending our view of physical combinations (i. e., such as are marked by actually known lines) to the whole of the spectrum, we obtain the following intervals:

Observed, 2 : 5 : 2 : 2 : 1 : 1 : 3 : 3 : 6 : 3 : 1

Physical, 2 : 5 : 6 : 6 : 6 : 3 : 1

or 7 : 6 : 6 : 6 : 4

both of which are almost as simple as possible. The latter would be formed of equal intervals, 6, if the last interval extended beyond 641 mm. K, and if some line should be discovered within the last observed interval, i. e., about midway between 2650 and 2840 mm. K. Hence the existing observations make it very probable that

IV. *The principal corresponding lines or groups of lines are equidistant* in regard to their wave-lengths.

It will be perceived that these successive laws may be considered as generalizations of the same principle. Hence it is of the utmost importance to see how far other substances give evidence of the same laws. We will first consider the other alkaline earths.

The spectrum of *strontium*, as given by Kirchhoff, is too deficient yet to be of any consequence.

The *barium* spectrum is more complete, but we have too few lines to discern the physical groups; therefore it seems best to rely on the more intense lines as corresponding ones.[1] We obtain as before:

| I. | K. | $G = 0^{mm} \cdot 00042 \cdot 91 +$ | Differences. |
|---|---|---|---|
| { 1*b* | 2502·4 | | |
| { 4*c* | ·2 | | $\delta = 1 \cdot 1$ |
| 6*b* | 2461·2 | 2·8 | |
| 2*c* | 2031·1 | | $3 \cdot 4 = 3\delta + \cdot 1$ |
| 6*c* | 1989·5 | 6·2 | |
| 1*b* | 1371·4 | 11·0 | $4 \cdot 8 = 4\delta + \cdot 4$ |
| 1*c* | 1287·5 | | $1 \cdot 1 = \delta$ |
| 3*b* | 1274·2 | 12·1 | |
| 2*a* | 1083·0 | 14·3 | $2 \cdot 2 = 2\delta$ |
| 2*a* | 1031·8 | 15·4 | $1 \cdot 1 = \delta$ |
| 1*b* | 890·2 | | $3 \cdot 1 = 3\delta - \cdot 2$ |
| 4*b* | 874·3 | 18·5 | |
| 3*a* | 719·6 | 19·8 | $1 \cdot 3 = \delta + \cdot 2$ |
| 2 | 718·7 | | |

[1] Except where single lines occur in great distances, for they probably represent the more intense ones of those parts.

Giving the intervals

| | |
|---|---|
| Observed, | 3 : 4 : 1 : 2 : 1 : 3 : 1 |
| Physical, | 3 : 4 : 4 : 4 |

These numbers thus confirm the conclusions drawn from the calcium spectrum. At the same time there appears yet one most remarkable new relation if we compare the interval $4\delta$ of barium, which interval seems to be a physical one, with the corresponding interval $6\delta$ of calcium. For we have

$$\text{Calcium,}\ \delta = \cdot 8\ \text{hence}\ 6\delta = 4\cdot 8$$
$$\text{Barium,}\ \delta = 1\cdot 1\ \text{"}\ 4\delta = 4\cdot 4$$

or only a difference of $0^{mm}$ 000004 in wave-length between numbers as large as $0^{mm}\cdot 00005$. Is this physical interval for calcium actually the same as that for barium? Only more accurate observations can decide it; but if it proves equal, very great consequences will flow out of it.

The *magnesium* spectrum is represented by Kirchhoff as but one brilliant group. We have:

| I. | K. | D. | *i* | C. | E. |
|---|---|---|---|---|---|
| 6*e* | 1655·6 | | | 55·8 | + ·2 |
| | | 6·8 | 1 | | |
| 6*f* | 48·8 | | | 48·8 | 0·0 |
| | | 14·1 | 2 | | |
| { 4*c* | 34·7 | | | 34·8 | + ·1 |
| { 6*g* | 34·1 | | | | |

showing $d = 7\cdot 0$ and conform to the first and second laws (I. and II).

Thus the four laws, first found for the calcium spectrum, are true for all the members of the group of alkaline earths, as far as their spectra are known. From this we conclude that they are essentially correct for all elements. How far this generalization is warranted we will now test by one of the most remarkable spectra: the brilliant and rich spectrum of *iron*, especially the three great groups of lines in this spectrum, groups almost without parallel both in regard to range, intensity, and closeness of the lines. Using the same signs as before, we have:

IRON, *Group* I, $d_1 = \cdot 8$, $\Delta_1 = 7d_1 = 5\cdot 6$.

| I. | K. | D. | *i.* | C. | E. |
|---|---|---|---|---|---|
| 4*b* | 1200·6 | | | 1200·6 | 0·0 |
| | | 6·7 | 8 | | |
| 5*g* | 7·3 | | | 7·4 | + ·1 |
| | | 10·5 | 13 | | |
| 5*d* | 17·8 | | | 17·8 | ·0 |
| | | 13·5 | 17 | | |
| 5*d* | 31·3 | | | 31·4 | + ·1 |
| | | 8·6 | 11 | | |
| 4*a* | 39·9 | | | 40·2 | + ·3 |
| | | 2·7 | 3 | | |
| 6*c* | 42·6 | | | 42·6 | ·0 |
| | | 3·0 | 4 | | |
| 4*d* | 45·6 | | | 45·8 | + ·2 |

| | |
|---|---|
| Intervals, observed, | 8 : 13 : 17 : 11 : 3 : 4 |
| physical, | 21 : 28 : 7 |
| or, | $3\Delta_1$ : $4\Delta_1$ : $\Delta_1$ |

thus having the simple ratio 3 : 4 : 1, and giving the physical difference $\Delta_1 = 7d_1 = 5{\cdot}6$.

*Group* II, $d_2 = {\cdot}8$, $\Delta_2 = 7d_2 = 5{\cdot}6$

| I. | K. | D. | *i.* | C. | E. |
|---|---|---|---|---|---|
| 4*d* | 1337·0 | | | 37·4 | + ·4 |
| | | 6·5 | 8 | | |
| 6*c* | 43·5 | | | 43·8 | + ·3 |
| | | 7·6 | 9 | | |
| 5*d* | 51·1 | | | 51·0 | − ·1 |
| | | 1·6 | 2 | | |
| 5*b* | 52·7 | | | 52·6 | − ·1 |
| | | 10·2 | 12 | | |
| 5*b* | 62·9 | | | 63·0 | + ·1 |
| | | 4·1 | 5 | | |
| 6*d* | 67·0 | | | 67·0 | 0·0 |
| | | 5·6 | 7 | | |
| 5*b* | 72·6 | | | 72·6 | 0·0 |
| | | 7·9 | 10 | | |
| 4*c* | 80·5 | | | 80·6 | + ·1 |
| | | 4·2 | 5 | | |
| 4*c* | 84·7 | | | 84·6 | − ·1 |
| | | 4·7 | 6 | | |
| 6*c* | 89·4 | | | 89·4 | 0·0 |
| | | 1·5 | 2 | | |
| 5*d* | 90·9 | | | 91·0 | + ·1 |
| | | 6·6 | 8 | | |
| 5*c* | 97·5 | | | 97·4 | − ·1 |
| | | 4·1 | 5 | | |
| 4*c* | 1401·6 | | | 1401·4 | − ·2 |
| | | 8·9 | 9 | | |
| 4*c* | 10·5 | | | 10·2 | − ·3 |
| | | 11·0 | 14 | | |
| 6*c* | 21·5 | | | 21·4 | − ·1 |
| | | 1·5 | 2 | | |
| 5*b* | 23·0 | | | 23·0 | 0·0 |
| | | 2·4 | 3 | | |
| 5*b* | 25·4 | | | 25·4 | 0·0 |
| | | 2·8 | 3 | | |
| 5*b* | 28·2 | | | 27·8 | − ·4 |

As starting point for C we made E=0 for the principal lines in the middle of the group, i. e., 67, 6*d*; and 89, 6*c*. Around these lines E are very small, but one extremity ΣE is positive, on the other negative: thus plainly indicating that the greater values of E are due to the great range—almost 100 mm. K—of the group, a range so great that the wave-lengths can not throughout fully correspond to the values K on Kirchhoff's scale.

As to the intervals, they again seem at first sight to be very capricious; yet we find

*i*, observed, 8 : 9 : 2 : 12 : 5 : 7 : 10 : 5 : 6 : 2 : 8 : 5 : 9 : 14 : 2 : 3 : 3

physical, 17 : 14 : 22 : 21 : 14 : 14 : 8

or, $2\Delta_2 + 3d_2 : 2\Delta_2 : 3\Delta_2 + d_2 : 3\Delta_2 : 2\Delta_2 : 2\Delta_2 : \Delta_2 + d_2$

the extremities of which series may be adjusted by extending the observation, thus giving

$$3d_2 + 2\Delta_2 : 2\Delta_2 : 3\Delta_2 + d_2 + 3\Delta_2 : 2\Delta_2 : 2\Delta_2 : \Delta_2 + d_2,$$

a symmetrical series of $7\Delta_2$, on each side of the median $d_2$, and on the left $3d_2$, on the right $\Delta_2 + d_2$ or $8d_2$; yet it may be that further observations will reduce the great interval $22d_2$ to $21d_2$ and thereby simplify the series. We must accept the observations at hand, and these show that $\Delta_2 = 7d_2$ is the physical difference of this group, and that these divisions of higher order are again simple, symmetric in regard to the middle of the whole group. It will be noticed that not only $\Delta_2 = \Delta_1$, as ought to be, but that even $d_2 = d_1$.

*Group* III, $d_3 = \cdot 9$; $\Delta_3 = 3d_3 = 2 \cdot 7$.

| I. | K. | D. | *i.* | C. | E. |
|---|---|---|---|---|---|
| 5*c* | 2001·6 | | | 2001·6 | 0·0 |
| | | 3·6 | 4 | | |
| 6*d* | 5·2 | | | 5·2 | ·0 |
| | | 2·0 | 2 | | |
| 6*c* | 7·2 | | | 7·0 | − ·2 |
| | | 33·9 | 38 | | |
| 6*c* | 41·3 | | | 41·2 | − ·1 |
| | | ·9 | 1 | | |
| 6*b* | 42·2 | | | 42·1 | − ·1 |
| | | 15·8 | 18 | | |
| 6*c* | 58·0 | | | 58·3 | + ·3 |
| | | 8·2 | 9 | | |
| 5*c* | 66·2 | | | 66·4 | + ·2 |
| | | ·9 | 1 | | |
| 5*c* | 67·1 | | | 67·3 | + ·2 |
| | | 14·9 | 17 | | |
| 6*a* | 82·0 | | | 82·6 | + ·6 |

The great value of the last E undoubtedly is caused by the great range of the last interval at the end of a great group; all the other values of E are very small.

The intervals are

Observed, 4 : 2 : 38 : 1 : 18 : 9 : 1 : 17

Physical, 6 : 39 : 18 : 9 : 18

or, $2\Delta_3 : 13\Delta_3 : 6\Delta_3 : 3\Delta_3 : 6\Delta_3$

and these intervals of second order,

2 : 13 : 6 : 3 : 6

give further 15 : 15

thus again a symmetric division—here into equal parts—of this great group.

The value of $\Delta_3$ differs considerably from $\Delta_1 = \Delta_2$; but it is obvious that $2\Delta_3 = 5 \cdot 4$ is almost the same as $\Delta_1 = \Delta_2 = 5 \cdot 6$.

These most magnificent groups of the elementary spectra thus fully confirm the four laws deduced from the calcium spectrum and extended to the alkaline earths. Hence we may conclude that those laws, in substance, will be found applicable to all elementary spectra. But until much more rich and accurate measurements, combined with direct determinations of wave-length, have been made, a further prosecution of this investigation will be useless; we therefore content ourselves with this preliminary research, hoping that future investigations based upon richer material will extend these laws, and by dividing the larger intervals, prove that these remarkable lines are not distributed at random but as regular as the stripes produced by diffraction.

As far as the observations are now at hand, the above four laws seem to point to the following one, including them all:

The dark lines of any element are regularly distributed over all the spectrum, in equidistant groups consisting of equidistant lines; but the intensity of these lines regularly increases and diminishes, so as to obliterate a number of lines and even of groups, thus producing gaps in the regular series, gaps which only by high optical powers—intense line of light and great condensing lens—can be completed.

Such observations therefore are most ardently desired, and it seems urgent to construct telescopes for this particular purpose.

If we, in the preceding, have succeeded in making the regularity of the apparently highly irregular lines probable—for they certainly show definite and simple laws in their distribution—it may naturally be asked: what *causes* this distribution, and what will probably be the reward of continued researches in this direction?

The lines can only have one of the following two sources. They are either produced by the dimensions of the solid particles or by the intervals between them, i. e., their distances. The latter is impossible, for these lines remain absolutely the same under such different circumstances as cannot but to some extent change the mutual distance of the particles. *Hence the lines must be produced by the bulk of the particles or atoms themselves, and an exact knowledge of these laws and distances must lead us to a knowledge of the relative dimensions of the atoms,* both as to length, breadth, and thickness. Thus optics will give us the form and size, as chemistry has given us the weight of the atoms. The remarkable result attained by comparing the distance between the calcium groups (4·8) and the barium groups (4·4) seems to show that *one dimension* of the atoms of these two elements is nearly—or if the above values should be found to be exactly equal—perfectly equal. How great the interest of such determinations is in regard to the constitution of the elementary bodies needs not to be accentuated. It may yet lead to an experimental demonstration of the existence of a primitive substance, the element of the elements.

How the dimensions of the atoms produce these lines is another question, and it is very difficult even merely to suggest any probable connecting link between the dimension of the atoms and the luminous wave. But this can not be any serious obstacle to the practical application to *the analysis of the elements:* for so the alkalies were decomposed by electricity although the causal connection manifested therein is but imperfectly known even at the present day.

But, however this may be, we hope those physicists who are favored by the necessary delicate apparatus will find in this unpretending preliminary investigation sufficient inducement to test—and as we think probable—to confirm and complete the result here deduced from the existing observations, that: *the dark lines of the spectra of elementary bodies are regularly distributed.*

Iowa City, March, 1864.

ART. V.—*On the structural characters of the so-called Melanians of North America;* by Dr. WM. STIMPSON.

IN the very interesting series of lectures on the Mollusca, by Dr. P. P. Carpenter, recently published by the Smithsonian Institution, under the heading of "Fam. Melaniidæ" the following passage occurs: "It is much to be regretted that American collectors, who have not been slow to avail themselves of the exuberant riches lying at their feet, which are so acceptable to European naturalists, have so generally entirely neglected the preservation and study of the opercula; and that so many points in the physiology and habits of these easily-observed animals have not yet been made known."[1]

There is only too much of truth in this remark. Not only "American collectors," but American naturalists, have been hitherto content with describing, from the shell alone, the multitudes of species of Melanians which swarm in our fresh-water streams and lakes, without any attempt to acquire a knowledge of the structure of these animals, or to determine the relations of the large and important group to which they belong. We are aware that several descriptions of the "animal" of certain species have appeared, but all of these are for the most part confined to characters of a trivial nature, such as color and proportions, while other characters, of paramount importance, have been overlooked or misinterpreted, though sufficiently easy of observation.

Indeed, as far as I am aware, no one has hitherto studied these animals sufficiently even to determine the primary characters of their sexual system: whether they are hermaphrodites, or have the sexes distinct; and, in the latter case, how the male may be distinguished from the female. And it is only by the recent publication of the invaluable work of Dr. Troschel, of Bonn, the "Gebiss der Schnecken," that we have been made acquainted with the nature of the lingual dentition of any members of the group.

My attention has been drawn to these animals incidentally while studying the characters of our North American *Amnicolæ*, and I will present here the results of the few observations which limited opportunities have enabled me to make upon two species inhabiting the waters of the Potomac River, namely, the *Melania virginica* of Say, and the *Mudalia dissimilis*[2] of Haldeman. These two species, though very distinct in the form of their shells, are so similar in the other characters of the animal

[1] Smithsonian Report for 1860, p. 206.

[2] The following appear to be synonyms of this species: *Paludina dissimilis* Say, *Anculosa nigrescens* Conrad, *A. dentata* Couth., *A. carinata* DeKay, and *A. trivittata* DeKay. All these forms are found living together in the Potomac above "Little Falls," and the exact similarity of the soft parts of the animal, in pattern of coloration, etc., in all of them, leads me to regard them as specifically identical.

that I am convinced that they cannot be separated generically upon other than conchological grounds. The same is true with regard to some other genera of the family, in which even the lingual dentition does not present distinctive characters of generic importance. But let it not be understood from these remarks that I deny the convenience and expediency of keeping separate as genera these groups founded on the characters of the shell. As long as our nomenclature is binomial and not trinomial, every well-marked group of species should be kept distinct as a genus, however trivial in importance the characters upon which it is based may seem to be; and this is all the more necessary in families containing a very large number of forms. A genus is simply the ultimate subdivision of species considered in respect to their relations to each other, and the elimination of the term "subgenus" from our categories seems to be required by the very nature of our binomial nomenclature.

The following brief account of the structural characters of the two Melanians above-named has no pretension to completeness, but is presented for the purpose of drawing attention to certain details of greater or less importance: with the hope that those who have materials at hand may be induced to follow out the train of investigation thus commenced, and particularly to make a thorough study of the sexual physiology of the group, which must prove of great interest in its diversity from that of ordinary Ctenobranchiate Gasteropods, as is indicated by the diverse character of the external sexual organs.

*General Characters.—Head.*—The head is provided with a rather large and somewhat elongated rostrum, tapering very little, and projecting normally considerably beyond the anterior margin of the foot. This rostrum or snout possesses considerable mobility, and has a strong resemblance in physiognomy to that of a hog, of which one cannot fail to be reminded while watching a crowd of these Melanians feeding at the river margin. It is somewhat contractile, and is wrinkled transversely upon the upper surface, except when protruded to its full extent, when the wrinkles disappear. In the living animal it is but little depressed, but it becomes much flattened in spirits. It is terminated by the oval disk, which is emarginated at the middle in front and behind; the mouth is indicated by a longitudinal slit at the centre, extending nearly to the posterior margin. The buccal mass is very little retractile, and its retraction is not accompanied by any invagination of the extremity of the rostrum. The jaws, according to Troschel,[3] consist of very numerous little scales, which have a polygonal, and for the most part a hexagonal, form. The teeth on the lingual ribbon are arranged in seven rows, (3, 1, 3), and their characteristics, according to the author above quoted, (whose observations on this point I have in part repeated,)

[3] Gebiss der Schnecken, i, 109.

consist in the form of the rhachidian tooth or plate, which is broader than long, rounded behind, and more or less sinuated anteriorly; the intermediate plate is of a rhomboid form, with a produced postero-exterior angle; and the inner lateral plate has fewer denticles at its extremity than the outer one. The tentacles are of moderate but variable length, tapering, and more or less pointed at their extremity. The small black eyes are situated on slight protuberances at the outer bases of the tentacula.

*Foot.*—The foot is short and thick, usually much shorter than the shell, truncated but not distinctly auricled in front, and rounded behind. The operculigerous lobe is conspicuous, and bears a corneous subspiral operculum of very few whorls, (generally only one), the nucleus of which, as was first observed by Professor Haldeman, is often broken off in adult individuals. In this case the operculum becomes elongated, projects beyond the posterior margin of the foot, and resembles that of the marine genus *Fusus.* On the right side of the body there is an impressed line, commencing at the bottom of the pallial cavity and running parallel to the median line of the back, until it curves downward behind the right tentacle, and reaches nearly to the margin of the foot.

*Pallial cavity.*—The margin of the mantle is simple in the species which have been examined, and probably so in all. At the right side of the opening of the pallial cavity it is slightly folded or sinuated, forming an afferent canal for the passage of water to the gills. The duct of the generative organ lies along the right side of the pallial cavity, in the angle formed by the juncture of the mantle with the back, and consists of two rather broad, unequal laminæ lying close against each other. Its base of attachment is narrow, and the outer edges of the laminæ are thin and sharp. Anteriorly, they taper gradually to their termination, near the aperture of the pallial cavity and close to the anus, where there is no free projecting extremity of the duct such as occurs in most Ctenobranchiata. I have been unable to discover any opening in either lamina at this point, so that the generative products are probably discharged from the inner surfaces of, and *between*, the laminæ, which conduct them like a channel toward the external element. The rectum lies in the usual position, parallel with and close above (to the right of) the generative duct. The fæces are not voided in a continuous worm-like cylinder, as in many gasteropods, but the cylinder is divided transversely, in the rectum, into sausage-shaped sections.

*Branchiæ.*—The gills, as in most other Ctenobranchiates, are two in number, lying upon the inner surface of the mantle. The principal or comb-like gill[4] is much elongated, extending to the

[4] It is difficult to imagine upon what H. & A. Adams base their character of the gills of the Melaniidæ,—"gill composed of rigid, *cylindrical plates.*" (Gen. of Recent Moll., i, 293.) The italics are my own.

bottom of the pallial cavity. Its width anteriorly is about two-thirds that of the rostrum, but it becomes gradually narrower posteriorly. It consists of fifty or more thin triangular laminæ, the height of which is less than their length at the base of attachment. The supplementary or feather-shaped gill[5] is rudimentary, reduced to the simple mid-rib, which forms a short, linear, fleshy ridge between the left commissure of the mantle and the base of the principal gill, to which it is parallel.

My specimens are not in sufficiently good condition to enable me to trace the position of the mucus-gland (the "organ of viscosity" of Cuvier) and its aperture.

*Sexual differences.*—The Prosobranchiate Gasteropods are distinguished as an order from the Pulmonates and Tectibranchs, by having the opposite sexual organs in different individuals, instead of existing together in the same individual. Prior to the more recent investigations into the structure of the genital system in this order, the animals of a considerable number of the families composing it were supposed to be hermaphrodites. For example, Cuvier states that the sexes are united in the mollusks composing his orders Scutibranchiata, Cyclobranchiata, and Tubulibranchiata. And even some of the most recent authorities still continue to regard some of these animals as hermaphrodites. Carpenter, in his "lectures," published in 1861, says that in the order Scutibranchiata (in which he includes the Trochidæ, Turbinidæ, etc.) "the arrangements for the continuance of the species, instead of being separated on different individuals, are united in the same individual, which is supposed to be capable of self-impregnation."[6] And Moquin-Tandon, in his work on the terrestial and fluviatile Mollusks of France, and in a special article, describes an organ in the generative apparatus of *Valvata*, which he considers to be an hermaphrodite gland.[7] So there are said to be hermaphrodite Littorinæ, etc.

But the Trochidæ and Turbinidæ and all of the Scutibranchiata are now known to have the sexes distinct; that the same is the case in the Cyclobranchiata, has been shown by R. Wagner[8] and Milne Edwards;[9] and that the Tubulibranchiata are not hermaphrodites, has been established by the observations of von Siebold[10] upon *Vermetus*, the exactitude of which has been

[5] It should here be remarked, however, that the function of this organ is a matter of controversy. Some have considered it a color gland. But it has been generally regarded as a supplementary gill; and in those genera in which it is most largely developed, as *Buccinum*, *Natica*, and *Cypræa*, it certainly has a branchiiform structure. I am inclined to regard it as a true gill, homologous with the right gill of *Haliotis*, which has the same structure and apparently the same normal position, if we may regard the pallial cavity as tubular, and revolved a little to the left.

[6] Smithsonian Report for 1860, p. 211.

[7] Journal de Conchyliologie, iii, (1852), 244.

[8] Froriep's Neue Notizen, vol. xii, No. 249, (1839), p. 98.

[9] Annales des Sciences Naturelles, [2], xiii, (1840), 376.

[10] Vergleichende Anatomie der wirbellosen Thiere, (1848).

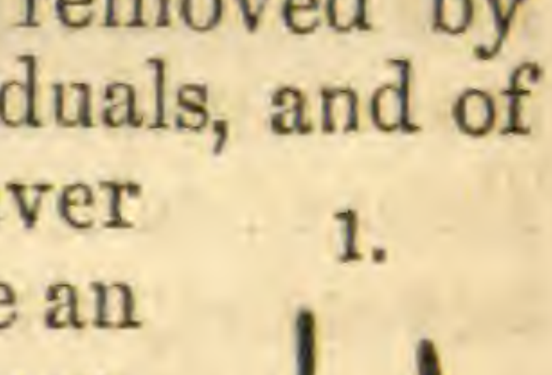
recently confirmed by Lucaze-Duthiers.[11] Indeed, wherever the generative organs of any of the Prosobranchiates have been submitted to a thorough examination, the sexes have been found to be distinct, and it may now be considered safe to conclude that they are so in all of them.

In the groups to which the Melanians have been supposed to be nearly allied, hermaphrodism has never been suspected. In *Rissoa*, *Amnicola*,[12] *Viviparus*, etc., the sexes are easily distinguished, the male being provided with a large and conspicuous external organ of generation, usually projecting separately from the side of the body behind the right tentacle, but in one group (the Vivipari) united to that tentacle, giving to it much greater thickness than occurs in the opposite one. In view of these facts, the occurrence of a copulatory organ in the Melanians also might well have been expected. Yet I did not succeed in finding it in any of my specimens; and the best endeavors to discriminate the sexes by dissection of the internal organs of generation were baffled by their similarity in all individuals, until the microscopic test was applied, when all doubts were removed by the discovery of ova in that organ in some individuals, and of spermatic particles in others, while the two were never found together; proving the organ in one case to be an ovary, and in the other a testis. The form of the spermatic particles in *Melania virginica* is nearly represented in fig. 1; the tails should be more slender.

1.

The fact being thus ascertained that the sexes are distinct in these animals, notwithstanding the agreement in form of the respective organs of generation, and the absence of a copulatory organ in the male, search was made for some external character by which the sexes might be distinguished, and it was shortly found that all those individuals in which the genital organ contained ova, had a conspicuous slit or sinus in the right side of the foot, about midway between the tentacle and the operculigerous lobe; while in those in which the organ contained spermatozoa, this sinus was entirely absent. Fig. 2 illustrates the position of the sinus in the female of *Mudalia dissimilis*.

2.



The female Melanian, then, may be distinguished from the male by the presence of this sinus. It consists of a deep pit on the impressed line mentioned in the description of the body and foot, from which a canal, formed by a fold of the superior parieties of the pedal muscles, extends downward nearly to the margin of the pedal disc. My observations have not extended far enough to enable me to ascertain

[11] Ann. des Sci. Nat., [4], Zool., xiii, (1860), 209.

[12] *Amnicola* has even been included in the family Melaniidæ by Lea. (Observations on the genus Unio, etc., vol. ix, (1862), p. 40).

the function of this lateral sinus; but from its occurrence in the female only, it is undoubtedly connected either with the fecundation, or the deposition, of the eggs. Before discovering it to be a sexual character, I had supposed this sinus to be connected with the water-circulatory system, and to be a water-pore like that seen in the pedal disc of *Conus* and the higher Gasteropods generally; this view having been favored by the fact that a strong and regular pulsation at the margins of the sinus may sometimes be observed in the living animal.

In view of these remarkable characters of the sexual system, the Melanians (the American species, at least,) must be separated from the ordinary Ctenobranchiate Gasteropods as a group of far more than family importance; for by the entire absence of an intromittent organ in the male, which must be connected with a very peculiar, and as yet unknown, method of impregnation, they diverge greatly from the other families of the group, and approach the Cyclobranchiata. Von Siebold indeed proposed to divide the entire order of Prosobranchiata into two sections, in one of which the copulatory organs are wanting (Cyclobranchiata, Scutibranchiata, (Cuv.) Tubulibranchiata, and Cirrobranchiata), while in the other they are highly developed.[13] Such a subdivision is, however, forbidden by the affinities shown by the sum of all the other characters of the animals under consideration, while at the same time the character so prominently brought forward by von Siebold has undoubtedly sufficient importance to suggest the propriety of dividing the Tænioglossate Ctenobranchiata into two groups, founded upon it.

The only family of Ctenobranchiates in which the absence of a male organ has previously been known to occur, is that of the Vermetidæ, which have been recently made the subject of an anatomical investigation by Lucaze-Duthiers. As these are sessile animals, each individual being glued to its place upon some foreign body, and therefore unable to seek out a mate, an intromittent organ would be entirely useless to the male. Cuvier, reasoning *à priori*, deduced from this fact of their sessile habits the view entertained by him, namely, that they must have the power of self-impregnation; and he consequently separated them from the Ctenobranchiata as a distinct order, under the name of Tubulibranchiata. But the circumstance of sessile life is in reality not inconsistent with separation of the sexes. There is no difficulty in conceiving that the impregnation may take place in the way which is known to occur in the Lamellibranchiata, the spermatic particles reaching the ovary of the female through the medium of the water, into which they are discharged by the male. In our freely-moving Melanians, however, such a mode of impregnation is quite unnecessary; it is far more probable

[13] Verg. Anat. der wirbellosen Thiere, 1848.

that some direct connection takes place between the sexes; and it is highly desirable that this subject should be carefully investigated, at the proper season, by those who have opportunity of doing so.

These two groups, the (American) Melaniæ[14] and the Vermeti, the characters of which are now known, evidently constitute a distinct tribe of Ctenobranchiates, for which I would propose the name *Anandria*. Their lingual dentition is of the same type (3, 1, 3), though differing considerably in details. And it is not improbable that the Turritellidæ and some of the Cerithia must be referred to the same tribe, although I confess that the suggestion is based upon the examination of a limited number of individuals preserved in spirits. My opinion with regard to the Turritellidæ is strengthened by the remark of Forbes and Hanley in their description of this family,[15] that "the sexes are probably united;" an idea perhaps founded upon the absence of a copulatory organ in the male. And in Kiener's figure of the soft parts of *Turritella duplicata* Lam.,[16] there is represented a sinus in the side of the body and foot somewhat like that of the female Melanian.

I regret that I have nothing further to offer upon a subject of so much interest, and that materials are not now at hand for pursuing its investigation. Before closing, however, it will be proper to pass in review what has already been written upon the soft parts of our American Melanians, in order to substantiate the statements in the quotation and remarks at the head of this paper, which some may be inclined to regard as somewhat sweeping.

The first published description which I have been able to find of any species of the group under consideration, is by Professor Haldeman, the well-known author of the beautiful work on our fresh-water univalve mollusca, entitled, "A Monograph of the Limniades." In a paper "On the Melanians of Lamarck,"[17] he describes the *Melania virginica* of Say, as follows: "Animal with a truncated proboscidiform head, bearing two annulated tentacles, upon an enlargement of the outside basal portion of which the eyes are situated, but never beyond the middle of the tentacle; the mouth is provided with a double row of file-like teeth on each side; the foot is oval, not extending beyond the muzzle, slightly thickened, and of medium size; edge of the mantle continuous and simple. The exposed parts are colored with blackish upon a yellowish ground, which run transversely

[14] I have every reason to believe that the Old-World Melanians are also characterized by the absence of copulatory organs, but will not include them in the same group with the American species, until the fact is established by the study of the living animals, or at least of well-preserved alcoholic specimens.

[15] British Mollusca, vol. iii, p. 171.

[16] Kiener's "Iconographie des Coquilles vivantes;" *Turritelle*, pl. I.

[17] American Journal of Arts and Sciences, xli, (1841), 22.

across the rostrum and tentacles. Oviparous." The main fact of importance here brought out is that the animal is oviparous; the statement that the eyes are never beyond the middle of the tentacle would have been better left out, as it might lead to the supposition that they were sometimes placed near or at the middle, which is never the case; the mistake with regard to the lingual teeth is quite natural, as the armature of the tongue was little understood at that time; and the other characters mentioned (except the colors) are common to a large number of Rostrifers, but the description is satisfactory as far as it goes. In his subsequent remarks in the same paper, Prof. H. considers *Anculosa* to be intermediate between *Melania* and *Melanopsis*, and to be distinct from *Melania virginica* in the small size and discoidal shape of its foot. But, though the foot may assume this shape when contracted in the act of adhering to a rock in a strong current, it is not dissimilar to that of *M. virginica* in its normal shape, though undoubtedly somewhat shorter. On the other hand, the approximation of *Anculosa* to *Melanopsis* is erroneous, as the typical fresh-water forms of the latter group appear to have the foot prolonged anteriorly beyond the tip of the rostrum,—a character never seen in the true Melanians. The Melanopsis-group seems to be but little understood; some of the forms referred to it (the *Clionellæ*),[18] as I have recently discovered, are marine, and closely allied to the Pleurotomidæ, particularly to *Clavatula*.

The next notice on the structure of these animals occurs in Professor Baird's translation of the "Iconographic Encyclopedia," (New York, 1851), article Mollusca, vol. ii, p. 82, from the pen of Prof. Haldeman. In this work the Melaniidæ constitute the first family of the Ctenobranchiata, which is thus characterized: "The mantle is simple, without fringe or siphon; the head ends in a short trunk, and the food is vegetable, chiefly decaying Algæ." The family is made to include *Melania*, *Littorina*, *Planaxis*, *Eulima*, *Paludina*, *Amnicola*, *Valvata*, *Ampullaria*, *Leptoxis*, *Melanopsis*, *Io*, and (doubtfully) *Scalaria*;[19] thus uniting Lamarck's Mélaniens and Péristomiens, and adding a few genera from various other families. The rather comprehensive character of this "family," as understood by Prof. Haldeman, seems to depend upon the entirely unwarrantable dependence placed upon the ornamentation of the mantle edge, as a character of importance for purposes of classification. But it must here be remarked that this character, upon which Prof. H. so strongly insists in all of his papers, is one of very slight importance, as

[18] The lingual dentition in the type of *Clionella* (*C. buccinoides* = *Buccinum sinuatum* Born, dredged by me at the Cape of Good Hope) is of a very peculiar type, in some degree intermediate between that of Toxoglossa and Hamiglossa.

[19] "The mantle of Scalaria not having been described, its place remains doubtful." Hald., l. c., p. 83.

every one knows who has examined any considerable number of molluscous forms, both in gasteropods and acephals. There are even species in which the margin of the branchial siphon (perhaps the most important part of the mantle-edge) is beset with complicated fringes in some individuals, and nearly or quite simple in others, while every gradation between these two extremes may be found by the study of a sufficiently large number of specimens. "Fam. 2d, Cerithiidæ," of the work under consideration, is said to have "the general character of Melania, except that the mantle is scolloped." There are, however, plenty of Cerithia in which the mantle is simple-edged, and these animals have much more intimate relations to Melania than have most of the genera included in "Fam. 1, Melaniidæ."

Dr. Lea, in the ninth volume of his "Observations on the genus Unio," etc., and in minor papers published previously, recognizing the importance of subdividing the numerically enormous collection of forms commonly included under the "genus Melania," has very properly proposed distinct generic names for the several groups of species which are indicated by the conchological characters. Whether these characters are truly the most important that can be discovered, we do not pretend to decide; but there is no doubt that distinct types exist, around which the species may be grouped, in accordance with their affinities. That these genera run into each other, is no argument against the necessity of their acceptance, for the same may be said of very many genera in all classes of the animal kingdom, especially when we study the remains of their representatives in geological times. Dr. Lea, has, however, erroneously included the genus *Amnicola* in his family Melaniidæ.[20] That genus belongs to an entirely different group, nearly allied to the Rissoidæ, (if not, indeed, belonging to that family); the intromittent organ in the male being distinctly exserted.

The next-published observations upon the soft parts of American Melanians are those of Dr. Lewis,[21] who describes *M. subularis* Lea, and *M. exilis* Hald. The descriptions are confined to the colors and other characters to be observed in that part of the animal which is protruded from the shell in progression. It is interesting to notice that Dr. L. has observed the sinus in the side of the body of the female, although he has entirely misinterpreted its nature, and considered it a specific character. Thus he distinguishes the two species by "the presence of a sinus or fold in the sides of the foot and neck of *M. subularis*, and its absence in *M. exilis*." It is, therefore, evident that Dr. L. had before him the female of the former species and the male of the latter.

[20] Observations on the genus Unio, etc., vol. ix, (1862), p. 40.

[21] Proceedings of the Philadelphia Acad. Nat. Sci. for 1862, p. 588.

At a meeting of the Academy of Natural Sciences of Philadelphia, in February, 1863,[22] Dr. Lea "read part of a letter from Dr. Lewis, of Mohawk, New York, in which he said that he was gratified with one thing, which was not apparent to him at first. In his notes on *Melania subularis* Lea, and *M. exilis* Hald., two species of his neighborhood, he finds an evident confirmation of Mr. Lea's views about *Trypanostoma* and *Goniobasis*, to which two sections of Melaniidæ the two species belong. The soft parts affirm the correctness of Mr. Lea's generalizations from the shells. Dr. Lewis thinks the *sinus* in the sides of *subularis* is peculiar, and will be found in the whole group of *Trypanostoma* and the *granular sides* of *exilis* in the whole group of *Goniobasis*." We have here a proposition to consider the lateral sinus, which is now demonstrated to be a sexual difference, as a *generic* character. This simply shows how necessary it is to become acquainted with the general structure of the animals we investigate, before studying their generic and specific relations.

In connection with the subject of the relations of this *sinus*, it may be remarked incidentally that Dr. Lewis, in a paper published in 1861,[23] gives a description of the so-called *Amnicola lapidaria* (*Pomatiopsis*), stating that the soft parts in this species are "identical in form with *Melania*;" and in the paper in the Proceedings of the Academy of Natural Sciences of Philadelphia above cited, he gives a detailed account of his reasons for referring the species to the neighborhood of *Melania*, basing them chiefly upon the similarity in the movements of the animal, and in "the expansions and contractions of the foot in progressing." Dr. L. was here probably misled by the resemblance of certain sinuses in the sides of the foot of *Pomatiopsis lapidaria*, to that seen in the females of the Melanians. But the sinuses in the *Pomatiopsis* are of an entirely different character, having no relation to the sexual system, and being the result of a peculiar arrangement of the muscles, by which the "looping" method of progression on dry land, characteristic of that animal, is effected; and they occur on both sides of the body, and exist, of course, in both sexes.

We now come to the paper of Mr. Theodore Gill, published in the Proceedings of the Philadelphia Academy of Natural Sciences for 1863. This writer (l. c., p. 34) alludes only incidentally to the Melanians, following his predecessors in considering them to form a distinct family, which he has, however, restricted in a far more natural manner than had been previously done, as he excludes several incongruous forms formerly referred to it. He has also for the first time separated the American from the Old-World Melanians; saying, "The American Melaniidæ form a peculiar sub-family, Ceriphasiinæ," and (note under

[22] Proc. P. A. N. S., 1863, p. 26.

[23] Proceedings of the Boston Soc. Nat. Hist., viii, 255.

Amnicolidæ) "The American Melaniidæ, so far as I know, have not a fringed mantle, and consequently belong to a different group."

Finally, we have the recent paper of Professor Haldeman, "On Strepomatidæ as a name for a family of fluviatile Mollusca usually confounded with Melania."[24] Prof. H. here says that he had formerly "proposed, in accordance with the position assigned by Lamarck to his family Mélaniens, to restrict the name *Melania* to the American group," and that "it must not be forgotten that Lamarck's family of Mélaniens includes the three genera, *Melania*, *Melanopsis*, and *Pirena;* and that from its position in his system, and the structure of the European *Melanopsides* [opides], he would not have included the species (like the oriental *Melania amarula*, or the African *Pirena aurita*,) with a festooned mantle, which have gradually become the representatives of 'Melania' from the accidental circumstance that the Mollusc was first described from them. The fact that Lamarck commences his series with the large oriental species, is of little value, as he commences the genus *Planorbis* with *cornuarietis*, a discoid *Ampullaria*." But I have searched in vain through Prof. Haldeman's previous papers for a proposition to separate the American from the old world Melanians, and I must confess myself entirely at a loss to understand his allusion to Lamarck's views. That author says nothing about the character of the mantle-margin, either in his "*Systême*," or in his "*Animaux sans Vertèbres;*" nor does he base his classification, either in the higher or the lower divisions, upon such a character, which could not therefore have affected his assignment of the family to a position, even if it had been known to him. The preceding family in the "*Animaux sans Vertèbres*," and some of the succeeding families, have sometimes a fringed mantle and sometimes a simple one. And Lamarck does not commence his *series* with the large oriental species, but with two American species.[25] Also, Lamarck's reference of *cornuarietis* to *Planorbis* has no bearing upon this subject, since the genus was not founded by him, but was used long previous for the true air-breathing Planorbes by O. F. Muller and other binomial writers.

Another quotation from Prof. Haldeman's paper: "Europeans are averse to giving up the name[26] for the oriental group; and as it is a matter of little scientific importance, if the *families* are properly recognized, I now reluctantly yield it to the oriental form, and consequently withdraw the American species from it."[27] With regard to this point, it may be remarked that the restriction of the name to the old-world species is a matter not of

[24] Proceedings of the Acad. Nat. Sci. of Philadelphia, 1863, p. 273.
[25] *M. asperata* and *M. truncata*. See An. sans Vert., vi, (1822), 164.
[26] Melania. [27] Proc. A. N. S. Phil., 1863, p. 273.

opinion, but of necessity; since the type of *Melania* Lamarc, and indeed the only species mentioned by that author when he established the genus,[28] is the *Helix amarula* of Linnæus, an old-world Melanian.

I trust that it is unnecessary to reiterate here that the character of the margin of the mantle, whether fimbriated or not fimbriated, though made a family or a sub-family character by Haldeman and Gill, respectively, is of the slightest possible importance in comparison with others easily discoverable upon patient observation. That the old-world Melanians are distinct as a sub-family from the American forms, I do not deny. But the truly important distinction consists in this: the old-world species are ovo-viviparous, like *Paludina* (*Viviparus*), while the American species are oviparous. Woodward,[29] H. and A. Adams,[30] and Carpenter,[31] all state that the species of the Melaniidæ are "sometimes viviparous," but do not attempt to establish a distinction on this account between the American and the old-world species, nor do they inform us in what geographical area their viviparity has been observed. I am inclined to consider the old-world group as ovo-viviparous, from having found it to be the case in all of the species of that group accessible to me,[32] while all observations which have been made upon the American species lead to prove them to be oviparous.

Prof. Haldeman, in referring to the paper of Mr. Gill (above quoted), remarks that Mr. G. "there includes the Melaniidæ in his 'Synopsis of the families of Pectinibranchiates represented in the fresh-water streams of North America,' although he admits (note under Amnicolidæ) that they 'have *not* a fringed mantle, and consequently belong to a *different* group' from the 'true Melanians.' How, then, can they be Melaniidæ? Of this group he forms 'a peculiar *sub*-family, *Ceraphasiinæ*.' From his heterogeneous Melaniidæ he rejects certain forms, including *Melanopsis* and *Pirena*, (probably *P. atra* (Linn.) and *P. fluminea* (Gmel.,) ) to form a family Melanopidæ (and also a sub-family Melanopinæ), to which his Cera (Ceri-?) phasiinæ should belong, as Melanopsis seems to have a simple mantle."[33] But it is plain that Mr. Gill, in saying that the American Melanians belong to a different *group* from the old-world species, did not mean that they belong

[28] Mém. de la Soc. d'Hist. Nat. de Paris, Prairial an vii (1799), p. 75, and Systéme des Animaux sans vertèbres, (1801), p. 91.

[29] Manual of the Mollusca, p. 130.

[30] Genera of Recent Mollusca, i, 294.

[31] Smithsonian Report for 1860, p. 206.

[32] I have not had opportunities of examining any of the animals of this group preserved in spirits, but have found no difficulty in making out the fact of ovo-viviparity by macerating the dried animal in cabinet specimens of the shells, when the oviduct in the females, in several species, was found to be filled with young animals with four- to six-whorled shells about one-eighth of an inch in length.

[33] *Loc. supra cit.*, p. 274.

to a different family, as he distinctly states that they constitute a sub-family only; and he was quite right in considering the fringing of the mantle as not of sufficient importance to form a family difference. Indeed, it is doubtful whether this could be considered as of sub-family importance, were it not coördinate with the far more important character of ovo-viviparity alluded to above. The family Melaniidæ of Mr. Gill is far from being "heterogeneous;" it was indeed so well restricted, even upon the meagre data then at hand, that subsequent investigations do not make it necessary to alter its limits. Whether Mr. Gill's family "Melanopidæ" is sufficiently distinct in "the projecting foot of the animal and the notch in the aperture of its shell,"[34] remains to be determined by future investigation. But, in any case, it would be quite unscientific to approximate this group to the American Melanians, on account of their simple pallial margin.

[*Note.*—While the above article is in press, I have an opportunity, through the kindness of Mr. Binney, to examine the soft parts of a single specimen of the *Io fusiformis*, preserved in spirits. Here I find the glandular laminæ of the generative duct to be arranged nearly as in *Melania virginica* (p. 43). The left or inner lamina is distended with the generative products, while the outer one is folded over it like a sheath.]

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ART. VI.—*The original accounts of the displays in former times of the November Star-Shower; together with a determination of the length of its cycle, its annual period, and the probable orbit of the group of bodies around the sun;* by H. A. NEWTON.

[Concluded from vol. xxxvii, p. 389.]

IN the last volume of this Journal (pp. 377–389) were given the accounts of displays of the November star-shower on thirteen different years, from A. D. 902, to A. D. 1833. From them may be obtained some important conclusions.

1. *The length of the annual period.*—The middle of the first display may be considered as Oct. 13th, A. D. 902, at 5 o'clock, A. M., Italian time. The same hour, New Haven time, may be taken for the middle of the last shower, Nov. 13th, A. D. 1833, or in old style, Nov. 1st. Between these two dates were 931 years, of which (in old style) 233 were leap years. There are 19 odd days, and six hours are to be added for the difference of longitude. This interval contains 931 periods. Hence, one period is $365 + (233 + 19{\cdot}25) \div 931$, or $365{\cdot}271$ days.

2. *The length of the cycle.*—A glance at the dates shows that there is a cycle of about one-third of a century, and that during a period of two or three years at the end of each cycle we may

[34] Gill, *loc. cit.*, p. 34. The *Clionellæ*, as stated in a previous paragraph of this paper, should be rejected from this group, being marine species allied to *Pleurotoma*.

expect a shower. The two showers in A. D. 1832, and 1833, indicate that the latter was toward the close of this short period. In like manner, the two showers in A. D. 902 and 934, being only 32 years apart, belong evidently, the former toward the close of the period, and the latter toward its beginning. The two years A. D. 902, and A. D. 1833, occupy then, approximately, corresponding places in the cycle. The interval, divided by 28, gives 33·25 years for the length of one cycle.

3. Various facts respecting these showers are shown by the following table. The first column contains the number of the display as given in the earlier part of this article, the second the year, and the third the day and hour of what I consider the historic date of the shower. The hour is somewhat arbitrary. I have assumed that the maxima of the displays recorded in the European annals were 5 o'clock, A. M., Paris time, or 17 hours from the preceding noon. This of course may involve an error of some hours, which is to be borne in mind in considering the remainders in another column. For the showers taken from the Chinese records, seven hours are subtracted from the seventeen to allow for the difference of longitude; and for the two American showers, four and five hours are added. No. 8 was observed both in Europe and China, and No. 6 at Baghdad. Hence, three hours are taken from each date. The shower of 1832 was mainly east of Paris, and one hour is therefore subtracted.

In the fourth column is the earth's longitude at each date, computed by Le Verrier's tables (*Annales de l'Observatoire*, iv, 102–206). These longitudes are approximately represented by the formula $a - nt$, where $a$ is $51° 17'{\cdot}7$, $n$ is $1'{\cdot}711$, and $t$ is the number of years from the time of the shower to Jan. 1st, A. D. 1850. These values of $a - nt$ are given in the fifth column. Subtracting them from the corresponding longitudes in the fourth column, gives the remainders in the sixth column.

We may suppose a cycle to begin midway between the displays of November, 1832, and November, 1833. If the beginning of a year be reckoned from the day of the shower, the beginning of the cycle may be written 1832·50. Subtracting now from this date multiples of 33·25 years, we have the numbers in the seventh column for the dates of the beginning of the cycles. Subtracting now these from the years given in the second column, and we have the remainders in the eighth column. Each remainder represents evidently the number of years from the beginning of the cycle to the time of the shower.

The last column contains the sum of the perturbations of the earth's distance from the sun, caused by the moon and planets, computed by the tables of Le Verrier. The unit is the seventh decimal of the sun's mean distance, and represents about 9·5 miles.

TABLE.

| No. | A.D. | Day and hour. | Longitude. | $a - nt$. | Diff. | End of cyc. | Diff. | Perturb. |
|---|---|---|---|---|---|---|---|---|
| | | d h | ° ′ | ° ′ | ′ | | | |
| 1 | 902 | Oct. 12 17 | 24 16·6 | 24 18·1 | − 1·5 | 901·50 | + ·50 | −238 |
| 2 | 931 | 14 10 | 25 57·5 | 25 7·7 | +49·8 | 934·75 | −3·75 | +497 |
| 3 | 934 | 13 17 | 25 31·6 | 25 12·8 | +18·8 | 934·75 | − ·75 | +467 |
| 4 | 1002 | 14 10 | 26 44·8 | 27 9·2 | −24·4 | 1001·25 | + ·75 | +366 |
| 5 | 1101 | 16 17 | 30 2·4 | 29 58·6 | + 3·8 | 1101·00 | 0 | +126 |
| 6 | 1202 | 18 14 | 32 25·5 | 32 51·4 | −25·9 | 1200·75 | +1·25 | +622 |
| 7 | 1366 | 22 17 | 37 47·9 | 37 32·0 | +15·9 | 1367·00 | −1·00 | −621 |
| 8 | 1533 | 24 14 | 41 11·7 | 42 17·8 | −66·1 | 1533·25 | − ·25 | − 48 |
| 9 | 1602 | 27 10 | 44 18·9 | 44 15·9 | + 3·0 | 1599·75 | +2·25 | −381 |
| 10 | 1698 | Nov. 8 17 | 47 20·6 | 47 0·1 | +20·5 | 1699·50 | −1·50 | −269 |
| 11 | 1799 | 11 21 | 50 1·6 | 49 52·9 | + 8·7 | 1799·25 | − ·25 | −146 |
| 12 | 1832 | 12 16 | 50 49·0 | 50 49·4 | − 0·4 | 1832·50 | − ·50 | + 37 |
| 13 | 1833 | 12 22 | 50 49·5 | 50 51·1 | − 1·6 | 1832·50 | + ·50 | +316 |

4. *The mean motion along the ecliptic of the node of the orbit of the group.*—The mean longitudes of the node are approximately represented by the numbers in the fifth column. Hence, the coefficient 1′·711, by which these numbers were obtained, represents the annual procession of the node along the ecliptic, longitude being reckoned from the mean equinox. The remainders in the sixth column represent the distances of the earth from the mean positions of the node at the times represented in the second and third columns. These may be changed into time by allowing 2′·5 for each hour. Errors of the assumed historic dates are of course included in these remainders.

5. *The length of the part of a cycle during which showers may be expected.*—The remainders in the eighth column of the table show that the display in A. D. 1366 was one year before the beginning of the cycle, as above determined, while that of A. D. 1202 was a year and a quarter after it. Both were extraordinary showers. Hence the length of that part of the cycle during which extraordinary displays may occur, is at least 2·25 years. The minor displays, Nos. 2, 9, and 10, indicate that unusual numbers of shooting stars, sufficient to attract attention, may be seen through a period of five or six years, at least.

6. *Does a ring around the sun, of* UNIFORM *density through its circuit, properly represent the shape of the group?*—I think not. It is not absurd to suppose that the earth passes very near to a ring of bodies, and that by the action of the planets and moon on the earth and the ring, we are sometimes thrown into it, and sometimes pass by without meeting it. Let us see whether this is probable.

If the planetary and lunar perturbations of the earth's radius vector were at the time of the showers always very large, and of the same sign, it would suggest the existence of a ring passing at the node, just outside, or just inside, of the earth's orbit. But the last column of the table shows no regularity. On the contrary, while the sum of the maxima of all the plus perturbations of the radius vector is 884, and that of the minus perturbations is 958, we have two remarkable showers occurring, one in A. D. 1202, when the increase was 622, and one in A. D. 1366, when the decrease was 621. If then a uniform ring, crossing the ecliptic just inside or just outside of the earth's orbit, be supposed, the cause of the periodicity cannot be the perturbations of the earth's radius vector. The greatest possible perturbation in either direction being only about 9000 miles, it is highly improbable that the earth passes sometimes inside and sometimes outside of a thin ring.

We might however suppose that the ring sways back and forth, and thus produces the periodicity. The mean motion of the bodies of the ring and its eccentricity are unknown, and hence we cannot speak positively of its irregularities. But it is probable that its radius vector would not suffer larger perturbations than that of the earth. The lunar action is wanting, while the earth ought to work the same effect each year. The motion is retrograde, as will be shown, hence the action of the planets ought to be less. On the other hand, there is needed a very large action, sufficient to entirely mask the irregular perturbations of the earth's radius vector.

A revolution of the apse of the ring in 33·25, or even in 66·5 years, could explain the periodicity, but this motion would be enormously greater than those usual in the planetary system. *The supposition of a ring of uniform density through its circuit seems then very improbable.*

7. The appearance of meteors in rather more than usual numbers on the 13th and 14th of November of other years of the cycle than the two or three near its beginning, suggests however a ring of some kind as the true form of the group. The probability of this shape is strengthened by the analogy of the August meteors, whose regular recurrence seems to imply that that group is a ring.

We may indeed suppose a globular group, whose mean motion is exactly that of the earth, and that perturbations in longitude produce the periodicity. But what planets, acting on a body whose periodic time is exactly one year, can cause a large equation of longitude, having a period of 33·25 years?

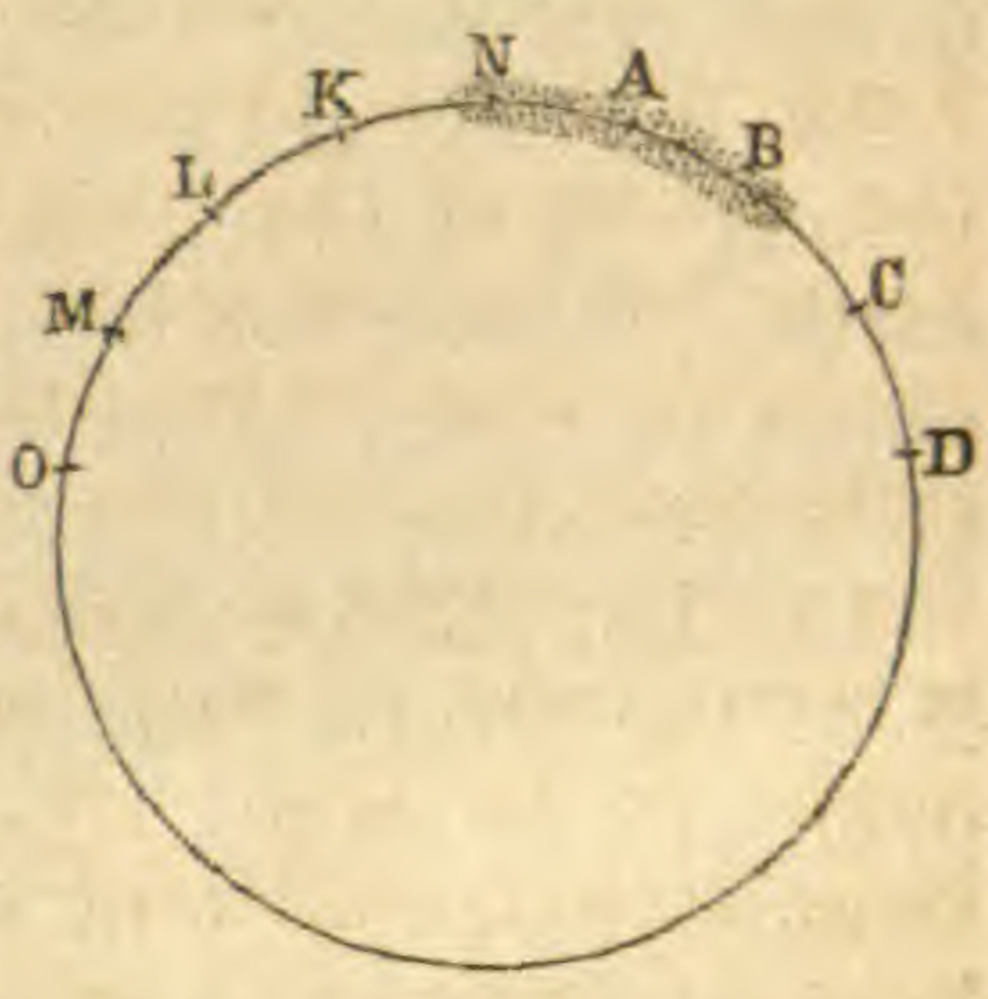
8. If the group is a ring, it has then, in all probability, very unequal density through its circuit. We pierce through it every year, and each time in a new place. It is only in those parts of the ring that are at the node with us near the end of the cycle, that we meet with very large numbers of shooting stars. The most natural supposition is that there is a small section of the ring where the bodies are numerous, a few stragglers being scat-

tered along the rest of its circuit. In short, we are to conceive the group as a cloud of small bodies, extending along the arc of an ellipse whose focus is the sun, that arc being the base of a sector equal to one-tenth, or one-fifteenth, of the ellipse. In this limited portion, however, there may be great inequalities, or even discontinuity.

9. If the motion of a body in an orbit inclined a few degrees to the ecliptic is retrograde, the longitude will evidently increase with the time. If the motion is direct, the longitude of the nodes will decrease as time increases. *The motion of the group of bodies to which the November meteors belong, is therefore retrograde.*

10. Let NABC represent the orbit of the group, cutting the ecliptic in N. Let the earth pass through the middle of the group on a certain year. One year later, the centre of the group will not be at N, but at some other point, as A, or K, having perhaps meanwhile described one or two revolutions. On following years, the centre of the group will be at B, C, D, &c., or at L, M, &c., the distances NA, AB, BC, &c., being arcs of the orbit described in equal times. The length of the group must be at least two and one-fourth times the distance NA, since a shower may occur during a period of at least two and one-fourth years.

1.




Now it is evident that NA is $\frac{1}{33\cdot25}$th part of the whole orbit. For, after a complete cycle, the group must be near N when the earth is there; and the series of points A, B, C, &c., cannot go more than once around the orbit in one cycle without causing more than one period in each 33·25 years.

11. The number of complete revolutions in a year cannot exceed two. For an orbit whose periodic time is $\frac{1}{3}$ has a major axis $2(\frac{1}{3})^{\frac{2}{3}}$, which is less than unity, or less than the distance from the sun to the earth. In one year, then, the group must describe either $2 \pm \frac{1}{33\cdot25}$, or $1 \pm \frac{1}{33\cdot25}$, or $\frac{1}{33\cdot25}$ revolutions.

12. These five velocities are not all equally probable. Some of the members of the group that visited us last November gave us the means of locating approximately the central point of the region from which the paths diverge. Mr. G. A. Nolen has, by graphical processes specially devised for the purpose, found its longitude to be 142°, and its latitude 8° 30′. This longitude is very nearly that of the point in the ecliptic toward which the earth is moving. Hence, the point from which the absolute motion of the bodies is directed (being in a great circle through the other two points), has the same longitude. The ab-

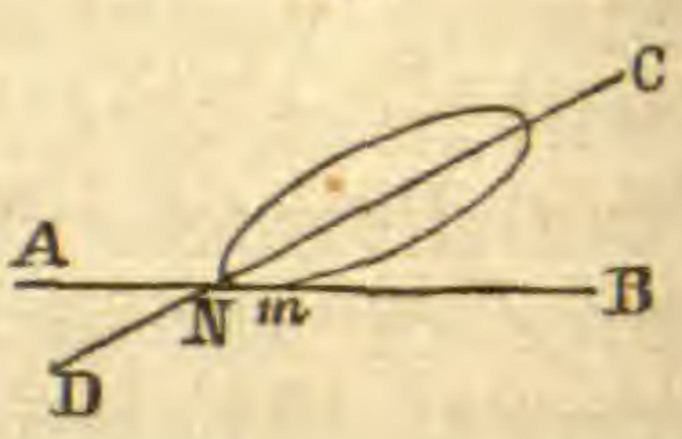
solute motion of each meteor, then, is directed very nearly at right angles to a line from it to the sun, the deviation being probably not more than two or three degrees.

Now if in one year the group makes $2 \pm \frac{1}{33\cdot25}$ revolutions, there is only a small portion of the orbit near the aphelion which fulfils the above condition. In like manner, if the periodic time is 33·25 years, only a small portion of the orbit near the perihelion fulfils it. On the other hand, if the annual motion is $1 \pm \frac{1}{33\cdot25}$ revolutions, the required condition is answered through a large part of the orbit. Inasmuch as no reason appears why the earth should meet a group near its apsides rather than elsewhere, we must regard it as more probable that the group makes in one year either $1 + \frac{1}{33\cdot25}$, or $1 - \frac{1}{33\cdot25}$ revolutions.

13. If either of these two mean motions are correct, the absolute velocity of the meteors will be very nearly equal to that of the earth. Hence, the inclination of the ring to the ecliptic will be about twice the latitude of the radiant, or 17°, the motion being, as before shown (9), retrograde.

14. Let AB be the earth's path near the node N of the orbit of the group, and let CD be the orbit of the group. Let the directions of the motions be from A to B, and from C to D. It is reasonable to suppose that the shape of a section of the group made by a plane tangent to the two orbits, will be an oval. Also, a curve in that section representing parts of the group of equal density, will be an oval. If now the group has not arrived at N when the earth is there, the maximum display of meteors is to be expected later in the year, that is, when the earth is at *m*. On the contrary, if the group has passed the node before the earth reaches it, the maximum display is to be expected earlier in the year.

2.




Now if the group makes $1 + \frac{1}{33\cdot25}$ revolutions in a year, its positions on successive years will pass from C toward D. Hence, in that case, in the earlier part of the two or three years during which a shower is to be expected, the display would be later in the year than usual, and conversely.

On the other hand, if the group describes $1 - \frac{1}{33\cdot25}$ revolutions in a year, its position on successive years will pass from D to C, and hence in the earlier part of the two or three years the display is to be expected earlier than usual, and conversely.

By the former of these two suppositions, we ought evidently to have the sign of each remainder in the sixth column unlike that of the corresponding remainder in the eighth column. By the second supposition the signs should be alike. It ought rather to be said that there is a tendency to produce these effects, for the sixth column contains unavoidable errors resulting from the inexact historic dates. An examination of the two columns shows a decided tendency toward opposition of sign of the remainders, and hence the first must be considered as the more probable velocity. This opposition of signs and its significance was pointed out to me by Mr. J. W. Gibbs, to whom I am indebted for other valuable suggestions.

15. If the annual motion of the group is $1 + \frac{1}{33\cdot25}$ revolutions, we may determine the orbit. Between the shower of A. D. 902, and that of A. D. 1833, were $365 \times 931 + 233 + 19{\cdot}25$, that is, 340067·25 days. During this time the group has made $931 + 28$ revolutions, excepting a small fraction of one revolution to be determined from the procession. The procession is 26° 33′, of which 12° 58′ is due to the precession of the equinoxes. The remainder is to be reduced to the plane of the orbit, and regarded as described with a radius vector greater than the mean distance. Hence it may be called $\frac{1}{25}$th of a revolution. The time of a sidereal revolution of the group around the sun is, then, 340067·25 ÷ 958·96, or 354·621 days.

16. The accuracy of this periodic time is worthy of notice. The principal uncertainty arises from the assumption (2) that the shower of A. D. 902 occupies the same place in the cycle as that of A. D. 1833; in other words, that the earth passes through the same part of the group in those two years. An examination of the remainders in the eighth column of the table leads me to believe that the probable error of this supposition would be less than one year. Now if this error was one year, plus or minus, the periodic time would be $\frac{340067{\cdot}25 \pm 365{\cdot}271}{958{\cdot}96 \pm 1}$, or $354{\cdot}621 \pm {\cdot}011$ days. This error of the periodic time would be only about 16 minutes. There is a similar accuracy in each of the other possible periodic times.

The cause of this accuracy is evident. The periodic times of the group and of the earth are like the two divisions of an immense vernier, extending back nearly a thousand years, and we measure from the twenty-ninth coincidence.

17. Each body of the group must have its own elliptic orbit about the sun, this orbit being, of course, slightly modified by the action of the rest of the group. *The major axes of all these ellipses are equal.* For otherwise the bodies would soon scatter themselves along the whole circuit of the ring, and there would be a display every year. A very slight deviation from a common velocity might be corrected by the attraction of the group.

18. *The elements of the mean of these orbits.*—The semi-major axis is evidently $\left\{\frac{354{\cdot}621}{365{\cdot}256}\right\}^{\frac{2}{3}}$, or 0·98049, the mean distance of the earth being unity. The earth's radius vector, at the time of a shower, represented below by $r$, is 0 98887. The velocities of the earth, and of any member of the group, are represented

(Laplace, *Méc. Cél.*, i, 190) by the formula $\left(\frac{2}{r} - \frac{1}{a}\right)^{\frac{1}{2}}$, where $a$ is the semi-major axis. These are, respectively, 1·0112 and 1·0013, the earth's mean velocity being unity. The eccentricity of an orbit is obtained by the formula (*Méc. Cél.*, p. 191),

$$a(1-e^2) = r^2 \sin^2 \varepsilon \left\{ \frac{2}{r} - \frac{1}{a} \right\},$$

where $e$ is the eccentricity, and $\varepsilon$ is the angle which the motion of the meteor makes with a line from it to the sun. Reducing, we have $e^2 = \cos^2 \varepsilon + \left(\frac{r-a}{a}\right)^2 \sin^2 \varepsilon$.

The value of $\varepsilon$ is not well known, but its mean value does not probably (12) differ more than two or three degrees from a right angle. We may then consider $\sin \varepsilon$ as unity. The fraction $\frac{r-a}{a}$ is ·00853. Hence, if we take the base of a triangle equal to ·00853, and the perpendicular equal to $\cos \varepsilon$, the hypotenuse will be equal to the mean value of the eccentricity. The ring is then nearly circular. Its inclination is, as before stated, about 17°; that is, twice the latitude of the radiant.

19. The velocity with which these bodies enter the atmosphere, is easily computed. Allowing for the earth's attraction, it is about 20·17 English miles per second, or 32·44 kilometers, considering the earth's mean distance 95,000,000 miles.

20. The length of the group, as it is at least one-fifteenth of the length of the orbit, must be more than 40,000,000 of miles. If a shower lasts five hours, the thickness of the ring would be the distance passed over by the earth in that time multiplied by the sine of the inclination of the orbit, or more than 100,000 miles.

21. Since the periodic time is limited to five possible values, each capable of an accurate determination, and since therefore from the position of the radiant the inclination of the orbit can be found, it seems possible to compute the secular motion of the node for each periodic time with considerable accuracy. Since now the actual motion of the node is known, we have thus an apparently simple method of deciding which of the five periods is the correct one.

22. It may be well to indicate those parts of the earth in which we have most reason to look for unusual numbers of meteors in coming years. The annual period being 365·271 days, the excess over even days is 0·271. This multiplied by 31 gives 8·401 days. The excess over eight days corresponds to about 144° of longitude. If, then, a shower occurs in A. D. 1864 (31 years after 1833), it seems most reasonable to look for its greatest display (on the morning of Nov. 14th) 144° west of our At-

lantic States, that is, in the western part of the Pacific Ocean and in Australia. In 1865, it may be looked for as central 97° farther west, or in western Asia and eastern Europe; and in 1866, on the western Atlantic.

The year in which we have most reason to expect a shower, is 1866, since the cycle of 33·25 years is probably to be reckoned from some date between November in 1832, and in 1833.

These places and times are named with hesitation—rather to guide observation, than as predictions. The causes alluded to above (14), and the possible perturbations and irregularities of structure of the group, may cause unexpected variations of time and place.

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ART. VII.—*On Molecular Physics;* by Prof. W. A. NORTON.

IT is proposed, in the present paper, to give a general exposition of a Physical Theory of Molecular phenomena, based upon the highest generalizations, and the most reliable physical conceptions to which the progress of science has hitherto conducted.[1] A theory, so comprehensive in its scope, can be completely substantiated only by undertaking a thorough discussion of the minute details of special phenomena in the several departments of physical science, and subjecting it at all practicable points to the rigid test of numerical calculation. But before any detailed discussions can be entered upon, we must deduce from the fundamental conceptions adopted the general principles of molecular action, or the laws of the molecular forces, note the characteristic features of the different provinces into which the entire field of research is naturally divided, and trace out the general relations which they bear to each other, or, in other words, recognize the mutual dependence and essential correlation of special physical forces.

The established truths and generally received ideas, which form the basis of the theory, are as follows:

1. All the phenomena of material nature result from the action of force upon matter.

2. All the forces in operation in nature are traceable to two primary forces, viz: attraction and repulsion.

3. All bodies of matter consist of separate indivisible parts, called atoms, each of which is conceived to be spherical in form.

4. Matter exists in three different forms, essentially different from each other. These are: (1) ordinary, or gross matter, of

[1] The principal features of the general theory here propounded, have, with few exceptions, been advocated by the author before the Connecticut Academy of Arts and Sciences, at various meetings of the Academy during the last six years.

which all bodies of matter directly detected by our senses either wholly, or chiefly consist. (2) A subtile fluid, or ether, associated with ordinary matter, by the intervention of which all electrical phenomena originate, or are produced. This *electric ether*, as it may be termed, is attracted by ordinary matter, while its individual atoms repel each other. (3) A still more subtile form of ether, which pervades all space and the insterstices between the atoms of bodies. This is the medium by which light is propagated, and is called the *luminiferous ether*, or the *universal ether*. The atoms, or "atomettes," of this ether mutually repel each other; and it is attracted by ordinary matter, and is consequently more dense in the interior of bodies than in free space.

5. Heat, in all its recognized actions upon matter, manifests itself as a force of repulsion.

The corner stone of a physical theory of molecular phenomena must consist in the conception that is formed of the essential constitution of a single molecule; understanding by a molecule an atom of ordinary matter indued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules. In seeking for this, the most philosophical course that can be pursued is to follow out to their legitimate conclusions the general principles already laid down. We have admitted the existence of a subtile ether, attracted by all bodies, and pervading their interstices; now if bodies attract this ether, the atoms of which they are composed must exert an attractive action upon it. Every atom must, therefore, be surrounded with an *ethereal atmosphere*, condensed upon its surface, and extending indefinitely outward. Again, it is conceded that the electric ether, or fluid, if it exists, must be attracted by ordinary matter; but if this attraction subsists, it must be exerted by the individual atoms, and therefore every atom must also be surrounded with an atmosphere of electric ether—an *electric atmosphere*, as it may be termed. We must suppose that the interstices between the atoms of this electric atmosphere will be pervaded by the more subtile ethereal atmosphere. We are thus led to conceive of a molecule as consisting of an atom surrounded with two atmospheres, ethereal and electric—the former being the more attenuated, and pervading the other. We may suppose either that these two ethers exercise no direct action upon each other, or, what is more probable, that the electric atoms attract the ethereal, and are therefore surrounded, like the central atoms of the molecules, with ethereal atmospheres. To this supposed fact we may attribute the mutual repulsion subsisting between the electric atoms; and thus restrict the fundamental property of repulsion to the atoms of the universal ether.

The conception here formed of a molecule involves the idea of the operation of the two forces of attraction and repulsion; a force of attraction is exerted by the atom upon each of the two atmospheres surrounding it, and a force of mutual repulsion between the atoms of each atmosphere. These we regard as the *primary forces* of nature, from which all known forces are derived. They determine, primarily, the physical relations of the atom to its atmospheres. In seeking for the molecular actions that may result from their operation, there are two different routes that may be taken. We may conceive that the atmospheres surrounding each atom are, naturally, in a condition of statical equilibrium, and that the primary forces with which the molecule is invested take effect at all distances, without the intervention of any medium, and unobstructedly through all intervening matter; or we may conceive the natural equilibrium of the molecular atmospheres to be a dynamical one, and that, as a necessary consequence, recurring impulses, both attractive and repulsive, are propagated outward by the surrounding ether, from each molecule, and take effect upon other molecules. Here, as before, we shall follow the indications of existing science, which, as will be generally conceded, point to a dynamical origin of the molecular forces. The ideas we have thus been led to form with regard to the real nature and mode of action of these forces, are as follows:

The *molecular forces* consist of—

1. A repulsive action of the electric atmosphere of a molecule exerted, primarily, upon the electric ether immediately exterior to it. This force of repulsion is made up of recurring impulses, which are propagated in waves through the circumambient electric ether. These impulses fall upon the electric atmospheres of contiguous molecules, are thence propagated down to the surfaces of the central atoms, and take effect upon these as a force of repulsion.

2. An attractive action exerted by the central atom of the molecule upon the electric ether surrounding it; originating a series of successive contractions of this atmosphere, and thus of inward acting impulses, which are propagated outward and form a set of attractive waves. These are received, like the repulsive impulses, upon the surfaces of contiguous electric atmospheres, and propagated to the central atoms, upon which they take effect as an attractive force. The recurring contractions of the atmosphere, here supposed, do not necessarily imply that the force which produces them acts by impulses, for every such contraction must develope a resistance, which will occasion a subsequent expansion; and, at the same time, recurring expansions should result from the similar impulses propagated from surrounding molecules. The electric atmospheres that envelop the atoms of bodies may accordingly be in a perpetual dynamical

condition of alternating contractions and expansions, or of alternating inward and outward movements of their atoms, although the primary forces acting upon these atoms should be continuous in their action.

But if we confine our attention to the action of a single atom upon its electric atmosphere, it will be seen that the expansions, which of necessity follow the contractions, must be of less extent than the contractions; for a part of the contractile force is expended in impelling a portion of the universal ether compressed upon the surface of the central atom normally outward from this surface. To the extent that this effect takes place will the contraction of the atmosphere exceed the expansion which immediately follows it, and an effective attractive force be propagated through the surrounding electric ether. We are thus led to recognize the existence of a third molecular force, viz: a force of repulsion originating in the attractive action exerted by the atom of the molecule upon its electric atmosphere.

3. A third molecular force, then, consists of a series of repulsive, or outward acting impulses, imparted to the universal ether at the surface of the atom of a molecule by the contractile force exerted by the atom upon its electric atmosphere. This repulsion is equal, at its origin, to the attraction which developes it. It is propagated in waves which, unlike the waves conveying the other molecular forces, proceed through the universal ether. These waves, if each contraction of the atmosphere were not followed by a partial expansion, would be of the character of "waves of translation," and would convey only outward acting impulses; they are, in fact, oscillatory waves, in which the outward predominate over the inward acting impulses.

The force thus originating may be regarded as the primary force of *heat*, and may be termed *heat-repulsion.* The other two molecular forces may be designated as the forces of *electric attraction* and *electric repulsion.* But they should not be confounded with the special electric forces that come into play whenever the natural quantity of electric ether associated with atoms, or present on different sides of atoms, experiences any material increase or diminution—which will be considered in another connection.

The molecular forces that have now been specified might be otherwise characterized as follows: (1.) A repulsion of the one electric atmosphere for the other, operating through the intervention of the electric ether posited between the two. (2.) An attraction of the gross atom of the one molecule for the electric atmosphere of the other, also taking effect by means of the intervening electric ether. (3.) A repulsion exerted by the atom of the one molecule upon that of the other, through the intervening universal ether, and originating in the attraction just mentioned. These forces consist of recurring impulses propagated in waves through the ethereal media, which take effect ultimately as attractive, or repulsive, impulses, upon the central atoms of molecules. The law of diminution of the propagated forces is that of the inverse squares.

If the *ethereal*, as well as the electric, atmospheres of particles, be conceived to be in a state of dynamical equilibrium, their alternate contractions and expansions should originate oscillatory waves that would be propagated indefinitely onward through the ether of space. If we admit, with Professor Challis, that such purely oscillatory waves, when they fall upon particles, will give rise to an attraction, or a repulsion, according to the breadth of the waves, in comparison with the diameter of the particles, and that the force of gravitation may be conveyed by such waves, we have in the supposed dynamical condition of ethereal atmospheres of particles a possible origin of waves of gravitation, which cannot be found, primarily, in any supposed motion of gross atoms in an isolated condition. It should be observed, too, that the dynamical condition of atmospheres, here considered, is really a necessary consequence of the first operation of the force of attraction of atoms upon the surrounding ether, if the elasticity of the ether be perfect.

A different view of the possible nature and origin of the molecular forces from that which has been given, may be obtained by changing our stand point.

We may conceive the same three forces, viz: one of attraction, and two of repulsion, to be in operation, but we may replace the forces of electric attraction and repulsion by equivalent forces propagated through the universal ether. This may be realized as a physical conception by regarding the atoms of the molecules and those of the surrounding electric ether, each encompassed by its ethereal atmosphere, as being, or rather their atmospheres, in the dynamical condition of alternate contraction and expansion, and thus as being centres from which proceed oscillatory waves; and that as the result, in accordance with the general theory so ably advocated by Professor Challis, the electric atoms of two atmospheres may repel each other, and the central atoms which they surround may also repel each other; the general result being that similar atoms repel, and dissimilar attract. Upon this view the forces we have deduced from the dynamical state of the *electric* atmospheres, which must still be in operation, must be overshadowed by those now considered. Upon the former idea it is the forces now derived from the dynamical state of the *ethereal* atmospheres that must be overshadowed by the others. A discussion of phenomena can alone decide which of these two general views should be adopted. In the present memoir we shall chiefly occupy the ground first taken.

Among the physicists of the present day there seems to be a growing inclination to discard the notion of an electric fluid as distinct from the ether of space, and attempts have been made by Challis, Tyndall, and others, to frame a consistent dynamical theory of molecular forces and phenomena, based upon the supposed existence of only two forms of matter, viz: gross matter, and the ether of space. The fundamental position taken by these distinguished physicists is that the molecular forces, including heat, are conveyed by purely oscillatory waves, and originate in a vibratory motion of the ultimate particles of bodies. Against this idea, however plausible it may seem, and however admirable may be the ingenuity and skill with which it has been sustained, many serious objections may be urged. One or two of these may be briefly stated.

1. No possible mode of explaining the phenomena of electricity and magnetism has yet been indicated by the advocates of this theory. The electric fluid is expelled by them from the vast field it has hitherto occupied, but all attempts to supply its place have proved futile.

2. Another obvious objection is that vibratory motions of gross atoms are supposed to originate the forces by which such atoms are primarily aggregated into masses, whereas it is essential to the possibility of such vibrations that contiguous atoms should exercise a mutual action upon one another, that is, be previously aggregated. We must suppose, then, the existence originally of other forces, to bring isolated atoms together and make the supposed forces due to vibratory motions of the atoms, possible; that is, these latter forces become possible only when there is no longer any farther occasion for them. We have seen that another possible origin may be ascribed to such oscillatory waves that does not involve the physical impossibility just referred to, from which those who seek for the key to all molecular phenomena in the motions of gross atoms, can hardly escape.

3. The notion advocated by Tyndall in his admirable work on "Heat considered as a mode of Motion," that heat and light originate in a vibratory motion of ordinary atoms, involves the supposition that these atoms are capable of vibrating at the astonishing rate of six hundred trillion vibrations in a second, while the most rapid vibration of atoms, or of a collection of atoms, known to take place in the production of sound, does not exceed 24,000 per second. It may be conjectured that this immense chasm may be spanned by the idea that the ultimate particles of bodies are immeasurably smaller than any collection of atoms which may be simultaneously vibrating when bodies emit sound; and that since a musical string vibrates more rapidly in proportion as it is shorter, a single particle may vibrate at an inconceivably rapid rate by reason of its exceeding

minuteness. But the analogy here supposed does not exist, as a physical fact, and no such inference can be drawn from it; for the rate of vibration of the string depends upon the distance between its two fixed points, but in no proper sense can it be said that two particles between which another is situated, are fixed, so as to be incapable of taking on the motion imparted to the intermediate one. So far from this being the case, the displaced particle can only vibrate by reason of the reaction of the contiguous particles to the action which it exercises upon them; and in receiving this action the motion must be transmitted. If, to remove the difficulty, we conceive the particle to oscillate as if it were wholly isolated, in union with an oscillatory wave falling upon it, we then fall upon the second objection stated above, and seek in vain through the universe for the vibratory motion of atoms of ordinary matter in which this wave conveying such wonderfully rapid vibrations can have originated. We, at the same time, remove the necessary foundation for the explanation of a variety of special facts and phenomena, which require the assumption of special rates of vibration, proper to the particles of different bodies; as the different colors of bodies, &c.

Again, if the rates of vibration of ultimate particles depend upon the mutual actions subsisting between the displaced particle and those adjacent to it, the vibrations in which the heat-force is supposed to consist, should be propagated from particle to particle, just as any mechanical force is; in other words, heat should be conducted after the same manner, essentially, and at the same rate that sound is conducted by the same medium.

By ascending to the reservoir of primary force, from which all the different streams of force flow, as has been attempted in this communication, we may avoid some of the difficulties attending the rejection of the idea of the existence of an electric ether; and in many portions of the field of physical science the part played by the electric ether is so similar to that which we may suppose would be performed by the universal ether under similar circumstances, that the suspicion at times arises that all the offices now attributed to the former will eventually be found to be discharged by the latter. If so, the processes of operation will not of necessity be changed, but only the agent or medium.

Admitting that the molecular forces consist of two forces of repulsion and one of attraction, as characterized on p. 64, let us proceed to inquire into the variations that may occur in the effective action of two similar molecules, separated by various intervals of distance. Let $x=$ the distance between two molecular atmospheres; $r=$ the radius of either atmosphere; $m=$ the constant of electric repulsion, that is, the force of electric repulsion exerted upon either atom, when $x=1$; and $n=$ the constant of the electric attraction, which will also be the constant of the equal force of repulsion propagated from the surface of the atom through the universal ether. Also let $u=$ the force of electric repulsion, and $v=$ the excess of the attractive force over the ethereal repulsion developed by the attraction; all the forces being considered as taking effect upon the central atom. The effective action exerted by either molecule upon the other will be the difference between the values of $u$ and $v$. Denote it by $f$; then $f=v-u$. When the calculated value of $f$ is positive the action will be attractive; when it is negative the action will be repulsive. We have for the force of attraction the general expression $\frac{n}{(r+x)^2}$; for the ethereal repulsion the expression $\frac{n}{(2r+x)^2}$; and for the electric repulsion $\frac{m}{x^2}$. Then

$$v=\frac{n}{(r+x)^2}-\frac{n}{(2r+x)^2}=\frac{n(3r^2+2rx)}{(r+x)^2(2r+x)^2} \quad . \quad . \quad . \quad . \quad (1)$$

$$u=\frac{m}{x^2} \quad . \quad . \quad . \quad (2) \qquad\qquad f=v-u=\frac{n(3r^2+2rx)}{(r+x)^2(2r+x)^2}-\frac{m}{x^2} \quad . \quad . \quad . \quad (3)$$

As the two forces of electric attraction and repulsion have an

TABLE I.

| $f=0$, when $x=20r$. | | $f=0$, when $x=10r$. | | $f=0$, when $x=5r$. | | $f=0$, when $x=3r$. | | $f=0$, when $x=2r$. | |
|---|---|---|---|---|---|---|---|---|---|
| $n=12·410m$ | | $n=7·576m$ | | $n=5·428m$ | | $n=4·938m$ | | $n=5·143m$ | |
| $x$ | $f$ | $x$ | $f$ | $x$ | $f$ | $x$ | $f$ | $x$ | $f$ |
| 0·5$r$ | −0·470 $k$ | 0·5$r$ | −1·8450 $k$ | 0·5 $r$ | −2·4560 $k$ | 2·0$r$ | −0·00996$k$ | 1·8$r$ | −0·00881$k$ |
| 0·6 | +0·2341 | 0·7 | −0·4586 | 1·0 | −0·2461 | 2·5 | −0·00075 | 1·9 | −0·00361 |
| 0·7 | +0·5510 | 0·9 | −0·0368 | 1·5 | −0·01906 | 2·7 | −0·00001 | 1·95 | −0·00164 |
| 0·9 | +0·7275 | 1·0 | +0·0522 | 1·672 | 0·00000 | 2·8 | +0·00009 | 2·0 | 0·00000 |
| 1·0 | +0·7236 | 1·2 | +0·1310 | 1·7 | +0·00207 | 2·9 | +0·00008 | 2·1 | +0·00246 |
| 1·3 | +0·6146 | 1·3 | +0·1447 | 2·0 | +0·01386 | 3·0 | 0·00000 | 2·2 | +0·00408 |
| 1·4 | +0·5709 | 1·4 | +0·1497 | 2·2 | +0·01576 | 3·2 | −0·00034 | 2·3 | +0·00508 |
| 1·5 | +0·5360 | 1·5 | +0·1492 | 2·25 | +0·01585 | 3·5 | −0·00102 | 2·4 | +0·00563 |
| 2·0 | +0·3533 | 1·7 | +0·1398 | 2·3 | +0·01584 | 4·0 | −0·00215 | 2·5 | +0·00586 |
| 5·0 | +0·0514 | 2·0 | +0·1183 | 2·5 | +0·01506 | 5·0 | −0·00360 | 2·7 | +0·00568 |
| 10·0 | +0·0064 | 4·0 | +0·0301 | 2·75 | +0·01322 | 6·0 | −0·00416 | 3·0 | +0·00460 |
| 15·0 | +0·00109 | 5·0 | +0·0158 | 3·0 | +0·01102 | 7·0 | −0·00421 | 4·0 | +0·00036 |
| 20·0 | 0·00000 | 7·0 | +0·00444 | 4·0 | +0·00384 | 8·0 | −0·00404 | 4·1 | +0·00003 |
| 25·0 | −0·000265 | 10·0 | 0·00000 | 5·0 | 0·00000 | 9·0 | −0·00377 | 4·5 | −0·00109 |
| 30·0 | −0·000317 | 15·0 | −0·00107 | 6·0 | −0·00181 | 10·0 | −0·00349 | 5·0 | −0·00211 |
| 35·0 | −0·000306 | 20·0 | −0·00097 | 7·0 | −0·00280 | 15·0 | −0·00224 | 7·0 | −0·0036 |
| 40·0 | −0·000280 | 30·0 | −0·00061 | 8·0 | −0·00289 | 20·0 | −0·00150 | 8·0 | −0·0036 |
| 80·0 | −0·000110 | 40·0 | −0·000413 | 9·0 | −0·00292 | 40·0 | −0·000487 | 10·0 | −0·0032 |
| | | 80·0 | −0·000128 | 10·0 | −0·00283 | 80·0 | −0·000138 | 15·0 | −0·00215 |
| | | | | 15·0 | −0·00202 | | | 20·0 | −0·00146 |
| | | | | 20·0 | −0·00141 | | | 40·0 | −0·000481 |
| | | | | 40·0 | −0·00047 | | | 80·0 | −0·000137 |
| | | | | 80·0 | −0·000136 | | | | |

entirely different origin, we have no reason to suppose that $n=m$; nor have we any means of ascertaining, on *à priori* grounds, their comparative values. But we can assume that $u=v$, for some supposed value of $x$, determine the ratio of $n$ to $m$ on this supposition, and calculate the values of $f$ for various values of $x$, both greater and less than that for which we have taken $f=0$.

The preceding table contains the results of numerous calculations made after this manner: in which $k$ stands for $\frac{m}{r^2}$.

From these results it appears that for values of $\frac{n}{m}$ greater than 4·938, or thereabouts, there are *two alternations of the effective force f, as the distance between the molecular atmospheres increases indefinitely from zero.* The first is from a repulsion to an attraction; the second is from an attraction to a repulsion. The repulsion which becomes effective beyond the limit of the attraction, at first increases, and then decreases, extending to an indefinite distance. If the ratio, $\frac{n}{m}$, be less than about 4·938, the effective action of the two molecules upon each other will be repulsive at all distances. It will be observed also that the range of distance within which an attractive force takes effect is greater in proportion as the value of $\frac{n}{m}$ is greater, and that this becomes reduced nearly to zero, when this ratio is equal to 4·938; also, that in all cases in which an effective attraction manifests itself at any distance whatever between the molecules, that is in the case of every known solid and liquid, the effective repulsion within the limit of the attraction, obtains at less distances between the electric atmospheres of the molecules than about $3r$, that is, than once and a half the diameter of either atmosphere.

For the more accurate determination of the least value of the ratio $\frac{n}{m}$, we have the following results of computation:

For $f=0$, when $x=3r$, $\frac{n}{m}=4{\cdot}93827$; for $f=0$, when $x=2{\cdot}9r$, $\frac{n}{m}=4{\cdot}93449$; for $f=0$, when $x=2{\cdot}8r$, $\frac{n}{m}=4{\cdot}934409$; for $f=0$, when $x=2{\cdot}7r$, $\frac{n}{m}=4{\cdot}93847$. If then the ratio $\frac{n}{m}$ be greater than 4·9344, the two alternations of effective molecular force above mentioned will have place; if the ratio be less than 4·9344, the effective action of the one molecule upon the other will be repulsive at all distances.

It is assumed in the foregoing calculations, that the surface of each molecular atmosphere which receives the impulses, whether attractive or repulsive, propagated from the other through the intervening electric ether, may be regarded as the same as that from which the electric repulsion proceeds outward, but it will be readily seen that they may be supposed to differ within certain limits, without vitiating the result that for certain values of $\frac{n}{m}$ two alternations of the effective force will subsist. The forces may also experience losses, to a certain extent, in their propagation, and this general principle still hold good.

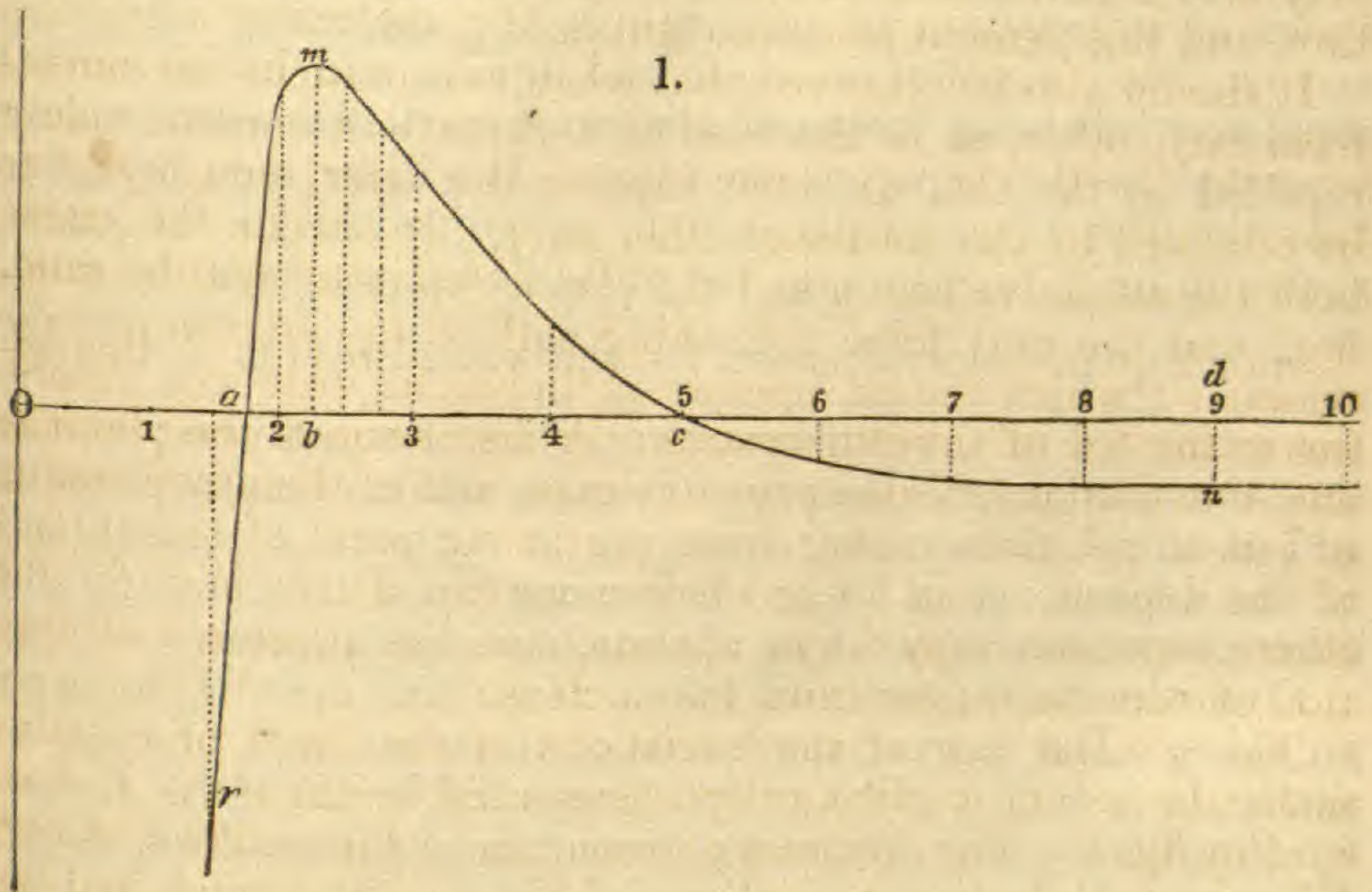
It should also be observed that when two particles are remote from each other, as in the case of a particle of cometic matter repelled by the sun, we must suppose the intervening space to be occupied by the universal ether only. In such a case, then, both the attractive action and the *electric* repulsion will be wanting; and the only force remaining will be the *ethereal* or *heat-repulsion;* which should operate at indefinite distances, according to the law of inverse squares. A discussion of the phenomena of evolution of cometary envelopes, and of the outstreaming of jets of nebulous matter from particular parts of the surface of the nucleus, must be had before we can decide how far the *electric* repulsion may be in operation in the processes of ejection of cometic matter from the nucleus.[2]

The general law of the variations of the force of effective molecular action is graphically represented by the curve $r$, $a$, $m$, $c$, $n$, in fig. 1. The abscissas represent the comparative distances between the electric atmospheres of the two molecules, and the ordinates the intensities of the effective force corresponding to these distances. When the ordinate lies above the axis of abscissas, the force is attractive; when it lies below, the force is repulsive. The two axes are asymptotes to the curve. The curve has been constructed from the calculated results obtained on the supposition that $f=0$, when $x=5r$.

There are four points, marked $a$, $b$, $c$, $d$, to be especially noted. $a$ and $c$, where $f=0$, represent positions of equilibrium; $a$ being a position of stable, and $c$ of unstable equilibrium. When the atmospheres are separated by the distance $Ob$ the attraction has its maximum value, $bm$; and when they are at the distance $Od$, the repulsion, beyond the outer limit of the attraction, has its maximum value, $dn$. In order that two particles may unite, when influenced by their own proper forces only, the distance

[2] In the memoir by the author "On the Theoretical Determination of the Dimensions of Donati's Comet," published in No. 87, vol. xxix, and in No. 94, vol. xxxii, of this Journal, the conclusion was reached, as one result of the computations, that "the repulsion exerted by the sun, and also by the nucleus (of the comet), is not a property belonging to all the particles of the mass, like the attraction of gravitation; and is probably, therefore, a force emanating from the surface of the body, or from a portion only of its mass." We now see that the existence of such a force is also a legitimate deduction from the theory of molecular forces under consideration; and that it consists in the force of ethereal repulsion, which we have denominated *heat-repulsion.* Its impulses constitute the entire force of radiant heat given off by the body into free space, and vary in intensity, or amount, with the temperature.

between their atmospheres must be less than *Oc*. But if they are subject to an external pressure urging them toward each other, it suffices that this pressure should exceed the maximum repulsion *dn*, at the greater distance *Od*. To separate the particles the force in operation must exceed in intensity the maximum molecular attraction, *bm*.[3]




If heat be imparted to the two particles under consideration, it will obviously tend to depress the entire curve of molecular action, and diminish the range, *ac*, of the attractive force. If the amount be continually increased, the distances between the two positions of equilibrium, *a* and *c*, will eventually be reduced to zero, and the curve thrown entirely below the axis of abscissas, or the effective force become a mutual repulsion at all intervals of distance between the particles. This would be the necessary tendency of the introduction of new repulsive impulses into the system, even if the original forces continued in full operation as before; but, heat, by expanding the molecular atmospheres, should also tend to diminish the ratio of *n* to *m*, and therefore to modify in a similar manner the natural curve of molecular action.

It is easy to see that a similar curve will also serve to represent the mutual actions of dissimilar molecules, which obtain

[3] To guard against misapprehension it may be well to observe here, that the resisting force of cohesion which is brought into play when a body is ruptured by a pulling force is not necessarily proportional to the intensity of the maximum force of attraction between two of its particles. For it must depend, not only upon the intensity of this attraction, but also, on the number, distance, and position of the other particles that oppose, by their attractive action, the separation of the two. Incidentally the displacement these particles may experience, from the action of the rupturing force, comes in as a modifying cause.

when a chemical union is formed between two different substances. But in this case a force of electric attraction not yet considered may come into play.

The general principle of two alternations of the effective action of molecules, to which we have been theoretically conducted, is distinctly recognized as a physical fact in the three different states of aggregation of matter;—in the molecular attraction and repulsion manifest in solids and liquids, and in the mutual repulsion subsisting between the same particles when widely separated in the vaporous condition. It will be seen hereafter that the law of molecular action, as portrayed in the curve shown in fig. 1, furnishes, in the probable variations of the ratio, $\frac{n}{m}$, an adequate general cause for the varied results of this action exhibited in the different properties of substances; and at the same time reveals the probable explanation of many physical and chemical phenomena, occurring at surfaces of contact, and of the dependence of these phenomena upon temperature and other circumstances, as in oxydation, combustion, &c.

From the stand-point now taken new views open up to us on all sides. The more conspicuous of these we will proceed to sketch, in general outline, under the several heads of the *Molecular Constitution and Mechanical Properties of Bodies*, *Heat*, *Light*, *Electricity*, *Magnetism*, and *Chemical Action*. The intimate relations subsisting between the phenomena occurring in these different departments of Nature, and between the special agents by which they are produced, or the "correlation of the physical forces," will be seen to be deducible from the fundamental conceptions adopted. It will be observed that all these varied phenomena are but different results of the action of the primary forces, which consists in an attraction of atoms for their atmospheres, and in a mutual repulsion between the atoms of these atmospheres;—that they are, primarily, movements or disturbances, produced in these subtile atmospheres, from which ethereal waves of impulses, and motions of molecules and masses, may result.

### *Molecular Constitution, and Mechanical Properties of Bodies.*

Every body of matter consists of separate particles, or molecules, in a state of equilibrium under the action of the forces proper to the particles, or of these in connection with extraneous forces taking effect upon the particles. The interstices between the molecules we conceive to be pervaded by both the electric and the universal ether; having, probably, different densities in different substances. The state of equilibrium in which each particle of the mass subsists, implies that the effective forces acting upon it, from opposite sides, are equal and directly opposed, or else that the effective forces of each side are equal to zero. The different mechanical properties of different substances may be ascribed, primarily, to differences in the value of the ratio of the constants of electric attraction and repulsion ($\frac{n}{m}$, in Table I); and to a certain extent also to differences in the size of the molecular atmospheres, upon which the value of $k$ in Table I. partly depends. In consequence of these supposed differences in the value of the ratio, $\frac{n}{m}$, each substance should have its own special curve of molecular action. It is natural to suppose that the constant, $n$, of the force of attraction exerted by the atom upon its atmosphere would in general increase with the mass of the atom, and so that the force of cohesion would be greatest in those substances whose atomic weights are the greatest. But as we cannot affirm that the weight of an atom must necessarily be proportional to the force of attraction exerted by it upon its electric atmosphere, and as the constant, $m$, may also be subject to variations, substances of nearly equal atomic weights (e. g., gold, platinum, bismuth, and lead) may have different properties.

The molecules of a substance in the solid state may be aggregated together as a homogeneous mass, or in groups more or less complex. The mechanical properties of the mass vary with the mode of aggregation. The form of aggregation assumed, in the process of solidification, depends upon the circumstances, with respect to cooling, pressure, &c., under which the solidification occurs. The effect of the same circumstances should vary with different substances, with their properties in relation to heat; but these properties are primarily dependent upon the general features in the constitution and condition of the molecules, upon which the laws of effective molecular action, as shown by the proper curve, depend.

Contemplating, from our present point of view, the varying mechanical states and conditions which the same substance may assume under different circumstances, we are led to recognize, as an essential physical feature, upon which such changes either wholly or partially depend, the fact that the mechanical condition of the individual molecules is not fixed and unchangeable, but liable to material variations. We perceive their atmospheres expanding under the influence of heat, and contracting from the effect of external pressure, and that certain phenomena and permanent changes of property result from these atmospheric changes; (e. g., changes of property in passing from the solid to the liquid form, or vice versa; permanent displacement of particles produced by the temporary action of forces of a certain intensity, upon bodies).

*States of Aggregation of Matter.*—These are three essentially different states of equilibrium. In the *solid* form, the particles immediately contiguous to each other are in a condition of equilibrium under the action of their own molecular forces; if more distant particles exercise any effective action, it is attractive, and neutralized by a similar action on the other side of the particle. To be more definite, each molecule of the mass is surrounded by others at various orders of distance from it; and each pair of molecules at the first order of distance from each other are in a condition of equilibrium by themselves, which is equivalent to saying that their electric atmospheres are separated by the distance O*a*, fig. 1. For the second order of distance the action should then be attractive, but it may very well be that when a permanent equilibrium of the mass has been reached, the atmospheres of two particles at this order of distance will be so expanded by their attractive action, on the line of their centres, that, for the diminished value of $\frac{n}{m}$ thus resulting, the distance between the atmospheres on this line will be the increased distance O*a* for the curve corresponding to this diminished value of $\frac{n}{m}$. Upon this supposition, each particle would be separately in equilibrium with every particle contiguous to it, both at the first and second order of distance. We shall have occasion to note hereafter that this state of things is probably more or less perfectly realized under different circumstances of solidification. As to the action of more distant molecules, it is first to be observed that if two molecules are in equilibrium, under their mutual actions, the attractive and repulsive impulses exerted by each upon the central atom of the other, must be equal, and therefore that no effective action, either attractive or repulsive, can be transmitted to other more distant particles on the same line. Under these circumstances, one molecule, in receiving the action of another, *intercepts* the action that would otherwise take effect upon other more distant molecules. This being admitted, it may be perceived, on examining Table I, that the attractive actions of particles which lie beyond the second order of distance from a given particle, will be in a great measure intercepted by intervening particles. In what has now been stated with respect to the solid condition, we have had in mind a homogeneous mass of molecules only. We cannot here enter upon the consideration of the case in which the molecules are aggregated into groups.

In the *liquid* state, the contiguous particles repel each other; and particles more distant exert no sensible action, or a feeble attractive one. Here, as in the case of a solid, the sensible action is confined chiefly to particles that lie at the first and second orders of distance. These remarks apply to the general mass of the liquid. The molecular atmospheres are in an expanded condition from the effect of the heat of fluidity, and it is from this fact that the peculiar properties of the liquid state result. As we draw near the surface of the liquid, the atmospheres are in a condition of greater and greater expansion as the necessary result of the process of liquefaction, and therefore their proper attractive actions are less and less. From this cause it happens that each particle near the surface is more effectively attracted by those below it, beyond the first order of distance, than by those above it, and thus each layer of particles is compressed upon that immediately below it; also to a certain depth more particles will exert their attraction from below than from above. As a consequence, the density must increase from the surface to a certain small depth below it, and a force of compression be exerted throughout the whole liquid mass. This force determines, and is in equilibrium with, a mutual repulsion between the particles of the liquid. From the essential nature of a liquid, as we shall soon see, this increasing molecular repulsion, from the surface downward, operates in all directions from each molecule, and so tends to neutralize the attractive actions between molecules separated by the second order of distance; as the final result, therefore, at the depth at which the density ceases to increase, and all greater depths, the action between two such molecules should be either feebly attractive, or altogether evanescent.[*]

The views which have now been presented enable us to form a definite conception of the probable arrangement of the molecules of a liquid. If the state of equilibrium be such as we have represented, we must conclude that a perfectly symmetrical arrangement of particles, similar to that which subsists in crystals, prevails throughout the whole mass.

We conceive the fundamental distinction between a solid and a liquid, from the mechanical point of view, to be that the external impulses which fall upon the molecule of a solid, are propagated, either wholly or chiefly, in their original line of direction; while those which fall upon the molecule of a liquid are radiated in every direction from it. The physical cause of this difference in the mode of propagation of a force appears to be the simple fact that in the process of liquefaction the molecular atmospheres are forced by the heat of fluidity to a decidedly greater distance from the atoms which they surround; thus, leaving below them a much larger volume of universal ether, to

[*] The theory of the existence of a contractile force at the surface of a liquid, as the result of molecular action, was advocated by Young and Poisson, and employed by them in explanation of the phenomena of capillarity. It has also been ably sustained and illustrated by Professor Henry, by many ingenious experiments.

receive the impulses propagated down to it. If this difference between the mode of propagation of impulses by the molecules of a solid and liquid be admitted, it is not difficult to see that we have a sufficient cause for the different mechanical properties attendant upon these two states of aggregation, without having recourse to the prevalent idea of a permanent polarity of simple atoms. So far as any polarization of molecules comes into operation, we shall have occasion to remark in discussing briefly the topic of crystallization, that it is simply an induced, and for the most part a temporary condition of the molecular atmospheres, developed in the act of solidification.

In the *aëriform* state the particles are so widely separated that each is repelled by all those which surround it, within the limit of effective action, and the equilibrium is determined by external pressure. The properties of gases and vapors, and the laws of their expansion and contraction, are deducible from equ. (3) (p. 68). The value of $x$ that obtains when a vapor formed at any temperature has its maximum tension, is the distance O*d*, fig. 1, answering to the maximum molecular repulsion, *dn*; and this varies for different temperatures because the ratio, $\frac{n}{m}$, decreases as the temperature rises. (See different values of maximum repulsion answering to different values of the ratio $\frac{n}{m}$, given in Table I.)

The process of transition from the solid to the liquid state occurs at the surface of the mass. As the heat is absorbed, the molecules near the surface recede from each other; and when this expansion has reached a certain point, the attractive forces of the particles at the different orders of distance come successively into action, being less intercepted by intervening particles. At the same time, the individual molecular atmospheres expand, or recede from their central atoms, under the action of the heat pulses that penetrate to these atoms; and so the energy of the attractive force of each of these molecules declines. The surface particles will thus continue to recede at the same time that they are restrained by the attractions of those below them. This effect will extend from the surface downward; and as a final result, a certain number of layers are brought into the liquid condition, in which, as we have seen (p. 75), the particles mutually repel each other, in consequence of the exertion of a compressing force at the surface. In the case of a liquid that emits vapor at the temperature of liquefaction, we must conclude that the particles at the very surface become ultimately subject to an effective repulsion from the united action of those below it, which is in equilibrium with the tension of the vapor resting on the surface; and that this effective repulsion extends to all points above the surface.

The heat of fluidity is consumed in forcing up the molecular atmospheres. As a final result of the liquefaction, these atmospheres remain in an expanded condition. The effect of this expansion is to diminish the values of $v$ given by equ. (1) (see p. 68), and increase the distance $Oa$, fig. 1. The actual distance between two contiguous atmospheres is less than the increased distance $Oa$, by reason of the compressing force that takes effect throughout the liquid mass. But the ultimate compression imparted to the individual atmospheres will depend in a great degree upon the final value of the attractive action, $v$, between the molecules, and may therefore still be less than that which obtained in the solid state. In this diminished value of $v$ we have, at the same time, the explanation of the diminished force of cohesion attendant upon the liquid state. The comparative densities of the liquid and solid also depend upon $v$. For we have just seen that the distance between the contiguous atmospheres of two particles of the liquid is less than the increased value of $Oa$, but this distance may, according to the intensity of the attractive force, $v$, be either greater or less than the original value of $Oa$; which was the distance between the atmospheres of the same particles in the solid condition. Accordingly, the liquid may be either more or less dense than the solid from which it is derived.

The passage from the liquid to the solid state is, essentially, the inverse of that which has just been under consideration, and in the general survey we are now taking need not be considered in detail. The mass of molecules and their individual atmospheres now contract instead of expanding, and in the final act of solidification the contiguous molecules assume the positions due to their own special forces. While all this is being accomplished, the molecular atmospheres contract, and heat is given out.

The explanation of the process of *evaporation* will be readily inferred from what has already been stated with regard to the condition of the surface of a liquid (p. 76). The nice equipoise of the surface particles may be disturbed either by a slight elevation of temperature, or a diminution of the tension of the vapor resting upon them. The cooling effect of the evaporation is to be attributed to the expansion which the electric atmospheres experience, on being freed from the compressing forces previously existing.[5]

[5] It is apparently not necessary to suppose, as has been done on page 76, that the tension of the vapor resting on the surface of a liquid, when at its maximum, is in equilibrium with the outward repulsion experienced by the outer layer of liquid particles. The equilibrium may be a dynamical one; the vapor may be continually rising at certain points of the surface, and continually passing back into the liquid condition at other points,—the condensation compensating exactly for the evaporation.

In the process of *ebullition*, the expansive action of the heat absorbed by the lower layers of the liquid increases until the superincumbent pressure, the cohesive attraction of the vessel for the liquid, and the effective attractions subsisting between the molecules of the liquid (represented by the ordinates between $a$ and $b$, fig. 1), are overcome. When this point is reached at any part of the liquid stratum, the separated particles will expand rapidly into bubbles of vapor, in opposition to the pressure of the atmosphere, and the attractions denoted by the decreasing ordinates between $b$ and $c$, fig. 1. The expansion should continue until the distance between the atmospheres of two particles increases to the limit $Od$, at which the repulsion attains to its maximum value; or rather to a limiting distance somewhat greater than $Od$, at which the repulsion due to the heat pulses present in the molecules, *plus* the molecular repulsion at that distance, is equal to the external pressure.

It cannot proceed further than this without a direct expenditure of heat-force, which will raise the temperature of the vapor. The heat which becomes latent, as the phrase is, is expended in the act of expansion, and in forcing up the molecular atmospheres in opposition to the attractive action of the atoms and all compressing forces. The amount of work thus taken up by the atmospheres manifests itself also as work of expansion, since it is so much work of the atomic attraction and of the compressing forces neutralized. When the heat pulses are not wholly expended in this manner, a portion of them pass into the molecular atmospheres and elevate the temperature of the liquid. If the pressure upon the free surface of the liquid exceeds the pressure of the atmosphere, the molecular atmospheres are more compressed, the value of $m$ becomes greater, and the ratio, $\frac{n}{m}$, diminishes in consequence; from this cause the limit of the recess of the particles, $Od$, fig. 1, diminishes, and the maximum repulsion, $dn$, increases (see Table I). The resulting vapor has, therefore, at the same time, a higher tension and a greater density.

According to the theoretical views now advanced, the "interior work" which Tyndall maintains is expended in the act of liquefaction, and also in that of vaporization, in "moving the atoms into new positions," or in conferring "potential energy" upon them, is consumed in each instance in pressing up the electric atmospheres that surround the atoms of the substance; and heat disappears in the process in proportion to the effect thus produced.

[To be continued.]

ART. VIII.—*On the Improvement of the Elements of a Comet's Orbit: Brünnow's method;* communicated by C. ABBE.

THE following method of conducting the computation of the elements of a parabolic orbit was taught by Dr. Brünnow in lectures at the University of Michigan in 1858; it is here faithfully reproduced from my notes taken at that time, and whatever of merit it possesses is of course due to my instructor. The formulæ are thrown into the natural order of computation, and they will thus be readily available to the computer. The present is quite a general method: it presumes a previous approximate knowledge of the orbit, and is suited to normal places or very exact observations, and is not limited to the use of three places. The intervals $t''-t'$ and $t'-t$ are not restricted.

We use at least three complete observations, i. e., the times $t, t', t''$; the geocentric longitudes $\lambda, \lambda', \lambda''$, and the geocentric latitudes $\beta, \beta', \beta''$. We shall also need the sun's geocentric longitudes $\odot, \odot', \odot''$, and the logarithms of the sun's geocentric distances R, R′, R″. From our previous knowledge of the orbit we shall have been able to correct our observations for velocity of light, aberration and parallax; they should be referred by proper corrections for nutation and precession to the same equinox.

We first may prepare the angles

$$\odot''-\odot, \quad \lambda-\odot, \quad \lambda''-\odot,$$

the number R, and the logarithm of $t''-t$.

Compute the number $g$ and angle G from

$$R''\cos(\odot''-\odot)-R = g\cos(G-\odot)$$
$$R''\sin(\odot''-\odot) = g\sin(G-\odot)$$

$g$ and G are the distance and longitude of the first place of the earth as seen from the third.

Compute the numbers $c$ and $c''$ and logarithms C and C″ from

$$\cos\beta\cos(\lambda-\odot) = \cos\psi \qquad \cos\beta''\cos(\lambda''-\odot'') = \cos\psi''$$
$$R\cos\psi = c \qquad R''\cos\psi'' = c''$$
$$R\sin\psi = C \qquad R''\sin\psi'' = C''$$

$\psi$ and $\psi''$ are the angles at the earth between the sun and comet at the first and third observations.

From our previous knowledge of the orbit we now assume two values $\Delta$ and $\Delta_{,}$ of the distance between the comet and earth at the time $t$. It will be a little more convenient and elegant to assume $\log\Delta$ and $\log\Delta+i$. The following computation is to be made for each assumption. Compute $\log D$ and angles B and L from

$$\Delta\cos\beta\cos(\lambda-G)+g = D\cos B\cos(L-G)$$
$$\Delta\cos\beta\sin(\lambda-G) = D\cos B\sin(L-G)$$
$$\Delta\sin\beta = D\sin B$$

D, B, L, are the distance, latitude and longitude of first comet's place as seen from third earth.

Compute the number $a$ and logarithm A from

$$\begin{aligned} \sin \beta'' &= n^{*} \sin m \\ \cos \beta'' \cos (\lambda'' - \mathrm{L}) &= n \cos m \\ n \cos (\mathrm{B} - m) &= \cos \varphi \\ \mathrm{D} \cos \varphi &= a \\ \mathrm{D} \sin \varphi &= \mathrm{A} \end{aligned}$$

Compute the number $r$ from

$$r = +\sqrt{(\Delta - c)^2 + \mathrm{C}^2} = \frac{\Delta - c}{\cos x} \text{ if we put } \tan x = \frac{\mathrm{C}}{\Delta - c}$$

$r$ is the heliocentric distance of comet at the first observation. We now desire to find $r''$, which must be accomplished by a tentative process.

Assume a value, $\Delta''$, of the distance of third comet from third earth. Compute the number $r''$ from

$$r'' = +\sqrt{(\Delta'' - c'')^2 + \mathrm{C}''^2} = \frac{\Delta'' - c''}{\cos x} \text{ if we put } \tan x = \frac{\mathrm{C}''}{\Delta'' - c''}.$$

With the $r$ and $r''$ thus found we compute logarithm $\varkappa$ from Lambert's equation, as follows:

$$[8{\cdot}5366114] \frac{t'' - t}{(r'' + r)^{\frac{3}{2}}} = \eta$$

$$\frac{3\eta}{\sqrt{8}} = \sin \beta$$

$$\frac{3 \sin \frac{1}{3}\beta}{\sin \beta} \sqrt{\cos \tfrac{2}{3}\beta} = \mu$$

$\log \mu$ is tabulated with argument $\eta$. (See Davis's *Gauss Theoria.*)

$$\varkappa = \eta\mu\,(r'' + r)$$

If this value of $\varkappa$ does not agree with that found from

$$\varkappa = +\sqrt{(\Delta'' - a)^2 + \mathrm{A}^2} = \frac{\Delta'' - a}{\cos x} \text{ if we put } \tan x = \frac{\mathrm{A}}{\Delta'' - a}$$

we must then assume another value of $\Delta''$ and renew the computation of $\varkappa$. A few trials will give the required value of $\Delta''$. $\varkappa$ is the chord between the first and third places of the comet.

We are now able to find the heliocentric distances $r$ and $r''$, latitudes $b$ and $b''$, and longitudes $l$ and $l''$, of the first and third positions of the comet, from the following formulæ:

$$\begin{aligned} \Delta \cos \beta \cos (\lambda - \odot) - \mathrm{R} &= r \cos b \cos (l - \odot) \\ \Delta \cos \beta \sin (\lambda - \odot) &= r \cos b \sin (l - \odot) \\ \Delta \sin \beta &= r \sin b \end{aligned}$$

* Sign of $n$ is plus.

and

$$\begin{aligned} \Delta'' \cos\beta'' \cos(\lambda''-\odot'') - R'' &= r'' \cos b'' \cos(l''-\odot'') \\ \Delta'' \cos\beta'' \sin(\lambda''-\odot'') &= r'' \cos b'' \sin(l''-\odot'') \\ \Delta'' \sin\beta'' &= r'' \sin b'' \end{aligned}$$

The values of $r$ and $r''$ found from these formulæ must agree with those found previously.

Having now the heliocentric coördinates of the comet for the times $t$ and $t''$, we are able by well known formulæ to find the elements of the orbit, and from these elements we compute the latitude and longitude for the time $t'$. A comparison of these results with the observed $\beta'$ and $\lambda'$ will show that

the assumption of $\log\Delta$ gives $\lambda'+a$ and $\beta'+b$
" " $\log\Delta+i$ " $\lambda'+a'$ " $\beta'+b'$
but
" " $\log\Delta+x$ must give $\lambda'$ " $\beta'$

therefore neglecting second differences we must have

$$a - \frac{a'-a}{i}\,x = 0 \qquad b - \frac{b'-b}{i}\,x = 0.$$

From as many such equations as we choose to employ we may determine $x$, and a new computation assuming $\log\Delta+x$ as the logarithm of the distance between first comet and first earth, will lead to our desired improved elements.

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ART. IX.—*Notes on the Platinum Metals, and their Separation from each other;* by M. CAREY LEA, Philadelphia.—Part I.

(I.)

FEW branches of inorganic chemistry present difficulties comparable with those involved in the study and separation of the platinum metals. Their close analogy with each other, and the remarkable manner in which the relations of each to chemical reagents are controlled by the presence of the others, give rise to difficulties in their detection and separation which are only by degrees being surmounted. Much time and unwearied labor on the part of the chemist are required to reach results which when obtained appear insignificant in proportion to the effort which they cost, and it may in fact be said that the platinum metals constitute a chemistry in themselves, governed by special rules and to be studied by special methods. Each step in the simplification of the processes by which the separations are effected, each decisive reaction by which the presence or absence of a member of the group may be certainly inferred, is so much gained toward conquering a complete knowledge of these rare and interesting bodies.

For much the better half of all we know upon this subject, we are indebted to Dr. Claus, whose method of separation I have followed up to a certain point, and then have diverged from it, with I think some advantage. I propose to introduce the use of *oxalic acid*, as an agent in effecting the separation, in the manner which I shall presently describe.

From my friends, Prof. Booth, of the U. S. Mint, and Mr. Garrett, I received the material upon which I have worked. This was Californian osmiridium, which had already undergone a preliminary fusion with nitre and caustic potash.

This material was next boiled with aqua regia to extract all the soluble portions, the residue was then ignited with nitre and caustic soda,[1] the fused mass was heated with water. From the resulting solution small portions of osmite of potash crystallized out. The metallic oxyds were next precipitated, and this precipitate, together with the portions insoluble in water, was boiled again with aqua regia, ignited again, &c. These ignitions, in addition to that which it had undergone before coming into my hands, still left a small portion of unattacked residue.

The boiling with aqua regia was continued for a very long time in order to get rid as thoroughly as possible of the osmic acid; in all, this treatment was extended over two hundred hours. Even this however still left osmium in the solution, in easily recognizable but in comparatively small quantity. The greatest advantage was found throughout the whole of this part of the operation from the use of the blowing apparatus, which I described in a former number of this Journal, and with the aid of which all inconvenience from the fumes of osmic acid was avoided. The apparatus was constantly swept clear by a powerful air-current, and the osmic acid was removed as fast as it volatilized. The treatment which the ore had undergone before it was placed in my hands, had removed the greater part of the osmium; a portion of what remained had separated out as osmite of potash, and it was not deemed worth while to attempt to save the little that remained. It would be easy, however, in operating upon fresh material with the aid of this blowing apparatus, to conduct the osmic fumes through an appropriate reducing agent, and at the same time to sweep out every trace which escaped reduction. As the ignition of the ore with alkaline nitrate and caustic scarcely drives off any osmium, and as almost all inconvenience in manipulating the resulting solutions can be avoided by throwing down the metals with alcohol from

[1] Attention is necessary to the order in which these substances are employed. If the caustic soda is melted first, it attacks the iron vessel strongly and may even go through. If added last, it causes sudden and violent effervescence, with danger of boiling over. Therefore, place the nitre first in the vessel, and when it is fused, add the caustic soda. When a red heat is attained, add the osmiridium by degrees.

the hot alkaline solution, in place of using acid, it is clear that the difficulties arising from the noxious effects of osmic acid can be almost wholly removed from each of the various stages of the process.

A very prolonged treatment with aqua regia was found to have the great advantage of converting nearly the whole of the ruthenium into bichlorid. The separation of ruthenium in this form from the other metals is so easy in comparison with the difficulties presented by the separation of the sesquichlorid, that this advantage cannot be looked upon as other than a very material one.

Salammoniac was next added to the mixed solution in quantity sufficient to saturate it. The sandy crystalline precipitate (A) was thoroughly washed out, first with saturated, and then with dilute salammoniac solution. The saturated solution of ammonium salt carried through with it nearly the whole of the ruthenium as bichlorid (B), the dilute solution was found to contain small quantities of iridium, rhodium, and ruthenium (C).

Over (A), water acidulated with chlorhydric acid was placed, and allowed to stand for some days. This was treated with ammonia and boiled. The precipitate was inconsiderable, and, when treated with chlorhydric acid, furnished green chlorid of osmium, with traces of ruthenium.

In these preliminary steps I have used Claus' process, which undoubtedly offers advantages over any other, and best brings the metals into a convenient state for separation, varying it only by prolonging the treatment with aqua regia, and converting the ruthenium principally into bichlorid instead of sesquichlorid. We have now three portions of material, (A) consisting of iridium salammoniac, containing also ruthenium, osmium, rhodium, and platinum in small quantities. (The ore which I examined contained no palladium, which metal, if present, has always its own peculiar mode of separation, and does not enhance the difficulties of the operation.) (B) containing bichlorid of ruthenium, together with iron in quantity, copper, and other base metals which may be present. Finally, (C) containing chiefly bichlorid of ruthenium, mixed with small quantities of iridium and rhodium.

The next step in the process is to introduce the iridium-salammoniac (A) into a large flask with twenty to twenty-five times its weight of water, and apply heat until the solution is brought to the boiling point; the whole of the iridium-salammoniac should be brought into solution in order that the reduction to be operated may not occupy too long a time, as otherwise the platinum and ruthenium salt, if any be present, might likewise be attacked. Crystals of oxalic acid are thrown in as soon as the solution actually boils, whereupon a lively effervescence takes place, and the iridium salt is rapidly reduced. As fast as the effervescence subsides, more oxalic acid is added until further additions cease to produce any effect. When this is the case, the liquid is allowed to boil for two or three minutes longer, not more; the heat is to be removed, and the flask plunged into cold water.

By this treatment any platinum present is unaffected. Salammoniac in crystals is added, about half enough to saturate the quantity of water present. The salammoniac may be added immediately before the flask is removed from the fire. After cooling, the solution should be left for a few days in a shallow basin, whereby the platinum salammoniac will separate out as a yellow, a reddish, or even (especially if the quantity of water used was insufficient) as a black crystalline powder, according to the quantity of bichlorid of iridium which it may contain.

The mother water is to be again placed in a flask and boiled with aqua regia. On cooling, the platinum salammoniac crystallizes out, and any traces of rhodium and ruthenium which may be present remain in solution. The iridium salt is to be washed with a mixture of two parts saturated solution of salammoniac and three parts of water, and may then be regarded as pure.

The treatment by oxalic acid, which is now proposed for the first time, affords iridium free from all traces of ruthenium. The detection of very small quantities of ruthenium in presence of much iridium has been hitherto an impossibility, or could only be effected by Claus' method of allowing a small quantity of water acidulated by chlorhydric acid to remain in contact with the iridium salammoniac for some days. The ruthenium salt, by its superior solubility, tended to dissolve first, hence the acidulated water after standing contained ruthenium in larger relative proportion than the original crystals—the ruthenium reactions were more marked, and if it was present and in sufficient quantity, it could be detected by sulphocyanid of potassium, or better, to an experienced eye, by acetate of lead. The objections to this method are sufficiently obvious. I shall presently describe a reaction which will detect ruthenium in the presence of any quantity of iridium, and, scrutinized by that test, the iridium prepared in the manner which I have just described, is free from ruthenium, as well as from the other more easily separable cognate metals.

The treatment of solutions (B) and (C) presents no difficulty. With (B) the best plan is to place the solution aside in a beaker covered with filter paper for some time. Treated in this way the bichlorid gradually crystallizes out, and by recrystallizations may be obtained in a state of perfect purity.

Solution (C) is to be evaporated to dryness and reduced to an impalpable powder. It is then to be thrown upon a filter and thoroughly washed with a perfectly saturated solution of salammoniac. The bichlorid of ruthenium is thus carried through, with perhaps a trace of sesquichlorid of rhodium, from which, however, it is easily freed by crystallization. From the residue, the sesquichlorid of rhodium and ammonium, $3NH_4Cl, Rh_2Cl_3 + 3HO$, is removed by a dilute solution of salammoniac, perfectly free from the iridium, which is left behind.

In connection with this separation I may make a remark, which, though of special reference to this particular case, is also applicable to all those cases in which the double chlorids of the platinum metals are to be separated by their various solubilities in solution of salammoniac. This most valuable process, in which we are indebted as for so much else, to Claus, whose untiring labors have made him the father of this department of chemistry, requires to be applied with some attention to minutiæ. The crystalline matter must be reduced to the finest powder, and after being thrown upon the filter, it must be washed continuously until the separation is effected. Any interruption of the washing is followed by more or less crystallization of salammoniac through the material which precludes an effectual separation. The same material which in a state of coarse powder will hardly yield up enough $RuCl_2$ to color the salammoniac solution, will, when thoroughly pulverized, give an almost opaque blood-red filtrate.

Solution (C) may be subjected to a different treatment from the foregoing, and oxalic acid may be used to effect the separation. The solution is to be brought to the boiling point and oxalic acid added as long as effervescence is produced. The bichlorid of iridium is thereby reduced, the bichlorid of ruthenium and the sesquichlorid of rhodium are not affected. Salammoniac is then to be dissolved in the solution to thorough saturation. By standing and repose the double chlorid of rhodium and ammonium, $3NH_4Cl, Rh_2Cl_3 + 3HO$, separates out. The solution is then re-oxydized by boiling with aqua regia; by standing for some days in a cool place the iridium-salammoniac crystallizes out, and the supernatant solution contains double chlorid of ammonium and bichlorid of ruthenium which may be rendered pure by several recrystallizations.

For purifying the double chlorid of iridium and ammonium, $NH_4Cl, IrCl_2$, I give a decided preference to the method which I have described, with oxalic acid. It is simple and less trouble, and there is the further advantage that the platinum is left in the condition of double chlorid, whereas when the usual method of treating with aqueous sulphuretted hydrogen is used, the platinum is apt to be converted partly into sulphid, together with any traces of rhodium and ruthenium which may be present. When oxalic acid is used, the platinum remains behind as a reddish powder, containing some iridium, from which it may be freed in the ordinary manner, if it is present in quantity sufficient to be worth working.

For treating a mixture such as that which I have here designated as (C) containing no platinum and ruthenium only as $NH_4Cl, RuCl_2$, it is unnecessary to apply reducing agents, and the first method which I have described is the best. But if it be proposed to effect the separation by the reduction of the iridium compound, the method which I describe in this paper, is preferable to that based on the use of sulphuretted hydrogen, even in this case.

The action of oxalic acid on the platinum metals is interesting. It's reducing effect upon bichlorid of iridium at the boiling point is immediate. On bichlorid of ruthenium it seems to have no effect whatever, may be boiled with it for a length of time without sensible result. In a trial made with sesquichlorid of ruthenium and ammonium the oxalic acid was boiled with the metallic salt for a considerable time without any apparent effect becoming visible, but by long-continued boiling a gradual precipitation took place. When platinum salammoniac, $NH_4Cl, PtCl_2$, was boiled with oxalic acid, no effect was produced for a considerable time, but gradually the platinum salt diminished in quantity and the liquid acquired a stronger yellow color, perhaps owing to formation of soluble platinic oxalate. I was at first in hopes that the treatment with oxalic acid would have furnished a most easy and convenient method of purifying commercial platinum from the iridium always found in it. But the reduction of very small quantities of double bichlorid of iridium and ammonium in the presence of a large proportion of the corresponding platinum salt is difficult and slow, and the platinum salt itself is evidently attacked. It is for this reason that at the beginning of this paper I recommended in purifying iridium-salammoniac to use a sufficient quantity of water to hold all the iridium salt in solution at the boiling point, and to stop the operation as soon as the cessation of effervescence indicated that the action upon the iridium was terminated. The difference between the time of action of the oxalic acid on the two bichlorids is so very wide as to make it perfectly easy with proper care to effectually reduce the one without acting upon the other, except where the platinum is present in very large excess.

(II.)

The reactions of the alkalies on the chlorids of iridium are altogether peculiar. Upon the bichlorid they exert a reducing effect, converting it into sesquichlorid. An excess of alkali does not precipitate the sesquioxyd, but seems to hold it in solution. On the application of heat the sesquioxyd gradually oxydizes itself at the expense of the atmosphere, and is then precipitated as deutoxyd of iridium. If to the cold solution containing excess of alkali, an acid be added, in small quantity, an impure sesquioxyd is precipitated in consequence of the neutralization of the alkali which held it in solution. But the sesquioxyd thus precipitated always contains a considerable quantity of the alkali used. It is very unstable, and by the action of the air is rapidly converted into blue oxyd. This is nearly all that we know of the sesquioxyd of iridium.

Under these circumstances it appeared to me desirable to investigate the action of some of the alkaline earths upon iridium solutions which it seemed possible, might throw a clearer light upon the relations of the sesquioxyd.

When a solution of caustic baryta is poured over iridium-sal-ammoniac, the iridium salt rapidly dissolves with a slight effervescence, the solution presently becomes comparatively decolorized, and a dark olive-green precipitate falls. This reaction, it will be observed, is completely different from that caused by potash.

The filtrate from this precipitate, which precipitate is but small in quantity, still contains a large quantity of iridium. If it be exposed to heat, as soon as it is moderately warm, its dark olive color changes almost suddenly to an Isabella color, it becomes cloudy, and an abundant precipitate falls, which in different experiments varies from pale grayish-yellow to yellowish-brown.

It was intended to submit both of these precipitates to a rigorous analysis. But the first was obtained in too small quantity, a few centigrams only. The second substance was much more abundant in quantity. A portion of it was prepared with great care, and with thorough exclusion of air, to prevent any admixture of carbonate of baryta. But upon careful examination, it was not sufficiently homogeneous in its composition to enable one to draw positive conclusions from an analysis, and the intention was therefore abandoned. The following were the properties observed.

The olive green precipitate seemed to be permanent in the air, and contained no baryta, or, at most, only traces. It dissolved in acids, leaving however a trace of black powder behind, and gave an olive colored solution, indicating that the iridium was in the condition of sesquioxyd.

The Isabella colored precipitate contained a considerable quantity of baryta. It dissolved in acids, and gave, when freshly prepared, also olive colored solutions. When dried at 212°, it was completely converted into an indigo colored mass, which dissolved in acids to an intense blue solution. When allowed to dry at ordinary temperatures, even by exposure to the air, it was only oxydized in small part.

To observe the action of potash on this compound, a portion of the Isabella colored precipitate was placed in a watch glass, and a little caustic potash solution was poured over it. In a few minutes a blue tint was observable along the borders of the solution, and by exposure for a few hours the whole precipitate became intensely blue. We thus see that while potash is capable of reducing the bi-salts of iridium to sesqui-salts, its presence causes the sesquioxyd to take up oxygen from the atmosphere, with production of bioxyd. And that, for this action to take place, it is not necessary that the sesquioxyd should be in that anomalous state of solution in alkali, in which it seems to exist in solutions of bichlorid decolorized with excess of alkali; but that the barytic compound here described, which is comparatively permanent by itself, commences immediately to oxydize rapidly when placed in contact with potash.

Other reactions of platinum metals with baryta are as follows:

*Sesquichlorid of ruthenium and ammonium* is immediately and completely precipitated by baryta in the cold.

*Bichlorid of ruthenium and ammonium* gives no precipitate to the cold. Heated, it turns yellowish brown and becomes troubled. This troubling is completely removed by the addition of a large excess of the precipitant. If the baryta have been added to large excess at first, no troubling is occasioned by the application of heat.

*Protochlorid of palladium* is precipitated immediately in the cold by Ba. The brownish yellow precipitate does not redissolve in excess of the precipitant. In this the reaction of baryta differs from that of potash.

*Bichlorid of platinum* is scarcely affected by baryta water in the cold. Heat immediately produces a dirty white precipitate, the supernatant liquid remaining of a yellow color.

*Sesquichlorid of rhodium* gives an immediate light colored precipitate with baryta, which completely redissolves in a very large excess of the precipitant, even in the cold.

It will be seen from the foregoing that the reactions of baryta with the platinum metals differ widely from those produced by potash, and are highly characteristic.

Two of these solutions which much resemble each other, those of bichlorid of ruthenium and sesquichlorid of rhodium, are very well distinguished from each other by baryta, the former remaining for some time clear, the latter being instantly precipitated. The production of a well marked precipitate is generally a better test than a mere change of color, as produced by potash, which has hitherto been considered the best reagent to distinguish between these two substances.

The relations to color of the double sesquichlorid of iridium and ammonium, $3NHCl, Ir_2Cl_3 + 3HO$, are curious, and have not been exactly described. It is generally said that its solution is olive-green by reflected, and reddish by transmitted light. The following is a more correct description.

A dilute solution of the salt is always olive-green, whether seen by reflected or transmitted light. If it appears red by transmitted, when very dilute, this can only arise from the presence of bichlorid of ruthenium, or sesquichlorid of rhodium.

As its concentration increases, it gradually acquires a red color, visible by both reflected and transmitted light, but more conspicuous by the latter. A very strong solution is almost opaque to transmitted rays; by dilution, passes to a deep wine red. This wine-red solution is by reflected light olive-green, but with a distinct tinge of red. Nor has it before been remarked, I believe, that the crystallized salt exhibits the same dichroism. The crystals are deep green by reflected light, almost black. When placed so that light can strike through a dihedral angle, its color is ruby red.

Strong and weak solutions of this salt differ so much in color (in tint, not merely in intensity,) that at first one has a difficulty in believing that they contain one and the same substance.

*New Ruthenium Reaction.*—A series of experiments on the reactions of the platinum metals are as yet unfinished, but I take the present opportunity to mention a new and very beautiful reaction of the sesquichlorid of ruthenium.

When a solution of hyposulphite of soda is mixed with ammonia, and a few drops of solution of $Ru_2Cl_3$ are added, and the whole boiled, a magnificent red-purple liquid is produced, which, unless the solutions are very dilute, is black by transmitted light. The coloration is permanent, and the liquid may be exposed to the air without alteration.

This reaction is obtained with great ease and certainty, and will, I believe, be found far superior to any known test for ruthenium.

In order to determine the limits of the sensibility of this reagent, experiments were made with ruthenium solutions of different strengths. A portion of perfectly pure $Ru_2Cl_3$ was weighed out in a delicate balance, and the following indications were obtained:

With $\frac{1}{5000}$ $Ru_2Cl_3$, bright rose purple.

With $\frac{1}{20000}$ and $\frac{1}{30000}$, fine rose-color.

With $\frac{1}{50000}$, paler, but still perfectly distinct.

With $\frac{1}{100000}$, the color, though very pale, was still unmistakably present.

Where the solutions are so very dilute as these last, the boiling must be continued for some minutes.[2]

Although the sulphocyanid test is very delicate, it is not equal to this, as was determined by a comparative trial. It is also liable to the objection that when employed to examine mixtures, the presence of iron might be confusing. For although the reaction of ferric salts with sulphocyanid gives a different shade of red, yet it is to be observed that the two colors approach each other considerably when much diluted. Moreover, in using the sulphocyanid test, I find it best to acidulate the liquid strongly with chlorhydric acid, and to obtain chlorhydric acid absolutely free from iron, so that it does not give the slightest coloration with the sulphocyanid; it is generally necessary (in this country, at least,) to prepare it for oneself.

The delicacy of this test does not however constitute its greatest value and superiority. The reactions of ruthenium are remarkably affected by the presence of iridium; and in proportion as this last named metal is present in larger quantity, the indications afforded by all the tests hitherto known grow less and less decided, and some lose all efficacy. The test here proposed is the first known reagent that is capable of detecting ruthenium in the presence of any excess of iridium. No precautions are necessary, and the reaction is always obtained with the greatest facility. The iridium solution is to be rendered alkaline with ammonia, a crystal of hyposulphite of soda is dropped into it, and the whole is boiled for two or three minutes. If no indication of a red-purple tint appears, (or, in case of small quantities of ruthenium, a rose color,) the iridium solution may be pronounced free from ruthenium.[3]

It would appear that there must exist very beautiful purple and green compounds of ruthenium, with which we are at present unacquainted. The purple compound appears to be produced in several reactions, and notably in that just described and in the sulphocyanid. There can be little doubt that sulphur enters into its composition. The green compound is produced when a solution of $Ru_2Cl_3$ is boiled with ferrocyanid of potassium. Claus remarks, in reference to this coloration, that he is unable to explain its cause. It is, however, unquestionably due to a compound of ruthenium, and not to the production of any

[2] When the presence of ruthenium in very small quantity, or in very dilute solution, is suspected, it is often advisable to boil the solution with a little chlorhydric acid, previous to the application of the hyposulphite, sulphocyanid, or other test. The explanation of this will be given in the second part of this paper. With the hyposulphite test the acidulated solution must be rendered alkaline by addition of ammonia before heating with hyposulphite.

[3] The second part of this paper will contain an examination of the reaction of hyposulphite of soda, and other reagents, upon the various metals of the platinum group.

prussian blue or green compound; for I find that the green reaction can be obtained in presence of a large excess of free ammonia, together with which, it is hardly necessary to remark, the last mentioned compounds could not exist. I propose to return to the subject of the ruthenium reactions at a future occasion.

Philadelphia, March, 1864.

(To be continued.)

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ART. X.—*Contributions to Lithology;* by T. STERRY HUNT, M.A., F.R.S.; of the Geol. Survey of Canada.

PART III.—ON SOME ERUPTIVE ROCKS.

IN this Journal for March, 1860, ([2], xxix, 282) there is a short note, pointing out the existence, in the vicinity of Montreal, of several interesting classes of eruptive rocks, including quartziferous porphyries, trachytes, phonolite, dolerites, and diorites. It is proposed in the third part of the present paper to describe the results of some chemical and mineralogical examinations of these rocks, and to give, by way of preface, a description of their geographical distribution and geological relations. They may be considered geographically as belonging to two groups, of which the first and most important for the number and variety of its rocks may be conveniently described as the Montreal group. It consists of a succession of intrusive masses along a belt running nearly transverse to the undulations of the Notre Dame mountains, which are the prolongation of the Appalachians into eastern Canada. Commencing at Shefford mountain, an isolated trachytic mass not far removed from the western base of the Notre Dame range, we find, going westward, the detached hills known as Yamaska, Rougemont, Rouville or Belœil, Montarville or Boucherville, Mount Royal or Montreal, and Rigaud mountains; the last being distant about ninety miles from Shefford. Brome mountain, which occupies a large area to the south of Shefford, approaches within two miles of it. In like manner, a few miles to the south of Belœil is another intrusive mass known as Mount Johnson or Monnoir; making in all nine hills of eruptive rock belonging to the Montreal group. Beside these, numerous smaller intrusive masses in the form of dikes are met with around and between the hills. From Mount Royal to Rigaud mountain, a distance of about thirty miles, a gentle undulation of the strata is observed, which increases to the westward of Rigaud, and finally gives place to a considerable fault.

This disturbance has been traced to the Laurentide hills on the Lac des Chats, 140 miles west of Montreal; but to the eastward the strata exhibit no evidence of this transverse undulation, unless the appearance of the intrusive rocks already mentioned be supposed to indicate the prolongation of a fracture without sensible dislocation.

The whole of these eruptive rocks rise through unaltered paleozoic strata, which, however, in the immediate vicinity of the intrusive rocks, exhibit a local metamorphism. The hills of Shefford, Brome, and Yamaska break through the strata of the Quebec group, and lie a little to the east of the great line of dislocation which, in this region, brings up the lower members of the paleozoic series against the superior portion of the Lower Silurian, and divides into two districts the great paleozoic basin. (*Geology of Canada*, pp. 234, 597). The other hills all belong to the western division of this basin, and break through various members of the Lower Silurian series from the Potsdam to the Hudson River formation. Among the numerous dikes which traverse not only the sedimentary strata, but the intrusive masses, there are some which intersect the conglomerates of St. Helen's island; these are of uncertain age, but repose unconformably on the Lower Silurian series, and enclose pebbles and masses of Upper Silurian limestone characterized by fossils of the Lower Helderberg period. (*Ibid.*, p. 356.)

This group of intrusive rocks offers very great varieties in composition; thus Shefford and Brome consist of what we shall describe as a granitoid trachyte, while the succeeding mountain, Yamaska, and the most western, Rigaud, both consist in part of a kind of trachyte, and in part of diorite. Monnoir and Belœil also consist of diorites, which however differ from the last two, and from each other; while Rougemont, Montarville, and Mount Royal consist in great part of dolerites, presenting however many varieties in composition, and sometimes passing into pyroxenite. The dolerites of Rougemont and Mount Royal are cut by dikes of trachyte. Similar dikes also traverse the diorite of Yamaska, and may perhaps be connected with the trachytic portion of this mountain. It is probable, judging from some specimens from Rougemont, that the dolerite is there intersected by veins of diorite, some of which resemble that of Belœil, and others that of Monnoir. Dikes both of trachyte, phonolite, and dolerite are also found traversing the Lower Silurian strata in the vicinity of the great eruptive masses; and the conglomerate of St. Helen's mentioned above is traversed by dikes of dolerite, which in their turn are cut by others of trachyte.

A second and smaller group of intrusive rocks occurs to the northwest of Montreal, chiefly in the county of Grenville, where they traverse the gneiss and limestones of the Laurentian system. The principal undulations of these rocks have, like those of the Appalachians, a north and south direction; but there is apparent also a second series of undulations, affecting in a less degree the geographical distribution of the strata, and having, like the Montreal and Rigaud undulation, an east and west direction. Coincident with the latter system of folds is a series of doleritic dikes, which no where attain a great breadth, but have in some cases been traced more than fifty miles in a nearly east and west direction. These dikes are interrupted by a great mass of reddish syenite, passing in some parts into granite, and occupying an area of about thirty-six square miles in the townships of Grenville, Chatham and Wentworth. Dikes of this syenite extend from the central mass, and traverse the surrounding gneiss and limestone. Numerous dikes of quartziferous porphyry intersect both this syenite and the surrounding gneiss, and are seen in one case to proceed from a considerable nucleus of porphyry, which rises into a small mountain; rendering it probable that numerous other porphyry dikes of the region radiate in like manner from other nuclei of the same rock. Some parts of this porphyry enclose fragments of syenite, dolerite, and gneiss, which vary in size from small grains to several feet in diameter, and often give to the rock the character of a breccia. In one instance a bed of gneiss, upwards of a hundred yards in length, is completely surrounded by the porphyry.

### Orthophyre and Syenite.

*Orthoclase-Porphyry or Orthophyre.*—Under this head may be noticed a rock which has for its base a compact-petrosilex, or intimate mixture of orthoclase and quartz, rendered porphyritic by the presence of grains or crystals of orthoclase, of quartz, or of both of these minerals together. The occurrence of this rock at Grenville, where it forms dikes in the syenite of that region, has just been noticed. The fine-grained petrosiliceous base of this rock varies in color from dark green to various shades of red, purple, and black, these differences probably depending upon the degree of oxydation of the contained iron. Throughout this paste are disseminated well-defined crystals of a rose-red or flesh-red feldspar, apparently orthoclase, sometimes very abundant; and less frequently small grains of nearly colorless translucent quartz. An analysis was made of a characteristic variety of the rock, the base of which was greenish-black, jasper-like, conchoidal in fracture, and feebly translucent on the edges, with a somewhat waxy lustre. The hardness was nearly equal to that of quartz, and the specific gravity 2·62. A few distinct crystals of red orthoclase, and some grains of quartz, were present. The base, freed as much as possible from these, gave as follows:

| | I. |
|---|---|
| Silica, | 72·20 |
| Aulmina, | 12·50 |
| Peroxyd of iron, | 3·70 |
| Lime, | ·90 |
| Potash, | 3·88 |
| Soda, | 5·30 |
| Volatile, | ·60 |
| | 99·08 |

The oxygen ratio of the alkalies and alumina is 2·02 : 5·84, or nearly 1 : 3. The alumina requires 43·80 parts of silica to form with the alkalies 65·48 parts of a feldspar having the ratios 1 : 3 : 12, which are those of orthoclase and albite. There will then remain 28·4 parts of silica. This, with the exception of a small amount which is probably united with the oxyd of iron and lime, may be regarded as uncombined. The porphyries of this region receive a high polish, and are sometimes very beautiful.

*Syenite.*—The syenite of this region consists of orthoclase, usually flesh-red in color, and grayish vitreous quartz, with a small portion of blackish-green hornblende, which is sometimes almost or altogether wanting, and is occasionally accompanied with a little mica. The orthoclase is often nearly compact, but more generally distinctly crystalline and cleavable, and, so far as observed, is not associated with any triclinic feldspar. The hornblende is apparently subject to decomposition, becoming soft, earthy, and ferruginous in its aspect, while the feldspar retains its brilliancy. The partial analysis of such a specimen of the syenite gave only 0·56 of lime, and traces of magnesia, with 3·75 per cent of peroxyd of iron, and of alkalies, potash 4·43, soda 4·35. This large proportion of soda is also to be remarked in the orthophyre just described, and in the red orthoclase-gneiss of this region, a portion of which gave 3·86 per cent of potash and 3·70 of soda; while the red orthoclase from the rocks of this Laurentian series, named perthite by Dr. Thompson, gives in like manner 6·37 of potash to 5·56 of soda. A nearly pure potash-orthoclase, generally white in color, is however found in some of the stratified Laurentian rocks. (*Geology of Canada*, p. 474.)

This syenite of Grenville has in some portions undergone a peculiar decomposition, which has reduced it to a soft greenish matter having the aspect of serpentine, or rather of pyrallolite. This change has been remarked only in the vicinity of some remarkable veins of chert which are here found cutting the syenite, and as described by Sir W. E. Logan, is more or less complete for a distance of two hundred yards on each side of them. In specimens of this altered rock, the quartz remains unchanged; while the feldspar, still preserving its cleavages, has a hardness no greater than carbonate of lime. It is somewhat unctuous to the touch, with a feeble waxy lustre, and its color is occasionally reddish, but more often of a pale green. Such a specimen was selected for analysis, and gave of silica 80·65, alumina 12·60, lime 0·60, soda and a little potash 2·65, volatile 2·10, magnesia and oxyd of iron, traces = 98·60. From this result it appears that the feldspar of the syenite has lost nearly two-thirds of its alkali; the iron and other bases having also for the most part disappeared. This removal of the protoxyd bases would appear, from the character of the resulting mineral, to be different from that which takes place during the kaolinization of feldspar. The nature of the process requires further investigation, but it was not improbably connected with the deposition of the adjacent chert or hornstone. This substance, according to Sir W. E. Logan, forms two large veins which cut the syenite vertically, and have a breadth of from four to seven feet. It is generally arranged in bands or layers parallel to the walls of the veins, and varying in color from white to yellowish and flesh-red. The mineral has the chemical characters of flint or buhrstone, and like the latter presents numerous irregular cells, the walls of which are sometimes incrusted with crystals of quartz, and in other cases bear the impression of small cubes, perhaps of crystals of fluorspar, which have themselves disappeared. The relations of these singular veins of silex show that it cannot be of sedimentary origin, and it can scarcely be doubted that it is an aqueous deposit, and results from a similar process to that which on a lesser scale gives rise to agate and chalcedony in various rocks. (*Geology of Canada*, p. 41.)

### TRACHYTES.

Under this head we shall describe a class of rocks which are very abundant in Eastern Canada, and present a great variety of aspects. There are many dikes in the vicinity of Montreal which resemble some of the typical trachytic rocks of Auvergne and of the Rhine; while the rocks of the mountains of Brome and Shefford consist almost entirely of distinctly crystalline feldspar. These will be described as granitoid trachytes, under which head may also be included a somewhat similar rock from Yamaska mountain.

*Brome and Shefford Mountains.*—The trachytes of Brome and Shefford occupy two considerable areas near to each other, and, as already stated, are the easternmost of the eruptive masses now under description. The larger area covers about twenty square miles in Brome and the western part of the township of Shefford. It consists of several rounded hills, of which the principal are named Brome and Shefford mountains, and rise boldly about 1,000 feet above the surrounding plain. The rock shows divisional planes, giving it an aspect of stratification, and separates by other joints into rectangular blocks. The second area includes about nine square miles in the township of Shefford, to the northwest of the last, and at the nearest point is only about two miles removed from it. This is known as Shefford mountain.

The rocks of these two mountainous areas present but very slight differences, being, so far as examined, everywhere made up in great part of a crystalline feldspar, with small portions of brownish-black mica, or of black hornblende, which are sometimes associated. The proportion of these two minerals is never above a few hundredths, and is often less than one hundredth. The other mineral species are small brilliant crystals of yellowish sphene, and others of magnetic iron, amounting together probably to one thousandth of the mass. In some finer-grained varieties a few rare crystals of sodalite and of nepheline are met with. But for the uniform absence of quartz, these rocks might be taken for varieties of granite and syenite. They are very friable, and subject to disintegration, so that the soil for some distance around these mountains is almost entirely made up of the separated crystals of feldspar, which however show but little tendency to decomposition, and retain their lustre. The rock is sometimes rather finely granular in its texture, but is often composed of cleavable masses of orthoclase, which are from one-fifth to one-half of an inch in breadth, and sometimes nearly an inch in length. The lustre is vitreous, and in the opaque varieties, pearly; but the crystals never exhibit the eminently glassy lustre nor the fissured appearance that characterizes the feldspars of many European trachytes which are similar to them in compotion. The color of the feldspar of these rocks is white, passing into reddish on the one hand, and into pearl-gray or lavender-gray on the other.

Specimens of the rock of Brome mountain were taken from the side near to the village of West Shefford. It was coarsely crystalline, lavender-gray in color, and contained a little brown mica, sphene, and magnetic iron, but no hornblende. The density of fragments of the rock was found to be 2·632–2·638. Selected grains of the feldspar had a specific gravity of 2·575, and gave by analysis the result II. The analysis of a second specimen from another portion of the hill, is given under III.

The rock from the south side of Shefford mountain was next examined. In one part it consisted of a coarse grained grayish-white feldspar with a little black mica, and closely resembled the rock just described from the adjacent mountain. A little lower down the hill, however, was a variety which, though completely crystalline, was more coherent and finer grained than that of Brome, the feldspar rarely exhibiting cleavage-planes more than a fourth of an inch in length. Brilliant crystalline grains of black hornblende, about the size of grains of rice, were sparingly disseminated through the mass, together with very small portions of magnetite and yellowish sphene. Fragments of the rock had a density of 2·607–2·657. The feldspar was yellowish-white and sub-translucent, with a somewhat pearly lustre. By crushing and washing the mass, the grains of feldspar were separated from the heavier minerals, and found to have a specific gravity of 2·561. The results of its analysis, which scarcely differs from that of Brome, is given under IV.

| | II. | III. | IV. |
|---|---|---|---|
| Silica, - - - - - - | 65·70 | 65·30 | 65·15 |
| Alumina, - - - - - | 20·80 | 20·70 | 20·55 |
| Lime, - - - - - - | ·84 | ·84 | ·73 |
| Potash, - - - - - - | 6·43 | .... | 6·39 |
| Soda, - - - - - - | 6·52 | .... | 6·67 |
| Volatile, - - - - - - | ·50 | .... | ·50 |
| | 100·79 | | 99·99 |

*Yamaska mountain.*—About twelve miles to the north of west from Shefford mountain rises the hill of intrusive rock known as Yamaska mountain, which has an area of about four square miles, and breaks through the strata of the Quebec group near the line of the great dislocation which brings these up against the limestones of the Trenton group. The southeastern part of this hill consists of a granitoid diorite hereafter to be noticed; but the greater portion of the mass may be described as a granitoid trachyte, differing in aspect from that of Brome and Shefford, in being somewhat more micaceous and more fissile. The mica, which is dark brown, is in elongated flakes, and there is neither hornblende nor quartz in the specimens collected, which however hold small portions of magnetite, and minute crystals of amber-yellow sphene. These seem to be contained in veins of segregation, which are of a lighter color than the mass. The cleavable feldspar grains, which make up by far the greater part of the rock, are brilliant, with a vitreous lustre, and are often yellowish or reddish-gray in color. A portion of this feldspar separated by washing from the crushed mass of the rock, had a specific gravity of 2·563, and gave by analysis the result V. Another portion of selected grains of the feldspar gave VI. Both specimens were however somewhat impure.

| | V. | VI. |
|---|---|---|
| Silica, - - - - - - - | 61·10 | 58·60 |
| Alumina, - - - - - - - | 20·10 | 21·60 |
| Peroxyd of iron, - - - - - | 2·90 | 2·88 |
| Lime, - - - - - - - - | 3·65 | 5·40 |
| Magnesia, - - - - - - - | ·79 | 1·84 |
| Potash, - - - - - - - - | 3·54 | 3·08 |
| Soda, - - - - - - - - | 5·93 | 5·51 |
| Volatile, - - - - - - - - | ·40 | ·80 |
| | 98·41 | 99·71 |

Besides these great trachytic hills, numerous smaller masses of different varieties of trachyte, in the form of dikes and beds, are found along the line of country between Rigaud and Yamaska mountains. The diorite of the latter is cut into dikes of a white or brownish-gray trachyte, which is often porphyritic, and may be connected with the great mass just described.

*Chambly.*—At Chambly a mass of porphyritic trachyte is intruded in the form of a bed among the strata of the Hudson River formation; and about midway on the Chambly canal a similar trachyte is met with, which contains in drusy cavities, crystals of quartz, calcite, analcime, and chabazite. The base of this rock is of a pale fawn color, and appears at the first sight to be micaceous; but on closer examination it is seen to be almost entirely feldspathic. Minute portions of pyrites, and grains of magnetic iron are rarely met with, and small scales of a dark green micaceous mineral are very sparsely disseminated. The crystals of orthoclase, which are very abundant, are sometimes an inch in length, and one-fourth of an inch in thickness; they are more or less modified, and terminated at both ends. They are easily detached from the rock, and are yellowish and opaque on the exterior, but the inner portions of the large crystals are transparent and vitreous. The composition of the crystals is given under VII. The paste of this porphyry, when carefully freed from crystals, lost by ignition 2·1 per cent. When pulverized and digested with dilute nitric acid, it effervesced slightly, giving off carbonic acid, together with red fumes, arising in part from the oxydation of the pyrites. The portion thus dissolved equalled carbonate of lime 1·76, carbonate of magnesia 0·98, peroxyd of iron with a trace of alumina 2·12 per cent. The residue dried at 300° F. gave the result VIII.

| | VII. | VIII. |
|---|---|---|
| Silica, | 66·15 | 67·60 |
| Alumina, | 19·75 | 18·30 |
| Peroxyd of iron, | .... | 1·40 |
| Lime, | ·95 | ·45 |
| Potash, | 7·53 | 5·10 |
| Soda, | 5·19 | 5·85 |
| Volatile, | ·55 | ·25 |
| | 100·12 | 99·85 |

The paste of this trachyte thus differs but little from the crystals in composition. It contains only a slight excess of silica, and seems to be made up of lamellæ of orthoclase, mingled with small portions of carbonates of lime and magnesia. A part of the iron also is probably present as carbonate, which, by its decomposition, gives rise to the rusty red color of the weathered surface of the trachyte.

*Montreal.*—The island of Montreal offers a great variety of trachytic rocks, which traverse both the Lower Silurian strata, and the dolerite of Mount Royal. Some of these dikes are finely granular, occasionally crumbling to sand, and frequently are earthy in texture. In some cases they assume a concretionary structure, and they are often porphyritic from the presence of feldspar or hornblende. One variety exhibits large feldspar crystals in a compact purplish or lavender-gray base, with a waxy lustre. This effervesces with acids from an admixture of earthy carbonates, and closely resembles in its aspect certain trachytes from the Siebengebirge on the Rhine. Other varieties can scarcely be distinguished from the so-called domite, the trachyte of the Puy de Dôme, and exhibit small drusy cavities. The presence of carbonates in trachytic rocks has generally been overlooked; Deville however found seven per cent of carbonate of lime in a trachytic rock from Hungary, and it occurs disseminated in some of the trachytes of the Siebengebirge. Some of the trachytes about to be described contain moreover carbonates of magnesia and protoxyd of iron, and weather to some depth of a reddish-brown color from the peroxydation of the latter, like the trachyte from Chambly just noticed. Acids remove from many of these rocks, in addition to the carbonates, portions of alumina and alkalies. These are derived from a soluble silicate, which in the trachytes of Brome appears only as rare crystals of nepheline, and in Chambly as analcime and chabazite. In some of the compact and earthy varieties about Montreal, however, this soluble silicate exists to a large extent, and has the composition of natrolite. By this admixture of a zeolite the trachytes pass into phonolite.

The first of these trachytes which will be noticed forms a dike near McGill College. The rock is divided by joints into irregular fragments, whose surfaces are often coated with thin bladed crystals of an aluminous mineral, apparently zeolitic. Small brilliant crystals of cubic iron-pyrites, often highly modified, are disseminated through the mass. The rock has the hardness of feldspar, and a specific gravity of from 2·617 to 2·632. Its color is white, passing into bluish and grayish-white; it has a feebly shining lustre, and is slightly translucent on the edges, with a compact or finely granular texture, and an uneven sub-conchoidal fracture. Before the blowpipe it fuses with intumescence into a white enamel. The rock in powder is attacked even by acetic acid, which removes 0·8 per cent of carbonate of lime, besides 1·5 per cent of alumina and oxyd of iron; the latter apparently derived from a carbonate. Nitric acid dissolves a little more lime, oxydizes the pyrites, and takes up, beside alumina and alkalies, a considerable portion of manganese. This apparently exists in the form of sulphuret, since, while it is soluble in dilute nitric acid, the white portions of the rock afford no trace of manganese before the blowpipe; although minute dark colored grains, associated with the pyrites, were found to give an intense manganese reaction. From the residue after the action of the nitric acid, a solution of carbonate of soda removed a portion of silica; and the remainder, dried at 300° F., was free from iron and from manganese. Its analysis is given under IX, while that of the matter dissolved by nitric acid and carbonate of soda from 100 parts of the rock will be found under IX *a*.

A dike of trachyte near to the last, and very similar to it in appearance, was submitted to the action of nitric acid, but the insoluble residue was not treated by carbonate of soda. Its analysis is given under X, while that of the soluble matters is to be found under X *a*. A white trachyte from a dike at Lachine resembled the preceding, but was somewhat earthy in its aspect, and effervesced with nitric acid, which removed a portion of lime equal to 7·40 per cent of carbonate. On boiling the pulverized rock with nitrate of ammonia, an amount of lime equal to 5·33 per cent of carbonate was dissolved. An accident prevented the complete determination of the alkalies in the feldspathic residue of this trachyte; and the soluble silica was not removed previous to the analysis, whose result is given under XI. The proportion of the potash to the soda was however found to be, by weight, nearly as two to three. The matters dissolved by nitric acid will be found under XI *a*.

Another dike of trachyte from Lachine was concretionary, and stained by infiltration; the interior of the concretions was white and earthy. The substances removed from 100 parts of the rock by nitric acid and carbonate of soda, are given under B. A partial analysis of the insoluble residue showed it to be a feldspar allied to those of the preceding trachytes; the quantities of potash and soda were however nearly in the ratio of four to three.

A large dike of trachyte in the limestone quarries at the Mile End, near Montreal, is remarkable for the amount of carbonates which it contains. It is grayish-white, with dark gray spots, granular, sub-vitreous in lustre, and holds a few crystals of hornblende. By ignition it loses 11·0 per cent of its weight. In powder it effervesces freely with nitric acid, disengaging carbonic acid, and, when heat is applied, red fumes from the peroxydation of the iron. One hundred parts of the rock yielded in this way the soluble matters given under XII *a*. The composition of the residue, from which the soluble silica was not removed, is given under XII.

| | IX. | X. | XI. | XII. |
|---|---|---|---|---|
| Silica, - - - - | 63·25 | 62·90 | 58·50 | 61·62 |
| Alumina, - - - - | 22·12 | 23·10 | 24·90 | 21·00 |
| Lime, - - - - | ·56 | ·45 | ·45 | 2·69 |
| Potash, - - - - | 5·92 | 2·43 | .... | 4·66 |
| Soda, - - - - | 6·29 | 8·69 | .... | 5·35 |
| Volatile, - - - - | ·93 | 1·40 | 2·10 | 237 |
| | 99·07 | 98·97 | | 97·69 |

A second determination of the alkalies in a portion of the trachyte, IX, which had not previously been treated by acid, gave potash 5·40, and soda 6·49. A second analysis of X. gave potash 2·28, and soda 7·95.

| | IX *a.* | X *a.* | XI *a.* | *b.* | XII *a.* |
|---|---|---|---|---|---|
| Silica, - - - - | 1·43 | .... | .... | 5·00 | .... |
| Alumina, - - - - | 2·43 | .... | 1·27 | 1·32 | 4·84 |
| Peroxyd of iron, - - | 2·40 | 2·84 | 1·47 | 2·51 | 2·63 |
| Lime, - - - - - | ·60 | 1·86 | 4·14 | 3·50 | 6·49 |
| Magnesia, - - - | .... | .... | 1·34 | 1·35 | 1·70 |
| Potash, - - - - | ·40 | ·25 | undet. | undet. | undet. |
| Soda, - - - - - | ·98 | ·21 | " | " | " |
| Red oxyd of manganese, - | 1·31 | ·87 | .... | .... | .... |

Of the matters soluble in nitric acid in the last-described trachyte, XII, the lime in the form of carbonate would equal not less than 11·60 per cent, the magnesia 3·58, and the iron 3·82 per cent of carbonate, in which condition by far the greater part of these bases are probably present.

### Phonolite.

Associated with the numerous trachytic dikes at Lachine is one of the phonolite already referred to. It is brittle and somewhat schistose, breaking into angular fragments, and appears to consist of a reddish fawn-colored base, in which are disseminated greenish-white rounded masses, often grouped, and apparently concretionary in their structure. These greenish portions are sometimes half an inch or more in diameter, and cover from one-third to one-half of the surfaces. They are not very distinctly seen unless the rock is moistened. The hardness of the different portions does not greatly vary, and is nearly that of apatite. The specific gravity is very low, being only 2·414. The mass contains small cavities filled with carbonate of lime, which is rarely stained purple: it is also found in small films in the joints. The rock is granular in its fracture, without lustre, and is feebly translucent at the edges. When pulverized, and treated with nitric acid of specific gravity 1·25, a slight effervescence ensues, with abundant red fumes. The mass grows warm and gelatinizes; and on washing out the acid solution, and treating the insoluble portion with a solution of caustic soda, a white granular residue remains. These reactions are obtained both with the fawn-colored and the greenish portions, but the amount of insoluble matter is greater from the last. The rock is but slightly hygroscopic: a portion of it in powder lost only 0·2 per cent by a prolonged exposure to 212° F., but 7·10 per cent at a red heat.

For the quantitative analysis, the method already indicated was followed. It was found that while a dilute solution of caustic soda removed all of the gelatinous silica separated by the acid, it took up only a trace of alumina; leaving a feldspathic residue which was no longer attacked by nitric acid. The silica was separated from the alkaline liquid, and the acid solution was found to contain, beside alumina and soda, a little potash, some lime, magnesia, and iron, and traces of manganese. The greater part of the lime is evidently present as carbonate; for when a portion of the pulverized phonolite, which gave to nitric acid lime equal to 4·36 per cent of carbonate, was boiled with a solution of nitrate of ammonia, there were dissolved 3·87 per cent of carbonate of lime, beside which there was a separation of a considerable amount of oxyd from the decomposed carbonate of iron. From this reaction, and from the entire absence of sulphur, which was carefully sought for, it is probable that the whole of the iron, except the small portion of peroxyd which colors the rock, exists in the state of carbonate. In the following analyses, therefore, the lime and the iron, as well as a little magnesia, are calculated as carbonates. XIII. is the result obtained with four grams of the reddish portion of the phonolite, as free as possible from the green; and XIV. was obtained with two and a half grams of a mixture of the two colors.

| | XIII. | XIV. |
|---|---|---|
| Soluble silicate, zeolite (*a*), by difference, | 46·57 | 36·16 |
| Insoluble silicate, feldspar (*b*), | 45·75 | 55·40 |
| Carbonate of lime, | 3·63 | 4·36 |
| " iron, | 3·52 | 3·72 |
| " magnesia, | ·53 | ·36 |
| | 100·00 | 100·00 |

In order to fix the composition of the soluble silicate, the larger amount of the insoluble residue and that of the separated silica, alumina, and alkalies, having been carefully determined, and the lime, magnesia, and oxyd of iron calculated as carbonates, the water was estimated by the loss. In this way were obtained the results given under XIII *a*, and XIV *a*; while the analyses of the insoluble silicate, which is a potash feldspar, are given under XIII *b*, and XIV *b*.

| | XIII *a*. | XIV *a*. | Natrolite. | Analcime. |
|---|---|---|---|---|
| Silica, | 51·96 | 51·66 | 47·40 | 54·06 |
| Alumina, | 24·42 | 24·88 | 26·09 | 23·20 |
| Soda, | 12·93 | 13·05 | 16·02 | 14·10 |
| Potash, | 1·15 | 1·28 | .... | .... |
| Water, | 9·54 | 9·13 | 9·05 | 8·10 |
| | 100·00 | 100·00 | 100·00 | 100·00 |

In composition, this zeolitic mineral is intermediate between analcime and natrolite; but the readiness with which it gelatinizes with acids leads to the conclusion that it belongs, in great part at least, to natrolite. The theoretical composition of these two zeolites is, for the sake of comparison, placed alongside of the two analyses of the soluble portion of the phonolite.

| | XIII *b*. | XIV *b*. |
|---|---|---|
| Silica, | 59·70 | 60 90 |
| Alumina, | 23 25 | 24 45 |
| Lime, | ·99 | ·45 |
| Potash, | 9·16 | undet. |
| Soda, | 2 97 | " |
| Volatile, | 2 23 | 2·10 |
| | 98·30 | |

The feldspars of the above trachytes and phonolite offer some considerable variations in their composition, especially in the proportions of the alkalies. In IX, the proportions of potash and soda are nearly the same as in the trachytes of Brome, Shefford, and Chambly, and the same is true of XII. These are doubtless to be regarded as varieties of orthoclase with a large amount of soda, while in the feldspar from the phonolite the proportion of soda is very small. In X, on the contrary, the large predominance of soda indicates a composition approaching that of albite. It is further apparent, from a comparison of the feldspars of the other trachytes whose complete analyses are not given, that the proportions of the alkalies are liable to considerable variation, even in adjacent and apparently similar dikes. All of the above feldspars are probably to be referred to orthoclase, or to albite; but these, in the earthy trachytes, have undergone a commencement of decomposition, which consists in the loss of a portion of silica and alkali, and the combination of water, resulting in the formation of kaolin. An admixture of this substance will explain the increased amount of alumina, the deficiency of silica, and the presence of water in the feldspars of the more earthy of these trachytes.

These trachytic dikes are not confined to the vicinity of Montreal. To the southward, on the shores of Lake Champlain, there is found in and about Burlington, Vermont, a vast number of dikes of intrusive rock; some of which appear to intersect the strata of the Quebec group, and others those of the Trenton group. Some of these are described as being of greenstone; and others, as a white or yellowish-white feldspathic rock, often porphyritic from the presence of feldspar crystals. The base of a yellowish-gray porphyritic dike from Shelburne, having a rough fracture, and a specific gravity of 2·60, gave to Prof. G. F. Barker, silica 67·30, alumina and peroxyd of iron 19·10, lime 0·79, magnesia, traces, potash 4·74, soda 6 04, volatile 1·70 = 99·67. It contained a little intermingled quartz; and the mass resulting from the fusion of the rock with an alkaline carbonate, afforded traces of a sulphuret. (*Geology of Vermont*, pp. 579–707.)

Somewhat to the south of Burlington, on the west side of Lake Champlain, and near to Essex, there is a great mass of intrusive rock, found in the slates of the Hudson River formation. As described by Emmons, it is interstratified in an irregular manner among the layers of the unaltered sedimentary rocks, and has a fissile and schistose structure, which gives, at first sight, the aspect of stratification to what is undoubtedly an intrusive rock. When exposed to the action of the waves on the lake-shore, its structure appears to be columnar, and sometimes concretionary. This rock is described as composed of a reddish or pale leek-green compact feldspar, holding crystals of the same mineral. (*Geology of New York*, vol. ii, p. 84.) These intrusive feldspathic rocks on Lake Champlain resemble closely the trachytes of Montreal and Chambly,—with the latter of which, the trachyte of Shelburne, the only one of them which has been chemically examined, closely agrees in composition.

[To be continued.]

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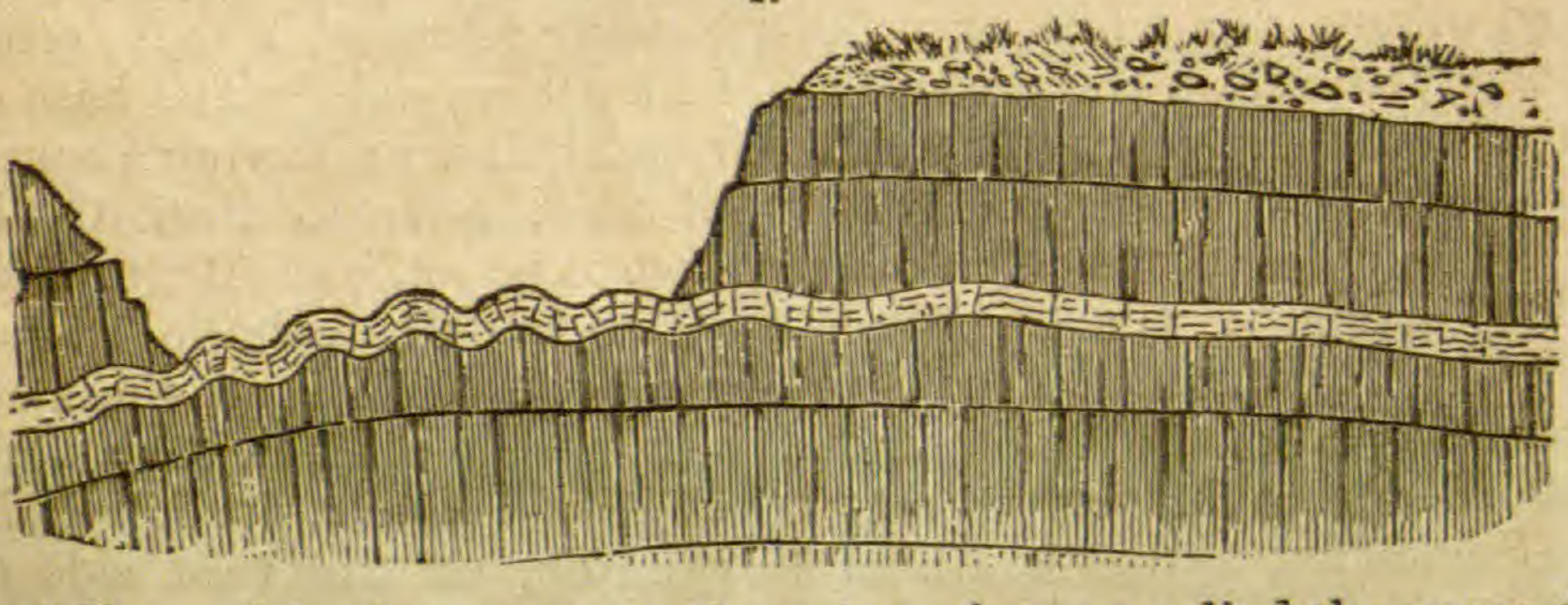
ART. XI.—*On the so-called "Barrel-Quartz," of Nova Scotia;* by B. SILLIMAN, Jr.[1]

ON Laidlaw's Hill, forming the eastern division of the Waverley Gold District, has been found, in great abundance, a peculiar variety of quartz-rock which has acquired a wide reputation under the name of *barrel-quartz.*

Mr. Phillips, of London, has thus described it:

"The most remarkable deposit of auriferous quartz hitherto found in Nova Scotia is undoubtedly that at Laidlaw's Farm. The principal workings are here situated near the summit of a hill composed of hard, metamorphic shales, where openings have been made, to the depth of four or five feet, upon a nearly horizontal bed of corrugated quartz of from eight to ten inches in thickness. This auriferous deposit is entirely different from anything I had before seen, and when laid open presents the appearance of trees or logs of wood laid together side by side, after the manner of an American corduroy road.

1.



"From this circumstance the miners have applied the name of 'barrel-quartz' to the formation, which, in many cases, presents

[1] From a Report by B. SILLIMAN, Jr., on the Waverley Gold Mining Co., Boston, 1864.

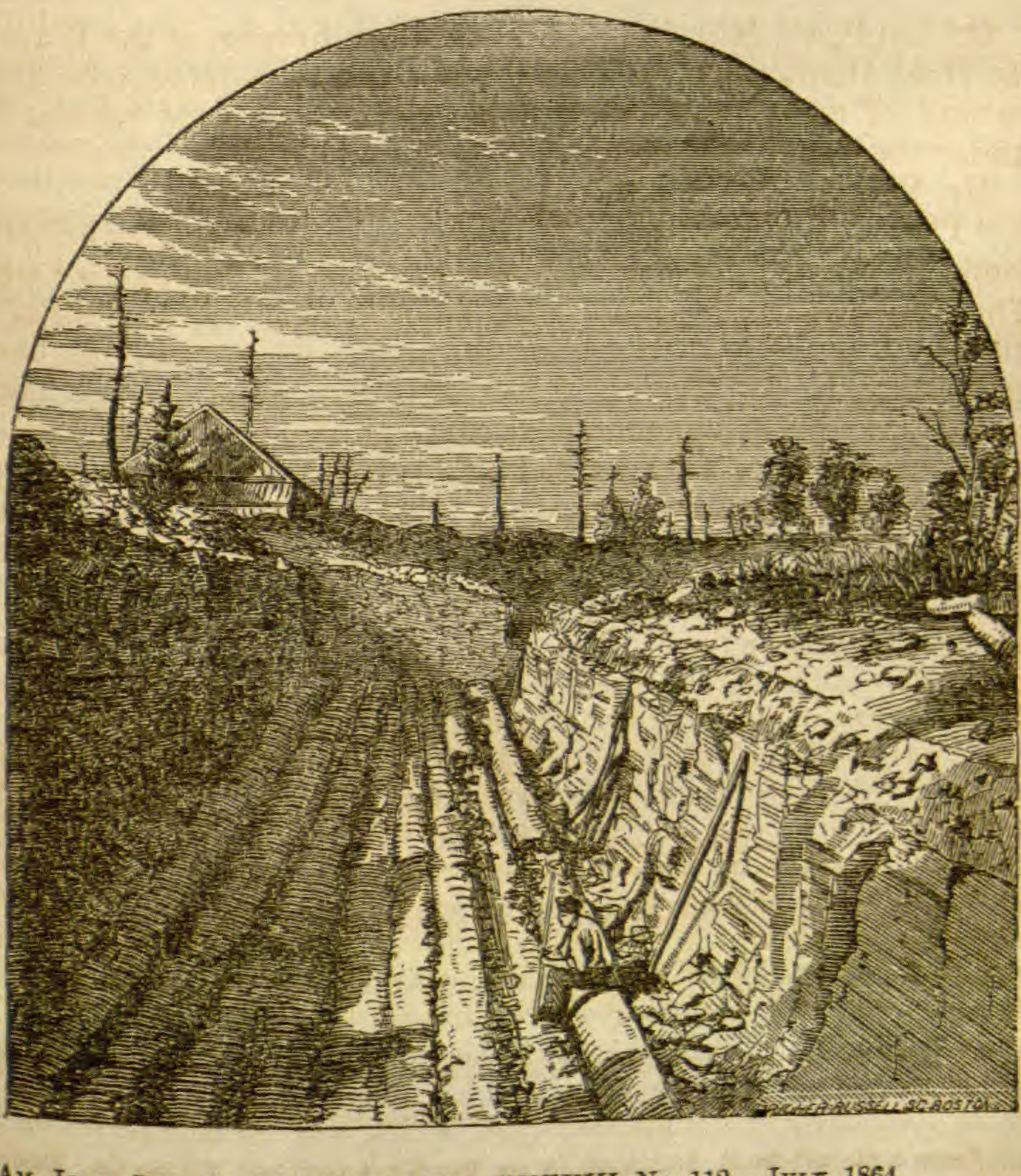
an appearance not unlike a series of small casks laid together side by side and end to end.

"The rock covering this remarkable horizontal vein is exceedingly hard; but beneath it, for some little distance, it is softer and more fissile. The quartz is itself foliated parallel to the lines of curvature, and exhibits a tendency to break in accordance with these striæ.

"The headings, and particularly the upper surfaces of the corrugations, are generally covered by a thin bark-like coating of brown oxyd of iron, which is seen frequently to enclose numerous particles of coarse gold, and the quartz in the vicinity of this oxyd of iron is itself often highly auriferous."

The accompanying section, (fig. 1) which I have prepared from a sketch of the place as I saw it in December, will, together with the following perspective view of the opening, convey a clear idea of its peculiar structure.

2.



Only the corrugations in the open part of the cut are visible; the extension of the vein to the right and left, in fig. 1, is ideal, the superincumbent mass covering it. I measured, however, the quartzite above, dipping to the right and left at a small angle, and I think no geologist would doubt that the crest of an anticlinal axis here comes to the surface and has escaped the denudation which has removed the top of the crest in most places. The corrugations, or folds, appear to be accounted for on the hypothesis of a lateral thrust producing the undulations. The perspective view (fig. 2) of this interesting locality was taken from a stereoscopic photograph, showing the appearance of the barrel-quartz after the surface-rock (quartzite) had been removed, and before the miners had broken up the quartz layer for removal.

The value of the barrel-quartz has been not so much from its large average yield of gold as from the comparative cheapness with which it has been mined. Thus it appears from the statements in the Chief Gold Commissioner's Report, dated Jan., 1863, that each miner on Laidlaw's Farm averaged for the last three months of the previous year over nine tons per month, while in other districts the average monthly product per man was from two to three tons. The average yield of gold was small,—about five pennyweights to the ton; the maximum being three ounces, not including remarkable discoveries, like that of the Chebucto Company, of a mass of this quartz, yielding, as already mentioned in the Introduction, for a volume of not over two cubic feet, over $4000 in value, of gold.

# SCIENTIFIC INTELLIGENCE.

## I. PHYSICS AND CHEMISTRY.

1. *On the constitution of the Sun.*—Magnus has communicated a brief note in which he endeavors to show that purely thermic relations permit us to draw important inferences as to the physical character of the sun. If we observe the quantity of heat which is radiated from a non-luminous gas flame and then introduce soda into the flame, the quantity of heat radiated will be increased. The experiment was made in such a manner that a definite portion of the soda flame was constantly compared with the same part of the non-luminous flame, but so that the soda introduced into the flame could not radiate directly toward the thermo-electric pile. Although the temperature of the flame was lowered by the introduction of the soda, yet one-third more heat was radiated. When a plate of platinum was introduced into the flame instead of the soda, the quantity of heat radiated was much greater than before, although the plate absorbed more heat from the flame than the soda. With the plate of platinum employed, which was 55 millimetres broad, the amount of heat radiated was nearly twice as great as that of

the non-luminous flame. Variations in the thickness of the plates of platinum employed made no sensible difference in the heat radiated, provided that the plates had the same diameter. When the plate of platinum was covered with carbonate of soda, the radiation increased in a remarkable degree, being one-half greater than that of the plate alone. The quantity of heat radiated was still further increased when, in addition to the covered platinum plate, the flame contained soda vapor arising from a little soda placed below the plate in the flame and not itself radiating heat to the pile. Under these circumstances the covered plate radiated nearly three times as much heat as the flame alone. Salts of strontium and lithium behaved like salts of sodium. These experiments show that gaseous bodies radiate very much less heat than solids or liquids, and that consequently it can scarcely be assumed, that the gaseous photosphere of the sun is the source of the sun's heat. They show also that ignited soda at the same temperature has a much higher radiating power than ignited platinum. Finally, they prove that soda vapors absorb very little of the heat which is radiated by the solid or fluid ignited body, for the radiation of the solid body in the flame filled with the vapor of soda was but little less than the sum of the radiation of the solid alone and the vapor alone. This behavior of ignited soda in the fluid and gaseous condition, confirms the views of Kirchhoff on the constitution of the sun in a very striking manner.—*Pogg. Ann.*, cxxi, 510. W. G.

2. *On the Spectrum of Thallium.*—W. A. Miller has found that when pieces of thallium are made to form the electrodes of an induction coil, the spark exhibits in the spectroscope five new lines in addition to the well known characteristic green line. The new lines are a very feeble line in the orange, two almost equally intense in the green and more refrangible than Tl$\alpha$, a third and weaker line in the green, and finally a bright sharp line in the blue. All these lines could be observed in an atmosphere of hydrogen, excepting the weakest, but they were in this case less intense. The photographed thallium spectrum exhibits a very characteristic group of lines resembling the spectra of cadmium and zinc, and somewhat less that of lead.—*Phil. Mag.*, xxvi, 223. W. G.

3. *On the photographic transparency of various bodies, and on the photographic effects of metallic and other spectra obtained by means of the electric spark.*—W. A. Miller has published an interesting paper upon photographic transparency and kindred subjects, from which we extract the principal results only, referring for details to the original memoir. The author employed prisms, lenses, and plates of rock crystal to avoid the effect of absorption due to the use of glass. It was found that—

(1.) Colorless bodies which possess equal powers of transmitting the luminous rays vary greatly in permeability to the chemical rays.

(2) Diactinic solids (that is to say, solids which are permeable to the chemical rays) preserve their diactinic power both when liquefied and when converted into vapor.

(3.) Colorless solids which are transparent to light, but which exert a considerable absorptive effect upon the chemical rays, preserve their absorptive power with greater or less intensity both in the liquid and the gaseous state.

These conclusions are equally true as regards liquids, whether the compound be dissolved in water or liquefied by heat. Water is perfectly permeable to the chemical rays, and the process of solution does not interfere with the special action of the substance dissolved upon the incident rays. This fact makes it possible to submit to examination a great variety of saline bodies which cannot be obtained in large or limpid plates. Of the substances examined by the author no one was found to surpass rock crystal in diactinic power. Water, ice and white fluor spar rival it, and pure rock salt approaches it very closely. With respect to saline bodies, it was found that the *fluorids* rank first in diactinic power; then follow in order the chlorids, bromids, and iodids. The sulphates, carbonates, and borates of the alkalies and earths are largely diactinic, the phosphates less so, and the arseniates still less. The sulphites are less diactinic than the sulphates, and the hyposulphites less than the sulphites. Nitric acid and the nitrates exert a specific absorptive action, interrupting the spectrum abruptly at the same point whatever be the base, provided that this be capable of forming diactinic salts. Among bases, potash, soda, ammonia, lime, baryta, strontia, magnesia and alumina are eminently diactinic. The oxyds of zinc, mercury and lead approach them in power; but colored bases, like the oxyds of iron, nickel, cobalt and copper are very inferior, and when the salts formed are green or yellow, they are nearly opaque. Notwithstanding the high diactinic quality of silica, none of the different varieties of glass transmit rays extending beyond one-fifth or one-sixth of the range afforded by quartz. A plate of glass less than $\frac{1}{100}$th of an inch in thickness cuts off the more refrangible rays almost as completely as a plate of twenty times the thickness. Of the liquids examined, water is most diactinic, and next in order alcohol. Among gases, air, hydrogen, carbonic acid, carbonic oxyd and ammonia are about equally diactinic with each other and with water. Other gases and vapors exhibit a marked absorptive power, and this is especially the case with the compounds of sulphur. Chlorine, bromine and iodine absorb most powerfully the least refrangible rays, which is an exception to the general rule; peroxyd of nitrogen and peroxyd of chlorine in a stratum two feet in length wholly absorb the chemical rays; but when more dilute or in shorter columns, they give peculiar absorption bands. Aqueous vapor is highly diactinic, though not diathermic.

The reflecting power of different polished surfaces for the chemical rays was found to be very different for different substances, for the same angle of reflection. Gold reflects all the rays very equally, though somewhat feebly, and next to gold ranks burnished lead. These two metals reflect all the light of the silver-spectrum, or 74 divisions of the author's scale, while with other metals the reflected spectrum covered only 63 divisions of the same scale. In examining the electric spectra of different metals taken in air, the author found that as we advance toward the more refrangible end of the spectrum, the lines become less intense in the central portions; until toward the extreme limit the two marginal ends alone are visible, so as to give the appearance of two dots of color separated by a dark space. This appears to be due to the less intensity of the electric spark in the centre. The "dots" may usually be seen to consist of groups of very short lines closely aggregated. The spectra of alloys usually exhibit the lines characteristic of each constituent, but this is not the case when one constituent is present in comparatively very small quantity. Thus plumbago, containing 3·94 per cent of iron, gave a distinct iron-spectrum, but no indication of iron was observable in the spectrum of brass containing 0·23 per cent of iron, or of lead in brass containing 0·7 per cent of this metal. The examination of the photographic effects of electric spectra produced by transmitting the spark through gases other than air, led to the following conclusions, which correspond with those of Angström, Alter, and Plücker.

(1.) Each gas tinges the spark of a characteristic color; but no judgment can be formed from this color of the kind of spectrum which the gas will furnish.

(2.) In most cases, in addition to the lines peculiar to the metal used as electrodes, new and special lines characteristic of the gas, if elementary, or of its constituents, if compound, are produced. When compound gases are employed, the special lines produced are not due to the compound as a whole, but to its constituents.

(3.) The lines due to the gaseous medium are continuous, not interrupted or broken into dots.—*Journal of Chemical Society*, [2], xi, 59.

W. G.

4. *On the spectrum of chloro-chromic acid.*—GOTTSCHALK and DRECHSEL have observed that the vapor of chloro-chromic acid, $CrO_2Cl$, produces a spectrum containing 17 characteristic rays, of which 3 are violet, 8 green, 1 yellow, 3 orange, and 2 red. The most brilliant are the violet and one of the green. When the vapor is mixed with oxygen previously to its introduction into the flame, we obtain a spectrum of a brilliancy which the eye can scarcely support; the violet rays appear with peculiar brightness, and might be confounded with those of rubidium, though in reality they are somewhat less refrangible than these. Neither chlorine nor chlorid of chromium alone in the flame produce characteristic rays, so that the observed rays are due exclusively to the chlorid of chromium. —*Bull. de la Soc. Chimique de Paris*, Jan., 1864, p. 20. W. G.

5. *On the condensation of vapors upon the surface of solid bodies.*—MAGNUS has observed that a thermo-electric pile becomes warmer when brought into contact with moist air which has the same temperature as the pile, and becomes colder whey dry air of the same temperature passes over it. This phenomenon may be explained by the assumption that the surface of the pile absorbs aqueous vapor from the atmosphere, and thus becomes warmer by the latent heat which becomes sensible; and that the dry air again takes up the vapor from this surface, which becomes colder by evaporation. This holds good not only for surfaces covered with lampblack, but, which is very remarkable, even for clean metallic surfaces. Thus, thin platinum foil laid upon the face of the pile became warmer or colder according as moist or dry air was passed over it, and the same was true for plates of other metals. Finally, the results of a very careful series of experiments justify the generalization that all substances whatever become warmer when air containing more moisture than that which surrounds them impinges upon them, and colder when they are brought in contact with air containing less moisture than that in which they are placed. It further appears from the experiments of

Magnus that the most different vapors are condensed upon the surfaces of solid bodies to such a degree that appreciable changes of temperature result. Hence it follows that at all times a layer of condensed vapors exists upon the surfaces of bodies, which becomes greater or less with the state of moisture of the atmosphere.—*Pogg. Ann.*, cxxi, 174. W. G.

6. *On the influence of condensation in experiments on diathermancy.*—MAGNUS, in a brief reply to Tyndall's criticism on his previous remarks upon the influence of the condensation of moisture upon the thermo-electric pile, maintains without reservation the position which he at first held, namely, that air containing moisture permits the passage of rays of heat with almost the same facility as dry air.—*Pogg. Ann.*, cxxi, 186. W. G.

7. *Mr. Scoutetten on the electricity of the blood.*—At a recent meeting of the Paris Academy, Dr. Scoutetten returned to the important question of the electricity of the blood, and gave a historical sketch of the observations which have been made in the endeavor to prove, by an experiment which leaves no room for doubt, the electricity of the blood, and so arrive at the measurement of its electro-motive force. A large vase of porcelain, with a wide opening and capable of holding a litre and a half, was half filled with venous blood; in the midst of this was placed a porous vessel containing four hundred grams of arterial blood; two other small porous vases, of a capacity of sixty cubic centimetres, contained a solution of sulphate of zinc; these two vases were placed at the same time in the two sorts of blood. The zinc electrodes were placed in the solutions and did not touch the blood. As soon as the electrodes, which were previously attached to the galvanometer by brass wires, were inserted in the liquid, the current was established. The experiments were made on the 29th of October in presence of chemists, physicists, and other distinguished men of science. The blood was taken from a very old horse, in good health, which was to be slaughtered in the course of the day. The arterial blood came from the right carotid at the same time that the venous blood was taken from the left jugular vein; the porous vessel containing the arterial blood was then placed in the venous blood, and the whole apparatus surrounded by water at a temperature of 40° C., in order to prevent coagulation. The small porous vessels containing the solution of sulphate of zinc were sunk up to two-thirds of their height in the two sorts of blood. The amalgamated zinc electrodes were inserted lightly and simultaneously; the current manifested itself by the deviation of the needle; it indicated, as in the first experiments of Mr. Scoutetten, that the positive current travelled from the arterial to the venous blood across the galvanometer. After having reached the stop, the needle oscillated and became fixed at 66 degs., where it remained for an hour. The galvanometer employed was Nobili's, with a coil of 10,000 turns. To measure the electromotive force of blood, Mr. Scoutetten has had recourse, in this second series of experiments, to the method of opposition proposed by Poggendorff and so ably carried out by Mr. Jules Regnauld. On placing the coupling wire of the two small porous vases in opposition to a normal wire at a constant current, he saw at first that the current became reversed; and hence he concluded that the force produced by the reaction of the two sorts of blood is com-

prised between zero and 4·50. Proceeding thence to more exact results, he arrived at last at the conclusion that the electro-motive force sought was 1·82; that of Daniel's battery being 58; 100 representing the electro-motive power of pure zinc.—*Reader*, *May* 7, 1864.

8. *Note on Binocular Vision.*—The application of binocular vision to the microscope has been considered justly as the greatest improvement since the introduction of the achromatic objective. Mr. Wenham's single prism seemed to leave little further to be accomplished, the instrument being theoretically as perfect as could be devised, and, practically, giving great satisfaction. Mr. Tolles, of Canistota, N. Y., long favorably known by the great excellence of his objectives for the microscope and telescope, has advanced one step further, and, as it appears to me, in the only right direction to give complete satisfaction. I have lately received from him a *binocular eye-piece*, which is, as far as I can at present judge, every way superior to Mr. Wenham's ingenious arrangement.

In comparing them, I do not wish to be understood as at all depreciating Mr. Wenham's invention, so freely made public, and undoubtedly the best hitherto devised. The only instrument of this form I have had an opportunity of examining was a first class one by Smith, Beck & Beck, and it was certainly an excellent microscope. Yet, with no higher objective than the $\frac{2}{3}$ds in., there was a marked difference in the definition by reflected and direct vision; how far this was accidental I am unable to say, as I had no opportunity to examine the prism. The slanting tube, attached to one side, appeared to me awkward, and the reflected ray had to travel a longer path than the direct one; moreover, the use of a draw-tube for micrometry had to be dispensed with, as no longer length of draw-tube could be allowed than sufficient to adjust for different distances of the eyes of observers. Perhaps this is not a very serious objection; yet it is an objection. The binocular eye-piece meets all these difficulties. The division of the pencil is effected so far from the objective that the interference with definition is a minimum. I can clearly see the fine lines of *Hyalodiscus subtilis* and *Grammatophora subtilissima* with the $\frac{1}{10}$th in. objective, and the fine endings of the wedge-shaped markings on Podura are neatly shown with the $\frac{1}{5}$th and higher powers. The tubes are symmetrical and parallel, and are adjusted for different eyes by a screw and milled head, giving a range much greater than can ever be required; they slide upon a dove-tail hollow bar, and are always parallel with each other and the main tube.

No alteration is required for the adaptation of the eye-piece to any microscope, as it is used in precisely the same manner as the ordinary eye-piece; it lengthens the microscope, however, about four inches, though not necessarily so much.

The eye-piece, with its present arrangement, is a first class erecting one, giving, with the various powers up to the $\frac{1}{10}$ths, full and clear fields beautifully illuminated by the mirror alone, and without any special trouble with the higher powers, as is the case with Mr. Wenham's arrangement. Some care and special apparatus are required to bring out the full effect, and equally illuminate both fields. I have found that the ordinary achromatic condenser, stopping out the central rays and using the plain side of the reflector, or better a reflecting prism, and the bull's-eye condenser, to work very well; and with this arrangement have had fine views of the test objects with the $\frac{1}{5}$th and $\frac{1}{10}$th in. objectives. Some facts as to the structure of certain of the Diatoms, have been brought out with wonderful distinctness; there is a rotundity and solidity which gives a more complete idea of the structure of such forms as *Aulacodiscus Petersii*, and the *Amphiproræ*, than can be had by monocular vision. These effects, however, are not to be had but by careful manipulation, and there is yet needed some means of more ready equable illumination with the higher powers, and Mr. Tolles is now attending to this. An experienced microscopist would, however, succeed in bringing out very satisfactory effects with the means at present employed.

With the lower powers, 3 in. to $\frac{2}{3}$ds in. or $\frac{4}{10}$ths in., nothing can exceed the beauty and brilliancy of objects seen for the first time with binocular vision. The transparent injections (German) sold by Smith, Beck & Beck are exceedingly beautiful. Upon some of these I compared the Wenham binocular and Mr. Tolles's eye-piece. It is with opaque objects that the finest effect is produced, without any special care in illumination. When both eyes are employed there is greater depth of vision, or capability in the eyes—the top and bottom of tolerably thick preparations being seen together, or, at least, much better than when one eye alone is employed. At the same time, the stereoscopic effect is wonderful: the field of view appears suddenly enlarged and comprehensive, and the magnifying power increased, and the relief to the eyes is very great.

The living Diatomaceæ and Desmidiæ are shown with great effect with a good $\frac{4}{10}$ths or $\frac{1}{5}$th objective. They appear floating at different depths in a vast ocean, and with so little loss in definition that the ciliæ of *Closterium Lunula* are readily observed, as also those of the monads and larger animalcules, which appear like monsters in a vast deep.

I am not at liberty at present to explain the principle upon which Mr. Tolles has been enabled to divide the pencil so far from the objective; it is theoretically, as well as practically, correct, and it appears a little strange that so obvious a principle did not at once occur to those eminent European opticians who have devoted so much time and skill to the perfection of binocular arrangements for the microscope. That the present form is the ultimatum, neither Mr. Tolles nor myself anticipates, although it is at present quite perfect and gives me great satisfaction.

The power of the eye-piece is variable in the same manner as with Wenham's binocular. No alteration is required for polarization experiments, but the prism is inserted in precisely the same place and manner as with the single eye-piece. In fact, the old arrangements of the microscope are left untouched, the whole effect of binocular vision being produced in the eye-piece.

The use of the binocular eye-piece at once tests the quality of the objective, as none but perfectly corrected glasses will give satisfaction; when the two images are formed by halves of the objective, and calling into play especially the extreme portions, none but objectives of large angle and first quality will stand the test; and this is the more necessary with the high powers, since, in order to obtain sharp, clear vision, the central pencil should be stopped out, and the object illuminated by oblique light. The condenser Mr. Tolles is now at work upon will, I hope, render the use of the binocular comparatively as easy, with the higher powers, in the hands of a novice, as now it is with the lower powers under the management of an experienced microscopist.

The eye-piece works very well when applied to the telescope, but the stereoscopic effect is not equal to that produced when applied to the microscope, owing to the small angle of the object-glass. I have had very fine views of Saturn and the moon with it; there is some slight loss of light, not perceptible when looking at land objects, but noticeable upon such faint objects as the nearer satellites of Saturn.

The difficulties of illumination with the higher powers of the microscope are only with transparent objects; with opaque objects there is no trouble; and as the condenser of Mr. Tolles' will no doubt remove this difficulty, the binocular eye-piece should become a part of every well equipped microscope. H. L. S.

9. *On the isomorphism of silica with deutoxyd of manganese.*—G. ROSE has directed attention to the constitution of the two minerals Braunite and Marceline, and has shown that in order to explain their isomorphism it is necessary to admit that $SiO_2$ is, in combination at least, isomorphous with $MnO_2$. Thus, we have

$$\text{Braunite,}\ \left.\begin{matrix}\dot{Mn}\\ \dot{Ba}\end{matrix}\right\}\ddot{Mn} \quad = \quad \dot{Mn}\left\{\begin{matrix}\ddot{Mn}\\ \ddot{Si}\end{matrix}\right.\ \text{Marceline.}$$

From this appears that all the five degrees of oxydation of manganese are isomorphous with other and corresponding oxyds. Rose suggests that the deutoxyd of manganese may be called manganous acid.—*Pogg. Ann.*, cxxi, 318. W. G.

10. *On a new cobalt compound.*—BRAUN has observed that when a solution of nitrite of potash is added to one cobalto-cyanid of potassium, a beautiful dark orange-red color is produced. When the cobalt solution is very concentrated, the solution appears intensely blood-red. The reaction succeeds in very dilute solutions of cobalt, but not in the presence of a very large excess of nickel. Braun suggests that a nitro-cyanid of cobalt is formed, and promises a further investigation of the subject.—*Journal für prakt. Chemie*, No. 2, 1864, p. 107. W. G.

11. *Indium.*—At a recent session of the Freiberg "Miners' Union," REICH communicated some further results in regard to this new metal, discovered by T. Richter and himself.[1] Two hundred pounds of black blende ore from the Himmelfahrt Mine, on treatment with chlorhydric acid, and subsequent evaporation and distillation, gave about 43 pounds of impure chlorid of zinc; this, treated with water left a residue containing indium, from which a few grams of the new element were obtained. The *metal* can be reduced from the oxyd by fusion in a carbon (brasqued?) crucible with carbonate of soda and borax, and refusion with cyanid of potassium; by fusing alone with the cyanid the oxyd is reduced, but only to pulverulent metallic powder. The density of two specimens, weighing respectively 327 and 343 milligrams, gave 7·11 and 7·14; a rolled leaflet of 415 milligrams gave a density of 7·277 at 20·4° C. The color of the metal is between tin and silver white; it is exceedingly soft and very ductile, and retains its metallic lustre when exposed to the air

[1] This Journal, [2], xxxvii, 269.

or in water, even when the latter is brought to the boiling point. Hydrogen reduces the oxyd to a metallic powder, which cannot be fused in the bulb-tube. On charcoal, before the blowpipe, it fuses easily, and, while retaining a lustrous metallic surface, colors the flame blue, and covers the coal with a coating, which is dark yellow while hot, and lighter yellow on cooling; the coating is volatilized with difficulty when treated directly with the blowpipe flame. The oxyd imparts no color to the fluxes; flamed with borax it gives a gray enamel, with salt of phosphorus and tin yields a gray pearl. The metal is slowly dissolved by chlorhydric and sulphuric acids in the cold with evolution of hydrogen; heat increases the rapidity of the solution. Dissolves rapidly in nitric acid, even when cold and dilute. The hydrated oxyd is completely precipitated from the acid solutions by ammonia and potash, is of a white color, and of a slimy consistence so that it adheres to the sides of the vessel; the presence of tartaric acid prevents this precipitation. The *oxyd* is white when cold; on heating it is colored dark yellow. Sulphuretted hydrogen gives no precipitate in nitric, sulphuric, and chlorhydric solutions of indium, but from an acetic solution it is completely precipitated as sulphid of indium, of a beautiful yellow color; this, on drying becomes nut-brown, and when pulverized it assumes an orange color. If sulphid of ammonium is added to a tartaric ammoniacal solution, a white precipitate of what may be hydrated sulphid of indium is thrown down; this, treated with acetic acid, as well as when dried and heated, becomes yellow. *Chlorid of indium*, obtained by treatment of a mixture of the oxyd with carbon in a stream of chlorine gas, forms white crystalline scales, is very volatile, attracts moisture with rapidity, and deliquesces. The hydrated chlorid is, for the most part, decomposed by evaporation, leaving a residue of oxyd of indium or basic chlorid. The *sulphate of indium* crystallizes with difficulty in indistinct scales. Carbonate of soda throws down from acid solutions a crystalline granular *carbonate of indium* of a white color. Solutions of the neutral salts of indium give with ferrocyanid of potassium a white precipitate, with ferridcyanid no precipitate.

The most striking property of the new metal, and the one which led to its discovery, is the indigo-blue line which it shows under spectroscopic examination. This is given by the metal, as well as by all the compounds thus far investigated. The chlorid gives the most brilliant effect, the sulphid is, however, the most lasting. The flame of an ordinary Bunsen-burner is colored blue when an indium-compound is brought into it.—*Berg. u. Hüttenmännische Zeitung*, xxiii, 142. G. J. B.

12. *Note on the formation of Aldehyds;* by M. Carey Lea. (Communicated for this Journal.)—M. Carstanjen describes as new a mode of formation of aldehyds by the oxydation of substituted ammonias.[1]

This mode of formation is, however, not new. In the pages of this Journal I have already indicated it. I have shown that when triethylamin is acted upon by chlorid of gold, the gold is reduced and aldehyd given off.

[1] Rep. de Ch. Pure, 1863, p. 616, quoted from Journal für praktische Chemie.

## II. MINERALOGY AND GEOLOGY.

1. *Pollux, a silicate containing a large amount of Cæsium.*—PISANI has made an analysis of this rare mineral species, and finds it to contain 34·07 per cent of cæsia, with but traces of potash. The specimens examined were obtained from the locality in the island of Elba, through Mr. L. Sæmann, of Paris. One crystal of 20 grams weight had distinct cubic faces, with trapezohedral planes like analcime, thus confirming Des Cloizeaux's optical examination, which determined the species to belong to the monometric system. The surfaces of the crystal were rough, resembling carious quartz. Lustre vitreous on the fracture, but dull and gum-like on the natural surfaces of the crystals. Colorless. H. = 6·5, G. = 2·901. In the closed tube, becomes opaque, and gives off water. In the forceps, whitens and fuses with difficulty, coloring the flame yellow. Small particles of the mineral heated with fluorid of ammonium on a platinum wire, and subsequently moistened with chlorhydric acid, give the characteristic blue lines of cæsium with the spectroscope. Slowly decomposes in chlorhydric acid with separation of pulverulent silica. The filtrate from the silica gives an abundant precipitate of the platin-chlorid of cæsium when treated with bichlorid of platinum. Analysis gave:

| | Si | Al | Fe | Ca | Cs | Na* | H |
|---|---|---|---|---|---|---|---|
| | 44·03 | 15·97 | 0·68 | 0·68 | 34·07 | 3·88 | 2·40 = 101·71 |
| Oxygen, | 23·48 | 7·43 | 0·20 | 0·19 | 1·97 | 1·00 | 2·13 |

* With a little lithia.

The platin-chlorid of cæsium obtained in the analysis showed traces of potash when submitted to spectroscopic examination. It was reduced by hydrogen, and subsequently the quantities of chlorine, platinum and cæsium were determined, and found to accord with theory, thus showing it to be a pure cæsium salt.—*Comptes Rendus*, lviii.

[This mineral species was previously analyzed by Plattner, who obtained:

| Si | Al | Fe | K | Na* | H |
|---|---|---|---|---|---|
| 46·20 | 16·39 | 0·86 | 16·51 | 10·47 | 2·32 = 92·75[1] |

* With a little lithia.

Pisani refers to this as an *incomplete* analysis, and remarks that as cæsium was unknown at the time when Plattner's investigation was made, that the platin-chlorid of cæsium obtained in the analyses was considered as platin-chlorid of potassium, and the amount and character of the alkalies are thus erroneously stated in the result. Calculating the amount of chlorid of potassium corresponding to 16·51 potash, we have 26·13 KCl; and converting the 10·47 soda into chlorid, we obtain 19·74 NaCl, or 45·87 alkali chlorids. Now, if we convert the 26·13 KCl into platin-chlorid of potassium, it amounts to 85·65 $KCl + PtCl_2$, and considering this to be a cæsium salt, instead of a potassium salt, it would be equal to 42·65 CsCl. As the soda in the analysis was probably calculated by ascertaining the difference between the supposed chlorid of potassium, and the total weight of alkali-chlorids, this amount is materially lessened when the chlorid of cæsium found (42·65) is subtracted from the total amount of chlorids (45·87), giving but 3·22 NaCl,

[1] Pogg. Ann., lxix, 443.

or 1·72 per cent of soda, while we have 35·69 per cent of cæsia in the mineral. Plattner's analysis would then read:

| | $\ddot{\bar{S}}i$ | $\ddot{\bar{A}}l$ | $\ddot{\bar{F}}e$ | $\dot{C}s$ | $\dot{N}a$ | $\dot{H}$ |
|---|---|---|---|---|---|---|
| | 46 20 | 16·39 | 0·86 | 35·69 | 1·72 | 2·32 = 103·18 |
| Oxygen, | 24·64 | 7·66 | 0 26 | 2·02 | 0·44 | 2·06 |

corresponding in all, except the soda, very closely with the results obtained by Pisani. The excess in both Pisani's and Plattner's analyses would seem to indicate that a portion of the alkalies in the mineral were lithia and potash, although Pisani established the absence of anything more than traces of potash in the cæsium salt obtained in his analysis. Plattner proved by experiment that neither chlorine nor fluorine was contained in the mineral.—G. J. B.]

2. *Composition of Tourmaline, Mica, Hornblende and Staurotide.*—A. Mitscherlich has found that when a gram of finely pulverized tourmaline is heated for six hours with a mixture of 9 cubic centimetres of sulphuric acid, and 6 c. c. of water in a closed tube of Bohemian glass, at a temperature of 220°–240° C., it is completely decomposed. In this manner he has been enabled to determine with accuracy the amount of protoxyd of iron contained in tourmaline and other minerals. Six specimens of tourmaline, the same as analyzed by Rammelsberg, were found to contain no sesquioxyd of iron, although at the time when Rammelsberg's investigations were made, the methods for the determination of the state of the oxydation of iron in silicates were so imperfect, that this chemist found from 4·63 to 9·33 per cent of sesquioxyd of iron in them. It was also demonstrated by Mitscherlich that there can be no sesquioxyd of manganese in tourmaline, otherwise in passing into solution a portion of sesquioxyd of iron would have been formed. He further observes, that the specimens examined were entirely free from carbonic acid, and as the method used admitted of the detection of the minutest trace of this substance, Hermann's supposition that carbonic acid is one of the constituents of tourmaline, is incorrect. Mitscherlich also decomposed several specimens of mica and hornblende by this method, and obtained results, in regard to the relative proportion of the oxyds of iron, differing very materially from those obtained by earlier analysts. Other interesting observations made in the same manner prove that the *staurotides* of St. Gotthardt, Airolo and Brittany contain no sesquioxyd of iron, although Rammelsberg's recent results gave from 2·40 to 5·21 per cent.—*Jour. prakt. Chem.*, lxxxvi, 1. G. J. B.

3. *Contributions to the chemical knowledge of several minerals; by* C. F. Rammelsberg.—I. *Kobellite.*—This species occurs at Hvena, in Sweden, associated with actinolite, chalcopyrite, and small reddish white crystals of a cobaltiferous mispickel (Kobaltarsenikkies). Kobellite resembles antimony-glance in general appearance; G. = 6·145. The analysis was made by decomposing the mineral with chlorine. It was impossible to get the mineral entirely free from the associated arsenical and copper pyrites. Composition:

| | S | Bi | Sb | As | Pb | Fe | Cu | Co |
|---|---|---|---|---|---|---|---|---|
| | 18·22 | 18·60 | 9·46 | 2·56 | 44·25 | 3·81 | 1·27 | 0·68 = 98·85 |
| Sulphur combined, | | 4·30 | 3·79 | 1·64 | 6·85 | 2·18 | 0·32 | 0·36 = 19·44 |

Especial care was taken to ascertain the absolute purity of the bismuth and lead given in the analysis, as considerably more of lead and less of bismuth were found than obtained in the previous analysis by Setterberg. The cobalt was considered as due to the cobaltiferous mispickel, which with the formula $(CoS^2+CoAs)+4(FeS^2+FeAs)$ would correspond to 5·61 per cent. In the same manner, the copper calculated as chalcopyrite equals 3·67 per cent. These amounts subtracted from the analysis, and the remainder averaged up, gives for the composition of pure kobellite:

| | S | B | Sb | Pb | Fe |
|---|---|---|---|---|---|
| | 17·47 | 20 52 | 10·43 | 48·78 | 7·55 = 98·75 |
| Sulphur combined, | | 4·73 | 4·18 | 7·55 | 0·88 = 17·34 |

Rammelsberg writes the formula, $(PbS)^3 BiS^3+(PbS)^3SbS^3$ = S 16·82, Bi 18·23, Sb 10·54, Pb 54·41 = 100.

II. *Siegenite.*—A new analysis of the so-called siegenite (Kobaltnickelkies) from Müsen shows that the earlier analyses of this mineral are erroneous. This is due to the fact that at the time they were made no sufficiently accurate method was known for the separation of cobalt and nickel. The separation of these metals was effected by means of nitrite of cobalt. Analysis of the crystals, selected as pure as possible from associated chalcopyrite, gave: S 42·76, Co 39·35, Ni 14·09, Cu 1·67, Fe 1·06 = 98·93. Considering the iron as combined with 1·21 sulphur and 1·20 copper, forming chalcopyrite, and subtracting these from the analysis, the composition of the pure mineral is as follows:

| S | Co | Ni | Cu |
|---|---|---|---|
| 43·04 | 40·77 | 14·60 | 0·49 |

giving the established formula $RS+R_2S_3$. The amount of cobalt found in former analyses was 22·09 and 11·00 per cent, while the nickel varied from 33·64 to 42·64. This difference is due chiefly to the imperfect method previously used in the separation of the nickel and cobalt, and not to varying composition of the mineral, although another specimen gave on analysis, 36·82 cobalt and 17·72 nickel. A specimen of cobaltine from Tunaberg was found to contain 0·64 per cent of nickel, and another specimen of the same species from Skutterud contained 0·48 per cent, showing that the present method of separating these metals is far more delicate than that formerly employed. [Plattner has previously shown that cobaltine contained traces of nickel.—*Löthrohr-Probirkunst*, p. 318. —G. J. B.]

III. *Vivianite.*—An analysis of the vivianite from Allentown, Monmouth, Co., New Jersey, gave:

| | $\bar{P}$ | $\bar{Fe}$ | $\dot{Fe}$ | $\dot{H}$ |
|---|---|---|---|---|
| G. = 2·68. | 28·81 | 4·26 | 38·26 | 28·67 = 100 |
| Oxygen, | 16·23 | 1·28 | 8·50 | 25·48 |

The mineral occurred in concentric radiated crystals of a light bluish-green color.

IV. *Analyses of Tremolite and Diopside from Gulsjö.*—In confirmation of *Rammelsberg's* view in regard to the composition of those varieties of pyroxene and hornblende which are free from alumina, he now gives further analyses, one of tremolite and another of diopside, from Gulsjö:

| | $\dddot{\mathrm{Si}}$ | $\dot{\mathrm{Mg}}$ | $\dot{\mathrm{Ca}}$ | $\dot{\mathrm{Fe}}$ |
|---|---|---|---|---|
| Tremolite (G. = 3·003), | 57·62 | 26·12 | 14·90 | 0·84 = 99·48 |
| Diopside (G. = 3·249), | 55·11 | 18·39 | 25·63 | 0·54 = 99·67 |

Both analyses give almost precisely the ratio of bases to silica of 1 : 2.

V. *Skolopsite.*—This mineral, described by v. Kobell, occurs at Kaiserstuhl, forming a crystalline granular mixture with a dark green pyroxene. Pure fragments are colorless and transparent, but the larger masses have a gray, greenish or reddish color. Gelatinizes with chlorhydric acid, giving off a trace of sulphuretted hydrogen, and sometimes a little carbonic acid from adhering calcite; this solution contains sulphuric acid. When the decomposition has been effected with nitric acid, chlorine may be detected by nitrate of silver.

The results of the analyses, after deducting the associated augite, were:

| | Cl | $\dddot{\mathrm{S}}$ | $\dddot{\mathrm{Si}}$ | $\overline{\mathrm{Al}}$ | $\overline{\mathrm{Fe}}$ | $\dot{\mathrm{Ca}}$ | $\dot{\mathrm{Mg}}$ | $\dot{\mathrm{Na}}$ | $\dot{\mathrm{K}}$ | $\dot{\mathrm{H}}$ |
|---|---|---|---|---|---|---|---|---|---|---|
| 1, | 1·36 | 4·62 | 35·41 | 21·39 | 2·24 | 15·50 | 2·31 | 12·17 | 2·87 | 3·29 |
| 2, | | 4·17 | 34·17 | 20·61 | 3·16 | 14·70 | 3·04 | 11·74 | 2·73 | |
| Mean, | 1·36 | 4·39 | 34·79 | 21·00 | 2·70 | 15·10 | 2·67 | 11·95 | 2·80 | 3·29 = 100·05 |
| Oxygen, | 0·31 | 2·63 | 18·25 | 9·83 | 0·81 | 4·31 | 1·07 | 3·09 | 0·47 | 2·92 |
| | | | | 10·64 | | 8·94 | | | | |

The chlorine replaces 0·31 oxygen, and the sulphuric acid requires 0·88 oxygen to form a sulphate of a protoxyd base; this amount (0·31 + 0·88), subtracted from the oxygen of the protoxyds, leaves 7·75, which number added to 10·64, the oxygen of the sesquioxyds, equals 18·39, almost exactly the same as the oxygen of the silica: giving the ratio of 1 : 1 between acid and bases, and of 2 : 3 between the protoxyds and sesquioxyds. Rammelsberg writes the formula, $2\dot{R}^2\dddot{Si} + \overline{R}^2\dddot{Si}^3$, which would be a ratio of 2 : 2 : 5, instead of 2 : 3 : 5. He considers it analogous to Sodalite, Nosean, etc., and, inclusive of the chlorid and sulphate, gives the following formula for the mineral, $2RCl + 3(2\dot{R}_2\dddot{Si} + \overline{R}^2\dddot{Si}_3) + 3[2\dot{R}\dddot{S} + 3(2\dot{R}^2\dddot{Si} + \overline{R}^2\dddot{Si}_3)]$.

VI. *Pyroxene.*—Analysis of the dark-green augite associated with skolopsite:

| $\dddot{\mathrm{Si}}$ | $\overline{\mathrm{Al}}$ | $\dot{\mathrm{Ca}}$ | $\dot{\mathrm{Fe}}$ | $\dot{\mathrm{Mg}}$ | $\dot{\mathrm{Mn}}$ |
|---|---|---|---|---|---|
| 48·02 | 2·67 | 25·34 | 13·57 | 9·74 | 1·28 = 100·62 |

—*Jour. prakt. Chem.*, lxxxvi, 340. G. J. B.

4. *On a Volcanic Island in the Caspian;* by Count MARSCHALL of Vienna.—The existence of this island was first made known by Captain Koumani, of the Russian schooner Jourkmen, on the 7th of May, 1861. Its situation is in 39° 34′ 14″ N., and 47° 15′ 20″ E. from Paris. At the time of its discovery it was 400 to 500 paces in circuit, elliptical in outline, and in surface a plain slightly elevated at centre. About the highest point, which was between 18 and 19 feet above the level of the sea, there were small depressions containing muddy waters from which gases were escaping; the temperature of the waters, according to Mr. Abich's observations, was 28°·4 R., that of the air being 20°·3 R. On the 20th of June, 1861, its elevation was only 12 feet; near the close of July but 6 feet; and towards the end of that year it had sunk to 2 feet below the level of the sea. It continued to sink in 1862, and soundings over it, taken in January, 1863, gave depths of 12 to 13 feet.

The region of which the Caspian sea is the centre, and especially the peninsula of Abcheron, presents almost constant volcanic phenomena in its naphtha springs, mineral and gaseous waters, and occasional muddy eruptions, and sometimes also is disturbed by igneous eruptions, and by earthquakes, as recently those of May 30, 1831, and June 11, 1859.—*Les Mondes*, v, 106, *from the Institut Imp. Geol.* 1863.

5. *Notes on the Geology and Mineralogy of the Spanish Province of Santander;* by WM. K. SULLIVAN, Ph.D., M.R.I.A., Prof. Chem. Catholic Univ. of Ireland, &c., and JOSEPH P. O'REILLY, C.E. 196 pp. 8vo. With maps, views and sections. London and Edinburg. Williams & Norgate. First published in the *Atlantis*, vol. iv.—Santander is one of the northern provinces of Spain, lying on the Bay of Biscay. This work treats of the Physical Geography of the Province, and its general geology, and particularly of its metalliferous deposits, mining industry, and ores, and is illustrated by a number of detailed maps. The subject of the ores is treated mineralogically and chemically, and also with reference to their geological age and the order of succession according to which the different kinds were formed. A chapter containing five plates is devoted to the deposit of sulphate of soda in the valley of the Jarama near Aranjuez, Province of Madrid; and another to observations on the mammillated, reniform, globular and botryoidal structure in minerals.

6. *Notes on a Cave and Coal Pit near Peking;* by S. WELLS WILLIAMS.—Among the interesting objects of investigation in the vicinity of Peking, the coal and lime pits, and the caves near them, have attracted several visitors. They all lie within a few miles of each other on the lowest hills of the mountain range which forms the beginning of the ascent to the Table Land of Central Asia, and closes the northern side of the Great Plain of Eastern China, a few miles west of the capital.

The road thither leaves the southwestern gate of Peking on the great stone causeway that was laid by the Emperor Kanghi, and goes on to *Lú-kau-kiau*, about eight miles, where it ends.

From this spot the road to the mines leads through the district city of *Liáng-hiáng-hien*, a town situated in the midst of well cultivated fields, but greatly dilapidated in its walls and gates, showing the poverty of the people and neglect of the officers. Seven miles farther on is the district city of *Fangshan*, much better built and cared for, as its comparatively wide streets, solid double gates, and well stocked markets attest. Between these two towns, and also nearer the base of the hills, are several channels, in which very large boulders and much water-worn shingle prove the great quantity and force of the torrents that occasionally flow from the mountains, though during most parts of the year a brooklet hardly finds its way down the dusty bottom.

The village of *Kuh-shan-kau* lies at the base of the hills in which the caves occur, and, at the time of our visit, near the autumnal equinox, was filled with animals laden with the harvest, which the peasantry were bringing in from the surrounding fields.

In the morning, we started with a guide for the principal cave, called *Yun-shwiu-tung*, or Cloud-water cave, situated about 1,500 feet above the valley. The rock throughout this region is metamorphic limestone, fertile wherever it is covered with soil; the hillsides are hidden with vegetation, so that the winding path led up through a succession of pretty spots and along the brink of cliffs, all in charming variety, and, at this time, adorned by myriads of autumnal flowers in full bloom. The mouth of the cave is protected by a shrine and temple—three or four ancient and dilapidated buildings under the charge of a superannuated priest, whose vacuity, senility and dirt comported with the dusty and faded gods committed to his keeping. He was a painful object, but did his best to entertain us, assisted by a laic who is the guide into the cave.

The throat of the cave just admits a man to crawl on his knees about twenty steps, when he enters a room of irregular shape, some 80 by 30 feet in extent, and rising here and there 50 feet high, with an uneven floor, on which stalagmites had formed a thick crust. The walls were black with smoke, and no stalactites of any size were in this room. A gradual descent led to a still narrower but short gullet opening into other rooms; one was full of stalagmites. This long fissure in the rocks presented a more pleasing variety of grotesque rocks, which have been named the seat of Kwanyin, the Pearl rock, the Eighteen Rahan, the Lotus Stalk, and such like fanciful names, all of which were duly pointed out by the talkative guide. Some pretty pieces of alabaster were clipped from one of them. Another turn carried us off nearly at right angles, and further progress was soon stopped by water—a pool lying across the path. We were told that the end had never been reached in consequence of this obstruction, and legends of adventurous explorers who have perished in the search are told to inquiring travellers. The entire distance through is about half a mile, and the end of the cave is somewhat lower than the entrance. No petrifactions or fossils have yet been found, but it is not improbable that some might be discovered under the floor of the cave, for its conformation and position are very similar to the fossil caves of Gailenreuth in Germany.

The path back to the Tsieh-tâi Gan led over the ridge into the upper part of the valley, where most of the convents are situated, grouped in clusters of houses as space has been found for them on one side or the other of the stream. During this winding walk over the cliffs and along the edge of precipices, scenes of great beauty were continually opening from one point to another, which beguiled the fatigue and rewarded the toil. One hermitage was perched far above on the opposite ridge, attainable by a toilsome ascent of nearly 2,000 feet; and others lay lower down. The hills are known as the *Fáng-shan*, or Square Peaks, but each monastery has its own name, and is devoted to some Budhist saint or legend. The valley is too narrow to be cultivated, but small plats, near the houses, furnish a few vegetables and flowers. Nearly at the bottom is a sheer descent of about 300 feet, over which the water occasionally pours in quantity with no small foam and noise. The temple placed at the top of this cascade is most romantically situated; and if the constant contemplation of the beauties and wonders of creation had, of itself, any power to lead the mind up to the knowledge of their creator, the priests, who have chosen this place for their prayers, should certainly be among the most devout. From one temple to another, all the way down, the priests received us courteously, and were curious to learn where the *Ta-mei-kwoh*, i. e. the United States, was situated; but those living at this spot were more interested than the others. The steps leading to it are chiseled out of the sides of the rock, and chains are hung along to assist the passenger in making the toilsome ascent.

A roughish path leads from the mouth of this valley to the coal mines over a steep hill into a broader one, where the vegetation is less abundant. The lime-kilns are conspicuous from their white debris scattered over the ground for a good distance around; the lime is excellent; it is burned with coal, but the kilns were not in operation at the time of our visit, nor were they ever extensively worked. They lie about two miles northerly from Kuh-shan-kau, and the coal pits are three miles farther, up a steep ravine or woody opening into the main valley, the path to them rough and stony in the extreme. There are about fifteen shafts open, each of their entrances being enlarged into a room where the colliers sleep and eat at times, though more comfortable dwellings have been built for overseers and contractors.

We engaged a miner to show us down the largest shaft, which measured on the average only 4½ feet high by 5 wide; it is cased with willow sticks in a secure manner, and the roof is particularly well guarded. The bottom is lined with the same to form a ladder, up and down which the miners travel in their daily labor. This shaft is about 150 feet deep, and the ladder down to the digging is perhaps 600 feet long. The coal is secured on small wooden sledges, and drawn, as the miner slowly crawls up along the narrow and slippery steps, by a strap passing over his forehead, each load weighing 80 catties. One workman brings up six loads as his day's work. The sides of this shaft showed the width of the veins of coal, but the top and bottom were not dug out; at the bottom the shaft divided and led toward two deposits, but neither passage had been dug out. The whole was very dry, owing probably to its elevation up the hill; but some shafts had been abandoned from wet and bad air, and their mouths closed. The laborers are hired out by contractors, who sell their coal to the dealers coming from Peking and elsewhere; it is all carried away on the backs of camels or mules, and it was a painful sight to see the unwieldy camels coming down the rocky, uneven road, bringing their loads of coal. It is delivered in Peking at about three piculs for a dollar, and a large part of the price is for carriage. The coal is hard, but such examination as the time afforded disclosed not a vestige of a stump or leaf to compare with the fossils of other coal regions; more careful research will doubtless bring to light some indications of this kind, enabling scientific men to compare the numerous deposits of soft and hard coal in this part of China with the European coal-measures.—*China Mail, Nov.* 26, 1863.

7. *On the probable identity of the Oneida Conglomerate of Central New York with the Medina formation.*—E. Jewett, Curator of the State Collections at Albany, N. Y., in a letter to one of the editors, states that he has found the *Fucoides* (*Arthrophycus*) *Harlani*, a characteristic Medina fossil, in the Oneida Conglomerate, near Utica, Oneida Co., N.Y., and concludes on the ground of this discovery, and also, as he observes, for stratigraphical reasons, that the Oneida conglomerate is in fact only a northern portion of the Medina sandstone. The occurrence of this or a related Fucoid is stated by Dana in his Manual of Geology (p. 230), a specimen

having been obtained from the rock near Utica by the author more than thirty years since, which was in all probability of the same species, although, as the specimen was afterwards lost, the fact is given in the Manual with a query as to the species. J. D. D.

8. *Coal in the Alps of Mt. Cenis.*—Mr. DICKINSON exhibited to the Geological Society of London, at its meeting on Feb. 23d, a number of specimens taken from the rocks now being tunneled through the Savoy side of Mont Cenis. They are principally from metamorphic rocks, and as yet no granite has been touched upon. The most interesting mineral of all is the coal, which is found associated with these metamorphic rocks. It has been cut through in different places in the tunnel. Between San Michel and Modan similar coal is being worked to supply the district. It is anthracite coal, very similar to the Welsh anthracite. There is no regular dip in any of these rocks. In one part they are seen standing up like a cone, the coal sometimes vertical, and dipping in a variety of directions.—*Reader, April* 16.

9. *New Fossils from the Lingula-flags of Wales;* by J. W. SALTER, Esq.—Since the author's paper at the last session of the Geological Society on the discovery of *Paradoxides* in Britain, the researches of Mr. Hicks have brought to light so many new members of the hitherto scanty fauna of the Primordial zone that Mr. Salter was now enabled to describe two new genera of *Trilobites* and a new genus of sponge, and to complete the description of *Paradoxides Davidis.* He also remarked that the fauna of the Lingula-flags shows an approximation in some of its genera to Lower Silurian forms, and some—the shells and a Cystidean—are of genera common to both formations; but the Crustaceans, which are the surest indices of the age of Paleozoic rocks, are of entirely distinct genera; and their evidence quite outweighs that of the other fossils. The Primordial zone is, moreover, in Britain separated from the Caradoc and Llandeilo beds by the whole of the Tremadoc group, at least 2000 feet thick.—*Proc. Geol. Soc., May* 23, in *Reader, Ap.* 9.

## III. BOTANY AND ZOOLOGY.

1. *Heath (Calluna vulgaris) in North America.*—The earliest published announcement that we have been able to find of *Calluna vulgaris* as an American plant, is that by Sir Wm. Hooker, in the Index to his *Flora Boreali-Americana* (2, p. 280), issued in 1840. Here it is stated that: "This should have been inserted at p. 39, as an inhabitant of Newfoundland, on the authority of De la Pylaie." Accordingly, in the 7th volume of De Candolle's *Prodromus*, to the European habitat is added, "Etiam in Islandia et in Terra Nova Americæ Borealis." But it does not appear that Mr. Bentham had ever seen an American specimen. He also overlooked the fact (to which Dr. Seemann has recently called attention) that Gisecke, in Brewster's *Encyclopædia*, records it as a native of Greenland. No mention of it is made by Dr. Lang, in his enumeration of the known plants of Greenland, appended to Rink's Geographical and Statistical Account of Greenland, published in 1857,—from which we may infer that the plant is perhaps as rare and local in Greenland as in Newfoundland or even in Massachusetts!

In this Journal, for September, 1861, the present writer announced the unexpected discovery, by Mr. Jackson Dawson, of a patch of Heath in Tewksbury, Massachusetts; adding the remark, that: "It may have been introduced, unlikely as it seems; or we may have to rank this Heath with *Scolopendrium officinarum*, *Subularia aquatica*, and *Marsilea quadrifolia*, as species of the Old World so sparingly represented in the New, that they are known only at single stations,—perhaps late-lingerers rather than new-comers." And when, in a subsequent volume of this Journal, Mr. Rand, after exploring the locality, gave a detailed account of the case, and of the probabilities that the plant might be truly native, we added a note to say that the probability very much depended upon the confirmation of the Newfoundland habitat. As to that, we had been verbally informed, in January, 1839, by the late David Don, that he possessed specimens of *Calluna* collected in Newfoundland by an explorer of that island. Our friend Mr. C. J. Sprague, however, after having in vain endeavored to find in any publication of Pylaie's any mention of this Heath in Newfoundland, and having ascertained that no specimen was extant in Pylaie's herbarium, or elsewhere that he could trace, naturally took a skeptical view, and in the Proceedings of the Boston Natural History Society for February and for May, 1862, he argued plausibly, from negative evidence, against the idea that any native Heath had ever been found in Newfoundland or on the American continent. It is with much interest, therefore, that we read the announcement by Dr. Hewett C. Watson (in the Natural History Review for April last), that—

"Specimens of *Calluna vulgaris* from Newfoundland have very recently come into my hands, under circumstances which seem to warrant its reception henceforth as a true native of that island. At the late sale of the Linnæan Society's Collections in London, in November, 1863, I bought a parcel of specimens, which was endorsed outside, " A collection of dried plants from Newfoundland, collected by — McCormack, Esq., and presented to Mr. David Don." The specimens were old, and greatly damaged by insects. Apparently, they had been left in the rough, as originally received from the collector; being in mingled layers between a scanty supply of paper, and almost all of them unlabelled. Among these specimens were two flowerless branches of the true *Calluna vulgaris*, about six inches long, quite identical with the common heath of our British moors. Fortunately, a label did accompany these two specimens, which runs thus:—"Head of St. Mary's Bay—Trepassey Bay, also very abundant.—S.E. of Newfoundland considerable tracts of it." The name "*Erica vulgaris*" has been added on the label in a different handwriting. All the other species in the parcel (or nearly all) have been recorded from Newfoundland, so that there appeared no cause for doubt respecting the *Calluna* itself. And, moreover, the Collector had seemingly some idea that an especial interest would attach to the *Calluna*, since in this instance he gave its special locality, and also added two other localities on the label. But there is very likely some mistake in the name of the donor to Mr. Don. It is believed by Sir William Hooker that he was the same Mr. W. E. Cormack, whose name is frequently cited for Newfoundland plants in the "Flora Boreali-Americana." This gentleman was a merchant in Newfoundland, to which he made several voyages.

We should recollect that the *Calluna* advances to the extreme western limits (or out-liers) of Europe, in Iceland, Ireland, and the Azores. The step thence to Newfoundland and Massachusetts, though wide, is not an incredible one."

Without doubt these are the very specimens referred to by Mr. Don, then curator of the Linnæan Society. And now that the stations where they were collected are made known, we may expect that the plant will soon be rediscovered, and its indigenous character ascertained.

We notice that an earlier announcement of Dr. Watson's discovery is contained in Dr. Seemann's Journal of Botany for February last, where the record of Gisecke's discovery of *Calluna* in Greenland is referred to. In view of this, and of its common occurrence in Ireland, Iceland, and the Azores, Dr. Seemann opines that "its extension to Newfoundland and the American continent is therefore not so much a paradox as a fact at which we might almost have arrived by induction." It seems to us that the *induction* was all the other way until the plant was actually discovered on American soil. A. G.

2. *Lessons in Elementary Botany: The part on Systematic Botany based upon material left in manuscript by the late Professor Henslow; with numerous illustrations;* by Daniel Oliver, F.R.S., F.L.S., *Keeper of the Herbarium and Library of the Royal Gardens, Kew, and Professor of Botany in University College, London.* London and Cambridge: Macmillan & Co., 1864. pp. 317, 24mo.—As a simple introduction to Botany for beginners, this little volume, which has just reached our hands, appears to be almost unrivalled. It is very simple, but truly scientific, and written with a clearness which shows Professor Oliver to be a master of exposition. The elements of Structural and Physiological Botany are presented in eight chapters, occupying a little more than a third part of the book, not in strict systematic order, but in a series of lessons upon familiar plants, beginning with a Buttercup. Three short and easy lessons are devoted to the Buttercup, upon which all the organs of a flowering plant and their functions are compendiously taught. The fourth lesson introduces "Common flowers to compare with Buttercup," viz: Wallflower, Pea, Bramble, Apple or Pear, Cow Parsnip or Carrot, Daisy, Dead-Nettle, Primrose, Stinging Nettle; bringing out clearly the general morphology of the flower, and the characters of the great exogenous or dicotyledonous class. The fifth deals with Arum, Spotted Orchis, Daffodil, Tulip, and Wheat; bringing out the characters of the monocotyledonous class. The sixth introduces the use of "Flower-schedules," upon a plan devised and successfully employed by the late Professor Henslow; a good mode of directing attention to the more important points in the structure of flowers, and of training young pupils to precise observation. The seventh lesson sketches the development and morphology of the organs, from root to seed, in regular order. The eighth is devoted to the anatomy, or "the minute structure and vital processes of plants." The second part, on "The Classification of Plants," after a few well-chosen remarks on the plan and nature of classification and binomial nomenclature, illustrates by means of common types the natural orders of British plants, their sensible properties and common uses. And finally, an appendix explains how to dry plants, and, by a set of examples,

how to describe them in botanical language. No one would have thought that so much thoroughly correct botany could have been so simply and happily taught in so small a volume. A. G.

3. *Radicle-ism.*—That the stem or ascending axis in Phænogamous and the higher Cryptogamous plants is composed of a series of similar parts, viz: of nodes or leaf-bearing points separated by internodes, each internode developed from the summit or node of its predecessor, is the fundamental doctrine in structural botany. That the embryo (with undeveloped plumule) is simply the initial term of the series; that its so-called *radicle* is not root, but answers to internode, just as the cotyledons borne on its summit answer to leaves, are propositions which we suppose are generally accepted on the continent of Europe, as they certainly are by the botanists of this country. Simple, clear, and indubitable as this view appears to us, our endeavors to impress it upon English botanists, although likely to succeed in the end, make slow progress. Some of these endeavors, or protests, are recorded in this Journal, e. gr., in the nos. for Nov., 1857, p. 435; in Nov., 1858, p. 416; in July, 1861, p. 126; in Sept., 1863, p. 291; and finally in Nov., 1863, p. 435. In the article last referred to, we noted what we took for an admission decisive of the question, viz: that "the radicle is rightly regarded as an axis and not a root." The word 'axis,' as here used in contradistinction to 'root,' we understood to mean ascending axis or stem. We were hasty, it appears; and our mistake arose from our not considering a third possible alternative, i. e., that the radicle might be *neither* root *nor* stem, but a *tertium quid.* This very view is now propounded by Prof. Oliver, in the Natural History Review for April last (p. 314), in an article which, replying as it does to our criticism, may be presumed to express the opinion of Dr. Hooker also. Propounded by such authorities, the view is entitled to the most attentive consideration.

Upon it we remark, first, that, as it introduces a new element into the structural composition of the plant, the burden of proof rests upon the propounders. In what we supposed to be the accepted view, we have the plant built up by the successive repetition of homologous parts, of superposed joints of stem, each bearing a leaf or leaves at its superior extremity, and each capable of sending out a root or roots, actually producing them under favorable circumstances, preferentially at the lower extremity. It being conceded that the radicle of the embryo is not root, but that from the base of which the primary root is produced, while its other end is a leaf-bearing node, the inference is natural that it is the primordial internode, the initial joint of stem. Such inference must be rebutted by evidence,—by showing, for instance, that the radicle has not the structure or the behavior of an internode. Prof. Oliver supports his denial of our proposition, that the radicle answers to an internode of the ascending axis, by two arguments,—one of which is stated *in extenso*, but may be shortly expressed, and as shortly answered. It is, that an internode, being in terms the part between two nodes, and there being a node only at the upper end of the radicle, none at the lower, therefore the radicle cannot be an internode! This is *literally* true. The series of nodes and internodes—not being infinite, nor in a circle like the old Egyptian symbol of a serpent with its tail in its mouth—must needs begin with the one or the other; and in our view it begins with internode, i. e. with axis itself, and not with leaf-bearing apex of axis. But if the *name* here really confuses any one's ideas as to the *thing*, let us substitute for *internode*, Gaudichaud's original technical term of *merithallus* or *merithalle*, and so have done with this verbal argument.

The other argument, to prove that the radicle is a *tertium quid*, is, that in some respects the behavior of the axis below the cotyledonar node is dissimilar to that above, "apart, of course, from the circumstance that the one develops a succession of leaves, the other a root." But the radicle bears the cotyledons, and therefore begins this very succession of leaves,—is to the cotyledons and the plumule just what the next *merithalle* is to its leaf or leaves and the terminal bud; and any of these merithalles, if under ground, will be pretty sure to produce roots;—would produce a root directly from their lower end, no doubt, were that not impossible under the circumstances. What the other dissimilarities are is not suggested. Perhaps Dr. Hooker has alluded to them in his admirable Memoir on *Welwitschia* (p. 17), in his references to the papers of Clos in the *Ann. Sci. Nat.*, on the *collet* and on *rhizotaxie*. Upon which it may suffice to remark, that, whatever minor discrepancies there may be between the number and disposition of the vascular bundles in the axis above and below the cotyledons, they seem capable of easy explanation; and that we should refer to those very figures by Clos, in *Ann. Sci. Nat.*, 3, 18, t. 16 and 17, and to the original subjects which can so readily be examined, as evidence that the radicle, as to internal structure, is stem.

Upon this very ground of internal structure; upon its mode of increase in length (elongating throughout, and often, as in *Cucurbitaceæ*, in Beans, &c., continuing to elongate in its upper portion long after the portion toward the root has ceased to lengthen); indeed, upon its whole behavior (lengthening in some cases, remaining undeveloped in others); upon what it bears; and upon every comparison which we have yet been able to make, we are bound still to maintain that the radicle is not a *tertium quid*, but *is* "to be regarded as part of the stem or ascending axis, in the same sense as the other internodes of the plant may be so regarded." If an opposite view is tenable, we crave an explicit statement of the grounds upon which it is maintained. A. G.

4. *Göthe's Essay on the Metamorphosis of Plants*, translated into English by Emily M. Cox, is published in Dr. Seemann's Journal of Botany, for November and December last. The translation is augmented by some explanatory foot-notes by Dr. Masters, which will aid the general reader to a correct understanding of this celebrated essay. It would be well to translate and reprint Wolff's *Theoria Generationis* also. A. G.

5. *Equisetum.*—In the same useful Journal (for November, 1863,) is a translation of a paper by Dr. Milde, On the Geographical Distribution of the *Equisetaceæ*. In the summary it appears that—

At present only 26 pieces of *Equisetum* can be distinguished with certainty, viz: ten E. PHANEROPORA, *E. arvense* L., *E. Braunii* Milde, *E. Telmateia* Ehr., *E. sylvaticum* L., *E. diffusum* Don, *E. Bogotense* H. & Bonpl., *E. palustre* L., *E. limosum* L., and *E. littorale* Kühler); and sixteen E. CRYPTOPORA, (*E. Martii* Milde, *E. xylochætum* Metten., *E. Brasiliense* Milde, *E. Schaffneri* Milde, *E. giganteum* L., *E. myriochætum* Schlecht. & Cham., *E. debile* Roxb., *E. Mexicanum* Milde, *E. elongatum* Willd., *E. robustum* A. Braun, *E. lævigatum* A. Braun, *E. hiemale* L., *E. Schleicheri* Milde, *E. trachyodon* A. Braun, *E. variegatum* Schleich., and *E. scirpoides* Michx. It appears that Dr. Seemann has an additional species from the Feejee Islands.

*E. elongatum* is the most widely dispersed species, viz: in Europe to lat. 51°, N. Asia, North and South Africa, Mexico, and Chili.

Europe with thirteen species does not possess a single peculiar one, strictly speaking, *E. littorale* being a hybrid, and *E. Schleicheri* and *E. trachyodon* being regarded as only subspecies.

America contains the greatest number of species (21), and those of South America are the most peculiar. *E. xylochætum* of Peru, and *E. Brasiliense* of Brazil, have the stem 10 feet high and an inch in diameter; while *E. Martii*, found in both these countries, is still more gigantic. *E. myriochætum*, one of the five known Mexican species is likewise gigantic. Northern N. America has nearly all the European species, viz: *arvense, pratense* (Labrador), *sylvaticum, palustre, limosum, hiemale, variegatum*, and *scirpoides;* likewise the striking *E. Telmateia* about the northern Great Lakes; also the Californian species confounded with the latter, *E. Braunii* Milde, and A. Braun's *robustum* and *lævigatum*, the former extending into Mexico. A. G.

6. *A. Braun on Marsilia and Pilularia.*—As a precursor to an extended monograph, Prof. Braun has brought out, in the Proceedings of the Berlin Academy, in 1863, a full list and arrangement of the species of *Marsilia* (as he, with evident correctness, writes the name). He recognizes 37 species, seven of them, however, perhaps to be merged in others. As to geographical distribution, 4 are found in Europe, Northern Africa and Asia, 6 in Southern Asia, 12 in Central and Southern Africa and the islands adjacent, 9 in North and South America—one of which is common to both North America and Europe,—5 in Australia, and 4 in the South Sea Islands—only two of which, however, are peculiar to them. The species enumerated from North America, including Mexico, are as follows, under their respective sections.

A. Fruits 8 to 20, placed on recurved peduncles in a single row far up the petiole, from the outer edge of which they spring, globose, without teeth.

*M. polycarpa*, Hook. & Grev. Mexico and southward.

B. Fruits 2 to 6, mostly 2 (exceptionally 1), seated on a little above the base of the petiole, more or less compressed, mostly oblong, with two teeth. (Peduncles erect, confluent for some distance, sometimes more than half way up.)

*M. quadrifolia*, L. Connecticut, at only one known locality, where it was discovered by Dr. T. F. Allen. (Temperate Europe and Asia.)

*M. macropus*, Engelm. (non Hook.) Texas.

C. Fruit solitary at the base of each petiole, more or less compressed, with or without teeth. (Peduncles erect or ascending.)

*M. uncinata*, A. Braun. Arkansas, Texas.

*M. mucronata*, A. Braun. (*M. vestita*, Torr.) Minnesota.
*M. vestita*, Hook. & Grev. Oregon, New Mexico.
*M. tenuifolia*, Engelm. Texas.

Prof. Braun also enumerates four species of *Pilularia*. Under the division with peduncles erect, *P. globulifera* L., the original species, of Europe and Northern Asia. Under the other division, with peduncles bent downward, there is *P. minuta* Durieu, from the Mediterranean region; *P. Novæ Hollandiæ* A. Braun, of Australia; and, which is especially interesting, *P. Americana* A. Braun, from Arkansas!

The above account is abstracted from the notice in Dr. Seemann's Journal of Botany. A. G.

7. "*A National American Herbarium.*—Two years ago, Prof. Asa Gray made the munificent offer to the University of Cambridge, Massachusetts, of his valuable Herbarium and Library, upon condition that a suitable fire-proof building should be erected for their reception, and a fund invested for their adequate maintenance. The subject has been in abeyance until recently, when a banker of Boston liberally offered to defray the cost of the required building, provided others raised a fund to meet the current expenses of the establishment. We rejoice to find that this truly national collection, of the greatest importance to American Botany, is in a fair way of being disposed of in accordance with Dr. Gray's views. We understand, moreover, that this Herbarium is likely to prove a nucleus around which other collections of much importance will probably accumulate.

"We sincerely hope that, through the well-known liberality of American citizens, this Herbarium and Library may be put upon such a footing that Prof. Gray may be so far relieved of its management as to be able to devote himself to the object which we know to be very near his heart —the completion of a Flora of the North American Continent. For this great work Professor Gray has accumulated a very large amount of material; and no botanist is more thoroughly qualified in every way to carry out such an undertaking."—*Nat. Hist. Rev.*, London, April, 1864, p. 313.

8. *Annual Report of the Trustees of the Museum of Comparative Zoology of Cambridge, together with the Report of the Director*, 1863. 56 pp., 8vo. 1864.—The Museum of Comparative Zoology at Cambridge, under the direction of Prof. Agassiz, is making rapid progress in the enlargement of its collections and the arrangement of the specimens. The additions during the year 1863 are as follows:

| | Species. | Specimens. |
|---|---|---|
| Mammals, | 117 | 206 |
| Birds, | 820 | 1676 |
| Reptiles, | 183 | 1984 |
| Fishes, | 630 | 4537 |
| Insects, | Between 2000 and 3000 | Nearly 17000 |
| Crustaceans, | 273 | 3042 |
| Mollusks, | 1443 | 33,594 |
| Echinoderms, | 240 | 1912 |
| Acalephs, | 40 | 175 |
| Polyps (and Corals), | 125 | 1006 |

It is also doing much work in investigation, and in exchanges with her institutions both European and American.

The Museum has a grant from the Legislature of Massachusetts of $10,000 for the publication of an illustrated catalogue, the first part of which is already in the press. An important feature in the Museum is the collection of diagrams and drawings of minute species, or of those that lose their form in alcohol, on which Prof. Agassiz remarks as follows:

"For many years past I have caused diagrams to be drawn to illustrate more fully those specimens in the Museum, the characteristics of which are not easily preserved in the usual mode of exhibiting objects of natural history. Many animals are so very small that, unless they are magnified, their peculiarities are not readily perceived; others contract so much when preserved in alcohol, or lose their natural form and color to such an extent, that they appear like shapeless masses in the jars in which they are put up; still others are so delicate in their structure that they can hardly be preserved at all. It appeared nevertheless desirable that all these objects should be exhibited to the eye of the student as fully as the largest animals which from their very nature may easily be preserved either whole or in parts. The simplest way to attain this end was to have enlarged drawings made of all these objects, either from living specimens or copied from works not readily accessible to the students of natural history, in which satisfactory illustrations may have been published. Many hundreds of these diagrams have already been made by my friend Mr. Bourkhardt, some of which are now on exhibition in the Museum, and in a few weeks every available space in our public rooms will be occupied by those which thus far have remained in portfolios. This will greatly add to the interest of our collections and form a novel feature in the Museum, which I have no doubt will soon be imitated by others."

9. *Observations on the development of Raia Batis;* by JEFFRIES WYMAN, M.D., Hersey Prof. Anat. in Harvard College. 14 pp. 4to, with a plate. (From the Memoirs of the American Academy, vol. ix, pp. 31–44.)—These investigations by Dr. Wyman were made on a series of eggs collected in the spring of 1851 and of the three subsequent years. The more important conclusions arrived at are stated as follows at the close of the paper:

(1.) The yelk case is formed in the glandular portion of the oviduct, and is begun previously to the detachment from the ovary of the yelk which is to occupy it.

(2.) The embryo, before assuming its adult form, is at first eel-shaped, and then shark-shaped.

(3.) The embryo is for a short time connected with the yelk by means of a slender umbilical cord; the cord afterward shortens, and the young skate remains in contact with the yelk until the end of incubation.

(4.) There are seven branchial fissures at first; the foremost of these is converted into the spiracle, which is the homologue of the Eustachian tube and the outer ear canal; the seventh is wholly closed up, and no trace remains; the others remain permanently open.

(5.) There are no temporary branchial fringes or filaments on the first and seventh arches; on the others the fringes are developed from the outer and convex portion of the arch, and are not at first prolongations of the internal gills.

(6.) The nostrils, as in all Vertebrates, consist at first of pits or indentations in the integuments; secondly, a lobe is developed on the inner border of each; and, finally, the two lobes become connected, and thus form the homologue of the fronto-nasal protuberance. The transitional stages of these correspond with the adult conditions of them in other species of Selachians.

(7.) The nasal grooves are compared with the nasal passages of air-breathing animals, and the cartilages on either side of these to the maxillary and intermaxillary bones.

(8.) The foremost part of the head is formed by the extension of the facial disk forward; while this extension is going on, the cerebral lobes change their position from beneath the optic lobes to one in front of them.

(9.) Two anal fins, one quite large and the other very small, are developed, but both are afterward wholly absorbed.

(10.) The dorsals change position from the middle to the end of the tail. At the time of hatching, however, there is still a slender terminal portion of the tail, which is afterward either absorbed or covered up by the enlarged dorsals, as they extend backward.

10. *On the Embryology of Echinoderms;* by ALEXANDER AGASSIZ. 30 pp. 4to, with 4 plates. (From the Memoirs of the American Academy, vol. ix, 1864.)—The author takes up in this paper, in succession, the Echinoids, Ophiurans, and Holothurians, illustrates the development in species under each with detailed descriptions and figures, and then compares the results with what is already known of the development of our common star-fish, in order to trace out the agreement of the mode of formation of the young in these four subdivisions of Echinoderms. Mr. Agassiz concludes that, in each, the young in its earliest stage is in form an open spiral star. He says, respecting Müller's observations, that it is natural that his idea, that we have in the development of Echinoderms a passage from the bilateral to the radiated form, "should have made such a strong impression as to prevent his noticing the radiated character of the young embryo, hidden as it is by an external appearance of bilateral symmetry. And had it not been for the clear idea we now have of the character of the parts of radiated animals, (see L. Agassiz, Contrib. Nat. Hist. U. S., iii, iv,) I doubt not that Müller's view would have gained general acceptance among investigators; and the whole framework of classification, based upon the idea that a plan pervades the different types of the animal kingdom, would have fallen to the ground, if it could have been clearly proven that in Echinoderms we had a transition from one of these plans to another."

11. *On Dimorphism in the Hymenopterous genus Cynips*, with an appendix containing hints for a new classification of Cynipidæ and a list of Cynipidæ, including descriptions of several new species inhabiting the Oak-galls of Illinois; by BENJ. D. WALSH, M.A. 58 pp. 8vo. (From the Proceedings of the Entomological Society of Philadelphia, March, 1864, pp. 443–500.)—The Cynips studied by Mr. Walsh make galls on a species of Oak, the *Quercus tinctoria*. Part of these galls produce males and females of the *Cynips spongifica* in June. Another portion of them, of wholly similar general character, remain green till autumn, and produce in October and November, and also in the following spring, another form

of Cynips—the *Cynips aciculata*, hitherto regarded as a distinct species, all the individuals of which are *females*. Mr. Walsh appears to prove that the latter, although widely different in many characters, is only another form of the *C. spongifica*, and, thence, that this species is dimorphous. The individuals produced in June live but 6 or 8 days; what place in nature, then, the author asks, is filled by the *aciculata?* In reply, he suggests, from the analogy of *Apis*, *Bombus*, etc., that "the female *aciculata* generates galls which produce by parthenogenesis male *spongifica*, and that the females and males of the latter, coupling in June, oviposit in the same month, in the young buds of the oak, eggs that remain dormant till the following spring, some of which then produce *female spongifica* in June and some *female aciculata* in the autumn or early in the following spring, and these last, in their turn, generate *male spongifica* to appear in the following June." He continues, "It may also be the case that some few male *spongifica* are generated by female *spongifica*." The author next sustains this opinion by mentioning some of the analogies that have been observed in other Hymenopterous Insects.

12. *On the mineral secretions of Rhizopods and Sponges;* by G. C. WALLICH. (Ann. and Mag. Nat. Hist., [3], xiii, 72.)—Mr. Wallich sustains the view of Prof. Max Schultze that the siliceous spicules found sometimes in the shells of Rhizopods are foreign to the Rhizopods, and a result of a sponge-growth within. He shows that the spicules of a sponge commence in vaccuoles in its sarcode-mass, and consist of successive layers of silica about a linear vaccuole as its axis, the layers being secreted at unequal rates on the inner and outer surfaces; and that no such mode of secretion is to be found in any Rhizopod. In the formation of the shell of a Rhizopod, the first layer of shell is deposited from the immediate surface of the sarcode-mass, and no further deposit takes place within; every subsequent addition is made from without, and takes place from a special layer of sarcode which is connected with the interior sarcode through the foramina, or in imperforate genera "probably by a reflexion of the sarcode-substance through the main aperture of the last-formed chamber, after the fashion of *Gromia*." This exterior layer of sarcode, which thickens and gives the markings and structure to the shell, he calls the *chitosarc* (from *χιτων a coat*). This same method of secretion he recognizes also in the *Polycystines*, the siliceous shell being an internal framework or skeleton formed by additions derived from an outer layer of sarcode. These siliceous secretions are solid throughout their thickness, the spines never being tubular. The same general fact is essentially true of the *Acanthometrina* and *Thalassicollidæ*. In the *Dictyochidæ*, however, which are intermediate between the last and sponges, the siliceous framework is tubular, and it is formed of two isometrical portions. The shells of *Globigerina* among Rhizopods and of *Haliomma* among Polycystines are stated to have often their chambers choked up with entozootic sponge-growth; whilst the chambers of *Globigerina* are at times filled with effete frustrules of a free-floating pelagic surface Diatom, namely, *Chætoceros*.

[The tendency to the growth of sponge tissue with its siliceous spicules may have some connection with the formation of Glauconite (green material of the green sand) in Rhizopod shells.]

13. *On the Law of the Production of the Sexes in Plants and Animals;* by Prof. THURY, of Geneva.[1]—Mr. Thury's memoir is divided into three parts. In the first, entitled "Deduction of the Law of the Sexes," the author indicates the course of ideas which has led him to his theory. The second, which is shorter, contains, under the title of "Résumé," the complete exposition of the author's notions. The third is a "Notice," prepared by Mr. Cornaz, in which this clever agriculturist describes the experiments which he has made, during two consecutive years, for the verification of the author's theory, and by which this theory appears to be completely confirmed.

The limits of this article do not allow of our following the author through the whole series of reasonings by which he establishes his theory. We shall only state that the study of plants, in which, by the management of the influence of external agents, the observer is enabled to instigate the development of either one or the other sex, seems to prove that the development of the male sex is always related to those general causes which induce a more complete maturation of the juices and a more perfect development of the organs. This fundamental fact the author applies to the animal kingdom. He refers, in the first place, to the fundamental identity of the two sexes—an identity which allows us to explain the characteristic differences of the sexes by simple differences in the mode and amount of development. He then seeks the causes of these differences, by analogy with plants, in the conditions which, at a certain moment (very near the first origin of the organism, since it is anterior to the determination of the sex), produce a more complete development in the case of a male, and a less advanced or less complete development in that of a female.

It remained to fix the precise moment at which this primary determination of the sex takes place. This might be *before* fecundation, or *during*, or *after*, this act. In the former case, if the fecundation were retarded, this retardation, permitting a more complete development of the ovum, must generally induce the production of male individuals. Now, in bees, according to the observations of Huber, if the fecundation take place early, workers (i. e. females) are chiefly produced; whilst, if the fecundation be retarded beyond the twenty-second day, all the eggs deposited are male eggs. According to Mr. Thury, the decisive moment for the production of the sex will therefore precede the act of fecundation.

It is true that in bees the interpretation of the facts is very complex, partly on account of parthenogenesis, partly in consequence of some other peculiar circumstances in the reproduction of these insects. But the author also knew, from some previous experiments, that, in domestic poultry, the eggs last laid nearly always furnish the cocks of the clutch; and he thought it probable that the last eggs which detach themselves from the ovary of the fowl are those which have had the most time for maturation. These eggs are fecundated, as all physiologists are aware, during their passage through the upper part of the oviduct. Therefore here also, when the fecundation is retarded, males are the result.

[1] Translated by W. S. Dallas, F.L.S., from the abstract by Prof. Pictet in the "Bibliothèque Universelle," September 20, 1863, p. 91, for the Ann. Mag. Nat. Hist., from which ([3], xiii, 68), it is here cited.

It was easy to apply the preceding data to the uniparous Mammalia. In these the ovum separates from the ovary at the commencement of the rutting season, and it may be fecundated at any time during the whole period that the female continues in heat, and consequently when its maturation or development is more or less advanced. If the fecundation take place at the commencement of the period of heat, a female is the result; if at the end of this period, a male. This is the conclusion which is fully justified by the experiments of Mr. Cornaz.

It is plain, according to the author, that the life of the unfecundated ovum is divisible into two periods. In the first of these it is in principle a female ovum, in the second a male ovum. The turning moment (*moment de vire*), according to the author, is the time (probably very short) which separates the two periods, and in which the natural course of development induces some sudden change, the nature of which histology should reveal to us. He assumes that the relative duration of the two periods of the life of the ovum may be modified under the influence of the organic state of the female, whence would result a predisposition on the part of some females to give birth either to individuals of their own sex or to males. Temperature, by its direct action on the ovum, and the influence of the fecundating male upon the organic condition of the female, would also produce similar results.

The author, in all his deductions, appears to start from a general point of view, which he certainly indicates, but nowhere demonstrates in a positive manner, regarding it apparently as a sort of axiom. He assumes that "*sexual life, being common to animals and plants, must be subjected to identical fundamental laws in both kingdoms.*" If this be true of the two kingdoms, it must apply with still more reason to the various divisions of the same kingdom. This admits of much generalization; but (and this is the difficulty) we have yet to distinguish with certainty the facts which bear upon essential laws from the infinitely varied manifestations by which these same laws are realized in combination.

The second and third parts of Mr. Thury's memoir are here reproduced entire.

*Second Part—Summary and Practical Observations.*—1. Sex depends on the degree of maturation of the ovum at the moment of its fecundation.

2. The ovum which has not attained a certain degree of maturation, if it be fecundated, produces a female; when this degree of maturation is passed, the ovum, if fecundated, produces a male.

3. When, at the rutting-season, a single ovum separates from the ovary to descend slowly through the genital canal (as in uniparous animals), it is sufficient that the fecundation takes place at the commencement of the rutting-season to produce females, and at the end to produce males—the turning-point of the ovum occurring normally during its passage in the genital canal.

4. When several ova separate successively from the ovary during a single generative period (multiparous and oviparous animals in general), the first ova are generally the least developed, and produce females; the last are more mature, and furnish males. But if it happens that a second generative period succeeds the first one, or if the external or organic conditions change considerably, the last ova may not attain to the superior degree of maturation, and may again furnish females.

*Cæteris paribus*, the application of the principle of sexuality is less easy in the case of multiparous animals.

5. In the application of the above principles to the larger Mammalia, it is necessary that the experimenter should first of all observe the course of the phenomena of heat in the very individual upon which he proposes to act, in order that he may know exactly the duration and the signs of the rutting-season, which frequently vary in different individuals.

6. It is evident that no certain result can be expected when the signs of heat are vague or equivocal. This is scarcely ever the case in animals living in a state of freedom; but cattle in the fattening-sheds or in the stable sometimes present this abnormal peculiarity. Such animals must be excluded from experimentation.

7. From the mode in which the law ruling the production of the sexes has been deduced, it results that this law must be general and apply to all organized beings,—that is to say, to plants, animals, and man.

It is necessary to distinguish carefully the law itself (1 and 2 of this summary), which is absolute, from the applications of it which may be made with more or less facility.

*Third Part—Notice by Mr. George Cornaz.*—I, the undersigned, George Cornaz, administrator of the estate of my father, the late M. A. Cornaz, President of the Agricultural Society of "La Suisse Romande," at Montet, in the Canton de Vaud, certify that I received from Mr. Thury, Professor in the Academy of Geneva, under date of the 18th February, 1861, some confidential instructions the object of which was an experimental verification of the law which governs the production of sex in animals.

I have applied to the management of my herd of cows the data furnished to me by Mr. Thury, and obtained *at once, without any uncertainty, all the expected results.*

In the first place, in *twenty-two* successive cases, I wished to obtain heifers; my cows were of the Schwitz breed, and my bull a pure Durham; the heifers were in demand amongst breeders, and the bulls were only sold to the butchers. I obtained the desired result in *all* cases.

Having subsequently purchased a cow of pure Durham breed, I desired to obtain from them a new bull, which might replace the one which I had bought at great cost, without waiting for the chance of the birth of a male. I operated in accordance with the directions of Prof. Thury, and the success again confirmed the truth of the process which had been communicated to me—a process the application of which is direct and very easy.

Besides my Durham bull, I obtained six other bulls, of a cross-breed between the Durham and Schwitz, which I intended for work: by selecting cows of the same color and size, I obtained very well-matched pairs of bulls.

My herd consists of forty cows of all ages.

To sum up, I have made in all twenty-nine experiments according to the new process, and all have given the desired product, male or female: I have had no case of non-success. All the experiments were made by myself, without the intervention of any other person.

I can consequently declare that I regard the method of Prof. Thury as real and perfectly certain, hoping that he will soon be able to profit all breeders and agriculturists in general by a discovery which will regenerate the business of cattle-breeding. (Signed) G. CORNAZ.

Montet, Feb. 10, 1863.

14. *Catalogue of North American Butterflies;* by J. WM. WEIDEMEYER. 42 pp., 8vo. From the Proceedings of the Entomological Society, Philadelphia. Printed by the Society, 1864.—The species included in this carefully prepared Catalogue are those of the Diurnal Lepidoptera, and the names of all are embraced that are known, thus far, to inhabit the continent of North America from Panama to the Arctic. References to the more accessible works are given and also to some extent the synonymy.

## IV. ASTRONOMY.

1. *Altitudes of Shooting Stars;* compiled by H. A. NEWTON. (Communicated for this Journal.)—The following table contains the computed altitudes, above the earth's surface, of certain shooting stars. It is believed that it includes nearly all those which have been published. Many of them are unreliable, and for all we may reasonably assume a large probable error. Yet taken together they have value in investigations in terrestrial physics, and they furnish a basis for important deductions respecting the shooting stars themselves. The observations in August and November last will furnish considerable additions to the table.

In the second and third columns are the dates of observations in local time. The apparent brightness of each shooting star is expressed by the symbol or figure in the fourth column. This indicates the planet, or the magnitude of the fixed star, that is equal in brilliancy to the shooting star. Two numbers in the same line indicate that the observers differed in their estimate. In the fifth column is the computed altitude above the earth's surface of the shooting star at its first appearance. The unit is the geographic mile, the sixtieth part of a degree. In the sixth column is the altitude of the shooting star at its disappearance.

In the next two columns are placed such altitudes as the observers, or computers, considered uncertain, and some others which for certain reasons seem especially unreliable. To the former class belong Nos. 2, 9, 16, 20, 35, 39, 40, 48, 56, 65, 73, 83, 95, 96, 102, 103, 106, 107, 110, 146, 176, 206, 219, 223, 228, and 292. There was a small parallax, or a large probable error, for Nos. 3, 50, 51, 60, 79, 88, 90, 91, 121, 123, 124, 154, 160, 196, 200, 217, and 218. The altitudes of Nos. 125, 128, 131, 135, 136, and 167, are very improbable from their magnitude. Other large altitudes, as well as many of the smaller ones, might perhaps with good reason be also transferred from the fifth and sixth, to the seventh and eighth columns.

The following are the books referred to in the last column.

Brandes, *Versuche die Entfernung, die Geschwindigkeit*, etc., 8vo, Hamburg, 1800.
Benzenberg, *Die Sternschnuppen sind Steine*, etc., 8vo, Bonn, 1834.
Brandes, *Unterhaltungen für Freunde der Phys. und Astr.*, 8vo, Leipzig, 1825.
*Astronomischen Nachrichten*, referred to by the letters *A. N.*
Coulvier Gravier, *Recherches sur les étoiles filantes*, 8vo, Paris, 1847.
Heis, *Die periodischen Sternschnuppen* etc., 4to, Cöln, 1849.
Schmidt, *Resultate aus Zehnjahrigen Beobach.* über *Sternsch*, etc., 8vo, Berlin, 1852.
Jahn, *Unterhaltungen im Gebiete der Astr., Geogr. und Meteor.*, 8vo, Leipzig.

*Table of altitudes of Shooting Stars.*

| No. | Date of Obs. | Hour. | Mag. | 1st alt. | 2d alt. | 1st alt. | 2d alt. | Authority. |
|---|---|---|---|---|---|---|---|---|
| 1. | 1798, Sep. 11, | 8.1 | 3 | | 14 | | | Brandes, *Versuche.*, &c. |
| 2. | " | 9.5 | 1 | | | | 20 | " |
| 3. | 13, | 9.4 | 1 | | | | 132 | " |
| 4. | Oct. 6, | 8.5 | Small | | 6 | | | " |
| 5. | " | 10.1 | 4 | | 18 | | | " |
| 6. | 9, | 8.5 | 2 | | 45 | | | " |
| 7. | " | 9.3 | 1 | | 35 | | | " |
| 8. | " | 9.3 | 2 | | 52 | | | " |
| 9. | " | 9.9 | 3 | | | | 88 | " |
| 10. | " | 10.1 | 1–2 | | 66 | | | " |
| 11. | " | 10.8 | 2 | | 52 | 21 | | " |
| 12. | " | 11.2 | 3 | | 67 | | | " |
| 13. | 14, | 12.3 | 5 | | 28 | | | " |
| 14. | " | 13.6 | 3 | | 43 | | | " |
| 15. | " | 13.8 | 4 | | 38 | | | " |
| 16. | Nov. 4, | 8.9 | 3 | | 43 | 20 | | " |
| 17. | " | 9.0 | 2 | | 82 | | | " |
| 18. | " | 9.3 | 1 | | 92 | | | " |
| 19. | " | 10.1 | 1–2 | 64 | 41 | | | " |
| 20. | " | 13.8 | ♃–1 | | | | 44 | " |
| 21. | " | 14.5 | 1 | 68 | 46 | | | " |
| 22. | 1801, Sep. 15, | | 5 | 31 | 33 | | | Benzenberg, *Ster.* &c. p. 16. |
| 23. | Oct. 3, | | 4 | | 28 | | | " |
| 24. | 1802, Aug. 10, | | 5 | | 15 | | | " |
| 25. | 1823, May 2, | 9.8 | 2 | 76 | 15 | | | Brandes, *Unterhalt.* &c. |
| 26. | " | 10.7 | ♀ | 59 | 50 | | | " |
| 27. | 7, | 10.1 | 4 | 6 | 6 | | | " |
| 28. | " | 11.7 | 4–5 | 4 | | | | " |
| 29. | 10, | 9.6 | 1 | 49 | 33 | | | " |
| 30. | Aug. 4, | 10.8 | Small | 47 | 31 | | | " |
| 31. | 10, | 9.7 | Small | 31 | | | | " |
| 32. | " | 9.8 | Small | 36 | | | | " |
| 33. | " | 9.9 | Small | 25 | | | | " |
| 34. | 11, | 9.7 | 3 | 119 | 135 | | | " |
| 35. | " | 10.2 | | | | 30 | 16 | " |
| 36. | " | 10.5 | 1 | 67 | 32 | | | " |
| 37. | " | 10.6 | 1 | 52 | 40 | | | " |
| 38. | " | 10.8 | | 41 | 27 | | | " |
| 39. | " | 11.0 | 4 | | | | 28 | " |
| 40. | " | 11.0 | 5 | | | | 48 | " |
| 41. | " | 11 0 | 2 | 108 | 57 | | | " |
| 42. | " | 11.1 | 5 | 38 | 34 | | | " |
| 43. | 29, | 9.9 | 2 | | 68 | | | " |
| 44. | 30, | 9.4 | Small | 43 | 55 | | | " |
| 45. | " | 10.6 | 3–4 | 108 | 73 | | | " |
| 46. | Sep. 1, | 9.2 | 5 | 49 | 51 | | | " |
| 47. | " | 9.6 | 3 | 57 | 52 | | | " |
| 48. | " | 9.7 | 1 | | | | 64 | " |
| 49. | 2, | 9.3 | 3–4 | | 12 | | | " |
| 50. | " | 9.4 | 3 | | | 73 | 70 | " |
| 51. | " | 9.7 | 1 | | | | 22 | " |
| 52. | " | 9.9 | 2–3 | | 23 | | | " |
| 53. | " | 9.9 | | | 34 | | | " |
| 54. | " | 9.7 | 3 | 122 | 78 | | | " |
| 55. | " | 10.2 | 3 | 19 | | | | " |
| 56. | 11, | 9.5 | | | 21 | 16 | | " |
| 57. | " | 10.5 | | 83 | 45 | | | " |
| 58. | 12, | 10.0 | 1–3 | 63 | 65 | | | " |
| 59. | 27, | 7.6 | 1–3 | 58 | 44 | | | " |
| 60. | " | 7.8 | 4 | | | 104 | 128 | " |

| No. | Date of Obs. | Hour. | Mag. | 1st alt. | 2d alt. | 1st alt. | 2d alt. | Authority. |
|---|---|---|---|---|---|---|---|---|
| 61. | 1823, Sep. 27, | 7.8 | 4–5 | 59 | | | | Brandes, *Unterhalt.*, &c. |
| 62. | " | 8.0 | 3–4 | 55 | 45 | | | " |
| 63. | " | 8.3 | 2–5 | | 40 | | | " |
| 64. | " | 8.6 | 3–4 | 56 | 47 | | | " |
| 65. | " | 9.5 | 2–4 | | | | 16 | " |
| 66. | Oct. 7, | 8.2 | 3 | | 45 | | | " |
| 67. | " | 8.4 | Large | 79 | 57 | | | " |
| 68. | " | 8.7 | 2–3 | 48 | 33 | | | " |
| 69. | " | 8.8 | 4–5 | 35 | 25 | | | " |
| 70. | " | 8.9 | 1–4 | 23 | 28 | | | " |
| 71. | " | 8.9 | 1–4 | | 34 | | | " |
| 72. | " | 9.0 | 1–2 | 59 | 62 | | | " |
| 73. | " | 9.2 | 2 | | 81 | 260 | | " |
| 74. | 8, | 7.6 | Large | 72 | 46 | | | " |
| 75. | " | 8.3 | 4–5 | | 55 | | | " |
| 76. | " | 8.3 | | | 55 | | | " |
| 77. | " | 8.5 | 4 | | 22 | | | " |
| 78. | " | 8.6 | 3 | 44 | 54 | | | " |
| 79. | " | 8.6 | 3 | | | | 540 | " |
| 80. | " | 8.8 | 1–2 | 183 | 99 | | | " |
| 81. | " | 8.8 | 3 | 8 | 10 | | | " |
| 82. | " | 9.0 | 2–4 | 53 | 54 | | | " |
| 83. | " | 9.2 | Small | | | | 16 | " |
| 84. | " | 9.3 | 2–3 | 52 | | | | " |
| 85. | " | 9.3 | 1–3 | 52 | 38 | | | " |
| 86. | 9, | 8.4 | 2–3 | 66 | 44 | | | " |
| 87. | " | 8.6 | 5 | | 53 | | | " |
| 88. | 1825, Aug. 30, | 9.0 | 1 | 83 | | | 261 | Herther, *A. N.* xvii, 316. |
| 89. | Sep. 1, | 8.6 | 1 | 57 | 29 | | | " |
| 90. | 9, | 10.0 | 1 | | 16 | 65 | | " |
| 91. | 17, | 9.8 | 1 | 71 | | | 174 | " |
| 92. | Oct. 5, | 10.3 | 2 | 47 | 29 | | | " |
| 93. | 1833, Aug. 6, | 10.3 | | 29 | 33 | | | Brandes (son), *A, N.* xvii, 17. |
| 94. | 7, | 9.7 | | | 67 | | | " |
| 95. | " | 9.8 | | | 44 | 14 | | " |
| 96. | " | 10.4 | | | | | 10 | " |
| 97. | 9, | 9.9 | | | | 24 | 19 | " |
| 98. | " | 9.9 | | | 65 | | | " |
| 99. | " | 10.0 | | | 37 | | | " |
| 100. | " | 10.0 | | 49 | 35 | | | " |
| 101. | " | 10.8 | | | 104 | | | " |
| 102. | " | 10.8 | | | | | 26 | " |
| 103. | 10, | 9.3 | | | | | 32 | " |
| 104. | " | 9.4 | | | 14 | | | " |
| 105. | " | 9.5 | | | 86 | | | " |
| 106. | " | 9.7 | | | | 74 | 60 | " |
| 107. | " | 10.1 | | | | | 8 | " |
| 108. | " | 10.1 | | | 20 | | | " |
| 109. | " | 10.2 | | | 8 | | | " |
| 110. | " | 10.5 | | | | | 68 | " |
| 111. | " | 10.7 | | | 4 | | | " [343. |
| 112. | Nov. 12, | 17.8 | Large | 62 | 25 | | | Twining, *Amer. Jour.*, xxvi, |
| 113. | 1834, Nov. 27, | 10.5 | Small | 50 | 36 | | | Loomis and Twining, *MS.* |
| 114. | " | 10.8 | | 28 | 22 | | | " |
| 115. | Dec. 10, | 16.7 | 3 | 67 | 44 | | | " |
| 116. | " | 17.8 | 1 | 62 | 25 | | | " |
| 117. | 1836, Nov. 11, | | | 18 | 12 | | | Boguslauski, *C. G.*, p. 91. |
| 118. | " | | | 61 | 36 | | | " |
| 119. | 13, | | | 41 | 12 | | | " |
| 120. | 14, | | | 53 | 66 | | | " |
| 121. | " | 12.2 | 3 | | | 90 | 69 | Erman, *A. N.*, xvii, 317. |

| No. | Date of Obs. | Hour. | Mag. | 1st alt. | 2d alt. | 1st alt. | 2d alt. | Authority. |
|---|---|---|---|---|---|---|---|---|
| 122. | 1836, Nov. 14, | 12.3 | 3 | | | 197 | 184 | Erman, *A. N.*, xvii, 317. |
| 123. | " | 13.3 | 1–2 | | | 21 | 21 | " |
| 124. | " | 14.1 | 3 | | | 92 | 65 | " |
| 125. | 1837, July 10, | | 3 | | | 247 | 171 | Boguslauski, *C. G.*, p. 101. |
| 126. | " | | 1 | 190 | 110 | | | " |
| 127. | " | | 1 | 31 | 15 | | | " |
| 128. | Aug. 10, | 12.2 | 3 | | | 207 | 182 | Petersen, *A. N.*, xvii, 318. |
| 129. | " | 12.2 | 1 | 164 | 138 | | | " |
| 130. | " | 12.4 | 2 | 88 | 89 | | | " |
| 131. | " | 12.6 | 4 | | | 1041 | 420 | " |
| 132. | " | 12.7 | | 123 | 75 | | | " |
| 133. | " | 13.0 | 3 | 117 | 78 | | | " |
| 134. | " | 13.3 | 1 | 184 | 100 | | | " |
| 135. | " | 13.4 | 1 | | | 565 | 414 | " |
| 136. | " | 13.8 | 1–2 | | | 200 | 323 | " |
| 137. | " | 14.0 | 1 | 148 | 81 | | | " |
| 138. | " | 14.1 | 2 | 65 | 45 | | | " |
| 139. | " | 14.1 | 1 | 178 | 148 | | | " |
| 140. | " | 14.3 | 1 | 151 | 166 | | | " |
| 141. | " | 15.1 | 1 | 86 | 67 | | | " |
| 142. | " | 13.4 | 3 | 115 | 115 | | | " |
| 143. | 1838, Aug, 29, | | | | 38 | | | Littrow, *C. G.*, p. 113. |
| 144. | " | | | 26 | 9 | | | " |
| 145. | " | | | 86 | 52 | | | " |
| 146. | " | | | | 60 | 55 | | " |
| 147. | " | | | 55 | 28 | | | " |
| 148. | " | | | 52 | 39 | | | " |
| 149. | " | | | 26 | 29 | | | " |
| 150. | " | | | 39 | 14 | | | " |
| 151. | " | | | 52 | 24 | | | " |
| 152. | " | | | 16 | 11 | | | " |
| 153. | " | | | 27 | 12 | | | " |
| 154 | 1839, Aug. 10, | 10.0 | | | | 68 | 52 | Erman and Petersen, *A. N.*, |
| 155. | " | 10.0 | 1–2 | | | 96 | 192 | [xix, 28. |
| 156. | " | 10.1 | ♀ | 72 | 44 | | | " " |
| 157. | " | 10 2 | 2 | 56 | 52 | | | " " |
| 158. | " | 10.3 | 1 | 48 | 32 | | | " " |
| 159. | " | 10 3 | 1 | 120 | 60 | | | " " |
| 160. | " | 10 5 | 1 | | | 184 | 176 | " " |
| 161. | " | 10.5 | 4 | 68 | 84 | | | " " |
| 162. | " | 10.6 | 4 | 128 | 112 | | | " " |
| 163. | " | 10.6 | 1 | 48 | 48 | | | " " |
| 164. | " | 10.7 | 1 | 68 | 52 | | | " " |
| 165. | " | 10.7 | 1 | | | 176 | 160 | " " |
| 166 | " | 11.0 | 3 | 40 | 36 | | | " " |
| 167. | " | 11.2 | 1 | | | 292 | 264 | " " |
| 168. | 1842, Aug. 9, | 9.7 | 2 | 68 | 52 | | | Heis, *Per. Sternsch.*, p. 35. |
| 169. | " | 10.4 | 1 | 44 | 28 | | | " |
| 170. | 11, | 10.2 | 2 | 76 | 44 | | | " |
| 171. | " | 11.4 | 1 | 80 | 64 | | | " |
| 172. | 1845, Aug. 24, | | 2 | 19 | 13 | | | Coul. Grav., p. 179. |
| 173. | 25, | | 4–5 | 54 | 54 | | | " |
| 174. | 31, | | 4–5 | 43 | 39 | | | " |
| 175. | " | | 3 | 26 | 28 | | | " |
| 176. | " | | 4 | | | 28 | 12 | " |
| 177. | Sep. 1, | | 4 | 15 | 15 | | | " |
| 178. | " | | 4 | 15 | 15 | | | " |
| 179. | 2, | | 6 | 13 | 15 | | | " |
| 180. | " | | 5 | 32 | 32 | | | " |
| 181. | " | | 5–6 | 22 | 21 | | | " |
| 182. | 3, | | 2–3 | 35 | 35 | | | " |

| No. | Date of Obs. | Hour. | Mag. | 1st alt. | 2d alt. | 1st alt. | 2d alt. | Authority. |
|---|---|---|---|---|---|---|---|---|
| 183. | 1847, Aug. 10, | 9.5 | | | 64 | | | Weyer, *A. N.*, xxvi, 212. |
| 184. | " | 10.9 | | | 48 | | | " |
| 185. | " | 11.0 | | | 72 | | | " |
| 186. | " | 11.7 | | | 40 | | | " |
| 187. | " | 11.7 | | | 40 | | | " |
| 188. | " | 11.7 | | | 36 | | | " |
| 189. | " | 11.7 | | | 40 | | | " |
| 190. | " | 11.7 | | | 40 | | | " |
| 191. | " | 11.9 | | | 32 | | | " |
| 192. | " | 11.9 | | | 60 | | | " |
| 193. | " | 10.6 | | | 56 | | | " |
| 194. | " | 11.3 | | | 28 | | | " |
| 195. | " | 11.4 | | | 44 | | | " |
| 196. | 1848, July 29, | 10.0 | Large | | 28 | 104 | | Schmidt, *A. N.*, xxvii, 370. |
| 197. | Aug. 10, | 9.7 | 1 | 38 | 26 | | | Heis, *Per. Sternsch*, p. 35. |
| 198. | 1849, July 28, | | | 48 | 38 | | | Schmidt, *Resultate*, p. 142. |
| 199. | 29, | 11.2 | 1 | 37 | 40 | | | " p. 123. |
| 200. | " | 11.4 | 1 | 69 | | | 98 | " " |
| 201. | Aug. 10, | | Large | 80 | 18 | | | " p. 142. |
| 202. | 11, | 9.7 | 2 | 178 | 66 | | | " p. 124 ff. |
| 203. | " | 10.2 | 1 | 84 | 70 | | | " " |
| 204. | " | 10.9 | 3 | 41 | 19 | | | " " |
| 205. | " | 11.1 | 2 | 96 | 74 | | | " " |
| 206. | " | 11.6 | 4 | 44 | 39 | | | " " |
| 207. | " | 11.9 | 4 | | | 496 | 272 | " " |
| 208. | " | 11.4 | 2 | 72 | 76 | | | " " |
| 209. | " | 12.1 | 3 | 49 | 38 | | | " " |
| 210. | " | 10.1 | | 61 | 54 | | | " " |
| 211. | " | 9.8 | | 31 | 41 | | | " " |
| 212. | " | 9.7 | | 35 | 68 | | | " " |
| 213. | 20, | 9.4 | 3 | 56 | 40 | | | " " |
| 214. | Sep. 27, | 8.6 | 2 | 20 | 16 | | | " " |
| 215. | Oct. 22, | 10.2 | 3 | 124 | 69 | | | " " |
| 216. | Nov. 11, | 7.2 | Large | 52 | 45 | | | " " |
| 217. | " | 7.7 | 2 | 35 | | | 115 | " " |
| 218. | " | 7.5 | 4 | | 12 | 78 | | " " |
| 219. | 12, | 6.9 | 2 | | | 215 | 31 | " " |
| 220. | " | 7.4 | | 13 | 4 | | | " " |
| 221. | " | 7.4 | 3 | 36 | 54 | | | " " |
| 222. | " | 9.1 | 5 | 40 | 24 | | | " " |
| 223. | " | 9.4 | 3 | | | 2 | 7 | " " |
| 224. | " | 11.7 | 2 | 19 | 14 | | | " " |
| 225. | " | 12.0 | 4 | 6 | 5 | | | " " |
| 226. | " | 12.1 | 3 | 4 | 4 | | | " " |
| 227. | 13, | 9.4 | | 52 | 46 | | | " " |
| 228. | 19, | 7.5 | Large | | | 156 | 116 | " " |
| 229. | 1850, Aug. 10, | 10.1 | | 47 | 60 | | | " " |
| 230. | " | 11.8 | Large | 80 | 56 | | | " p. 142. |
| 231. | 1853, Aug. 9, | 10.1 | 3–4 | | 20 | | | Heis, *Unterhalt.*, viii, 15. |
| 232. | " | 10.6 | 1–2 | 98 | 90 | | | " |
| 233. | " | 11.4 | 2 | 92 | 85 | | | " |
| 234. | 10, | 10.4 | 1 | 32 | 27 | | | " |
| 235. | " | 10.7 | 2 | 41 | 34 | | | " |
| 236. | " | 10.8 | 1–2 | 52 | 53 | | | " |
| 237. | " | 11.1 | 1 | 43 | 30 | | | " |
| 238. | " | 11.1 | 1 | 24 | 23 | | | " |
| 239. | 11, | 10.9 | 1 | 19 | 15 | | | " |
| 240. | 1856, Aug. 9, | | | 19 | 5 | | | Le Verrier, *Unterhalt.*, xi, 254. |
| 241. | " | | | 19 | 14 | | | " |
| 242. | " | | | 17 | 11 | | | " |
| 243. | " | | | 20 | 3 | | | " |

| No. | Date of Obs. | Hour. | Mag. | 1st alt. | 2d alt. | 1st alt. | 2d alt. | Authority. |
|---|---|---|---|---|---|---|---|---|
| 244. | 1856, Aug. 9, | | | 45 | 39 | | | LeVerrier, *Unterhalt.*, xi, 254. |
| 245. | " | | | 64 | 36 | | | " |
| 246. | 1858, Aug. 8, | 9.4 | 1 | 46 | 29 | | | Heis, *A. N.*, 1, 148. |
| 247. | " | 10.5 | 1–4 | 128 | 62 | | | " |
| 248. | " | 9.6 | ♀–2 | 67 | 29 | | | " |
| 249. | " | 11.0 | 1–2 | 186 | 108 | | | " |
| 250. | 10, | 9.0 | ♀–1 | 216 | 40 | | | " |
| 251. | " | 9.8 | 1–2 | 22 | 19 | | | " |
| 252. | " | 10.0 | ♀–1 | | 42 | | | " |
| 253. | " | 10.0 | 2–3 | 48 | 42 | | | " |
| 254. | " | 10.1 | 2–3 | 70 | 64 | | | " |
| 255. | " | 10.4 | 1 | 108 | 78 | | | " |
| 256. | " | 10.3 | 1 | 66 | 44 | | | " |
| 257. | " | 10.3 | 1 | 60 | 48 | | | " |
| 258. | " | 10 5 | 1–2 | 46 | 32 | | | " |
| 259. | " | 10.5 | 1–2 | 62 | 42 | | | " |
| 260. | " | 10.8 | 1 | 84 | 52 | | | " |
| 261. | " | 10.7 | 1 | 78 | 48 | | | " |
| 262. | " | 11.1 | ♀–2 | 88 | 48 | | | " |
| 263. | " | 11.4 | 1–2 | 62 | 58 | | | " |
| 264. | " | 11.6 | 2 | 36 | 34 | | | " |
| 265. | " | 11.7 | 1 | 58 | 58 | | | " |
| 266. | 11, | 10.2 | 1 | | 32 | | | " |
| 267. | " | 10.8 | 1–2 | 52 | 52 | | | " |
| 268. | " | 10.8 | 1 | 60 | 48 | | | " |
| 269. | " | 11.5 | 1 | 72 | 60 | | | " |
| 270. | " | 9.6 | 2 | 32 | 20 | | | " |
| 271. | " | 9.8 | 2 | | 32 | | | " |
| 272. | 12, | 10.8 | 1–2 | 48 | 35 | | | " |
| 273. | " | 10.6 | 1–2 | 45 | 27 | | | " |
| 274. | " | 10.9 | 1–2 | 82 | 72 | | | " |
| 275. | " | 11.6 | ♀–1 | 108 | | | | " |
| 276. | " | 10.3 | 2–3 | 96 | 60 | | | " |
| 277. | " | 11.8 | 3 | | 44 | | | " |
| 278. | 1861, July 16, | 10.2 | Large | 148 | 38 | | | *Brit. Assoc. Rep.*, 1862, 76 ff. |
| 279. | " | 11.5 | Large | 168 | 56 | | | " |
| 280. | Aug. 6, | 11.4 | ♀ | 109 | 18 | | | " |
| 281. | 8, | 10.5 | 2 | | 58 | | | " |
| 282. | " | 10.6 | 1 | | 43 | | | " |
| 283. | 10, | 10.5 | 1 | | 40 | | | " |
| 284. | " | 10.8 | 3 | | 60 | | | " |
| 285. | " | 11.5 | Large | 60 | 46 | | | *Amer. Jour.*, [2], xxxii, 449. |
| 286. | 11, | 10.3 | ♀ | 38 | 18 | | | Herschel, and *Brit. As. Rep.* |
| 287. | Nov. 12, | 5.8 | Large | 83 | 17 | | | *Brit. Assoc. Rep.*, 1862, 78 ff. |
| 288. | 19, | 9.6 | Large | 47 | 26 | | | " |
| 289. | 1862, Jan. 28, | 11.1 | 1–3 | 39 | 41 | | | " |
| 290. | Feb. 2, | 8.3 | Large | 165 | 13 | | | " |
| 291. | 23, | 9.4 | Large | 35 | 17 | | | " |
| 292. | Nov. 13, | 16.5 | Large | | 27 | 41 | | *Amer. Jour.*, [2], xxxv, 147. |
| 293. | 1863, Aug. 9, | 9.9 | 1 | 74 | 50 | | | Herschel, *Proc. Br. Met. Soc.*, [ii, 19. |
| 294. | " | 10.3 | 1 | 65 | 38 | | | " " |
| 295. | 10, | 9.4 | ♃ | 62 | 46 | | | " " |
| 296. | " | 9.6 | ♃ | 98 | 63 | | | " " |
| 297. | " | 9.8 | ♀ | 48 | 22 | | | " " |
| 298. | " | 9.9 | 2 | 75 | 73 | | | " " |
| 299. | " | 9.9 | 2 | 61 | 45 | | | " " |
| 300. | " | 10.1 | 1 | 48 | 36 | | | " " |
| 301. | " | 10.1 | 1 | 113 | 57 | | | " " |
| 302. | " | 10.1 | 1 | 54 | 46 | | | " " |
| 303. | " | 10.2 | 1 | 94 | 54 | | | " " |
| 304. | " | 10.2 | 1 | 105 | 48 | | | " " |
| 305. | " | 10.3 | 1 | 68 | 50 | | | " " |

| No. | Date of Obs. | Hour. | Mag. | 1st alt. | 2d alt. | 1st alt. | 2d alt. | Authority. |
|---|---|---|---|---|---|---|---|---|
| 306. | 1863, Aug. 10, | 10.6 | 2 | 55 | 46 | | | Herschel, *Proc. Br. Met. Soc.*, |
| 307. | " | 10.7 | 2 | 62 | 52 | | | " " [ii, 19. |
| 308. | " | 10.7 | 2 | 73 | 72 | | | " " |
| 309. | " | 10.8 | ♀ | 74 | 59 | | | " " |
| 310. | " | 10.9 | 1 | 66 | 61 | | | " " |
| 311. | " | 11.1 | 1 | 54 | 30 | | | " " |
| 312. | " | 11.2 | 1 | 74 | 56 | | | " " |
| 313. | " | | | 105 | 64 | | | Heis, Ibid., p. 21. |
| 314. | " | | | 57 | 55 | | | " " |
| 315. | " | | | 34 | 31 | | | " " |
| 316. | " | | | 72 | 48 | | | " " |
| 317. | " | | | 74 | 68 | | | " " |
| 318. | " | | | 64 | 22 | | | " " |
| 319. | " | | | 55 | 20 | | | " " |
| 320. | " | | | 60 | 45 | | | " " |
| 321. | " | | | 88 | 36 | | | " " |
| 322. | " | | | 49 | 42 | | | " " |
| 323. | " | | | 86 | 41 | | | " " |
| 324. | " | | | 54 | 47 | | | " " |
| 325. | " | | | 30 | 25 | | | " " |
| 326. | " | | | 49 | 38 | | | " " |
| 327. | " | | | 28 | 20 | | | " " |
| 328. | " | | | 43 | 30 | | | " " |
| 329. | " | | | 69 | 45 | | | " " |
| 330. | " | | | 58 | 46 | | | " " |
| 331. | " | | | 86 | 50 | | | " " |
| 332. | " | | | 64 | 48 | | | " " |
| 333. | " | | | 156 | 55 | | | " " |
| 334. | " | | | 84 | 66 | | | " " |
| 335. | " | | | 40 | 20 | | | " " |
| 336. | " | | | 42 | 32 | | | " " |
| 337. | " | | | 70 | 38 | | | " " |
| 338. | " | | | 102 | 37 | | | " " |
| 339. | " | | | 61 | 48 | | | " " |
| 340. | " | | | 40 | 36 | | | " " |
| 341. | Nov. 13, | 9.5 | 1 | 49 | 42 | | | *Amer. Jour.* [2], xxxvii, 143. |
| 342. | " | 9.7 | 1 | 56 | 26 | | | " " |

The altitudes Nos. 231–239 were computed from the observations. A part of those from No. 25 to No. 87, are taken from Feldt's article, *Astr. Nach.*, xvi, 339. A few numbers in the sixth column represent the height of the middle point of the path, as Nos. 281–284.

The altitudes Nos. 313–340 are taken from a diagram given by Herschel in the *Proceedings of the British Meteorological Society.* I have not seen the printed results of Prof. Heis's observations. But as the series is very much more reliable than most of those in the table, I have ventured to add it to the rest, although the numbers are liable to considerable error in the two processes, of engraving, and of measuring from the diagram.

2. *On Sun-Spots and their Connexion with Planetary Configurations;* by Balfour Stewart, Esq., Observatory, Kew. (Proceedings of Royal Society.)—The author was led to investigate the solar autographs taken at Kew and Cranford, under the superintendence of Mr. Warren De La Rue, with the view of ascertaining if the behavior of sun-spots has any reference to planetary configuration. The following law was found to hold:—If the sun's disc be full of spots at any time, and if one of these begins to wane as it comes up by means of the sun's rotation to a

certain elliptical longitude, another will do the same. In fine, all spots at the same time behave in the same manner as they pass the same longitude. The author then remarked that, although this mode of investigation can only be considered as approximate, yet it furnishes us with an extremely delicate test of the *fact* of planetary action; for, if it be once clearly proved that sun-spots behave in this way, the only possible explanation is an influence from without. It was then shown that the influence of the planet Venus appears in this respect to be particularly powerful, the law being that, as any portion of the sun's disc recedes from the neighborhood of Venus, it acquires a tendency to break out into spots, while, as it approaches Venus, these spots begin to heal up. The author then referred to Mr. Carrington's observations on sun-spots, which appeared to indicate that the period when Jupiter is farthest from the sun is most favorable for the development of spots over all portions of the sun's disc, more or less (the sun's diameter being here small compared to the distance of the planet). Coupling this with the observation regarding Venus, it may perhaps be inferred that the approach of a planet to its primary is favorable to luminosity, while the secession of the planet is favorable to spot production. Proceeding now to variable stars, the best formal explanation of the phenomena there presented is given by that hypothesis which supposes the star to be partly dark and partly bright, and to rotate on its axis, presenting to us the dark and bright portions alternately; but this is physically improbable. If, however, we suppose a large planet to revolve near the star, we shall have, if the above law holds true, a bright part next the planet and a dark part farthest from it; and this appearance will revolve with the planet, and will be phenomenally equivalent to a body partly dark and partly bright. Again, if a large planet have a very elliptical orbit, there will be a long space of time during which the former is far removed from its primary, and a short space during which it is very near; and, if we assume the law stated above, we should here have a long time of comparative darkness and a short time of intense brightness. Such an alternative is presented to us by temporary stars. On the whole, therefore, this law seems to explain all that is yet known on this subject, and may, perhaps, be of use as a temporary hypothesis.—*Reader, April* 23, 1864.

3. *Observations on the Spots on the Sun from Nov.* 9, 1853, *to March* 24, 1861, *made at Redhill;* by R. C. Carrington, F.R.S. With 166 plates. London: Williams & Norgate.—The following is from a notice of this great work (signed J. N. L.) in the Reader of May 7:—[A copy of the work has not yet reached us.—Eds.]

All our text-books tell us that the Sun turns on its axis, the period of his axial rotation having been deduced from observations of his spots. But, from the time of Galileo, who made the period of rotation about a lunar month, down to our own, authorities have differed very considerably. Thus Grant, in his "History of Physical Astronomy," gives a period of $27^d$ $8^h$ (he quotes no authority). Laugier found $25{\cdot}34^d$, and later observers have made it still less.

Mr. Carrington now comes to the rescue, and tells us that *the spots travel at different rates, depending upon their distance from the equator* either north or south, and that the different rates are bound together by a law, so that he is enabled to represent all the rates very nearly by the formula: $865 - 165' \sin \frac{7}{4}$ lat.

Consequently the sidereal rotation of the equatorial photosphere is accomplished in 30·86 days, and of that at a latitude of 50° N. or S.—the highest point at which spots have been observed—in 28·36 days.

We said of the *photosphere:* the Sun itself—whether it be the glade-bedeckt world imagined by Sir Wm. Herschel, or the incandescent globe required by both the old and the new philosophies—has revealed none of its secrets to Mr. Carrington. But it is clear that *it* must be content with one only of these differing rates of motion; and the question is, which is it? Sir John Herschel, in an admirable article on sun-spots, in the last number of the *Quarterly Journal of Science*, deals with this question. Mr. Carrington considers that the views of Professor Thomson "on the Mechanical Energies of the Solar System" are supported by his discovery, supposing that the Sun itself travels more slowly than the equatorial photosphere. He remarks;—"In the absence of an impressed motion from some such external force, it would be expected that the currents of the surface of the Sun would resemble those of the Earth's ocean and atmosphere, and be westerly and toward the poles in the tropical latitudes, and easterly in the higher latitudes; the direction of rotation in such cases being the same, and the equatorial region in each the hottest."

Besides determining anew the elements of the Sun's equator—in other words, the position of the Sun's pole-star—Mr. Carrington has put us in possession of an important fact regarding the minimum period of sun-spots. He detected "a great contraction of the limiting parallels between which spots were found previously to the minimum, . . . and soon after this epoch the apparent commencement of two fresh belts of spots in *high latitudes*, north and south, which have in the subsequent years shown a tendency to coalesce, and ultimately to contract, as before, to extinction."

In Sir John Herschel's paper, to which we have before alluded, there is a passage which shows in a very strong light the value of these remarks of Mr. Carrington. In attempting to account for the phenomena of sun-spots by the presence of a nebulous ring, he writes:—

"Let us suppose (and such a supposition has not been deemed inadmissible in attempting to account for the periodical return of meteors) the existence of an elliptic ring of vaporous, nebulous, or small planetary, matter, with such a major semi-axis (4·979) as corresponds to a periodic time of each of its particles = 11·11 years; of such eccentricity as to bring its perihelion within the limits of the solar envelopes; and revolving either in the plane of the ecliptic or in some other plane at a more considerable inclination of the sun's equator. Let it be further assumed (still in analogy with assumptions not regarded as unreasonable in the meteoriferous ring), that the distribution of the circulating matter in it is not uniform—that it has a maximum and minimum of density at nearly, but not quite, opposite points, and no great regularity of gradation between them. It is very conceivable that the matter of such a ring, introducing itself with planetary velocity into the upper and rarer regions of the sun's atmosphere at an incidence oblique to its regular and uniform equatorial drift, might create such disturbances as, either acting directly on the photosphere, or intermediately through a series of vortices or irregular movements propagated through the general atmosphere, should break its continuity and give rise to spots, conforming in respect of their abundance and magnitude to the required law of periodic recurrence. If the change of density from the maximum to the minimum were gradual, but from the minimum to the maximum more abrupt, so as to allow the disturbances to subside gradually and recommence abruptly—the fresh and violent impulse would be delivered first of all on a region remote from the equator (by reason of the obliquity of the ring), and would give rise to a recommencement of the spots in comparatively high latitudes.

If the section of such a ring as we have supposed at its aphelion were *nil*, the period of 11·11 years would be strictly carried out; the maxima and minima would succeed each other with perfect regularity, and the paucity and abundance of the spots in the several phases of the same period would follow a fixed ratio. But if not, the several parts of the ring would not revolve in precisely equal times—the period of 11·11 years would be that of some dominant medial line, or common axis of all the sections in which a considerable majority of its matter was contained—and the want of perfect coincidence of the other revolutions would more or less confuse without obliterating the law of periodicity, which, supposing the difference to be comprised within narrow limits, might still stand out very prominently. Now, it might happen that there were two such medial lines, or more copiously stocked ellipses, each having a maximum or minimum of density, and that their difference of periodic times should be such as to bring round a conjunction of their maxima in 56 or any other considerable number of years; and thus would arise a phenomenon the exact parallel of Dr. Wolf's long period and his series of greater and lesser maxima."

We have given this extract to show the value of a *single well-ascertained fact;* and we congratulate our author upon the possession of that sagacity which, by limiting his field, has enabled him to produce such facts.

## V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Medal of the Royal Society to Prof. William Thomson.*—At the meeting of the Royal Society of April 18th last, the Keith medal was presented to Prof. Wm. Thomson, with the following remarks by Sir David Brewster, Vice-President of the Society and Chairman of the meeting:—"Professor William Thomson, who was elected a Fellow of the Society in 1847, has, during the last seventeen years, communicated many valuable papers to the Society which have added greatly to the value of its transactions. These papers, and others elsewhere published, relate principally to the theories of electricity, magnetism, and heat, and evince a genius for the mathematical treatment of physical questions which has not been surpassed, if equalled, by that of any living philosopher. In studying the mathematical theory of electricity he has greatly extended the general theorems demonstrated by our distinguished countryman, Mr. Green, and was led to the principle of 'electrical images,' by which he was enabled to solve many problems respecting the distribution of electricity in conductors, which had been regarded as insolvable by the

most eminent mathematicians in Europe. In his researches on thermodynamics, Professor Thomson has been equally successful. In his paper 'On the Dynamical Theory of Heat,' published in our *Transactions* for 1851, he has applied the fundamental propositions of the theory to bodies of all kinds, and he has deduced many curious and important results regarding the specific heats of bodies, which have been completely verified by the accurate experiments of Mr. Joule. No less important are Professor Thomson's researches on solar heat contained in his remarkable paper 'On the Mechanical Energy of the Solar System'—his researches on the conservation of energy as applied to organic as well as inorganic processes; and his fine theory of the dissipation of energy, as given in his paper 'On a Universal Tendency in Nature to the Dissipation of Mechanical Energy.' To these we may add his complete theory of diamagnetic action, and his investigations relative to the secular cooling of our globe, and the influence of internal heat upon the temperature of its surface. The value of labors like these could not escape the notice of the Council of this Society, and they would have entitled their author to the Keith Prize had they not been presented to the Society when the prize was devoted to other branches of science. It is not, therefore, for these researches and discoveries that the Keith Medal has been awarded to Professor Thomson, but for the very interesting and important discovery in abstract dynamics which he has communicated to the Society during the biennial period appropriated to physical sciences. In presenting this prize to Professor Thomson, I am proud to think, and I am sure that all here will participate in the sentiment, that Scottish science has such a representative in the University of the west, while, in our own, it has one of kindred genius and power."—*Reader*, April 23, 1864.

2. *Sir R. I. Murchison.*—The Wollaston Gold Medal was awarded to Sir R. I. Murchison by the Geological Society, at its annual meeting in February last, for his "distinguished services in Paleozoic Geology, especially (1) for his great work entitled the Silurian system, (2) his work on the Geology of Russia, and (3) for his discovery of the true relations of all the rocks beneath the Old Red Sandstone that form the Highlands of Scotland."—*Reader*, March 5.

3. *Man formerly accompanied by the Reindeer in Central France.*—Lartet and Christy have found in caves in central France (that of Eyzies and others) a floor-breccia containing bones of the Reindeer and other animals, ashes, fragments of charcoal, flint chippings, and weapons and utensils of Reindeer bones and horns, with slabs of stone having sometimes the forms of animals scratched upon them. Among the remains of the Reindeer, several vertebræ are sometimes found united, and also jointed bones with their parts still in connection, showing that the animals must have lived in the region; and the long bones are usually broken in the same uniform way, and evidently to get the marrow out. The remains of the common stag, wild boar and hare are very rare. A few teeth of the Irish Elk (*Megaceros Hibernicus*) are found, and an occasional dental plate of the old Elephant (*E. primigenius*) is met with. There is no written record of the existence of the Reindeer, or of a subarctic climate, in what is now temperate Europe.—*Nouv. Obs. de MM. Lartet et Christy, etc., Comptes Rend.*, lviii, Feb. 29, 1864.

4. *Extracts from a paper on the Geography of British Columbia and the Condition of the Cariboo Gold District;* by Lieut. H. S. PALMER, R.E.—Lieut. Palmer mentioned that since the first discovery of gold in British Columbia in 1858, fresh deposits had gradually been traced farther and farther northward, till ultimately the well-known fields of Cariboo had been reached, 500 miles from the mouth of the Fraser. Entrusted with the task of a general survey, he details the geographical outline of the colony, the seaboard of which extends 500 miles, protected throughout almost its entire length by Vancouver and Queen Charlotte islands. This seaboard is indented in the most extraordinary manner by deep bays and arms of the sea, presenting an extent of sheltered inland navigation, and an actual length of shore-line, such as are nowhere equalled on any similar stretch of coast in the world.

The most marked physical feature of the country, viewing it from the shore inland, is the parallelism of two mountain ranges with an elevated intervening plateau of rolling country 100 miles in breadth. The coast-line of mountains is known as the Cascade or Coast range, 120 miles wide, the western slopes of which are covered with the most magnificent forest. Its sea-front is everywhere bluff and abrupt and quite close to the shore, except where the Fraser falls into the San Juan Fuca Sound, when it recedes some 40 miles. The eastern side of the range is drier, the trees more scattered, and the general profile less abrupt. The principal crest of this chain is about 5000 feet above the sea, a peculiar characteristic being the almost entire absence of peaks. The rivers on the east side are naturally longer and less impetuous than those on the west; but occasionally some of them rise on the plateau, and thread the mountains till they fall into the sounds. Above some of these, glaciers are said to have been seen; but nothing authentic seems to be known on this subject.

The scenery of the table-land, which is well suited for pastoral purposes, is described in high terms; the rivers having occasionally hollowed out for themselves channels of immense depth, in which occur splendid cascades, some of which are mere fissures; in other cases running through broad-terraced valleys, or in vales of gently undulating slopes covered with grass and picturesquely dotted with yellow pines. Here and there are pretty sheets of water, which, like the rivers, are well supplied with numerous kinds of fresh-water fish. Above 3000 feet, the grass, which gradually gets less nutritive with the increased elevation, gives place to a universal mantle of dwarf fir. Here farming has proved moderately successful at an elevation of 2100 feet, but Lieut. Palmer doubts whether a considerable time must not elapse ere enough grain can be raised in the more sheltered and well-irrigated valleys, to admit of its finding a market at the mines or settlements.

Just beyond begins the second mountainous range, which extends without a break to the watershed of the Rocky Mountains, which as far north as the Peace river, flowing eastward, forms the eastern boundary of the colony on this side. The only portion of this unexplored region where white men are to be met, is Cariboo.

Cariboo lies in the elbow formed by the upper waters of the Fraser, and is bounded on the south by the Quesnelle river. A marked phenomenon is the confused congeries of hills of considerable altitude, from 6000 to 7000 feet high, thickly timbered, whence subordinate ranges radiate as centres. Each valley thus formed is the bed of a stream of more or less proportions, from the tiniest, called 'gulches' by the miners, which may be jumped over, to respectable-sized rivers. All these have long since been 'prospected,' every creek having been discovered to be more or less the site of the richest deposits of gold. A circle of three miles radius from the top of Bald mountain contains five creeks, two of which are the most notorious gold-beds in the colony. Snowshoe mountain contains the headwaters of no fewer than six of these within a similar area, the streams in every case radiating to every point of the periphery. The views from the summits of these mountains are described as splendid.

A succession of auriferous deposits have been traced, following the general trend of the main chain of mountains extending from the southern boundary of the colony to the Peace river, i. e., over 7° of latitude, while the extremities, so far as ascertained, lie between the meridians of 119° and 122° W.

The winter of Cariboo appears to be much more severe and prolonged than that of the coast, or of Vancouver Island, and will much retard the development of the mines, which are accordingly during that season 'laid over,' as it is termed—i. e., the laws enforcing the mode of working them, &c., are remitted for the time. The thermometer sometimes falls to −35° C. (31° below zero of Fahr.), when of course nothing but underground claims can be worked. The thaw, which commences about April, renders Cariboo for a season anything but an enviable residence, owing to the rains and the steaming mists, while locomotion is all but impossible. In past years the trail at this season has been loathsome from the numbers of horses that lay unburied after succumbing to the tremendous toil of conveying the first convoys of provisions.

Although for ten or eleven months in the year the country has a gloomy, cheerless aspect, August and September being the only bright exceptions, it is remarkably healthy. The sun is late in making his appearance, even in midsummer, owing to the hills enclosing the diggings on every side.

Of late 400 miles of excellent waggon-roads lead from Yale, the present head of steam-navigation, so that the entire distance from New Westminster to Cariboo can now be accomplished in from six to seven days. The author, in summing up, said that at the estuary of the Fraser, the winters somewhat resembled those of England, though the extremes were greater; and that the rainfall there is about 54 inches annually.—*Proc. Roy. Geogr. Soc.*, March 14th, viii, 87.

5. *A newly discovered pass across the Andes.*—Señor Cox, the son of an English physician at Valparaiso, has discovered a pass across the Andes, not over 2800 feet high in its most elevated part. He started in 1862 from Port Montt, a new German settlement now containing 15,000 inhabitants, near the island of Chiloe, and proceeded by way of the two lakes Llanquilhue and Todos-os-Santos, and crossed over the pass to the almost unknown inland sea of Nagel-huapi (Lake of Tigers), on the eastern side of the Andes.—*Proc. Roy. Geogr. Soc.*, May 9.

6. *Violet colors from iodine.*—Prof. HOFMANN has patented in England the process of manufacturing a new color, obtained from iodine, which affords several beautiful varieties of violet. The material, which is to be used for dying, is made by mixing rosaline with the iodids of ethyl, methyl or amyl.—*Athenæum,* Ap. 2.

7. *The Holy Land and Dead Sea.*—An expedition from France conducted and equipped by Duc de Luynes, and having L. Lartet, as its geologist, is now in the region of the Holy Land and Dead Sea, engaged in investigations bearing on unsolved problems connected with that part of Western Asia. According to a brief report made by Daubrée to the Academy of Sciences at the last meeting in March, cave deposits abounding in flint knives and fossil bones were found soon after landing at Beyrout. It is reported also that Mr. Vougoué has discovered flint knives in a cave at Bethlehem, and also on Mt. Sinai; and that the former locality will no doubt be examined by the Duc de Luynes's expedition.—*Reader,* April 2.

8. *Bone-Cave in Borneo.*—A bone-cave has been stated to occur in Northwestern Borneo, containing numerous bones in the hardened guano which constitutes its floor; but none of the bones have yet been collected or examined.

9. *Heights in the Rocky Mountains.*—Pike's Peak, according to observations by C. C. Parry, in July, 1862, has an elevation of 14,215 feet, and Mt. Gray, on the upper waters of South Clear Creek, of 14,245 feet. Mr. Parry remarks that the observations in both these cases were made under unusually favorable circumstances, and are believed to furnish accurate results.—*C. C. Parry, in the Daily Rocky Mountain News, for March* 13, 1863, *Denver, Colorado Ter.*

10. *Astronomy in France.*—A proposition has emanated from Le Verrier for the establishment of a comprehensive Astronomical and Meteorological Association, the head office to be in the Imperial Observatory. The Association, the plan of which has been approved by the Emperor, will be under the direction of Le Verrier.—*Athenæum, May* 28.

11. *Law for the primary and secondary barometrical maxima and minima in each half-month.*—Mr. PLINY EARLE CHASE, of Philadelphia, announces in a letter dated June 4th, that on the 17th he will read before the Philosophical Society a paper sustaining his law for the primary and secondary barometrical maxima and minima in each half-month by showing its precise correspondence with the St. Helena three years' hourly observations in four-fifths of the averages, and that in the remaining fifth, the greatest deviation is one day.

12. *Chicago Observatory.*—An astronomical observatory has been recently commenced at Chicago, through the liberality of some of her citizens, to be connected with the University of Chicago. A large equatorial telescope, now partly finished, has been ordered from Alvan Clarke, of Boston, which is to be 23 feet in length, and 18¾ in aperture. It is to cost, including transportation and mounting, $18,187, and to be finished in June, or some time before the tower is ready for its reception. The tower is to be octagonal in shape, 35 feet in diameter and 100 feet high to the hemispherical top. Another tower, also, is to be erected, for the meridian circle.—*Chicago Tribune,* Feb. 23, 1864.

OBITUARY.

RUDOLPH WAGNER.—The eminent comparative anatomist, R. Wagner, died at Göttingen on the 13th of May. He was born at Baireuth in 1805. He studied at Erlangen and Würtzburg, and afterwards at Paris; in 1833 he became Professor of Zoology at Erlangen, and in 1840, of Comparative Anatomy at Göttingen. In 1834 he published his *Lehrbuch der vergleichenden Anatomie*, in 1839, his *Lehrbuch der Physiologie*, and in 1854, his *Neurologische Untersuchungen*, embracing the results of his investigations on the electrical organs of the torpedo, made in Italy in 1845, 1846, and of other researches. He also contributed to the *Handwörterbuch der Physiologie*, of which he was editor, and to various scientific Journals.

EVAN PUGH, Ph.D., President of the Agricultural College of Pennsylvania, died April 29th, 1864. [An obituary notice will appear in our next number.—EDS.]

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Metallurgy: The art of extracting metals from their ores, and adapting them to various purposes of manufacture;* by JOHN PERCY, M.D., F.R.S., Lecturer on Metallurgy at the Royal School of Mines. *Part II: Iron and Steel.* 8vo, pp. 934. Murray, London, 1864.—This volume forms the second division of Dr. Percy's great work on Metallurgy, already briefly noticed in a former number of this Journal.[1] Although its issue from the press has been deferred for more than a year after it was promised, this delay has been greatly to the advantage of the work, for we have now given us one volume devoted entirely to *Iron and Steel.* It is characterized by the same thoroughness, fairness, and comprehensiveness which we noticed in speaking of the first volume. It is copiously illustrated with drawings, chiefly from original sources, and carefully laid down to scale. It also contains the results of a large number of original investigations, having an important bearing on many of the practical and theoretical points discussed, and constituting one of the most interesting and valuable features of the work. The author has had the coöperation of some of the leading Ironmasters and Metallurgists in Great Britain and on the Continent, in many matters regarding special processes and furnaces. Prof. J. P. Lesley has furnished information and statistics of iron-smelting in this country, and we are glad to notice two large folio lithographs of the Thomas Anthracite Iron Furnaces at Hokendauqua, Pa., from drawings made under the direction of Samuel Thomas, Esq.

It is scarcely possibly within our limits to give even a brief synopsis of the work. The first 146 pages are devoted to the *physical and chemical properties of iron*, then 50 pages of matter on the *alloys of iron*, and this is followed by a description of the *ores of iron*, their *chemical composition* and the methods for their *assay*,—in all some 55 pages. Dr. Percy then gives about a hundred pages to the discussion of the methods for the *indirect extraction of iron in the malleable state from the ore*, in which he makes mention of many interesting historical facts of the simple processes for iron-smelting used by the natives of India, Burma, Borneo, Africa and Madagascar; including also long no-

[1] [2], xxxv, 118.

tices of the *Catalan Process* and the *German Hearth*, of great interest from their extensive application in some parts of our country. Under the *indirect extraction of iron in the state of cast-iron from the ore*, we have a full account of the chemical phenomena of the blast-furnace, a description of the blast-furnaces used in different countries; a discussion of the "hot-blast" and of the "waste gases"; the form of the blast-furnace; the character and composition of the pig-iron produced, etc., occupying over 200 pages. In the chapter *on the production of malleable iron from cast-iron*, are given all the details in regard to *fineries* and *hearths*, and the *puddling process*, with remarks on special qualities of iron, and interesting commercial details. The section on *Steel* is equally comprehensive. In conclusion, there is given an exceedingly interesting *sketch of the history of iron*. The whole book is full of valuable observations and criticisms: the latter, though sometimes bordering on the personal, are alike entertaining and instructive. It is not only the greatest work on Iron and Steel in the English language, but it is also one of the most original, comprehensive and extensive practical works ever published on these subjects. G. J. B.

2. *Annual Report of the Board of Regents of the Smithsonian Institution for the year* 1862. Washington, 1863.—Forty-five pages of this volume are occupied by the Report of the Secretary, Prof. Henry, which shows that the Institution is doing a large amount of excellent work "for the increase and diffusion of knowledge," in the way of the promotion of scientific researches and explorations, the increase of its Museum, the distribution of specimens to American Institutions, the publications of new memoirs or works, and the sustaining of a system of lectures at Washington. The lectures of the year are in many cases printed in the Annual Report, and add greatly to its value. The volume for 1862 contains the Lectures of Pres. F. A. P. BARNARD on the Undulatory Theory of Light, pp. 107–239; and of Prof. DANIEL WILSON, of Toronto, on Physical Ethnology, pp. 240–302. And in addition there are the following foreign and American selected memoirs: A. MORLOT's introductory lecture on the study of high antiquity, delivered at the Academy of Lausanne, Switzerland, Nov. 29, 1860; a memoir by J. LUBBOCK, Esq., on North American Archeology; FLOURENS's Historical Sketch of the Academy of Sciences of Paris, and also his Memoirs of von Buch and Thenard; QUATREFAGES's Memoir of I. Geoffroy Saint Hilaire; a translation of a paper by T. L. PHIPSON on the Catalytic Force, or Studies of the Phenomena of Contact; views by J. P. LESLEY on the Classification of Books; and a Catalogue of Prize Questions of Scientific Societies. The volume also contains an account of a female mummy from Patagonia.

3. PARRISH's *Practical Pharmacy: Third Edition, thoroughly revised and improved, with important additions. With* 238 *Illustrations.* Philadelphia: Blanchard & Lea, 1864. pp. 850.—The present edition of this valuable work contains many improvements, adapting it to the wants of apothecaries, physicians and students, and, in general, brings up the topics of which it treats to the present standard of the science and art of pharmacy.

In successive editions a work of this character naturally grows upon the author's hands. In the effort to insert all that is valuable to the different classes of readers for whom it is designed, it is more difficult to tell what to omit than to find new matter of interest. So far as we can determine, the author has preserved a happy medium, and has furnished a new edition of an already popular work, which admirably maintains the high character universally accorded to previous editions. Many new and valuable illustrations have been introduced in this edition, and the publishers' part has been well executed.

4. *Verhandelingen der Koninklijke Akademie van Wetenschappen.* Amsterdam. *Vierde deel*, 4to, pp. xxxiii, and 572, *Tables d'Intégrales Définies*, par D. Bierens de Haan: and *Achtste deel*, pp. xii, and 702, *Exposé de la Théorie, des Propriétés, des Formules de Transformation, et des Méthodes d'Evaluation des Intégrales Définies*, by the same.—These two volumes are here placed together, although the first has been issued several years, because the second is its natural supplement. In the *Tables* are collected and arranged, in a form convenient for use, six or eight thousand definite integrals. One who has ever had occasion to use a definite integral not familiar to him, one the indefinite form of which does not admit of being expressed in finite terms, can appreciate the value of this collection, merely as a dictionary. In no part of the Mathematics are errors so easily made, as in obtaining the values of definite integrals, and nowhere are errors more difficult of detection. Results obtained by one analyst are often pronounced false by another. For this reason, it is of very great value that with each definite integral is given a reference to the memoir, or volume, where the process of obtaining it is explained. The bibliography thus furnished is of great importance.

It was most natural that this second volume should follow the first. After collecting and arranging the forms of the various known integrals, it was to be expected that a collation of theorems and methods, and an extension of many of them should follow. The evaluation of over two thousand new integrals is but one of the results in this second volume.

Report, Historical and Statistical, on the collection in Geology, Zoology and Botany, in the Museum of the University of Michigan, made to the Board of Regents, Oct. 2, 1863, by A. Winchell, A.M., Prof. Geol. and Botany. 26 pp. 8vo. Ann Arbor, Michigan, 1864. The collections in Zoology and Geology at the Michigan University are among the best in the country.

A list of Animals dredged near Caribou Island, Southern Labrador, in July and August, 1860; by A. S. Packard, Jr. 30 pp. 8vo, with 2 plates.—From the Canadian Naturalist and Geologist, Dec. 1863.

Report of the Astronomer in charge of the Dudley Observatory, for the year 1863, 44 pp., 8vo. Albany, 1864.—This report by G. W. Hough, contains an account of the condition of the principal instruments of the Dudley Observatory, with discussions of methods of adjustment, etc.

On the Nomenclature of the Foraminifera; by W. K. Parker, Esq., and Prof. T. R. Jones, F.G.S. Part X. 13 pp. 8vo.—From the Ann. and Mag. Nat. Hist., for Dec. 1863.

Lectures on the Elements of Comparative Anatomy; by Thomas Henry Huxley, F.R.S., Prof. Nat. Hist. in the Roy. School of Mines, and of Comp. Anat. and Phys. to the Roy. College of Surgeons. London, 1864.—The inaugural volume of a series; it consists of some 300 pages, 112 of which are devoted to a sketch of the classification of animals, and 190 to the structure and theory of the vertebrate skull.—*Reader, May* 21.

Matter and Force (Kraft und Stoff); by Dr. Louis Büchner. Translated by J. F. Collingwood. Stated to expound and carry out the evolution theory of nature to its extreme logical result of no God, no spirit, no immortality.

Blackwall's Spiders of Great Britain and Ireland. Part II. Roy. Society. London, 1864.—Treats of the *Theridiidæ, Linyphiidæ, Epeiridæ* and *Scytodidæ.*

Histoire Naturelle des Araignées; by M. EUGÈNE SIMON. 450 pp. 8vo. Paris, 1864.

Dictionnaire Général des Sciences, edited by MM. PRIVOT-DESCHANEL and AD. FOCILLON, with the collaboration of such men as Barral, Foucault, and Marié Davy 1st part, 650 pages, A to C. Paris, 1864.

La Kabylie et les Kabyles, Esquisse géologique et geographiques; and Le Sahara, ses differents types de Deserts et d'Oasis; two pamphlets by E. DESOR. From the Bulletin of the Soc. des Sci. Nat. de Neuchâtel, 1864. 1864.

L'Année Scientifique et Industrielle, ou exposé annuel des travaux scientifiques, des Inventions et des principales applications de la science à l'Industrie et aux arts qui ont attiré l'attention publique en France et à l'étranger; par LOUIS FIGUIER. Huitième année. 558 pp. 12mo. Paris, 1863. L. Hachette et Cie.—The subjects are introduced under the heads of Astronomy, Physics and Mechanics, Meteorology, Chemistry, Hydrography (in the course of which there is much in condemnation of Maury's notions), Natural History, Travels, Public Hygiene, Medicine, Agriculture, Industrial arts, Academies and Learned Societies, Scientific Necrology.

ANNALS OF THE LYCEUM OF NAT. HIST., OF NEW YORK, Vol. VII, Nos. 13–16, Dec. 1861—Feb. 1862.—p. 367, Analytical Synopsis of the Order of Squali, and revision of the nomenclature of the genera; *T. Gill.*—p. 414, On the extension of the Carboniferous system of the U. States so as to include all true coals; *R. P. Stevens.*—p. 420, On certain species of N. American Helicidæ; *T. Bland.*—p. 449, On the occurrence, within the limits of the U. S., of Bucephala Islandica; *D. G. Elliot.*—p. 455, Descriptions of six new species of Birds of the families Charadridæ, Trochilidæ and Caprimulgidæ; *G. N. Lawrence.*—p. 461, Catalogue of a collection of Birds made in New Grenada by J. McLeannan, with notes and descriptions of species; *G. N. Lawrence.*—p. 480, Descriptions of two new species of Mollusca of the genus Corbicula; *T. Prime.*—p. 482, Description of new species of Venus; *T. Prime.*

Vol. VIII, No. 1. May—October, 1863.—p. 1, Catalogue of a collection of Birds made in New Grenada by J. McLeannan, with notes and descriptions; *G. N. Lawrence.*—p. 14, On the family Proserpinacea, with a description of a new species of Proserpina; *T. Bland.*—p. 17, Remarks on Classifications of N. American Helices by European authors, and especially by H. and A. Adams and Albers; *T. Bland.*

PROCEEDINGS OF THE AMERICAN PHIL. SOC., PHILADELPHIA, Vol. IX, No. 70.—p. 234, On Solar Spots: *O. Reichenbach.*—p. 271, On the number of vocal sounds foreign to the English language; *P. E. Chase.*—p. 283, On the diurnal variations of the barometer; *P. E. Chase.*

PROCEEDINGS OF THE ACAD. NAT. SCI. OF PHILADELPHIA, No. 2, March—April, 1864.—p. 51, Additions to the catalogue of Stars which have changed their colors, or which have appeared with different colors at different times; *J. Ennis.*—p. 57, A new Labroid genus allied to *Trochocopus* Gthr.; *T. Gill.*—p. 59, Note on the nomenclature of genera and species of the family Echeneidoidæ; *T. Gill.*—p. 62, Notes on the Birds of Jamaica; *W. T. March* and *S. F. Baird.*—p. 72, Critical review of the family Procellaridæ: Part I, embracing the Procellarieæ, or Stormy Petrels; *E. Coues.*—p. 92, Synonymy of the species of Strepomatidæ, a family of fluviatile Mollusca inhabiting N. America, Part 3d; *G. W. Tryon, Jr.*—p. 105, New species of Mordellistena collected in Illinois; *C. A. Helmuth.*—p. 106, New species of Birds of the families Cærebidæ, Tanagridæ, Icteridæ and Scolopacidæ; *G. N. Lawrence.*—p. 108, Six new species of Unionidæ from Lake Nyassa, Central Africa, &c.; *I. Lea.*—p. 109, Six new species of Succinea of the U. States; *I. Lea.*—p. 111, New species of Planorbis; *I. Lea.*—Thirteen new species of Melanidæ of the U. S.; *I. Lea.*—p. 113, Five new species of Lymnæa of N. America; *I. Lea.*—Two new species of Unionidæ from South Africa; *I. Lea.*—p. 114, Twenty-four new species of Physa of the U. States and Canada; *I. Lea.*—p. 116, Critical review of the family Procellaridæ, Part II, embracing the Puffineæ; *E. Coues.*

PROCEEDINGS OF THE ESSEX INSTITUTE, Vol. IV, Jan., Feb., March, 1864, No. 1.—p. 3, On the sodalite at Salem, Mass.; *D. M. Balch.*—p. 6, On magnetite and an unknown mineral at Nahant; *G. H. Emerson.*—p. 7, Notes on the family Zygænidæ; *A. S. Packard.*

This Society proposes to publish a Naturalists' Directory, to be issued in connection with the quarterly numbers of its Proceedings, "to contain the name, address and special department of study of every naturalist in the world, whose address can be obtained, under such a classification as will be most convenient for reference." Naturalists are requested to send their address and those of others with whom they may be in correspondence, to F. W. Putnam, Essex Institute, Salem, Mass.

THE

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[SECOND SERIES.]

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## ART. XII.—*Barometric Indications of a Resisting Aether*;[1] by PLINY EARLE CHASE, M.A., S.P.A.S.

IN a recent communication to the American Philosophical Society, I stated that there are indications in the hourly barometric means, of disturbances which may, perhaps, be owing to a resisting medium. As the proper interpretation of those disturbances seems, in my opinion, to involve a consideration of the principal laws that govern molecular force, I desire to present some of the evidences on which my statement was based, and to invite an investigation of the subject by mathematicians and physicists.

The contrast between the light, volatile atmosphere, and the sluggish, ponderous mercury, is so great, that few persons would expect any slight variations of one to be accurately recorded by the other. But that such is the case, has been shown by my barometric investigations; and so minute is the apparent correspondence between the two fluids, that it does not seem unreasonable to look even for traces of æthereal disturbance in the mercurial column.

I believe no attempt was ever made to represent the daily ærobaric tides by general algebraic expression, prior to the one to which I was led by an *a priori* consideration of the effects of rotation.[2] The remarkable correspondence between the theoretical formula and the results of observation, being, as it is, far

[1] I prefer this spelling for the fluid which is supposed to pervade all space, to distinguish it from the liquid Ether of the chemists.

[2] Proc. Am. Phil. Soc., vol. ix, p. 285; and Sill. Jour. for May, 1864, p. 410.

within the limits of probable error, lends strong confirmation to my original hypothesis, and seems to require a revision of the reasoning which has led to the prevalent belief that all the influence of rotation must be constantly uniform, and therefore incapable of producing any tidal action.

Perhaps the error of that reasoning, as applied to the earth, has arisen from considering the joint action of rotation and orbital revolution, and making undue allowance for the mutual compensation of their mutual disturbances. That such compensation must be effected at every instant, I have no reason to doubt, but how is it modified by the stress of gravity and the stretch of molecular elasticity? That stress, at the surface of the earth, is nearly three hundred times as great as would be necessary to retain the particles in orbits at the same mean distance from the earth's centre; and since it probably follows a different law from the countervailing elasticity, it seems to me that each of the forces must be constantly accomplishing work. The value of this work must form an important element in all calculations of the effects of the earth's rotation, and, if it were properly ascertained, I have little doubt that it might be represented by a formula analogous to the one which I have deduced from the relative motions of the air and the earth's centre.[1]

I am inclined to believe, as I have intimated elsewhere, that a due consideration of these relative motions may help to explain other compensations, such as the variations in temperature and the deposition of dew during the night, the fluctuations of magnetic force, &c. I content myself, for the present, with a simple statement of the belief, because its confirmation or refutation will depend principally upon the results of the investigation that I am now inviting.

The following table is constructed from data furnished by Gen. Sabine's summary of three years' hourly observations at St. Helena,—a work of great value, on account of the number and accuracy of the observations, and the proximity of the station to the Equator. The 4th column, (B), represents the averages of the observed heights of the barometer at 0, 1, 2, and $3^h$ from each high and low ærobaric tide; the fifth column, (C), is the theoretical height, computed by the formula,

$$B = M\left(\frac{1 + \sin\theta\cos\theta\cos l}{R^3}\cdot\frac{2C}{gt^2}\right);$$

the other columns require no explanation.

[1] It willl be readily seen that the sun's attraction is in the same direction as the earth's, and opposed to the molecular elasticity, at midnight; but at noon it acts with the elasticity, and against terrestrial gravity.

TABLE OF HOURLY THERMOMETRIC AND BAROMETRIC MEANS.

| Astronomical time. | Temperature Fahrenheit. | (A) Observed height of barometer. | (B) Observed barometric averages. | (C) Theoretical height of barometer. | A—B. | A—C. | B—C. |
|---|---|---|---|---|---|---|---|
| h. | ° | 28 in. + | 28 in. + | 28 in. + | in. | in. | in. |
| 0 | 64·392 | ·2985 | ·296125 | ·2970 | ·002375 | ·0015 | −·000875 |
| 1 | 64·731 | ·2819 | ·28145 | ·2821 | ·00045 | −·0002 | −·00065 |
| 2 | 64·946 | ·2660 | ·267125 | ·2672 | −·001125 | −·0012 | −·000075 |
| 3 | 64·639 | ·2553 | ·2572 | ·2563 | −·0019 | −·0010 | ·0009 |
| 4 | 64·108 | ·2521 | ·25355 | ·2523 | −·00145 | −·0002 | ·00125 |
| 5 | 63·174 | ·2562 | ·2572 | ·2563 | −·001 | −·0001 | ·0009 |
| 6 | 62·120 | ·2642 | ·267125 | ·2672 | −·002925 | −·0030 | −·000075 |
| 7 | 61·403 | ·2764 | ·28145 | ·2821 | −·00505 | −·0057 | −·00065 |
| 8 | 61·067 | ·2899 | ·296125 | ·2970 | −·006225 | −·0071 | −·000875 |
| 9 | 60·828 | ·3003 | ·3077 | ·3079 | −·0074 | −·0076 | −·0002 |
| 10 | 60·666 | ·3061 | ·31225 | ·3119 | −·00615 | −·0058 | ·00035 |
| 11 | 60·491 | ·3025 | ·3077 | ·3079 | −·0052 | −·0054 | −·0002 |
| 12 | 60·330 | ·2913 | ·296125 | ·2970 | −·004825 | −·0057 | −·000875 |
| 13 | 60·184 | ·2777 | ·28145 | ·2821 | −·00375 | −·0044 | −·00065 |
| 14 | 60·002 | ·2646 | ·267125 | ·2672 | −·002525 | −·0026 | −·000075 |
| 15 | 59·868 | ·2562 | ·2572 | ·2563 | −·001 | −·0001 | ·0009 |
| 16 | 59·779 | ·2550 | ·25355 | ·2523 | ·00145 | ·0027 | ·00125 |
| 17 | 59·691 | ·2611 | ·2572 | ·2563 | ·0039 | ·0048 | ·0009 |
| 18 | 59·664 | ·2737 | ·267125 | ·2672 | ·006575 | ·0065 | −·000075 |
| 19 | 59·868 | ·2898 | ·28145 | ·2821 | ·00835 | ·0077 | −·00065 |
| 20 | 60·575 | ·3048 | ·296125 | ·2970 | ·008675 | ·0078 | −·000875 |
| 21 | 61·692 | ·3163 | ·3077 | ·3079 | ·0086 | ·0084 | −·0002 |
| 22 | 62·635 | ·3184 | ·31225 | ·3119 | ·00615 | ·0065 | ·00035 |
| 23 | 63·578 | ·3117 | ·3077 | ·3079 | ·004 | ·0038 | −·0002 |

The greatest theoretical error, (A–C,) is only ·0084 in. at $21^h$, or ·000297 of the entire height, and ·127 of the daily range; the least error is at $5^h$ and $15^h$, ·000004 of the height, or ·00151 of the range; and the average error ·0042 in., which is ·000147 of the height, or ·063 of the range.

The greatest difference between the observed height and the observed average, (A–B,) is at $20^h$, when it is ·131 of the daily range; the least difference is ·0068 of the range, at $1^h$; the average difference, ·0639 of the range.

The greatest difference between the observed averages and the theoretical height, (B–C,) is at low tide, or at $4^h$ and $16^h$, when the difference is less than ·02 (say ·0189) of the daily range; the least difference is at $2^h$ from low tide, or at $2^h$, $6^h$, $14^h$ and $18^h$, when it is but little more than ·001 (·00113) of the range; the average difference is only ·00926 of the range.

Both A–B and A–C show that some cause is operating from 1 or $2^h$ to $15^h$ inclusive, (say 2 P. M. to 3 A. M.,) to reduce the barometric pressure, while for the rest of the day the normal pressure is increased. That this cause is not to be found in differences of temperature, I think is evident, because the observations are already corrected for *known* effects of temperature, and because the average height of the thermometer from $2^h$ to $15^h$ ($61°·7$) corresponds very closely with the average from $16^h$ to $1^h$ ($61°·66$).

The greatest unexplained reduction of barometric pressure is at $9^h$; the greatest increase, at $20^h$ or $21^h$. All these facts appear to me to admit of a ready explanation, on the hypothesis that the disturbances are caused by the resistance of an æther, which is condensed, as Fresnel supposes, by planetary attraction, and I can imagine no other hypothesis by which they could be satisfactorily accounted for.[4]

Mr. Colburn's inquiry into the nature of heat suggests some interesting speculations concerning other effects of rotation than those that can be measured by the barometer. Recognizing the impossibility that the sun should warm the whole solar system, as a simple incandescent body,—the improbability that its heat should result from continuous combustion, and the probable approximate uniformity of temperature in the upper regions of the atmosphere, in summer and in winter, by day and by night —Mr. Colburn looks for the principal sources of heat in the earth itself. He supposes, 1, that the solar attraction tends to draw into closer proximity the particles of air on the heated side, and to separate them on the night side of the earth, thus producing heat of compression, and cold of expansion: 2, that the change of eastward velocity from 69,000 miles per hour at midnight, to 67,000 miles at noon, (*sic*) necessarily produces a conversion of motion into heat, and of heat into motion: and 3, that if the earth is moving in a resisting medium, by which it is so retarded that it approaches the sun at the rate of 1,000,000 miles in 3,000,000 years, its "lift" involves the annual abstraction of a heat-force equivalent to 752,665,108,390,000 horse-power!

The third hypothesis has been often broached; the indications of a resisting ether, which, as we have seen, are furnished by the hourly barometric means, may, perhaps, yield the data for its final verification or rejection. The supposed separating effect of the sun's action in the most remote portions of the atmosphere, is so problematical that it seems hardly deserving of any consideration, and even if it existed, it is difficult to understand how it could produce a difference of more than a fraction of a degree in the range of the thermometer. The alternate acceleration and retardation of orbital velocity can produce no *accumulation* of heat to supply any loss that may arise from radiation into space, but it must modify the *distribution* of heat throughout the day in a manner that may be readily calculated. The available data are not sufficient to furnish us with complete results,

[4] The remainder of this article is taken from my communication to the American Philosophical Society.

but they give curious approximations that seem to open a wide field for profitable investigation.

"Sir John Herschel finds the direct heating effect of a vertical sun at the sea level to be competent to melt ·00754 of an inch of ice per minute, while according to M. Pouillet, the quantity is ·00703 of an inch."[5] Taking the mean of these two estimates (·00728 in.), multiplying by the latent heat of water (142·6° F.), and dividing by the number of cubic inches in 1 lb. of water (28), we obtain $\frac{\cdot 00728 \times 142\cdot 6}{28} = \cdot 037076$ units of heat received per minute on each square inch of the earth's surface that is exposed to a vertical sun. The weight of the aërial column being 15 lb., and its ratio of specific heat 25, the maximum effect of the direct solar rays is sufficient to heat the whole atmosphere $\frac{\cdot 0371}{15 \times \cdot 25}$ per minute, or 7·12° F. in 12 hours.

Now, in consequence of the earth's rotation, the difference of atmospheric "lift" between noon and midnight, is 182,336 ft. per minute. The average difference for the twelve hours is one-half as great. "Rapid rotation, without friction or resistance, cannot in itself alone be regarded as a cause of light and heat;"[6] but we have found in our barometric investigations, that the ratio of the half-daily velocity of rotation to that which would be conferred by twelve hours' action of terrestrial gravity, is ·00109, which may be regarded as the modulus of heat-producing resistance. If we multiply the average difference of lift by the weight of the atmosphere and by the effective resistance, dividing the product by the ratio of specific atmospheric heat, and the number of foot-pounds raised by a unit of heat, we obtain $\frac{91168 \times 15 \times \cdot 00109}{770 \times \cdot 25} = 7\cdot 74°$ F. as the amount of heat communicated to the air by rotation between midnight and noon, and abstracted between noon and midnight.

The theoretical barometric lift is, as we have seen, ·00219 of the entire weight of the atmosphere. Estimating the height of the aërial column when reduced to uniform surface density, at 24,000 feet, the heat-producing disturbance that is indicated by the barometer is represented by a lift of 15 lb. on each square inch to a height of ·00219 × 24000 feet. The quarter-daily disturbance from this cause is, therefore, $\frac{24000 \times 15 \times \cdot 00219}{770 \times \cdot 25} =$ 4·1° F.

It is more than likely that each of these results will require important modifications when the entire influence of the several

[5] Tyndall, Heat considered as a Mode of Motion. N. Y. edit., p. 431.
[6] Dr. J. R. Mayer.

conditions of the problem is better understood. I have thought it proper to present them in their present crudity, in order to show the true points of departure, and to prepare the way for some further considerations.

Whatever other heat-disturbing causes there may be, there can be little doubt that the three we have just been considering are the most important. Dividing the astronomical day into four quadrants, and representing the solar effect by S., rotation by R., and barometric by B., it will be readily seen that the several positive and negative influences must be distributed as follows:

| | S. | R. | B. |
|---|---|---|---|
| From $0^h$ to $6^h$, | + | − | − |
| " $6^h$ to $12^h$, | − | − | + |
| " $12^h$ to $18^h$, | − | + | − |
| " $18^h$ to $0^h$, | + | + | + |

The tables of average temperature at any given place would therefore furnish us with four equations for determining the value of each of the disturbing elements, providing those that are unknown were so insignificant as to be safely neglected. The effects of these unknown disturbances are confined within certain limits that can be pretty satisfactorily determined.

Our discussion of the barometric fluctuations demonstrated a tendency of inertia to retard the effects of rotation, so that the mean daily altitudes are found nearer to $1^h$, $7^h$, $13^h$, and $19^h$, than to $0^h$, $6^h$, $12^h$, and $18^h$. A like tendency is discernible in the thermometer.

There are three, and only three, quadrantal divisions of the day, commencing respectively at $0^h$, at $1^h$, and at $2^h$, for which we could obtain approximate positive values of S., R., and B. The maximum solar effect is deduced from the first, and the minimum from the third of these divisions; while the maximum rotative and barometric effects are exhibited in the third, and the minimum in the first division.

The nearest average temperatures are found in the third division, as is shown below.

Average of temperature at $2^h$, $8^h$, $14^h$, and $20^h$, and of the entire day.

| Station. | Mean of the four hours. | Daily mean. |
|---|---|---|
| | ° | ° |
| At Girard College,......... | 52·1 | 52·1 |
| At St. Helena,........... | 61·65 | 61·69 |

The following table presents all the coördinate positive values of S., R., and B., that can be obtained from the Girard College and St. Helena means.

| Station. | Dividing at | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 0, 6, 12, 18h. | | | 1, 7, 13, 19h. | | | 2, 8, 14, 20h. | | |
| | S. | R. | B. | S. | R. | B. | S. | R. | B. |
| | Per ct. | Per ct. | Per ct. | Per ct. | Per ct. | Per ct. | Per ct. | Per ct. | Per ct. |
| Girard College, | 45·92 | 41·32 | 12·76 | 31·3 | 49·5 | 19·2 | 13·8 | 63·2 | 23·0 |
| St. Helena, | 25·97 | 42·96 | 31·07 | 15·8 | 46·7 | 37·5 | 5·6 | 56·6 | 37·8 |

The percentages of the calculated values correspond very nearly with the means of the earliest Girard College and St. Helena values.

| | Calculated values. | Percentage. | Mean Percentage. | Limits. | |
|---|---|---|---|---|---|
| S., - - - - - | 7°·12 | 37·6 | 35·95 | 5·6 | 45·92 |
| R., - - - - - | 7°·74 | 40·8 | 42·14 | 40·8 | 63·2 |
| B., - - - - - | 4°·1 | 21·6 | 21·91 | 12·76 | 37·8 |

It may be inferred from this comparison that the rotation element of daily heat is least affected, and the solar element most affected, by extraneous causes (of which moisture is probably the chief); that the first division gives the best, and the third division the poorest, results; that the proportion of thermometric variation which is attributable to rotation is between ·4 and ·5 of the average total daily variation, and that the most difficult element to determine satisfactorily is S., which is modified by many local disturbing influences, such as the nature of the soil, amount of vapor, clouds, altitude of the sun, &c.

**Philadelphia, May, 1864.**

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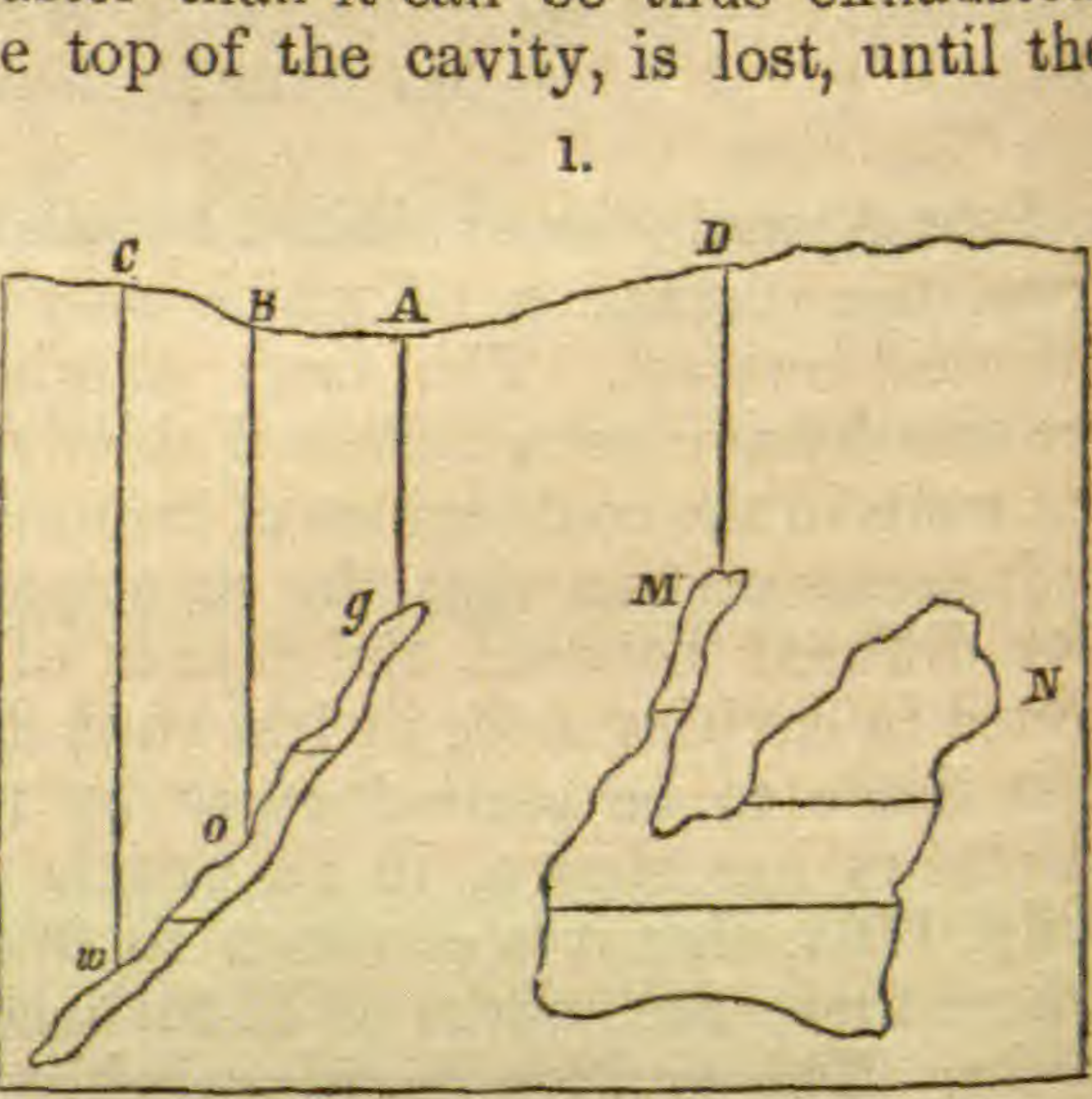
## Art. XIII.—*On the Action of Oil-Wells;* by Prof. E. W. Evans, Marietta College.

The phenomena exhibited by oil-wells suggest various problems, the discussion of which may be of scientific as well as practical interest. The facts on which the following remarks are based have been collected chiefly from the history of different wells in the coal regions of Southern Ohio and West Virginia.

It seems certain that the principal supplies of petroleum are not diffused between the planes of stratification, but are collected in cavities more or less sunken in the strata, whence it is less liable to be carried away by running water. Prof. E. B. Andrews has shown, in an article published in this Journal, July, 1861, that it is common to find large quantities in places where there are marks of disturbance and displacement of the rocks. The cavities have probably been caused sometimes by uplifts and sometimes by erosion and the dissolving action of water; but whatever may be their origin, they are not usually of great horizontal extent. It is seldom that two neighboring wells strike oil at the same depth, whether the strata be horizontal or dipping. It is one chance out of many to strike oil at all, even in neighborhoods where it exists in abundance. The drill, as it enters the cavity, sinks variously from four or five inches to as many feet, sometimes sticking fast, as if between the oblique sides of a narrow fissure. But there are facts connected with the history of oil-wells, particularly their intermittent action and their interference with one another, which serve to show the existence, in many cases, of systems of these cavities connected together by channels of communication more or less free, running sometimes along the strata and sometimes across them. The productiveness of a well depends on its entering either one of the main reservoirs or some of its important connections.

Let us begin with the most simple case, that of a single or isolated oil-cavity; of which a cross section is represented by *gw*, fig. 1. Every collection of oil is accompanied with varying quantities of gas and water, the gas occupying of course the top of the cavity and the water the bottom, according to the order of their specific gravities. First suppose that a well is bored at A, so as to enter the gas. Being in a high state of tension the gas escapes, sometimes with explosive violence, carrying out with it whatever water there may be collected in the boring. If water enters the cavity freely, as is usually the case, the oil, floating on its surface, is soon driven upward to the mouth (i. e., lower end) of the tube; it may then be pumped out till the line of division between it and the water rises to the mouth of the tube; after which, mixed oil and water will be drawn. But it often happens that the water rises faster than it can be thus exhausted, and the oil, driven into the top of the cavity, is lost, until the water is reduced by machinery of greater working power. But as it cannot be reduced below the mouth of the tube, unmixed oil cannot again be obtained from the well. In all wells from which the gas has escaped, there is ultimately a saving of work if the oil is pumped out as rapidly as possible before the intrusion of water. Secondly, suppose that the boring is at B

1.




and enters the oil. In this case, the oil rises in the tube to a height depending on the tension of the gas above it; a mode of action which is illustrated by the familiar apparatus called the fountain with condensed air. Sometimes it is thrown into the air a distance of 30 or 40 feet, and large quantities wasted. If the oil continues to be ejected till its surface in the cavity descends to the mouth of the tube, the fact first becomes known by a gurgling and spurting action, and the gas, or the greater portion of it, escapes, after which the pump becomes necessary, and the same series of actions take place as in the first case. But if the gas reaches its equilibrium with the hydrostatic pressure before the oil is reduced so low, we may then pump out the oil till the water rises to the mouth of the tube, after which we shall obtain mixed oil and water as before, till the whole supply of oil is exhausted, provided the pump is of sufficient working power to prevent interruptions by the too rapid rise of the water.

Next suppose that the boring is at C and enters the water. If the gas has sufficient tension, water is raised until its surface in the cavity descends to the mouth of the tube, then mixed oil and water is obtained, then pure oil, after which the same circumstances exist as in the second case. It must not be inferred, however, that when the water is not thrown to the surface there is no oil. It may happen that the pressure of the gas will raise a column of water only part of the way up the boring, and yet the well be found productive. Hence no considerable quantity of water should be passed without ascertaining by reducing it with the pump whether there is oil confined above it in some side chamber. The Shattuck well on the Little Kanawha had to be drained of water with a steam pump for two weeks before oil was obtained; but after that it yielded abundantly.

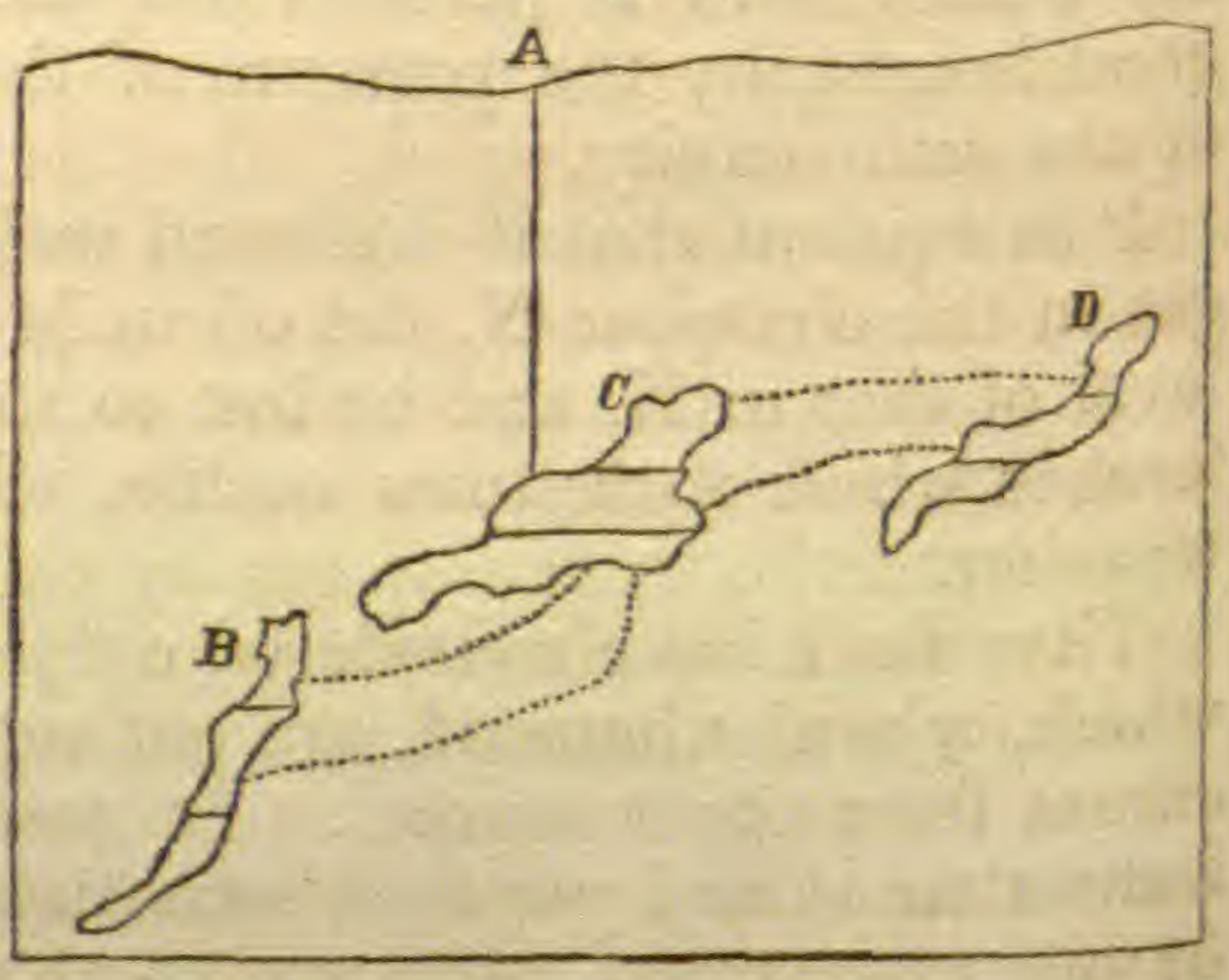
Some varieties of action are to be accounted for on the supposition that there are, in the same cavity, different collections of gas separated by a partition descending from the top. Such a cavity is represented by MN, fig. 1. A well enters the gas chamber M. The gas escapes with violence, and yet the oil immediately begins to flow in a continuous stream over the top of the boring, and is perhaps projected in the form of a jet to a great height, by the pressure of the gas in another chamber N, of the same cavity.

It is evident that if a second well be sunk so as to enter the gas in the chamber N, the oil in M will immediately sink to the level of that in N, and be lost to the first well; a mode of interference which sometimes occurs, when two wells are quite near together.

Thus far I have considered only isolated oil-cavities, or those which, when exhausted, are not replenished to any considerable extent from other sources. In general these run their course in a short time, and yet they sometimes yield very large quantities of oil.

There is a second class of wells, in general more productive, which exhibit the same phenomena at first, but as often as they are exhausted are replenished again, and repeat a certain series of actions indefinitely, and with remarkable regularity of time. This is to be explained by supposing that they are connected with other reservoirs by slight channels of communication, whose capacity for replenishing is less than that of the tube for exhausting. Let C, fig. 2, be an oil cavity having connections with two other cavities, B and D. Suppose that a well A enters the oil in C. After this well has thrown out oil, and perhaps afterward water, by force of the condensed gas, it comes to a stop. Then owing to the diminished tension of the gas in the enlarged space in C, the gas and oil in B and D force slight passages, represented by the dotted lines, into C, until the gas in this cavity again becomes sufficiently compressed to raise oil and water successively; after which the well comes to another stop until it is replenished with oil and gas as before; and the same process is repeated an indefinite number of times. The Newton well, on a branch of the Little Muskingum, a few miles from Marietta, repeats this process (with some escape of gas) at regular intervals of about half an hour, expelling about a barrel of oil each time. A note-worthy fact connected with this well is that when it stops it is necessary to pump out a little water in order to start it again; then the oil issues spontaneously. This is to be explained as follows. The pressure of the gas is not quite sufficient to raise the water to the surface; but the position of the mouth of the tube is such that a few strokes of the pump suffice to reduce the surface of the water in the cavity below that point. Now a column of oil will be raised by a given pressure so much higher than a column of water as its specific gravity is less. In this case it is raised not far from a fourth higher (the specific gravity of the oil being ·816); and the difference is sufficient to make it flow over the top of the tube. Examples of this kind are common.

2.




The well in the figure is represented as having but a few connections, sufficient perhaps for the purpose of illustration; but it is probable that these lines of slow communication are usually numerous: the gas and oil, like the water, forcing their way in through a multitude of pores and slight crevices, until a state of equilibrium is gradually reached or approximated to, as mercury forces its way in through the pores of wood into the exhausted receiver of an air-pump. Sometimes it happens that the cavity is filled with sediment of clay and sand by these little streams, and the well becomes inactive.

The class of wells here described may be distinguished from others as intermittent wells. The finding of one of these may be regarded as a certain sign that there are numbers of oil cavities near together in the same locality. Especially if it yields copiously for months in succession, as often happens, without any material diminution in quantity, or increase of the intervals between the successive yields, the rocks in its neighborhood may be presumed to contain rich supplies of oil that may be directly reached.

On Oil Creek in Pennsylvania the greatest quantities of oil are found in the same horizontal stratum of sandstone. It would seem that this rock is very porous, and perforated like a honey-comb with numerous cells and fissures containing petroleum. The history of many of the wells is as follows. When oil is entered, the gas begins to raise it up over the top of the boring, increasing gradually in force until it projects it into the air, often to a height of 40 or 50 feet, then alternately diminishing and increasing in force at regular intervals, but without any cessation in the flow for a long time. These variations in the force of the gas (the "breathings of the earth," as they are called,) are to be explained on the same principle as before, by supposing that as the tension of the gas is relaxed by the removal of oil, the gas and oil from other cavities around rush in through the pores and slight fissures till a certain maximum tension is reached, and the influx ceases; then by the expansion of the gas already in the chamber the oil continues to come up, but with a diminishing flow, until a relative vacuum is again created; after which the influx is renewed and gradually increases as at the beginning. These regular alternations vary in different wells from two or three times a day to as many times an hour; the intervals, however, gradually increasing in length as the supply of oil is diminished; unless, as sometimes happens, new communications are forced, and the well, deriving new supplies, starts off again with a new period. It often happens that the same well has two periods;—one of variation in the flow, and another of cessation, consequent on the escape of gas.

A more uniform flow may be secured by making the orifice at the mouth of the tube smaller. This is often desirable in order to prevent the escape of gas by the exhaustion of the oil in the cavity down to the bottom of the boring. Sometimes such a quantity will thus rush out, before the oil raised up by the water, closes the passage again, as not only to render the pump necessary after that to raise oil, but also to diminish materially the influx of oil from other cavities by reducing the pressure of the gas in them. Another expedient sometimes resorted to, when the spontaneous flow of oil becomes slight, is to stop up the boring till another "head of gas," as it is called, accumulates. But the stoppage should not be continued long; for instances are known where the gas has in consequence forced a way from its new channels in other directions, and found vent in other wells.

It is not an uncommon thing for intermittent wells to throw out at first 300 or 400 barrels a day, or to yield in all as much as 20,000 barrels. They sometimes run two or three years before exhaustion. The productiveness of the Lewellyn well on the Little Kanawha greatly exceeded these figures.

It is evident that if a second well were sunk so as to enter the cavity B or D, fig. 2, the well C would lose one portion of its supply of gas and oil, and be to this extent interfered with. Sometimes a very productive well thus cuts off the main supplies of a number of less considerable ones in its neighborhood, or, if the first sunk, it is itself tapped by them.

But some of the most marked cases of interference that are known, show the existence of a third class of oil cavities, connected with one another by perfectly free channels of communication, so that when the equilibrium between them is disturbed, it is immediately restored. Fig. 3 will serve to illustrate. A well A enters the cavity D finding oil. Another well B is bored so as to enter an open channel *g* between the two cavities D and E. This will drain oil from A; but if, as in the figure, its mouth is lower than that of A, it can be made a valuable auxiliary to it when the rising water drives the oil into the upper part of the cavity; for it can be used to reduce the water, and thus to keep the oil within reach of A.

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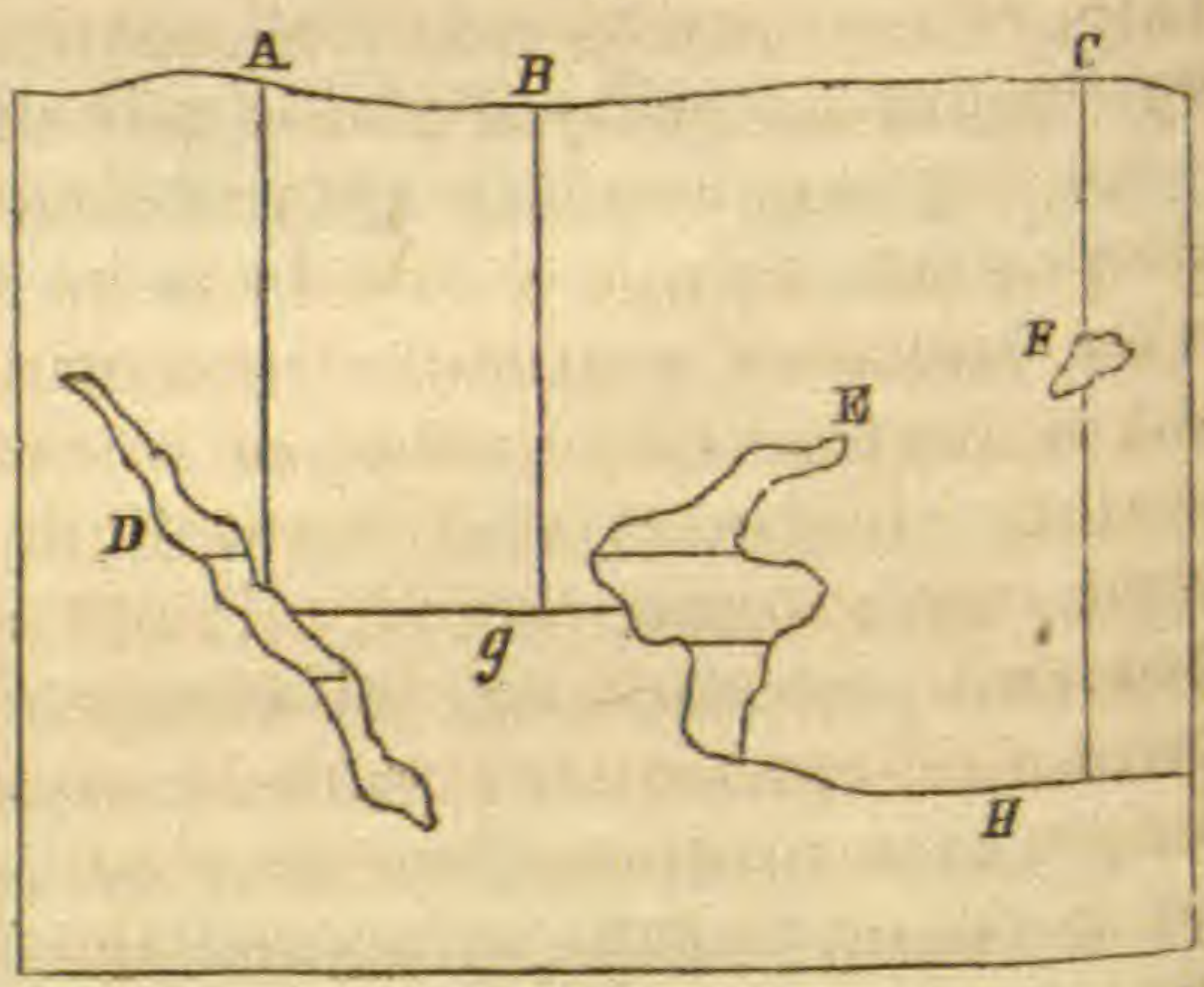


Again, a third well C is bored, and passes through a strong current of water, a cross section of which is represented by F. It finally descends to a fissure H, which communicates freely with E and consequently also with D, and interferes with both the other wells by letting in such a head of water as to drive the oil in both cavities above the mouths of the tubes. Pumping the water out of all these simultaneously might bring the oil down again within reach of that tube at least which enters at the highest point. A better expedient is to stop up tightly the space on the outside of the tube in the well C, just below the stream of water F. This is often effected by lowering a leather bag filled with dry seeds to the required depth. As water penetrates it, the seeds swell and close the passage.

On the Little Muskingum there are four or five wells (from 100 to 200 feet apart), so connected together as to illustrate both modes of interference shown by fig. 3. Had the well B entered the gas in E, it would have interfered with A by causing the escape of this gas; a case analogous to that mentioned before, where there were supposed to be two gas chambers in the top of the same cavity. After this the irruption of the water from C would have temporarily assisted B by raising the oil in E to the mouth of the tube.

Examples differing in details might be multiplied indefinitely. I have aimed only to point out in a general manner the different modes of action, and the hypotheses on which they are to be explained.

In the foregoing illustrations the quantity of gas has been supposed considerable. In many cases however it is so slight that the pump has to be used throughout. Yet wells of this kind often partake of the intermittent character to some extent. As it is not usual to work them at night, they begin each day with a new accumulation, which gives them a certain regularity of daily action often considered mysterious. There is a well a few miles from Marietta which yields oil only for a short time in the morning; when neglected till that time is passed, it is unproductive for the day. This is owing to the proximity of another well, which drains it of its water in the daytime, but by resting at night allows it to be replenished. Wells of small supply often require a certain interval of rest to be replenished, but never exceed a certain amount, however that interval may be extended—the column of oil having reached its maximum height by pneumatic or hydrostatic pressure.

Oil-wells commonly vary in depth from 100 to 800 feet. The deepest are as apt to raise oil to the surface as the shallowest. This indicates a greater compression of the gas at the greater depth, owing doubtless to its connection with higher columns of water. The activity of some wells is increased by rains; others, with less gas, are rendered unproductive till the water can be reduced. It must not be assumed however that their connection with subterranean currents is immediate and unobstructed. I know of no instance where there is reason to suppose that the oil is raised to the surface by the direct pressure of a stream of water whose head is higher than the issue, as the jets of Artesian wells are said to be produced. In spouting wells, the presence of gas as the immediate agent becomes known, not only from their variable action, but also from the actual escape of gas, and consequent cessation of flow whenever the oil is reduced to a certain level. If collections of oil had direct and free connection with strong currents of water, the mechanical agency of these currents would bear them rapidly away.

As it is, minute quantities come to the surface with the springs, showing a very slow process of drainage. As an index of the location of oil-cavities this sign is not reliable; for that which issues may have been carried by the streamlets many miles from its source. Gas springs are less deceptive signs; for the gas, being more buoyant than the oil and not liable to be carried along by descending currents, is not likely to wander so far before it issues. But the "show of oil" increases in value as a sign with the depth at which it is found. Especially is the finding of large quantities of imprisoned gas, though no oil may be present, regarded as a good indication that there is oil near.

Marietta, May 4th, 1864.

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## Art. XIV.—*Description of a new Machine for Cataloguing and Charting Stars;* by G. W. Hough, A.M.

The progress of instrumental astronomy has been so rapid during the last half century, not only in the perfecting of the older instruments, but also in the invention of new methods of observation, that at the present time, in certain kinds of work, more observations can be made in one year than could formerly have been done in five.

In the year 1848, the application of electricity to the recording of Astronomical Observations was first suggested. This happily conceived idea soon resulted in the construction of Chronographs by various persons, by which the instant of transit of a star was accurately recorded in a legible and permanent manner. Success in the recording of one ordinate of a star's position would naturally suggest the possibility of fixing the other by the same agency. But with the exception of some experiments made by the late Prof. O. M. Mitchel for the recording of declinations by electricity, this subject, so far as I know, has not been undertaken by any other astronomer.

In the formation of catalogues of zone stars, astronomers have almost invariably used the Telescope in a fixed position, and, by means of a diaphragm or scale placed in the focus, determined the time of transit and difference of declination. In our method, the Telescope is moved in zenith distance, the amount of motion giving us the difference of declination.

This method of observing the difference of declination between two objects, by magnifying by mechanical means the angular motion of the Telescope, is due to the late Prof. O. M. Mitchel, who first put it in practical operation in the year 1849; the apparatus used for this purpose being called the "Declinometer," an account of which will be found elsewhere.

Perhaps nothing is more desirable at the present time than accurate Ecliptic Charts of all stars down to the 14th magnitude. We already have the Charts of Chacornac, Argelander and others, which are of great value in the search for Asteroids; but were these charts filled out, as it were, with stars of a higher order of magnitude, it would greatly enhance their value for this purpose.

All standard charts, that have heretofore been constructed, have been made by laying down the positions of the stars, as given by a catalogue previously formed. This, of course, is an extremely difficult and tedious task. It seemed to me that much time and labor might be saved, provided we could make an accurate map at the same time that we observed for exact positions. This result we have succeeded in accomplishing by means of easy and simple mechanism, a description of which will be given in this connection.

In the cataloguing of zone stars with the Olcott Meridian Circle, during the year 1862, I found it desirable to have some contrivance by which we could observe the same zone, star for star, on a subsequent night. In order that we may be understood, we add that the clamp arm for giving slow motion to the Telescope in zenith distance, is moved by a screw pressing against its lower end, one revolution of the screw being about 6′. That we might get more rapid motion, an extra cog wheel was made to drive one fastened to the screw. To the axis of this new wheel was attached two cylindrical pulleys, each carrying a small weight suspended by a cord wound on the surface of the pulley. The width of the zone was then regulated by the length of the end left free to move.

It was found that with this contrivance (although we had the play between the two cog wheels), we could follow the same zone with a deviation of less than 5″. Were the pulleys attached directly to the screw, we know the error would be still less. From this fact, we were led to surmise, that difference of declination could easily be read to the tenth of a minute, from a screw head used for giving slow motion to the Telescope, in zenith distance.

In thinking on this subject, I conjectured that if a cylinder were attached to this screw, and a pen be made to move over it with a uniform velocity in the direction of its length, we could readily record both Right Ascension and Declination, or, in other words, make a map of the stars observed. Owing to inconvenience in attaching such an apparatus to our instrument, the plan was not put into execution.

I will now proceed to give a description of the Charting machine. Fig. 1 is a perspective of the machine, as seen from the southeast. This apparatus is firmly fastened to the south side of the west pier. It is connected with the clamp arm of the Telescope by means of the horizontal rod (*f*), 40 in. in length.

A clock work mechanism, having a half second's pendulum (*p*), carries the cylinder (*a*), 6 in. in length and 10 in. in diameter; which revolves from west to east, and makes a complete revolution every hour.

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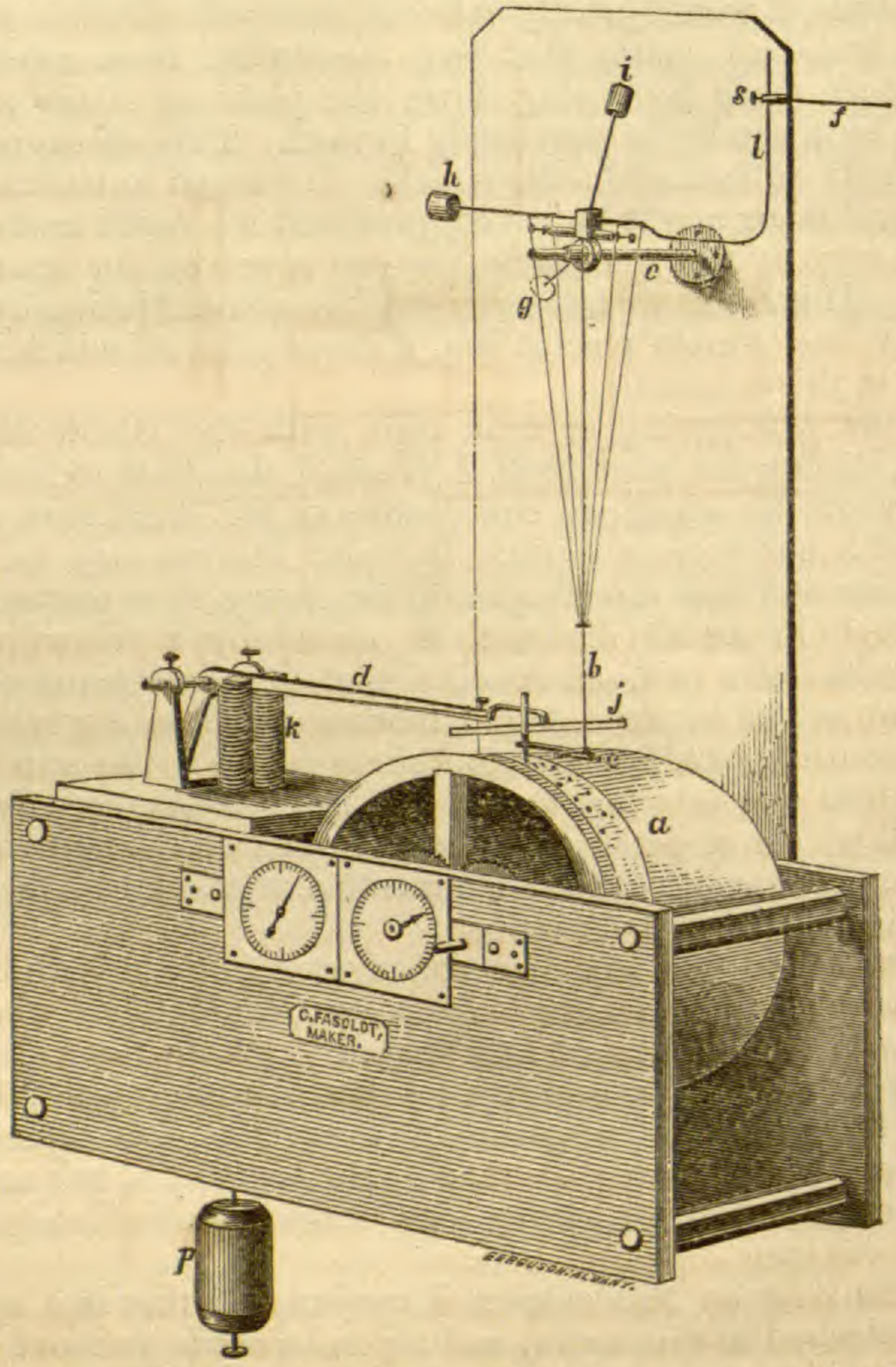


Directly over the cylinder is mounted, on a horizontal axis, the compound lever (*bl*); to the lower end of which, by means of a short horizontal arm and joint, a hollow cylindrical steel pen is held in a vertical position over the axis. The lower part of this lever (*b*) is 18 in. long; the upper part (*l*) is 6 in. long. In order to magnify as much as possible the angular motion of the clamp arm, we attach to it a strong iron bar, 25 in. in length. At the lower end of this bar is a cross piece, fig. 2, 6 in. long, holding a number of cylindrical pins (*p*).

Each of these pins has a notch cut in the middle, of the form which would result from placing the vertices of two cones together. By this arrangement, there can be no loss of motion; besides, it affords great facility in changing the rod from one pin to another.

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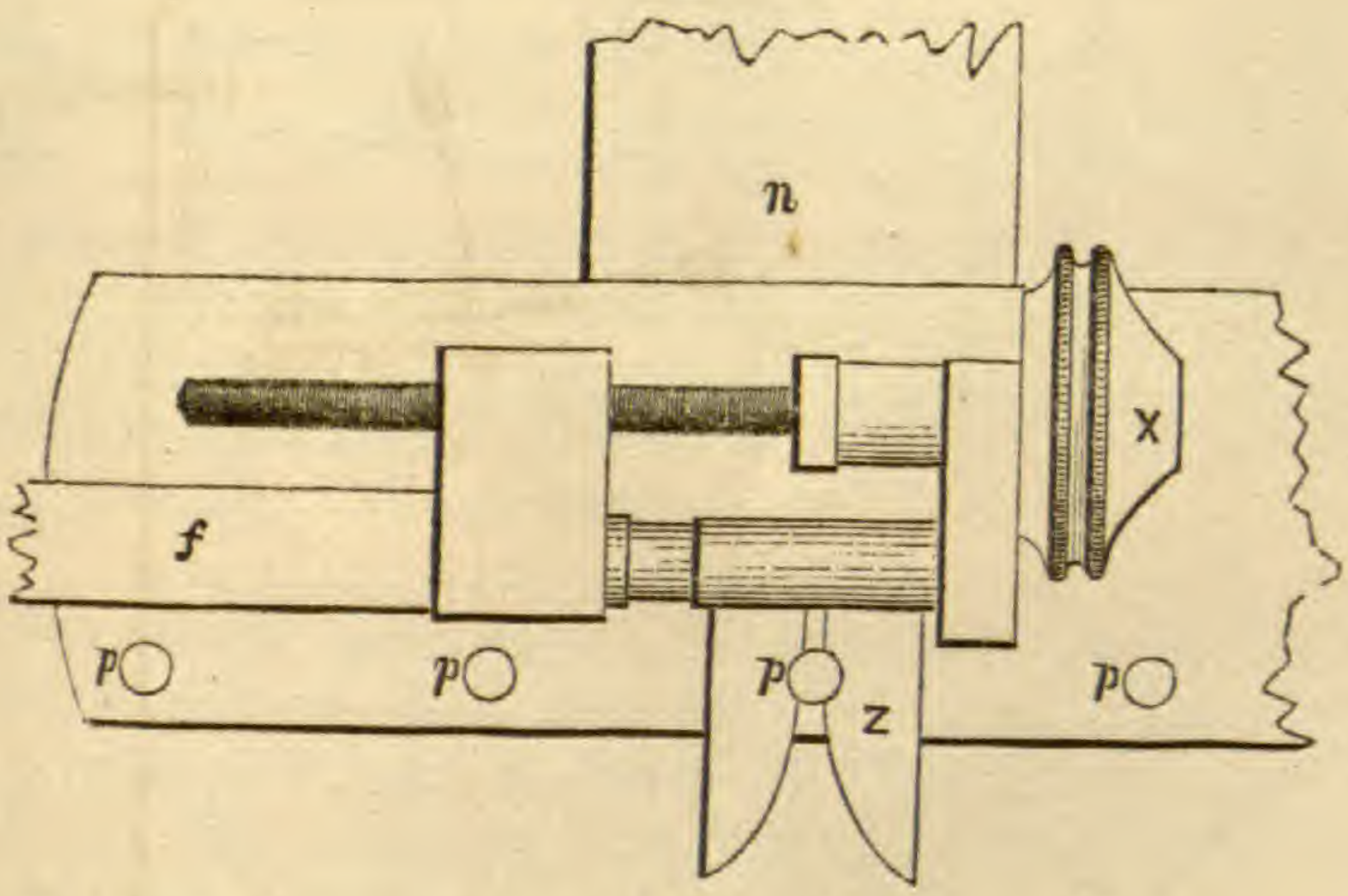
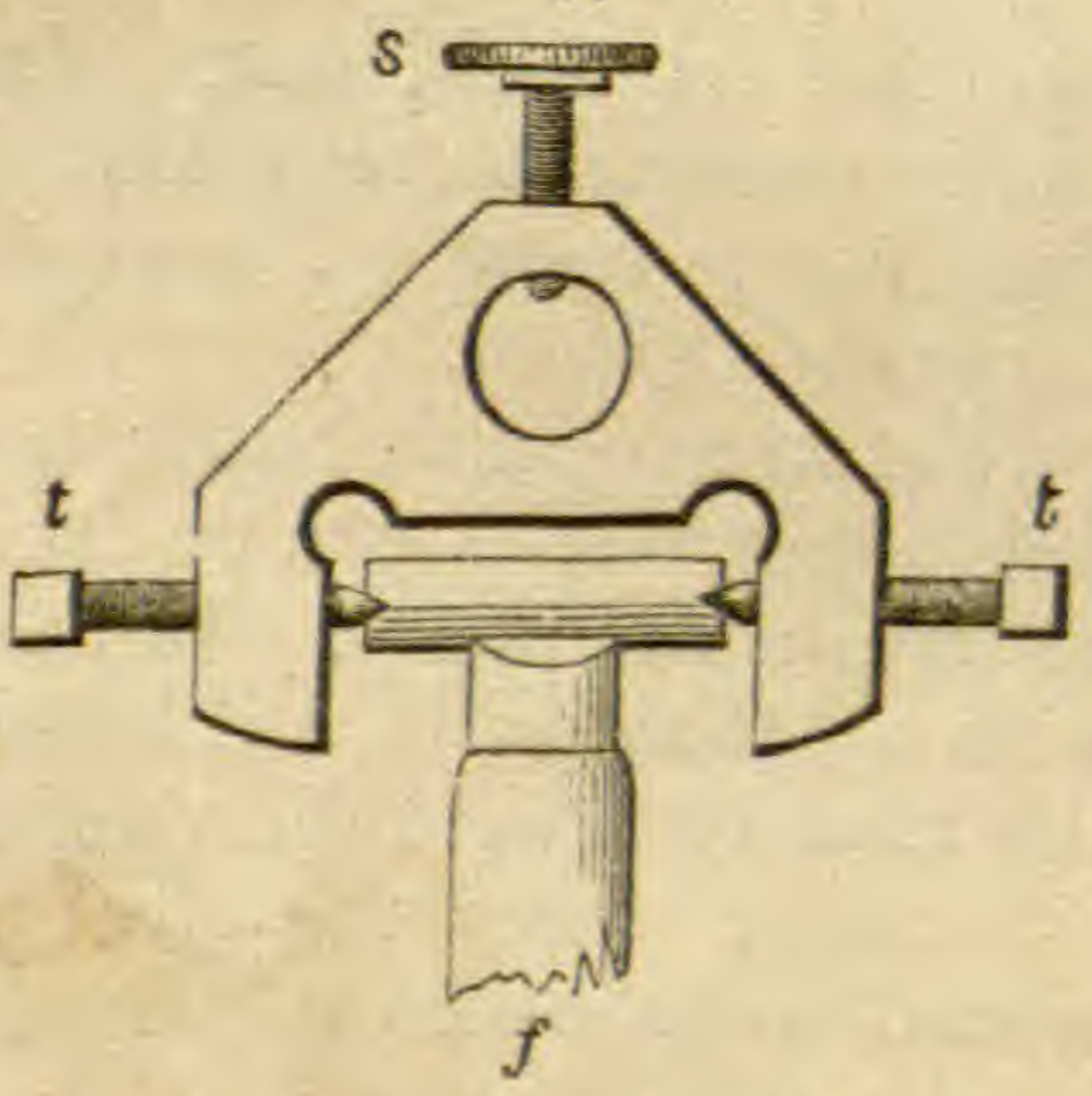



The rod (*f*) is connected with the clamp arm by dropping the notch (*z*), fig. 2, on one of the pins.

The other end of the rod is attached to the lever (*l*), fig. 1. A sectional view of the mechanism for this purpose is seen in fig. 3; (*t, t*) being two screws having conical points, (*s*) a set screw for clamping to (*l*).

3.




The arm carrying the steel pen, shown at (*c*), fig. 1, is attached to the lever (*b*), by an arrangement similar to that seen in fig. 3, with the exception that there is no joint or axis, the steel pen being held in position by a flat spring attached directly to the lever (*b*).

The lever (*b*) is supported on the horizontal post (*e*), fig. 1; *g* is a set screw for clamping to any part of the post (*e*); *h* and *i* are weights for counterpoising the lever in any position.

The supporting axis of *b* is seen in fig. 4, where *m m* are two screws having conical points. By this arrangement, we avoid all loss of motion, and have but very little friction.

In fig. 1, *k* is an electro-magnet operating the arm (*d*), at the end of which, and parallel to the axis of the cylinder, is attached the cross piece *j*.

The dials seen in fig. 1 indicate minutes and seconds.

Now, when the telescope is moved in zenith distance, motion is given to the steel pen so that it moves over the cylinder in the direction of its axis. Whenever we wish to make a record, a

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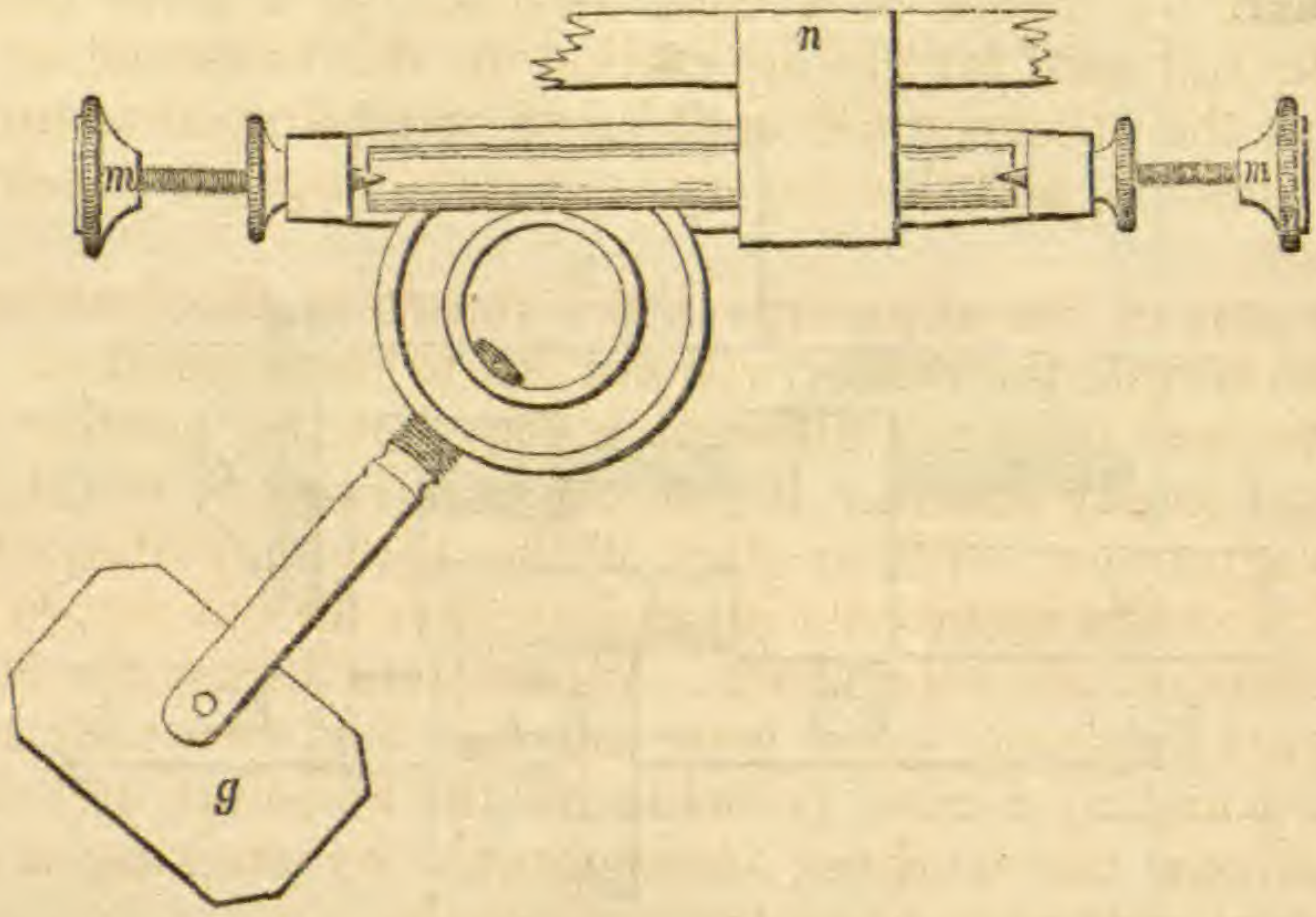


key is pressed which closes the circuit through the electro-magnet, and a blow is struck on the arm carrying the steel pen; so that a small dot is made on the sheet of paper covering the cylinder.

It remains now to show how the magnitudes of the stars are recorded on the Chart. Various plans were suggested, and I finally decided to represent the magnitudes by different colors. For this purpose we use prepared paper, known as duplicating impression paper.

If a strip of this paper be laid over a sheet of ordinary writing paper, and a pen be drawn over it, a colored impression will be left on the paper. In the same manner, if a blow be struck with a blunt point, a colored dot will be the result. Now, if at the time of observation an assistant should introduce a strip of this paper under the steel pen, when the record was made, we would have a colored impression denoting the magnitude. So, it is readily seen, should an assistant introduce these strips of paper as required, we should produce our chart with the magnitudes all recorded. But obviously this method would require an extra-assistant, and consequently be an unnecessary waste of time. It is then very desirable that the introduction of these strips of paper should be in the power of the observer himself.

Various kinds of apparatus might be employed to take the place of the assistant; but what we need is simplicity and certainty. We at first placed our strips on a belt running over two rollers, so that, by giving motion to these rollers, any strip of colored paper could be brought under the recording pen. This plan answered the purpose, but made it somewhat inconvenient to put our sheets on the cylinder. We therefore removed it and placed the strips of paper on an arm moving about a vertical axis; and by means of a cord attached to a lever connected with the rod used for giving motion to the telescope in zenith distance, the observer was enabled to bring any color under the pen he desired, and this too without removing his eye from the telescope.

This part of the apparatus is not shown in the drawings, but being so simple the reader will not fail to understand it. So far we have used only five different colors, but the number can be increased to any extent. These colors indicate 9, 10, 11, 12 and 13th magnitudes. When stars of the 14th magnitude are observed, no color is introduced, and we have for our record merely a puncture on our zone sheet. When stars below the 9th magnitude are observed, (and there are but few, generally three or four in a night,) a note is made by the assistant, and they are recorded on the working chronograph by striking a certain number of dots to indicate the magnitude.

As fast as the stars enter the field of the telescope, they are brought to the intersection of a horizontal and vertical wire, when, the circuit being closed, the record is made. In this way, the position of the stars in the heavens are transferred to the surface of the cylinder, so that when our observations are finished, we have a perfect "*fac simile*" copy of the zone of stars observed.

This apparatus we believe is the first which has been constructed to record accurately, by mechanical means, the Right Ascension and Declination at the same instant, or in other words, to make a chart of the stars observed. When the dot is made on the cylinder, a record is also made on the working chronograph, which gives us the time to the hundredth part of a second. For the exact declination, an assistant reads the declinometer scale to the five-tenths of a second. Therefore, when our zone is observed, we have not only a complete catalogue of the exact positions of the stars, but also a perfect map of the heavens.

In case we do not read our declinometer scale, we can determine the declination from the chart, within one-tenth of a minute of arc. The precision with which this machine will map stars is all that could be desired; since if two charts of the same zone, made on different nights, be placed one over the other, the stars will be superimposed so that the eye can detect no difference.

By means of movable adjustments, we can set the machine (having our sheet ruled for Right Ascension and Declination) so that it will give the position of the zone, at the beginning of the year, without sensible error. For adjusting in Right Ascension, the cylinder can be moved about its axis, being held in position by a friction block. For the declination, we lengthen or shorten the rod ($f$) by means of the screw ($x$), fig. 2. The scale for declination can be varied at pleasure, by changing the position of the connecting rod ($f$) on the lever $l$, fig. 1. This apparatus can be adapted to any telescope, either transit or equatorial; neither does its use interfere with the ordinary work for exact positions.

In the observation of asteroids on the meridian, a great deal of time is wasted, especially when the error of the ephemeris is considerable. And even when the error is only 2′ or 3′ in declination, in certain portions of the heavens, it is almost impossible to find the body with a meridian instrument.

This apparatus affords great facility in finding these bodies, when we have an approximate Ephemeris; since it is only necessary to observe, on two nights, a short zone of five minutes in Right Ascension and 10 minutes in declination. The comparison of these two charts will at once show which is the planet, provided it is included within those limits; when, the Ephemeris being corrected, it can be observed on the meridian in the usual way. This has already been tested in finding some of the old asteroids, using for our Ephemerides Hind's Supplement to the Nautical Almanac.

In our ordinary work, as we observe all stars visible, the limit being 13-14 magnitude, it is usually impracticable to observe a zone of greater width than 10′ or 12′; and within these limits, it is not unusual to find more than 200 stars in one hour of Right Ascension.

In case we wish to extend our observations over more than one hour of Right Ascension, we loosen the clamp screw ($g$), fig. 1, and slide the whole apparatus carrying the pen, on the post $e$; the end of the connecting rod ($f$) being raised up and dropped on another pin. These changes can all be made in less than one minute.

During the past year we have observed and charted six thousand stars, being the result of forty hours' observation. From the comparison of the positions given by the chart with those found with the declinometer, the mean error in the chart position, in declination, is found to be less than the one-tenth of a minute of arc. Our work with this apparatus so far has been almost entirely confined to cataloguing and charting a zone of stars lying between the equator and 10′ of south declination. In the prosecution of this work these charts have been found of great value in correcting doubtful observations, without the necessity of re-observing the zone. In case a wrong minute is entered by the assistant charged with taking down the declinometer readings and other remarks, on comparing with the chart we readily make the necessary correction. The use of this apparatus in no wise interferes with our work for exact positions, since we have found the mean error of our observations to be the same whether the stars were charted or not.

Our work for the past year has demonstrated the practical utility of this apparatus. If for any purpose we desire a map of the stars in a certain position of the heavens, we can make one in a few minutes, which by any other method would require hours. In the region of the milky-way, where small stars are very numerous, we have charted them at the rate of 480 per hour, and at the same time observed every star above the 14th magnitude.

Every one will at once see, that a series of charts, even in the condition in which they are taken from the cylinder, will be of great value to the observatory in which they are made. For, after being numbered and filed, they become so many maps, although the width does not exceed 10 minutes of arc. But obviously they can be made of greater service, with but little additional labor, by transferring contiguous zones to one sheet. This is easily accomplished by merely pricking through the paper, with a series of points which shall at once indicate the magnitudes.

In case we wish to search for Asteroids, we believe much labor can be saved, and equal if not greater facility afforded in their discovery. For, suppose we have already completed a series of charts for one hour of right ascension, and one degree declination, it is only necessary to observe and map the same zone, or any portion of it; when it is readily seen, should there be any asteroid in that region which was not there when the former charts were made, it will at once be detected.

The objection may be offered, that, with the ordinary meridian instruments, we do not have optical power sufficient to detect these faint bodies. Granting this to be the case, it does not affect the principle of the method, for we can use the apparatus with an equatorial of any size. In the latter case, we would clamp the telescope securely in the meridian, and, attaching an arm to the declination axis, at once connect our apparatus in the same manner as with the transit. Slow motion in declination can now be given to the telescope with the tangent screw, and the width of the zone limited by employing any mechanism suitable to the instrument. These minor details, of course, will be arranged by the observer, as circumstances require.

Dudley Observatory, March 16th, 1864.

ART. XV.—*Contributions to Lithology;* by T. STERRY HUNT, M.A., F.R.S.; of the Geol. Survey of Canada.

(Concluded from p. 104.)

DOLERITES.

The anorthosites, which yet remain to be described, may be divided into two groups, those composed of anorthic feldspars with augite, constituting the dolerites, and those in which similar feldspars are associated with hornblende. The general geognostical relations of these two groups of rocks in the districts under discussion have already been indicated.

*Grenville.*—It has already been stated on page 93, that the oldest known intrusive rocks which traverse the Laurentian series are of dolerite, and that the dikes of this rock are intersected by the syenite, which was succeeded by the orthophyre or quartziferous porphyry. Nothing corresponding to the syenite or the orthophyre is met with among the adjacent Lower Silurian strata, which are seen to repose upon the worn surfaces of these intrusive rocks. A fourth series of dikes, of a porphyritic dolerite, is however found to cut all the preceding rocks, and is perhaps identical with some of the dolerites which intersect the Silurian rocks of the island of Montreal. In the other parts of the Laurentian series, so far as yet examined, intrusive rocks have been but seldom met with. Much of what has been called syenite and granite in various parts of the Laurentian region, seems, like the hypersthenite and other anorthosites of the Labrador series to be indigenous.

The dikes of this most ancient dolerite or greenstone in Grenville, have a well marked columnar structure at right angles to the plane of the dike. They are fine grained, dark greenish-gray in color, and weather grayish-white. Under a lens, the rock is seen to consist of a greenish-white feldspar with a scaly fracture, mingled with grains of pyroxene, occasional plates of mica, and grains of pyrites. It contains no carbonates. Two analyses of portions of the dolerite, from dikes differing a little in texture, gave as follows under XV and XVI:

| | XV. | XVI. | XVII. |
|---|---|---|---|
| Silica, - - - - - - | 50·35 | 50·25 | 52·20 |
| Alumina, - - - - - - | 17·35 } | 32·10 | 18·50 } |
| Peroxyd of iron, - - - | 12·50 } | | [1]10·00 } |
| Lime, - - - - - - - | 10·19 | 9·63 | 7·34 |
| Magnesia, - - - - - | 4·93 | 5·04 | 4·17 |
| Potash, - - - - - - | ·69 | ·58 | 2·14 |
| Soda, - - - - - - | 2·28 | 2·12 | 2·41 |
| Volatile, - - - - - - | ·75 | 1·00 | 2·50 |
| | 99·04 | 100·72 | 99·26 |

[1] With some titanic acid.

The iron in these analyses, although given above as peroxyd, exists in the form of protoxyd, and in the second specimen, in part as a sulphuret. These rocks, which appear to have the composition of mixtures of a basic feldspar with pyroxene, do not differ from ordinary dolerite.

The newer dolerite, which cuts the three other classes of eruptive rocks in the Laurentian region, has a grayish-black, very fine-grained base, earthy and sub-conchoidal in fracture, and resembling somewhat the preceding. It contains small brilliant black grains of ilmenite, with others of sphene, and small scales of mica. Occasional masses of black cleavable augite, sometimes half an inch in diameter, give to the rock a porphyritic character. It contains besides, small cleavable masses of white carbonate of lime, with which the whole rock seems penetrated. When in powder it effervesces freely in the cold with dilute nitric acid, and the solution evolves red fumes on heating. In this way there were dissolved, lime, equal to 8·70 per cent of carbonate, 0·50 of magnesia, and 6·50 of alumina and oxyd of iron =15·70 per cent. The residue dried at 212° F., equalled 83·80 per cent. A portion of aluminous silicate had evidently been attacked by the acid. The dried residue gave on analysis the results which will be found above under XVII.

The dolerites of the Montreal district, besides forming numerous dikes, constitute the chief portions of the mountains of Montarville, Rougemont, and Mount Royal. In all of these however great diversities of composition are met with, which will be successively noticed.

*Montarville.*—The greater part of Montarville is composed of a coarse-grained granitoid dolerite, in which black cleavable augite predominates,—sometimes almost to the exclusion of any other mineral. Small portions of white feldspar, and scales of brown mica, are sparsely scattered through the rock, with grains of carbonate of lime. The removal of these by solution from the weathered surface often gives to it a pitted character. In other portions, the feldspathic element predominates, and the rock becomes porphyritic from the presence of large crystals of augite. The worn surfaces of the dolerite sometimes show alternations of this variety with another which is finer grained and whiter. The two are arranged in bands, whose varying thickness and curving lines suggest the notion that they have been produced by the flow and the partial commingling of two semi-fluid masses.

Another and a remarkable variety of dolerite, found at Montarville, appears to be confined to a hill on the shore of a little lake about half a mile northward from the manor house. The whole of this hill, with the exception of some adherent portions of indurated shale, seems to be composed of a granitoid dolerite, containing a large proportion of olivine. This mineral occurs in rounded crystalline masses or imperfect crystals from one tenth to one half an inch in diameter, associated with a white or greenish-white crystalline feldspar, black augite, a little brown mica, and magnetite.

The proportion of olivine is very variable, but in some parts it is the predominant mineral. Its color is olive-green, passing into amber-yellow. The grains, which are translucent, are much fissured and very brittle. The pulverized olivine gelatinizes with chlorhydric acid in the cold, and is almost instantly decomposed when warmed with sulphuric acid diluted with its volume of water, the silica separating chiefly in a flocculent form, and enclosing small grains of the undecomposed mineral, which are left when the ignited silica is dissolved by a solution of soda. A little silica is however retained in solution, and is precipitated by ammonia with the oxyd of iron. Two analyses of different portions of the olivine made in this way gave, after deducting the undecomposed mineral, the following results:

| | | | |
|---|---|---|---|
| Silica - - - - - - | 37·13 | 37·17 = oxygen | 19·82 |
| Magnesia, - - - - - - | 39·36 | 39·68 = " | 15·87 |
| Protoxyd of iron, - - - | 22·57 | 22·54 = " | 5·10 |
| | 99·06 | 99·39 | |

The augite of this olivinitic dolerite appears in the form of small crystalline grains, and also in short thick and terminated prisms, which are readily detached from their matrix. They are often an inch in length by half an inch in diameter, and are sometimes partially coated by a film of brown mica. These crystals cleave readily, presenting brilliant surfaces, and are black in color, with an ash-gray streak. Their hardness is 6·0, and their specific gravity 3·34. Analysis gave as follows:

| | |
|---|---|
| Silica, - - - - - - - - - - - - - | 49·40 |
| Alumina, - - - - - - - - - - - - | 6·70 |
| Lime, - - - - - - - - - - - - | 21·88 |
| Magnesia, - - - - - - - - - - - | 13·06 |
| Protoxyd of iron, - - - - - - . - - | 7·83 |
| Soda and traces of potash, - - - - - - - - | ·74 |
| Volatile, - - - - - - - - - - - - | ·50 |
| | 100·11 |

The augite which abounds in the non-olivinitic dolerite, which forms the greater part of Montarville, does not appear to differ from that just described.

An average specimen of this olivinitic dolerite, or peridotite, was reduced to powder; it did not effervesce with nitric acid, and when ignited lost only 0·5 per cent. When gently warmed with sulphuric acid, the olivine was readily decomposed, with the separation of flocculent silica; and by the subsequent use of a dilute solution of soda, followed by chlorhydric acid, and a second treatment with the alkaline ley, 55·0 per cent of the whole were dissolved. This portion consisted of silica 37·30, magnesia 33·50, protoxyd of iron 26·20, alumina 3·00=100·00; being equal to 18·4 of magnesia for the entire mass. In another experiment, 18·0 per cent were obtained. Taking the mean of the two analyses of olivine above referred to, which gives 39·5 per cent of magnesia, 18·0 parts of this base corresponds to 45·5 parts of olivine. The remaining 9·5 parts of dissolved matter represent alumina and silica from the feldspar, and oxyd of iron from the magnetite; both of which were somewhat attacked by the acids. The undissolved portion of the rock equalled 44·7 per cent, and appeared to consist of a feldspar with pyroxene, some mica, and a little magnetite. Its analysis afforded silica 49·35, alumina 18·92, protoxyd of iron 4·51, lime 18·36, magnesia, 6·36, loss (alkalies?) 2·50=100·00.

In some portions of the dolerite of Montarville the feldspar is more abundant, and appears in slender crystals with augite, and with a smaller proportion of olivine than the last. A specimen of this variety being crushed and washed gave 3·9 per cent of magnetite, and 10·0 per cent of a mixture of ilmenite with olivine. The feldspar was obtained nearly pure, in yellowish vitreous grains, having a specific gravity of 2·73—2·74, and nearly the composition of labradorite. The results of its analysis are seen under XVIII.

| | XVIII. | XIX. |
|---|---|---|
| Silica, | 53·10 | 53·60 |
| Alumina | 26·80 | 24·40 |
| Peroxyd of iron, | 1·35 | 4·60 |
| Lime, | 11·48 | 8·62 |
| Magnesia, | ·72 | ·86 |
| Potash, | ·71 | undet. |
| Soda, | 4·24 | " |
| Volatile, | ·60 | ·80 |
| | 99·00 | .... |

The dolerite of Montarville is traversed by veins belonging to several different periods. In one instance, the black and highly augitic mass is cut by a dike of a fine-grained grayish-white dolerite. This is intersected by a dike of a fine-grained greenish rock, which, in its turn, is cut off by another small dike which is grayish-white like the first.

*Rougemont.*—The rocks of Rougemont offer a general resemblance to those of Montarville. Some portions are a coarse-grained dolerite, in which augite greatly predominates, with grains of feldspar, and a little disseminated carbonate of lime. In some parts, the augite crystals are an inch or more in diameter, with brilliant cleavages; and grains of pyrites are abundant, with calcite in the interstices. This rock resembles the highly augitic dolerite of Montarville. Olivine is very abundant in two varieties of dolerite from Rougemont. One of these has a grayish-white finely granular feldspathic base, in which are disseminated black augite and amber-colored olivine, the latter sometimes in distinct crystals. The proportions of these elements sometimes vary in the same specimen, the feldspar forming more than half the mass in one part, while in another the augite and olivine predominate. By the action of the weather, the feldspar acquires an opaque white surface, upon which the black shining augite and the rusty-red decomposing olivine appear in strong contrast.

The dolerite of this mountain is traversed by numerous dikes, some of which are diorites like those of Monnoir and Belœil about to be described. A dike of compact dolerite, holding crystals of feldspar and grains of olivine is found intersecting the strata of the Hudson River formation at St. Hyacinthe.

*Mount Royal.*—This hill, which rises immediately in the rear of Montreal, consists for the most part of a mass of highly augitic dolerite. In some parts large crystals of augite, like those of Montarville, are disseminated through a fine-grained base, which is dark ash-gray in color, and often effervesces freely with acids from the presence of a portion of intermingled carbonate of lime. At other times this is wanting, and the rock is a mass of black crystalline augite, constituting a veritable pyroxenite, from which feldspar is absent. Mixtures of augite with feldspar are also met with, constituting a granitoid dolerite, in parts of which the feldspar predominates, giving rise to a light grayish rock. Portions of this are sometimes found limited on either side by bands of nearly pure black pyroxenite, giving at first sight an aspect of stratification. The bands of these two varieties are found curiously contorted and interrupted, and, as at Montarville, seem to have resulted from movements in a heterogeneous pasty mass, which have effected a partial blending of an augitic magma with another more feldspathic in its nature.

The more augitic parts of Mount Royal contain, like the similar varieties from Rougemont and Montarville, considerable portions of magnetite and some ilmenite. At the east end of the mountain a variety of dolerite containing olivine occurs. It consists of a base of grayish-white granular feldspar, which in the specimen examined constitutes about one half of the mass, and encloses crystals of brilliant black augite, and of semi-transparent amber-yellow olivine. This rock closely resembles the feldspathic peridotite of Rougemont, described above; but the imbedded crystals are somewhat larger, although less than those in the dolerite of Montarville. A portion of the feldspar, freed as much as possible from augite, furnished by analysis the result already given under XIX; which shows that it approaches labradorite in composition.

DIORITES.

*Yamaska.*—It now remains to describe the diorites which have already been noticed as forming several important masses among the intrusive rocks of the Montreal group. In the first place may be considered that of Yamaska. The greater part of this mountain consists, as already described, of a micaceous granitoid trachyte; but the southeastern portion is entirely different, being a diorite made up of a pearly white crystalline translucent feldspar, with black brilliant hornblende, ilmenite, and magnetic iron. This rock is sometimes rather fine-grained, though the elements are always very distinct to the naked eye. In other parts are seen large cleavage surfaces of feldspar half an inch in breadth, which exhibit in a very beautiful manner the striæ characteristic of the polysynthetic macles of the triclinic feldspars. The associated crystals of hornblende are always much smaller and less distinct, forming with grains of feldspar, a base, to which the larger feldspar crystals give a porphyritic aspect. Finer grained bands, in which magnetite and ilmenite predominate, traverse the coarser portions, often reticulating; and the whole mass is also occasionally cut by dikes of a whitish or brownish-gray trachytic rock; which are often porphyritic, and may perhaps be branches from the trachytic part of the mountain.

A portion of the coarse-grained diorite selected for examination contained, besides the minerals already enumerated, small portions of blackish mica, with grains of pyrites and a little disseminated carbonate of lime, which caused the mass to effervesce slightly with nitric acid. The macled feldspar crystals, sometimes half an inch in length, were so much penetrated by hornblende that they were not fit for analysis; but by crushing and washing the rock, a portion of the feldspar was obtained which did not effervesce with nitric acid, and contained no visible impurity, except a few scales of mica; its specific gravity was 2·756—2·763. It was decomposed by hydrochloric acid, with separation of pulverulent silica; and its analysis, which is given under XX. and XXI, shows it to be near to anorthite, and identical in composition with the feldspar of a diorite from Bogoslowsk, in the Ural mountains. This is associated with a greenish-black hornblende containing some titanic acid, with a little mica and some quartz. (R. H. Scott, *L. E. and D. Philos. Magazine*, [4], xv, 518.)

*Monnoir.*—Monnoir, or Mount Johnson, is composed of a diorite, which, in its general aspect, greatly resembles that of Yamaska just described, except that it is rather more feldspathic. The finer-grained varieties are grayish in color, and exhibit a mixture of grains and small crystals of feldspar, with hornblende, brown mica, and magnetite. Frequently, however, the rock is much more coarsely grained, consisting of feldspar grains, with slender prisms of black hornblende, often half an inch long and one-tenth of an inch broad, and numerous small crystals of amber-colored sphene. In this aggregate, there are imbedded cleavable masses of the feldspar, sometimes an inch long by half an inch in breadth. At the southern foot of the mountain, large blocks of the coarse grained diorite are found in a state of disintegration, affording detached crystals of feldspar with rounded angles, and weathered externally to an opaque white from a partial decomposition. Near to the base of the mountain, a coarse-grained variety of the diorite encloses small but distinct crystals of brown mica; and a fine-grained micaceous variety, containing sphene, occurs near the summit.

The feldspar, in all the specimens examined from this mountain, appears to be uniform in character. Its color is white, rarely greenish or grayish; it has a vitreous lustre, inclining to pearly, and it is somewhat translucent. The cleavages of this feldspar resemble those of oligoclase, with which species it also agrees in specific gravity and chemical composition. The macled forms, so common in the crystals of triclinic feldspars, have not however been detected in the specimens from this locality. A fragment of a crystal gave a density of 2·631, and another portion in powder 2·659. The results of its analysis are given under XXII. and XXIII.

| | XX. | XXI. | XXII. | XXIII. | XXIV. |
|---|---|---|---|---|---|
| Silica, | 46·90 | 47·00 | 62·05 | 62·10 | 58·30 |
| Alumina, | 31·10 | 32·65 | 22.60 | .... | 24·72 |
| Peroxyd of iron, | 1·35 | | 75 | .... | |
| Lime, | 16·07 | 15·90 | 3·96 | 3·69 | 5·42 |
| Magnesia, | ·65 | .... | .... | .... | ·91 |
| Potash, | ·58 | .... | 1·80 | .... | 2·74 |
| Soda, | 1·77 | .... | 7·95 | .... | 6·73 |
| Volatile, | 1·00 | .... | ·80 | .... | ·50 |
| | 99·42 | .... | 99·91 | ... | 99·32 |

*Belœil.*—The specimens which have been examined from this mountain consist of a kind of micaceous diorite. The feldspar, which so far predominates as to give a light gray color to the mass, is in white translucent vitreous cleavable grains; associated with small distinct prisms of black hornblende, scales of copper-colored mica, and grains of magnetite. The analysis of the feldspar, extracted by washing a portion of the crushed rock and still containing a little mica, is given above under XXIV. This result approaches to those obtained from the micaceous feldspar rock of Yamaska, V. and VI; which has been described as a kind of trachyte, and, with the rock of Belœil, seems to constitute a passage between the trachytes and diorites.

*Rigaud.*—A portion of Rigaud mountain consists of a rather coarsely grained diorite, which is made up of a crystalline feldspar, white or greenish in color, with small prisms of brilliant black hornblende, and crystals of black mica. In some specimens the feldspar, and in others the hornblende predominates. This rock resembles the diorites of Belœil and Monnoir.

The granitoid dolerites of the Montreal group, containing coarsely crystalline augite and olivine, break through the Lower Silurian strata; and portions of these two minerals, probably derived from these intrusive rocks, are found in the dolomitic conglomerates near Montreal, which in some cases include masses of Upper Silurian limestone, and are cut by dikes of a fine-grained dolerite. These, which perhaps correspond to the newer dikes of the same rock at Grenville, show that there were at least three distinct eruptions of dolerite,—one during the Silurian period, one before it, and another after it. The trachytes of Montreal and Chambly appear to be still more recent, and to traverse these newest dolerites.

The trachytes of Brome and Shefford seem to constitute a group apart; but the diorites of Yamaska and Mount Johnson, although similar in aspect, differ widely in chemical composition. Facts are still wanting to establish the geological age of these intrusive masses. The different dolerites, which are related in mineral composition, belong as we have seen to different geological periods; and it would not be safe to affirm that the different diorites or the different trachytes of this vicinity are contemporaneous. Nor, on the other hand, should even great discordances in chemical or mineralogical constitution be necessarily regarded as establishing a difference in the age of eruptive rocks. Evidence to the contrary of this is seen in the contiguous and intermingled masses of black pyroxenite and gray feldspathic dolerite in Mount Royal and Montarville; and it is not improbable that the olivinitic dolerite, which is associated with these, may be contemporaneous. If, as has been maintained in the first part of this paper, the various intrusive rocks are only displaced sediments of deeply-buried and probably unconformable strata, it will readily be conceived that plastic masses of very unlike characters may be ejected simultaneously along a line of disruption.

The various intrusive masses of the Montreal group which have here been described, appear, from their compact and crystalline structure, to have been displaced and consolidated under the pressure of a considerable mass of superincumbent strata. The fact that even their summits, which are in some cases more than 1000 feet above the present level of the plain, appear equally solid and crystalline with their bases, implies the removal by denudation, since the eruption of these masses, of a thickness of sedimentary strata much exceeding their present height. This denudation must however have taken place before the eruption of the later trachytes and dolerites; since the dolomitic conglomerates, which enclose the fragments both of the olivinitic dolerite and of Lower and Upper Silurian rocks, repose unconformably upon the Laurentian and the various Lower Silurian strata, in such a manner as to show that these offered nearly their present distribution at the epoch of the deposition of the conglomerates. If then, as is probable, the exposure by denudation of the whole of the eight hills which have been described, took place at one epoch, these are all shown to have a greater antiquity than the trachytes and the dolerites which traverse the conglomerates. The fine-grained and earthy trachytes of Montreal are consequently far more recent than the crystalline ones of Brome and Shefford; with which, however, some of them agree in chemical composition.

The general absence of granite from among these intrusive masses is a fact worthy of notice. Quartz has not yet been detected in the feldspathic rocks of Brome and Shefford; although, as above mentioned, the base of the feldspathic porphyries of Chambly, and of Shelburne, contains a slight excess of silica. The granitic rocks of Shipton, and of St. Joseph on the Chaudière, appear to be indigenous masses, belonging to the strata of the Quebec group; but the higher fossiliferous formations to the east of the Notre Dame mountains, are traversed in various places by veins and great masses of intrusive granite, as in Stanstead, Barford, and many other places to the northeast, and along the frontier of Canada. It is worthy of note that the intrusive masses on the two sides of the mountain range are, so far as yet observed, entirely distinct in character; and that eruptive rocks are generally wanting among the Notre Dame mountains, which consist chiefly of stratified rocks. It is also to be remarked, that the intrusive granites at their eastern base are not unlike, in mineralogical characters, to the indigenous granites of the mountains; thus suggesting the view that these are possibly the source of the intrusive granites which break through the Devonian strata. A similar relation has been pointed out by Durocher, in Scandinavia, where the Paleozoic strata are broken by intrusive masses of granite, orthophyre, zircon-syenite, and diorite. These rocks, according to him, are specifically analogous to those of the underlying primitive gneiss, but petrographically distinct. (*Bull. Soc. Géol. de France*, [2], vi, 33.) These facts are in accordance with the theory of eruptive rocks developed at the commencement of this paper; and it would be easy to extend the comparison to the intrusive diorites and dolerites about Montreal, and to show their resemblance with the stratified feldspathic rocks of the Labrador series. (*This Journal*, [2], xxix, 283, and xxxi, 414.)

## IV. Local Metamorphism.

In the second part of this paper I have asserted that the silicated minerals of crystalline rocks have a two-fold origin. In the first place, they may result from the molecular change of silicated sediments. These are either derived from the mechanical disintegration and partial decomposition of pre-existing silicates, or have been generated by chemical processes in waters at the earth's surface. In this way, steatite, serpentine, pyroxene, hornblende, chlorite, and in many cases garnet, epidote, and other silicates are formed by a crystallization and molecular re-arrangement of chemically-formed silicates, in a manner analogous to that in which mechanically-derived clays are converted into crystalline species. I have however pointed out that in the second place many of these silicated minerals may be generated by chemical reactions which take place among the mechanically mixed elements of sediments under the influence of heat aided by alkaline solutions. Both of these methods are involved in rock-metamorphism, and in the case of the local alteration of rocks by igneous masses, it is easy by comparative examinations to trace the chemical changes involved in the production of silicated minerals by the second method. In this way, Delesse has shown that in several cases, where the chalk of Ireland has been altered by the proximity of intrusive traps, the sand and clay which the former contains have been converted into calcareous silicates. (*Ann. des Mines*, [5], xii, pp. 189, 208, 212.)

An instructive example of this process is furnished at Montreal, where the bluish fossiliferous limestone of the Trenton group is traversed by dikes of dolerite, which are subordinate to the great intrusive mass of Mount Royal. The limestone for a distance of a foot or two is hardened, but retains its bluish tint. Within a few inches, it is changed to a greenish-white color, which is seen to be due to a granular mineral disseminated in the white carbonate of lime. The unaltered limestones from the vicinity contain variable amounts of insoluble argillaceous matters. A specimen treated with dilute chlorhydric acid, left a residue of about twelve per cent of a fine clayey substance, colored by a small amount of carbonaceous matter, and mixed with a little pyrites, which was removed by dilute nitric acid. This residue, after ignition, gave to a solution of carbonate of soda, 9·5 per cent of its weight of soluble silica; and the insoluble portion, being submitted to analysis, gave the result I. A portion of the limestone which was near to the intrusive rock, and was hardened and partially altered, was subjected to the action of dilute nitric acid, and gave an insoluble residue with the composition II. The more thoroughly altered greenish limestone was also treated with dilute nitric acid, which dissolved the carbonate of lime, and left a residue, the analyses of which, from two different portions of the rock, are given under III. and IV.

| | I. | II. | III. | IV. |
|---|---|---|---|---|
| Silica, - - - - - | 73·02 | 54·00 | 42·60 | 40·20 |
| Alumina, - - - - | 18·31 | 14·00 | 13·70 | 9·30 |
| Lime, - - - - - | ·93 | 16·24 | 31·69 | 36·40 |
| Magnesia, - - - - | ·87 | 5·27 | 4·17 | 3·70 |
| Protoxyd of iron, - - - | traces | 3·60 | 4·68 | 5·22 |
| Potash, - - - - | 5·55 | 3·14 | undet. | undet. |
| Soda, - - - - - | ·89 | 1·22 | " | " |
| Volatile, - - - - | .... | 90 | 1·20 | 1·20 |
| | 99·57 | 98·77 | 98·04 | 95·02 |

The residue from the unaltered limestone, including the silica soluble in alkalies, contains nearly 75·5 hundredths of silica, and 16·5 of alumina. These, in the vicinity of the dolerite, have become saturated with protoxyd bases, including the small portions of magnesia and of oxyd of iron which the limestone contains. This process evidently involves a decomposition of the carbonate of lime, and the expulsion of the carbonic acid. It is worthy of remark that while the unaltered limestone contains a little carbonate of magnesia, the rock from which III. was obtained yielded not a trace of magnesia to dilute nitric acid; II. marks an intermediate stage in the process, and shows moreover that the alkalies are still retained in combination with the aluminous silicate. These amorphous silicates, which have been formed by local metamorphism, might, under favorable circumstances, have crystallized in the forms of feldspar, scapolite, garnet, pyroxene, or some other of the silicious minerals which so often occur in metamorphic limestones. The agent in producing these silicates of protoxyds at the expense of the carbonates of the limestone, was probably a portion of alkaline salt, either derived from the feldspathic matter of the limestone, or possibly infiltrated from the contiguous feldspathic rock; whose elevated temperature produced the reaction which has resulted in thus altering this limestone.

Similar examples of local alteration are met with in several other places near to the intrusive rocks of the Montreal group. The schists of the Utica formation in contact with a dike of intrusive rock at Point St. Charles, and also near a mass of trachyte on a small island opposite the city of Montreal, occasionally exhibit small crystals of pyroxene, and in some cases prisms of hornblende. Among similarly altered shales at Rougemont are beds which consist of a highly ferriferous crystalline dolomite intermingled with dark-green cleavable hornblende, which forms thin layers, or in other cases encloses small rounded masses of the dolomite. (See for a description and analyses of this rock, the *Geology of Canada*, p. 634.)

At Montarville, the shales of the Hudson river formation are altered in the vicinity of the dolerite which forms the mass of the mountain. Some portions of the strata are very fine-grained, reddish-brown, and have an earthy sub-conchoidal fracture, with occasional cleavage joints. The hardness of this rock is not great, and it is apparently a kind of argillite, but between two beds of it is one of a harder coarse-grained rock, greenish-gray in color, and mottled with a lighter hue. This appears to be feldspathic in composition, and is penetrated in various directions by numerous slender prisms of black cleavable pyroxene, sometimes half an inch in length. The layers of sedimentation are distinctly marked in this bed, as well as in the finer grained strata which enclose it; and the whole affords an interesting example of the different effects of the same agency upon beds of unlike composition; although it would be impossible without comparative chemical analyses to determine whether the silicate which has here crystallized in the form of pyroxene, existed in the unaltered sediment, or whether, as in the case of the uncrystallized silicate from the altered limestone at Montreal, it had been generated under the influence of the intrusive rock. In by far the greater number of cases, the only apparent effect of the igneous rocks of the region under description upon the paleozoic limestones and shales, has been a very local induration; the appearance of crystals in these circumstances is a comparatively rare occurrence, and seems to depend upon conditions which are exceptional, showing, as I have elsewhere remarked, that heat and moisture are not the only condition of metamorphism. (*This Journal*, [2], xxxvi, 219.)

With these few examples of local metamorphism I conclude the present paper, proposing however to give in a subsequent one the results of some investigations of certain indigenous crystalline rocks.

Montreal, March 15, 1864.

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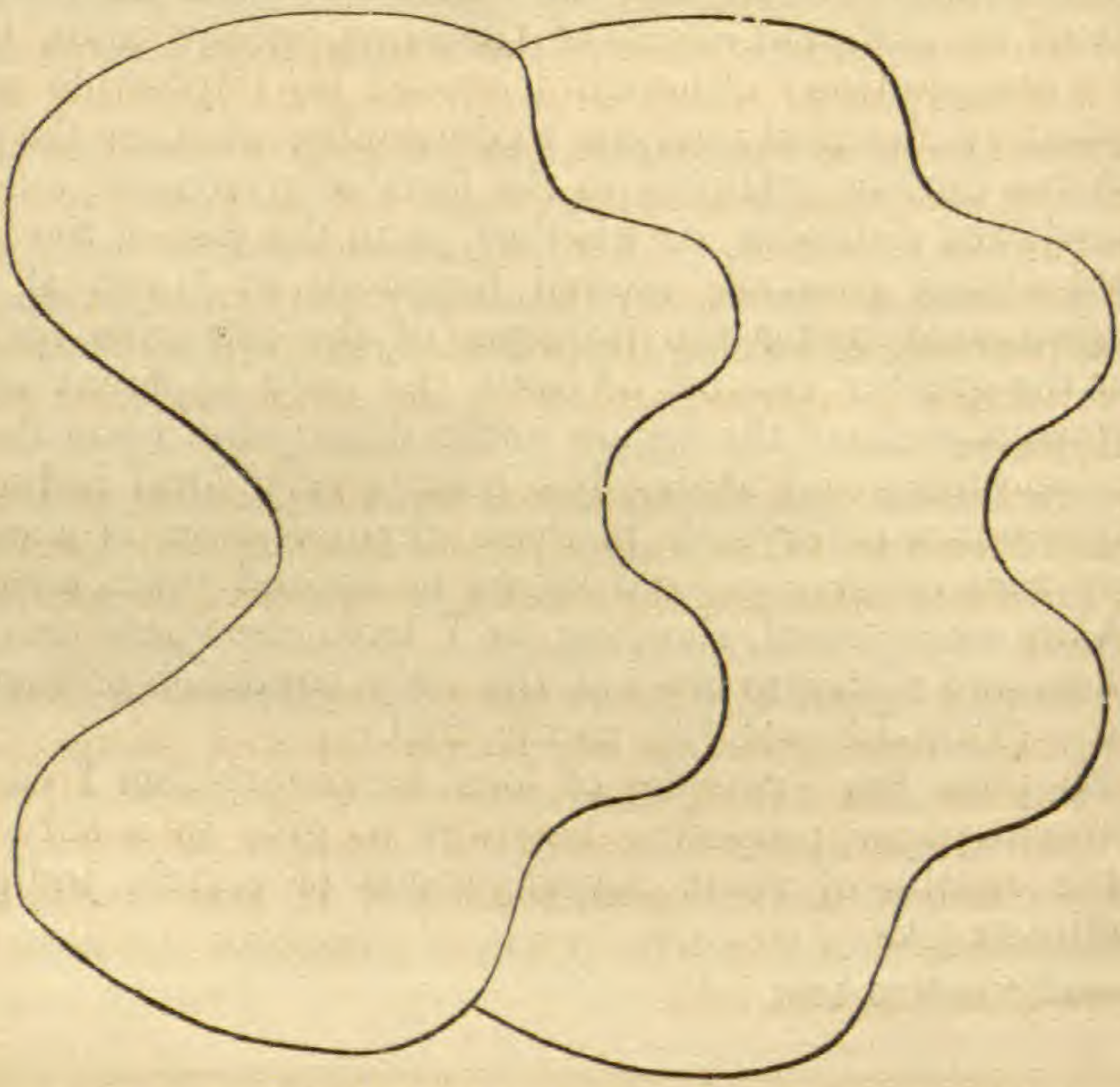
## ART. XVI.—*Description of a new species of Chiton;* by WILLIAM PRESCOTT, M.D., of Concord, N. H.

CHITON CALIFORNICUS.—The specimen of Chiton here described is in a dried state, having several years since been found cast upon the seashore at Santa Cruz Bay, California. It must, therefore, have been considerably larger when found than it now is. The color has also undergone a material change. It is said, when found, to have been of a uniform brilliant red: but now, the most prominent portion of the dorsal surface, (which would naturally be the first to dry,) is mainly of a *chesnut-red*, while the sides, together with some irregular blotches on the back, are of a dark brown, approaching in many places to black—occasioned, no doubt, by incipient putrefaction previous to becoming dry.

The whole length of the specimen is 6·8 inches; greatest breadth, 3·1 inches; greatest perpendicular height, 1·7 inches. The *coat of mail*, or shell-like covering, which gives shape and form to the whole animal, is ovate-oblong, convex above, considerably narrowed before and much wider posteriorly. It consists of eight testaceous pieces, or valves, which are imbricate (over-

1.



Outline of 4th and 5th valves of *C. Californicus*, natural size.

lapping each other), with the extremities of their anterior wings deeply imbedded and concealed beneath the skin or mantle. *Valves* smooth, destitute either of lines of growth or geometrical markings and not carinated. A convex tubercle or prominence occupies the centre of the dorsal portion of each, being most prominent on the 2nd, 3rd, 7th and 8th, much less so on the 4th and 5th, while on the 1st and 6th it is in the form of an elongated ridge—on the former longitudinally, on the latter diagonally. (The latter may be accidental, or the result of an injury.) On the 7th and 8th valves the form is more perfectly mastoidal and more distinctly pointed.

The first, or anterior valve, is externally in the form of an obtuse V, with four deep narrow fissures at its anterior inner margin. The posterior or 8th valve is rounded externally, with a small emargination or circular notch at its posterior margin.

The shape of the six other valves is papilioniform, or butterfly-shaped, neither keeled nor beaked; posterior margin lunated, being deeply and circularly arched between two wing-like projections which extend backward, their rounded extremities overlapping the valve immediately behind. The anterior wings, which are much larger than the posterior, are rounded, and project forward beneath the valve immediately before it, forming an arch much deeper than that formed by the posterior wings, but not so regularly circular.

The whole exterior surface of the mantle, from the margin upward, is thickly besprinkled with minute granulations which are of the same color as the mantle, most numerous on the sides and resembling shagreen, less numerous on the back, and least of all on the convex tubercles. There are also numerous indentations, which are most numerous on the convex portion or back, giving it the appearance of having the granules removed by friction.

The *margin* is narrow, and, with the whole inferior surface, is coriaceous.

The gigantic size of the specimen and the peculiar and unique form and structure of its valves render it extremely interesting, and a full description and figure of it very desirable and highly important.

The name of *gigas* at once suggested itself; but as that name had already been given to an African species (although one much smaller than this), the name of *Californicus* (from the State upon whose shores it was found) has been adopted.

It was obtained in California, some 8 or 10 years since, by the Rev. O. C. Baker, of this city, in whose possession it still remains.

Concord, N. H., March 20, 1863.

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ART. XVII.—*On the rising of Springs and Streams in California, before the winter rains;* by H. GIBBONS, M.D., of San Francisco.

IT is a subject of popular remark in this country, that the springs and small streams begin to rise a long time before the setting in of the rainy season, and before a drop of rain has fallen. The common notion is that the rise of the springs has some relation to the near approach of the rainy season. Although I had no doubt of the fact for several years, yet the demonstration of it did not occur to me until the autumn of 1858.

Two or three miles east of the Bay of San Francisco, and running parallel with the bay, is a range of hills about 1000 feet in height, the summits of which are naked, or sparsely occupied by oaks and redwoods. The springs and small streams, which abound on these hills in the winter and spring, mostly dry up in the course of the summer, though springs and swampy spots may be found here and there almost on the very summits through the whole course of the driest seasons. During the year 1858, I frequently traversed the hills by a road running in the trough of a crooked ravine, skirted by a pleasant little stream which started from a spring within two hundred yards of the summit and increased considerably in its descent. The dry season commenced rather earlier than common, not enough rain to lay the dust falling after the first week in April. My little rivulet continued to murmur refreshingly by the roadside until July, when it disappeared in places; and by the beginning of August it formed a chain of swampy spots and pools. At the end of August, when I expected to find it almost desiccated, judge of my surprise on encountering a brisk streamlet about a mile from the top of the hill, at a spot which had been perfectly dry. From that time it steadily increased, the pools being connected for the greater part by a continuous stream, by the 10th of October, though no rain had fallen.

Another instance still more striking has fallen under my notice. One mile from the foot of the hills, toward the bay, the county road is crossed by a small winter stream, never more than six feet wide, which became perfectly dry early in August. The channel of the stream is not more than two feet in depth. In the latter part of August, I was surprised to find in it a small pool of water, at the side of the road. On the fourth of September, the wet space extended some fifty feet. On the 9th of October, it had become a continuous, running stream, discharging five or six gallons per minute. And this happened without a drop of rain.

There is no great difficulty, I apprehend, in solving the phenomenon. The water which falls in the winter and spring penetrates the earth, and finds an impervious bed not far beneath the surface. This bed being more or less inclined, the water of course gravitates laterally, till it finds vent in the form of a spring. In the long days of summer, when the sun is fifteen hours above the horizon, and almost vertical, and when the atmosphere is very arid, evaporation is so rapid as to exhaust the supply at the springs and cause them to disappear; or at least to diminish the supply, and carry off entirely the water from the bed of the stream in the intervals of the springs. As the season advances, the days become shorter and the power of the sun also diminishes. Evaporation becomes proportionally slower, and at length it is so retarded as not to carry off the percolations of the springs; and the springs first reappear, and then the streams.

The diminishing evaporation after the summer solstice may be forcibly represented by figures. Thus, the time occupied by the process of evaporation, in proportion to that of influx or replenishment—or, in other words, the number of hours of sunshine and of night, in this latitude, is nearly as follows:

| | | | |
|---|---|---|---|
| June 20—sun 15h., | night 9h.; | or as | 10 : 6. |
| July 20— " 14h., | " 10h.; | " | 10 : 7·14. |
| Aug. 20— " 13h., | " 11h.; | " | 10 : 8·45. |
| Sept. 20— " 12h., | " 12h.; | " | 10 : 10. |
| Oct. 20— " 11h., | " 13h.; | " | 10 : 11·82. |
| Nov. 20— " 10h., | " 14h.; | " | 10 : 14. |

But this exhibits only the space of time occupied by the evaporating process. The greatly diminished power of the sun's rays in the autumnal months enhances the effect very materially. Besides, it is quite possible that, in the longest days, when the soil is most heated, a portion of the water in the strata supplying the springs is drawn directly upward by capillary attraction. This would be an additional source of exhaustion, which would cease or diminish with the advance of the season.

I have been informed by the late J. W. Osborne, Esq., of Napa, that it is very common for the springs and streams, after rising in the autumn, to disappear again before the rains set in. Though this fact has not come under my own observation, yet I can scarcely doubt it, the cause is so obvious. We very seldom have rains sufficient to penetrate a foot into the soil before December. And though the evaporation may be almost completely suspended, yet the supply in the strata furnishing the springs must in time be exhausted, and in that case the springs would again disappear.

It is worthy of remark that, on sinking wells, water is found near the surface, in most of the plains and valleys of California. Impervious beds of clay or rock appear very generally to underlie the superficial strata. If it were otherwise, the phenomena to which this paper refers might not be so conspicuous. There is a prominent feature of the scenery of our plains dependent on the same cause. In the autumn, after travelling five or ten miles without meeting with a drop of water, or any growing vegetation, you observe a clump of green willows in the bed of what appears to be a "dry creek." You find there a pool of water, and perhaps in the course of the bed other pools will be found, where a good supply of water is always on hand in the driest seasons.

ART. XVIII.—*Notes on the New Almaden Quicksilver Mines;* by B. SILLIMAN, Jr.

THE New Almaden quicksilver mines are situated on a range of hills subordinate to the main coast-range, the highest point of which at the place is 1200 to 1500 feet above the valley of San José. Southwest of the range which contains the quicksilver mines, the coast-range attains a considerable elevation, Mt. Bache, its highest point, being over 3800 feet in height.

New Almaden is approached by the railroad running from San Francisco to San José, a distance of 45 miles. In the course of it there is a rise of 100 feet, San José being of this elevation above the ocean. From San José to New Almaden the distance is 13 miles, with a gradual rise of 150 or perhaps 200 feet.

The rocks forming the subordinate range in which the quicksilver occurs, are chiefly magnesian schists, sometimes calcareous and rarely argillaceous. As a group they may be distinguished as steatitic, often passing into well characterized serpentine. Their geological age is not very definitely ascertained, but they are believed by the officers of the State Geological Survey to be not older than Cretaceous. But few fragments of fossils, and these very obscure, have yet been found in these metamorphic rocks. At a point just above the *dumps*, behind the reduction works at the hacienda (or village), there is an exposure, in which may be clearly seen in projecting lines the waving edges of contorted beds of steatite and serpentine, interspersed with ochery or ferruginous layers, more easily decomposed; and the partial removal of the latter has left the steatitic beds very prominent.

The mine is open at various points upon this subordinate range over a distance of 4 or 5 miles, in a northeast direction. The principal and the earliest workings of the mine were in a right line, but little more than a mile distant from the hacienda. The workings are approached, however, by a well-graded wagon-road, skirting the edges of the hills, which is 2⅜ miles in length.

It appears partly from tradition, and partly from the memory of persons now living, that the existence of cinnabar upon the hill was known for a long time prior to the discovery that it possessed any economic value. In fact, upon the very loftiest summit of this subordinate range, cinnabar came to the surface, and could be obtained by a slight excavation or even by breaking the rocks lying upon the surface. In looking about for physical evidences such as would aid the eyes of an experienced observer in detecting here the probable presence of valuable metallic deposits, one observes on the summit of the hill, at various points along the line of its axis for 2 or 3 miles, and also beyond, toward the place called Bull Run, occasional loose boulders of drusy quartz, with more or less well characterized geodes and combs; accompanying which is an ochraceous or ferruginous deposit, such as frequently forms the outcrop of metallic veins. There is, however, no such thing as a well characterized vein, the quartz and its associated metals occurring rather in isolated masses or bunches segregated out of the general mass of the metamorphic rocks, and connected with each other, if at all, somewhat obscurely by thread veins of the same mineral.

The main entrance to the mine at present is by a level about 800 feet long, and large enough to accommodate a full-sized railroad and cars. This level enters the hill about 300 feet from its summit, and is driven into a large chamber, formed by the removal of a great mass of cinnabar, leaving ample space for the hoisting and ventilating apparatus employed in working the mine.

At this point a vertical shaft descends to an additional depth of nearly 300 feet, over which is placed a steam "whim" with friction gearing and wire rope, worked by a steam engine, and by means of which all the ore from the various workings of the mine is conveniently discharged from the cars, which convey it out of the level to the dressing floors.

The first thing which strikes the observer on entering the mine is the liberal scale of its exploration. Every thing indicates a liberal and judicious use of capital in the development of a property which upon any other principle of exploration would probably have been unremunerative. We note also the absence of the usual galleries or levels, cut at regular distances of ten fathoms, common in the exploration, for example, of copper mines, and of other metallic deposits in which the ore is confined to well characterized veins.

In order to reach the lower workings of the mine, the observer may employ the bucket as a means of descent, or he may, in a more satisfactory manner, descend by a series of ladders and steps, not in the shaft, but placed in various large and irregular openings, dipping for the most part in the direction of the magnetic north, and at an angle of 30° to 35°. These cavities have been produced by the miner in extracting the metal, and are often of vast proportions; one of them measures 150 feet in length, 70 feet in breadth, and 40 feet in height—others are of smaller dimensions; and they communicate with each other sometimes by narrow passages, and at others by arched galleries cut through the unproductive serpentine.

Some portions of the mine are heavily timbered to sustain the roof from crushing, while in other places arches or columns are left in the rock for the same purpose.

The principal minerals associated with the cinnabar are quartz and calcareous spar, which usually occur together in sheets or strings, and in a majority of cases penetrate or subdivide the masses of cinnabar. Sometimes narrow threads of these minerals, accompanied by a minute coloration of cinnabar, serve as the only guide to the miner in re-discovering the metal when it has been lost in a former working.

Veins or plates of white massive magnesian rock and sheets of yellow ochre also accompany the metal. Iron pyrites is rarely found, and no mispickel was detected in any portion of the mine; running mercury is also rarely, *almost never*, seen.

The cinnabar occurs chiefly in two forms, a massive and a subcrystalline. The first is fine granular, or pulverulent, soft, and easily reduced to the condition of vermillion; the other is hard, more distinctly crystalline, compact and difficult to break; but in neither of these forms does it show any tendency to develop well formed crystals. It is occasionally seen veining the substance of greenish white or brown compact steatite or serpentine.

The ores are extracted by contract, the miners receiving a price dependent upon the greater or less facility with which the ore can be broken. By far the larger portion of the work people in the mines are Mexicans, who are found to be more adventurous than Cornishmen, and willing oftentimes to undertake jobs which the latter have abandoned. The price paid for the harder ores in the poorer portions of the mine is from $3 to $5 per cargo of 300 lbs. This weight is obtained after the ore is brought to the surface and freed by hand-breaking from the superfluous or unproductive rock; by this arrangement, the company are secured from paying for anything but productive mineral. All the small stuff and dirt formed by the working of the "labors," are also sent to the surface to form the adobes used in charging the furnaces.

It has often happened in the history of this mine, during the past fifteen years, that the mine for a time has appeared to be completely exhausted of ore. Such a condition of things has, however, always proved to be but temporary, and may always be avoided by well directed and energetic exploration. Upon projecting, by a careful survey, irregular and apparently disconnected chambers of the mine in its former workings in a section, there is easily seen to be a general conformity in the line of direction and mode of occurrence of the productive ore-masses. These are found to dip in a direction toward the north, in a plain parallel, for the most part, to the pitch of the hill, but at a somewhat higher angle. An intelligent comprehension of this general mode of structure has always served hitherto in guiding the mining superintendent in the discovery of new deposits of ore.

Since the settlement of the famous law-suit, which has so long held this company in a condition of doubt, the new parties, into whose hands the property has now passed, have commenced a series of energetic and well directed explorations at various points upon the hill, with a view to the discovery of additional deposits of ore. At one of these new openings, distant at least 500 feet from the limit of the old workings, and not more than 200 feet from the summit of the hill, a deposit of the richest description of the softer kind of cinnabar has been discovered, which, so far as hitherto explored, has a linear extent of at least 70 or 80 feet, and in point of richness has never been surpassed by any similar discovery in the past history of the mine. A charge of 101,000 pounds, of which 70,000 were composed of this rich ore, 31,000 pounds of "granza," or ordinary ore, and 48,000 pounds of adobes, worth 4 per cent, making a total charge of 105,800 pounds, yielded on the day of our visit, 460 flasks of mercury at 76½ pounds to the flask. This yield is almost without parallel in the history of the mine. The only preparation which the ores undergo, preparatory to reduction, consists of hand-breaking, or "cobbing," for the removal of the unproductive rock.

The small ores and dirt hoisted from the mine are made into "adobes," or sun-dried bricks, sufficient clay for the purpose being associated with the ore. The object of these "adobes" is to build up the mouths of the furnaces to sustain the load of richer ores. No flux is employed, there being sufficient lime associated with the ores to aid the decomposition of the sulphurets.

The furnaces are built entirely of brick, in dimensions capable of holding from 60,000 to 110,000 pounds, according to the character of the ores employed. The chambers are fired from a lateral furnace, fed with wood, and separated from the ore by a wall pierced with numerous openings by the omission of bricks for that purpose.

Connected with the furnace is a series of lofty and capacious chambers, also of masonry, through which the whole product of combustion is compelled to pass alternately above and below, from chamber to chamber, until all the available mercury is condensed. The draft from these furnaces is carried by inclined stacks up to the top of a lofty hill several hundred feet distant; and here the sulphurous acid and other effete products of the furnace are discharged. Formerly, no precautions were taken to prevent the escape of mercury through the foundations of the furnace to the earth beneath: now, the furnaces stand upon double arches of brick-work, and plates of iron are built into the foundations, so as to cut off entirely all descending particles of the metal and turn them inward. To be convinced of the importance of this precaution, it is sufficient to watch the operation of the furnace for a few moments, when an intermittent stream may be seen to flow into a reservoir provided for it, and which by the former process was completely lost in the earth.

On taking up the foundations of some of the old furnaces, within the last two years, the metal was found to have penetrated, or rather permeated, completely through the foundation and clay of the substructure down to the bed-rock beneath, a depth of not less than 25 or 30 feet. Over 2,000 flasks of mercury were thus recovered in a single year from the foundations of the two furnaces. This loss is entirely avoided by the improved construction which has been adopted.

The whole process of reduction is extremely simple, the time occupied from one charge to another being usually about seven days. The metal begins to run in from four to six hours after the fires are lighted, and in about sixty hours the process is completed. The metal is conducted through various condensing chambers by means of pipes of iron, to a "crane-neck," which discharges into capacious kettles. It undergoes no further preparation for market, being quite clean from all dross.

Deducting 2½ years, during which the mines were in a state of inactivity, pending the decision of the law-suit, the average monthly product for 12½ years has been not far from 2,500 flasks, of 76½ pounds each, of mercury. The selling price in San Francisco is, at present, and has been for some time past, 75c. per pound, while in London and New York it has ranged from 40 to 50c. per pound.

San Francisco, May, 1864.

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ART. XIX.—*Observations on a Seam of Coal;* by Prof. E. B. ANDREWS, Marietta College, Ohio.

THE seam of coal here described is located in the northern part of Washington Co., Ohio. It extends through the hills for several miles, and is generally of workable thickness. On Bear Creek, a small tributary of the Muskingum river, which latter stream is rendered navigable by large dams and locks, the coal is five feet in thickness. To the south the coal disappears, and evidence will presently be given to show that the original coal marsh was in that direction, skirted by open water. The geological position is near the top of the Coal Measures. I know of no other seam of coal of economic value above it in our Ohio rocks, although we find perhaps two hundred feet of unproductive sandstones and shales still higher. It is about sixty feet higher in the series than a peculiar group of limestone strata, with which is associated a seam of coal now worked at Coal Run on the Muskingum river. This group is very persistent, and, after showing itself over a considerable part of Washington

County, dips below the Ohio river to the northeast and reappears in the vicinity of Wheeling, W. Virginia, where its accompanying coal is extensively mined. In this connection it may be remarked, that there is great confusion among our geologists relative to the equivalency of the Pittsburg, Wheeling, and Pomeroy seams of coal. Dr. Caleb Briggs, who was connected with the early surveys of Western Virginia and Ohio, regards the three seams as equivalent; while Lesquereux makes the Pittsburg and Wheeling seams equivalent, to which he adds another, in a seam of coal at Athens, Ohio. My own investigations have led me to believe the Wheeling and Pomeroy seams to be entirely distinct, while the Athens coal is probably the continuation of the Pomeroy seam. The Athens coal is probably the same with the Federal Creek Coal, (Athens Co.) which, by all our Ohio geologists, is regarded as the continuation of the Pomeroy seam. The Pomeroy seam, as traced by myself, from Federal Creek into the northwestern part of Washington County, is found to be from sixty to seventy feet above the very limestone group which is so persistent, and which carries with it the Coal Run and Wheeling seam of coal. The Pomeroy seam sweeps around through the northern and eastern parts of Washington County, and is everywhere from fifty to seventy feet above the limestone group. Doubtless the seam of coal which is found in the hills above the Wheeling seam at Belleair (near Wheeling), is the equivalent of the Pomeroy coal.

The Bear Creek coal, which I propose to examine, is without doubt the geological equivalent of the Pomeroy seam. It is, however, greatly modified in structure by causes which I shall hereafter explain. Before the Bear Creek coal disappears to the south, there is evidence of a struggle in that direction between the vegetation and the water. The coal-marsh was repeatedly flooded, and over the vegetation thick deposits of sediment were made which are now fire-clay. We have these repeated alternations of coal and clay until, at last, the coal disappears, and we reach a region of unbroken water. There is nothing peculiar in the rocks associated with this coal.

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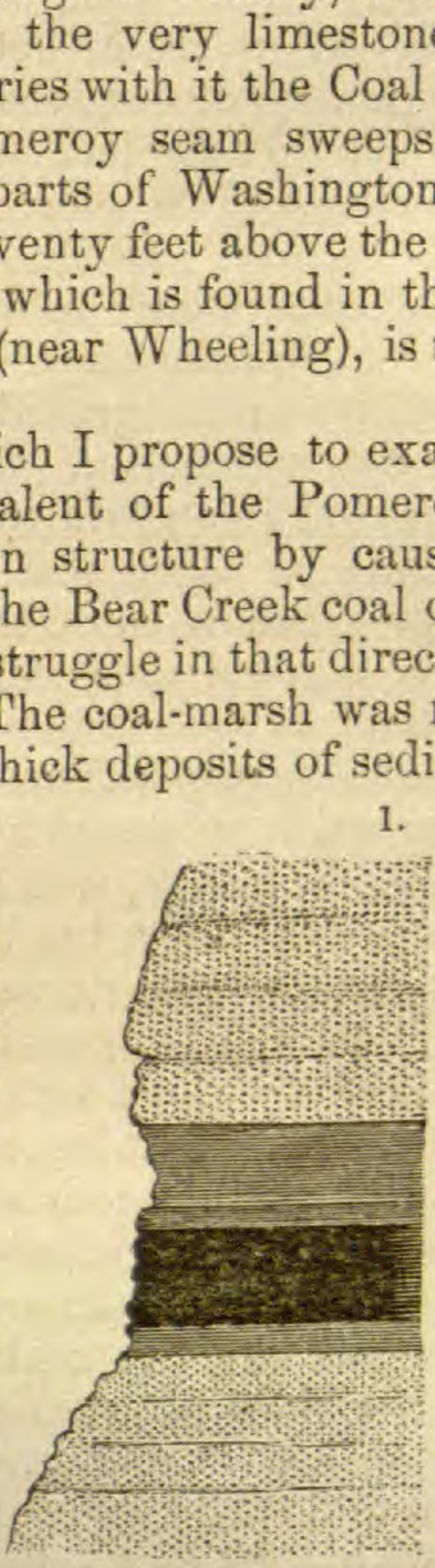


Above the coal is a thin stratum of bituminous slate. Above the slate several feet of fire-clay, and above the clay a heavy sandrock. Below the coal is fire-clay, and below that a considerable development of sandstones and shales.

The coal was analyzed by Dr. J. S. Newberry and gave:

| | |
|---|---|
| Volatile matter, | 49 to 51 per cent. |
| Fixed carbon, | 44 " 46 " " |
| Ash, | 3 " 5 " " |

Probably this percentage of ash is a little below the average for the whole seam. The coal is hard, and breaks with an unusual conchoidal fracture, resembling cannel coal. The appearance of the laminations as seen on the surface of the polished vertical planes, resembles varnish spread with a coarse brush, thus giving a peculiar streakiness to the surface. These laminations often show themseves to be immeasurably thin layers of sedimentary matter. In the fracture these layers present the appearance of exquisitely-fine steel medal-ruled engraving. It is doubtless to these peculiar laminæ that the coal owes the property of parting with remarkable readiness with its gas or hydro-carbons, for the coal is very rich in gas and burns with remarkable freedom. In the grate it differs from the Wheeling coal, as dry wood differs from wet green wood. Why the presence of these laminæ of sedimentary matter—thinner than any tissue paper—should thus facilitate the production of gas, a fact which I have observed in some cannel coals, has not been explained so far as I know. Besides these peculiar laminations, there are the usual ones, which most geologists regard as produced by the different increments of vegetable matter, and showing perhaps periodicity in the growth and falling of the leaves and fronds of the coal vegetation. The Pittsburg coal exhibits this class of laminations in smooth and highly polished horizontal surfaces, and it is by these that this coal is readily distinguished from other Western coals. In the Bear Creek coal we find also laminæ of mineral charcoal. This is sometimes found in plates one-fourth of an inch in thickness. It exhibits a fibrous, woody structure, and is very soft and easily reduced to powder. Whether this charcoal is produced by some peculiar property in the original vegetable matter, which did not permit the usual bituminization, or the bitumen has been subsequently removed by great pressure, leaving the fibres dry like the pressed stalks of sugar cane, it is perhaps impossible to determine. The first class of laminæ, those having a sedimentary appearance, doubtless tells a tale of water, and indicates that the vegetation grew where it was very marshy and constantly inundated. This water, however, contained only the slightest possible amount of impalpable sediment, and could scarcely have been much discolored. This marshy character of the coal field and the overflow of water doubtless caused an unusual softening and maceration of the fallen vegetable matter, and produced the cannel-like character of the coal. But while the coal marsh was ordinarily more or less covered with comparatively pure water, at one time it was flooded by more turbid water, which left a distinct earthy deposit. This thin slaty streak in the coal, now black and highly bituminous, is evenly distributed over a large area, and indicates a general overflow.

Another interesting feature of the Bear Creek coal is the presence, in various parts of the seam, of drifted and beach-worn wood. This is generally found in small fragments with the ends rounded, as if long water-tossed. Though now changed into sulphuret of iron, they show plainly a woody structure, and are entirely unlike the flat plates of sulphuret of iron which are formed within all coals by chemical affinity. I have found them showing the bark-markings of the original tree.

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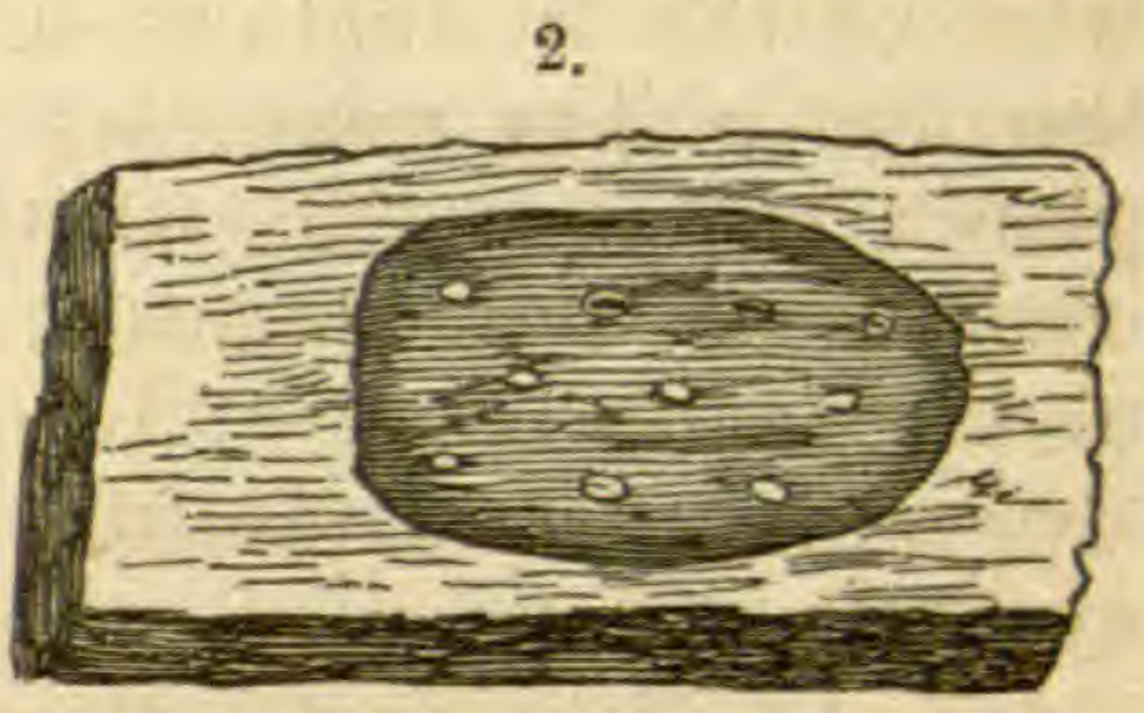
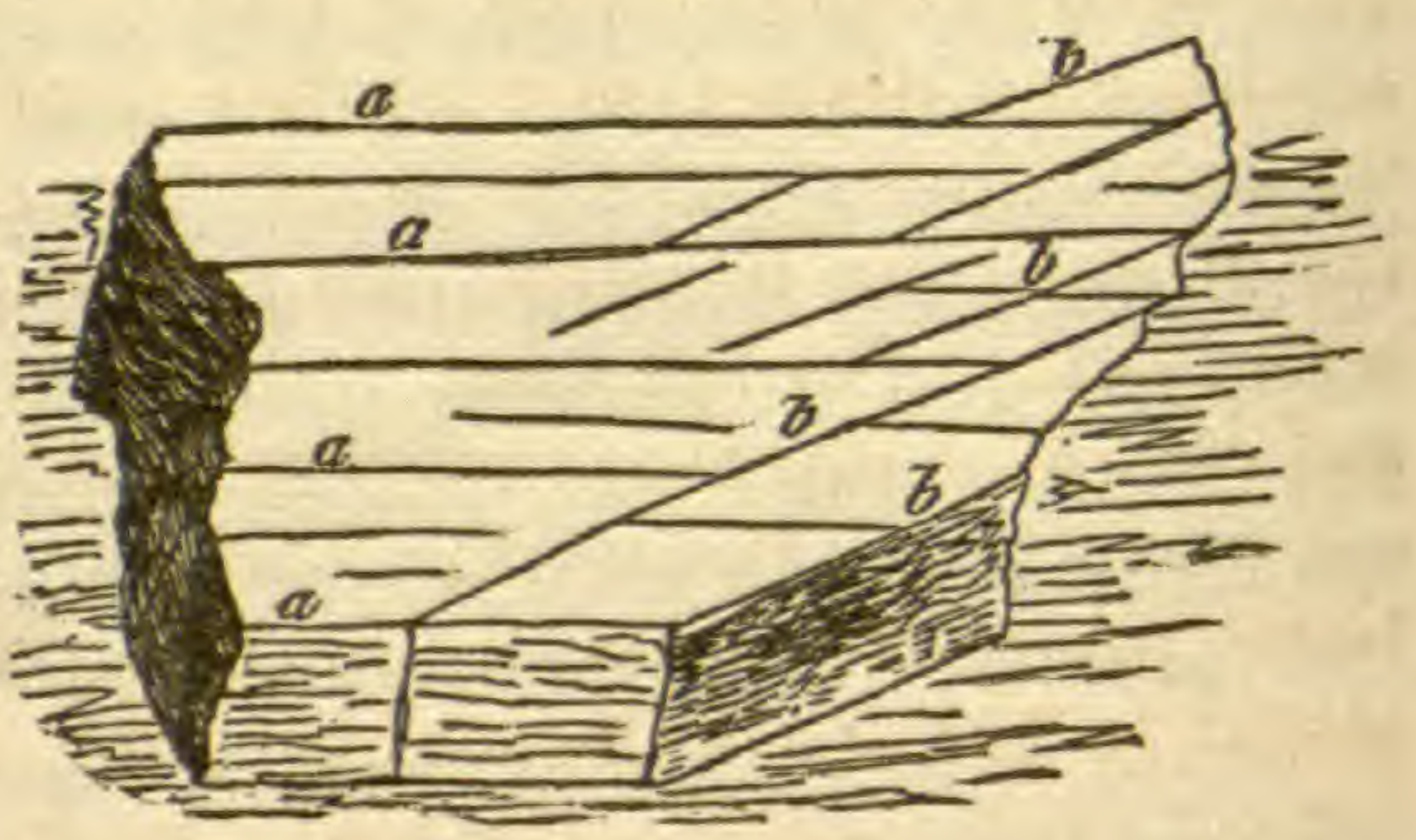


In some instances they are sticks six or eight inches long, an inch and a half in diameter, and rounded at the ends. They are not confined to any particular part of the seam, but may be met with any where, even in the very purest portions of coal. This would indicate that the coal was formed under circumstances allowing these vegetable waifs to be floated in and lodged among the vegetation. But the water on which they were borne could have been only slightly charged with sediments, as no such sediments, with the exceptions of the extremely thin laminæ previously alluded to, are found in the coal. I doubt whether these bits of drifted wood are to be found in the coal very far inland from the water edge of the coal marsh. I have not noticed them in the same seam of coal where it is mined, a few miles northeast of Bear Creek. There is, moreover, evidence to show that the coal changes in character as it recedes from the water's edge of the original marsh. It becomes more soft and caking, and less like cannel in fracture and behavior in the fire. These facts are interesting, as tending to verify Dr. Newberry's theory, that cannel coal is produced by the action of water in rendering the vegetable matter more soft and pulpy.

*Vertical planes.*—The existence of vertical planes in all coal has long been known. I have improved many opportunities of studying these planes, and have, so far as our Western coal fields are concerned, verified a conclusion which I presented in a paper read before the American Association for the Promotion of Science, at the meeting in Springfield, Mass., viz: that these planes have a uniform direction—nearly east and west. I have examined seams of coal in all parts of the Coal Measures, from the bottom to the top, as well as seams far apart geographically, and have invariably found the general direction of these planes the same. So confident have I become in this conclusion, that, in my geological rambles among the hills and mountains, I have often taken my points of compass from the coal, as I have found the seams exposed in the various hillsides.

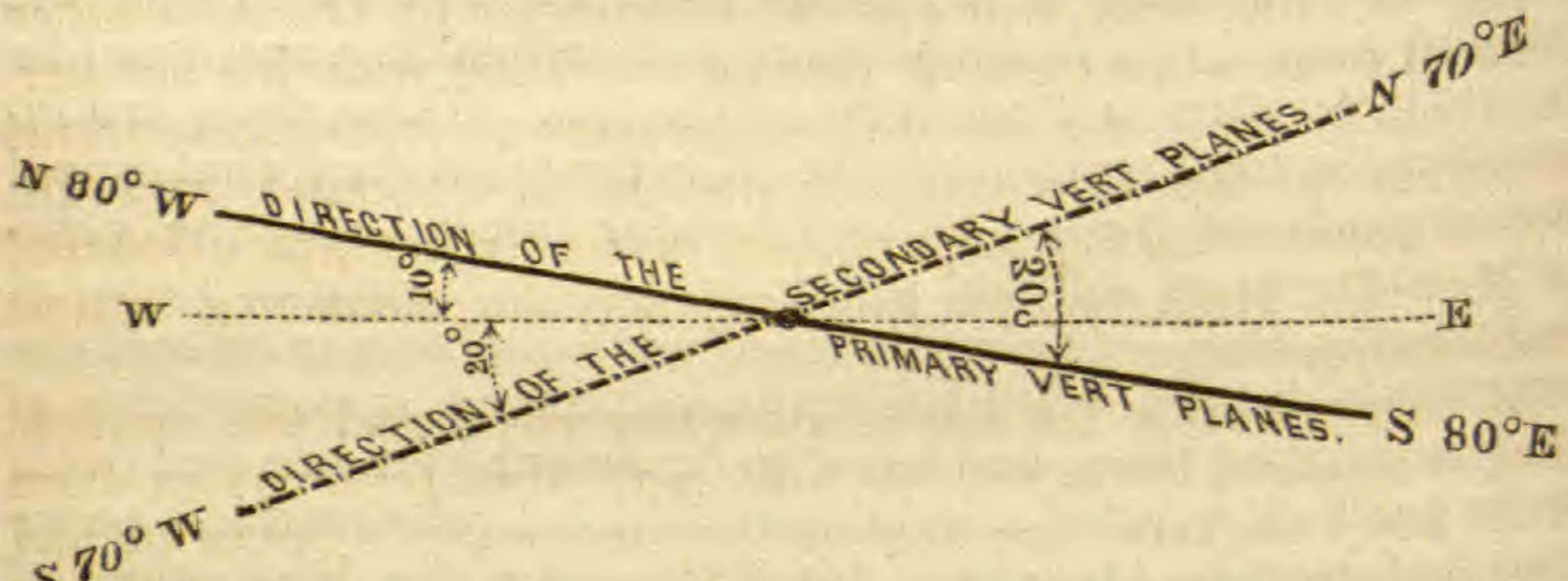
In the Bear Creek coal the principal planes lie in the direction S. 80° E. Besides these principal or primary planes there are local developments of other planes, which make an angle with the first. The secondary planes are parallel with each other, and constitute at the point where exhibited a perfect and beautiful system, and as distinctly marked as the primary system. The accompanying figure, (fig. 3,) shows the systems of planes in a block of the Bear Creek coal. The lines marked *a* represent the primary planes; those marked *b* the secondary ones.

3.




In very rare instances the plane *b* curves into the plane *a*, but generally there is a sharply defined angle of intersection. These angles vary in different localities in the same mine; but wherever the secondary planes appear, they are parallel and true to their own local system. The highest angle I have measured is over 30°, but generally the angles are less. So far as I have examimed the Bear Creek coal, the secondary planes cross the primary ones in the direction of south-of-west and north-of-east. This brings the primary planes on one side of the east and west line, and the secondary planes on the other (see fig. 4). If these

4.




may be called crystalline planes, as their regularity and polish would indicate, they were doubtless formed at the time of the solidification of the coal, and their direction determined by terrestrial electricity. The great east and west currents of thermal electricity might induce a sort of polarity in the particles of coal, causing them to part in the magnetic directions north and south. The presence of iron in the coal might have facilitated this action. The divergence of these planes from an east and west line are not greater than the aggregate secular variations of the needle, and may have been produced by the same cause. That these planes are entirely unlike slaty cleavage and jointed structure, seen in other rocks, is, I think, sufficiently apparent. There are no such planes, nor jointed structure of any kind, in the bituminous slates and shales accompanying the coal. A thin lamina of coal, not thicker than paper, shows these planes perfectly, while nothing of the kind is found in the bituminous slate above and below. There is no jointed structure to be found in any of our rocks associated with the coal. So far as I have observed, these joints are generally found in rocks which have been subjected to heat as well as pressure. If this is so, we should not expect such joints in the coal; as in the West, it has never been subjected to any heat, the coal still retaining its full and normal quantity of bitumen.

Other lessons may be learned from this seam of coal, but I defer their consideration to another time.

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ART. XX.—*On the Chimenti Pictures;* by CHARLES A. JOY, Professor of Chemistry in Columbia College, New York.

PHOTOGRAPHS of the famous Chimenti drawings have been exhibited in this country, upon the back of which was pasted the following announcement:

"The stereoscopic pictures executed by Jacopo Chimenti, born 1554, died 1640. These remarkable pictures, from the Museum Wicar at Lille, were found to be stereoscopic by Dr. Alexander Crum Brown, when visiting that city in 1859 with his brother, Dr. John Brown. Sir David Brewster communicated this curious discovery to the Photographic Society of Scotland, and Dr. Brown's account of it was published in the Photographic Journal of May 15, 1860, and in the Encyclopedia Brittanica, article Stereoscope.

In these works, Sir David Brewster expressed the opinion, that as Baptista Porta had, in 1593, published (after Galen, born A. D. 130,) the true principle of the stereoscope,[1] Chimenti had executed the drawings from the binocular principle of Porta, and was therefore the true inventor of the ocular stereoscope.

In order to satisfy himself of their stereoscopic character, Sir David applied for a photograph of them, but failed in procuring it. Mr. Wheatstone, however, having obtained one, declared

[1] See his Treatise on the Stereoscope, pp. 6 and 8.

that *the pictures were not stereoscopic;* and Dr. W. B. Carpenter, being of the same opinion, charged Sir David Brewster publicly with dishonesty, in not having retracted his opinion of them, alleging that he must have known that they were not stereoscopic from copies in his possession. Sir David never saw the pictures till March 7, 1862, when he received a photograph of them from Professor Kuhlemann, of Lille, and found them to be *truly stereoscopic!* Mr. Wheatstone and Dr. Carpenter *apparently* not knowing that they should be *transposed* when placed in the stereoscope, though this is distinctly shown in Dr. Brown's communication."

The authorship of the above history of the Chimenti pictures is usually attributed to Sir David Brewster.

Prof. Emerson, in the following note, has again called my attention to these prints, being impelled thereto by an article from Sir David Brewster in the Philosophical Magazine:

"PARIS, Feb. 24, 1864.

*To Professor Joy, Vice-President of the Am. Photographic Society:*

MY DEAR SIR:—As I know your lively interest in such matters, you will probably have noticed a letter of Sir David Brewster in the Philosophical Magazine for January, 1864, in which he controverts the conclusion I draw from my investigations of the Chimenti Pictures.[2] It is my intention to reply to this letter of Sir D. Brewster; but as an important element in the final settlement of this scientific controversy is the testimony, on the point in dispute, of persons skilled in stereoscopic investigation, it is my opinion that you would render valuable aid to the cause of optical science if you would collect and publish, in Silliman's Journal, American testimony on this subject. You will greatly oblige me by undertaking this investigation, especially as I know you will do it thoroughly and exhaustively.

I am very sincerely yours, EDWIN EMERSON."

In compliance with Professor Emerson's request, I have been at some pains to obtain American testimony on the matter in dispute.

Three sets of the Chimenti pictures were pasted on stereoscopic cards. The first set was mounted correctly according to Sir David Brewster; the second, as they were found, that is, wrong, according to Sir David; on the third, two prints from the same negative were placed side by side. These prints were sent to persons familiar with the use of the stereoscope, and a large amount of testimony was obtained. Truth compels me to say that not a solitary person found them to be "truly stereoscopic."

A few observers imagined that they saw some slight relief; but as they pronounced the relief to be best where the right and left hand prints were from the same negative, their testimony was unfavorable to Sir David Brewster.

[2] This Journal, Nov. 1862.

When photographs of the Chimenti drawings were first sent to this country, they underwent a thorough and searching scrutiny; the verdict was unanimous against Sir David, and all were at a loss to account for the pertinacity with which that distinguished observer adhered to his opinion.

Out of the mass of testimony which was cheerfully furnished to me, I have made a few selections.

Professor William B. Rogers, of Boston, whose opinions upon questions in physical science rank as high as those of any man of science in this country, has sent me the following valuable suggestions, which I publish at length upon my own responsibility, as the writer has gone to Europe and cannot be applied to for his consent.

Prof. Rogers writes: "I will state the impression gathered from a hasty scrutiny made both without and with the stereoscope. It is simply this; that the difference between the two pictures is such in kind and degree as might easily arise from imperfect or careless copying, and furnishes to my mind no evidence of having been designed for stereoscopic effect. In my own attempts to draw simple figures in pairs as nearly alike as I could make them without measurement, I have rarely succeeded in producing twin pictures which on combination did not present more or less relief. An expert draftsman could doubtless reproduce a drawing so closely as to avoid any *decided* effect of this kind; but if careless even to the extent of a very slight change of direction of some of the lines, his copy, however generally true, would not fail, on combination with the original, to exhibit decided stereoscopic relief. It is needless to remind those who are practiced in binocular combination, of the exquisite test which it affords of the slightest deviation from identity in the drawings combined.

I have now before me two copies made by a young friend from one of the photographs which you sent me, in which she endeavored to reproduce the original as exactly as she could, without resorting to measurement. On combining them with the eyes or with a stereoscope, they furnish a picture quite as clearly and strongly stereoscopic as the twin Chimenti pictures. I should add, that although ready with the pencil, my friend has but small experience in the use of it.

It seems to me that until some clear proof is furnished, independent of the Chimenti pictures, that the stereoscope was known at the time they were drawn, the stereoscopic availability of these pictures may most reasonably be attributed to accident, and can hardly weigh as evidence that the instrument had then been discovered.

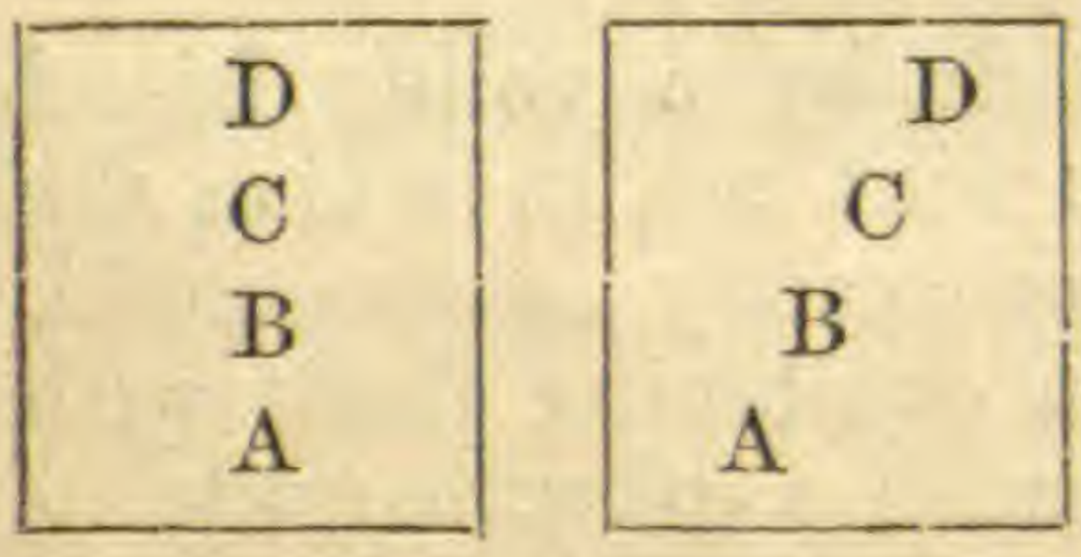
Such are my impressions from a brief examination of the photographs; but the opinion of Sir David Brewster, who has studied the drawings critically, and who is so distinguished for knowledge and experience in this department of enquiry, makes me hesitate in adopting a conclusion to which he is opposed."

Another authority writes: "To my eye they are as lacking in all stereoscopic effect as would be two *straight lines*, or a sheet of paper pasted on the wall. I venture no opinion as to what they were intended to be, only as to what they are."

Another gentleman very familiar with the subject, says: "There is not the remotest approach to stereoscopic effect. There are slight differences between the two, but not of the kind which usually exists between stereoscopic pictures. One is evidently a copy (and carelessly done) of the other. The positions of the figures differ. In one the head is raised higher than in the other. The same figure is $\frac{1}{32}$ of an inch *taller* than the other, and the bench it sits on stands level, while in the other it is tipped downward. There are many such differences, which prove conclusively that they never were intended for stereoscopic effect. In the stereoscope it is impossible to make these differences coincide."

In a paper "On a method of producing stereographs by hand,"[3] Professor Rood has shown that where we "adopt the left hand picture as a standard of comparison, we find that the right hand picture is an exact reproduction of it, with one important exception, viz: that in the right hand picture the position of an object a little more distant from the spectator than the immediate foreground, is shifted a small distance toward the right hand; objects farther back have their position shifted still more to the right hand.

This is illustrated by fig. 1, where A is in the immediate foreground and D in the background."




The bulk of the variations in the Chimenti pictures are not to the right or left, but up and down. If these differences were designed by the artist to produce what we now term stereoscopic effect, they fall short of the intent. They produce a slight blur on the pictures when we attempt to combine them; so that a majority of persons find the best effect with the Chimenti pictures when two prints from the same negative are presented to their observation: while experts denounce the whole thing as an attempt to practice upon their credulity.

In some of the paintings by Teniers of a chemist in his laboratory, can be seen apparatus highly suggestive of a spectroscope. We trust that no future Dr. Brown will announce that the spec-

[3] This Journal, [2], vol. xxxvi, 71, Jan., 1861.

troscope was so well known at the time of Teniers as to be included in the inventory of apparatus of a chemist's laboratory.

Is not the evidence that the theory of binocular vision and of the stereoscope were known at the time of Chimenti, quite as weak as would be afforded by Teniers' pictures, to disprove the modern invention and use of the spectroscope?

The Chimenti pictures must therefore be ruled out of court and judgment entered for Mr. Wheatstone, unless new evidence can be found worthy of the consideration of scientific men.

New York, June 1, 1864.

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ART. XXI.—*Results of some recent Observations of the Solar Surface, with Remarks;* by the Rev. W. R. DAWES.[1]

DURING nearly the whole of last year and the first three months of this, the state of the air was rarely sufficiently good to permit a successful scrutiny of the solar surface with high powers on a large telescopic aperture;—on such occasions, at least, as I was able to make much use of. But on three or four days in April I found that vision was remarkably steady and distinct, and I improved the opportunities to the utmost.

As it was of considerabie importance that the same minute portions of the surface should be examined and compared together under various magnifying powers, and with different shades of dark glass, I preferred, after a careful comparison with the ordinary eye-pieces and an excellent transparent diagonal, to employ a solar eye-piece of my own construction. And I would take this opportunity of correcting an idea which has been advanced, that the diffraction of the rays at the edge of a small aperture in that eye-piece interferes with the distinctness of the object viewed. Close to the edge of the field there is, of course, as Mr. Huggins well expressed it at the last meeting of the Society, a narrow ring of diffracted light: but the breadth of this ring does not, I think, exceed 2″ on my telescope; and the smallest aperture in the diaphragm-plate has a diameter of 15″. Consequently, there is a central space of at least two-thirds of the diameter in which the definition is perfect, even in the smallest aperture of the eye-piece. Yet, for the purpose now referred to, I rarely use a field of much less than *one minute* in diameter; and any object not exceeding 45″, when placed in the centre of the field and kept there by the driving-clock, is recognized with certainty under different powers, and seen with the utmost possible distinctness.

1. One of the objects I had principally in view was, to ascer-

[1] Monthly Notices of the Royal Astronomical Society, May 13, 1864.

tain whether in any part of the photosphere any objects could be found which could reasonably be compared to "willow-leaves" (as called by Mr. Nasmyth) in their form.—It may be well to state here that I have always found it difficult to devise any appropriate appellation for the small bright irregularities of the surface, which would avoid an assumption of their character, or ascribe to them a regularity of form they do not possess. In my first paper I expressed my strong objection to any *name* of this kind, as calculated to convey an erroneous impression. The term *willow-leaves* seemed utterly inapplicable to anything I had ever succeeded in discovering. A far less objectionable term, as it appears to me, is that of *rice-grains* applied by Mr. Stone to those objects with which all careful Sun-observers must be acquainted, as there is no difficulty in seeing them in a moderately favorable state of the air, and which have been familiar to myself for many years;—so much so, indeed, that when they were not discernible I invariably abstained from any further scrutiny of the solar surface as being useless. Yet even this appellation conveys the idea of uniformity of shape and size which these objects do not possess, and is, I think, on that ground, objectionable. But I have been led by it to apply the term *granulations*, or *granules*, which assumes nothing either as to exact form or precise character; and I venture to hope that the term will be generally adopted.

To proceed then with the results of my recent observations.

Various portions of the surface were diligently examined with powers from 131 to 407. The bright little granulations were easily seen in all parts not very near the edge of the disk. Their forms and sizes were carefully noted, and found to be so various as to defy every attempt to describe them by any one appellation or comparison. But, as I have observed in a former communication, the rarest of all forms was the long and narrow; and in no instance did I succeed in finding one which could with propriety be compared to a willow-leaf. Occasionally, some that were nearly in contact differed so greatly in size that one was four or five times as large as the other; and while, in a remarkably bright mass, one somewhat resembled a blunt and ill-shaped arrow-head, another, very much smaller and within 5″ of it, was an irregular trapezium with rounded corners. The more quiet and perfect the views I obtained of them, the more convincing did the evidence appear that they were not individual and separate bodies of a peculiar nature, but only different conditions as to brightness or elevation of the larger masses forming the mottled surface; just as the brighter portions of that surface, and the *faculæ* also, are different conditions of the general photosphere. In these researches I met with nothing which had the slightest resemblance to the *interlacing* which Mr. Nasmyth has so clearly described and so distinctly depicted in his communication to the Manchester Literary and Philosophical Society; of which letter I was not aware when I penned my former paper on this subject. Mr. Nasmyth having subsequently favored me with a copy, I was so struck with the clearness and decision of his assertions that I began to think I must have overlooked the peculiar appearances of the objects which he has depicted in his diagram as being "*the exact form of these remarkable structural details*," which he describes as "*forming the entire luminous surface of the Sun;*" and, therefore, to leave nothing untried, I collected all the information I could as to the means employed by him and by other observers who had seen something of the same kind, in the hope that some change in my apparatus or mode of using it might at length render me successful. Now, however, it appears that Mr. Nasmyth has withdrawn his former statements as to the exact and uniform figure of the objects he claimed to have discovered on the entire surface; and that, in fact, all that might have been regarded as a *discovery* resolves itself into an appearance perfectly well known many years before.

The darker or shaded lines between the granules I distinctly observed in many places to be pretty thickly covered with dark dots, like *stippling with a soft lead-pencil;* and these are what have been called "*pores*" by Sir John Herschel, and "*punctulations*" by his father. Some of these were almost black, and looked like excessively small spots just breaking out. But none of them were seen to enlarge or materially alter their form, though at times so sharply defined with powers 276 and 407 that it was obvious they were in general not quite round.

It appears to me quite incomprehensible that, under such definition, any bright objects of peculiar form should have escaped detection when specially looked for; and I was certainly struck with the extreme rarity of a long and narrow shape among the hundreds of granules which I examined in those four days' observations. If therefore there are any willow-leaf-shaped objects to be seen, they must be quite distinct from these granulations, and can form no regular or usual ingredients in the composition of the solar surface.

On the *penumbræ* of the spots then visible I found several long and narrow bright lines, extending like bits of white thread completely across the whole breadth of the penumbra without any break whatever; and, though there were several smaller pieces, not one of them would have suggested to my mind the comparison with a willow-leaf. Perhaps the bright granules of the general surface may sometimes be compressed into a longer form under the influence of the same forces which produce the longer threads or *straws* on the penumbra; but one of the most striking features of that part, as well as of the general surface, is, according to my observations, the entire absence of uniformity in the brighter portions with respect both to their size and shape.

2. A second object of my investigations was to determine whether the granulations existed equally in the brightest and in the less luminous portions of the surface, which together form the general and comparatively *coarse* mottling of the photosphere.

On comparing together in the same field of view several of the more and less luminous masses which produce the coarse mottling of the surface, I came to the conclusion that the granules are generally *larger* as well as brighter on the brightest parts than on the darker; the difference in brightness of the individual granules in each part being much the same as in the different masses themselves. One fact struck me as very remarkable, that, on each of these larger masses, the individual granules are all very nearly of *equal* brilliance throughout the mass to which they belong. They are not in general, if ever, mixed together, some much brighter and others far less bright on the same mass. Occasionally I have noticed that some of the brighter masses are decidedly brighter than others of the same class, and that such have extended to a much greater length than usual. The granulations on them are also generally, I think, of uncommon size, and the shaded lines between them of a ligher tint. I strongly suspect that these specimens, if near the Sun's edge, would be seen as *faculæ*.

3. A third principal object I had in view was, to scrutinize especially the brighter parts immediately surrounding the penumbræ of spots (which in a former paper I have referred to as indicating a heaping up of the photosphere by some eruptive force proceeding from the centre of the spot), with the purpose of ascertaining whether the granulations elsewhere easily visible were to be found there; and also to examine the *faculæ* of various forms, sizes, and degrees of brightness, to determine whether any such granulations were visible upon them.

With reference to the reality of this brighter region surrounding all the considerable *profound* spots, and also some of the smaller ones, I have lately been much gratified by the corroboration of the fact afforded by some solar photographs taken by our President, and obligingly presented to me since the publication of my former paper. While the general mottling of the surface is well brought out in them, they show that this mottling never extends to the margins of the spots; which precisely agrees with my observations. In this locality I have never been able to detect any of the granulations so abundant and so easily seen elsewhere. My recent observations fully confirm the conclusion I have formerly arrived at, namely, that the commotion, of whatever kind it might be, had produced the effect of heaping up the material of the photosphere; and had confounded all the distinctions elsewhere seen of the large brighter and shaded masses, and also of the granulations on both, and the pencil-lines, occasionally stippled, by which the individual granules are distinguished.

Precisely the same results attended my examinations of the *faculæ*. Nothing resembling granulations could be found on any of them, whether they were the tortuous, thread-like objects seen near the east and west borders, or the shorter lumps of apparently similar composition near the poles. Yet the granulations were discernible in several places nearer to the Sun's edge than the faculæ were situated; so that it could not arise from the obliquity of the view that they were not visible on the faculæ themselves. I conclude, therefore, that the same disturbance which produces the elevated ridges confuses the minute features elsewhere seen; and that, though there may be some traces of granulation when the faculæ are viewed almost perpendicularly, yet this is entirely lost when their sides only are seen near the Sun's limb. In this position, however, there are often distinct evidences of irregularity in the elevation of different parts of the ridge; and these may, perhaps, when viewed perpendicularly, produce variations of brightness, like the granules of extraordinary size mentioned above.

Hopefield Observatory, Haddenham, Bucks, May 9, 1864.

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## ART. XXII.—*On Molecular Physics;* by Prof. W. A. NORTON.

[Continued from p. 78.]

IN considering the changes of state through which the same substance may pass, we have been led to recognize as an important physical principle upon which the mechanical properties manifested in each new condition in a great degree depend, that the physical condition of the individual molecules is liable to permanent variations from the effect of heat; and that these variations consist in expansions of the electric atmospheres which surround the atoms of the molecules. If we take a more extended view, and consider the diverse permanent changes of condition which the same substance may experience, while in the same state of aggregation, we may discern the operation of of a still more comprehensive principle; viz., that the physical state of the atmosphere of a molecule, and therefore the curve which represents its action upon surrounding molecules, is liable to permanent alteration from the action of external forces generally. It is well known that if a mechanical force, of considerable intensity, be applied for a short interval of time to a body, the result will be a permanent change in its form. The experiments of Hodgkinson have indeed established that a certain set may be imparted to bars of cast iron, by a temporary load which is but a small fraction of its breaking load, and that "there is no weight however small that will not injure the elasticity" of such a bar. As we cannot suppose that a given mass of molecules, while retaining forces of mutual action of unvarying intensity, can take up an infinite number of positions of equilibrium, differing but slightly from each other, we must conclude that the individual molecules experience some change of condition, which occasions a change in the intensities of the forces they exert in a given direction.

From our present theoretical point of view, such possible changes of condition consist in compressions, or expansions, of the molecular atmospheres; either as a whole, or unequally on different sides. In the former case there will be a variation in the size of the atmosphere, and in the intensities of the forces of attraction and repulsion exerted by the molecule at a given distance, but the forces exerted in different directions will have an equal intensity. In the latter case there will be a variation in the form of the atmosphere, and an inequality of action in different directions. The form assumed will be spheroidal, or approximately so, supposing it to have been originally spherical; and the mechanical result will be the exertion of an increased force of attraction from the sides of the molecule at which its atmosphere is compressed, and a diminished force from the sides at which the atmosphere is expanded. This follows, as a necessary consequence, from the fundamental conception of molecular forces, developed on pages 68 to 71; as may be distinctly seen by attending to the values of $\frac{n}{m}$ and $r$. It will thus be seen that the molecular atmospheres, in assuming the spheroidal form, under special circumstances, determine the existence of molecular axes of cohesive attraction, "whose force is inversely related to their length." In the direction of such axes, then, the limit of stable equilibrium ($Oa$, fig. 1, p. 71) will be least for the shorter axis, and greater for the longer axis.

When a force of pressure applied to a body determines a permanent compression, the molecular atmospheres remain compressed, or in closer proximity to their central atoms, and the force of cohesive attraction is permanently increased, and the limit of stable equilibrium diminished. The heat developed is a necessary result of the compression of the atmospheres. If the elastic reaction to the pressure after it is withdrawn, were perfect, the atmospheres would resume their original form and dimensions, the original molecular forces would be recovered, and the heat evolved would be absorbed again. In general when mechanical forces are applied to a body, the heat evolved, or absorbed, is a necessary accompaniment of the compressions, or expansions, superinduced in the atmospheres of the particles, and may be regarded as a sensible indication of the extent of such changes of molecular condition. The mechanical work, of which the heat evolved serves as the measure, is expended in urging the atmospheres nearer to their central atoms.

On the other hand, if heat be directly applied to a body, it has a tendency opposite to that of a mechanical pressure, or to expand the molecular atmospheres, and so to reduce the intensity of the cohesive attraction, at a given distance. It is to be observed, also, that heat has a tendency to dissolve the groups in which the particles of a solid may be aggregated; and when the point of fusion is reached, will effectually break up such groups, and bring the mass to the condition of a homogeneous and symmetrical arrangement of molecules.

*Solidification, or Crystallization.*—It is a well recognized principle that solidification and crystallization are the same process. This great principle was first propounded by the learned and acute Dr. Young in 1807, in his lectures on Natural Philosophy. It has also been advocated by Biot and other physicists; and more recently has been reasserted and ably sustained by Professor Dana, in his admirable paper on Cohesive Attraction, published in vol. iv, second series, of this Journal. If now it be admitted that solidification is in every instance but a more or less perfect crystallization, it will be perceived that the investigation of the mechanical process of crystallization must consist essentially in an inquiry into the conditions and results of the operation of the molecular forces under special circumstances. The general nature of these forces, and the laws of the variation of effective molecular action with the distance between two molecules, have already been under discussion. We have also seen (pp. 207, 208) that the mechanical condition of an individual molecule is subject to change under the operation of heat, and external forces generally, by reason of a change produced either in the dimensions, or form, of the atmosphere of the molecule, and that it may thus acquire permanent axes of attractive force. To establish a sufficient basis for a general explanation of crystallization we have only to remark further that the molecule of every particular substance has primarily, and inherently, its own special physical condition, by virtue of which it exercises an effective action that would be represented by a special curve, and experiences under the operation of heat, and other causes, its own peculiar changes of mechanical condition. Upon this idea it may be seen that every substance may have its particular form of crystallization, although the molecules should be devoid of all natural polarity.

The different systems of crystallization may be regarded as so many different systems of equilibrium of masses of molecules, under the operation of molecular forces diversely modified by the circumstances that determine the crystallization. The general nature of the modifications consists in a spheroidal form imparted to the molecular atmospheres, and the consequent development of certain axes of attraction; that is, of diameters of least or greatest length, in the direction of which the attraction has, at a given distance, a maximum or minimum value, and the limit of stable equilibrium ($Oa$, fig. 1) a minimum or maximum value.

Crystallization begins at a certain point of a liquid and is generally determined by the loss of heat, or the evaporation of the liquid solvent. We already have seen reason to believe that the molecules of the liquid have a symmetrical arrangement previous to the crystallization (p. 75). Whether this be admitted or not, such an arrangement obtains in the crystal formed from the liquid. The particles successively take positions in the corners or angular points of a series of polyhedral figures; as cubes, prisms, octahedrons, &c. Any two such figures, lying contiguous to each other, have a common face, or, as in the case of the octahedron, a common angular point. The crystallization takes place, either successively or simultaneously, in the faces of these figures. We have then first to consider the process of crystallization, as it may occur in a single plane.

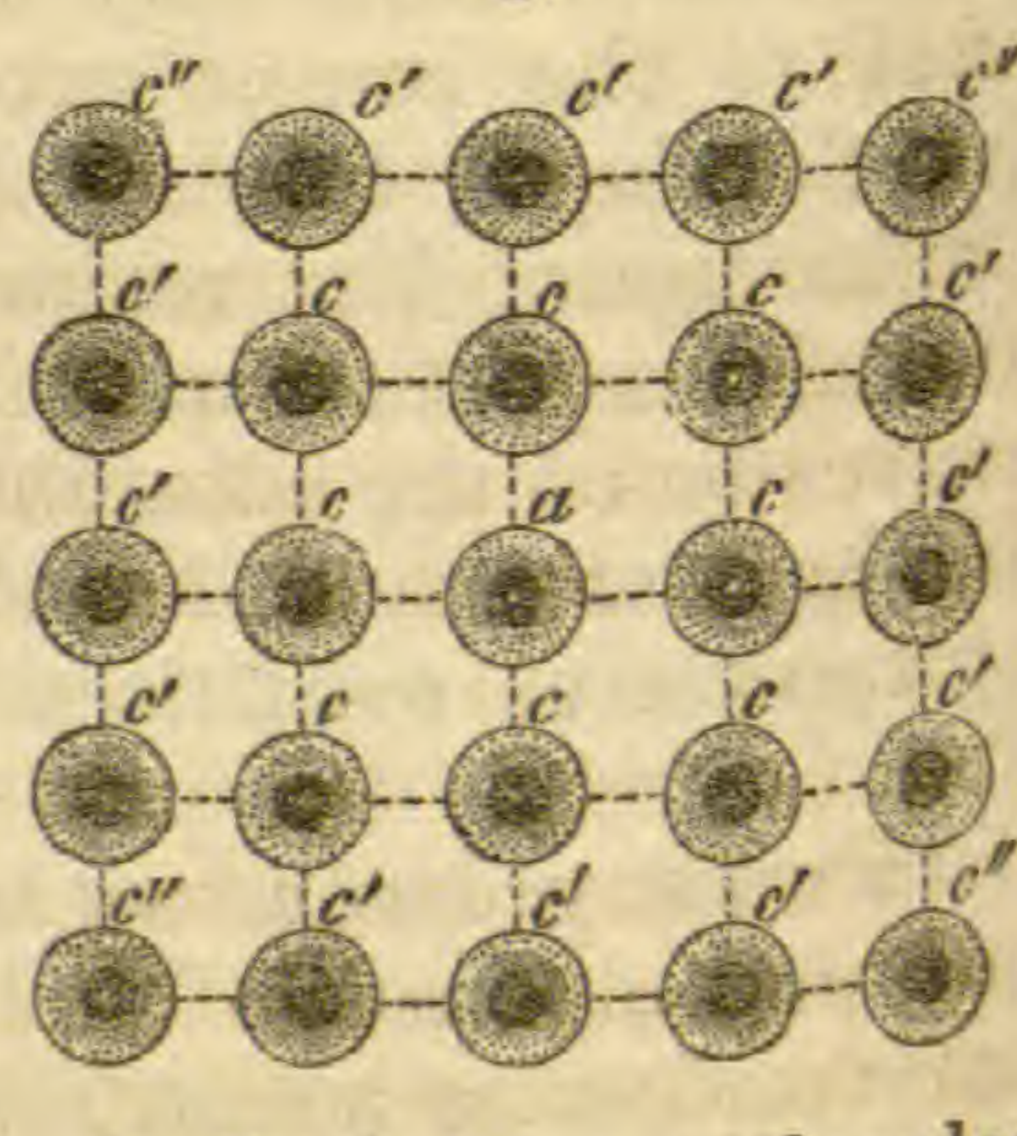
The result of every such process is the arrangement of the molecules in the angular points of a series of quadrilaterals; which may be squares, rectangles, rhombuses, or rhomboids. If we suppose, in the first instance, several molecules to unite along a single line, and molecules posited on either side of this line to unite with those already crystallized, three different general modes of arrangement may occur; the new particles may take up positions opposite those of the first line, or opposite the middle points of the intervals between these particles, or opposite other than the middle points of these intervals. In the first case, squares or rectangles will be formed; in the second, rhombuses, which may in special cases be squares; and in the third, rhomboids, which in special cases may be rectangles. The general tendency of the crystallization occurring along the first line should be, by reason of the compression of the molecular atmospheres along this line, and the consequent expansion of them in a direction perpendicular to it, to develop an axis of increased attraction in this primary line of crystallization, and an axis of diminished attraction in the perpendicular direction. When this result is reached, and successively along the lines of particles parallel to the first, the figure assumed will be either a square, a rectangle, or a rhombus. The two molecular axes will be coincident with the sides of the minute rectangular figures that make up the larger rectangle, and with the diagonals of each minute rhombus. The condition essential to the formation of a square is that the properties of the molecules in reference to cooling, (or, in general, in reference to the propagation and absorption of impulses) should be such that each set of four contiguous molecules are, when in the incipient state of crystallization, in the same physical condition. That a rectangle may be formed, a group of four particles must unite, but the escape of the heat-pulses that occurs primarily in the direction of one of the sides of the rectangle must determine a greater compression of the molecular atmospheres in this direction than in that of the other side. That the figure of a rhombus may be assumed, two particles must first unite, and subsequently two other particles must take up, under the attractive action of these, positions opposite the middle of the interval between them. To understand how a rhomboid may result we must observe that when a line of particles is crystallizing, each particle, $m$, as it becomes united, exerts a certain disturbing action upon a particle $m'$ next in the line, and also upon a contiguous particle, $n$, at one side of the line. When the particle $m'$ unites it also modifies the condition of $n$, but as its action is subsequent to that of $m$, it is possible that in special cases the final result may be an inequality of disturbance, so that $m$ and $m'$ will attract $n$ unequally, and the position of equilibrium assumed by it will in consequence not be opposite the middle of the interval between $m$ and $m'$. In this case the electric atmosphere of $n$ will have a form differently modified, and its second axis will be oblique to the line of primary crystallization in which the axis first developed lies.

The figure first assumed must then depend upon the fundamental properties, with respect to heat, &c., of the individual molecules of the substance; and the general tendency must be, for the first group of molecules, to acquire increased dimensions by successive solidifications of a series of figures similar to the first.

In what precedes we have had regard only to the play of the ordinary molecular forces, and the modifications which the ordinary molecular action may experience from the loss of heat, and in the act of solidification; but there must result, in each instance of the union of two particles, a modification of the condition of their atmospheres, which should develop a new force of attraction, that may play an important part in the continuation of the process of crystallization. For, when two particles unite by crystallization, their atmospheres, on their nearer sides, will become compressed, and consequently on their farther sides expanded. Each molecule will thus virtually be brought into an electro-polar condition, with the positive pole turned outward. This positive pole, or excess of electric ether, tends to bring all the molecules lying in the prolongation of the line of the first two into the same electro-polar condition, and, in this state of induced polarization, a force of electric attraction will subsist between the particles. As one particle after another in the line comes to unite with those previously crystallized, its previous polarization will be enhanced, and it will exert an increased attractive force upon those not yet crystallized. At the same time, by the compressive and repulsive action of the contiguous atmospheres, the particle with which the new one unites will lose a considerable portion of its polarity. This reflex action, attendant upon every act of union, should eventually greatly diminish, if not wholly remove, the prior induced polarization.

Another effect of this reflex action, to be noticed, is the increased expansion of the atmosphere of the molecule which experiences the reactive compression, in the direction perpendicular to the line of crystallization. This molecule thus acquires an increased positive polarization on the outer side, lying in this perpendicular direction, and therefore exerts an increased force of electric attraction in this direction. In the varying operation of this induced electric polarization, and of the reflex action just noticed, we may discern the probable origin of those supposed variations of axial attraction, which Professor Dana has shown will suffice for the explanation of secondary planes in crystallization.

2.




To illustrate by a special case, let fig. 2 represent a process of crystallization in which the particles are arranged in successive squares. When *c*, *c*, *c*, are the outer particles, their outer sides will be positively polarized, and will consequently exert an electric attraction, in addition to the increasing molecular attraction that results from the cooling, upon the molecules *c*′, *c*′, *c*′, immediately exterior to them. The next step in the process should be the union of the molecules thus attracted; and all of these molecules should have the same tendency to unite, unless there should be a material inequality in the physical condition of the outer particles *c*, *c*, *c*, *c*, on different sides of the square *c*, *c*, *c*, *c*. But the four corner particles, *c*″, *c*″, *c*″, *c*″, cannot thus be directly brought into union with the particles of the crystal. They must either unite with the nearest particles of those newly attached, or remain disunited, to become incorporated at a later stage of the process. In the normal or complete growth of the crystal, the first would be the result. If, on the other hand, the forces of the new lines of outer particles *c*′, *c*′, *c*′, *c*′, should fall off materially in intensity, so that the corner particles, $c''$, are not taken up in the same step of the process as the others, secondary lines would arise at the angles of the square $c'$, $c''$, $c'$, $c''$. In the whole cubical crystal of which fig. 2 represents a section, *secondary planes* would be formed at the edges. Every such line or plane may have different positions, according as the corner particles in question ($c''$) become incorporated in the next stage of the growth of the crystal, or in some of the subsequent stages. This Professor Dana has distinctly shown. What we have here to observe is simply that by reason of the reflex action above noticed (p. 212), when new particles become united to $c'$, $c'$, these particles, $c'$, $c'$, will in fact exert a more energetic attraction laterally upon $c''$, $c''$; and hence the union of $c''$, $c''$ with the crystal may then be determined. If not, the next augmentation of the attractive force attendant upon the union of the next set of molecules may determine this result.

In order that a complete polyhedral crystal may be formed, it is necessary that molecules, on one or both sides of a plane in which crystallization occurs, should become united with those that take up their positions at the angles of the plane figure. Thus two sets of four molecules in parallel planes may take up positions of equilibrium at the eight angular points of a cube, or six may form an octahedron, &c. The conditions that will determine the figure of equilibrium assumed, may be inferred from the general considerations already presented (pp. 210 and 211). The compression of the molecular atmospheres in the first plane of crystallization, will tend to develop a third axis of attraction perpendicular to this plane. Various systems of crystallization are possible, since the particles on one side of the first plane may take up positions of equilibrium perpendicularly opposite to those that crystallize in this plane, or opposite the intervals between parallel pairs of these particles, or opposite the centre, of the quadrilateral figure, which they form.[1]

Professor Dana has shown that the various fundamental forms of crystals may be obtained by regarding the crystal as a mass of bi-polar molecules, of a spherical, or spheroidal form, in contact along certain lines, which are the conjugate axes, or conjugate diameters of the spheroids. This conception of the constitution of a crystal is, in a geometrical point of view, equivalent to that which has now been given. For we have only to conceive spheroids to be inscribed in the polyhedral figures of the compound crystal molecules just supposed, to obtain the representative spheroidal molecules of Prof. Dana; which will also touch each other along similar conjugate diameters of the different spheroids. It is not difficult to make out the various positions of equilibrium of the particles that must obtain in the

[1] This is only a partial view of the matter.

different fundamental forms, and the various physical conditions of the particles upon which these forms must depend.[2]

In his paper on Cohesive Attraction, Professor Dana has apparently put the explanation of the *cleavage* of crystals on the true basis, by attributing it to alternations in the intensity of the attraction in a series of parallel planes. If such alternations really exist, we naturally seek for the explanation of them in alternations of the mechanical condition of the molecules upon which the energy of the molecular forces depend. It is conceivable that such differences may result from the heat evolved in the process of crystallization. Let fig. 3, *a*, *b*, *c*, *d*, *e*, &c., be a line of particles crystallizing in regular succession. When *a* unites with *b*, the heat given out will expand the atmosphere of *c*, and it is possible that after this effect has been produced, the expanded atmosphere will not become condensed under the operation of the crystallizing forces, as much as it otherwise would have been; and hence that the molecular attractive force of *c* will be less than would have otherwise resulted. If the attraction between *b* and *c* should thus be materially lessened, there would be in consequence less heat evolved in the union of the two, and so the attractive force of *d* would be less weakened than that of *c* has been. Accordingly, in the union of *c* and *d* an excess of heat would again be given out. In this way, a series of alternations in the intensity of the cohesive attraction might be brought about along the line of crystallized molecules.

3.


It will be observed that the same fundamental idea pervades all the explanations that have now been given of changes of molecular aggregation, whether these are attended with a change of state, or only with a change of density and form. This is the idea that the physical and mechanical condition of a molecule may change with varying circumstances, and that it may undergo a permanent change, although the temperature should remain the same. The change of condition consists simply in the expansion or contraction of the electric atmosphere of the molecule (p. 208). We have recognized, also, that while the processes of transformation are going on, the normal distribution of the electric ether, that forms the atmosphere of a molecule, may become disturbed, and that thus a transient

[2] The hypothesis of a permanent polarity of atoms, or molecules, has subserved a valuable purpose in linking together phenomena under one physical conception, in several departments of Physical Science; but the progress of science has materially tended of late to shake the confidence reposed in it as a supposed truth of Nature. It will be conceded that it is the dictate of true philosophy to hold it in abeyance until it shall have become abundantly evident that the phenomena in question cannot be deduced from the fundamental conception of the constitution of a molecule, and of the primary forces of attraction and repulsion, to which all other molecular phenomena can be referred.

electric polarity of the molecule may be induced, which may play an important part in the process.

Upon the theory of crystallization here offered, the phenomenon of *dimorphism*, and all changes of form, in the same crystal, produced by heat and external causes generally, are but simple results of the modifications superinduced by these causes, in the form, or distance from the central atoms, of the molecular atmospheres; that is, in the physical features of the molecules, upon which the system of crystallization in every instance depends.

*Heat.*

The general nature of heat, and the general cause of its evolution, have already been considered (p. 64). According to the fundamental ideas presented, heat must be developed whenever the electric atmospheres of the molecules of any substance are urged into closer proximity to the atoms which they surround. Any disturbance of the equilibrium of the particles of a given mass, or any change in the relations of the particles of any body to those of other bodies, which may have the effect to produce this compression of the molecular atmospheres, should then be a source of heat. Thus chemical combination of two particles, in which they are drawn into close union, collision of bodies, external pressure, and friction, are different sources of heat. The disturbance of the electric equilibrium of contiguous molecules may also give rise to the evolution of heat; by reason of the increased repulsion exerted by the excess of electric ether accumulated upon certain of the molecules, or upon one side of them, or of a discharge of the ether occurring from one molecule to another.

*Propagation of heat.*—Primarily, the heat-pulses are developed in the universal ether associated with the atoms of bodies. These pulses may be conveyed outward through the universal ether, posited between the atoms, and in this way be freely *transmitted* through substances; or they may be more or less taken up, or *absorbed*, by the electric atmospheres of the molecules which they encounter. Such absorbed pulses may be given out again, or *radiated*, in their original form, or they may pass on to contiguous molecules through the *electric* ether that pervades the interval between them. It is probably in this latter mode, chiefly, that heat is *conducted* from particle to particle of a substance, though the pulses that are given out by any particle to the surrounding *universal* ether may be in part propagated onward, absorbed by the particles they encounter, and partially propagated onward again in the same manner to the next particles.

The flow of heat from one particle to another of a substance tends to disturb the electric condition of the particles; for the repulsive action of the heat-pulses in the atmosphere of one particle tends to urge away a portion of the electric ether from the contiguous side of the next particle in the line of propagation, and so to induce a negative electric state upon that side, and a positive state on the farther side. This disturbance of the electric equilibrium of contiguous molecules may give rise to a discharge of the electric ether from the one to the other, and a consequent more ready propagation of the heat pulses from the one to the other. Upon this idea there is a close analogy between the conduction of heat and the conduction of electricity in the galvanic current; both depending upon the facility with which an electric polarization of contiguous particles is determined. The origin of this propagated polarization, in the one case, is the addition of repulsive pulses to the atmosphere of a molecule, and, in the other, the accumulation of an excess of the repulsive electric ether around a molecule, or upon one side of it. A confirmation of these views is afforded by certain phenomena of thermo-electric currents, from which it appears that the conduction of heat is in reality attended with the disturbance of the electric equilibrium;[3] and they are also sustained by the fact of the close agreement that subsists between the conducting powers of the metals for heat and electricity. The analogy between the conduction of heat and the conduction of a galvanic current may be more fully stated, thus: A stream of heat consists of two sets of pulses, viz: those conveyed by the electric ether, and those conveyed by the universal ether; and the propagation is attended with, and partly by means of, an induced molecular polarization. The pulses propagated by the electric ether tend to develop this polarization, which determines a discharge of ether from one atmosphere to the next. But the other set of pulses, in proportion as they are taken up by the nearer side of the next atmosphere, tend to weaken the induced polarization, and so to check the flow of the heat. A galvanic current comprises two similar sets of pulses, is attended and promoted still more effectually by a similar molecular polarization, and the absorption of the ethereal pulses, and their subsequent emission as heat tends to check the flow of the current.

The feeble conducting power of many substances is probably due to an aggregation of their particles in groups, with large intervening spaces. The bad conductibility of gases and liquids, both for heat and galvanic electricity, agreeably to the views just offered, must be ascribed to a feeble polarizing action of one particle upon another. This appears to be a consequence of the peculiar state of the molecular atmospheres which determines the fluid condition (p. 75), combined with the effective mutual repulsion of the particles in this condition. In the case

[3] See De la Rive's Treatise on Electricity, vol. ii, p. 536, &c.

of liquids, like water, whose ultimate molecules are compound, a portion of the heat propagated should be consumed in expanding the compound molecules.

*Capacity of Bodies for Heat.*—The fundamental law discovered by Dulong and Petit, that the specific heats of elementary substances are inversely proportional to their chemical equivalents, or atomic weights,—or, in other words, that the atoms of such substances have the same capacity for heat,—if interpreted in the language of the present theory, amounts to this, viz: whatever may be the difference of condition of the molecular atmospheres of elementary substances, if the same amount of heat be imparted to these atmospheres, the same amount will be given off again and interchanged with surrounding bodies having the same temperature. This would seem to imply that the portion of the heat absorbed, which is consumed in expanding the atmosphere, is the same for different simple molecules, and that the remaining portion, which is taken up as new pulses by the atmosphere, is also the same for molecules of different elementary substances. There is nothing in the conception we have formed of a molecule, and of the probable difference of physical condition in molecules of different substances, that is apparently opposed to this result.

The capacity for heat of compound is in general greater than that of simple molecules, and is greater in proportion as the molecule is more complex. This indicates that when the temperature is raised 1°, a certain portion of the heat is expended in urging asunder the constituents of the molecules, and that this portion is greater in proportion as the molecule is more complex.

*Heat-rays of different rates of vibration.*—The calorific spectrum shows that the heat emitted from a hot body is composed of rays of an infinite variety of rates of vibration, between certain limits. The physical cause of this fact will appear if we reflect that the heat-rays have their origin in the vibrations of the atomettes of the molecular atmospheres, and that these are situated at every variety of distance from the central atoms, between certain limits. For the circumstances of equilibrium of these atomettes are different, and their rates of vibration when displaced should be different. The fact that the most intense heat-rays, in ordinary cases of combustion, are those of low refrangibility, and the phenomena of the evolution of calorific and luminous rays when a body is heated to incandescence, indicate that the electric atomettes which are nearest the central atoms have the lowest rate of vibration.

We have seen that the expansive action of heat is a necessary consequence of the fundamental principle that the heat-pulses constitute a repulsive force, and that they are absorbed, more or less, by the molecular atmospheres which they encounter. It may be urged as an objection to the notion that radiant heat is a repulsive force, that bodies when heated do not exert any sensible repulsive action upon other contiguous bodies. We are not prepared to admit that experiment has furnished no evidence of such repulsive action, under any circumstances, but the entire absence of such action upon bodies of sensible magnitude would in fact be no decisive proof that waves of radiant heat do not convey a series of preponderating repulsive impulses; for if these impulses penetrate to the atoms of the molecules, they should be ultimately taken up by their atmospheres, and expended as an expansive force upon these atmospheres, and in urging the molecules asunder; and if they do not reach the atoms, no motion should be imparted to them. Since it is improbable that the more intense impulses of heat will be wholly absorbed by the particles immediately at the surface of the body upon which they fall, a direct repulsive action of the heat may take effect to a certain depth below the surface. Have we not in the spheroidal state of liquids evidence of such action exerted by the radiant heat from the hot vessel upon the liquid resting upon its interior surface?

It is only when the heat-waves impinge upon isolated particles, or a small group of particles, that a progressive motion should be imparted. This supposition is apparently realized in the case of cometary matter repelled by the sun.[4]

*Light.*

The question of the precise relation which the two physical agents, light and heat, bear to each other, has not been definitively settled; but the weight of evidence preponderates very decidedly in favor of the doctrine of their essential identity. The only "formidable outstanding objection" to this view consists in the fact that a strong light may be obtained which has little, if any, heating power.[5] According to Melloni, the greenish light, obtained by transmission of white light through a peculiar species of green glass colored by oxyd of copper, "exhibits no calorific action capable of being rendered perceptible by the most delicate thermoscopes, even when it is so concentrated by lenses as to rival the direct rays of the sun in brilliancy."

[4] In the article by the author, "On the Theoretical Determination of the Dimensions of Donati's Comet," (see this Journal, vol. xxxii, No. 94, p. 54, &c.) it is established, by rigorous calculation, that the particles of matter disseminated over the breadth of the tail of the comet, were exposed to a force of repulsion from the sun, of various degrees of intensity, between two ascertained limits. In the light of the theoretical views now offered, we must conclude that the matter thus unequally repelled consisted of particles of different sizes, or different absorptive powers for heat, or of different sized groups of particles.

[5] (See Report of Rev. Baden Powell, M.A., F.R.S., for 1854, on Radiant Heat, published in the Smithsonian Report for 1859, p. 368.)

May it not be that the explanation of the possible existence of light without heat, thus made out, is to be found in the presence, in the luminous beam, of a certain number of radiations which have individually too feeble an intensity to exercise any calorific action upon bodies, but are still capable of producing a sensible impression upon the organ of vision? Upon the theory of the constitution of molecules adopted in the present paper, we may suppose that vision results from some action of the luminous pulses upon the molecular atmospheres merely, while heat-expansion is not produced unless the individual pulses have sufficient intensity to penetrate to the surface of the central atoms of the molecules. According to this idea, as rays that penetrate to different depths in the molecular atmospheres of a medium should be unequally absorbed, the rays of feeble intensity, or of pure light, may be separated from those which by reason of their greater intensity are capable of penetrating to the atoms of the medium, and exercising a calorific action upon them.

It is generally admitted that vision is produced by the transverse vibrations of the rays of light. The fact that when two rays of heat, polarized in perpendicular planes, meet in opposite states of vibration, they do not neutralize each other, has been generally regarded as an indication that the calorific action of a ray must also result from transverse vibrations; but this does not appear to be a legitimate conclusion if we adopt Professor Challis's theory, that the luminiferous ether is a highly elastic fluid, having the same properties as elastic fluids in general, and that the ethereal undulations consist of simultaneous longitudinal and transverse vibrations, attended with variations in the density of the medium, as in the case of waves of sound. For if transverse vibrations, in perpendicular planes, meet in opposite states, they cannot neutralize each other, and must develop direct vibrations, which will take the place of those which counteract each other, and will exert a calorific action. In fact, Prof. Challis conceives that "heat is the result of the mechanical action of the direct vibrations;" while "light is due to the transverse vibrations."

The intimate association of heat and light leads to the inference that they emanate from the same source, viz: the molecular atmospheres of bodies; and if, as has been intimated, the two emanations are essentially the same, we infer that rays of light, as well as of heat, originate in vibratory movements of the atomettes of these atmospheres. The atomettes lying at different distances from the central atom of a molecule will have different rates of vibration, increasing with the distance, and so the waves proceeding from them will have every variety of pulsation between the lowest limit, answering to the bottom, and the highest, answering to the top of a molecular atmosphere. Accordingly the red rays will proceed from the lower portions of the atmosphere, and the violet from more elevated portions.

If the electric atmospheres diminish in density by insensible degrees from bottom to top, there should be no break in the continuity of the rays between the two extremes. But we know, from the existence of bright bands in the spectra obtained from colored flames, and from the highly heated vapors of metals and other substances, that the rates of vibration of the luminous rays given off by incandescent vapors, seldom, if ever, vary by insensible degrees from one extreme to the other. We must conclude therefore that the electric atmospheres of highly heated molecules are made up of distinct layers of unequal density.

*Phenomena*, attending the propagation of light. The *absorption* of light by a medium may be regarded as the taking up of the ethereal pulses by the electric atmospheres of the medium. In order that a ray may be completely absorbed it must encounter a layer of the electric atmosphere of a molecule, which pulsates naturally in unison with the wave-pulsation of the ray. The absorbing action of a molecule should therefore depend upon the physical condition of its atmosphere as to rates of pulsation, density, &c., and also upon the comparative extent of its electric and ethereal atmospheres. For example, a medium would permit the free passage of any ray that did not penetrate to the surface of the electric atmospheres of its molecules. On the other hand, a medium would intercept rays that should penetrate to atmospheric layers that are in unison with the rays. Accordingly, if an incandescent vapor should emit rays of certain colors, as shown by bright bands in its spectrum, those colors, if transmitted through the vapors, should be absorbed, and the spectrum given by transmitted light should show dark lines answering to the bright lines of the other spectrum—which is the well known discovery of Kirchhoff and Bunsen.

According to the theory of crystallization presented on p. 209, &c., in all the systems of crystallization in which the axes of molecular attraction are unequal, the electric atmospheres of the molecules are compressed unequally in the lines of direction of these axes. Now if these atmospheres are compressed unequally, the same will be true of the ethereal atmospheres which pervade them. Thus in all forms of crystals which have unequal axes, the ethereal atmospheres of its separate particles will have unequal densities in the directions of the molecular axes. It is well known that all such crystals have the property of double refraction, and that this property is attributed to a supposed inequality of density, or of elasticity, of the ether in the direction of certain molecular axes. A mechanical pressure exerted along a certain line, or plane, also develops the property of double-refraction, in ordinary refracting media; and such compression should give rise to an increased density of the ethereal atmospheres along the line of pressure. Accordingly our general theory of the constitntion of molecules, and of molecular forces, conducts to the physical basis assumed in the undulatory theory of light, in explanation of double-refraction.

The phenomenon of "atomic circular polarization" by liquids, discovered by Biot, who established that the effect depended solely upon the number of atoms encountered by the light, whatever may be the density of the medium, and the phenomenon of "magnetic circular polarization," in which the direction of rotation of the plane of polarization corresponds with that of the revolution of the circular magnetic currents, are decided indications that optical phenomena result mainly from the action of the *atmospheres* of molecules upon the rays of light. Numerous facts, which go to show that the absorptive action of media upon light and heat depends in a great degree upon the physical constitution of the separate molecules, confirm this conclusion.

The general phenomena, and laws, of reflexion, refraction, polarization, diffraction, &c., should obtain upon this supposition no less than upon the conception that the phenomena are to be referred to the interstitial ether. It remains to be seen whether the theorems and formulæ deduced by Fresnel, and other physicists, from the undulatory theory of light, can be shown to be substantially in accordance with this notion of molecular actions.

*Note.*—Objections to the theoretical views offered on the preceding pages, will readily occur to the scientific reader, but it does not comport with the design of the present communication to anticipate objections, or to attempt to enforce the general conclusions deduced from the fundamental positions taken, by appeals to special facts. We must be content, for the present, to rest our conclusions mainly upon general considerations. A connected view of the whole ground to be surveyed is almost a necessary preliminary to the many detailed investigations that must be undertaken before the theory can be established on a firm foundation.

It will be perceived that the most characteristic feature of the general theory under discussion is that it locates the source of physical phenomena in the atmospheres of molecules, instead of in the atoms, or in the interstitial ether. In pursuing our deductions into other departments of Physics, other general conceptions have been reached, some of which it may be advisable to state here; as circumstances may delay somewhat the publication of the remainder of the article.

1. An *electric current* (hydro-electric, or thermo-electric) has its origin in the opposite polarization of the adjacent sides of contiguous molecules, developed by the play of the molecular

forces, under special circumstances, or by an inequality in the action of the pulses of heat upon the atmospheres of the molecules. The current consists of an actual flow of electric ether from molecule to molecule, determined by a previous electric polarization propagated from that which is the source of the current. There are also conveyed in the direction of the positive current, streams of impulses, both by the electric, and universal ether, which, by a partial lateral dispersion, produce the magnetic effects of the current.

2. The *mutual attractions*, or *repulsions*, exerted between two electric currents may be ascribed to a change in the tension of the ether between the wires, produced by the lateral actions of the currents.

3. *Induction currents* result from an electric polarization of the molecules, suddenly induced by the same lateral action of a primary current when first established, or by the increased action of a previous electric, or magnetic current; or suddenly vanishing, when the circuit is broken, or the force of action decreases. The polarization, in the first instance, is the opposite to that which prevails in the primary current; owing to the indirect nature of the inducing action.

4. The *circular currents of a magnet*, consist of electric currents that follow continuous chains of particles lying in the surface of the compound molecules of the magnet. These currents have their origin in an electric polarization of the particles, developed by a direct action of the impulses propagated from the exciting current. In permanent magnets the polarization thus originated becomes permanently established, and a permanent current remains, *as a necessary consequence of the play of the molecular forces in the chain.* A magnet, therefore, derives its power directly from the inexhaustible primary forces of attraction and repulsion, and must retain its virtue unimpaired until the chain of molecules is broken by heat, or the excited molecular conditions upon which the currents depend are removed by some external cause.

5. *Terrestrial magnetism* is due to electric currents in the mass of the earth, running in the general direction from east to west, and developed by the collision of the molecules with the ether of space. Both the rotatory and orbital motion of the earth are concerned in producing these currents. A part of the force of such currents must be converted into heat, and the earth derive a portion of its heat from this source. If this be true the remarkable formal relations that subsist between the magnetism and heat of the earth, are probably the result, in a great degree, of this physical bond by which the two principles are partially united. (See the investigation, by the author, of these relations in an article on Terrestrial Magnetism, published in vol. iv, second series of this Journal).

This theory of the origin of terrestrial magnetism, as a part of the general theory of Molecular Physics, here presented, was brought, by the author, to the notice of the Connecticut Academy, about two years since. It appears, from a pamphlet recently received by the editors of this Journal, that a theory quite similar to this was propounded several years since by Gustav Hinrichs, of Copenhagen. The theory of Hinrichs, or one having the same essential features, I find is advocated by Prof. Challis, in the Number of the Philosophical Magazine published in February, 1861.

[To be continued.]

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ART. XXIII.—*Notice of the Remains of a Mastodon recently discovered in Michigan;* by Professor ALEXANDER WINCHELL.

SOME remains of a Mastodon have recently been exhumed in Michigan, which possess sufficient interest to deserve a scientific notice. They were found on Sec. 7, in the township of Adrian, Lenawee county, about seven miles northwest of the city of Adrian. The bones thus far discovered consist of the cranium, with the exception of the nasal bones; five molars, four of which lack their roots; the terminal portions of the two tusks, each about 18 inches long, and another fragment of a tusk a foot in length; two caudal vertebræ; two scapulæ; two femora; two tibiæ; one fibula; two calcanea; two humeri; one radius; sundry carpal, tarsal, metacarpal and metatarsal bones; three or four digital bones; three perfect ribs, and numerous fragments. The whole series of vertebræ (except two caudal) has disappeared, as well as the pelvis, the lower jaw, the nasal bones, about half of the bulk of the anterior extremities, and many of the ribs. The epiphyses of the long bones are nearly all detached, but most of them have been recovered. The state of ossification indicated by the separation of the epiphyses, the unworn teeth, and the inferior size of the bones, all tend to demonstrate that we have here the remains of a Mastodon not more than two-thirds grown.

These relics were found buried about two feet only beneath the surface of a small peat bog which was being ditched. Beneath the peat, which is not more than two and a half feet thick, we have marly clay, passing, at the depth of four feet, into loose sand. The skeleton was lying on the side. One fore-leg was extended; the other bent under the body. The hinder parts were a little the deepest in the mire. It is stated that many years ago this spot was known as a "deer lick."

It is generally supposed that the occurrence of elephantine remains in miry bogs indicates the mode of death of these ponderous quadrupeds. It may be doubted, however, whether their occurrence exclusively in peat, or beneath it, is not attributable to the antiseptic properties of that substance.

The bog in which the present remains were found, is perfectly identical with thousands of others in our State, which are known, from observation, to be in process of formation in the sites of ancient lakelets, and at a rate which argues a comparatively short duration for the alluvial period of the State. Indeed, the watery and shaking condition of this bog, with the thinness of the peaty stratum, furnish data for the belief that it was the bed of a lakelet within a comparatively short period. It is much more credible that the Mastodon under consideration was living within 500 or 1000 years, than that an interval of time, greater than the age of the human race, has been occupied in the accumulation of two or three feet of vegetable deposits, under circumstances which suffer the same work to be accomplished, in neighboring localities, within the space of a human life-time. It is more than probable that the American Indian, according to his own traditions, and according to the evidences adduced by Dr. Koch, has listened to the thunder-waking tread of these monsters of the forest and the field.

Other mastodon remains have been found at various points within the lower peninsula of Michigan, some of which are Petersburg, Monroe county; the city of Adrian, Lenawee county; Utica, Macomb county; Green Oak, Livingston county; Fentonville, Oakland county; and Terre Coupée, Berrien county. (See Proc. Bos. Soc. Nat. Hist., v, 133, 146, 158.) The localities of several other discoveries have been lost. The molar teeth of *Elephas Jacksoni* are also of occasional occurrence, in the same situations; as well as the antlers of the deer and American elk. Some years ago, the caudal vertebra of a Cetacean was identified by Dr. Sager from the western portion of the State.

The remains of the Mastodon noticed above will probably be secured for the Museum of the University, when an occasion may be furnished for a fuller account of the fossil mammals of Michigan.

University of Michigan, June 16, 1864.

ART. XXIV.—*Chladnite of the Bishopville Meteoric Stone proved to be a Magnesian Pyroxene;* by J. LAWRENCE SMITH, Prof. Chem. Med. Dep. University of Louisville.

IN 1846, Prof. C. U. Shepard published an account of an exceedingly interesting meteoric stone that fell at Bishopville, South Carolina, in 1843, differing in its external character from other meteoric stones; the fractured mass being exceedingly white, except where metallic iron and other associate minerals occur. I would refer the reader to Prof. Shepard's description of it in this Journal, Sept. 1846, p. 381.

The composition of the snow-white mineral (constituting about 90 pr. ct. of the entire mass) as given by Prof. Shepard is—

| | | Oxygen. | Ratio of ox. |
|---|---|---|---|
| Silica, | 70·41 | 35·205 | 3 |
| Magnesia, | 28·25 | 11·300 | 1 |
| Soda, | 1·39 | ·338 | |

From the results of this analysis he considered it a *tersilicate of magnesia*, constituting a new species to which he gave the name *chladnite.*

Several years after this examination, a fragment of this meteoric stone came into my possession, and separating a small portion of the mineral in question it was examined. The result of this incomplete examination justified the statement in a note to a memoir of mine on meteorites, presented to the Amer. Scientific Association in April, 1854, and published in this Journal for March, 1855, p. 162, "that from some investigations just made, chladnite is likely to prove a pyroxene."

Since that announcement I have been placed in possession of other fragments of the meteorite, and have been able to separate the "chladnite" perfectly pure, and in sufficient quantity to submit it to a thorough analysis.

To render the chladnite soluble in acid, it was fused with four times its weight of carbonate of soda and potash, with a small fragment of caustic potash placed on the top of the mixed powders in the crucible.[1] After fusion, the analysis was proceeded with in the ordinary way; the results of two analyses were as follows:

| | 1. | 2. |
|---|---|---|
| Silica, | 60·12 | 59·83 |
| Magnesia, | 39·45 | 39·22 |
| Peroxyd of iron, | ·30 | ·50 |
| Soda, with feeble potash and strong lithia reaction, | ·74 | ·74 |
| | 100·61 | 100·29 |

[1] I would remark that I seldom or ever fuse a silicate with the alkaline carbonates, without the addition of a small piece of caustic potash or soda, and never analyze a known or supposed pyroxene or hornblende without this precaution. I have no doubt that there are many minerals classified with hornblende, which properly belong to pyroxene, the silica in the analyses being rated too high, an error arising from an imperfect fusion.

The minute quantity of peroxyd of iron came from exceedingly fine particles of iron diffused through the minerals, and could be seen by a magnifying glass. One separate analysis was made for the soda.

The constitution of the mineral, as made out from the numbers in analysis 1, is—

| | Oxygen. | Oxygen ratio. |
|---|---|---|
| Silica, - - - - - | 31·22 | 2 |
| Magnesia, - - - - | 15·51 } | 1 |
| Soda, - - - - - | ·19 } | |

corresponding to the formula $\dot{M}g^3\,\ddot{S}i^2$, equivalent to the general formula of pyroxene, $\dot{R}^3\,\ddot{S}i^2$.

The excess of silica obtained by Prof. Shepard in his analysis is doubtless due to an imperfect fusion of the mineral with the carbonate of soda, an error easily made, if the precautions I have already mentioned are not attended to.

"Chladnite" approaches those forms of pyroxene known as white augite, diopside, white coccolite, &c.; these last named minerals having a part of the magnesia replaced by lime. It is identical in composition with *Enstatite* of Kenngott, a pyroxenic mineral from Aloysthal in Moravia (this Journal, [2], xxi, 200).

From these observations it will be seen that the Bishopville meteoric stone, however different in external characteristics from other similar bodies, is, after all, identical with the great family of pyroxenic meteoric stones.

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## Art. XXV.—*Aerial Tides*; by Pliny Earle Chase, M.A., S.P.A.S.[1]

The remarkable coincidence, which I have pointed out, between the theoretical effects of rotation and the results of barometrical observations, has led me to extend my researches with a view of defining more precisely some of the most important effects of lunar action on the atmosphere. The popular belief in the influence of the moon on the weather, which antedates all historical records, has received at various times a certain degree of philosophical sanction. Herschel and others have attempted partially to formulate that influence by empirical laws; but the actual character of the lunar wave that is daily rolled over our heads appears never to have been investigated. Major-General Sabine showed that the moon produces a diurnal variation of the barometer, amounting to about ·006 of an inch, which is

[1] From the Proceedings of the American Philosophical Society.

equivalent to nearly one-tenth of the average daily variation near the equator. This would indicate a tidal wave of rather more than one foot for each mile's depth of atmosphere, or from three to six feet near the summits of the principal mountain chains. It is easy to believe that the rolling of such a wave over the broken surface of the earth may exert a very important influence on the atmospheric and magnetic currents, the deposition of moisture, and other meteorological phenomena. As the height of the wave varies with the changing phases of the moon,[1] its effects must likewise vary, in accordance with mathematical laws, the proper study of which must evidently form an important branch of meteorological science.[2]

Besides this daily wave, there appears to be a much larger, but hitherto undetected, weekly wave. Mr. Flaugergues,[3] an astronomer at Viviers in France, extended his researches through a whole lunar cycle, from Oct. 19, 1808, to Oct. 18, 1827, and he inferred, from his observations:

1. That, in a synodical revolution of the moon, the barometer rises regularly from the second octant, when it is the lowest, to the second quadrature, when it is the highest, and then descends to the second octant.

2. That the varying declination of the moon modifies her influence, the barometer being higher in the northern lunistice than in the southern.

3. That the action of the moon also varies with its distance from the earth, the mean barometric height being less in perigee than in apogee.

The observations indicate the following average meridional fluctuations of the barometer:

1. In a semi-synodical revolution, 1·67 mm., or ·065 in.
2. Between the lunistices, ·29 mm., or ·011 in.
3. Between perigee and apogee, 1·12 mm., or ·044 in.

The more recent and more complete observations at St. Helena give somewhat different results, which serve to confirm the natural *à priori* conviction that there must be two maxima and minima in each month. The means of three years' hourly observations indicate the existence of waves, which produce in the first quarter a barometric effect of +·004 in.; in the second quarter of −·016 in.; in the third quarter of +·018 in.; and in the fourth quarter of −·006 in.; results which appear to be *precisely* accordant, in their general features, with those which would be naturally anticipated from the combination of the cu-

[1] The height at St. Helena appears to fluctuate between about ·9 and 1·6 feet.
[2] For some interesting experimental evidences of the effect of the moon's changes on the fall of rain, see the published observations of Messrs. F. Marcet (this Journal, xxvii, 192); and J. H. Alexander (this Journal, [2], xii, 1).
[3] Bib. Univ., Dec. 1827, and this Journal, xv, 174.

mulative effect of the moon's attraction with the daily wave of rotation, and the resistance of the æther.

One peculiarity of this lunar-aerial wave deserves notice for the indirect confirmation that it lends to the rotation theory of the daily aerobaric tides, and the evidence it furnishes of opposite tidal effects, which require consideration in all investigations of this character. When the daily lunar tides are highest their pressure is greatest, the lunar influence accumulating the air directly under the meridian, so as to more than compensate for the diminished weight consequent upon its "lift." But in the general aerial fluctuations, as we have seen, and also in the weekly tides, a high wave is shown by a low barometer, and *vice versâ.* The daily blending of heavy and light waves produces oscillations, which are indicated by the alternate rise and fall of the barometer and thermometer at intervals of two or three days.

Mr. Flaugergues's observations at perigee and apogee seem to show that a portion of the movement of the air by the moon is a true lift, which, like the lift of rotation, must probably exert an influence on the thermometer as well as on the barometer. On comparing the daily averages at each of the quadratures and syzygies, I found the difference of temperature too slight to warrant any satisfactory inference; but a similar comparison of the hourly averages, at hours when the sun is below the horizon, gave such results as I anticipated, as will be seen by a reference to the following

*Table of barometric and thermometric means at the moon's changes.*

| Moon's phase. | Average height of barometer in inches. | Height of weekly tides. | Height of daily tides. | Height of thermom. daily average. | Thermom. at 12 P. M. | Thermom. at 4 A. M. |
|---|---|---|---|---|---|---|
| Full,........ | 28·270 | −·0115 in. | ·0054 in. | 61·67° | 60·22° | 59·787° |
| Third qr.,... | 28·289 | +·0065 " | ·0087 " | 61·68 | 60·41 | 59·824 |
| New, ....... | 28·282 | +·0005 " | ·0064 " | 61·65 | 60·31 | 59·716 |
| First qr., ... | 28·286 | +·0044 " | ·0047 " | 61·63 | 60·37 | 59·823 |

In obtaining the above averages I was obliged to interpolate for such changes as took place on Sundays or holidays, when no observations were taken. The interpolation, however, does not affect the general result; and, on some accounts, the table is more satisfactory than if the observations had been made with special reference to a determination of the lunar influences, accompanied as such a reference would very likely have been, by a bias to some particular theory.

The thermometric and barometric averages show a general correspondence in the times of the monthly maxima and minima,—the correspondence being most marked and uniform at midnight, when the air is most removed from the direct heat of the sun, and we might therefore reasonably expect to find the clearest evidences of the relation of temperature to lunar attraction.

By taking the difference between the successive weekly tides, we readily obtain the amount of barometric effect in each quarter. The average effect is more than three times as great in the second and third quarters, as in the remaining half month,—a fact which suggests interesting inquiries as to the amount of influence attributable to varying centrifugal force, solar conjunction, or opposition, temperature, &c.

Although, as in the ocean tides, there are two simultaneous corresponding waves on opposite sides of the earth, these waves are not of equal magnitude, the barometer being uniformly higher when the moon is on the inferior meridian, and its attraction is therefore exerted in the same direction as the earth's, than when it is on the superior meridian, and the two attractions are opposed to each other.

I find, therefore, marked evidences of the same lunar action on the atmosphere as on the ocean,—the combination of its attraction with that of the sun producing, both in the air and water, spring tides at the syzygies, and neap tides at the quadratures; and I believe that the most important normal atmospheric changes may be explained by the following theory:

The attraction and rotation-waves, as will be readily seen, have generally opposite values, the luni-solar wave being

Descending, from 0° to 90° [*], and from 180° to 270°.
Ascending, from 90° to 180°, and 270° to 360°.

While a rotation wave is

Ascending, from 330° to 60°, and 150° to 240°.
Descending, from 60° to 150°, and 240° to 330°.

From 60° to 90°, and 240° to 270°, both waves are descending while from 150° to 180°, and 330° to 360°, both are ascending. In consequence of this change of values, besides the principal lunar maxima and minima at the syzygies and quadratures, there should be secondary maxima and minima at 60° in advance of those points.

The confirmation of these theoretical inferences by the St. Helena observations appears to me to be quite as remarkable as that of my primary hypothesis. If we arrange those observations in accordance with the moon's position, and take the average daily height of the barometer, we obtain the following

[*] Counting from either syzygy.

*Table of the lunar barometric tides.*

| Moon's position. θ* | Mean daily height of barometer at St. Helena. 28 inches + the numbers in the Table. | | | |
|---|---|---|---|---|
| | 1844. | 1845. | 1846. | Average. 1844-6. |
| 0° | ·2621 | ·3020 | ·2701 | ·2781 |
| 15 | ·2650 | ·3058 | ·2693 | ·2800 |
| 30 | ·2707 | ·3153 | ·2707 | ·2856 |
| 45 | ·2691 | ·3165 | ·2688 | ·2848 |
| 60 | ·2625 | ·3077 | ·2688 | ·2797 |
| 75 | ·2682 | ·3093 | ·2783 | ·2853 |
| 90 | ·2667 | ·3184 | ·2800 | ·2884 |
| 105 | ·2593 | ·3170 | ·2721 | ·2828 |
| 120 | ·2595 | ·3124 | ·2686 | ·2802 |
| 135 | ·2677 | ·3099 | ·2691 | ·2822 |
| 150 | ·2712 | ·3118 | ·2715 | ·2848 |
| 165 | ·2710 | ·3104 | ·2735 | ·2850 |
| 180 | ·2621 [5] | ·3020 | ·2701 | ·2781 |

This table shows—

1. That the average of the three years corresponds *precisely* with the theory, except in the secondary maximum, which was one day late.

2. That the primary maximum occured at the quadratures in 1845 and 1846, and one day before the quadratures in 1844.

3. That the primary minimum occurred at the syzygies in 1844 and 1845, and one day after the syzygies in 1846.

4. That 1846 was a disturbed year; and, if it were omitted from the table, each of the remaining years, as well as the average, would exhibit an entire correspondence with theory, except in the primary maximum of 1844.

5. That 1845 was a normal year, the primary and secondary maxima and minima all corresponding with theory both in position and relative value.

6. That the deviations from perfect correspondence with theory can be easily explained by the relative positions of the two aerial ellipsoids of rotation and attraction.

7. That the tertiary maxima and minima, or the turning-points between the primary and secondary maxima and minima, are less stable than the primaries and secondaries.

At extra-tropical stations I should look for important modifications of the theoretical results, some of which I propose to explain hereafter.

In a former communication on the rotation-tide, I stated that "the law of tidal variation, derived from an exclusive reference of the aerial motions to a supposed stationary earth, is precisely

* Counting from either syzygy.

[5] Since the tabular numbers represent the *semi-axes* of the barometric curve, and not the simple *ordinates*, the values for 0° and 180° are the same.

the same as the law that is derived from the consideration of the relative attraction of the two bodies revolving about their common centre of gravity." That such would be the case might have been reasonably expected from the dependent connection of rotation and revolution with gravity.

I was therefore led to believe that the daily lunar tides might be indicated by the same expression as the weekly lunar and daily rotation tides. On investigation, I find that such is indeed the case. If M is the barometric mean for any given day and place, and $\theta$ is the moon's altitude, the lunar tide may be expressed by M C $(\sin\theta\cos\theta)$, C being a constant to be determined for each station.

The rationale of Mr. Flaugergues's second and third inferences thus becomes evident; the phenomena of ocean tides are connected with those of the air, which, being subject to fewer extraneous disturbing influences, can therefore be more easily investigated; and the long-suspected obedience of the principal meteorological changes to fixed mathematical laws is at length demonstrated.

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ART. XXVI.—*Extracts from the Address of Dr. J. W. Dawson, President of the Natural History Society of Montreal, at its annual meeting, May* 18, 1864.[1]

### *Fossils of the Laurentian.*

BY far the most important publication of the past year, in the Natural History of Canada, has been the great Report of the Geological Survey, a work in which, as the achievement of members of this Society, we may very well take pride; and on which we may congratulate ourselves as facilitating the labors of those among us who pay attention to geology, either with a view to practical or scientific results, and as greatly raising the scientific reputation of this country. The Report of the Survey has already been reviewed in the *Naturalist*, and I propose here, not so much to say anything as to its general merits, as to refer to a few points in Canadian geology to which it directs our attention.

One of these is the discovery of fossils in the old Laurentian rocks, heretofore usually named *Azoic*, as being destitute of life, and much older than any rocks known to contain fossils. The oldest remains of living beings, until this discovery, had been found in rocks known as Cambrian, or Primordial, and equivalent in age to our oldest Silurian of Canada, or at the most to our Huronian. But the Huronian series in Canada rests on the

[1] From the Canadian Naturalist, 1864.

upturned edges of the Laurentian, which had been hardened and altered before the Huronian series was deposited. Again, Sir William Logan has shown that the Laurentian system itself contains two distinct series of beds, the upper of which rests unconformably on the lower. There are thus in Canada at least two great series of rocks, of such thickness as to indicate two distinct periods, each of vast length, below the lowest fossiliferous rocks of other countries. Yet, in the lowest of these so-called Azoic groups, fossils have now been found; Canada thus far distancing all other parts of the world, so far as yet known, in the antiquity of its oldest fossils.

I have had the happiness to submit these remarkable specimens to microscopic examination, at the request of Sir W. E. Logan, and have arrived at the conclusion that they are of animal nature, and belong to the very humblest type of animal existence known, that of the *Rhizopods*, though they far outstrip in magnitude any known modern representation of that group. The discovery of this remarkable fossil, to be known as the *Eozoön Canadense*, will be one of the brightest gems in the scientific crown of the Geological Survey of Canada.

In connection with this subject, it is to be observed that the grand order of succession in the older number of the Laurentian system of rocks seems to be the same with that so often represented in other parts of the geological scale. First, a coarse fragmentary series, represented by conglomerate and gneiss; next, a calcareous and fossiliferous band, represented by the Eozoön limestones; next, a finer earthy series, represented by dioritic rocks. This brings the Laurentian into a cycle somewhat similar to that of the Potsdam sandstone, the Chazy and Trenton limestone, and the Utica slate and Hudson river in the Lower Silurian; or to that of the Medina sandstone, the Niagara limestone, and Lower Helderberg in the Upper Silurian; or to that of the Oriskany sandstone, Corniferous limestone, and Hamilton and Chemung groups in the Devonian; or to that of the Lower Carboniferous conglomerates and sandstones, the Carboniferous limestones, and the Coal measures in the Carboniferous period. This recurrence of cycles of deposit cannot be accidental. It is more or less to be seen throughout the geological scale, and in all countries; and, as I have elsewhere pointed out, it includes numerous subordinate cycles within the same formation, as in the coal measures. Eaton, Hunt, and Dana have called attention to it; but it deserves a more careful study as a means of settling the sequence of oscillations of land and water in connection with the succession of life. It will also be important in giving fixity to our geological classifications, and may eventually aid in establishing more precise views of the dynamics of geology and of the lapse of geological time. The progress of the earth has, like most other kinds of progress, been not by a continuous evolution, but by a series of cycles, of great summers and winters, or days and nights of physical and vital change, in each of which all things seem to revolve back to the place of beginning, only to begin a new cycle, or new turn of a spiral, similar to the last in its general course, though altogether different in its details, accompaniments and results.

*On the Boulder Drift of Canada.*

There is another subject of great geological importance, on which the publication of the report enables strong ground to be taken. I refer to the conditions under which the *Boulder Drift* of Canada was deposited. It has been customary to refer this to the action of ice-laden seas and currents, on a continent first subsiding and then re-elevated. But this opinion has recently been giving way before a re-assertion of the doctrine that land glaciers have been the principal agents in the distribution of the boulder drift, and in the erosions with which it was accompanied. I confess that I have steadily rejected this last doctrine, being convinced that insuperable physical and meteorological objections might be urged against it, and that it was not in accordance with the facts which I had myself observed in Nova Scotia and in Canada. The additional facts contained in the present report enable me to assert with confidence, though with all humility, that glaciers could scarcely have been the agents in the striation of Canadian rocks, the transport of Canadian boulders, or the excavation of Canadian lake basins. In making this statement I know that I differ in some degree from many of my geological friends, but I know that they will be rejoiced that I should freely and frankly state the reasons of my belief.

The facts to be accounted for are the striation and polishing of rock surfaces, the deposit of a sheet of unstratified clay and stones, the transport of boulders from distant sites lying to the northward, and the deposit on the boulder clay of beds of stratified clay and sand, containing marine shells. The rival theories in discussion are: *first*, that which supposes a gradual subsidence and re-elevation, with the action of the sea and its currents, bearing ice at certain seasons of the year; and, *secondly*, that which supposes the American land to have been covered with a sheet of glacier several thousands of feet thick.

The last of these theories, without attempting to undervalue its application to such regions as those of the Alps, or of Spitzbergen or Greenland, has appeared to me inapplicable to the drift deposits of eastern America for the following, among other reasons:

1. It requires a series of suppositions unlikely in themselves

and not warranted by facts. The most important of these is the coincidence of a wide spread continent and a universal covering of ice in a temperate latitude. In the existing state of the world it is well known that the ordinary conditions required by glaciers in temperate latitudes are elevated chains and peaks extending above the snow line; and that cases in which, in such latitudes, glaciers extend nearly to the sea level, occur only where the mean temperature is reduced by cold ocean currents approaching to high land, as for instance in Terra del Fuego and the southern extremity of South America. But the temperate regions of North America could not be covered with a permanent mantle of ice under the existing conditions of solar radiation; for, even if the whole were elevated into a table land, its breadth would secure a sufficient summer heat to melt away the ice, except from high mountain peaks. Either then there must have been immense mountain chains which have disappeared, or there must have been some unexampled astronomical cause of refrigeration as, for example, the earth passing into a colder portion of space, or the amount of solar heat being diminished. But the former supposition has no warrant from geology, and astronomy affords no evidence for the latter view, which besides would imply a diminution of evaporation militating as much against the glacier theory as would an excess of heat. An attempt has recently been made by Professor Frankland to account for such a state of things by the supposition of a higher temperature of the sea, along with a colder temperature of the land; but this inversion of the usual state of things is unwarranted by the doctrine of the secular cooling of the earth, it is contradicted by the fossils of the period, which show that the seas were colder than at present; and if it existed, it could not produce the effects required, unless a preternatural arrest were at the same time laid on the winds, which spread the temperature of the sea over the land. The alleged facts observed in Norway, and stated to support this view, are evidently nothing but the results ordinarily observed in ranges of hills, one side of which fronts cold sea water, and the other land warmed in summer by the sun.

2. It seems physically impossible that a sheet of ice, such as that supposed, could move over an uneven surface, striating it in directions uniform over vast areas and often different from the present inclinations of the surface. Glacier ice may move on very slight slopes, but it must follow these, and the only result of the immense accumulation of ice supposed would be to prevent motion altogether by the want of slope or the counteraction of opposing slopes, or to induce a slight and irregular motion toward the margins or outward from the more prominent protuberances.

It is to be observed, also, that, as Hopkins has shown, it is only the *sliding* motion of glaciers that can polish or erode surfaces, and that any internal changes resulting from the mere weight of a thick mass of ice resting on a level surface, would have little or no influence in this way.

3. The transport of boulders to great distances, and the lodgment of them on hill tops could not have been occasioned by glaciers. These carry downward the blocks that fall on them from wasting cliffs. But the universal glacier supposed could have no such cliffs from which to collect, and it must have carried boulders for hundreds of miles, and left them on points as high as those they were taken from. On the Montreal Mountain, at a height of 600 feet above the sea, are huge boulders of feldspar from the Laurentide hills, which must have been carried from 50 to 200 miles from points of scarcely greater elevation, and over a valley in which the striæ are in a direction nearly at right angles with that of the probable driftage of the boulders. Quite as striking examples occur in many parts of this country. It is also to be observed that boulders, often of large size, occur scattered through the marine stratified clays and sands containing sea shells; and whatever views may be entertained as to other boulders, it cannot be denied that these have been borne by floating ice. Nor is it true, as has been often affirmed, that the boulder clay is destitute of marine fossils. At Murray Bay and St. Nicholas, on the St. Lawrence, and also at Cape Elizabeth, near Portland, there are tough stony clays of the nature of true "till," and in the lower part of the drift, which contain numerous marine shells of the usual Post-pliocene species.

4. The Post-pliocene deposits of Canada, in their fossil remains and general character, indicate a gradual elevation from a state of depression, which, on the evidence of fossils, must have extended to at least 500 feet, and on that of far-travelled boulders to nearly ten times that amount; while there is nothing but the boulder-clay to represent the previous subsidence, and nothing whatever to represent the supposed previous ice-clad state of the land, except the scratches on the rock surfaces, which must have been caused by the same agency which deposited the boulder clay.

5. The peat deposits with fir roots, found below the boulder clay in Cape Breton, the remains of plants and land snails in the marine clays of the Ottawa, and the shells of the St. Lawrence clays and sands, show that the sea, at the period in question, had much the temperature of the present Arctic currents of our coasts, and that the land was not covered with ice, but supported a vegetation similar to that of Labrador and the north shore of the St. Lawrence at present. This evidence refers not to the later period of the Mammoth and Mastodon, when the re-elevation was perhaps nearly complete, but to the earlier period con-

temporaneous with, or immediately following, the supposed glacier period. In my former papers on the Post-pliocene of the St. Lawrence, I have shown that the change of climate involved is no greater than that which may have been due to the subsidence of land and change of course of the Arctic current, actually proved by the deposits themselves.

These objections might be pursued to much greater length; but enough has been said to show that there are, in the case of northeastern America, strong reasons against the existence of any such period of extreme glaciation, as supposed by many geologists; and that if we can otherwise explain the rock-striation and polishing, and the formation of fiords and lake basins, the strong points with these theorists, we can dispense altogether with the portentous changes in physical geography involved in their views, and which are not necessary to explain any of the other phenomena.

It is on these points, more especially, that the Report of the Geological Survey throws new light; though Sir William, with his usual caution, has not committed himself to theoretical conclusions; and in one or two local cases he seems to favor the glacier theory. It has long been known to geologists, that in northeastern America, two main directions of striation of rock surfaces occur, from northeast to southwest, and from northwest to southeast; and that locally the directions vary from these to north and south, and east and west. Various attempts have been made, but without much success, to account for these directions of striation by the motion of glaciers; and while it is quite easy for any one prepossessed with this view to account in this way for the striation in a particular valley or part of a valley, so may exceptional facts occur as to throw doubt on the explanation, except in the case of a few of the smaller and steeper mountain gorges.

In the Report of the Survey of Canada, a valuable table of these striations is given, from which it appears that they are locally distributed in such a way as to throw a decided gleam of light on their origin.

It would seem that the dominant direction in the valley of the St. Lawrence, along the high lands to the north of it, and across Western New York, is northeast and southwest; and that there is another series of scratches running nearly at right angles to the former, across the neck of land between Georgian Bay and Lake Ontario, down the valley of the Ottawa, and across parts of the Eastern Townships, connecting with the prevalent southeast striation which occurs in the valleys of the Connecticut and Lake Champlain, and elsewhere in New England. What were the determining conditions of these two courses, and were they contemporaneous or distinct in time? The first point to be settled, in answering these questions, is the direction of the force which caused the striæ. Now, I have no hesitation in asserting, from my own observations, as well as from those of others, that for the southwest striation the direction was *from the ocean toward the interior, against the slope of the St. Lawrence valley.* The crag and tail forms of all our isolated hills, and the direction of transport of boulders carried from them, show that throughout Canada the movement was from northeast to southwest.[2] This at once disposes of the glacier theory for the prevailing set of striæ; for we cannot suppose a glacier moving from the Atlantic up into the interior. On the other hand, it is eminently favorable to the idea of ocean drift. A subsidence of America, such as would at present convert all the plains of Canada, and New York, and New England, into sea, would determine the course of the Arctic current over this submerged land from northeast to southwest; and as the current would move *up a slope*, the ice which it bore would tend to ground and grind the bottom as it passed into shallower water; for it must be observed that the character of slope which enables a glacier to grind the surface, may prevent ice borne by a current from doing so, and *vice versâ.*

Now we know that in the Post-pliocene period Eastern America was submerged, and consequently the striation at once comes into harmony with other geological facts. We have of course to suppose that the striation took place during submergence, and that the process was slow and gradual, beginning near the sea and at the lower levels, and carried upward to the higher grounds in successive centuries, while the portions previously striated were covered with deposits swept down from the sinking land, or dropped from melting ice. It would be easy to show that this view corresponds with many of the minor facts.

Further, the facts thus ascertained account for the excavation of the deep and land-locked basins of our great American lakes. Ocean currents, if cold, and clinging to the bottom, must cut out pot-holes, just as rivers do, though geologists are too apt to limit their function to the throwing up of banks. The course of the present Arctic current along the American coast has its deep hollows as well as its sand-banks. Our American lake basins are cut out deeply into the softer strata. Running water on the land would not have done this, for it could have no outlet; nor could this result be effected by breakers. Glaciers could not have effected it; for even if the climatal conditions for these were admitted, there is no height of land to give them momentum. But if we suppose the land submerged so that the Arctic current flowing from the northeast should pour over the Lau-

[2] The few exceptional cases appear to belong mostly to the later period of the stratified sands.

rentian rocks, on the north side of Lake Superior and Lake Huron, it would necessarily cut out of the softer Silurian strata just such basins, drifting their materials to the southwest. At the same time, the lower strata of the current would be powerfully determined through the strait between the Adirondac and Laurentide hills, and flowing over the ridge of hard rock which connects them at the Thousand Islands, would cut out the long basin of Lake Ontario, heaping up at the same time, in the lee of the Laurentian ridge, the great mass of boulder-clay which intervenes between Lake Ontario and Georgian Bay. Lake Erie may have been cut by the flow of the upper layers of water over the Middle Silurian escarpment; and Lake Michigan, though less closely connected with the direction of the current, is like the others due to the action of a continuous eroding force on rocks of unequal hardness.

The predominant southwest striation and the cutting of the upper lakes demand an outlet to the west for the Arctic current. But both during depression and elevation of the land, there must have been a time when this outlet was obstructed, and when the lower levels of New York, New England and Canada were still under water. Then the valley of the Ottawa, that of the Mohawk, and the low country between Lakes Ontario and Huron, and the valleys of Lake Champlain and the Connecticut, would be straits or arms of the sea, and the current, obstructed in its direct flow, would set principally along these, and act on the rocks in north and south, and northwest and southeast directions. To this portion of the process I would attribute the northwest and southeast striation. It is true this view does not account for the southeast striæ observed on some high peaks in New England; but it must be observed that even at the time of greatest depression, the Arctic current would cling to the northern land or be thrown so rapidly to the west that its direct action might not reach such summits.

Nor would I exclude altogether the action of glaciers in eastern America, though I must dissent from any view which would assign to them the principal agency in our glacial phenomena. Under a condition of the continent in which only its higher peaks were above the water, the air would be so moist and the temperature so low, that permanent ice may have clung about mountains in the temperate latitudes. The striation itself shows that there must have been extensive glacers, as now in the extreme Arctic regions. Yet I think most of the alleged instances must be founded on error, and that old sea beaches have been mistaken for moraines. I have failed to find even in the White mountains any distinct sign of glacier action, though the action of the ocean breakers is visible almost to their summits; and though I have observed in Canada and Nova Scotia many old sea beaches, gravel ridges, and lake margins, I have seen nothing that could fairly be regarded as the work of glaciers. The so-called moraines, in so far as my observation extends, are more probably shingle beaches and bars, old coast lines loaded with boulders, "trains" of boulders, or "ozars." Most of them convey to my mind the impression of ice-action along a slowly subsiding coast, forming successive deposits of stones in the shallow water, and burying them in clay and smaller stones as the depth increased. These deposits were again modified during emergence, when the old ridges were sometimes bared by denudation and new ones heaped up.

I shall close these remarks, perhaps already too tedious, by a mere reference to the alleged prevalence of lake basins and fiords in high northern latitudes, as connected with glacial action. In reasoning on this, it seems to be overlooked that the prevalence of disturbed and metamorphic rocks over wide areas in the north is one element in the matter. Again, cold Arctic currents are the cutters of basins, not the warm surface currents. Further, the fiords on coasts, like the deep lateral valleys of mountains, are evidences of the action of the waves rather than of that of ice. I am sure that this is the case with the numerous indentations of the coast of Nova Scotia, which are cut into the softer and more shattered bands of rock, and show in raised beaches and gravel ridges, like those of the present coast, the levels of the sea at the time of their formation.

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## ART. XXVII.—*On Celestial Dynamics*;[1] by J. R. MAYER.

THE surface of the sun measures 115,000 millions of square miles, or 6⅓ trillions of square metres; the mass of matter which in the shape of asteroids falls into the sun every minute is from 94,000 to 188,000 billions of kilograms; one square metre of solar surface, therefore, receives on an average from 15 to 30 grams of matter per minute.

To compare this process with a terrestrial phenomenon, a gentle rain may be considered which sends down in one hour a layer of water 1 millimetre in thickness (during a thunder-storm the rainfall is often from ten to fifteen times this quantity); this amounts on a square metre to 17 grams per minute.

The continual bombardment of the sun by these cosmical masses ought to increase its volume as well as its mass, if centrifugal[2] action only existed. The increase of volume could scarcely be appreciated by man; for if the specific gravity of these cosmical masses be assumed to be the same as that of the sun, the

[1] Extracted from the L. E. and D. Phil. Mag., [4], xxv, 399–402, and continued from vol. xxxvii, p. 198 of this Journal.

[2] [Centripetal?—TR.]

enlargement of his apparent diameter to the extent of one second, the smallest appreciable magnitude, would require from 33,000 to 66,000 years.

Not quite so unappreciable would be the increase of the mass of the sun. If this mass, or the weight of the sun, were augmented, an acceleration of the motion of the planets in their orbits would be the consequence, whereby their times of revolution round the central body would be shortened. The mass of the sun is 2·1 quintillions of kilograms; and the mass of cosmical matter annually arriving at the sun stands to the above as 1 to from 21 to 42 millions. Such an augmentation to the weight of the sun ought to shorten the sidereal year from $\frac{1}{42,000,000}$th to $\frac{1}{85,000,000}$th of its length, or from $\frac{3}{4}$ths to $\frac{3}{8}$ths of a second.

The observations of astronomers do not agree with this conclusion; we must therefore fall back on the theory mentioned at the beginning of this chapter, which assumes that the sun, like the ocean, is constantly losing and receiving equal quantities of matter. This harmonizes with the supposition that the *vis viva* of the universe is a constant quantity.

## VII. *The Spots on the Sun's Disc.*

The solar disk presents, according to Sir John Herschel, the following appearance. "When the sun is observed through a powerful telescope provided with colored glasses in order to lessen the heat and brightness which would be hurtful to the eyes, large dark spots are often seen surrounded by edges which are not quite so dark as the spots themselves, and which are called penumbræ. These spots, however, are neither permanent nor unchangeable. When observed from day to day, or even from hour to hour, their form is seen to change; they expand or contract, and finally disappear; on other parts of the solar surface new spots spring into existence where none could be discovered before. When they disappear, the darker part in the middle of the spot contracts to a point and vanishes sooner than the edges, Sometimes they break up into two or more parts that show all the signs of mobility characteristic of a liquid, and the extraordinary commotion which it seems only possible for gaseous matter to possess. The magnitude of their motion is very great. An arc of 1 second, as seen from our globe, corresponds to 465 English miles on the sun's disk; a circle of this diameter, which measures nearly 220,000 English square miles, is the smallest area that can be seen on the solar surface. Spots, however, more than 45,000 English miles in diameter, and, if we may trust some statements, of even greater dimensions have been observed. For such a spot to disappear in the course of six weeks (and they rarely last longer), the edges, whilst approaching each other, must move through a space of more than 1000 miles per diem.

"That portion of the solar disc which is free from spots is by no means uniformly bright. Over it are scattered small dark spots or pores which are found by careful observation to be in a state of continual change. The slow sinking of some chemical precipitates in a transparent liquid, when viewed from the upper surface and in a direction perpendicular thereto, resembles more accurately than any other phenomenon the changes which the pores undergo. The similarity is so striking, in fact, that one can scarcely resist the idea that the appearances above described are owing to a luminous medium moving about in a non-luminous atmosphere, either like the clouds in our air, or in wide-spread planes and flame-like columns, or in rays like the aurora borealis.

"Near large spots, or extensive groups of them, large spaces are observed to be covered with peculiarly marked lines much brighter than the other parts of the surface; these lines are curved, or deviate in branches, and are called faculæ. Spots are often seen between these lines, or to originate there. These are in all probability the crests of immense waves in the luminous regions of the solar atmosphere, and bear witness to violent action in their immediate neighborhood."

The changes on the solar surface evidently point to the action of some external disturbing force; for every moving power resident in the sun itself ought to exhaust itself by its own action. These changes, therefore, are no unimportant confirmation of the theory explained in these pages.

At the same time, it must be observed that our knowledge of physical heliography is, from the nature of the subject, very limited; even the meteorological processes and other phenomena of our own planet are still in many respects enigmatical. For this reason no special information could be given about the manner in which the solar surface is affected by cosmical masses. However, I may be allowed to mention some probable conjectures which offer themselves.

The extraordinarily high temperature which exists on the sun almost precludes the possibility of its surface being solid; it doubtless consists of an uninterrupted ocean of fiery fluid matter. This gaseous envelope becomes more rarefied in those parts most distant from the sun's centre.

As most substances are able to assume the gaseous state of aggregation at high temperatures, the height of the sun's atmosphere cannot be inconsiderable. There are, however, sound reasons for believing that the relative height of the solar atmosphere is not very great.

As most substances are able to assume the gaseous state of aggregation at high temperatures, the height of the sun's atmosphere is not very great.

Since gravity is 28 times greater on the sun's surface than it is on our earth, a column of air on the former must cause a pressure 28 times greater than it would on our globe. This great pressure compresses air as much as a temperature of 8000° would expand it.

In a still greater degree than this increased gravity do the qualities peculiar to gases affect the height of the solar atmosphere. In consequence of these properties, the density of our atmosphere rapidly diminishes as we ascend, and increases as we descend. Generally speaking, rarefaction increases in a geometrical progression when the heights are in an arithmetical progression. If we ascend or descend $2\frac{1}{2}$, 5, or 30 miles, we find our atmosphere 10,100, or a billion times more rarefied or more dense.

This law, although modified by the unequal temperatures of the different layers of the photosphere and the unknown chemical nature of the substances of which it is composed, must also hold good in some measure for the sun. As, however, the mean temperature of the solar atmosphere must considerably exceed that of our atmosphere, the density of the former will not vary so rapidly with the height as the latter does. If we assume this increase and decrease on the sun to be ten times slower than it is on our earth, it follows that at the heights of 25, 50, and 300 miles, a rarefaction of 10, 100, and a billion times respectively, would be observed. The solar atmosphere, therefore, does not attain a height of 400 geographical miles, or it cannot be as much as $\frac{1}{240}$th of the sun's radius. For if we take the density of the lowest strata of the sun's atmosphere to be 1000 times greater than that of our own near the level of the sea, a density greater than that of water, and necessarily too high, then at a height of 400 miles this atmosphere would be 10 billion times less dense than the earth's atmosphere; that is to say, to human comprehension it has ceased to exist.

This discussion shows that the solar atmosphere, in comparison with the body of the sun, has only an insignificant height; at the same time it may be remarked that on the earth's surface, in spite of the great heat, such substances as water may possibly exist in the liquid state under a pressure thousands of times greater than that of our atmosphere.

Since gases, when free from any solid particles, emit, even at very high temperatures, a pale transparent light—the so-called *lumen philosophicum*—it is probable that the intense white light of the sun has its origin in the denser parts of his surface. If such be assumed to be the case, the sun's spots and faculæ seem to be the disturbances of the fiery liquid ocean, caused by most powerful meteoric processes, for which all necessary materials are present, and partly to be caused by the direct influence of streams of asteroids. The deeper and less heated parts of this fiery ocean become thus exposed, and perhaps appear to us as spots, whereas the elevations form the so-called faculæ.

According to the experiments made by Henry, an American physicist, the rays sent forth from the spots do not produce the same heating effect as those emitted by the brighter parts.

We have to mention one more remarkable circumstance. The spots appear to be confined to a zone which extends 30° on each side of the sun's equator. The thought naturally suggests itself that some connexion exists between those solar processes which produce the spots and faculæ, the velocity of rotation of the sun, and the swarms of asteroids, and to deduce therefrom the limitation of the spots to the zone mentioned. It still remains enigmatical by what means nature contrives to bring about the uniform radiation which pertains alike to the polar and equatorial regions of the sun.

[To be continued.]

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ART. XXVIII.—*On a supposed change of level in a part of the Green Mountains;* by W. K. SCOTT, M.D., From a letter, dated Buffalo, March 28, 1864, addressed to Prof. O. P. Hubbard, of Dartmouth College, Hanover, N. H.[1]

I HAVE ascertained some facts, or what I believe to be such, relating to a change of elevation in a part of the Green Mountain range, which I wish to be made known to the Vermont geologists that they may make a thorough investigation of them. But I do not know who these geologists are, and have no means of getting the subject before them. I have therefore concluded to make a general statement of these facts to you, presuming

[1] Dr. Scott addressed the following note (dated May 9th) to Prof. Hubbard, in reply to a request from the latter that he should permit the publication of the above letter.

"I have heretofore refused to have any thing published that I have written about Mount Anthony, until the hill, the mountain, and their surroundings had been thoroughly examined by some competent person. But as you advise it, I now give my consent. The publication may draw the attention of some experienced geologist to the subject, and thus bring about the investigation I have so long desired.

It is a short journey from Dartmouth to Hoosick, and if you should feel disposed to view the ground yourself, I will meet you there at any time, except in the winter and extreme heat of summer, by your giving me a few weeks' notice. After I have pointed out to you the localities and the changes which sixty years have produced, you could determine whether an expensive survey would be advisable. If you cannot conveniently come, you may induce some one else to do so.

I send you a copy of three letters which I have received on the subject, all from candid, sensible, and reliable men. I can procure another from the eldest daughter of Mr. L. Turner, if you think it would be well to do so. Yours, &c."

that you know these gentlemen and will be willing to communicate to them what I write.

In the year 1796, when I was eight years old, my father moved to a part of the town of Hoosick, Rensselaer Co., N. Y., known as Mapletown. His house was on the road from Bennington, Vt. to Troy, N. Y., near the Mapletown meeting-house, about four miles from the village of Bennington. The residence of the late Garret Van Hoosen is within a few feet of the site of that old house.

In front of that house was distinctly visible the top of Mount Anthony, a spur of the Green mountain, south of the old village of Bennington and very near it. What most attracted my boyish curiosity was a large white spot on the side of the mountain, which was said by the inhabitants to have been produced by the bursting of a cloud (as they called it), which washed away the earth and left the white rocks naked. This white spot was situated about one-eighth of the apparent height of the mountain from the top of it, and extended downward about one-fourth of its height.

In 1803, after having learned something about linear perspective, I made my first attempt at landscape painting in water colors. The view was taken from a front window in our house, and the picture embraced the whole of Mount Anthony which was visible from that point. I well remember how much trouble I had in representing the white spot on the mountain; and I remember, too, what a miserable failure the whole performance was.—I mention these things to show that my recollections are not vague and shadowy, but clear, distinct, and certain.

I left that place in January, 1808, after having finished a course of medical lectures at Dartmouth College. After an absence of fifty years, I visited it again to see my friend, Garret Van Hoosen, who lived, as I have before mentioned, within a few feet of my old home. During my journey there I thought almost as much about seeing Mount Anthony again, as seeing my old friend; but when I arrived there, no part of the mountain was visible.

In front of the house, about a quarter of a mile distant from it, is a hill, now called Russel Hill, over which the mountain was formerly seen. A part of the top of that hill is now covered with high trees, which were small bushes when I was a boy, and my first thought was that these trees hid the mountain; but the next morning after my arrival, as the sun was rising, I found that I could see between the trees, and that no mountain was there. By going about fifty rods toward Bennington, I got out of the range of the trees, and could look over a lower portion of the hill, which was cultivated, and see the top of the mountain, so low that it was certain that no part of it could be seen from the house if no trees were on the hill.

My first impression was that the whole of this change was due to the subsiding of the mountain; but reflecting how many feet it must have settled to be hidden from our view, I gave that up, and settled upon the opinion that Russel Hill must have risen, for one foot rise of that hill would hide twelve feet or more of the mountain.

Had I been a practical geologist, I should have sought for some evidence of its having risen; but knowing very little of that science, I could only glean a few facts obvious to every one.

The road in front of the house in question runs nearly northeast for about fifty rods, and then turns east and crosses the line of Russel Hill continued. I say the *line* of the hill, for the hill itself does not cross the road. Near where the road crosses that line is a stream, on which was a grist-mill in 1800. As it now is, there appears to me to be no fall that could be used for that purpose; and the owner of a meadow some sixty rods above the mill told me that that meadow was formerly a very rich and productive one—that the mill pond never set back far enough to injure it—but that now, when there was no dam, it was so wet as to be almost worthless.—Below the mill for many rods, when I was a boy, the water was still enough to be a convenient place for little boys to bathe in. Now, there is quite a fall there—enough to make a valuable water power.

Following the line of the crest of the hill across the road, we come to a barn-yard, belonging to the widow of Lyman Andrews, which, in 1800, was supplied with water brought from a spring some little distance west of it. The spring was enough higher than the barn-yard to give great velocity to the water, so that it made a whizzing noise in running from a small orifice. As nearly as I could ascertain with a carpenter's level, the barn-yard is now a little higher than the spring. My investigation was not thorough, for I had but little time, and it may be that the yard has been filled enough to make the difference, but I saw no evidence of it. A man by the name of H. B. Walworth lives in a house opposite to Van Hoosen's, who remembers all about the water in the barn-yard, and can show where the spring is and where the water was discharged; and he remembers as well as I do the great velocity of the little stream as it escaped from the penstock.

In a matter of so much interest to geologists, I thought it important to have the evidence of some one besides myself; for it would hardly be satisfactory to the public to rely entirely on the memory of one man. I found some difficulty in obtaining this, for the inhabitants of that district had changed many times in fifty years, and most of those who lived there when I did were dead or moved to parts unknown. There was, however, a man by the name of Lebbeus Turner, who lived there several years and moved away about 1805, who was still living. He was a man of intelligence, and still retains his mental powers. He was a nailer, and had a house and shop near my father's house, on the opposite side of the road.

I wrote to him, asking if he remembered the white spot on Mount Anthony, and where he used to see it from. He answered[3] that he could always see it from his house, and described its shape and general appearance. He also said that he saw the storm on the mountain when the earth was washed away. This was before he lived in the neighborhood in question. His letter is quite interesting. I then made him a visit, and found that he and his wife were both clear in their recollections about it, and both certain that there could be no mistake about the mountain having been visible from their house. I had likewise an interview with his eldest son, Stillman Turner, who now lives in Worcester, Mass., and he is equally clear in his recollections. Mr. Turner's eldest daughter, too, remembers that the white spot on the mountain could be seen from their house. I have likewise a letter from Mr. Jacob Hallenbeck, who has lived all his life in another part of the town of Hoosick, and who was very often at Mr. Turner's shop, and he "thinks he remembers having often seen the mountain from that place." In a conversation with him after the letter was written, he spoke with absolute certainty of having seen the white spot from the back window of the shop.

All this appears to me to be sufficient evidence of the simple fact that Mount Anthony, and the white spot on the side of it, could be seen from my father's house and that vicinity, in the beginning of this century. All the rest is conjecture.

In order to ascertain how much the hill has risen, we must first find out how much higher the top of the mountain is than the bottom of the white spot, the distance from the mountain to the hill, and from the hill to the house; and all this will make quite a *job* of work. It is to be noted also that the white spot is not now visible from a distance, for it is covered with a growth of shrubs and small trees which have sprung up within the last thirty or forty years; but its boundaries can be easily traced, for so much earth was removed that a high bank was left at the sides and at the top of it.

If any geologist, whether employed by the State of Vermont or not, should deem this of sufficient importance to be worth a careful survey and a scientific investigation, and will undertake it, I shall take great pleasure in transmitting to him what I know about it, much more minutely than I have done in this communication, and to send him what letters I have received on the subject; and further, should he desire it, I will meet him on the

[3] His letter is given beyond, along with two from other observers.

spot and point out to him the various localities of which I have written. I should like, too, to have him see my witnesses and cross-examine them; for unless the evidence is strong enough to perfectly satisfy the public of the truth of my statements, I should not be willing to have any thing published about it.

And, furthermore, I think it would be well to have a permanent mark or monument placed on Russel Hill, and another at the house, showing the present difference in the elevation of these two points, so that in future it can be determined whether the hill continues to rise, and how much.

The following are copies of letters to Dr. Wm. K. Scott, attesting to the facts above stated.

"*Letter from Lebbeus Turner, of Aurora, Erie County, N. Y., dated East Aurora, Dec.* 17, 1855.

I received a few lines from you last Thursday, asking me to give you some information respecting Mount Anthony.

I well remember that when we lived in Mapletown the mountain was visible from our houses, and the white spot or streak on the side was plainly to be seen: the length of it went up and down the mountain. The mountain was seen some way below the spot; but for a number of years that spot has gradually filled up, and is now grown over with bushes of a size to obscure the spot. I presume the trees on the intervening hill have grown to a larger size than they were in those days, therefore I may justly conclude that Mount Anthony has not sunk down, nor the intervening hill risen to obscure the sight of that grand pile of rock and earth.

I well remember the time when that white spot was made. I was something like eight years old. At that time we lived on the Oak Hill, near where Miner Roberts lived. There was a very heavy thunder shower passed over the mountain from the southwest, so as to entirely obscure it; we could distinctly hear the rain roar; and as soon as the rain was over and the mountain was visible, that spot appeared. The neighbors at that time said that a cloud broke, and a large brook ran down the mountain.

My wife remembers well that the mountain was visible from our house.

[Signed,] Lebbeus Turner."

"*Letter from Jacob Hallenbeck, of Hoosick, N. Y., dated Hoosick, Sept.* 14, 1856.

You wish me to pen my recollections on the former appearance of Mount Anthony, and its present appearance from the place where Turner's shop used to stand. I think I can recollect some forty-five or fifty years ago, the mountain was plain to be seen from the place where the shop then was, but cannot be seen from that locality now, in consequence of the intervening hills. The mountain, hill, and plain where the shop stood, when viewed separately, have all the same appearance to me they ever had; but taken together, they present quite a different appearance from what they then did to me. I think there must be some alteration

in the form of the ground. I cannot believe it is owing to optical delusion, for I think I can see distant objects as well as I ever could.
[Signed,] JACOB HALLENBECK."

"*Letter from Stillman Turner, Esq., of Worcester, Mass., son of Lebbeus Turner, dated Worcester, April* 2, 1864.

Your communication is at hand, in which you wish me to state, according to my best recollection, whether Mount Anthony could be seen from my father's old residence in Hoosick, and also about the white spot on it. The most that I can say at this time is, that Mount Anthony was to me no rare sight to behold, for I think it was visible from almost any point in the vicinity of our old place. I certainly recollect seeing it when on the hill between our house and Uncle Harper Rogers', where we had a full view of the whole range of mountains east of us; I often used to stand and gaze with wonder and admiration on the scene; but whether it could be seen from our *house* or not, I am not able to say—certainly in that neighborhood I could see the white spot spoken of; but I do not feel justified in saying whether the mountain could be seen from our old residence or not. [Signed,] STILLMAN TURNER."

*Note by Dr. William K. Scott.*—From the hill of which Mr. Turner speaks, no part of the mountain is now visible. Nor can it be seen anywhere in that immediate neighborhood.

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## ART. XXIX.—*Notes on the Platinum Metals;* by M. CAREY LEA. Part. II. *On Reactions of the Platinum Metals.*

### (III.)

#### REACTIONS OF HYPOSULPHITE OF SODA.

IN the first part of this paper I described the reaction of sesquichlorid of ruthenium with hyposulphite of soda, a substance which will probably be found to be the best touchstone which we have for detecting its presence. I shall now briefly describe the behavior of that reagent toward other metals of the group, and then proceed to examine how its behavior toward ruthenium is modified when one or more other metals are present in the same solution. The remarkable properties which the platinum metals possess of exhibiting in many cases reactions, when mixed, wholly different from those which they show separately, renders this a point of much importance.

The hyposulphite is to have a little ammonia added before using as already mentioned. The ammonia must be in sufficient quantity to ensure that the solution after the addition of the solution of the platinum metal shall be alkaline.

*Bichlorid of Ruthenium and Ammonium* gives, with hyposulphite of soda mixed with ammonia and boiled, a rich sherry-wine colored solution. This differs materially from the reaction of ammonia alone, which produces a pale straw color—in both cases a very dilute solution is supposed to be used.

*Sesquichlorid of Ruthenium.*—As already described, a rich red purple by boiling; when dilute, nearly a rose color.

*Bichlorid of Iridium and Ammonium.*—Is simply decolorized.

*Sesquichlorid of Rhodium.*—Straw color, or yellow, according to strength of solution.

*Bichlorid of Platinum.*—The addition of hyposulphite of soda mixed with ammonia produces at first a precipitation of platinum salammoniac, which by heat (if the solution be not too strong) is redissolved to a yellow liquid. Boiling renders this at first paler, and almost colorless; the reaction then changes, and the color deepens to a rich wine brown.

*Protochlorid of Palladium.*—To apply the test to this metal, place a solution of hyposulphite in a test tube with a little liquid ammonia, and add a drop of palladium solution, so that it shall communicate a pale lemon color only, to the liquid. By boiling, this rapidly darkens to a wine brown shade, increasing in intensity until it finally appears black. Dilution however shows that this results from its intensity only; the diluted liquid is clear from troubling, and has a warm brown tint.

### *Detection of Ruthenium in Presence of Iridium by Hyposulphite of Soda and other Reagents.*

For the following examinations, solutions of sesquichlorid of ruthenium and of chloriridiate of ammonium were used. Both in a state of perfect purity were weighed dry, dissolved, and mixed in the following proportions:

$Ru_2Cl_3$, 1 part; chloriridiate of ammonium, 10 parts.

The hyposulphite test was not in any way impaired by the presence of iridium.

Sulphocyanid test gave a red coloration, but much less clear than in the absence of iridium,[1] and much inferior to the reaction with hyposulphite.

Acetate of lead added and boiled gave a precipitate, in which the purplish shade characteristic of ruthenium was very evident.

$Ru_2Cl_3$, 1 part; chloriridiate, 20 parts.

Hyposulphite gave a perfect reaction.

Sulphocyanid, reddish brown coloration and unsatisfactory.

Acetate of lead gives a precipitate still distinctly colored by ruthenium. It is to be regretted that to judge cor-

[1] Claus remarks that this test fails when the Ru is in proportion to the Ir less than 1 to 10. As here the proportion is 1 of $Ru_2Cl_3$ to 10 of iridium salammoniac, the proportion of Ru is even less than 1 to 10 of Ir. The hyposulphite test is not in the least affected by even a much larger proportion of Ir.

rectly of this test, it is necessary, either to be very familiar with the color of the precipitate which the lead salt produces with a ruthenium solution, or else to prepare it for comparison.

$Ru_2Cl_3$, 1 part; chloriridiate, 50 parts.

Hyposulphite gave a perfect reaction.

Sulphocyanid having failed in a solution containing a larger quantity of ruthenium, was not here again tried.

Acetate of lead gave a precipitate which when carefully compared with that afforded by a perfectly pure iridium solution, exhibited a shade of difference, but scarcely sufficient to afford any criterion. At least, this must be regarded as the extreme limit of the sensibility of mixtures of Ru and Ir to this reagent.

$Ru_2Cl_3$, 1 part; chloriridiate, 100 parts.

Hyposulphite, perfect ruthenium reaction.

$Ru_2Cl_3$, 1 part; chloriridiate, 200.

Hyposulphite, satisfactory ruthenium reaction.

$Ru_2Cl_3$, 1 part; chloriridiate, 500 parts.

Even in the presence of such an enormous excess of iridium salt, ruthenium is capable of being detected by a practised eye by means of the hyposulphite test, although the clear rose color produced in the previous trials was here changed to an orange shade.

It may therefore be concluded that for the detection of ruthenium in the presence of iridium, the hyposulphite test is at least ten times more delicate than acetate of lead, and even much more so in comparison with sulphocyanid of potassium.

Dr. Gibbs has proposed a test for ruthenium by the use of alkaline nitrite and sulphid of ammonium. It was my wish to compare this method with that which I here describe, but I did not succeed in obtaining the reactions of which he speaks, although I tried both nitrite of potash, prepared by passing the red fumes evolved by the reduction of nitric acid through a potash solution, and also with nitrite of soda prepared from the nitrate.

In the first part of this paper I recommended that when solutions containing very little ruthenium were to be tested, they should first be boiled with a little pure chlorhydric acid. This is an important point, the neglect of which may cause the presence of ruthenium to be overlooked, when it exists in sufficiently large quantity to be recognizable, even by the ordinary tests, after this precaution has been taken.

After making the foregoing experiments, I had occasion, after an interval of some hours, to repeat them on the same diluted and mixed solutions, which had been preserved in closed phials.

I found no effect from hyposulphite. Selecting one containing sufficient ruthenium to render the sulphocyanid test available, I tried it; but equally without effect. The ruthenium had lost its power of reacting even in solutions which contained it in the proportion of $\frac{1}{20}$th of the iridium present. It was immediately suspected that in consequence of the dilution, it had become decomposed. A portion of the solution was then boiled with a little chlorhydric acid, when it at once recovered its sensibility to the various reagents.

It was long since pointed out by Claus that neutral solutions of sesquichlorid of ruthenium were decomposed by boiling with separation of oxyd of ruthenium, and that even without heat the separation took place by standing. But it appears that even acid solutions spontaneously decompose when very dilute, if the excess of acid present is small.

I therefore recommend that in all cases where it is intended to test solutions for small quantities of Ru, that the solution be heated with a little dilute chlorhydric acid immediately previous. Of course when the hyposulphite test is employed, the solution must be rendered alkaline with ammonia, after boiling with the acid, and before adding the hyposulphite. Generally speaking, it is advisable to use dilute solutions for testing for ruthenium. Although it is then present in smaller quantity, it is immediately recognizable, because its reactions are then less marked by the iridium than when stronger solutions are employed.

The decomposition of dilute solutions of ruthenium, even when acid, may easily be observed without the aid of reagents. When such a solution of $Ru_2Cl_3$ is very largely diluted with water, it soon assumes a purple black color, and after a few hours nearly the whole of the Ru falls to the bottom, leaving the liquid almost colorless. This I have found to take place in solutions containing $Ru_2Cl_3$, 1; water, 5,000.

When the solution is somewhat less largely diluted, it gradually assumes a purplish red color by standing, and then behaves for the most part differently with reagents than ordinary solutions of $Ru_2Cl_3$.

*Detection of Ruthenium in Presence of Platinum by Hyposulphite of Soda.*

Small quantities of Pt scarcely affect the ruthenium reaction. When larger quantities are present, the color produced is a mixture of that which would result from each separately, and therefore rather a wine, than a rose color.

*Mixtures of Ir and Pt, or of Ru, Ir and Pt.*

In all these mixtures, the reaction of the hyposulphite is that which would result from a mixture of the separate colorations.

The hyposulphite is a valuable test for the purity of iridium, and affords an easy indication as to whether other metals of the platinum group are present. Let the chloriridiate of ammonium be boiled with H Cl, and then ammonia be added until the solution assumes the pale olive color produced by alkalies in solutions of bichlorid of iridium. The solution should be sufficiently dilute that, after the ammonia has been added, it becomes nearly colorless. Now add the hyposulphite and boil. *If any increase of color whatever takes place, it is a certain indication of impurity.* If the liquid becomes rose color, ruthenium is present; if wine color, platinum is probably present; if brown, palladium is probably indicated.

(IV.)

REACTIONS WITH TETRATHIONIC ACID.

Tetrathionic acid is capable of giving useful reactions with metals of the platinum group, and especially with palladium.

$Ru_2Cl_3$ boiled with tetrathionic acid is somewhat decolorized, and finally becomes muddy and grayish.

$RuCl_2$ boiled with tetrathionic acid gradually darkens in color, becomes muddy, and finally throws down a brown precipitate. But if the acid be at first supersaturated with ammonia, the solution becomes yellow and remains clear.

$Ir_2Cl_3$, when boiled with tetrathionic acid, the pale, almost colorless dilute solution darkens rapidly, and by some moments boiling becomes a deep wine brown, remaining clear. If the acid be first supersaturated with ammonia, the reaction does not take place.

$IrCl_2$ is quickly decolorized by boiling with the acid. The decolorized solution does not darken by further boiling.

PdCl is instantly precipitated in the cold by tetrathionic acid. The precipitate has a dark chocolate brown color. This precipitation without heat is highly characteristic of the protochlorid of palladium, and the test is of great delicacy. A single drop of a rather dilute palladium solution was added to two ounces of water. In a few drops of this very dilute solution, the presence of palladium was made evident by this test. When the quantity of palladium present is so very minute as in this case, no precipitation takes place, but a brown coloration is developed. And as this coloration is produced in the cold, it is highly characteristic of the metal in question.

$PtCl_2$. Tetrathionic acid produces no effect in the cold. By boiling, a wine brown color is developed, but no precipitation takes place.

As this test for palladium appeared likely to be a valuable one, a series of experiments was undertaken to ascertain whether, like so many of the old tests for metals of the platinum group, the reaction would be affected by the presence of other members of the group. Mixtures were therefore made of solution of protochlorid of palladium with the following substances respectively:

Sesquichlorid of ruthenium,
Bichlorid of ruthenium,
Sesquichlorid of iridium,
Bichlorid of iridium,
Bichlorid of platinum.

In all these cases it was found that the reaction was obtained without difficulty, so that this test is capable of detecting palladium with ease in the presence of either of the above-named compounds. When but little palladium is present, the reaction commences with a darkening of the solution, and the precipitate falls only after an interval of one or two minutes, or longer, according to the degree of dilution.

(V.)

REACTIONS WITH SULPHATE OF QUINIA.

With sulphate of quinia, protochlorid of palladium gives a bulky buff colored precipitate, which becomes somewhat blackish on boiling. Neither ruthenium nor iridium give similar reactions.

(VI.)

REACTIONS WITH PROTOCHLORID OF TIN.

$Ru_2Cl_3$ boiled with SnCl becomes perfectly colorless, if the solution is very dilute. Stronger solutions show a pale straw color.

$RuCl_2$, boiled with a small quantity of SnCl, gives a buff colored precipitate, which dissolves, in an excess of the precipitant, to a solution which, by further treating, passes to a splendid blood-red of great intensity.

The buff colored precipitate is soluble in solution of potash, producing an intense brown liquid.

$Ir_2Cl_3$. When the sesquichlorid of iridium and ammonium is boiled with SnCl, and potash added in sufficient quantity to redissolve the precipitate which it at first produces, further boiling produces an abundant leather-colored precipitate, which is insoluble in any excess of potash.

I felt much interested to observe whether this reaction would take place in the presence of sesquichlorid of ruthenium in the solution; and had the satisfaction to find that it did so. We thus have a mode of detecting iridium in the presence of ruthenium, which offers certain advantages.

The best way to observe the reaction is as follows: To the solution of sesquichlorid of ruthenium, add a little acidulated protochlorid of tin, and boil till the color disappears and then add excess of potash. The liquid should be perfectly clear and very nearly colorless. The addition of a single drop of dilute solution of sesquichlorid of iridium communicates a yellow color, which rapidly deepens by boiling, and an abundant leather-colored precipitate falls. The almost perfect decolorization of the ruthenium solution by the protochlorid of tin, adds to the nicety of this reaction. Those only who have been annoyed by the extreme difficulty of getting any indication of the presence of small quantities of iridium in ruthenium solutions, will appreciate the full value of this test.

## (VII.)

### REACTIONS WITH AMMONIO-CHLORID OF ZINC.

A solution of clorid of zinc in excess of ammonia gives an interesting and beautiful series of reactions with the metals of the platinum group. The metallic solutions, which are to be subjected to this test, must be either neutral or slightly acid. The presence of alkali in excess, or of acid in large excess, naturally interferes with these reactions.

To obtain the zinc solution in proper condition, chlorid of zinc must be added to ammonia until the smell of ammonia becomes tolerably faint, and a considerable proportion of zinc oxyd remains undissolved. The liquid is then to be filtered off, and should be used without too much delay. In this condition the affinities are in a state of very unstable equilibrium. The addition of even a few drops of water produces a precipitate. It is precisely this instability which gives the solution its value for the purpose under consideration. When it is added to a solution of a platinum metal, the precipitate which falls carries with it a part or the whole of the platinum metal, which imparts to it a characteristic coloration.

The following are the reactions:

$Ru_2Cl_3$ a brown precipitate: the solution becomes colorless.

$RuCl_2$ a rose-colored precipitate: the solution becomes colorless.

$Ir_2Cl_3$ a pale buff precipitate: the solution becomes colorless, or nearly so.

$IrCl_2$ a fire-red precipitate: solution decolorized.

With platinum and palladium the tendency of ammonia to form double salts, interferes, and prevents any characteristic reaction from the zinc solution.

## (VIII.)

### REACTIONS WITH SOLUTION OF FERRIDCYANID OF POTASSIUM IN CAUSTIC SODA.

When this solution is added to ruthenium and iridium solutions, the following reactions are obtained:

$Ru_2Cl_3$—bright yellow liquid.

$RuCl_2$—the same, but more on a wine color.

$Ir_2Cl_3$. When to a slightly acid solution of sesquichlorid of iridium, enough of the solution in question is added to make the liquid strongly alkaline, a green coloration is produced at once, which by boiling becomes olive.

But if the iridium solution be first rendered alkaline with ammonia, the addition of the above reagent gives a bright yellow coloration, which by boiling becomes deep wine red.

## (IX.)

### REACTIONS WITH SCHLIPPE'S SALT.

A solution of Schlippe's salt mixed with an equal bulk of ammonia, and added to the solutions of the Pt. metals, gave the following reactions:

$Ru_2Cl_3$ by boiling, a blackish precipitate.

$RuCl_2$. When the solution containing the bichlorid of ruthenium is boiled, and a single drop of solution of Schlippe's salt added, a yellow transparent liquid is obtained. A larger addition gives an abundant light brick colored precipitate. When this larger quantity of solution of Schlippe's salt is added, a slight warming is sufficient to throw down the precipitate.

$Ir_2Cl_3$. A similar precipitate is obtained, but only after some minutes' boiling.

$IrCl_2$, is instantly decolorized by solution of Schlippe's salt with ammonia, and when boiled remains clear for a few minutes, then lets fall an abundant brick-brown precipitate. In this it is distinguished from $RuCl_2$, which lets fall the precipitate by a slight warming.

PdCl. As ammonia precipitates palladium at once, the following course was adopted. Ammonia was placed in a test-tube, and a little palladium solution added. Heat was applied till the precipitate, which at first formed, was redissolved. An addition of Schlippe's salt then produced an instantaneous and abundant brown black precipitate.

$PtCl_2$ treated with Schlippe's salt (without ammonia) gave an immediate dark brick-brown precipitate in the cold.

$KO\ OsO_3$. Osmite of potash, dissolved in dilute caustic potash, gives with the aid of heat an immediate black precipitate with Schlippe's salt.

The following substances gave no characteristic reactions with the platinum metals: fulminurate of ammonia, nitroprusside of sodium, picrate of ammonia, nitrosalycylate of potash, purpurate of ammonia, benzoic acid, chloranilamate of ammonia.

The new reactions which I have described in this paper include characteristic criteria for all those cases in which it has been considered most difficult to discriminate. Platinum and rhodium offer no difficulties; the first can always be recognized with ease by its behavior with chlorid of potassium, and the latter by its behavior with caustic alkalies. For the other metals, I propose here very briefly to recapitulate what I consider the chief points of interest here developed.

$Ru_2Cl_3$. The characteristic reaction of sesquichlorid of ruthenium is its beautiful coloration when boiled with *hyposulphite of soda.* See section third.

$RuCl_2$. Bichlorid of ruthenium is recognized by its rose-colored precipitate with *ammonio-chlorid of zinc*, as described in section seventh.

$\left.\begin{array}{l}Ir_2Cl_3.\\IrCl_2.\end{array}\right\}$ Iridium is best detected by its behavior with *protochlorid of tin* and *potash.* The details and mode of application have been already described in section sixth.

PdCl. The reaction of protochlorid of palladium with *tetrathionic acid* is highly characteristic, and cannot well be confounded with any other. See section fourth.

For ascertaining the purity of solutions of iridium, particularly as respects ruthenium, the *hyposulphite of soda* is especially valuable, as described at the end of section third.

Philadelphia, May, 1864.

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## ART. XXX.—*Progress of the Geological Survey of California.*

PROFESSOR WHITNEY, having recently returned from California, is now engaged in superintending the publication of a portion of the materials collected by the Geological Survey of California. He communicates the following statement in regard to the present condition and probable future of the Survey :—

Those interested in the California Survey will already have learned something of its progress from the brief reports, or letters to the Governor of that State, which have appeared from time to time, during the past three years, and of which extracts or notices, more or less complete, have appeared in this Journal. (See Journal for July, 1863, and May, 1864.)

No serious opposition has been made to the continuance of the Survey, during any of the Legislative sessions, except on the score of economy, an argument which was urged with more reason than usual before the last Legislature; since, owing to a variety of causes, which it is not necessary here to specify, the finances of the State, for the past three or four years, have been in a deplorable condition. The vast extent of area to be explored, the variety of subjects claiming attention, and the necessarily heavy expense of travelling everywhere on the Pacific slope, make it impossible to carry on this work with anything like satisfaction, even with appropriations which would be deemed ample on the Atlantic side of the continent. The heavy and entirely unprecedented rains of the winter of 1861–2, and the equally unprecedented drought which has prevailed since that time, have been great obstacles to the progress of the field-work, partly by impeding communication, and partly by increasing the price of provisions and forage, so that the field-expenses have been double or triple what they would have been in ordinary seasons.[1]

The sessions of the Legislature being, under the new Constitution of the State, which has come in force, biennial, at the last session an appropriation was made for the two fiscal years commencing July 1st, 1864. The sum appropriated for the continuance of the Survey for two years was $31,600;[2] of which, however, $6000 was to be set apart for the publication of two volumes of the Report, making, with $3000 previously appropriated for that purpose, $9000 available for printing and engraving—or, rather, which will be available when the money is received from the Treasury.

The old law organizing the Survey having expired by constitutional limitation, a new one was passed, somewhat modifying the details of the work, and especially in regard to the method of publication. The Act, the essential portion of it at least, is worded as follows: "It shall be his (the State Geologist's) duty, with the aid of such assistants as he may appoint, to complete the geological survey of the State, and to prepare a report of said survey for publication. Such Report shall be in the form of a geological, botanical, and zoological history of the State," &c.

In accordance with the Act now in force, it is presumed that the Survey will be continued for four years longer; and it is the intention of the State Geologist to close it up within that time, at least the active field-work, if spared to continue it so long.

The appropriation being so small, for the next two years the work will necessarily be continued on a rather small scale; but it is thought that, if the State returns to its normal condition of prosperity, the extreme disturbing influences of the past three years passing away, a liberal appropriation will be made by the next Legislature for closing up the work and for the publication of its results.

Following the provisions of the present Act, each volume of the Reports now and hereafter to be published will form a part of the Final Report, which it is believed will comprise about ten large royal-octavo volumes.

The first volume to be issued will comprise the first installment of the Paleontology of the State. It will be illustrated by 32 crowded plates on steel and stone, which have been for some time in the hands of the engravers, and of which the accompanying text is now passing rapidly through the press.[3]

[1] Professor Brewer, now in charge of a party exploring in the central portion of the Sierra Nevada, in a letter recently received, describes the journey from San Francisco to Visalia, through Pacheco's Pass, and across the plain of the San Joaquin, as one of great hardship, owing to the extreme difficulty of procuring water or forage along the whole route.

[2] It will be observed that *gold* is the only circulating medium in California.

[3] As the engraving of the plates will still require some time, the letter-press, which will soon be completed, will be issued by itself, in a limited number of copies, to which the plates will be added on their appearance.

This volume includes the Carboniferous, Triassic, Jurassic and Cretaceous fossils; the Carboniferous and Jurassic by Mr. Meek, the Triassic and Cretaceous by Mr. Gabb. Of the Cretaceous fossils 249 new species are described, from a formation not distinctly recognized in California previous to the commencement of this survey; but now known to extend through the State from one end to the other, and indeed from Mexico to British Columbia, and to be in many localities rich in well-preserved fossils.

The second volume of the Paleontology of California will be devoted to the Tertiary formation, and to such additions to what had already been done in the lower formations as may have been accumulated during the continuance of the Survey.

Immediately after the appearance of the first volume above-mentioned, a second will be put to press, which will be devoted to the General Geology of California. This volume, it is hoped, may be issued early in the next year. In a third volume, which will follow soon after the appearance of the second, it is proposed, although not definitely determined, to make a beginning with the Zoological history of the State.

A party is now in the field, in charge of Professor Brewer, engaged in exploring the region of the high Sierra Nevada, south of the Mono Pass and the region examined last year: a portion of the State which is almost entirely unexplored, and of which we know only that it contains some of the loftiest and most extensive mountain-groups of the Sierra.

Some of the principal results of the Survey, up to the present time, may be thus briefly summed up:

1. *Topography.*—For a sketch of what had been done in this department up to the beginning of the season for field-work in 1863, see this Journal, vol. xxxvi, p. 119. During that season a reconnoissance was made in the High Sierra, from the region adjacent to the Mono trail (which trail leads from Big Oak Flat or Coulterville, along the edge of the Yosemite valley, to Aurora, a little south of the 38th parallel of latitude) to the northern line of the State. Nearly all the high points of the Sierra Nevada, on this line of reconnoissance, were ascended and measured barometrically. The highest portion of the Sierra Nevada is that near the head-waters of the Tuolumne river, west and southwest of Mono Lake. The culminating peak of the Sierra, the highest point in the State, with the exception of Mt. Shasta, we found to be just north of the Mono trail, eight miles southwest of Mono Lake; it is 13,200 feet high, and, being one of the numerous unnamed peaks of the Sierra, was called by us Mount Dana, in honor of Professor J. D. Dana. The next point in height to this—the centre of a magnificent group of snow-covered peaks—we named Mount Lyell: it is about 15 miles, a little west of south, from Mt. Dana, and about 100 feet lower than that elevation. The scenery of this portion of the Sierra is truly Alpine, and can hardly be surpassed in grandeur.

One of the most interesting facts observed here, for the first time in the Sierra, was the proof everywhere surrounding us of the former existence of glaciers, on an immense scale of magnitude. All the phenomena of former glacier action are exhibited to the greatest advantage in the upper Tuolumne valley, through which once flowed a mass of ice nearly 1000 feet in thickness. Thousands of acres of granite retain the most exquisite glacial polish, and the existence of lateral, medial, and terminal moraines is as easily observed as in the Alps at the present day.

These traces of extinct glacier action were afterward discovered by us in many other places to the north of this, through to Mt. Shasta.

The work of laying down the topography will be continued during the present season by Mr. Hoffman in the region south of the Mono trail, and by Mr. Wackenreuder in that north from Silver Mountain to the Henness pass. Thus our observations, when combined, will enable us to give the first approach to a tolerably accurate map of this great chain of mountains. It is uncertain, as yet, how and in what form our topographical work will be laid before the public, except that the publication of the maps of the vicinity of the Bay of San Francisco and of the Monte Diablo region has been determined on, and they will be soon placed in the engravers' hands. It is believed, however, that such arrangements will be made as shall ensure the publication of a map of the entire State, greatly improved on anything which has yet appeared, and as large as can be conveniently used for a wall-map—say, on a scale of twelve miles to the inch. A map of the central portion of the State, on a scale of about six miles to the inch, embracing the principal mining regions, and covering the area occupied by at least four-fifths of the population of the State, will also probably form a part of the materials prepared for publication by the Survey.

2. *Physical Geography.*—Observations in this department have been steadily carried on during the progress of the Survey, and one of the volumes of the Report will be devoted to this department of our work. Allusion may be made here to the very important investigations which have been carried on in California, for some years past, by Major R. S. Williamson, U. S. Engineers, on the laws governing the fluctuations of the barometer on the Pacific coast. These investigations, for which small amounts have been appropriated by the Topographical Bureau, and which have been prosecuted by Major Williamson with the greatest zeal, will, when their results are published, be of material aid to the Geological Survey, and we shall be able to use them in the final working up of our own very numerous hypsometrical observations. The data collected in the course of these investigations have been employed by Major Williamson in working up the observations taken by the U. S. and California Boundary Commission in 1861, in and near Death's Valley, enabling him to fully confirm and give a scientific basis to the idea previously vaguely entertained, that this remarkable valley, which is 180 miles from the Pacific and which may be compared to that of the Dead Sea of Palestine, is indeed considerably depressed below the level of the ocean.

3. *Geology.*—Up to the present time, and including this season of field-work, we may be said to have been chiefly engaged in making a general geological reconnoissance of the State, and we have travelled very extensively over it, in all directions. But considerable areas remain, for various reasons, yet unexplored. The northwestern counties have remained inaccessible on account of Indian troubles; and portions of the southeastern for the same reason—with the additional one, that the drought has seriously interfered, for the last two years, with operations in that quarter.

In addition to work carried on within the boundaries of California, we have considerably extended the area of our field of labor by observations made in the adjacent territories, and not considered, or paid for, by the State as part of the regular work of the Survey. Mr. Gabb was employed by me, in the autumn of last year, to make a reconnoissance in Oregon and Washington Territory, and also on Vancouver Island, where he made important additions to the little previously known of the geology of that region. He has now joined an exploring party through south-eastern Oregon, emphatically a geological *terra incognita*, and from which region he can hardly fail to bring back geological facts of importance. The expedition was to start from Fort Klamath, on the north end of Klamath Lake, about the 15th of June. Mr. Clarence King made, during the early part of the present season, some important explorations in the Humboldt range, and the ranges east of that, in Nevada Territory. Mr. Rémond has been for some time in Sonora, Mexico, and has made interesting discoveries, which will enable us to prove the continuance of the geological formations of California far to the south, and which connect with our line of observation, so as to form a chain—broken at many points, it is true—of about 2000 miles in length. It may here be added, as an important item in the development of the geological structure of the western side of the continent, although not connected with the Geological Survey of California, that Baron Richthofen, the eminent geologist formerly attached to the Reichsanstalt, or Austrian Geological Survey, has been for some time engaged in investigating the geology of the Pacific coast, and that he is specially devoting himself to the study of the Washoe region; those who have consulted this gentleman's publications on the districts examined by him in the Alps, especially in Tyrol, and also in Hungary, will not need to be told that whatever he may give to the world in regard to this, in many respects, strikingly similar region of Nevada Territory, will be of the greatest interest.

On the whole, it may be said that the labors of the past three, and of the coming four, years can hardly fail to furnish us with the means of giving a tolerable approximation to a geological map of the Pacific coast of our continent, and one, it may be added, which will have little resemblance to those which have heretofore been published. For anything like a detailed knowledge of the whole of this vast, mountainous, and extremely difficult region, we shall have to wait many years.

In alluding to a few of the results of the Geological Survey in this department, it will be necessary to be extremely brief; for, even if this were a suitable place for it, details would hardly be intelligible without maps and sections.

Perhaps the most striking result of the Survey is the proof we have obtained of the immense development, on the Pacific side of our continent, of rocks equivalent in age to the Upper Trias of the Alps, and paleontologically closely allied to the limestones of Hallstadt and Aussee, and the St. Cassian beds, that extremely important and highly fossiliferous division of the Alpine Trias.

This great Triassic belt of the Pacific coast has been most fully explored by the Survey in the latitude of 40°, and over a width east and west of nearly four degrees of longitude (117° to 121°). It is from this region that the largest portion of the fossils have been obtained, both from the three parallel ranges in long. 117° to 118°, called the Humboldt ranges, and from localities in Plumas county, California. But sufficient paleontological evidence has been obtained to enable us to state that this formation extends from Mexico to British Columbia, occupying a vast area, although much broken up, interrupted, and covered by volcanic and eruptive rocks, and usually highly metamorphosed.

Among the specimens from the Humboldt and Plumas county, Mr. Gabb recognizes at least four species as identical with European, while the whole facies of the collection is most strikingly like that of the Hallstadt beds—the same intermixture of orthoceratites, ceratites, goniatites, nautili and ammonites, the latter frequently of those peculiar globose forms occurring in the Alps, together with *Halobia*, *Monotis*, *Avicula*, *Pecten*, &c., a *Monotis* being the most widely diffused and the most abundant of all.

Accompanying this Triassic formation in the Sierra Nevada, and probably also in the Humboldt ranges, is an extensive development of Jurassic rocks, usually highly metamorphosed and extremely barren of fossils. Enough, however, have been found to justify the assertion that the sedimentary portion of the great metalliferous belt of the Pacific coast of North America is chiefly made up of rocks of Jurassic and Triassic age, with a comparatively small development of Carboniferous limestone, and that these two formations are so folded together, broken up, and metamorphosed in the great chain of the Sierra Nevada, that it will be an immense labor, if indeed possible at all, to unravel its detailed structure. While we are fully justified in saying that *a large portion of the auriferous rocks of California consist of metamorphic Triassic and Jurassic strata*, we have not a particle of evidence to uphold the theory that has been so often maintained, that all, or even a portion, of the auriferous slates are older than the Carboniferous; not a trace of a Devonian or Silurian fossil even having been discovered in California, or indeed anywhere to the west of the 116th meridian. On the other hand, we are able to state, referring to the theory of the occurrence of gold being chiefly limited to Silurian rocks, that this metal occurs in no inconsiderable quantity in metamorphic rocks belonging as high up in the series as the Cretaceous.

Allusion has already been made to the wide-spread occurrence in California of the Cretaceous formation. The coast-ranges of California and Oregon, indeed, are to a large extent made up of rocks of this age, usually more or less metamorphic in their condition, and frequently highly so; but still, on the whole, forming the richest fossiliferous formation of the State, having already yielded fossils at over thirty localities. According to Mr. Gabb, it appears that the formation, so far as is yet known, is represented on the Pacific coast by but a single member—the upper, or white chalk; and that it may be divided into two well-marked groups, the lower of which corresponds to the No. 4 of Meek and Hayden, or the Fort Pierre group. This division (Div. A of the California Report) has yielded 152 species in California, out of about 260 collected, and of these only about half a dozen are common to this and the upper division (Div. B). This latter should probably, judging from its stratigraphical position, correspond with the Fox Hill group, or No. 5 of Meek and Hayden;

although all the species, with a single doubtful exception, are peculiar to California, and that species is referred to one described from No. 4 in New Jersey and Tennessee.

Rocks of Tertiary age are extensively developed in the coast ranges, although not so much so as the Cretaceous, the greatest development of the former being in latitude about 34° and 35°. Although fossiliferous in many localities, the Tertiary beds do not usually retain their organic remains in as good a state of preservation as the Cretaceous, and there is a much greater variety of distinct groups of species. The paleontology of the Tertiary rocks has not yet been thoroughly worked out, but it will be taken up by Mr. Gabb during the coming winter. It will probably require a long period of time before the Tertiary of the Pacific coast will be wholly elaborated, and the exact equivalency of its divisions with those of the Eastern Tertiary be made out.

In regard to the relative ages of the different mountain-chains of the Pacific coast, a few words may be added, premising that this subject will be as fully discussed as our materials will admit, in the Report.

There can be no doubt that the chain of the Sierra Nevada is older than the Rocky Mountain chain, or that group of chains or ranges which forms the eastern border of the great mountain region of the western side of the continent. The great mass of the Sierra was uplifted and metamorphosed after the termination of the Jurassic epoch, and prior to the deposition of the Cretaceous, for we find the last-named formation resting horizontally and unaltered on the flanks of the Sierra, all through Central California. The same statement is probably true with regard to the ranges east of the main crest of the Sierra Nevada, as far at least as the 116th meridian. But, although we have good reason to believe that these last-mentioned ranges were uplifted and metamorphosed after the close of the Jurassic period, we have not positive evidence that this took place prior to the Cretaceous epoch. Still, combining all we know of the geology of New Mexico and Nevada Territory, there appears to be little doubt that the system of the Sierra Nevada extends over a considerable area, east and west, embracing a number of nearly parallel ranges of mountains, some of which, indeed, are little inferior in extent and elevation to the Sierra Nevada proper.

We have recognized at least three distinct periods of upheaval and metamorphic action in the coast-ranges. The main one was at the close of the Cretaceous epoch; the next in importance was after the deposition of the Miocene tertiary—or, at least, of a group of strata which, for the present, may be referred to that age. The next in age is a system of east and west upheavals, which took place at the close of the Miocene; and the third is one which appears to have commenced during the later Pliocene, and to be still going on.

It is a very interesting fact, that the exterior of the coast-ranges—that is to say, the mountains nearest the Pacific—are of earlier date, or older geologically, than the interior ones, or those which border the Sacramento and San Joaquin valleys. This is a repetition on a smaller scale of what has been the course of events in the formation of the whole continent, the exterior lines having been first marked out, and the interior filled up afterwards.

The vast Tertiary formations on the flanks of the Sierra Nevada, so important as being the locality of the hydraulic mining operations, are not of marine origin, as has been so often asserted. The history of these deposits, their position, age, and other characters, are exceedingly interesting; but it is impossible, in this connection, to do more than hint at some of their main features.

There is perhaps no subject connected with the geology of the Pacific coast, in regard to which there are so many misapprehensions, as there are in what has been published by geologists on the nature and distribution of the detrital deposits which are so extensively worked by the methods known as hydraulic and tunnel mining. It has been assumed that these deposits are of marine origin, and that they originally extended over the whole western slope of the Sierra Nevada, a condition of things which, were it true, it would be of vast importance for California to know; but the real facts of the case are entirely different.

In the first place, these deposits are not of marine origin, as is proved by the fact that, although frequently found to contain impressions of leaves, masses of wood and imperfect coal, and even whole buried forests, as well as the remains of land animals, and occasionally those of fresh water, not a trace of any marine production has ever been found in them.

Again, these detrital deposits are not distributed over the flanks of the Sierra in any such way as they would have been if they were the result of the action of the sea. On the contrary, there is every reason to believe that they consist of materials which have been brought down from the mountain-heights above and deposited in preëxisting valleys: sometimes in very narrow accumulations, simple beds of ancient rivers, and at other times in wide lake-like expansions of former water-courses; and this under the action of causes similar to those now existing, but probably of considerably greater intensity. This deposition of detritus, for the most part auriferous, took place during the later Pliocene epoch, and not as late as the drift or diluvial period, as is abundantly proved by the character of the remains of plants and land animals which are imbedded in it.

The deposition of this auriferous detritus was succeeded, throughout the whole extent of the Sierra Nevada, by a tremendous outbreak of volcanic energy, during which the auriferous gravel was covered by heavy accumulations of volcanic sediments, ashes, pumice, and the like, finally winding up by a general outpouring of lava, which naturally flowed from the summits of the Sierra through the valleys, into the lake-like expansions, filling them up and covering over the auriferous gravels, which were to remain for ages, as it were, in a hidden treasure chamber, concealed under hundreds of feet in thickness of an almost indestructible material.

The effect of the denudation which has taken place since these streams of lava flowed down the mountains, has been most extraordinary. For now, these deposits of gravel and overlying volcanic materials, instead of occupying the depressions of the surface, are found forming high plateaux between the present river cañons and flat-topped ridges, known as "Table mountains," hundreds, or even thousands, of feet above the present river beds. Thus the topography of the country is exactly the reverse of what it was at the commencement of the present geological epoch: what were once valleys are now ridges, and the ridges of former times were where the immense cañons of the rivers flowing down the western slope of the Sierra now are. The proof of this assertion, and the interesting bearing it has on the tunnel and hydraulic mining interests of California, will be fully set forth in the Reports of the Survey.

The Mammalian remains found in the tunnel and placer diggings of California seem to belong to two distinct epochs. The oldest represents the Pliocene, the other the Post-tertiary. The former are found under the volcanic beds, the latter in deposits which have been formed since the period of greatest volcanic activity, and which apparently belong to the epoch of Man. For it appears that the facts collected by this Survey, when fully laid before the public, will justify the assertion *that the mastodon and elephant, whose remains are so widely and abundantly scattered through California, have been contemporaneous with Man in that region.*

The above are a few of the more interesting facts developed by the Geological Survey of California, and of which the full details will be laid before the scientific public with as little delay as possible.

4. *Metallurgy and Mining.*—Particular attention will be paid to these departments of the Survey during its continuance, and no pains will be spared to throw all possible light on the mode of occurrence of the valuable metals and ores, the method of working them, and the processes now in use on the Pacific coast. It is estimated that two volumes will be required for this division of the Report.

5. *Botany.*—It is believed that the progress in this department, under Prof. Brewer's direction, has been sufficient to warrant the assertion that a "Manual of the Botany of California" will form a portion of the work of the Survey. The large collections of plants already made have been distributed to different high authorities in each department, and Prof. Brewer expects to return to the East in a short time, to commence the preparation of such a work as, it is believed, will be a most important help to the study of the botany of the Pacific coast.

6. *Zoology.*—The working up of the zoological collections of the Survey is now in progress under Dr. Cooper's direction. It has not yet been decided how many volumes will be required for their full description and illustration.

The publication of so large a mass of materials will necessarily occupy several years, and it will of course depend on the action of future Legislatures how fully and how rapidly our results are laid before the public in a printed form. Three volumes are already provided for, and, as has been already stated, they will be sold at a moderate price, and the proceeds, as required by law, paid over to the common school fund of the State.

J. D. Whitney.

Northampton, Mass., Aug. 1, 1864.

# SCIENTIFIC INTELLIGENCE.

## I. CHEMISTRY AND PHYSICS.

1. *On a new class of Sulphur compounds.*—VON OEFELE has succeeded in shewing that sulphur, like selenium, tellurium, lead, tin and many other elements, is capable of forming a true organic base with ethyl, and doubtless, therefore, with other organic radicals. The iodid of the new radical is easily formed by the direct combination of sulphid of ethyl with iodid of ethyl, the reaction being expressed by the equation

$$2C_4H_5S + C_4H_5I = (C_4H_5)_3S_2I.$$

The new iodid is a beautiful crystalline body which is easily soluble in water and alcohol and crystallizes from its solutions unchanged. Nitrate of silver precipitates iodine from an aqueous solution of iodid of triethyl-sulfyl as iodid of silver, while a nitrate of the new base remains in solution. Oxyd of silver and water digested with the iodid yield the hydrated oxyd of triethyl-sulfyl, which in the exsiccator gives transparent deliquescent crystals. The oxyd of triethyl-sulfyl is a very powerful fixed base, a solution of which exhibits a strong alkaline reaction and precipitates metallic oxyds from their solutions like caustic potash. It forms neutral salts with acids; the sulphate and nitrate are crystalline and deliquescent. The chlorplatinate $(C_4H_5)_3S_2Cl, PtCl_2$ crystallizes easily from the aqueous solution in long prisms. The author is engaged in investigations which appear to show that nitric acid converts triethyl-sulfyl into a new base which contains $S_2$ as a hexatomic instead of a tetratomic element, the hydrated oxyd being probably $\}(C_4H_5)_3S_2O_2\{O, HO$, and which perhaps still possesses weak basic properties.—*Journ. of the Chemical Society of London*, xi, 105. W. G.

2. *On a very sensitive reaction for Iron.*—NATANSON has observed that sulpho-cyanid of iron is soluble in ether, and that when a solution containing a trace of peroxyd of iron and sulpho-cyanid of potassium but exhibiting no visible red tint is agitated with ether, the latter assumes a rose color resembling that produced in chloroform by traces of iodine. Of course the precautions usual in testing for iron by sulpho-cyanid of potassium must be taken in employing Natanson's process. By means of it, the author easily detected iron in a solution of chlorid of platinum, in which, on account of the yellow color, sulphocyanid of potassium as usually employed, produces no sensible change of color. W. G.

3. *On the conversion of mono-carbon acids into the corresponding more highly carbonated di-carbon acids.*—KOLBE and MÜLLER, independently of each other, have succeeded in preparing malonic from acetic acid, and the latter chemist has also obtained succinic from propionic acid by the same process. When mono-chloracetic acid is heated with a solution of cyanid of potassium it is easily decomposed and yields cyanacetic acid, and chlorid of potassium. Chloracetic ether undergoes a precisely similar change when boiled with an alcoholic solution of cyanid of potassium, cyanacetic ether being formed. When cyanacetic acid is boiled with a large excess of potash, ammonia is given off and malonate of pot-

ash is formed, from which pure malonic acid is easily obtained. Kobel represents the reaction in this case by the equation

$$KO, C_2 \left\{ \begin{matrix} H_2 \\ C_2N \end{matrix} \right\} [C_2O_2]O + KO.3HO = 2KO.[C_2H_2)'' \left[ \begin{matrix} C_2O_2 \\ C_2O_2 \end{matrix} \right] O_2 + H_3N.$$

Müller obtained the same results by boiling cyanacetic ether with caustic potash, while by treating chloro-propionic acid by the same process a small quantity of a crystalline acid was prepared, which on being heated, emitted the suffocating vapor characteristic of succinic acid. Both authors promise a further investigation of this very interesting subject.—*Journal of the Chemical Society*, Ser. 2, vol. xi, p. 109. W. G.

4. *On Thallium.*—CROOKES has published the first part of a monograph of thallium, in which all the known facts in regard to this metal are collected and systematically arranged. For the details, many of which have already appeared in this Journal, we must refer to the original paper. We design at present only to notice a single point, the relations, namely, of thallium to the alkaline metals. Crookes maintains that it belongs to the same group with silver and lead, (sic) a view which, as he says, is generally taken in England. It seems difficult to understand how any chemist at the present day can place lead and silver in the same natural group, silver being monatomic while lead is either diatomic or tetratomic according to circumstances. All the relations of thallium show plainly that it is essentially monatomic, and the very interesting discovery by Church of a silver alum only illustrates the analogy between silver and the alkaline metals which are also monatomic. That potassium and silver are at different ends of the scale, so far as their electrical relations are concerned, is of no consequence whatever in determining their natural position with respect to each other, since the difference is one of degree and not one of kind. As gold with the equivalent 197 is also monatomic, we may expect hereafter to discover a gold-alum or at least a salt in which gold and potassium may replace each other isomorphously. It appears to us, after a careful examination of all the facts, incontestible that thallium, like potassium and silver, belongs to the monatomic group of elements, and that in its chemical relations it forms a connecting link between these two metals which is of the greatest interest and value for the purposes of classification. Further investigation may perhaps show that indium, which like thallium and the alkaline metals gives a single spectral line at moderate temperatures, belongs to the same group. W. G.—*Journal of the Chemical Society*, Ser. 2, xi, 112.

5. *On Cobaltic acid.*—WINKLER has observed that when finely divided metallic cobalt is heated with a strong solution of caustic potash, a deep blue solution is formed which proves on analysis to be a cobaltate of potash. Cobaltic acid has the formula $CoO_3$, and belongs therefore to the group of manganic, ferric and chromic acids. The blue solution is only formed and only exists in the presence of a large excess of caustic potash: deoxydizing agents and even the addition of a large quantity of water easily decompose it. Ether does not dissolve cobaltate of potash but forms a brown zone where the solutions are in contact, while, on shaking, the cobalt solution becomes brown and deposits a hydrate of the sesquioxyd. The author thinks that this reaction may be used as a test for cobalt. W. G.—*Journal für prakt. Chemie*, xci, p. 213 *and* 351.

6. *New method of reduction especially applicable to a large number of metals.*—Mr. Poumarède has proposed to use the vapor of zinc as a reducing agent, and has obtained by this means a large number of interesting products. Peligot exhibited to the Academy of Sciences, at its session, March 28, specimens of nickel and cobalt thus made, and also magnificent crystals of iron.

7. *Bromid of potassium, a powerful narcotic.*—Bromid of potassium after having been used as a remedy against diphtheria, photophobia, etc., has been proved to be a powerful narcotic. It produces its effects without any cerebral congestion, and therefore without either pain in the head or constipation, and it has therefore great advantages over opium.

8. *Elements of Chemistry, Theoretical and Practical;* by William Allen Miller, M.D., LL.D, &c. Part I, *Chemical Physics.* From the third London edition. New York, John Wiley. 1864. 512 pp. 8vo.—We had occasion in a late No. of this Journal to notice the issue by Mr, Wiley, of a portion of the first volume of Miller's Chemistry, viz: that treating of Magnetism and Electricity. We are now glad to announce the appearance of the volume complete. It is considerably enlarged above the first and second edition, both by including matter formerly contained in the second volume and by important additions made necessary from the progress of science. The subjects of Dialysis, Spectral Lines, Specific Heat and Diathermancy of Gases and Vapors, and Heat of Combination, are fully discussed with reference to the most recent investigations. From its fullness, systematic arrangement and lucid style, this volume deserves the warmest praise, and commends itself equally as a text book and work of reference. We look for the speedy completion of the other volumes.

9. *Manual of Qualitative Chemical Analysis;* by Dr. C. R. Fresenius: from the last English and German editions, edited by Prof. Samuel W. Johnson of Yale College. 8vo, pp. 434. John Wiley, New York, 1864.—The new edition of this excellent manual fills a want which has long been felt by chemists; for, in addition to the matter found in the earlier editions, it contains the properties of all the *rarer elements,* and includes a full account of *spectral analysis, dialysis,* and the *reactions of the alkaloids.* Prof. Johnson has substituted the accurate and plain spectrum-plate, recently published by Kirchhof and Bunsen, in place of the incomplete and erroneous colored spectrum-plate contained in the foreign editions, and has also added numerous notes, making the work complete to the date of publication. The value of this text-book is too well recognized to be enlarged upon here; it has already passed through eleven German, and five English editions.

10. *On the Nature of Heat-vibrations;* by Mr. James Croll.—In a most interesting paper on Radiant Heat, by Professor Tyndall, read before the Royal Society in March last, it is shown conclusively that the *period* of heat-vibrations is not affected by the state of aggregation of the molecules of the heated body; that is to say, whether the substance be in the gaseous, the liquid, or perhaps the solid condition, the tendency of its molecules to vibrate according to a given period remains unchanged. The force of cohesion binding the molecules together exercises no effect on the rapidity of vibration.

I had arrived at the same conclusion from theoretical considerations several years ago, and had also deduced some further conclusions regarding the nature of heat-vibrations, which seem to be in a measure confirmed by the experimental results of Prof. Tyndall. One of these conclusions was, that the heat-vibration does not consist in a motion of an aggregate mass of molecules, but in a motion of the individual molecules themselves. Each molecule, or rather we should say each atom, acts as if there were no other in existence but itself. Whether the atom stands by itself as in the gaseous state, or is bound to other atoms as in the liquid or the solid state, it behaves in exactly the same manner. The deeper question then suggested itself, viz.: what is the nature of that mysterious motion assumed by the atom called heat? Does it consist in excursions across centres of equilibrium external to the atom itself? It is the generally received opinion among physicists that it does. But we think that the experimental results arrived at by Prof. Tyndall, as well as some others which will presently be noticed, are entirely hostile to such an opinion. The relation of an atom to its centre of equilibrium depends entirely on the state of aggregation. Now if heat-vibrations consist in excursions to and fro across these centres, then the *period* ought to be affected by the state of aggregation. The higher the *tension* of the atom in regard to the centre, the more rapid ought its movement to be. This is the case in regard to the vibrations constituting sound. The harder a body becomes, or, in other words, the more firmly its molecules are bound together, the higher is the *pitch*. Two harp-cords struck with equal force will vibrate with equal force, however much they may differ in the rapidity of their vibrations. The *vis viva* of vibration depends upon the force of the stroke; but the rapidity depends, not on the stroke, but upon the tension of the cord.

That heat-vibrations do not consist in excursions of the molecules or atoms across centres of equilibrium follows also, as a necessary consequence from the fact, that the real specific heat of a body remains unchanged under all conditions. All changes in the specific heat of a body are due to differences in the amount of heat consumed in molecular work against cohesion or other forces binding the molecules together. Or, in other words, to produce in a body no other effect than a given rise of temperature requires the same amount of force, whatever may be the physical condition of the body. Whether the body be in the solid, the fluid, or the gaseous condition, the same rise of temperature always indicates the same quantity of force consumed in the simple production of the rise. Now if heat-vibrations consist in excursions of the atom to and fro across a centre of equilibrium *external to itself*, as is generally supposed, then the *real* specific heat of a solid body, for example, *ought to decrease with the hardness of the body*, because an increase in the strength of the force binding the molecules together would in such a case tend to favor the rise in the rapidity of the vibrations.

These conclusions not only afford us an insight into the hidden nature of heat-vibrations, but they also appear to cast some light on the physical constitution of the atom itself. They seem to lead to the conclusion that the ultimate atom itself is *essentially elastic*. For if heat-vibrations do not consist in excursions of the atom, then it must consist in alternate expansions and contractions of the atom itself. This again is opposed to the ordinary idea that the atom is essentially solid and impenetrable. But it favors the modern idea, that matter consists of a force of resistance acting from a centre.—*Phil. Mag.*, [4], xxvii, 346, May, 1864.

11. *On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface;* by Mr. BAXENDELL, F.R.A.S.—Considerations arising out of an investigation of the irregularities which take place in the changes of some of the variable stars led the author, some time ago, to regard it as highly probable that the light of the sun, and also its magnetic and heating powers, might be subject to changes of a more complicated nature than has hitherto been supposed, and that, besides the changes which are indicated by the greater or less frequency of solar spots, other changes of a minor character, and occurring in shorter periods, might also take place. In the hope of detecting these supposed changes, the author resolved to undertake the discussion of a series of magnetical observations, and for this purpose he selected the observations made at the Imperial Observatory of St. Petersburg, the most northern station at which hourly magnetic observations have been made for any lengthened period. Commencing, therefore, with the year 1848, the greatest and least values of the magnetic declination for every day were extracted from the observations, and taking the differences and arranging them in order, it was found, on a careful examination, that they indicated changes of activity taking place in a period of 31 days. The daily oscillations were then arranged in a table of 31 columns, and taking the means of the numbers in these columns, their differences from the general mean for the year were found to be as follows, the unit of value being one division of the scale of the magnetometer, or 26·3″ of arc:

| Day. | | Diff. from general mean. pts. | Day. | | Diff. from general mean. pts. | Day. | | Diff. from general mean. pts. | Day. | | Diff. from general mean. pts. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | − | 8·18 | 9 | − | 7·86 | 17 | + | 0·49 | 25 | − | 1·11 |
| 2 | − | 8·25 | 10 | − | 7·99 | 18 | + | 6·35 | 26 | − | 2·38 |
| 3 | − | 3·32 | 11 | − | 0·70 | 19 | − | 3·05 | 27 | − | 1·43 |
| 4 | + | 1·18 | 12 | + | 7·45 | 20 | + | 2·65 | 28 | + | 8·41 |
| 5 | − | 4·80 | 13 | + | 14·83 | 21 | + | 4·21 | 29 | − | 6 51 |
| 6 | − | 0·21 | 14 | + | 9·22 | 22 | + | 5·80 | 30 | − | 6·51 |
| 7 | − | 1·52 | 15 | + | 1·19 | 23 | + | 3·39 | 31 | − | 5·69 |
| 8 | − | 6·66 | 16 | + | 5·99 | 24 | + | 5·08 | | | |

A projection of these numbers shows that, of the *seventeen* consecutive days, 12 to 28, the amount of oscillation, or range of the magnetic needle, was *above* the mean value on *thirteen* days, and *below* the mean on only *four* days; while of the remaining *fourteen* days the range was *below* the mean on *thirteen* days, and above on *one* day only. The total amount of the differences for the seventeen days of maximum was 67·09, or 3·95 per day, and for the fourteen days of minimum it was 67·02, or 4·78 per day, the mean for the year being 39·86. The ratios of the average excess of a day of maximum, and of the average deficiency of a day of minimum, to the mean value, are therefore as 1 to 10·09, and as 1 to 8·34.

On proceeding to examine the observations for the succeeding years it was found that they could not be represented by a period of 31 days. It appeared, therefore, at first sight, that the period which had been obtained for 1848 was merely accidental; but, guided partly by conclusions drawn from his variable-star investigations, and partly by the high degree of improbability that the results for 1848 could be due to mere accident, the author was led to think that the period he had found for 1848 might be variable, gradually diminishing for a series of years, and afterward gradually increasing, to diminish again when it had completed its cycle of change. Assuming, therefore, that in every year periodic changes took place in the magnetic activity of the sun, the author proceeded to determine for each year the most probable approximate value of the period, and he obtained a series of values gradually diminishing till 1856, when the period was only about 23 days, and afterward rapidly increasing, until, in 1859, it amounted to about 32 days. A glance at these results at once suggested the idea that the variable period thus found was in some way connected with, and dependent upon, the great solar-spot period, the minimum value occurring in the year of minimum frequency of the solar spots, and the maximum values in the years when the spots were most numerous.

Several series of thermometrical observations were now examined for indications of periodical changes in the element of mean daily temperature, and it was found that they exhibited, with unexpected distinctness, changes in this element occurring also in a variable period, the range of variation being, however, somewhat less than in the case of the magnetic element, although the times of maximum and minimum were almost exactly the same. The maximum and minimum values were respectively 31 and 23½ days.

A table is given showing the number of days included in maximum and the minimum portions of each mean period for the years 1848 to 1859, and the number of exceptional days, or those on which during the maximum part of the period the temperature was *below*, and during the minimum part *above*, the mean value. From this table it appears that out of a total number of 165 days of maximum, only 14 were exceptional; and out of a total of 164½ days of minimum, the number of exceptional days was only 16. The mean gives a ratio almost exactly as 1 to 11. Considering that the values of the period in the different years are only approximate, this result may be regarded as affording satisfactory proof of the existence of a variable period of temperature; but a comparison of the total amount of the differences of temperature from the mean and the amount of exceptional differences which is given in another table is much more striking and conclusive. From this comparison it appears that, against a total amount of 255·61° of *plus* differences on maximum days, there were only 8·17° of *minus* differences; and against 258·61° of *minus* differences on minimum days, there were only 11·79° of *plus* differences, the mean ratio of the amount of exceptional differences to the total amount being therefore as 1 to 25·7.

At St. Petersburg the average temperature of the warmer half of the period is not less than 3° greater than that of the cooler half; and as this difference of temperature is repeated at least twelve times in every year, it must necessarily exercise a powerful modifying influence over many meteorological phenomena.

With reference to the differences between the maximum and minimum values of the magnetic and the corresponding values of the temperature period, it is remarked that, owing to occasional interruptions of the magnetical observations and to the enormous extent of the oscillations of the needle on particular days, the values of the magnetic period given in the paper may require some correction; still it is believed that the ranges of the two periods are not identical.

Another period of change having a mean duration of rather over eighteen months is then referred to. The author was first led to it from a discussion of the Greenwich magnetical observations for the years 1848 to 1859; and it has been confirmed by the results of a discussion of temperature observations made at Brussels in Europe, and at Yakoutsk in Asia. It is obvious that this period will at times interfere sensibly with the shorter one, and it is probable that some of the cases which have been called exceptional may be due to this interference.

As it may perhaps excite surprise that in this investigation no use has been made of the fine series of observations taken at the British Colonial Observatories, it is remarked that a little consideration will serve to show that in the early stages, at least, of an inquiry like the present, little or no reliance could be placed on results derived from series of observations in which every seventh day, or fifty-two days in the year, were complete blanks. It has, in fact, been found that the omission of a single day in a year will in some cases produce a very sensible effect upon the final results for the magnetic period. Hence it is that the author regards some of the values he has obtained for this period as being open to correction when he may have an opportunity of discussing more complete sets of observations. Gen. Sabine, in his very elaborate discussions of the magnetical observations made at the colonial observatories, separates the larger movements of the needle from the general mass, and treats them as extraordinary disturbances, assuming apparently that the two classes of ordinary and extraordinary disturbances are due to different causes. The author has, however, not ventured to adopt this mode of proceeding, but has preferred to regard all the movements of the magnet, whether large or small, as having a common origin, and his discovery of the temperature period, by acting upon this view, may be regarded as affording strong evidence in its favor.

With regard to the probable cause of the variability of the short period, the author remarks that the subject is one of great difficulty; for, while the facts seem clearly to belong to the domain of astronomical science, he has found it impossible to frame any hypothesis to account for them without calling in the aid of some principle which has not hitherto been applied to the explanation of astronomical phenomena. It is therefore not without any considerable hesitation that he ventures to observe that the facts would perhaps be best explained by supposing—

1st. That a ring of nebulous matter exists differing in density or constitution in different parts, or several masses of such matter forming a discontinuous ring, circulating round the sun in a plane nearly coincident with the plane of the ecliptic, and at a mean distance from the sun of about one-sixth of the radius of the earth's orbit.

2d. That the attractive force of the sun on the matter of this ring is alternately increased and diminished by the operation of the forces which produce the solar spots, being greatest at the times of minimum solar-spot frequency, and least when the spots are most numerous.

3d. The attractive force being variable, the dimensions of the ring and its period of revolution round the sun will also vary, their maximum and minimum values occurring respectively at the times of maximum and minimum solar-spot frequency.

In reference to the nature of the varying attractive force, it is not improbable that the matter of the supposed ring may be highly diamagnetic, and being much nearer to the sun than any of the known planets, of much greater bulk and lightness, and being subjected to a much higher temperature, it will be very sensibly affected by the changes which take place in the magnetic condition of the sun; and when interposed between the earth and the sun, it may act not only by reflecting and absorbing a portion of the light and heat which would otherwise reach the earth, but also by altering the direction of the lines of magnetic force. The changes of temperature at the surface of the earth will thus be due partly to differences in the amount of heat received from the sun, and partly to changes in the movements of the great currents of the air produced by alterations in the earth's magnetic condition. If the larger part of the difference of temperature is due to the latter mode of action, we might expect that during the warmer half of the period the mean direction of the wind at any given station would be sensibly different from that during the cooler half; and also, that the epochs of maximum and minimum temperature would not be the same at all parts of the earth's surface. Both of these conclusions are borne out by the results given in the paper. Thus at St. Petersburg, in 1859, the mean direction of the wind on maximum days was S. 54° W., and on minimum days S. 73° W. or 19° more to the west of South; and at Sitka, on the northwest coast of North America, in 1851, the mean direction on maximum days was S. 32° W., and on minimum days S. 56° W., the difference being 24°. As striking instances of the differences in the epochs at distant stations, it may be stated, that in 1859 the epoch of maximum at St. Petersburg corresponded precisely with the epoch of minimum at Madras; and that at Pekin, in 1851, the epoch of minimum was exactly coincident with the epoch of maximum at Sitka.

Changes in the amount of heat received from the sun, sufficient to produce the variations of temperature observed at any given station, would no doubt affect the movements of the great currents of the atmosphere, though not to the extent indicated by the observations; but it is difficult to conceive that they could produce the differences in the epochs which are found to take place. We may therefore fairly conclude that the action of the supposed ring of nebulous matter is principally of a magnetic, and but slightly of a thermal character.

It is suggested that the greater range of variation of the magnetic, as compared with the temperature period, may be due to the inertia and elasticity of the great currents of air, the inertia tending to lengthen the temperature period, and the elasticity to shorten it; but as the inertia will act with greatest effect when the magnetic period is at its minimum, while on the other hand the elasticity will be most effective when this period is at its maximum, the result will be a range of variation of the temperature period somewhat less than that of the magnetic.

Adopting, for the present, the maximum and minimum values of the temperature period as being determined with greater accuracy than those of the magnetic period, the greatest and least values of the sidereal period of revolution of the ring will be 29·12 and 22·08 days respectively. From these numbers we find that the greatest distance of the ring from the sun is 0·185, the radius of the earth's orbit being taken as unity; the least distance, 0·154; and the mean 0·169. Taking Mr. Hind's value of the mean distance of the earth from the sun, namely, 91,328,-600 miles, we have:

| | | | |
|---|---|---|---|
| Greatest distance of the ring | | | = 16,921,000 miles. |
| Least | " | " | = 14,068,000 " |
| Mean | " | " | = 15,494,500 " |

and the range of movement to and fro in a radial direction = 2,853,000 miles. The greatest attractive force of the sun on the ring being taken as unity, the least will be 0·691. The difference is therefore nearly one-third of the maximum amount. It will be evident that this difference may be regarded as a measure of the forces which are concerned in the production of the solar spots.

The results of the elaborate investigations of the motions of the planet Mercury, made by Leverrier, led that accomplished mathematician to attribute a certain unexplained excess in the motion of its perihelion to the action of a disturbing body circulating round the sun within the orbit of Mercury; and, from a discussion of the probable mass of the disturbing body, he concluded that it could not be concentrated in a single planet, and that it consisted of a ring of small bodies similar to that which is known to exist between the orbits of Mars and Jupiter; and it is remarkable that the mean distance, which he seemed to regard as the most probable, is precisely that which the author has found for the ring of nebulous matter, whose existence he has assumed to account for the phenomena described in his paper. This unexpected and unlooked-for agreement between results arrived at from considerations and by methods so totally different, seems to establish the existence of this ring with quite as much certainty, as the results of the profound researches of Adams and Leverrier established the existence of Neptune before that planet had been actually seen. This ring, however, owing to its proximity to the sun, may never be seen, and, like the dark companions of Procyon and Sirius, it may only be known to us through its action on the other bodies of the system of which it forms a part. Should future researches place its existence beyond doubt, this will, it is believed, be the first instance in which the conclusions of physical astronomy have been confirmed by the results of an investigation of magnetical and meteorological phenomena. Whether, however, the hypothesis which the author has ventured to put forward be accepted or not, it is now very evident that observations of solar phenomena merit a much larger share of attention than has ever yet been devoted to them. It has long been suspected that the same causes which produce the spots on the sun's disc must in some way have an important influence on the phenomena of our own atmosphere. The facts now given convert this suspicion into a certainty; and it is perhaps not too much to say that meteorology can never take rank as a true science while our knowledge of the sun remains in its present imperfect state. Moreover, there is little doubt that many questions of high physical interest depend for their solution upon our obtaining a more intimate acquaintance than we yet possess with the operations which are going on in the great centre of our system. It is therefore much to be desired that some of the many observatories which are now established in various parts of the world should be specially devoted to observations of the sun, and of solar phenomena generally; and that the principal magnetical and meteorological elements should be observed daily, without interruption, at all the regular magnetical and meteorological observatories.

It may be stated that the values of the variable-temperature period, given in this paper, were derived from observations made at St. Petersburg, Wardoe, Gorki, Barnaoul, Irkoutzk, Nertchinsk, Yakoutsk, Pekin, Madras, Novo-Petrovsk, Lougan, Zurich, Geneva, Milan, Brussels, Greenwich; Jakobshavn, in Greenland; and Sitka, on the northwest coast of North America. The comparison of the variable-temperature period with the solar-spot period extends over the twenty-seven years, 1833–59, and therefore includes three maxima and three minima of solar-spot frequency.—*Proceedings Manchester Lit. and Phil. Soc.*, March 8, 1864.

## II. MINERALOGY AND GEOLOGY.

1. *Analyses of Swedish and Norwegian Minerals;* by J. A. MICHAELSON.—(1.) *Radiolite.*—Specimens of the radiolite occurring in the zircon-syenite at Brevig (Norway), and collected at the locality by the author, had a flesh-red color, a radiated structure, and but a feeble lustre. H.=5. G.=2·22. In the closed tube gave water, and B.B. fused to a white opaline glass. Decomposed by acids. Composition:

| $\overline{\text{Si}}$ | $\bar{\text{Al}}$ | $\bar{\text{Fe}}$ | $\dot{\text{Ca}}$ | $\dot{\text{Na}}$ | $\dot{\text{K}}$ | $\dot{\text{H}}$ | |
|---|---|---|---|---|---|---|---|
| 47·73 | 26·04 | 0·53 | 2·22 | 13·37 | 0·40 | 10·24 | = 100·53 |

showing the mineral to be natrolite in which a small portion of the soda is replaced by lime.

(2.) *Schefferite: a supposed new variety of Pyroxene.*—Schefferite occurs at Langbanshytta. Color, reddish-brown. H.=5·5. G.=3·39. B.B. fuses slowly to a black glass. With borax and salt of phosphorus gives an amethystine bead in the outer flame, becoming colorless in the reducing flame; with salt of phosphorus leaves a skeleton of silica. Fused with soda yields a green bead. Treated with chlorhydric acid evolves chlorine, and effects a partial decomposition of the mineral. Composition:

| | $\overline{\text{Si}}$ | $\dot{\text{Ca}}$ | $\dot{\text{Mg}}$ | $\dot{\text{Mn}}$ | $\dot{\text{Fe}}$ | $\bar{\text{Fe}}$ | Ign. | |
|---|---|---|---|---|---|---|---|---|
| | 52·31 | 19·09 | 10·86 | 10·46 | 1·63 | 3·97 | ·0·60 | = 98·92 |
| Oxygen, | 27·19 | 5·42 | 4·34 | 2·35 | 0·37 | 1·19 | | |
| | | 13·67 | | | | | | |

The oxygen ratio of silica to bases is 2 : 1, and the author places the mineral near Jeffersonite. It is associated with rhodonite, and heretofore has been supposed by many mineralogists to be a variety of garnet. [Michaelson's analysis is interesting as proving the mineral to be a manganesian pyroxene, but the figures in regard to the relative amount, and state of oxydation of the iron and manganese are not trustworthy, for if the mineral evolves chlorine, on treatment with chlorhydric acid, it must contain one of the higher oxyds of manganese, although only protoxyd is mentioned in the analysis. Michaelson determined the amount of protoxyd of iron by titration after fusion of the mineral with borax, and subsequent solution of it in chlorhydric acid in the presence of an atmosphere of carbonic acid. But the solution of a mineral containing a higher oxyd of manganese with protoxyd of iron, necessarily oxydizes a portion of the iron, at the expense of the oxygen of the oxyd of manganese, even though the atmospheric air be excluded, consequently the amount of sesquioxyd of iron given in the analysis must be incorrect, and it is possible that the mineral contains no sesquioxyd of iron. It may be considered a slightly altered manganesian pyroxene, and it would seem, therefore, that the new name is superfluous.—G. J. B.]

(3.) *Hedyphane.*—Hedyphane, from Langbanshytta, has a grayish-white color, inclining slightly to yellow, is translucent, and has a greasy lustre, and uneven fracture. B.B. fuses easily to a white enamel, and on charcoal, gives an arsenical odor. H.=4. G.=5·46. Composition:

| Cl | P̈ | Äs | Ṗb | Ċa |
|---|---|---|---|---|
| 3·06 | 3·19 | 28·51 | 57·45 | 10·50 |
| 2·93 | 0·86 | | | |

This is equivalent to 11·70 PbCl, 2·02 P̈, 28·51 Äs, 48·13 Ṗb, 10·50 Ċa, giving the formula PbCl+3[(Ṗb, Ċa)³(Äs, P̈)].

(4.) *Orthite-like mineral from Aarö near Brevig.*—This dark-brown mineral is associated with melinophane. It has a vitreous lustre on the fracture, and in thin splinters is transparent to translucent. Hardness, between 3 and 4. G.=3·44. Not crystallized. Composition:

| | S̈i | Ċe | L̇a, Ḋi | Ẏ | B̈e | Äl | Z̈r | F̈e | Ċa | Ṁg | Ṅa | Ḣ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. | 29·21 | 9·79 | 15·60 | 1·63 | 4·27 | 2·81 | 5·44 | 6·42 | 14·93 | 0·45 | 2·45 | 5·50 |
| 2. | 28·80 | 11·47 | 14·12 | 1·49 | | 17·51 | | | 16·06 | *tr.* | — | — |

Analyses 2 was by Nobel, who obtained also 0·83 (?) precipitated by sulphuretted hydrogen. The mineral was decomposed by chlorhydric acid. A portion of the iron existed as protoxyd, but the small quantity of the mineral operated upon would not permit its determination. The author considers that the mineral is nearly related to *Erdmannite.*—*Jour. prakt. Chem.*, xc, 106. G. J. B.

2. *Kokscharovite.*—Hermann has further investigated kokscharovite. The specimen examined was associated with lapis-lazuli and calcite, and after careful picking out, the coarse powder was separated from adhering calcite by dilute chlorhydric acid.

The mineral thus purified was in crystalline fragments of a dirty-white color, with a vitreous lustre, and was translucent on the edges. G.=2·97. In the closed tube, gave only traces of water. B.B., in the forceps, fused easily to a white translucent pearl, coloring the flame yellow; with borax, gave a clear colorless glass. Composition:

| | S̈i | Äl | Ḟe | Ċa | Ṁg | K̇ | Ṅa | Ign. | |
|---|---|---|---|---|---|---|---|---|---|
| | 45·99 | 18·20 | 2·40 | 12·78 | 16·45 | 1·06 | 1·53 | 0·60 | = 99·01 |
| Oxygen, | 23·89 | 8·50 | 0·53 | 3·63 | 6·46 | 0·18 | 0·39 | | |

The form of the mineral, as has already been stated in this Journal ([2], xxvi, 354), is that of hornblende; if the alumina is considered as replacing silica, the oxygen ratio of these to the bases is 2·98 to 1, differing very materially from all of the aluminous hornblendes and pyroxenes investigated by Rammelsberg, and requiring a portion of alumina to be basic in order to be included under the general formula $(\dot{R}^3\bar{\bar{R}})\dddot{Si}^2$.—*Jour. prakt. Chem.*, lxxxviii, 197. G. J. B.

3. *Samarskite.*—Finkener has found that samarskite contains zirconia and thorina, both of these substances having been overlooked by previous analysts. Analysis gave 4·35 pr. ct. zirconia and 6·05 thorina. —*H. Rose in Ber. Preuss. Akad.*, Nov., 1862. G. J. B.

4. *Kupfferite.*—Kupfferite is from a graphite mine in the Tunkinsk Mountains, and was named by Kokscharow in honor of the eminent physicist Kupffer. It has the form of actinolite, and is remarkable from its containing oxyd of chromium. A similar mineral has been obtained by Hermann from the Ilmen Mountains; it occurs in granite, forming an aggregate of prismatic crystals, the planes of which have an angle of 124° 15′. Cleavage prismatic. Color of the crystals emerald-green when fresh, but on exposure weathering to a brownish color. In thin splinters, translucent. Lustre, vitreous. H.=5·5. G.=3·08. In the closed tube, gives traces of water, but is otherwise unchanged. B.B., in the forceps, becomes opaque and white, but is infusible. Dissolves in borax, giving a chrome-green glass. Composition:

| | $\dddot{Si}$ | $\bar{\bar{Cr}}$ | $\dot{Ni}$ | $\dot{Fe}$ | $\dot{Ca}$ | $\dot{Mg}$ | $\dot{K}, \dot{Na}$ | Ign. |
|---|---|---|---|---|---|---|---|---|
| | 57·46 | 1·21 | 0·65 | 6·05 | 2·94 | 30·88 | *tr.* | 0·81=100·00 |
| Oxygen, | 29·85 | 0·38 | 0·14 | 1·34 | 0·83 | 12·03 | | |

This gives the oxygen ratio of bases to silica 1 : 2·02, which is almost precisely that of pyroxene, while the form is like hornblende. Hermann considers that the presence of oxyd of chromium entitles the mineral to be considered a distinct species, and also calls attention to the large amount of magnesia, remarking that it may be looked upon as an *enstatite* with the form of hornblende.—*Jour. prakt. Chem.*, lxxxviii, 195. G. J. B.

5. *Planerite, a new mineral.*—Hermann has described a new phosphate from Gumeschefsk in the Urals. It resembles wavellite in its mode of occurrence and also in composition. It is found lining clefts in a quartzite, forming a thin botryoidal coating. Color olive-green to verdigris-green, the darker color being due to superficial oxydation of the iron. Structure crypto-crystalline, the surface drusy, and on the fracture fibrous. Lustre dull, under the magnifier glistening. Streak greenish-white. H.=5. G.=2·65. In the closed tube, decrepitates and gives off much water. In borax fuses easily, giving reactions for copper. Acids attack the mineral only imperfectly, but it is easily dissolved on boiling with caustic soda, leaving a black residue consisting of the oxyds of copper and iron. Analysis gave:

| | $\bar{\bar{P}}$ | $\bar{\bar{Al}}$ | $\dot{Fe}$ | $\dot{Cu}$ | $\dot{H}$ | |
|---|---|---|---|---|---|---|
| | 33·94 | 37·48 | 3·52 | 3·72 | 20·93 | = 99·59 |
| Oxygen, | 19·02 | 17·50 | 0·78 | 0·75 | 18·60 | |

Hermann makes the formula $4(\bar{\bar{Al}}^3\bar{\bar{P}}^2+9\dot{H})+3(\dot{Cu},\dot{Fe})\dot{H}$.—*Jour. prakt. Chem.*, lxxxviii, 193. G. J. B.

6. *Forcherite.*—The name Forcherite has been given by Auhhorn to a yellow opal found in gneiss at Reittelfeld in Styria. An examination by R. L. Maly proved the mineral to be hydrated silicic acid colored yellow by sulphid of arsenic.—*Jour. pr. Chem.*, lxxxvi, 501.

7. *Garnet.*—Pisani has analyzed the octahedral garnet from Elba. It occurs associated with chlorite and yellow epidote in octahedral crystals, from two to five millimetres in diameter, of a honey-yellow color. Hardness, somewhat greater than quartz. B.B., fuses to a black enamel. In the spectroscope, shows lime, with traces of soda. Slowly decomposed by chlorhydric acid, but more rapidly when the mineral is previously ignited. Composition:

| $\bar{\bar{Si}}$ | $\bar{\bar{Al}}$ | $\bar{\bar{Fe}}$ | $\dot{Ca}$ | $\dot{Mg}$ | $\dot{Mn}, \dot{Na}$ | Ign. | |
|---|---|---|---|---|---|---|---|
| 39·38 | 16·11 | 8·65 | 36·04 | 1·00 | *traces.* | 0·31 | = 101·49 |

This is nearly the same composition as *grossular.*—*Compt. Rendus*, lv, 216; *Jour. pr. Chem.*, lxxxvii, 383.

8. *Native zinc.*—Phipson has described the occurrence of native zinc in a basalt from Brunswick near Melbourne in Australia. A specimen of this zinc was exhibited at the London Exhibition of 1862.—*Comptes Rendus*, lv, 218.

9. *Esmarkite.*—A so-called *esmarkite* from Norway, analyzed by Pisani, proves to be identical with scapolite. It would thus seem that this name has been applied to three minerals: (1) a variety of iolite, (2) to datholite, and (3) to scapolite. Pisani suggests the propriety of dropping the name altogether.—*Comptes Rendus*, lv, 450.

10. *Infusorial earth from Bohemia.*—R. Hoffmann has analyzed the infusorial earth from Kutschlin near Bilin, and from Meistersdorf, and also the diatom deposit from near the Louisa-spring in Franzensbad. Analyses: 1. Upper layer, Bilin. 2. Lower layer, Bilin. 3. Meistersdorf. 4. Franzensbad.

| | 1. | 2. | 3. | 4. |
|---|---|---|---|---|
| Ammonia, | 0·03 | 0·01 | 0·34 | |
| Potash, | 0·02 | 0·30 | 0·24 | 0·40 |
| Soda, | 0·30 | *tr.* | *tr.* | |
| Magnesia, | —— | 0·43 | 0·36 | 0·05 |
| Lime, | 0·41 | 0·44 | 0·64 | *tr.* |
| Alumina, ferric oxyd, | 6·81 | 5·40 | 5·60 | 0·91 |
| Sulphuric acid, | 0·12 | *tr.* | 0·54 | —— |
| Phosphoric acid, | 0·24 | *tr.* | *tr.* | 0·19 |
| Silica, | 74·20 | 80·30 | 72·60 | 77·00 |
| Organic matter, | 4·20 | 1·30 | 13·20 | 15·45 |
| Water, | 13·30 | 10·90 | 7·00 | 6·00 (loss) |
| | 99·63 | 99·08 | 100·52 | 100·00 |

No. 1 is the "tripoli" or polishing powder; it has a density of 1·862, absorbs water rapidly, taking up about $1\frac{1}{2}$ times its weight, and splits into thin plates. Density of No. 2 equal 1·944; it is harder than No. 1, and not used for polishing. None of these infusorial earths scratch glass. The total amount of nitrogen contained in No. 4 was 0·491 p. c.—*Jour. prakt. Chem.*, xc, 467. G. J. B.

11. *On a cavern with human remains in the Pyrenees;* by Messrs. F. Garrigou and L. Martin.—The cavern which has been explored recently by Messrs. Garrigou and Martin, is one called *Espélugues*, situated

in the commune of Lourdes, and the department of the Hautes Pyrénées. Two years since, it was visited by Alphonse Milne Edwards and Lartet, who published a detailed description in the Annales des Sciences Naturelles. Messrs. Garrigou and Martin add many interesting facts to those brought forward by these two earlier observers, from which we cite the following:—

"Within the cavern, toward the entrance of the great hall, there are great numbers of large blocks of limestone lying together upon a bed of rounded stones. Among these blocks, and especially at their base, are heaps of cinders and charcoal, some fragments of which occur at different places in the general deposit of the cavern. Bones, jaws, and teeth of different Mammals were obtained, especially from the lower part of the deposit. The surface layers of the deposit, which had been already turned up, afforded us only rare fragments, and these, before commencing our second day's examination, we carefully laid aside for separate study. Quantities of cut flints, bones, and horns of various stags, worked and shaped into the form of instruments and weapons, and some carved bones, lay in confusion along with the ashes and coal. Some of these remains were from the upper level of the deposit already worked.

We shall describe first the relics in the upper layer, explored before us by Milne Edwards, and then those of the lower strata, now examined by us.

(1.) *Upper layer.*—We cannot do better, in giving the list of the Mammals found in the upper portion of the deposit, than to repeat what has been written by Alphonse Milne Edwards. He found remains of the Fox, Horse, Wild Boar, Stag, Chamois, Wild Goat (Bouquetin), Reindeer, Aurochs, Ox, Mole, Field Mouse, and Birds. We add, to complete this list, a Goat smaller than the Bouquetin and larger than the Chamois, and a Sheep of the size of the Goat.

The bones of all these animals are broken like those of the Kjoekkenmodding of Denmark, of the lake habitations of Switzerland, and of the caverns, of the age of stone, of Ariège.

Among these paleontological fragments, some, on careful examination, led us to infer that the domestication of certain animals had been in practice during the period under consideration.

Among the broken bones of the surface, some had evidently been attacked by Rodents. Near by were others bearing marks of the teeth of a Carnivore (a dog, beyond doubt).

Among the debris that we have ourselves examined, we collected, twenty centimetres below the surface, a small fragment of a rib of a Ruminant bearing a sculptured design of fine finish, and differing in this respect from objects of the same kind found at Bruniquel and in the caves of Périgord. The sculpturing is a portion of a larger design whose signification we can not give. An antenna of an insect, however, seems to be represented by one of the principal markings in the drawing.

We will conclude what we have to say of the upper part of the cavern deposit (a complete description of which is given by Alph. Milne Edwards), by saying that the specimens collected in this part seem more fresh, less altered, and less colored than those of the lower layers. This last fact has strongly impressed the minds of those to whom we have shown the results of our excavations.

(2.) *Lower layers.*—The list of animals found in the lower beds of the cavern differs a little from the preceding. We notice the Horse, the common Stag, the Reindeer, the Aurochs, an Ox smaller than the Aurochs but larger than that found in the upper beds, the Bouquetin, a large Sheep, two Rodents, and some bones of birds. The teeth of the Horse are more abundant than those of the Ox or Reindeer, but the bones of the Reindeer are more numerous than those of other Ruminants.

All these bones are broken like those which are found in caves inhabited by man; the heads of the bones alone are entire.

While the bones of the surface are grayish white externally, those of the lower part of the deposit are colored red, as at Bruniquel, Lyzies, May-d'Azil, and Izeste. The former do not adhere to the tongue, and evidently contain gelatine, while the latter adhere to the tongue and contain no gelatine. In order to be sure as to the gelatine, we burned two fragments of bone on live charcoal; that taken from the surface afforded almost immediately an insupportable empyreumatic odor, and the other, taken from below, no odor at all.

Throughout the extent of the bed examined by us, even to the rolled pebbles at the surface, there are found, along with the bones, wrought flints, and also instruments and tools made of the horns of the Reindeer and common Stag, and of bone. More than four hundred flints, most of them wrought, and coarsely so, were turned out. These may be classified as follows:

(1) Knives; (2) scrapers; (3) arrow heads roughly hewn, and sometimes having the lower extremity long for attachment to a handle; (4) wrought hatchets of small size, but of the same form with those from the diluvium of Abbeville and Amiens; (5) fragments of flint which were chipped from the instruments here described.

More than twenty-four objects of stag horn, of reindeer horn and of wrought bones, and also one bone very coarsely sculptured, rewarded our excavations in the lower beds. The sculptured bone represents, as nearly as we can judge, a fish with ventral fins and a divided tail. The skill of the artist was inferior to that in the case before mentioned.

The wrought objects may be divided into two categories: those coarsely wrought, and those of more finish. The collection of objects is very closely like that of the grotto of Izeste (Basses Pyrénées).

It appears evident to us that the inhabitants contemporary with the inferior deposits of Lourdes, and those of the cavern of Izeste, had a degree of civilization nearly equal to, and yet a little below, that of the occupants of the caverns of Périgord, Bruniquel, etc.

From a review of the facts, it is plain that the age of the upper layers of the deposit in the cavern of Lourdes is not the same as that of the lower.

Our examination of the bones collected by us from the upper beds, leads to the same result that has been announced by Alph. Milne Edwards, after his investigations with Mr. Lartet. We conclude from the presence of the Aurochs, the existence of domestic animals, the discovery of bones gnawed by dogs, the almost complete preservation of the gelatine in the bones and their deeper color, and by the discovery of a bone finely sculptured, that the upper beds belong to an age more recent than that of the lower beds. This we would call, as done by Messrs.

Edwards and Lartet, the age of the Aurochs, with which Man was contemporary.

As to the lower beds, it is evident to us, from the abundant remains of the Reindeer, including large quantities of its horns; from the coarseness of its wrought objects, its worked flints and its sculpture; from the reddish brown color of the bones, and from the absence of gelatine and their adhering to the tongue, that they pertain to an epoch more ancient than the preceding. It was the age of the Reindeer, parallel with that which we have distinguished in describing the grotto of Izeste.

The cave of Lourdes has thus afforded the first example of the direct superposition of the beds of the two consecutive paleontological epochs of the Quaternary or Post-tertiary period—such as they have been indicated by our learned and venerated master, Mr. Lartet."—*L'Institut, May* 11.

12. *On further discoveries of Flint Implements and Fossil Mammalia;* by J. WYATT, Esq., F.G.S.—The opening of a section at Summerhouse Hill gave the author an opportunity of ascertaining whether the gravels at that lower level exhibited any features different from those of the upper level at Biddenham. Although, as might have been expected, some of the species of mammals were found to be common to the two localities, yet that under notice furnished some species of mammals, as well as of land and freshwater shells, together with a few types of flint implements, differing from those met with at higher levels.

Mr. Wyatt described the section at Summerhouse Hill in detail, showing that it tended to support Mr. Prestwich's opinions respecting the formation of gravel beds; he also described the Flint Implements he had recently found, comparing them with known specimens from the Valley of the Somme and elsewhere; and he stated that he was now enabled to add two new localities near Bedford—Summerhouse Hill and Honey Hill—to those already known as having furnished similar weapons.—*Phil. Mag.*, [4], xxvii, 544.

13. *On some recent discoveries of Flint Implements in Drift Deposits in Hants and Wilts;* by JOHN EVANS, Esq., F.G.S., etc.—Flint implements having recently been found on the sea-shore about midway between Southampton and Gosport, by Mr. James Brown of Salisbury, and also at Fisherton, near Salisbury, by Dr. H. P. Blackmore of that place, the author visited these localities in company with Mr. Prestwich, and gave the results of his observations in this paper.

After describing the implements from near Southampton, and having shown that their condition is identical with that of the materials composing the gravel capping the adjacent cliff, Mr. Evans proceeded to review the evidence of the great antiquity of these remains, which rested mainly on the circumstance that these gravel-beds, like those of Reculver, are of fluviatile origin, although now abutting on the sea.

In like manner the author then described the Fisherton implements, and the gravel-pits from which they were obtained. The relation of the high-level gravels (in which the implements were found) to the lower-level gravels of the valley of the Avon was next discussed, and the geological features of the former deposits particularly described, lists of the fossils (including the Mammalia and the Land and Freshwater Shells) being also given. Mr. Evans came to the conclusion that the fossils bore evidence that the climate, at the time when they were deposited, was more rigorous, at any rate in the winter, than it now is; and to this cause he attributed the comparatively greater excavating power of the early Post-pliocene rivers.—*Phil. Mag.*, [4], xxvii, 544.

14. *Lake-dwellings or Pfahlbauten in Bavaria.*—Ancient lake-dwellings, in general like those of Switzerland, have been discovered in Bavaria in the Sternberg Lake.

15. *On some Bone- and Cave-deposits of the Reindeer-period in the south of France;* by Mr. John Evans, F.R.S.—The deposits to which the author particularly called attention in this paper are those which have been, and are still being, explored under the direction of Mr. Lartet and Mr. Christy, and which were visited by him under the guidance of the latter gentleman, and accompanied by Mr. Hamilton, Professor Rupert Jones, Captain Galton, Mr. Lubbock and Mr. Franks. Mr. Evans first gave a detailed description of the physical features of the valley of the Vézère, and of the contents of the caverns of Badegoule, Le Moustier, La Madeleine, Laugerie-Haute, Laugerie-Basse, the Gorge d'Enfer, and Les Eyzies, giving a list of the animal remains discovered, which are, for the most part, of the same species from all the caverns. The author then discussed the antiquity of the deposits according to four methods of inquiry—namely, from geological considerations with regard to the character and position of the caves, from the paleontological evidence of the remains found in them, from the archeological character of the objects of human workmanship, and from a comparison with similar deposits in neighboring districts in France; and he came to the conclusion that they belonged to a period subsequent to that of the *Elephas primigenius* and *Rhinoceros tichorhinus*, but characterized by the presence of the reindeer and some other animals now extinct in that part of Europe.—*Proc. Geol. Soc.*, in *The Reader*, July 2.

16. *On the Cavern of Bruniquel, and the Human Remains found therein;* by Professor Owen.—Professor Owen minutely details the circumstances under which these discoveries were made; and states that the contemporaneity of the human remains with those of the extinct and other animals with which they are associated together with the flint and bone implements is shown by the evidences of the plastic condition of the calcified mud of the breccia at the time of interment, by the chemical constitution of the human bones, corresponding with that of the other animal remains, and by the similarity of their position and relations in the surrounding breccia.

Among the principal remains of the men of the flint period described are the following: (1) The hinder portion of the cranium, with several other parts of the same skeleton, which were so situated in their matrix as to indicate that the body had been interred in a crouching posture, and that, after decomposition and dissolution of the soft parts, the skeleton had yielded to the superincumbent weight; (2) an almost entire calvarium, which is described and compared with different types of the human skull, which Professor Owen shows to be superior in form and capacity to the Australian type, and more closely to correspond with the Celtic type, though proportionally shorter than the modern Celtic and the form exhibited by the Celtic cranium from Engis, Switzerland; (3) jaws and teeth of individuals of different ages.

After noticing other smaller portions of human crania, the lower jaw and teeth of an adult, the upper and lower jaws of immature individuals are described, the characters of certain deciduous teeth being referred to. The proportions of the molars are not those of the Australian, but of other races, and especially those of ancient and modern Europeans. As in most primitive or early races in which mastication was little helped by arts of cookery, or by various and refined kinds of food, the crowns of the molars, especially of *m* 1, are worn down, beyond the enamel, flat and smooth to the stumps, exposing there a central tract of osteodentine without any signs of decay.

The paper was illustrated by a view and plans of the cavern, and by figures of the principal human remains, and of two implements of bone on which the Viscomte de Lastic had discovered, on removal of the breccia, outline figures of the head of a reindeer and the head of a horse in profile.—*Proc. Roy. Soc.*, in *The Reader*, June 18.

17. *On Human remains in Caves at Gibraltar;* by GEO. BUSK. (Letter addressed to the Editors of "The Reader," and dated 15 Harley street, July 16, 1864.)—On the 30th of January last you afforded me an opportunity of stating the circumstance of the discovery by Capt. Brome, Governor of the Military Prison at Gibraltar, of a cavern, or rather a series of caverns and fissures, on Windmill Hill in that place. I also stated that Captain Brome had forwarded, some time before, a very large and valuable collection of various animal and human remains, which were in course of examination by Dr. Falconer and myself. Since then, we have received from the same gentleman a second very large consignment of similar remains from the same locality, and which, like the former, were packed, arranged, and ticketed with the greatest care and discrimination. Still more recently, Captain Sayer (the author of the latest History of Gibraltar) has brought over for us some human and other remains from a different place, about 200 feet lower down than the Windmill Hill Flats. These remains, as we understand from Captain Sayer, were procured some years since by Sir James Cochrane from a very deep and till then unexplored cavern, the entrance of which is in his own garden. And again, within the last few days, we have been furnished with additional human and other bones from Captain Brome; but we are as yet uninformed as to the precise locality whence these have been derived. We have also received from Mr. Mawe two portions of bone-breccia containing a considerable number of fragments, amongst which the most important is a large portion of the plastron of a species of tortoise. Captain Douglas Galton has also communicated to us two large fragments of ossiferous breccia procured from Camp Bay, close to Rosia Bay.

In my former communication I gave a rough list of the chief animals whose bones were contained in the first collection sent by Captain Brome, and referred to some great peculiarities observable in many of the human bones. The second collection forwarded by the same gentleman, although it has not added many new species to those contained in the former, has yet been of inestimable value from the additional means it has afforded us for the proper identification of many of the species. The human remains contained in the second collection were, as in the previous one, very numerous, but, unfortunately, in an equally fragmentary condition, especially as regards the crania. In the two collections we have nearly 400 fragments of skulls, most of the fragments very small, and nearly all presenting signs of very ancient fracture. Out of this enormous mass I have as yet succeeded in building up no more than about half of the cranium of one individual and smaller portions of three or four others. But the larger specimen thus constructed suffices in some measure to give an idea of the general contour and size, &c., of the skull when entire; and, taken in conjunction with the numerous more or less perfect frontal and lower jaw-bones, this cranium and the other less complete specimens afford an insight into the cranial conformation of some of the human beings then existing on the Rock of Gibraltar. One conclusion that we were inclined to adopt, as I stated before, was that the lower jaws, at any rate, might be referred to two distinct types. This opinion appeared to be strengthened also by the circumstance that some of the other bones of the skeleton presented very remarkable distinctive characters. Among the numerous leg and thigh bones, belonging, as near as I can estimate, to 35 or 36 individuals, are many so singular and, as it may almost be said, so monstrous in their form as to have excited the astonishment of all anatomists who have beheld them. Notwithstanding diligent search and inquiry, we have as yet been unable to meet with, or hear of, any similar bones in collections in this country, although in Paris, through the great kindness of Messrs. Pruner-Bey and Lartet, Dr. Falconer, on a late visit to that city, was put in possession, for the purpose of comparison, of some from Algeria and one from Laugerie, in the valley of the Vézère, approaching the same type.

Under these circumstances, any further contributions to our anthropological materials from Gibraltar became of the utmost importance to us. The human cranium brought by Captain Sayer from the lower, or Sir James Cochrane's, cave was a welcome addition of this kind. It is fortunately quite perfect, except that the lower jaw properly belonging to it has been replaced by one of a different individual, and we are consequently unable to determine the character of that highly important bone. The skull itself, as were most of the bones with which it was accompanied, was encased in a very hard gray stalagmitic crust, in some parts several inches thick, and evidently the result of very long and slow deposition. But when this was removed, the bone stood out as fresh to all appearance as if it had been carefully macerated and cleaned. It is a small, roundish, symmetrical cranium; but we have not yet so critically compared it as to allow of any definite opinion being given on the present occasion as to its nearest probable affinities. In one respect it is of extreme interest, from its being associated with several leg bones presenting the peculiar compressed form above adverted to, and among which one, from the condition of the bone itself and the exact similarity of the calcareous incrustation upon it, most probably belongs to the same individual. We thus appear to be furnished with a clue to the cranial conformation of the "sharp-shinned" or *platycnemic* race—a point of considerable importance.

But by far the most important addition to the human remains from Gibraltar is contained in the last contribution just received from my friend Captain Brome. This collection includes, besides several quadrupedal bones, the greater part of a human cranium, and a lower jaw not belonging to it. The cranium resembles, in all essential particulars, including its great thickness, the far-famed Neanderthal skull; but, in many respects, it is of infinitely higher value than that much-disputed relic, inasmuch as it retains the entire occipital region, including the hinder margin of the foramen magnum, great part of the base, the whole of one temporal bone (thus giving the precise situation of the auditory opening), and nearly the entire face, including the upper jaw, with most of the much and curiously-worn teeth. As it is precisely these parts that are wanting in the Neanderthal calvarium, of which the present is, in other respects, almost an exact counterpart, the value of this cranium in the study of priscan man can not be rated too high. Its discovery also adds immensely to the scientific value of the Neanderthal specimen, if only as showing that the latter does not represent, as many have hitherto supposed, a mere individual peculiarity, but that it may have been characteristic of a race extending from the Rhine to the Pillars of Hercules; for, whatever may have been the case on the banks of the Düssel, even Professor Mayer will hardly suppose that a rickety Cossack engaged in the campaign of 1814 had crept into a sealed fissure in the Rock of Gibraltar.

As this cranium will shortly, I hope, be fully described and figured, I will not now enter into any further particulars concerning it, more than simply to remark that, in several respects, owing perhaps to its greater completeness, it presents more strongly marked pithecoid characters than even the Neanderthal calvarium possesses, and that, from the mineral condition of the bone itself, apart from its intrinsic characters of form, there can be no doubt of its enormous antiquity. The other bones also sent with it, and obviously taken from the same locality, though not themselves of an extinct species, yet belong to one (*Ibex*) whose remains occur very abundantly throughout the Rock in the oldest breccia, in which are also contained those of at least one, if not of two, wholly extinct species of Rhinoceros, and of several other animals which are extinct, so far as Europe is concerned.—*The Reader*, July 23.

18. *On the Rhætic Beds and White Lias of Western and Central Somerset, and on the Discovery of a new Fossil Mammal in the gray Marlstones beneath the Bone-bed;* by Mr. W. Boyd Dawkins.—After describing the sections in the district, and showing the paleontological relations of the White Lias to the *Avicula contorta* series and the zone of *Ammonites planorbis*, the author enunciated the following conclusions: (1) That the true position of the White Lias is immediately above the *Avicula contorta* zone of Dr. Wright, and at the base of the Lower Lias shales; (2) that it is entirely distinct from the Rhætic beds, lithologically and paleontologically; and (3) from the discovery of Rhætic fossils in the Gray Marls below the Bone-bed, that the latter belong to the Rhætic formation. He then proceeded to describe a two-fanged mammalian tooth which he had found in the Gray Marlstones below the Bone-bed, and which he considered to be the analogue of the trenchant four-ridged

premolar of *Hypsiprymnus* of the section to which *H. Hunteri* belongs. Until additional remains be found, its affinities to *Microlestes* or *Plagiaulax* can not be determined; Mr. Dawkins has, therefore, named it provisionally *Hypsiprymnopsis Rhæticus.* In conclusion, he traced the range of the Marsupials in space and time, showing that, of the six families into which Van der Hoeven divides the existing Marsupials, two—the entomophagous and sarcophagous *Dasyrurina*, and the phytophagous *Macropoda*—had been represented in England during the interval between the deposition of the Purbeck beds and that of the Rhætic Marlstones below the Bone-bed.—*Proc. Roy. Soc.*, in *The Reader*, June 18.

19. *Mesozoic Mammals.*—The announcement that Mr. Boyd Dawkins of the Geological Survey had discovered a tooth of a small mammal allied to the kangaroo in the Rhætic beds of Watchet has attracted during the past fortnight much attention among paleontologists. At the Geological Society, where the description of the specimen was read, the greatest possible interest was excited; and, although there may be a doubt as to the precise family of Marsupials to which the new *Hypsiprymnopsis Rhæticus* Dawkins, can be referred—whether, in point of fact, it may not be nearer allied to the *Microlestes* of Plieninger (best known to Englishmen through the researches of Mr. Charles Moore)—there can be no doubt whatever of the interest to be attached to the discovery now made. A retrospect of our present knowledge of Secondary Mammalia may now be interesting. The (Triassic) bone-beds of Würtemberg, and at Aust Cliff in Gloucestershire, afford us evidence of the diminutive *Microlestes;* in the N. Carolina coal-field we have Dr. Emmons's *Dromatherium sylvestre;* at Biegerloch was found the *Tristichodon* of Dr. Falconer, one of the most remarkable and aberrant forms, dissimilar in its characters of dentition to any known mammal, extinct or existing, unless possibly the slight analogy which the ternate division of its tooth-cusps bears to *Stereognathus* can be taken into account; *Stereognathus ooliticus* itself appears in the oolites; where also are to be found the *Amphitherium Prevostii*, the *Amphilestes Broderipis*, and the *Phascolotherium Bucklandi*, allied to the *Myrmecobii* of the existing Antipodes. Higher up in the Purbeck series the new Mammalia are so numerous that it would be premature to discuss the whole nature of the evidences which were discovered by Messrs. Brodie and Beckles, and described by Dr. Falconer and Prof. Owen. Such forms as *Plagiaulax*, *Spalacotherium*, or *Triconodon*, are of the highest interest to paleontologists. Whether we interpret the *Plagiaulax* by the analogy of *Hylacoleo*, and consider it to have been carnivorous, or by the affinities which it undoubtedly bears to the existing *Hypsiprymnus*, and classify it as a vegetable feeder, or whether we compare it, for example, with such forms as the pigmy *Dactylopsila*, discovered by Mr. Wallace in the Aru islands (*Proc. Zool. Society*, 1858, p. 109), there exists much scope for discussion; and we can only rejoice that it is especially to Englishmen that the discovery of all these remarkable forms in Mesozoic strata is due.—*The Reader*, June 25.

20. *On the transitions between the subdivisions of the Lias and Oolite in England;* by Prof. A. C. Ramsay, F.R.S., etc.—Mr. Ramsay, in his address delivered at the recent anniversary meeting of the Geological Society of London, after discussing the characters of the subdivisions of the Lias and Oolite, states the following general conclusions:

"The greater inferences that may be drawn from this general survey of the phenomena are:—

1. That there are 13 species common to the uppermost part of the Upper Lias and the Oolite. The break is by no means complete.

2. That progressively, from the lowest to the highest Oolitic formations, large percentages of species pass upward without any approach to a total break either in the whole or in individual groups, excepting in the instance of the Cephalopoda of the Inferior and the Great Oolite.

3. That species often disappear from an intermediate formation to reappear in a higher one, and the principle of migration and return is thus established.

4. That, notwithstanding migration and passage of species, it might perhaps be safely inferred that between the lowest and the highest Oolitic formation many forms had disappeared altogether, so greatly are their numbers diminished in the higher strata.

5. It seems not unlikely that, notwithstanding the large community of species, the succession of the Oolitic formations is not unbroken by minor gaps unrepresented by strata, of the kind explained in my last address. We are aided in this conclusion by a consideration of the physical conditions under which the Oolitic rocks present themselves. Thus the Inferior Oolite attains its maximum development near Cheltenham, where it can be subdivided at least into three parts. Passing north, the two lower divisions, each more or less characterized by its own fossils, disappear, and the Ragstone northeast of Cheltenham lies directly upon the Lias, apparently as conformably as if it formed its true and immediate successor, while at Dundry the equivalents of the upper freestones and ragstones (the lower beds being absent) lie directly on the exceedingly thin representative of the Upper Lias. In Dorsetshire, on the coast, the series is again perfect, though thin. Near Chipping Norton, in Oxfordshire, the Inferior Oolite disappears altogether, and the Great Oolite, having first overlapped the Fuller's Earth, passes across the Inferior Oolite, and in its turn seems to lie on the Upper Lias with a regularity as perfect as if no formation anywhere in the neighborhood came between them. In Yorkshire, the changed type of the Inferior Oolite, the prevalence of sands, land-plants, and beds of coal, occur in such a manner as to leave no doubt of the presence of terrestrial surfaces on which the plants grew, and all these phenomena lead to the conclusion that various considerable oscillations of level took place in the British area during the deposition of the strata both of the Inferior Oolite and of the formations that immediately succeed it.

Again, near Kempston, in Bedfordshire, the Cornbrash and Kelloway rock are both absent, and the Oxford clay was pointed out to me by Mr. Howell, resting directly and apparently quite comformably on the great Oolite. The fragmentary character of the Portland rocks is confessed by all.

It is probable that the oscillations of level that these phenomeua indicate may be intimately connected with the loss of old, and the appearance of new, species in our area; for it is certain that apparent conformity, as in the case of the Great Oolite lying on the Upper Lias, is often deceptive, and is in itself no positive proof of direct sequence; and it is not unlikely that during gaps in the regular sequence of formations, of some of which we may have no traces remaining, many old forms died out or changed, and new ones, at intervals, migrated hither. At the same time, it is clear that the breaks in succession in these Mesozoic strata are very different in magnitude from those of the Paleozoic age that were accompanied by total unconformity."[1]

Mr. Ramsay, in his address, next considers the Purbeck and Wealden strata, his remarks on which he commences as follows:

"We now come to a period in the geological history of the British marine Mesozoic rocks in which, though not accompanied by much apparent physical disturbance, the break in the succession of *species* is as great as in any part of the Paleozoic series. *I allude to the total change of species that marks the introduction of the marine Cretaceous formations.*

That this break in paleontological succession was accompanied by an enormous lapse of time, is proved by the presence of the Purbeck and Wealden strata lying between the Oolitic and Cretaceous series; and, but for them, when the two series come in contact, so conformable, apparently, are the Cretaceous to the Oolitic rocks, that, unlike some of the cases cited with regard to the Paleozoic formations, we have, by disturbance and denudation of the Oolite strata previous to the Cretaceous period, no very obvious hint of the enormous lapse of time that lay between the two great marine periods.

21. *On the Permian Rocks of the Northwest of England, and their extension into Scotland;* by Sir R. I. Murchison, K.C.B., etc., and Professor R. Harkness, F.R.S., F.G.S.—In this paper the authors propounded a new view of the composition of the Permian Group in the northwest of England, and, by the consequent re-arrangement of the rocks involved in this change in classification, they were enabled to place the Permian strata of Great Britain in direct correlation with those of the continent of Europe. This new feature in British classification is the assignment of a large amount of red sandstone in the northwestern counties to the Permian period, and its removal from the New Red Sandstone, or Trias-formation, to which they have hitherto been assigned in all geological maps. The authors showed that these red sandstones are closely and conformably united with the Magnesian Limestone, or its equivalent, and form the natural upper limit of the Paleozoic deposits. They affirmed that thus a tripartite arrangement of the Permian rocks holds good in Westmoreland, Cumberland, and Lancashire, and that the three subdivisions are correlative with those formerly shown by Sir R. I. Murchison to exist in the Permian deposits of Germany and Russia, thus proving the inapplicability of the term Dyas to this group of rocks.

The difference in lithological details of the Permian rocks of the northwest of England from those on the opposite side of the Pennine chain, was next adverted to; and it was observed that, with so vast a dissimilarity in their lithological development in England, we need not be surprised at finding still greater diversities in these protean deposits when followed into Germany and Russia.

[1] They are, however, somewhat comparable to the relations of the Lower and Upper Llandovery rocks to each other, or to the variations in the subdivisions of the magnesian limestone, which were formed during minor oscillations of level.

The discovery, by Professor Harkness, in the central member of this siliceous group in Westmoreland, of numerous fossil plants identical with the species of the Kupferschiefer in Germany, and in the Marl-slate of the Magnesian Limestone of Durham, was given as a strong proof of the correctness of the authors' conclusions.

The comparative scarcity of igneous rocks, and the evidence of powerful chemical action, in the Permian strata of Britain, is contrasted with their abundance in deposits of that age in Germany; but proofs are nevertheless brought forward to show that the hematite of Cumberland and Lancashire was formed in the early accumulation of the Permian deposits.

In describing in detail the different members of the Permian group of the northwest of England, the authors define the downward and upward limit of the strata which have undergone dolomitization; for whilst certain bands of calcareous breccia (the "brockrum" of the natives), which occur in the central portion of the series, contain much magnesia, the lower breccias, composed of the same mountain-limestone fragments, have no trace of it; nor is it to be detected in the upper member, or St. Bee's Sandstone.—*Phil. Mag.*, [4], xxvii, 542.

22. *On the Reptiliferous Rocks and Footprint Strata of the Northeast of Scotland;* by Professor HARKNESS, F.R.SS. L. & E.—The author showed that the foot-print sandstones of Rossshire constitute the upper portion of the *Old Red Sandstone* formation, and that the strata embraced in a line of section from the Nigg to Cambus Shandwick, from above the gneiss to the foot-print sandstones of Tarbet-ness inclusive, are conformable throughout, and are referable to each of the three divisions of the Old Red Sandstone—namely, the conglomerates and yellow sandstones (of a thickness of 1500 feet) belonging to the Lower Old Red Sandstone; the gray flaggy sandstones and shales of Geanies—the equivalent of the Caithness flags—containing *Osteolepis*, *Coccosteus*, and *Acanthodes*, and thus referable to the Middle Old Red; thirdly, conformable strata, consisting of conglomerates, and foot-bearing and other sandstones, appertaining to the higher members of the system. The foot-bearing sandstones have a thickness of 400 feet, and represent the reptiliferous sandstones of the Elgin area, though not overlaid by Cornstones, as in that district. The author, in conclusion, remarked that, though *Stagonolepis* is decidedly *Teleosaurian* in its affinities, it does not consequently mark a Mesozoic group of rocks; for *Mastodontosauriæ*, which abound in the Trias, occur in the Coal-measures; and stratigraphical evidence shows us that Teleosaurian crocodiles have a wider geological range, since they are met with in the Old Red Sandstone.—*Proc. Geol. Soc.* in *The Reader*, *July* 9.

23. *Coal in Venezuela.*—Dr. SEEMAN, who has been several months in Venezuela for the purpose of inspecting an estate of 100 square leagues on the banks of the river Tocuyo, has returned to England by the last West India steamer. Whilst exploring the valley of the Tocuyo he has discovered what may prove of the utmost importance to the railways and steamers now establishing in that part of the world—extensive coal-beds, the coal being valued in London at thirty shillings per ton, and resembling the best Welsh steam coal. This part of South America is as

yet little known, but abounds in natural wealth; in it are situated some of the richest copper mines of the world—those of Aroa, to which an English company is now making a railroad, sixty miles in length, ten of which have already been finished. The soil is of extreme fertility, and mahogany and other precious woods abound.—*Reader, No.* 74, *May* 28, 1864.

24. *On the Geology of Arisaig, Nova Scotia;* by the Rev. D. Honeyman, F.G.S.—A careful examination of the country in the neighborhood of Arisaig enabled the author to construct three sections and a map showing the geological constitution of the district. Two of these sections were nearly parallel to one another, running from N. to S., and taken some distance apart, while the third was nearly at right angles to the other two; thus a tolerably accurate idea of the geology of the country could be obtained. The author described each of these sections in detail, giving lists of the fossils found in the different beds, which proved them to be of Upper Silurian age; and he further considered that they justified the adoption for the subdivisions of these Nova-Scotian Silurians of the terms May-hill, Lower Ludlow, Aymestry, and Tilestones, the first and third of which had been used for them previously by Mr. Salter. Beside Silurian rocks, there occurs in the western part of this district a conglomerate of Lower Carboniferous age, while trap-rocks occur on the north and south.—*L. E. and D. Phil. Mag.*, [4], xxviii, 74.

## III. BOTANY AND ZOOLOGY.

1. *New Scirpi of the Northern United States.*—Among the species which, by continued research, are one by one added to the Flora of the Northern United States, are a few *Scirpeæ*, to which we may here call attention.

The most conspicuous addition is that of a tall *Scirpus* of the *triqueter* section, which was last year discovered by Mr. A. Commons and Mr. Wm. M. Canby, on the eastern shore of Maryland, and which has been named *S. Canbyi*. It is as tall a species as *S. Olneyi*, but has its radical leaf remarkably developed, as also the involucral one, which apparently continues the stem; and the spikes, which are half an inch long, are all on long and slender rays, which come off in pairs from the nodes of a zigzag rhachis, from the axils of bracts or involucels. The character which was published in January last, in the Proceedings of the Academy of Natural Sciences, Philadelphia (in a note appended to a short article by Mr. Canby on the plants of the lower part of Delaware and the Eastern Shore of Maryland), is here reproduced.

Scirpus Canbyi (Gray, supra cit.): culmo elato (3–5-pedali) folio prælongo canaliculato-triquetro stipato inferne obtuse trigono superne triquetro apice in involucrum monophyllum pseudo-umbellam plurifloram longe superans desinente; umbella sessili dichotomo-composita; umbellulis sæpissime biradiatis involucellatis, radiis omnibus elongatis plerisque monostachyis; spicis oblongis; squamis laxe imbricatis oblongo-ovatis acutiusculis dorso viridulis nervosis marginibus late scariosis pallidis; setis perigynii 6 patentim barbellatis achenium obovato-triquetrum subito rostellatum paullo superantibus.

Two or three years ago we identified *Scirpus pauciflorus* in the Northern States, on occasion of its being sent from the northern part of Illinois by Dr. George Vasey. We then ascertained that it had long before been gathered on the northern borders of Michigan and of the State of New York (vide Manual Bot. Addend., 1863, p. xcviii), but had been confounded with the very distinct *S. planifolius.* We have this summer received from another part of our northern frontier, viz: from the vicinity of Buffalo, New York, still another *Scirpus* of this group; one which might more naturally be confounded with *S. planifolius*, but which may be neatly, and we trust definitively, distinguished by the following diagnosis:

SCIRPUS CLINTONII (sp. nov.): folio e vagina suprema involuto-filiformi culmo multùm breviori, cæteris brevissimis vel subnullis; squamis capituli (præter infimam) carina vix prominula haud percurrente muticis; setis perigynii achenium subsuperantibus: rel. ut in *S. planifolio.*—On the plains between Buffalo and Williamsville, New York, Hon. George W. Clinton, June, 1864.—The hypogynous bristles are perhaps rather longer, and more strongly barbellate with spreading hairs than in *S. planifolius;* and the achenium may be rather smoother; otherwise no difference is noticeable. We should not rely much upon the short, narrow, and involute-filiform or setaceous leaves, if they were not accompanied by pointless scales of the spike; the midrib or keel, instead of projecting into a cusp as in *S. planifolius*, vanishing quite below the blunt and scarious apex of the scale. In naming this species after its discoverer, we welcome back, with this gift in his hands, a deserter of thirty years from the botanical ranks, into which he enlisted in youth, and from which he was led away by the demands of an exacting profession. And we confidently wish him a success worthy of his name and lineage in his laudable endeavors to promote a knowledge of the botany, and to complete the public herbarium, of his native State.[1]

SCIRPUS CÆSPITOSUS, L., a northern subalpine species, which Dr. Pitcher, however, had collected at the outlet of Lake Superior, and we had ourselves met with on the higher mountains of North Carolina, has this year been detected at Ringwood, in the northern part of the State of Illinois, far distant from any mountains.

ELEOCHARIS SIMPLEX, Torr., has to be added to the Flora of the Northern States, having been detected by Mr. Wm. M. Canby, in 1863, on the Eastern Shore of Maryland.

ELEOCHARIS ROSTELLATA, Torr., has been this year detected in the New Durham swamp, New Jersey, by Dr. T. F. Allen. A. G.

2. DE CANDOLLE, *Prodromus Systematis Naturalis Regni Vegetabilis, etc. Pars decima quinta. Sectio prior.* May, 1864.—This new part of the *Prodromus*, although ranking as only the first portion of the 15th volume, occupies 522 pages, has its proper index, and is intended to be separately bound. The other part, which will be yet larger, is devoted to the *Euphorbiaceæ;* and a fasciculus of it, containing the genus *Euphorbia*, was issued in the year 1862. The volume now before us con-

[1] We learn from Dr. Torrey that he has for some time had this *Scirpus* in his herbarium from Ogdensburgh, N. Y., also from Michigan, and that he takes it to be a variety of *S. planifolius.*

tains the *Lauraceæ*, by Meissner (a vast improvement upon Nees von Esenbeck), and the small group of *Hernandiaceæ* by the same; the *Begoniaceæ*, by Alphonse De Candolle himself, followed by the *Datiscaceæ* and the *Papayaceæ*, by the same; the *Aristolochiaceæ*, by Duchartre; and finally the *Stackhousiaceæ* by Bentham, the latter reduced to a single genus, of eleven species. The volume is an important one, especially for tropical botany. A. G.

3. *Bryology of British N. W. America.*—The most interesting article to us in the last (29th) no. of the *Journal of the Proceedings of the Linnæan Society*, issued in June last, is an elaborate one by Mr. Mitten, entitled, *The Bryologia of the Survey of the 49th Parallel of Latitude.* It is founded on the specimens gathered by Dr. Lyall, who, as our readers know, was the assiduous botanist of the British N. W. Boundary Commission. East of the Rocky Mountains and upon them he was gleaning after Drummond, who left nothing for his successor to discover. But on the western side, and on Vancouver's Island, he found much that was novel and interesting. While publishing the new species now brought to light, Mr. Mitten also revises the collections of Drummond, both the Northern and the Southern Mosses, and characterizes a goodly number of species which had remained obscure and undistributed, or had been confused with others. Again, others are described from Bourgeau's, Coulter's, Fendler's, and Wright's collections. A good set of the duplicates of Lyall's and Bourgeau's muscological collections, sent to this country, has been placed in the hands of Mr. Sullivant. A. G.

4. *Icones Muscovum;* by WM. S. SULLIVANT.—We are able to announce that this, the most exquisite of all illustrated bryological works, has been printed, and is about to be issued. It forms an imperial 8vo volume, with 129 copper-plates of unrivalled execution. A. G.

5. *On the Currant Worm of Ann Arbor, Michigan;* by Prof. A. WINCHELL. (Condensed from an article in the Detroit Free Press of July 9, 1864.)—This "currant worm," is the larve of a Hymenopter of the genus Selandria, and is named *Selandria Ribis* by Prof. Winchell. It was observed by him last summer. This summer it has been still more abundant, and has, in places, completely denuded the red currant bush of its foliage, doing it considerable injury, though for the present year the crop of fruit does not seem materially deteriorated.

The worm was first seen, May 23. Individuals were then about one-fourth of an inch in length, and had just begun to depredate upon that part of the foliage nearest the ground. They devoured rapidly the whole tissue of the leaf, leaving only the thicker part of the nerves, and moved from leaf to leaf, gradually extending their ravages toward the summits of the stems.

The full grown larve is three-fourths of an inch in length, of a pale-green color, with black head, tail and feet, and numerous black spots regularly arranged around the body, from the summit of each of which proceeds one, two or more short, stiff hairs. Number of segments of the body 14 (including head and tail), of which the 2d and 12th, are yellowish green. The 2d, 3d and 4th segments are furnished each with a pair of feet; the 5th is short and without feet; the 6th, 7th, 8th, 9th, 10th and 11th with short, extensile prolegs—that is, fleshy protuberances, to be used as legs; 12th and 13th, segments without legs.

The larves all disappeared about the 3d of June, undergoing first a moulting, and then digging their way into the ground.

On the 16th of June, a swarm of four-winged flies (Hymenopters) were seen under the currant bushes. They were generally inactive or sedentary, but at intervals became extremely excited, and I observed that these spells of activity were occasioned by the presence of a female insect about which there were hundreds of males. It appeared that the females were very few in number, and each moved as a queen among her subjects.

The males are one-fourth of an inch in length, and the females three-eighths, and much thicker. The color of the latter is uniformly ochre-yellow except the head and wings, while the male has, in addition, considerable black upon the back.

The queens proceeded immediately to deposit their cylindrical, whitish transparent eggs, in regular rows along the underside of the nerves of the leaves, attaching them at the rate of about one in forty-five seconds.

The ova appeared, under the microscope, at first to be filled with a granular fluid, which, in a few hours, became divided into distinct areas. On the second day the outline of the embryo was traced, and it was seen bent together with protuberances for the feet distinctly forming. On the third day nearly all parts of the external and internal structure were visible—the pulsating dorsal vessel, the intestine, the tracheary or breathing system, the mandibles, the feet, etc.; and the animal moved in its nidus. On the fourth day the embryo escaped from the egg and began to eat. This is the most rapid embryonic development I have ever witnessed.

When the larve first escapes, it is whitish, and one-tenth of an inch in length. It becomes one-third larger in twenty-four hours, and attains full growth by the 25th of June. The brood then begins to moult and descend into the soil as before. In this situation it probably remains until the succeeding spring.

The following is then a summary of its history:

May 17th, first brood of flies appear from larves or worms which went into the soil the previous summer. May 21st, first brood of larves, becoming noticeable about May 23d. June 3d, moulting and burrowing in progress. June 16th, appearance of second brood of flies. June 20th, second brood of larves escaping from egg. June 25th, flies disappear. June 28th, moulting and burrowing of second brood.

Incubation of ovum three or four days; time to moulting and burrowing eight days; time in burrow, first brood, thirteen days; life-time of fly nine days.

The usual torpidity of the fly prevents the rapid spread of this pest, but measures ought to be taken to resist its encroachments. I find that the full grown larve is not injured by ashes or lime, though these applications are fatal to the young one. Whale oil, soap suds, or infusion of tobacco stems will destroy them. The simplest method, however, is to jar the bushes over pans or cloths. The larves are very easily detached. There is no doubt that the strong odors of coal tar and petroleum would drive away the flies; but it would not cause their death.

6. *Casts of various parts of the structure of the Gorilla.*—Dr. Auzoux announces that having recently received a specimen of the gorilla through Admiral Didelot, he has, by his "processes of plastic anatomy," made

casts of great perfection of the following parts in the structure of the animal:—

The complete osteology—the skeleton being 1 m. 1 c. in length, and so like the original bones in appearance that it might easily deceive.—2. The myology, and all the viscera enclosed in the three larger splanchnic cavities. All these parts are so arranged that they may be studied in their natural positions, or taken up one after another for minute examination. The various peculiarities of the bones, the muscular insertions, the arrangement of the great aerial sacs which communicate with the larynx, the little development of the brain as compared with man, are among the points illustrated which are of special interest.

Dr. Auzoux proposes soon to add to the osteological, myological and splanchnological specimens, others illustrating the vessels and nerves.—*Les Mondes*, May 26.

7. *Animalcules in the blood.*—Dr. HARLEY of London, in a recent memoir on the disease called *Hæmaturia*, occurring at the Cape of Good Hope and the Island of Mauritius, shows that the disease is owing to the presence of a species of *Distoma* in the blood. Dr. Harley described it as a new species, after a study of the eggs and the embryos of the worm, and called it *Distoma Capense;* but Dr. Cobbold, who is especially versed in the science of intestinal worms, states that it is the *Distoma hæmatobium*, and that the disease is the same that is well known in Egypt. The same worm was discovered by Dr. Cobbold in the body of an African monkey (*Cercopithecus fuliginosus*), and as the species had previously been recognized by Dr. Bilharz of Cairo, the generic name, *Bilharzia*, has been applied to it. This parasite is introduced into the body of man and monkeys from the unfiltered waters of the African rivers. The larves live in some of the mollusks of the rivers.

8. *Cephalic vertebræ.*—Mr. LAVOCAT, of the Academy of Sciences of Toulouse, has presented the following observations on a monstrosity as confirming his view that the head corresponds to four vertebræ, one to each of the senses. A calf, at half-development, had the left half of the head quite regular, while the right was deprived of the nose and eye. On this same right side there were suppressed the osseous pieces constituting the nasal segment (the *vomer*, and *ethmoid*) and those which constitute the arch protecting the eye (*frontal*, *anterior*, *sphenoid*, and *pterygoid*). On the left side all the parts were present, as well as the corresponding organs of sense. This observation, he says, demonstrates as completely as possible a natural concordance of development between each organ of sense and the cephalic segment which protects it. The parts degraded, in the several cases of anomaly, are exactly those which are attributed by Mr. Lavocat to the several cephalic segments considered as vertebræ.—*Les Mondes*, May 26.

9. *Marine Crustaceans in freshwater lakes of Norway.*—G. O. SARS (son of Prof. Sars) has detected in one of the freshwater lakes of Norway, a red Copepod, the salt-water species *Harpacticus chelifer* of Lilljeborg, and in another, the *Mysis relicta* of Loven, also marine. It is supposed that they must have been introduced when the region was submerged beneath the sea, in the Post-tertiary period, and that they have survived the change to freshwater. A species of *Gammarus* (probably the *G. cancelloides* Gersfeldt) occurs with the latter species, which is also

found in Lakes Baikal and Angara; and this also is regarded by Loven as originally marine.—*Ann. and Mag. Nat. Hist.*, [3], xiii, 437.

10. *Hymenoptera.*—The following papers on American Hymenoptera by E. T. Cresson have been published in the course of the two years past in the Proceedings of the Entomological Society of Philadelphia. Proceedings for June 1863, *On a new species of Massaris;* for July 1863, *List of the N. American species of Bombus and Apathus;* for Nov. 1863, *On the N. American species of the genus Nomada;* for Feb. 1864, *On the N. American species of several genera of Apidæ;* for April 1864, *Descriptions of North American Hymenoptera—Apidæ.*

11. *Didunculus.*—At the meeting of the Academy of Natural Sciences of Philadelphia, of March 15th, Mr. Cassin called attention to the collection of birds presented by the Smithsonian Institution, and particularly referred to several species of great rarity and scientific value. The *Didunculus strigirostris* is one of two species of birds now known to be approaching extinction, the other species being *Alca impennis*, which is also in the Academy Museum. This bird is most nearly allied to the extinct Dodo, formerly of the Isle of France, and inhabits the Samoan or Navigator Islands. Its extinction or approach to it is said to be owing to the introduction into those islands of the domestic cat. Not more than four or five specimens are known to be extant.

12. *Note on the Muscovy Duck;* by Mr. Hill. (Proc. Acad. Nat. Sci. Philad., 1864, p. 72.)—The habitat of the Muscovy Duck is the Lake of Nicaragua. There travellers see them at all times, either in small breeding coteries, or large flocks. In the wild state their plumage is dark without any admixture of white. They were originally procured from the Mosquito shore, the country of the Muysca Indians, (see Humboldt's researches,) and hence is derived the name of Musco duck, corrupted into Muscovy duck. The West Indian Islanders had early naturalized them, for, on the discovery of Columbus, they speak of "ducks as large as geese," that they found among the Indians.

13. *The Elements of Comparative Anatomy*, by Thomas Henry Huxley, F.R.S., Prof. Nat. Hist. Royal School of Mines, and Prof. of Comp. Anat. and Phys. to the Roy. College of Surgeons of England. 304 pp., 8vo, with numerous cuts. London, 1864. John Churchill & Sons.—The two subjects of this volume are, *first*, the Classification of Animals, and *second*, the Vertebrate Skull. On both topics the author writes learnedly, but shows a better appreciation of details and their outer relations than of the profound system of Nature. Instead of four subkingdoms of animals, the author makes eight "primary categories or groups"—viz., the *Vertebrata*, *Mollusca*, *Molluscoida*, *Cœlenterata*, *Annulosa*, *Annuloida*, *Infusoria*, and *Protozoa.*—The subkingdoms of Mollusks and Articulates are subdivided, the former into Mollusca and Molluscoida, and the latter into Annulosa and Annuloida; yet with an expression of the possibility that "characters may be discovered which shall unite these pairs respectively;" and the "Radiate-mob," as he, with more feeling than philosophy, styles the subdivision of Radiates, (for even Cuvier, although he failed to see the precise limits of the division, comprehended the true idea at the basis of it,) is cut up—part (the Acalephs and Polyps) being retained in one group called the *Cœlenterata*, and the others (Echinodermata) being united with the Scolecida to make

the *Annuloida.* Such a system is arrived at by making the characters of the nervous system, digestive system, and others of the means by which the grand plans of structure are adapted to forms of differing grades, or of different conditions of existence, superior in importance to the fundamental plans of structure themselves.

The preface states that it is the intention of the author to make this work the first of a series—to be followed by a second "On Man and the other Primates;" a third on the remaining Mammalia, and others,—so as eventually to bring out "a comprehensive, though condensed, systematic work on Comparative Anatomy."

14. *Notice of the Megatherium Cuvieri, the giant fossil Ground-Sloth of South America, presented to the University of Rochester by Hiram Sibley, Esq.* 34 pp., 4to.—The Geological Cabinet of the Rochester University lately received, through the liberality of Mr. H. Sibley of that city, a copy in plaster of the Megatherium Cuvieri. The volume here announced, prepared by Prof. Henry A. Ward of the University, contains a brief account of the discovery, anatomical characters, and probable habits of the Megatherium, derived from the memoir of Prof. Owen and other publications on the subject, together with a few cuts, one of which exhibits the specimen as it stands in the Museum. There are also short notices of some related animals.

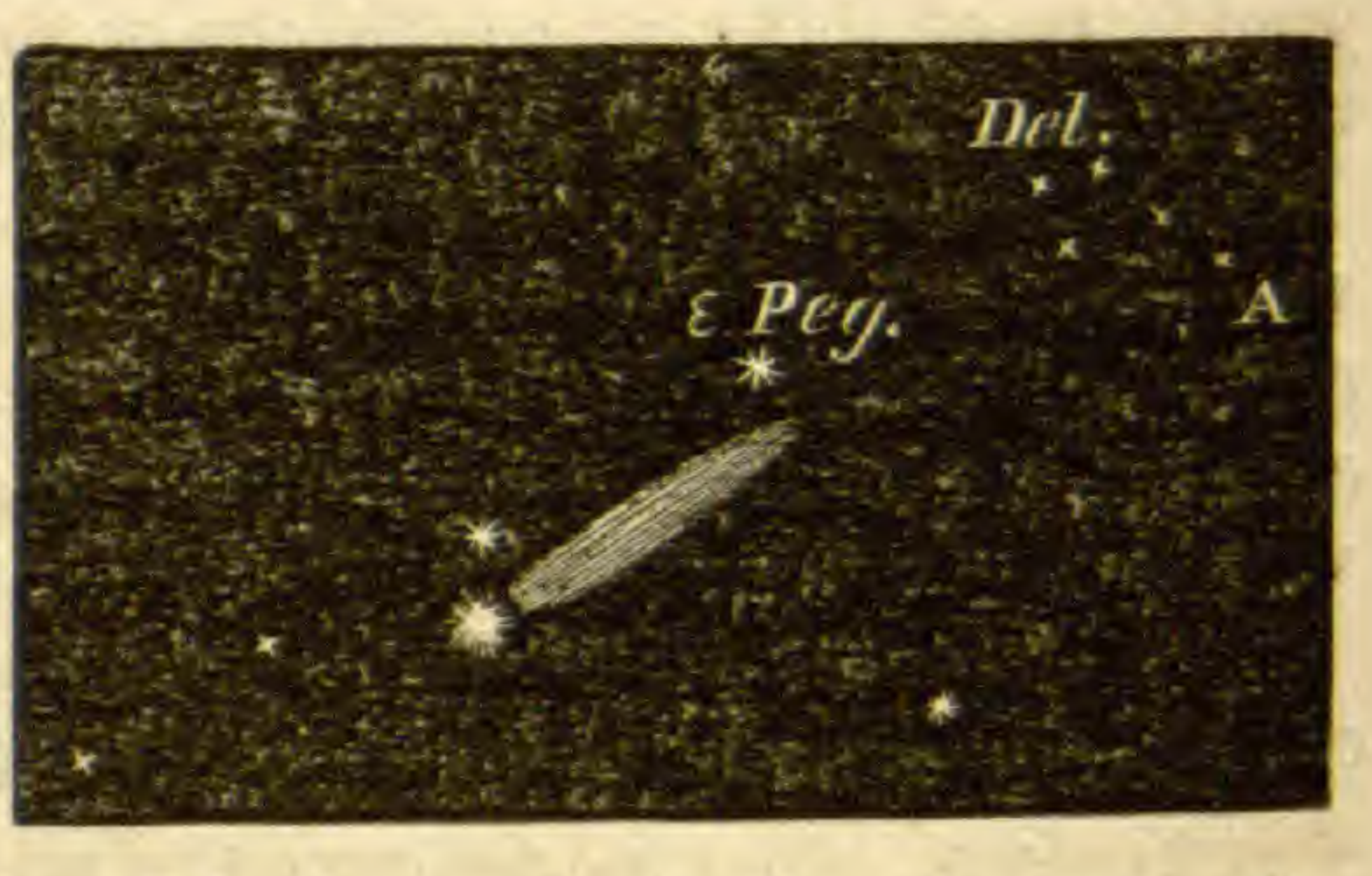
15. *Recherches sur la Faune Littorale de Belgique*, par P.-J. Van Beneden, Prof. à l'Univ. Cath. de Louvain.—Crustacés.—Memoir présenté à l'Académie royale de Belgique le 6 Mai 1860. 180 pp., 4to. Bruxelles, 1861.—This extended memoir is a continuation of the former papers of Van Beneden on the fauna of the Belgian coast. It treats descriptively, and in part embryologically, of many of the species there found.

## IV. ASTRONOMY.

1. *Note on a Meteor;* by John Gardner. (In a letter to the Editors, dated Boston, Mass., July 26th, 1864.)—On the evening of the 13th, at about quarter past ten o'clock, I had the pleasure of seeing a large and most beautiful meteor; and yesterday I noticed a short paragraph in the *Post*, stating that on the evening in question a meteor as large as Jupiter had been seen in Hartford, Conn. It will be interesting to learn if possible whether the latter was the same body which was seen here.

I was walking along the *west* side of Washington street, and toward the city from Roxbury, when my attention was suddenly arrested by the appearance of a very large ball of fire, extremely brilliant, similar to the appearance of *Vega* in a telescope with a low power. The houses on the opposite side were high, but the street at this part is wide. The sky was clear in the direction I was looking, and, though the moon was shining, the larger stars shone out definitely enough to enable me to trace their respective positions to each other. When the meteor first caught my eye it seemed a little below *Delphinus*, and pursued its path in a northeasterly direction toward *Pegasus*. At first it was merely a ball without any tail, but suddenly it appeared to start along with fresh vigor; a tail of at least 3° or 4° was produced with a *rushing noise*, and then the body itself exploded with a *loud report* (which, in spite of all the noise and bustle of the street, cars, &c., was most plainly distinguishable) and then died out.

In the annexed diagram it seemed to appear first at point A. The tail, which remained two or three seconds in view, had the appearance of myriads of particles of luminous matter condensed into a cigar-shaped form, hissing as it went along like a rocket; but unlike a rocket it did not make its appearance until almost immediately previous to the explosion.




Now as Hartford is 100 miles southwest of Boston, and as the view I had was in a direction opposite to that of Hartford from Boston, and taking into account the angle of elevation as shown by the height of the building near me, I think this body must have been a very large one and at a very great distance.

2. *List of Radiant Points of Shooting Stars;* by Professor Heis.—This paper consists of a list of all the radiant points of shooting stars observed and recorded during eleven years before 1860, at other times of the year than in August, November, and December. They are arranged as general radiant points of shooting stars, or as general centres of emanation of shooting stars, in successive half-months of the year from January to December, omitting only the latter half of July, the first half of August, the latter half of October, the first half of November, and the first half of December, concerning which Professor Heis has published his observations in detail. The present radiants have not yet been published, but will be published by Professor Heis in the ensuing winter. It is curious that *δ Virginis* occurs in this list as a general centre of emanation of shooting stars in the latter half of April, traceable also in May! This perfectly incidental coincidence is, Mr. Herschel considers, a fair sequel to his discovery of a star-shower existing on the 10th of April; because, after a well-marked star-shower, a number of shooting stars may always be seen to come for a considerable time from the same point of the heavens, and such shooting stars are sure to be picked out from other sporadic shooting stars by their common intersection. Mr. Herschel also thinks that there will be found, at last, to be no such meteors as sporadic shooting stars, but that all belong to some special star-showers, whose effects remain sensible for some time, and whose dates and radiant points have not yet been thoroughly examined.—*Proc. Roy. Astronom. Soc.*, in *The Reader*, July 9.

3. *New Comet.*—Mr. Tempel discovered a new comet on the 5th of July, having the appearance of a diffused nebulosity of some 3′ or 4′ in diameter. This comet was seen by Mr. Respighi at Bologna, on the 6th, and by Prof. F. Karlinski of the Cracow Observatory, on the 11th, near *γ Arietis.*

Mr. Valz gives the following approximate elements. Passage of perihelion Sept. 7·05, mean time at Marseilles; dist. of perihelion 1·823; long. of perihelion 289°·37; R.A., 66°·56; inclination 1°·45; movement retrograde.

## V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Further Human remains from the Quarry of Moulin-Quignon, near Abbeville, France.*—The long discussion which has taken place over the human jaw-bone asserted to have been found at the quarry of Moulin-Quignon has given great interest to explorations at that place. At the meeting of the Academy of Sciences of July 18th, Mr. de Quatrefages presented the results of new discoveries of human remains in communications from Mr. Boucher de Perthes. From these communications it appears that on the 24th of April last, Boucher de Perthes, along with Dr. Dubois, physician of the Hotel-Dieu at Abbeville, found, in a yellowish-brown bed to the right of the quarry, a portion of a human sacrum, fragments of other bones, some of which were parts of a cranium, and a human molar tooth. On the 1st of May, they obtained, on further digging, three small fragments of a cranium and a part of a tooth. On the 12th of May, Mr. Boucher de Perthes was joined by Mr. H. Duval. They procured from the brownish-yellow bed, at a depth of six to seven feet, portions of a cranium.

On the 11th of May, besides fragments of bones, a *human jaw-bone* was turned out, which was perfect excepting the extremity of the right ramus and the teeth. The depth from which it was obtained was about fourteen feet. Boucher de Perthes, being occupied with investigations elsewhere at the time, was not himself present; but a person delegated by him superintended the digging. Fragments of bones and some cut flints also were found.

On the 7th of June, the Abbé Martin, Curate of St. Gilles, Professor of Geology in the Seminary of St. Riquier, continued the diggings, during the temporary absence of B. de Perthes, and took out from the bed, at a place where it showed plainly by its regular stratification that it had not been disturbed since its first deposition, a *human cranium*, the frontal bone and the two parietal of which were nearly entire, and also two fragments of an upper jaw (perhaps of the same head with the cranium) and an iliac bone.

The number of specimens of bones collected amounts to 200, and they were all found within an extent of about 130 feet. Part are of animals, a catalogue of which is soon to be made out. The human remains apparently indicate a very small race of men.—*Les Mondes*, July 21.

2. *Interoceanic canal across the Isthmus between the two Americas.*—The idea of opening an interoceanic canal across the isthmus between the two Americas dates almost from the discovery of America. As early as 1528 Antonio Galvao, a Portuguese navigator of the sixteenth century, relates in his work, *Tratado dos Descubrimentos* (p. 73), that Sayavedra, in 1528, proposed to the Emperor to open communication between the two oceans, which, he says, is possible at four different points: 1, from the Gulf of Miguel to Uraba (Gulf of Darien); 2, from Panama to Nombre de Dios; 3, by Lake Nicaragua and the river San Juan; 4, by the isthmus of Tehuantepec. What is very remarkable, these four routes are actually the only ones which admit of the construction of an interoceanic canal, as has been demonstrated by all recent investigations, and especially by those made since 1825.—*Cosmos*, July 7.

3. *Note supplementary to the article on the Progress of the Geological Survey of California* (pp. 256–264).—Since the article on pages 256–264 was printed, a very interesting account has been received from Prof. Brewer, of a portion of his explorations in company with Messrs. Hoffmann, King, and Gardner, (referred to on p. 258). The party had made an extended reconnaissance in the High Sierra, between the parallels of 36° and 38°, starting from Visalia, and exploring the region of the head-waters of Kaweah and King's rivers. Contrary to our previously-formed opinions, based on all the information we had been able to obtain with regard to this entirely unknown portion of the State, it proves to contain the most elevated, as well as the wildest and grandest part of the Sierra. Indeed, it seems, from Prof. Brewer's letter, that it is possible that Mt. Shasta itself will be overtopped by one or more of the gigantic peaks discovered on this exploration. July 2d the party climbed a sharp granite cone which proved to be about 13,500 feet high, of the view from which Prof. Brewer writes as follows: "The snow peaks here form a belt from thirty to forty miles wide, over the whole of which rise extremely sharp granite ridges over 11,000 feet high, and there are *hundreds* of points over 12,000 feet. This cone is not less than 13,500 feet and perhaps 13,600! To our surprise, it proves not to be on the true crest, which is fifteen or twenty miles east of us, and there were in sight from it at least ten other peaks as high, several that were higher, and one that will probably reach 14,000 feet or very near it." One of these more elevated peaks was visited and ascended by Mr. King, July 6th—as may be supposed, not without great difficulty. It was found to be over 14,000 feet high, and it was also found that there were five more peaks as high as that one, and two higher, one of which it seems possible may exceed Mt. Shasta in altitude. These grand elevations lie between the heads of King's and Kern rivers, somewhat north of the north end of Owen's lake. The highest peaks can be best visited from the Owen's lake and Visalia trail, and probably only from that direction. Mr. King started about the middle of July to attempt to reach them, an enterprise the arduous character of which will not be appreciated except by the few who have undertaken pioneer-work of this kind. In the next number of the Journal I hope to be able to record his success in an exploration so interesting and important in its character. It would seem, therefore, that we have in this district, just visited by the Geological Survey, the greatest mass of mountains, taking width and average elevation into consideration, which has yet been discovered within the limits of the United States, and perhaps on the North American continent. And it may seem strange that this fact should have remained undiscovered so long. But it must be remembered that the region is an inconceivably rough and difficult one to reach, and that we have never been able to learn that it had been visited by any scientific man, hunter, or mineral-explorer even: all we knew of it was, as I have said on p. 258 of this Journal, "that it contained some of the loftiest and most extensive mountain groups of the Sierra." To make the statement more correct, it now appears that the words "some of" should be omitted.

The High Sierra in this region also, as well as farther north, abounds in traces of ancient glaciers, on a scale of unsurpassed magnitude.

Northampton, Mass., Aug. 11th. J. D. W.

4. *A New Silkworm.*—A *fourth* species of silkworm of the oak—the *Bombyx Roylei*—has been introduced into France. The three before introduced are the *B. mylitha* Fabr., from Bengal, *B. Pernii* Guerin-Méneville, from North China, and *B. Yama-maï* G.-M., from Japan. The new species is from the plateaus of the Himalaya, on the frontiers of Cashmere. It lives on a thick-leaved oak, the *Quercus incana.* Guerin-Méneville states that the worm can be easily acclimated in central and southern France. Twenty coccoons received on the 23d of March gave 3 males on the 7th of April, and, on the 19th, a male and female. Soon after, 108 eggs were laid, enough to insure the success of the experiment of introducing the species and distributing it over the country. Instructions for the care of the *Yama-maï* of Japan, published in Guerin-Méneville's *Revue de Sericiculture comparée* (1863, p. 33), serve also for the new species.—*Cosmos*, Ap. 28.

5. *A new style of barometer; effect of atmospheric pressure on practice in Gunnery.*—The French artillerists in Mexico have recently found, to their surprise, that the angle of elevation used in France for their guns, for any given range, does not afford the calculated results; and have ascertained that this is owing to the diminished pressure of the atmosphere on the Mexican plateau. It follows that cannon may serve as a kind of barometer for measuring altitudes.—*Les Mondes*, July 7.

6. *Paper from Maize.*—In the manufacture of paper from maize, which has been started in Austria, the leaves are digested in hot water for two days, at the end of which time they are separable into three parts: (1) the coarse ribs or veins, which serve, like hemp or flax, for making cordage and cloth; (2) the finer material uniting the ribs, which is converted into a soft paste, and which is made into a kind of bread, somewhat dark in color, and of agreeable taste; (3) a coarser paste finally remains, which is perfectly white and which is used in making the paper. The first of these materials may be kept for months in the air without injury. Dried, it is a good combustible; and in the soft state, an excellent fattening material (*engrais*).—*Cosmos*, May 19.

7. *Preservation of wood.*—The following method is used in Germany for the preservation of wood. Mix 40 parts of chalk, 50 of rosin, 4 of linseed oil, melting them together in an iron pot; then add 1 part of native oxyd of copper, and afterward, with care, 1 part of sulphuric acid. The mixture is applied while hot to the wood by means of a brush. When dry it forms a varnish as hard as stone.

8. *Morid's process for recovering writing on paper or parchment which has become nearly effaced.*—The paper or parchment written on is first left for some time in contact with distilled water. It is then placed for five seconds in a solution of oxalic acid (1 of acid to 100 of water); next, after washing it, it is put in a vessel containing a solution of gallic acid (10 grains of acid to 300 of distilled water); and finally washed again and dried. The process should be carried forward with care and promptness, that any accidental discoloration of the paper may be avoided.—*Cosmos*, March 24.

9. *The ravages of Insects a cause of their destruction.*—It is well known that after worms have for 5 or 6 years committed their ravon the trees of a region, they often suddenly disappear, and have no full return again for two or three or more years to come. It has been shown

that the destruction is sometimes at least a result of their numbers. The larves or worms, when very numerous, consume the leaves of the tree on which they are before they attain full maturity, and, as a consequence, they never pass to the chrysalis state; they remain for a while as larves, often showing by their movements that they are half-starved, and then die.

10. *Means of hardening fragile or friable specimens, used by Mr. Stahl.*—Mr. Stahl gives solidity to friable specimens, even if of loose material like a mould in sand of a shell or bone, by running in a mixture of resin and spermaceti melted together.—*Les Mondes*, May 26.

11. *Elevation of Lake Geneva above the sea.*—The mean level of Lake Geneva has been determined, by levelling along the railroads here terminating, to be 372·362 meters above the mean level of the Mediterranean, and 371·562 meters above the mean level of the ocean.—*Les Mondes*, May 26, from the *Arch. Sci. Phys. de Genève*, Jan., 1864.

12. *Investigations in Egypt.*—A legacy of 20,000 francs has been bequeathed to the French Academy of Sciences by Miss A. Letellier, which, under the name of the "Savigny Foundation," is to supply young zoologists with the necessary means of continuing Savigny's investigations in Egypt and Syria.—*The Reader*, June 25.

13. *Isthmus of Suez.*—The canal, which has for some time been in construction, from Ismaïlia to Port Saïd, has been completed, and fresh water is now furnished across the line of the desert, as well as at this important port.

14. *Making of Oases.*—Mr. Martins, in an address at one of the *Soirées scientifiques* of the Sorbonne, gives a glowing account of the effect over the African desert, through French enterprise, in sinking Artesian wells. He predicts the time when immense lines of railways shall run from the Mediterranean to Senegal, and from Senegal to the Red Sea; and when Suez, with its finished canal, shall become "le foyer des relations avec la feconde Afrique, le boulevard de toutes les mers, la route de tous les continents."—*Cosmos*, April 21.

15. *Acclimation of English Birds in Australia.*—The Thrush, Blackbird, Skylark, Starling, Chaffinch, various Sparrows, and the Wild Duck, are already domesticated in Australia through the efforts of the Acclimatization Society of Victoria. Great success has also attended the Society's efforts to introduce good fresh-water fish into the rivers, and it is expected that the Salmon will soon be naturalized in Tasmania.

16. *Dedication of the Museum of the Boston Society of Natural History.*—The new building, recently erected at Boston for the Natural History Society, was dedicated on the 2nd of June last. The building is an elegant and imposing structure of granite, brick and freestone, of the classic style of architecture with Corinthian pilastres, nearly square in outline. At the centre of the east front there is a grand doorway of granite, supported by massive buttresses, on which are hereafter to be placed life-sized figures of an elephant and a rhinoceros cut in stone. It measures 95 feet by 105, and is well arranged within for scientific purposes. It has been erected at a cost of nearly $100,000. Addresses were made by Prof. Wyman, Prof. Wm. B. Rogers, Mayor Lincoln, Lieut. Lutke of the Russian Navy, and Rev. Dr. Waterston.

17. *Universal Exhibition at New Zealand.*—A New Zealand exhibition will be opened at Dunedin, in the province of Otago, on the first

Monday of January, 1865. The building is to be of brick. Its regulations will be similar to those of the last London exhibition. Articles are to be received between the 1st of October and 12th of December, 1864.

18. *British Association.*—The next meeting of the British Association, to be held at Bath, will open on Wednesday, the 14th of September. Sir Charles Lyell is the President for the year.

19. *Sir Charles Lyell.*—Sir Charles Lyell, geologist, has been made a baronet, under the title of Sir Charles Lyell, Baronet of Kinnordy, in the county of Forfar.

20. *Prize to Mr. Ruhmkorff.*—The prize of 50,000 francs, offered by the Emperor Napoleon for the most useful application of electricity, has been awarded to Mr. Ruhmkorff for his induction coil. The king of Hanover, having heard of the award, forwarded to Mr. Ruhmkorff a large gold medal "pour le mérite."—*Reader*, July 9.

21. *Prize to Mr. Sorel.*—The prize founded by the Marquis d'Argenteuil for the most useful discovery for the perfecting of French industry, has been awarded to Mr. Sorel, the inventor of the process of the "zincage of iron," known under the name of *galvanizing iron.*—*Les Mondes*, May 26.

22. *Alger's Cabinet of Minerals for sale.*—The mineral cabinet of the late Francis Alger is one of the best in the country, and will be a valuable acquision to any institution desiring a first rate collection. It can be examined at the former residence of Mr. Alger, Fourth street, Boston. For terms, which it is stated will be low, reference should be made to S. E. Sewall, or Francis Alger, administrators of Mr. Alger's estate.

OBITUARY.

Evan Pugh, Ph.D., F.C.S., President of the Agricultural College of Pennsylvania, died at Bellefont, Pa., April 29, 1864, aged 36.

Dr. Pugh was one of the most able scientific men of this country. A blacksmith's apprentice at the age of nineteen, he bought the residue of his time, supported himself for one year at the manual labor Seminary at Whitestown, N, Y., and after teaching a private school for boys in Oxford, Pa., his native place, for about two years, he went to Europe where he spent four years in the universities of Leipsic, Göttingen and Heidelberg, and in Paris, a most diligent and successful student of natural and mathematical science. At Göttingen he honorably sustained the examinations for the degree of Doctor of Philosophy.

From the outset, his mind had been attracted toward agricultural science and his studies shaped themselves more and more toward his future career, though he found time to study, as he had the capacity to master, the highest mathematics. Before leaving Paris he addressed to Mr. J. B. Lawes the distinguished English agriculturist, so well known by the numerous and valuable researches carried on at his estate of Rothamstead near London, a proposition to undertake a new investigation of the question, then so vigorously mooted in France between Boussingault and Ville, as to the assimilability of free nitrogen by vegetation. Mr. Lawes received this proposition favorably and signified his willingness to have the research carried on in his laboratory and to defray all the costs, provided Dr. Pugh could satisfy him of his ability to estimate nitrogen with a certain degree of precisiou. Dr. Pugh repaired to Rothamstead, and by a skillful application of volumetric methods shortly satisfied Mr. Lawes. For two years, our lamented countryman devoted himself to the solution of the problem which was in dispute, repeating his trials and modifying his devices until he demonstrated, beyond any reasonable doubt, that Ville, and the Commission of the French Academy which reported favorably on his researches, were wrong, and Boussingault right.

Although Mr. Lawes was anxious to retain Dr. Pugh in his laboratory at a handsome remuneration, and notwithstanding the latter was passionately fond of cultivating the fields of scientific research, he returned home in the autumn of 1859, after an absence of six years, to assume the presidency of the Agricultural College of Pennsylvania which had been offered him. He entered at once upon his new duties with characteristic energy and intelligence. He had visited and carefully studied the chief agricultural academies and schools of Europe, and his idea of what an American agricultural college should be was as definite as it was comprehensive and just.

For a little more than five years, Dr. Pugh labored untiringly in establishing his college on a broad and enduring basis, securing funds, planning and superintending the erection of buildings, and besides taking the general guidance of the institution, himself giving instruction in scientific agriculture, chemistry, mineralogy and geology. In the midst of his heavy duties and still heavier cares, he continued vigorous and with every promise of long usefulness until one week previous to his unexpected death.

Dr. Pugh's career as a scientific investigator began when he was a student with unusual promise; but was suspended on his assuming the presidency of an unformed and struggling institution, unfortunately never to be resumed.

While in Europe Dr. Pugh made the investigations that form the subjects of his published contributions to science. They are principally the following, viz:

*Hämatinsalpetersäure identisch mit Pikraminsäure*, Journal für Prakt. Chemie, lxv, 362.

*Miscellaneous Chemical Analyses*, Inaugural Dissertation, Göttingen, 1856.

*On a new method of estimating Nitric Acid*, Quart. Jour. Chem. Soc., xii, 35; and

*On the Sources of the Nitrogen of Vegetation with special reference to the question, whether plants assimilate free or uncombined Nitrogen*, Phil. Trans., ii, 1861, 150 pp., 4to.

The last mentioned investigation was made in connection with Messrs. Lawes and Gilbert; but Dr. Pugh's share in the work was by no means the least. He devoted to it two years of nearly constant labor and its results are in a high degree satisfactory. He did not merely confirm Boussingault's conclusions and refute the gross errors of Ville, but by a careful investigation of collateral questions demonstrated a rare degree of talent in handling a scientific question.

The Agricultural College of Pa., the first institution of the kind established in this country, was attaining a high degree of success and usefulness, as a result of the rare combination of scientific and practical knowledge with administrative energy which characterized its lamented President. His death is a loss to Pennsylvania and to the nation which cannot be soon made good.

Francis Alger, author of "Alger's Phillips's Mineralogy," died on the 27th of November last, in the 56th year of his age.

## VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Review of American Birds in the Museum of the Smithsonian Institution;* by S. F. Baird. Part I, North and Middle America. Sheets 1, 2 and 3, 48 pp., 8vo; to be continued. Smithsonian Miscellaneous Collections, 181. June, 1864.—Dr. Baird, under the auspices of the Smithsonian Institution, has here commenced the publication of a work on the Birds of America. Although intended as a catalogue, with critical notes, of the species in the Smithsonian Collections, it will include references to all species examined anywhere by the author, besides a mention, at the end of genera or families, of species not seen by him. Under the term "North America" is comprised all of America north of a line from the Rio Grande on the Gulf of Mexico to Guaymas on the east side of the Gulf of California; and under that of "Middle America" the part south of this line to the continent of South America together with the West Indies. The references, in the first three sheets issued, are very complete, and the critical notes full and discriminating, such as might be expected from the accuracy and science of the author.

As the specimens of the Smithsonian Institution have been collected largely by the various government exploring expeditions or by special private expeditions, and under the direction in part of the Smithsonian Institution, the author has the material for a thorough discussion of the geographical distribution of species, and the presentation of the facts under this head will, as the author states, be one great object before him.

2. *Observations on the Terrestrial Pulmonifera of Maine, including a Catalogue of all the species of Terrestrial and Fluviatile Mollusca known to inhabit the State;* by Edward S. Morse. 64 pp., 8vo, with numerous cuts. From the Journal of the Portland Society of Natural History, March, 1864. Portland, Maine.—This annotated catalogue of the species of Maine Terrestrial Pulmonifera is quite fully illustrated by woodcuts. These cuts represent the shells of a number of the species, and also, for many of them, the buccal plate, and the lingual membrane with its denticles, besides, in a few cases, other details, such as magnified views of the exterior surfaces of the shells, etc.

3. *The American Annual Encyclopedia and Register of Important Events of the year* 1863—*embracing Political, Civil, Military and Social affairs; Public Documents; Biography, Statistics, Commerce, Finance, Literature, Science, Agriculture, and Mechanical Industry.* Volume III. 866 pp., large 8vo. D. Appleton & Co., New York. 1864.—The general character of this bulky annual, published as a continuation of Appleton's Encyclopedia, has been stated in former notices, and the scope of the work is mentioned in the above title. The political events of the year are detailed at much length and with a number of woodcut illustrations. In Science, there are articles in the departments of Chemistry, Astronomy, Physics, Ethnology and Anthropology, Geographical Explorations, on Iron and Steel, etc., etc., under which much valuable information is presented.

4. *Passages from the Life of a Philosopher;* by Charles Babbage, Esq., M.A., F.R.S., etc., etc.—The philosopher referred to in the title of this volume is "Charles Babbage, Esq., M.A., F.R.S.," etc., etc., the inventor of the Calculating Machine.

Abstracts of Meteorological Observations made at the Magnetical Observatory, Toronto, Canada West, during the years 1854 to 1859, inclusive. 136 pp. 4to. Toronto, 1864.

An elementary Text-book of the Microscope, including a description of the methods of preparing and mounting objects, &c.; by J. W. GRIFFITH, M.D., F.L.S. 12mo. London, 1864 — Van Voorst.

Manual of the Metalloids; by JAMES APJOHN, M.D., F.R.S., &c., Prof. Chem. Univ. Dublin. pp. viii, and 596. London.—Longmans.

On the Archeopteryx of von Meyer, with a description of the Fossil Remains of a Long-tailed species, from the Lithographic stone of Solenhofen; by Professor OWEN, F.R.S., &c. 15 pp., 4to, with 4 lithogr. plates.—From the Transactions of the Royal Society.

Expeditions on the Glaciers; including an ascent of Mont Blanc, Monte Rosa, Col du Géant, and Mont Buét; by a Private of the Thirty-Eighth Artists', and Member of the Alpine Club. London, 1864.

The Central Alps, including the Bernese Oberland, and all Switzerland excepting the neighborhood of Monte Rosa and the Great St. Bernard, with Lombardy and the adjoining portion of the Tyrol; by JOHN BALL, M.R.I.A., Late President of the Alpine Club.—An Alpine guide book.

The Dolomite Mountains. Excursions through Tyrol, Carniola, and Friuli, in 1861, 1862, and 1863; with a geological chapter and pictorial illustrations from original drawings on the spot; by JOSIAH GILBERT and G. C. CHURCHILL. London, 1864.—Longman & Co.

British Conchology; or an account of the Mollusca which now inhabit the British Isles and the surrounding Seas. Vol. II, comprising the Brachiopoda, and the Conchifera from the Family of Anomiidæ to that of Mactridæ; by JOHN GWYN JEFFRYS. London, 1864.—Van Voorst.

The Stream of Life on our Globe: its Archives, Traditions, and Laws, as revealed by modern discoveries in Geology and Paleontology. A Sketch in Untechnical Language of the Beginning and Growth of Life, and the Physiological Laws which govern its Progress and Operations; by J. L. MILTON, M.R.C.S. London.—Hardwicke.

Flora Fossile dell' Etna; by FRANCESCO TORNABENE. 147 pp. 4to, with 10 plates. Sicily.

Ontologie Naturelle, ou Etude philosophique des êtres; by M. FLOURENS. 3d edition. Paris.

Mission Scientifique dans l'Amerique du Sud, par le Dr. B. SCHNEPP. Large 8vo. Paris. 2½ *fr.*

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Sur la Viticulture du sud-est de la France; par Dr. J. GUYOT. 1 vol. large 8vo. 304 pp. Paris. Imperial press.

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## ART. XXXI.—*Heinrich Rose.*

WHEN a man who for more than the lifetime of one generation has filled a prominent place in science, whose labors have extended over every department of this science, while in one of them he has long been recognized as its ablest teacher and highest authority; when such a man dies, it is natural to inquire in what way his reputation was gained, and what part of our present knowledge is due to his services.

Distinction in science is attained by individual effort, and by influence exercised over others. There have been men, like Dalton, of humble origin and retiring habits, unable to command the advantages of a public position, and scantily provided with the means for private research, who by speculation and experiment confined to some one branch have reached the highest eminence in science, and discovered laws of nature with which their names are thenceforth connected. Others to appearance more fortunately situated, have been less limited in their range; enjoying by birth or position intercourse with men already distinguished, able to join with and assist them in their researches, and to diffuse the information which they had aided in acquiring, they have contributed as well by inciting others, as by their own works, to the advancement of knowledge. It is the privilege of men of this class to associate and surround themselves with others of even greater distinction in their individual departments. At Arcueil, Berthollet gathered about him the brightest names of the chemistry of his day; and Berlin was a centre to which the fame of Humboldt attracted much of the learning and genius of our own times. Humboldt himself is gone, and v. Buch, Ritter, Dirichlet, and now, too, Mitscherlich and Heinrich Rose.

In what way then did Rose get the name which he has left?

He was born on the 6th of August, 1795, in Berlin, where his father and grandfather were pharmaceutists and chemists. The grandfather is now best known as the discoverer of the fusible alloy called after him, Rose's metal; his father, a man of considerable scientific reputation, died in the year 1807, in the days so dark for Prussia, leaving a widow and four young sons. Rose studied pharmacy first in Danzig, at the time when the city was besieged by the French under Gen. Rapp, and was near losing his life from the typhus which prevailed in the town. He served in the campaign of 1815, with his three brothers, and on his return remained in Berlin until the end of August in the following year. He was not a pupil of Hermstädt, as has been asserted, but was engaged at this time in the laboratory of Klaproth, as we learn from the discourse which he delivered before the Berlin Academy in memory of Berzelius.

"I had the good fortune in my youth to assist the celebrated Klaproth in his chemical operations, but only in the last year of his life, in the summer of 1816, when his labors were interrupted by frequent and severe illness. When, several years afterward, I was permitted to work in the laboratory of Berzelius, I was able to compare his ways of operating with those of Klaproth; they corresponded to the difference in accuracy of their respective investigations."

In September, 1816, he went to Mietau, in Curland, and entered the pharmacy of Dr. Bidder; his leisure time was passed with the physicist Grothuss, who lived on his estate in the neighborhood, and in whose works Rose published his first scientific communication.[1] Leaving Mietau in 1819, he travelled by way of St. Petersburgh and Finnland to Stockholm, and accepted gladly the invitation of Berzelius to finish in his laboratory some investigations which he had begun upon the varieties of mica. Here he remained two years. In December he was joined by Mitscherlich, who was already known to Berzelius in Berlin. In the spring of 1821, his brother, Gustav Rose, came also to Stockholm, but left the laboratory in the summer for the purpose of travelling over Sweden and Norway. Mitscherlich and Heinrich Rose left Stockholm in the autumn of 1821; Mitscherlich to go directly to Berlin, Rose to spend some weeks in Kiel, where he took the degree of Doctor of Philosophy. He published there his first independent work, an inaugural dissertation in Latin upon titanium, and its compounds with oxygen and sulphur; a subject upon which he worked, and

[1] Prof. G. Rose. The works of Grothuss are not to be had here.

wrote several papers, in after years. He established himself in Berlin in 1822, and was appointed in the following year to a Professorship of Chemistry, which he retained till his death. From this time the history of his life is to be found in his works; it is a part of the history of science.

Born as we have seen of respectable parents who could however have left him but a moderate competency, and a name which, though already known in science, was to become more distinguished in their descendants, Rose owed his reputation to his works, the fruit of nearly fifty years passed in his laboratory and at his desk. These alone would have secured him a lasting reputation. But he was not unmindful of the other means by which he had it in his power to further the progress of science. His name and their own excellence attracted to his lectures a numerous audience, and gave him the opportunity of imparting his ideas to numbers of young chemists who came to him from all parts of the world. His lectures were marked by their simplicity and their soundness. In referring the phenomena of chemistry to the views which are alike their cause and explanation, he avoided all theories built up in advance of the facts. He distinguished the theories which have been so firmly established that they may safely serve as a foundation for new views, and as a guide to the discovery of new facts, from those that need to be themselves further confirmed by observation. He saw that to reject the one class was to give up the advance already made; to accept the other was to endanger future progress.

The facts were stated in his lectures, with their explanation in the plainest words, and the experiments designed to illustrate them were made in the simplest way, and, if possible, without the use of intricate or expensive apparatus; for very many, in fact, he required nothing more than an ordinary test-tube. It seemed to be his aim to avoid distracting the attention by the complexity or elegance of the apparatus, and to familiarize his hearers with the arrangements employed in actual research. This dislike to display has given rise to the story, which, if not true, is characteristic of himself as of his lectures, that when his assistant had caused the tarnished spirit lamps upon his table to be brightened, he found the Professor busily employed after the lecture in restoring to them their former dingy hue; remarking, "that he could not talk easily where there was so much glitter."

His lectures were delivered in an easy and conversational style, while he walked backward and forward behind the table upon which his apparatus was arranged. Even in his later years, when the subjects had become familiar to him from frequent repetition, the half hour before the lecture found him abstracted and with a thoughtful expression; as soon as his hour was over, he returned to the laboratory, and ever with a bright smile and kind greeting for the young men whom he found there at work.

Beside the public instruction given by him as Professor, Rose was in the habit of receiving a few pupils into his private laboratory, and teaching them, partly himself, but chiefly through his assistant, the practical operations of analysis.

Without neglecting accuracy of execution, it was his principal care that analytical operations should be conducted upon correct principles; and he always gave his personal attention to the selection of the most suitable course, and then followed the analysis in its progress with an interest which kept up that of his student. When a new subject was presented, the question was often asked, "How will you do this?" The answer was listened to with patience and interest, and the sources of inaccuracy of the proposed method clearly and kindly pointed out, or a better one suggested. The number of his pupils was limited, in the latter years of his life to one or two. They were required to furnish themselves only with such implements as would necessarily be injured by use, or were liable to accident; as also with a balance, that the convenience and accuracy of his own work might not be interfered with. All other apparatus and all the reagents, even the more expensive ones which in public laboratories are seldom to be had, were provided in abundance. No outlay was objected to, when it secured greater accuracy in the results.

For this great privilege, attended as it was with pecuniary expense to himself, Rose never accepted the slightest return. It was given from a pure love of science, and he expected it to be used in the same spirit. Even the ordinary fee for his lectures, conveyed to him from one of his students for greater delicacy through his assistant, was not received without hesitation. Rose never made use of his students to perform the drudgery and routine of his private researches; these he did himself, or left, generally, to his assistant; to his pupils was given some new or rare mineral, or other object of interest.

In a paper written by Liebig, in 1840, is the following passage; it shows the high consideration in which the teaching of Rose was held by one himself of an entirely different school.

"Heinrich Rose—the only man in Prussia who gives practical instruction in science, the only one to whom it is a pleasure, and who possesses the ability to educate young men in chemistry—is furnished with no adequate means. His laboratory, wholly unsuited to the purpose for which it is used, is in a private building, a part of the rent of which is paid by the Government; but he receives nothing for its current expenses. * * * For some time he delivered a course of four lectures in the week, but could only give instruction in mineral analysis, for which the least outlay is required; but even for this the fee which he received was insufficient, and he was compelled to add to it from his private resources."

Rose's laboratory, at the time when some of the happiest days of our life were passed in it, was nearly in the centre of Berlin, behind the new Museum, and within a few minutes' walk of the University and the other public buildings on the Linden. The street formed one bank of the Spree, and the laboratory looked out upon its sluggish stream.

—— "undique latius
Extenta visentur Lucrino
Stagna lacu."

The laboratory was in the house in which Rose had his private dwelling, and consisted of only three rooms: an ante-room filled with chemical preparations, the working laboratory, and a cabinet for the balances and other nice instruments. All furnace work, and the preparation of the reagents, was carried on in the cellar. Behind these was the auditorium, or lecture-room; but all in a private house, in no way connected with any Government building.[2]

The writings of Rose consist of his Treatise on Analytical Chemistry, and of a great number of papers published in the latter volumes of Gilbert's, and then in Poggendorff's Annals. In considering these writings, it must be remembered that they extend over a period of nearly fifty years. It is difficult to comprehend the circumstances under which works at such a distance of time were composed, to represent to ourselves the state of science previous to their completion. Each of them must be looked at from a different point of view, which is ever changing with the continued advance of science. To judge of them correctly, we must bear in mind what was known at the time when they were written, as well as that which has since been discovered; we must see them, neither as they were seen by their cotemporaries, nor as they are seen by those of our own times, but rather as they would be regarded by a cotemporary who had kept up with the knowledge of the present day. The number of these works makes it impossible even to allude to them all, while it will be our especial object to avoid giving such abstracts of them as have already been made in the Annual Reports. By selecting from the more important the points of par-

[2] For the greater part of the incidents in the life of Rose, I am indebted to a private communication from his brother, Professor Gustav Rose, the distinguished mineralogist and crystallographer of Berlin, who also kindly enclosed an obituary notice written by his son-in-law, Professor vom Rath, of Bonn, for the Kölnische Zeitung of Feb. 2, 1864, and a sketch by Professor Rammelsberg, of Berlin, of the chemical works of Heinrich Rose, from which I have allowed myself to make a few quotations. D.

ticular interest, and giving a sketch of the previous state of knowledge with regard to them, it is hoped that some idea may be conveyed of the part which Rose's labors have had in the advancement of science. It will be convenient for this purpose to take up first his researches upon general subjects; leaving the analytical articles to be mentioned in connection with his great work on Analytical Chemistry, into which they were from time to time incorporated.

"The first important work of Rose was his investigation of the minerals having the crystalline form of augite. It was made in 1820 in the laboratory of Berzelius, and together with the works of v. Bonsdorff upon hornblende, and of Graf Trolle-Wachtmeister on the garnets, is the foundation of our knowledge of isomorphism in the mineral kingdom." Rose showed that the different varieties of augite are isomorphous silicates of lime, magnesia, and the protoxyds of iron and manganese, which, like the artificial isomorphous salts described by Mitscherlich, crystallize together in the same form, and are capable of replacing each other in indefinite proportion. He also first noticed that the augites from Sahla in Sweden contain at times a quantity of water, which may even amount to five per cent; a fact afterwards brought forward by Scheerer in support of his theory of polymeric isomorphism.

Berzelius expressed a high opinion of the ability shown by Rose in this work, in a note appended to his account of it in the first volume of his Annual Reports. "The analyses of Rose and v. Bonsdorff were performed in my laboratory; not under my supervision, of which these skillful young chemists had no need, but that I might have the pleasure of bearing witness to the care and exceeding accuracy with which they were executed."

Rose afterwards made analyses of the analcime of Catania and Fassa, which gave to this mineral the same formula with that of leucite, with the difference that analcime contains two equivalents of water, and soda in the place of potash. Upon this Berzelius raises the question, "Is soda with two atoms of water isomorphous with anhydrous potash?" This is, perhaps, the first idea of a polymeric isomorphism.

In the laboratory of Berzelius too, were made many of Rose's analyses of mica, which showed such a difference in composition that he was unable to deduce from them a general formula. They led him, however, to the discovery in it of fluorine, which he found, but in very different proportions, in all the varieties examined. The presence of fluorine as a constituent of mica had been overlooked by both Klaproth and Vauquelin; it was found by Rose by heating the mineral in a porcelain retort, with the object of discovering the source of its loss of weight on ignition; fluo-silicic acid came over into the receiver.

The research upon titanium was also begun in the laboratory of Berzelius, and was taken, as has been said, for the subject of his inaugural dissertation.

Rose first showed the real nature of what had hitherto been called oxyd of titanium; that it is not a base, but, on the contrary, combines with bases to form a peculiar class of salts; he therefore changed its name to titanic acid. He also discovered the compound of titanium with sulphur.

"In Berlin he resumed this research and analyzed the native compound of titanium and iron, which he at first considered to be a titanate of protoxyd and peroxyd of iron, but afterwards recognized as a mixture in indefinite proportions of the isomorphous sesquioxyds of iron and titanium."

The atomic weight of titanium was determined with a degree of care and accuracy which secured its universal adoption. Deducing the atomic weight from the composition of the chlorid of titanium, Rose determined the chlorine, and the titanic acid formed when a known quantity of the chlorid was decomposed by water. His description of the mode in which the calculation was effected is somewhat obscure, but it is evident, that of the three values found, two were sufficient for the determination of the atomic weight; and that therefore the observed values could be combined in three different ways. As however the ignited titanic acid, in consequence of absorbing moisture from the air, could not be weighed with accuracy, Rose preferred to base the calculation upon the amount of chlorine. His results, recalculated for the atomic weight of chlorid of silver of Marignac and Stass, give for the equivalent of titanium the number 24·12—from the titanic acid, combined respectively with the chlorid of titanium taken, and with the chlorine found, are obtained the numbers 24·90 and 23·47, of which that assumed by Rose is very nearly the mean.

Rose's extreme values, according to his own mode of calculation, are 24·03 and 24·25.

The number 25, which has been recently adopted for the atomic weight, was obtained by Pierre by volumetric determination of the chlorine in the chlorid of titanium. The greater number of his determinations agree better than those of Rose; the lower atomic weight given by some of them is ascribed by him to the fact that they were made with a chlorid which had been exposed to the air; and from a portion which was intentionally exposed for a considerable time, still lower numbers were derived. The cause of this difference is not apparent, and the explanation given of it by Pierre seems hardly probable.

As the experiments of Mosander, made after those of Rose, gave a *lower* number than his, there seems to be no valid reason for abandoning the atomic weight found by Rose until the subject shall again have been thoroughly investigated.

In the further progress of this research, by showing that the minerals rutile, brookite, and anatase, though occurring in forms which belong to different systems of crystallization, consist essentially of titanic acid, Rose established the first decided instance of trimorphism in bodies having the same chemical composition.

He showed that the change in solubility in acids, produced in many metallic oxyds by ignition, is attended by phenomena which justify us in considering the body to have passed into a different, isomeric state. He pointed out that the change is often indicated by momentary incandescence, and by an alteration in the specific gravity. The passage from the one state to the other can sometimes be effected without the aid of heat, as in the case of the two forms of peroxyd of tin, whose properties he afterward minutely investigated. A similar change is produced in titanic acid by merely washing with hot water—the transition is attended with no perceptible disengagement of light, and the acid which has then passed into the insoluble modification shows no phosphorescence on ignition. The phenomenon is of the same nature as the incandescence of the ammonio-phosphate of magnesia in becoming pyrophosphate, and probably also of the oxalates when they are converted by heat into carbonates. In all these cases a change in the intimate structure of the substance is produced. The disengagement of light is sometimes, as in gadolinite, accompanied by evolution of heat, and diminution of the specific heat of the substance, which it has been supposed might be its explanation; but in other cases no heat can be detected, and the specific heat remains sensibly the same, or may be even slightly increased.

Without being able completely to explain these phenomena, Rose's experiments demonstrated that the difference of chemical properties without alteration of composition, which characterizes what is expressed by the term isomeric states, is intimately connected with certain physical phenomena, of which it is more probably the consequence than the cause.

The emission of light during crystallization was investigated, and was shown to occur when a body which is destitute of all crystalline structure, like the glassy form of arsenious acid, separates for the first time from a liquid in crystals. Certain salts too, which, when fused, solidify into a glass-like mass, exhibit on crystallization the same phenomenon; but the crystalline form once assumed, the phosphorescence cannot be reproduced unless the texture be destroyed by a second fusion. There would seem then to be a crystalline state of matter independent of all solid form, and which it retains even in solution.

Among his earlier works are the analyses of the lead ores from Tilkerode in the Harz, discovered by Finken, in which Rose found selenium, and the examination of the natural sulphur-salts of antimony and copper, the Fahlerz of the German mineralogists, or tetrahedrite, of whose composition little was known from the want of accurate methods for their analysis.

In a chemical point of view they are a continuation of the great work of Berzelius on the sulphur-salts, and show that the distinction made by him of sulphur acids and bases is as marked in nature as in the products of the laboratory. They also suggested the isomorphism of the sulphurets of copper ($Cu_2S$) and silver (AgS), and the consequent necessity of halving the atomic weight of the latter; an idea which he developed at length in several subsequent papers, which gave rise to his last discovery, and was the subject of his latest communication, written but a few months before his death.

One of the most important of Rose's works is a series of papers on the compounds of phosphorus with hydrogen and oxygen. The first was published in 1826, and they extend over a period of twenty years.

It was known that there were two kinds of phosphuretted hydrogen, one of which was spontaneously inflammable in contact with air; but the experiments of the ablest chemists of the age had failed to show in what the difference, if any, between them consisted, and they disagreed as to their composition. Gay-Lussac and Thénard had found, rightly as afterwards appeared, that on heating potassium in the inflammable gas a volume of hydrogen remained, equal to $1\frac{1}{2}$ times that of the original gas. Vauquelin and Thomson had obtained a different result, and Davy found the volume of the hydrogen to vary according as he employed the one or the other variety. The possibility was not yet admitted that the same elements combined in the same proportion could have different properties, for the fact by which it was first suggested had but just been discovered by Faraday. Of the compounds of phosphorus with oxygen, the phosphoric and phosphorous acids had been studied by Berzelius, with some of their salts, and their composition rightly determined. The hypophosphorous acid, discovered in 1816 by Dulong, was erroneously considered by him to contain half as much oxygen as the phosphorous acid, and Davy had mistaken the relation of the oxygen in all three of the acids. Thus the subject was in a most unsettled state; and, to increase the confusion, simultaneously with the work of Rose came out a research by Dumas, followed by one from Buff, both differing from the conclusion at which Rose had arrived. Rose was led in consequence to reëxamine the points about which he was at variance with these chemists. The results of his new investigation were published in 1832; it is the most important paper of the series, and is preceded by an abstract of the former ones, from which a sufficient idea of them may be obtained.

Finally, after the communication by Wurz of his theoretical views, according to which the lower acids are to be regarded as phosphoric acid in which one or more atoms of oxygen are replaced by hydrogen, Rose wrote, in 1843 and 1846 a careful and thorough examination of the arguments in favor of the several theories of the constitution of these acids.

Rose's first step was to determine the composition of the two kinds of phosphuretted hydrogen from that of the phosphuret formed when the gas was led over the chlorid and subchlorid of copper, ($CuCl$ and $Cu_2Cl$,) and the sulphurets of nickel and copper, ($NiS$ and $Cu_2S$.) The definite proportions of the first two phosphurets showed their stability in contact with the gas; as the sulphuret of copper is not decomposed by hydrogen alone, the possibility of error from presence of this gas was obviated. The spontaneously inflammable gas was shown to contain 1 eq. of phosphorus to 3 eq. of hydrogen; with the other variety the results were not constant enough to prove the absolute identity of the two. Rose then, in 1832, demonstrated that the density of the two gases is the same, and not that which had been found for them by Dumas; and that both combine with a number of bodies, principally volatile metallic chlorids, and give with them precisely the same compounds. He went still further; he showed that when these compounds are acted upon by water, or aqueous solutions, they are decomposed with reproduction of phosphuretted hydrogen, and that the nature of the evolved gas depends, not upon the kind used in the preparation of the compound, but solely upon the liquid by which decomposition has been effected.

From these facts Rose drew the conclusion that the two varieties of phosphuretted hydrogen have the same composition; that they are isomeric forms of the same body, for the doctrine of isomery had in the meantime been developed, and that by combination with the metallic chlorids and subsequent decomposition, the one of these forms may be converted into the other. The apparent convertibility of the two gases seems to us, however, in strictness to prove only the identity of their compounds.

More recent researches have changed the views upon this subject only so far as to make it possible that the difference in inflammability of the gases *may* be due to the presence of some foreign body in such minute quantity as to elude observation. If this substance be the liquid phosphuret of hydrogen described by Paul Thénard, and condensible at a temperature of $-10°$ C., the fact is in direct opposition to an observation of Rose, which seems to have been overlooked. "I made," says Rose, "some experiments to see if, by exposure to cold, phosphuretted hydrogen would lose the property of spontaneous inflammability in contact with air. During the severe weather in January, 1823, in Berlin, when the thermometer stood at $-15°$ C., ($+5°$ F.,) I led the gas in the open air through a very fine tube of glass, (durch eine sehr dünne Glasröhre,) 8 feet in length, of which 7 feet were surrounded by a freezing mixture, in which mercury speedily froze. The gas remained however as inflammable as at higher temperatures, and not the slightest condensation could be perceived."

Rose then examined the composition and products of decomposition of the salts of phosphorous acid, and showed that they contain two atoms of base to one of acid, and a quantity of water, generally two atoms, which is essential to their existence. The composition of the hypophosphorous acid was established from the facts that the salt of lime is oxydized by nitric acid to metaphosphate, and by ignition is converted, with evolution of spontaneously inflammable $PH_3$, to pyrophosphate. The salt therefore contains equal equivalents of phosphorus and lime, and gives off the half of its phosphorus combined with hydrogen.

$$2(P + xO) + 3HO = PO_5 + PH_3$$

This result was then confirmed by direct analyses of remarkable elegance, and which are well worthy of attention.

Besides the compounds of $PH_3$ with certain volatile chlorids, we owe to Rose the discovery of a class of bodies in which ammonia is combined with the chlorids of various metals, and the very remarkable combinations of the anhydrous acids with ammonia and with salts, the first instance of which, the union of sulphurous acid with ammonia, was observed by Döbereiner, but which Rose now first fully investigated. He made known too the true nature of what had been considered as the superchlorids of chrome, wolfram and molybdenum. The composition of the volatile metallic chlorids which are decomposed by water into hydrochloric acid and a metallic oxyd, had hitherto been inferred, without analysis, from that of the oxyd. Rose, determining both the chlorine and the chrome in the red volatile liquid produced by the action of sulphuric acid upon a mixture of chlorid of sodium and chromate of potash, found a great loss in his analysis; and that the chlorine was in quantity to combine only with a third part of the chrome. It followed that the liquid instead of being a chlorid of chrome, corresponding in composition to chromic acid, contained a large amount of oxygen, and could be considered as a compound of two equivalents of chromic acid, and one of perchlorid of chrome.

Two compounds of wolfram and chlorine were known, which, though entirely different in appearance, were supposed, as they had been found to give with water hydrochloric and wolframic acids, to have the same composition, and to be isomeric bodies. The one of them, which sublimes in small, yellowish white scales, was shown by Rose to contain oxygen and to be in composition precisely analagous to the acichlorid of chrome, or chloro-chromic acid; the other, which is red, and of a voluminous and wooly texture, he proved to be the chlorid corresponding to the oxyd of wolfram, ($WCl_2$). But it was only after twenty years labor with the similar compounds of the tantalite group, that experience taught him the precautions which are necessary to ensure their perfect separation.

Analogous in composition to these bodies, is the compound of sulphur, oxygen and chlorine, ($SClO_2$ or $SCl_3 + 2SO_3$,) discovered by Regnault, to which Rose added a large class formed by the action of anhydrous sulphuric acid upon the chlorids of sulphur, phosphorus, selenium and tin.

In the papers on the chemical decompositions produced by water, its solvent action, in which the complex particles of a compound may be conceived to be separated from one another, is distinguished from the chemical effects in which water acts in virtue of its quantity, rather than by the exercise of any powerful affinity. Here the complex particles are not merely distributed through the liquid, but their components are divided and forced into new states of combination. The research is an illustration of the law of Berthollet, according to which the mass, or chemical quantity of a body, may take the place of strong affinity. If the law be admitted, and it is unquestionably true in many cases, there is no reason why a compound, however strong the affinities by which its particles are held together, may not be decomposed by the feeble action of any third body which acts upon it in sufficient quantity. And we see, says Rose, in nature, where the action is longer continued than it can be in the laboratory, firmly united compounds gradually disintegrated by bodies of such feeble affinities as carbonic acid and water. Water acts sometimes as a base, uniting with the acid and separating the base with which the acid was previously combined, as oxyd, or as a basic salt; at other times as an acid, entering into combination with the base, and displacing its original acid. The subject is illustrated by examples of this decomposing action, of which Rose had mentioned a remarkable case in a former paper. A concentrated solution of borax gives with nitrate of silver a white precipitate of borate of silver, which is soluble in a not very large quantity of water. But if the solution of borax be very dilute, it produces no longer the white borate, but a brown precipitate of oxyd of silver. The borax has been decomposed by the water into boracic acid and soda, which latter acts as though the acid were not present. This action of water is also shown when a strong solution of borax is colored with litmus, and a little free acid is added until the liquid has a slight reddish tinge; on diluting with water, the blue color is at once restored.

The decomposition of the acid sulphate of potash by water into neutral salt which can be made to crystallize, and free acid, mixed with acid salt, is also brought forward: by dissolving the salt in the proper quantity of water, intermediate compounds can be formed, which, however, seem when crystallized to be in atomic proportion. The same action is the cause of the separation of some double salts into their constituent simple salts by solution and crystallization, which especially happens when they differ much in their solubility. Other double salts resist to a greater or less degree this action. Probably of the same nature is the fact which has since been noticed, that crystals of double salts may be obtained which are not in atomic proportion: the decomposing action of water seems to overcome the tendeney of crystallization to produce definite compounds. Whether in these cases the water takes the place of one of the simple salts in the crystal, is a point which has not yet been sufficiently determined.

A soluble salt may, on the other hand, be protected from the action of water by combination with one which is insoluble. Sulphate of baryta, if precipitated from solutions containing nitric acid, is well known in analytical operations to take down with it small quantities of nitrate of baryta, which a long continued washing is required to remove. [And carbonate of baryta, prepared from the nitrate or chlorid with carbonate of ammonia, always contains nitric or chlorhydric acid, which, at least in any quantity, it is impossible by water to free it from. The only way by which the pure carbonate could be obtained was by leading carbonic acid into a solution of caustic baryta.] The native carbonate of soda and lime, the mineral Gay-Lussite, gives up, according to Boussingault, but little of its soda to water: while carbonate of soda or of potash can retain carbonate of lime in solution, and deposits it by exposure to cold as a white crystalline powder, which Bauer found to have the composition of Gay-Lussite.

The action of water upon the metallic chlorids had been before examined by Rose. He showed that where heat is evolved on their solution, there is reason for considering that the chlorid does not dissolve unchanged, but is decomposed into chlorhydric acid and a metallic oxyd. Many of these chlorids undergo a still further change by the action of water, by which the oxyd, or generally a basic salt, is separated, while it was assumed that an equivalent acid salt was contained in the solution. The existence of this class of acid salts Rose denies. With some of these compounds, as the chlorid of bismuth, the decomposition is so complete, if sufficient water have been added, that in the liquid no trace, or only an infinitesimal one, can be detected. Where, as in the case of antimony, the liquid retains a certain quantity of the metal, Rose considers it to exist in the state of neutral salt, held in solution and protected from the decomposing action of the water by the excess of free acid. By evaporation the salt can be made to crystallize; but it is then a neutral salt, and never has one of the so-called acid salts been obtained in a crystalline, or in any solid form. The separation of the oxyd is analogous to the precipitation of certain bases by carbonate of baryta, and may be regarded, like it, as a criterion of the feebleness of their basic properties. That water should not be able to separate from the basic salt the whole of the acid, is not surprising; for, from the basic chlorid of bismuth, the strongest alkalies cannot take the whole of the chlorine.[3]

The decompositions in which water acts as an acid, are exemplified in the formation and composition of the greater number of the carbonates. That the precipitates produced by alkaline carbonates in metallic solutions are basic salts, was well known. They are regarded by Rose as compounds in which the feeble affinity of carbonic acid for bases is overcome by the action of mass of the water, which, displacing the acid, remains combined with the base; they are compounds of the neutral carbonate with the hydrate of the base. Their composition necessarily depends upon the nature of the base, and upon the larger or smaller quantity of hot or cold water which acted upon the carbonate at the moment of its formation, or during the washing required to separate the alkaline salt. The oxyd of silver alone was found to be without all affinity for water; the precipitate produced in its solutions being under all circumstances neutral carbonate of silver. Even the carbonates of the alkalies were found to lose a little of their acid, when the solution was long boiled. Water was then shown to have a similar action upon the salts of boracic acid; and the disintegration of the natural compounds of silicic acid, which is constantly going on through the agency of water and carbonic acid, is to be ascribed to the same power of feeble affinities, when aided by mass, and continued through long periods.

It will have been seen that it was Rose's habit to retain a subject, often for many years, in his thoughts, and to publish from time to time the results of his investigation, while in the intervals he was occupied with other works. This makes it impossible to consider his writings in strict chronological order, and accounts for the want of arrangement and unity which his series of papers sometimes present. Could he in after years have collected and re-arranged the thoughts and facts which are scattered through his successive publications, as was his intention to

[3] Analogous to this is the fact that the basic sulphate of mercury ($HgO,SO_3 + 2HgO$), cannot be converted into the neutral salt by digestion, even with concentrated sulphuric acid; only that portion which dissolves in the acid liquid, combines with the acid, and on cooling, separates, in part, as neutral salt. D.

do with his researches upon the acids of the tantalites, much would have been preserved which is now overlooked in the great extent of his writings, or has been lost in the imperfect abstracts which have been made of them. The first paper of his researches upon the decomposing action of water was published in 1851, but as early as 1839 this action was suggested as the cause of the production of ether in the distillation of alcohol with sulphuric acid.

Of the previous theories of this process, the catalytic or contact theory attempted no explanation; it only placed the formation of ether with a group of reactions which, from their general resemblance, were assumed to depend upon a common principle. The older theory that alcohol, regarded as the hydrate of oxyd of ethyl, was decomposed by the affinity of sulphuric acid for water, failed to show why the water distilled over with the ether. Liebig, assuming the formation in the first instance of sulphovinic acid, overcame this objection by supposing the production of ether and of water to be the result of two different, and not simultaneous reactions; the resolution of the sulphovinic acid was ascribed by him to the temperature of the boiling liquid. Two defects were pointed out in his theory. It was shown by Rose that the formation of ether takes place at a temperature inferior to that at which sulphovinic acid is decomposed; and it was objected that the theory supposes the formation and decomposition of this acid in the same liquid. The latter objection had especial force after it was shown by Mitscherlich that the production of ether continues when the alcohol is supplied in the state of hot vapor, where no great difference of temperature in the portions of the liquid is possible.

Rose, holding to the view of Liebig that sulphovinic acid is first generated, and that the formation of ether and water is the result of different actions which do not occur at the same moment, considered the decomposition of the sulphovinic acid to arise from the action of water, combining as a base with the sulphuric acid and displacing the oxyd of ethyl. Water acts in his view upon the double sulphate of water and ethyl precisely as it does upon the salts of feeble bases, like bismuth or antimony. The difficulty lies here again in conceiving the formation and decomposition of the sulphovinic acid in the same liquid.

In considering the possibility of the formation and decomposition of a compound under the same circumstances, two cases should, we think, be distinguished. When sulphovinic acid is decomposed by the basic action of water, there may be another cause beside the affinity of the sulphuric acid for water, which prevents the reunion of the water with the oxyd of ethyl. For, were they to combine, there would be a reproduction of alcohol in the same liquid in which it had just been decomposed. It might consequently be again resolved into sulphovinic acid and water, and the process be continued indefinitely without giving rise to any new product. Such a *perpetuum mobile* is as inconceivable in chemistry as in mechanics. The fact that alcohol, if reproduced, would be immediately again decomposed, seems to show that the balance of affinities cannot be in favor of its formation. It may then be the decomposing action of the sulphuric acid upon alcohol which prevents the combination of the liberated oxyd of ethyl with water. The production of alcohol instead of ether, when a not very concentrated solution of sulphovinic acid is distilled, does not prove that the same solution, with the addition of sulphuric acid, would not give the opposite result. The direct experiment could, of course, give only an ambiguous result.

The case is, however, very different when alcohol is first resolved into sulphovinic acid, and this acid is in turn decomposed, not back again into alcohol, but into the two new products, ether and water. It is conceivable that the existence of such a transition stage may be the cause of the ultimate decomposition of alcohol into ether and water. The duration of the sulphovinic acid is of no consequence in the production of ether; and if decomposed at the moment of formation, there would be apparently an action of contact, or catalytic action, on the part of the sulphuric acid. May not many, if not all, of the reactions which are ascribed to catalysis be really the result of the formation and direct decomposition of some compound, which have succeeded each other so rapidly that the compound itself has escaped our notice?[4]

Several facts, some of which have been discovered since the publication of Rose's paper, favor his view of the decomposition of the sulphovinic acid by the action of water. In the ordinary process for the preparation of ether, the greater the proportion of sulphuric acid which is added to the alcohol, the higher must the temperature of the mixture be raised; that is, where the action of water is counteracted by its union with a concentrated acid, a higher temperature is necessary to weaken its affinity for the acid and enable it to act as if uncombined. In the opposite case, when sulphovinic acid largely diluted with water is heated, alcohol and not ether distils over. The ether, at the moment of formation, meets with no sufficient impediment to its reunion with the water. Frankland and Reynoso have shown that bromid, and iodid, of ethyl, when heated with water in closed tubes, are resolved into hydro-bromic or iodic acid, and ether, together

[4] M. Barreswil (*Ann. Ch. Phys.*, [3], xx, 364) has, we find, previously advanced this view of the nature of catalytic action; his paper contains also a marked case of the formation of a compound, and its subsequent resolution into new products in the same liquid.

with other products of decomposition. Reynoso found that when sulphuric acid and alcohol were heated under pressure, more or less sulphovinic acid was produced in the greater number of cases; and only when a very dilute sulphuric acid was employed, could none of this acid be detected.

The experiment of Graham, which is quoted by Prof. Miller as opposed to Rose's view, seems capable of a different explanation. 100 parts of oil of vitriol, 48 of alcohol of sp. gr. 0·841, and 18·5 of water, were heated in a closed tube to 143° C. for one hour. The alcohol was evidently decomposed by the large quantity of acid, and no stratum of ether formed upon the surface of the fluid. The tube was opened, and the fluid divided into two equal portions. One of the portions was mixed with half its volume of water, and the other with half its volume of alcohol, and both sealed up in glass tubes and again exposed to 143° C. for one hour.

It would be expected, says Prof. Graham, on the ordinary view of water setting free ether from sulphovinic acid, that much ether would be liberated in the mixture above, to which water was added. The ether which separated, however, amounted only to a thin film, after the liquid had stood for several days. In the other liquid, on the contrary, to which alcohol was added, the formation of ether was considerable, a column of that liquid appearing which somewhat exceeded half the original volume of the alcohol added.

This is, however, not opposed to the views of Rose. It is well known that if, in the preparation of ether by the usual process, a too dilute alcohol be employed, so that the acid becomes diluted beyond a certain point, the formation of ether ceases, and the alcohol distils over unchanged. In the portion of the liquid to which Prof. Graham added water, the affinity of the sulphuric acid may have been so weakened that it was unable to prevent the recombination of the ether with water; for, as has been said, diluted sulphovinic acid gives, on distillation, not ether, but alcohol.

The more recent theory of Williamson, of the constitution and formation of ether, offers no other explanation of the decomposition of the sulphovinic acid than the tendency to duplication of the atomic weight, which Prof. Graham has ascribed to a "polymerizing action" of sulphuric acid; to this, however, at present, no more meaning can be attached than to the older term "catalytic action," of which it is, indeed, but a special case.

Nowhere are the qualities of Rose's mind, his perseverance, his clearsightedness and his earnest love of truth, more clearly shown than in his investigation of the acids of the tantalites. He was engaged upon it nearly twenty years, and to within a short time of his death; the papers upon it extend through nineteen of the volumes of Poggendorff's *Annals*. It was his intention to collect these papers into one memoir for the Berlin Academy; but this purpose was not to be accomplished.

His researches led him in the end to results in the highest degree curious, and without a parallel in the history of the science. They present the nearest approach which has yet been made to the transmutation of an element. Their point of interest has however been sometimes overlooked, and the research regarded but as a detailed account of the properties of bodies possessing little interest or importance. For the minerals are found in few places and in small quantities, and the acids which distinguish them are not characterized by well marked chemical affinities. For by far the greater part of our knowledge of them, and for all that we know of them individually, we are indebted to his work.

Rose was led to the study of the tantalites by an observation of his brother, Prof. Gustav Rose, that the tantalites of Bavaria and of America have the crystalline form of wolfram, while the tantalites proper of Finnland belong to a different system. His attention was also attracted by the fact, that while the tantalites of Bavaria and America, the columbites, and the acids derived from them, had been shown by Wollaston and Thomson to have a lower specific gravity than the tantalite and tantalic acid of Finnland, the columbites themselves from the same locality, and having the same form and composition, possessed different specific gravities; as did also the acid which they contained. He was hence led to suspect the presence of more than one acid in the columbites, by which their varying densities might be accounted for.

In 1844 the existence in the columbites of a new element was announced, which he named Niobium, from the daughter of Tantalus. Rose obtained, by heating the acid of the columbite mixed with charcoal in a current of chlorine, not only the yellow chlorid which the acid from the tantalite gave, but also a white sublimate, which was the chlorid of the new metal.

In 1846 Rose discovered that the yellow chlorid from the columbites was a different substance from that obtained from the tantalic acid; he considered it to be the chlorid of another new metal and gave it the name of Pelopium. The tantalites, then, contained tantalic acid, the columbites the niobic and pelopic acids. "There is a greater resemblance," says Rose, "between the tantalic and pelopic acids than between the compounds of any two other metals; the properties of the niobic acid, on the other hand, are very different from those of the others." Still the separation of the chlorids of niobium and pelopium was attended with the greatest difficulties. "I do not exaggerate when I say that the chlorid of niobium could not be considered as quite free from pelopium until the conversion of the acid into chlorid had been repeated from twenty to thirty times." This was the second stage of the research; Rose's remark gives some idea of the immense labor with which it was attended.

For seven years Rose was now occupied in studying the properties of the two acids, on the supposition that they were the oxyds of different metals, when, at last, their true nature was disclosed to him by a fortunate accident. And how many great discoveries have been owing to accident! to accident in the hands of one who knows how to make use of it!

A small quantity of the purest niobic acid had been mixed with a larger proportion than usual of the sugar which by ignition furnished the charcoal. The mixture happened to be heated very gradually in the current of chlorine, and the niobic acid gave, instead of the white chlorid of niobium, the purest yellow chlorid of pelopium, unmixed with a trace of white.

The writer was that morning in the laboratory; he knew nothing of what was going on, more than that the apparently never ending experiments with the tantalic acids were being continued, when the remark was made, "I have the idea that niobium and pelopium are but one and the same substance;" and repeated experiments soon enabled Rose to produce, from either acid, either chlorid at will.

It might be supposed that the two acids and chlorids are isomeric modifications of the same matter; but the yellow chlorid of pelopium was found to contain more chlorine than the white chlorid, and consequently the pelopic acid is a higher stage of oxydation of the niobic. Pelopium having disappeared as a separate element, the names of the acids were changed to the hyponiobic and niobic acid; so that what was before the pelopic, becomes now the niobic acid.

The hypochlorid of niobium, once formed, cannot be changed into the chlorid by the further action of chlorine; but as the chlorids by contact with water give rise to the corresponding acids, and either chlorid could be produced at will from the acid, here was a means of converting the one acid into the other. And this is the only way in which the hyponiobic can be oxydized to the niobic acid; the most powerful oxydizing agents are incapable in any known way of effecting this change. And what is still more remarkable, the acids retain their peculiarities, their individuality, when submitted to the different blowpipe tests.

From either the hypofluorid or the fluorid, metallic niobium can be obtained by the action of potassium; and the metal is the same from whichever it may have been derived. But when the metallic niobium is again oxydized, the product is always hyponiobic, and never the niobic acid. So that here again is a means by which, through the metal, the niobic may be changed into the hyponiobic acid; but not the contrary.

By the action of bisulphid of carbon, or of sulphuretted hydrogen, the two acids are readily converted into sulphids; and they may also be made to combine with nitrogen. But when these compounds are reconverted by direct oxydation into acid, the acid is always the same as that from which the sulphid or the nitruret has been formed. In this way no conversion of them is possible. Of this, Rose says "it is a phenomenon which has no analogy in chemistry; for the two acids behave in this respect not like the oxyds of one and the same metal, but of two different metals. The tendency to the formation of the one or the other acid must have existed in the metal of the sulphid previous to its oxydation. The subject had now touched upon ground which is as yet hidden from us by an impenetrable veil."

This is the point of interest in the history of these bodies; "this it is," says Rose, "which has induced me to devote so much time and labor to their investigation."

Throughout his life Rose was a firm supporter of the atomic system introduced by his master, Berzelius, and maintained the propriety of retaining the double atom for the bodies of the hydrogen and phosphorus groups. In the course of his investigations of the minerals known under the name of tetrahedrite, he became impressed with the necessity of halving the atomic weight of silver. Only on the supposition that the sulphurets of silver and of copper are isomorphous, and capable of replacing each other in indefinite proportions, could he explain the composition of these ores, or assign to them a rational formula. His later analyses of the mineral first recognized by his brother, Prof. Gustav Rose, and called by him polybasite, and especially of stromeyerite, which under the crystalline form of sulphuret of copper is a mixture of the sulphurets of silver and copper in various proportions, confirmed him in the correctness of his view. In 1857, he reviewed these facts in a carefully written paper upon the Atomic Weight of the Elementary Bodies. He shewed, in the first place, that the assumption of the double atom, and consequent halving of the atomic weight of H, Cl, I, Br and N, and of P, As, Sb and Bi, was in accordance with the chemical nature of their compounds, as well as with their isomorphous relations, their volume as permanent gases, and their specific heat. He then proposed to extend the division of the atomic weight to silver and the three metals of the alkaline group. If the isomorphism between the sulphurets of silver and copper require the atomic weight of the former to be halved, the isomorphous relations of the oxyds of silver and sodium make the same change necessary in regard to the latter. As an argument for altering the formula of the alkalies, Rose brought forward the fact that there is no known instance, at least in artificial products, of the isomorphous substitution of an alkali in place of a compound of equal atoms of metal and oxygen. An objection to the proposed change, however, is that the suboxyd of silver would then have the extraordinary composition of four atoms of metal to one of oxygen. This objection has weight, said Rose, so long as such a composition remains an isolated fact; but should other similarly constituted compounds be discovered, in the progress of science, it would necessarily lose its force.

The subject occupied Rose's thoughts, and led to many attempts to form compounds which would be admitted to contain four atoms of a metal combined with one of oxygen. In October, 1863, in the last paper which he wrote, he communicated the discovery of a lower stage of oxydation of copper, which has unquestionably this composition.

It was a fitting consummation of his labors that the view which he had advanced in his earlier works, and maintained throughout his life, should be so strikingly confirmed at its close.

Rose's reputation will, however, we think, rest mainly upon the prodigious impulse which his works gave to the study of analytical chemistry. To understand how this effect was produced, we must look at the condition of the science previous to his time.

Analytical chemistry had its rise in, and was first occupied with, the examination of natural products. Passing by the names of Wenzel, of Bergman, Scheele, and Kirwan, the earlier fathers of analysis, whose works have now only an historical interest, we find in Klaproth a man whose whole life was devoted to mineral analysis, and who connected, in point of time and by the accuracy of his results, the older chemists with the later school of which Berzelius was the head. He was, as has been mentioned, one of Rose's first masters. At this time, the close of the last, and the early part of the present century, Fourcroy and Vauquelin in France, and Stromeyer in Germany, were also especially engaged with the examination of minerals and other inorganic bodies. Lampadius, of Freiberg, now best known for his metallurgical essays, and as the discoverer of bisulphid of carbon, published in 1801 a Treatise upon Mineral Analysis, to which he added a Supplement in 1818. The works of Klaproth, however, were the most important in their bearing upon analytical science. He introduced the use of caustic alkalies in the decomposition of siliceous minerals, and was the first to draw attention to the necessity of properly drying or igniting the substances to be weighed. Klaproth's Essays were unsurpassed in their time, and retain their value as original works of experiment. Still, when we compare his analyses with those of Rose, which followed them after an interval of but a few years, we perceive an abrupt transition, a great change which the science had in that time made. To whom was this step due? Not to Rose, for his career was but just begun.

A short time after the death of Klaproth, a Text-book on Analytical Chemistry was published by Professor Pfaff of Kiel; it contained the results of the most recent investigations, and in arrangement and completeness far surpassed all previous compilations. And yet this work was so completely superseded by the treatise of Rose, published only eight years later, that it is now almost forgotten. The explanation is to be found in the circumstances under which the two treatises were compiled.

The work of Pfaff was the exponent of the analytical views of Stromeyer and Klaproth, and the other chemists of that school; the treatise of Rose contained the new and more precise ideas of analysis which the experience of Berzelius had developed. But Berzelius, though he has written a text-book upon general chemistry, which is a model for clearness and simplicity of style, has left no separate treatise upon analytical chemistry.

Rose, during the years spent at Stockholm, had ample opportunity of learning the new methods, and the accuracy in the operations of analysis which Berzelius introduced; he now supplied this want, and diffused the knowledge which had been hitherto confined to the immediate pupils of Berzelius. The processes of analysis had lost much of their tediousness and repulsiveness from the smaller quantities upon which they were effected; and the appropriate manipulations were described by Rose with so much clearness, that they were rendered practicable to all who had attained a moderate proficiency in the science. With the appearance of Rose's treatise, a new era was opened to analytical chemistry.

In the preface to the last French edition of 1859, he has himself given the best account of his great work.

"My first edition, published in 1829, was in one volume; it was intended principally for beginners. It contained the first attempt at a systematic plan of qualitative analysis. This plan has been retained, without essential change, in all the later editions. My book has, perhaps, been the means of introducing the study of analytical chemistry into Germany; and I have had the satisfaction of seeing the plan which I had laid down adopted in all subsequent treatises upon analytical chemistry.

The course to be pursued in qualitative analysis, should, it seems to me, be preceded by an account of the reactions of the separate bodies. This preliminary knowledge enables us to choose the reagents best suited to the substances which have been separated, or indicates in doubtful cases all the reactions by which their presence may be ascertained. This part of my work has been enlarged, in each new edition, by numerous experiments. The rarer substances have always more particularly attracted my attention; and I have given to them the more consideration, in proportion to the difficulties of their detection and recognition.

I am now arrived at an age at which self-deception is no longer possible; I am about to revise my work for the last time. I should, then, seek to make it as complete as possible. Impressed with this feeling, I engaged in the task with ardor. I collected all the facts with which the labors of chemists have enriched science in the last eight years; I submitted them to examination in the laboratory, and I added to them the results of the many researches which I have myself undertaken for the sake of this work. Lastly, every portion of the book has been carefully revised, and I do not hesitate to say that, after so much pains, it is become an entirely new work, rather than a new edition of my former Manual.

My work is now no longer intended only for beginners; I venture to hope that it may be consulted in doubtful cases even by experienced chemists."[*]

One circumstance is omitted in this simple statement: it is the complete success of his work. From its first appearance, and through all the alterations which it has undergone in successive editions, it has maintained its place as the standard authority upon analytical chemistry. Of the many works which have since been written upon the subject, not one has assumed to compete with it. As diffusing the knowledge acquired under Berzelius, as containing the results of his own researches, and his revision of those of other chemists, and, finally, as offering in a practical form a compendium of the whole science, Rose can claim a three-fold merit for his book. In his last year he was engaged in preparing an abridgment of his great work, for which a number of new experiments were making. About thirty sheets of it were printed at the time of his death. The modesty which led him to avoid mention of

[*] While we fully appreciate, from our own experience, the great utility of the general scheme of qualitative analysis devised by Rose, and feel how much the science is indebted to him for its introduction, we venture to suggest that the value of such a scheme consists, in the first place, in its teaching the beginner not to rely upon the action of individual tests, but methodically to separate each constituent previous to their application; and secondly, as furnishing a general idea of the mode in which *all* analysis ought to be performed. We would regard the scheme as a means of learning analysis rather than of practising it: and believe that a familiarity with quantitative analysis, which requires the student often to depart from the process laid down for qualitative examinations, and to recognize and think about the action which the constituents of a compound exercise upon each other, is the only means by which he can learn surely to detect them; and that qualitative analysis cannot be performed with certainty, so long asthe operator consents to follow general plans, or, still worse, systematically arranged tables.

himself, makes it difficult for one not familiar with his writings to judge how much of his work is derived from his own investigations. We would suggest, in justice to Rose, and as a means of tracing in his book the progress of the science, that should another edition be published, it be furnished with references to the original sources.

In an early paper on Iron as a Constituent of the Blood, Rose pointed out the effect of organic matter in preventing the precipitation of the peroxyd of iron and alumina by alkalies, and that the property is peculiar to such organic bodies as are decomposed by heat; while those which are volatilized by a high temperature do not, in general, interfere with the precipitation. He called attention to the error which may be introduced into analysis by the production of soluble organic matter from the action of nitric acid upon the paper of the filters; and showed how this property may be made available in such analytical operations as the separation of titanic acid from the oxyd of iron. The same idea had occurred to Berzelius, who used it in the separation of zircona; it was afterward proposed by Otto as a means of separating phosphoric acid from feeble bases, and is ascribed to him by Rose without the slightest reference to the previous application of the same principle by himself.

His examination of the acichlorid of chrome, or chlorochromic acid, suggested to Rose the means of solving a problem of great difficulty—the detection of minute quantities of chlorine in compounds containing bromine or iodine. The decomposition of the natural aluminates, and the use of the acid sulphate of potash in their analysis, were taught by him; he first showed that the silica which had been found in these minerals did not belong to them, but was derived from the mortar in which they had been pulverized.

In his analysis of the hypophosphite of cobalt, he discovered the property of the oxyd of this metal to combine with a further and indefinite quantity of oxygen by ignition; and he met the difficulty by reducing the oxyd by hydrogen, and determining its weight in the metallic state; a proceeding which has since been universally adopted.

We owe to Rose the use of chlorid of ammonium and of cyanid of potassium in the dry way in quantitative analysis, as also that of fluosilicic acid. The different behavior of strong and feeble bases toward carbonate of baryta, which he had investigated and shown to be one of their distinguishing characters, was also made use of to effect their quantitative separation. The sesquioxyds, in general, are precipitated; while the stronger bases which contain only a single atom of oxygen are not thrown down by carbonate of baryta at the ordinary temperature. To this, as to all general laws, there are exceptions, depending partly upon the peculiar nature of the metal of the base, partly upon the acid with which it is combined, and somewhat too upon the time during which the action is prolonged. His separation of cobalt and nickel was one of the applications of this principle.

The estimation of chlorine in liquids containing sulphuretted hydrogen was attended with a source of error, until Rose indicated the proper way of removing the latter by a salt of peroxyd of iron. If the sulphur be separated by combining it with a metal, whether by the use of a soluble or insoluble salt, or of the metal itself, the sulphuret always contains a portion of the chlorine; the nature of this compound Rose also described.

The separation of the boracic, fluohydric, and especially phosphoric acids, from the various bases, was most elaborately investigated, as was also the analysis of the sulphurets of the higher metals by the use of chlorine. We owe to him processes for the determination of tin, antimony and arsenic, and their separation from other metals by fusion with carbonate of soda and sulphur.

One of his latest analytical works, which, though contained in the French edition of his treatise, was published for the first time in German in the last year of his life, was the estimation of many of the more frequently occurring metals by ignition with sulphur in an atmosphere of hydrogen.

Rose's style is simple and clear; it is that of one who has a story to tell, and tells it in the plainest words. Its fault is diffuseness, arising from his ease of expression. It would seem that he did not give as much attention to the composition of his papers as to the execution of the research; his results were communicated from time to time as they were obtained, without always waiting until they could be methodically arranged. Hence, though his works show the thought which he gave to his subject, and the skill with which he conducted his experiments, they have not the finished character of the essays of Gay-Lussac.

The leading traits of his mind were love of truth and exactness, and the absence of vanity and affectation. Absorbed in his pursuits during the whole day, he yet did not consider himself a hard working man; for, as he once told us, "I do nothing in the evening: I only read the scientific journals." His industry increased instead of diminishing with his age. A year before his death, he said, "I have at most but a few years to live—and yet so much remains to be done!" In his last years he allowed himself scarcely an hour in the day for any other recreation than a long walk, which he took at dark throughout the year and in all weathers. The kindness of his feelings was shown in his intercourse with the young men whom he admitted into his laboratory; they were constantly invited to his house, and received into his family circle, or presented to the distinguished guests whom he had assembled. Many of our countrymen will recall with gratitude how Berlin was made to them like home by the cordial welcome of Rose.

In the beginning of the present year, the life that had accomplished so much, and which his friends hoped might yet long be spared, was suddenly brought to a close. But a week before his death, Rose was in his lecture-room. On the day of his death he asked for his proof-sheets, saying that he felt much better and should soon again be well. He died in the afternoon of the 27th of January, of an inflammation of the lungs, after an illness of only six days.[5]

Born in nearly the same year, his fellow-student in the laboratory of Berzelius, and Professor at the same University, Rose's death took place within a few months of that of Mitscherlich. Differing completely in their dispositions and in the nature of their minds, it is not surprising that their course in life and their success should have been unlike. They agreed only in this, that they both attained the highest eminence. It was Mitscherlich's fortune to make, in his twenty-fifth year, one of those great advances which open a new prospect in science. It was a step for which the age was waiting. It had been prepared by the labors of Leblanc, of Gay-Lussac, and of Beudant; it had almost been anticipated by the theory of Fuchs. There was wanting but the last idea for the genius of Mitscherlich to conceive. No such discovery fell to the lot of Rose. His works were eminently his own. Appropriating to himself a science, then in its infancy, but with rich materials for its development at his hand, Rose labored with untiring zeal, until he had built up its every part into a perfect and enduring monument of his industry and his skill. D.

[5] Of Professor Rose's immediate family, there remain his widow, and a granddaughter, the child of his only daughter who died a few years since, not long after her marriage with Prof. Karsten.

ART. XXXII.—*On the cellular structure of Actinophrys Eichornii;* by Professor H. JAMES CLARK.*

THE structure in the genus Actinophrys is particularly interesting, because of its manifesting a step higher than the simple homomorphous organization of Amœba, as described by Prof. Wyman. Prof. Clark, in his communication on this subject, referred to Kölliker's observations in 1849, as recorded in the *Zeitschrift für wissenschaftliche Zoölogie,* and showed that, even supposing Kölliker to be correct, the division of the mass of the body into an exterior and an interior portion, the former containing much larger vacuoles than the latter, indicated a heteromorphous organization, and tended toward specialization of parts. He also added that he could not agree with Kölliker, that Actinophrys is a homomorphous mass with vacuoles, but that he was convinced that the so-called vacuoles of the outer and inner layers are true cells, with a distinct wall about them; a wall that could be easily recognized with the help of the better sort of microscope-objectives of the present day. Owing to the exceeding transparency of the organism, no ordinary objective will show the walls; but, with a one-quarter inch lens, of one hundred and fifty degrees angular aperture, made for him last June, by Tolles, of Canastota, N. Y., he had no difficulty in working, with the proper adjustment and corrections, through a sufficient depth of water to completely cover the Actinophrys (A. Eichornii), and could readily detect the walls, not only of the superficial cells, but also of the innermost ones.[1]

What is remarkable, too, the *pseudopodia*, as frequent and careful observations have led him to determine, invariably alter-

* From the Proceedings Boston Soc. Nat. Hist., 1863, p. 281.

[1] The unprecedented working distance, which accompanies the great angle of aperture in the above-mentioned lens, prompts me to speak more fully of its excellence. It has been the chief desideratum of naturalists to obtain a large increase in the working distance of those lenses which have a great angle of aperture; but, hitherto, the latter condition has seemed to involve necessarily an excessively short working distance, and consequently great inconvenience in the investigation of all bodies which are not correspondingly thin. The idea of studying marine animals in their native element, with such lenses, could never be indulged in, for fear of ruining the objectives, by contact with salt water. At last we are relieved from this restraint, for within the last four or five years a great improvement has been made in this respect by opticians, at least by Mr. Tolles. The most recently constructed lens which I have received from that gentleman was made last June; it is a one-quarter inch objective, with an angular aperture of *one hundred and fifty degrees*, and a most unexpected working distance of *one fiftieth of an inch* for uncovered bodies. By experiment, I also find that it works through a glass covering, fully *one-fortieth of an inch thick*, and with some room to spare above that. The working distance through water I have not measured accurately; but that can be inferred from the difference between its refraction and that of glass. The defining power of this lens is certainly unsurpassed, if not unequalled.—H. J. C.

nate with the cells of the exterior layer; that is, they are *prolongations of the intercellular amorphous substance* of the body. This fact would seem to add to the proof that the so-called vacuoles are really cells; otherwise it would be hardly credible that simple vacuoles, which come and go in an amorphous substance, should always alternate with the pseudopodia.

Sometimes a pseudopod moves very rapidly, especially when it has seized upon some victim, for then it retracts with a sudden jerk, and draws the prey close to the body, which finally engulfs it in the same manner as does Amœba. The pseudopodia exhibit an adhesive power, which is remarkable when we consider the size of the animals which are sometimes drawn in by them, and, in this respect, remind one of the "adhesive vesicles" in the anchors of Lucernariæ, which hold fast to bodies with the greatest tenacity, and, to all appearance, by simple contact, just as glue and mucus adhere to anything which touches them.[2] In a Difflugia (very near *D. proteiformis*) Professor Clark had observed that, whenever the pseudopodia contract, they invariably become strongly wrinkled transversely; and, as he could not detect the least trace of an envelope, or wall-like layer, on this part of the body, he believed that the wrinkling is peculiar to the substance of the pseudopodia.

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## Art. XXXIII.—*On the Origin of the Prairies of the Valley of the Mississippi;* by Professor Alexander Winchell.[1]

The diversity of opinions in existence regarding the cause or causes of the absence of trees from the prairies of the valley of the Mississippi is, of itself, sufficient proof that no satisfactory theory of this phenomenon has as yet been advanced. In the mind of the writer, a conviction has for some time been growing up, that we may discover the origin of the prairies in the last great geological revolution of the globe. The boldness of some of the suggestions about to be offered, ought not to prevent the presentation of them to the judgment of the scientific world.

In discussing the origin of the prairies, it is to be borne in mind that there are two facts to be accounted for—1st. The physical peculiarities of the soil and subsoil of the prairies—2d. The absence of trees from these areas, in cases where no obvious

[2] See my paper on "Lucernaria, the Cœnotype of Acalephæ." *Proc. Bost. Soc. Nat. Hist.*, ix, p. 52, 1862; and also reprinted "with additions and notes," in this Journal, [2], xxxv, 346.

[1] The original views presented in the following paper were first shadowed forth in an article in the *Ladies' Repository* for May, 1863. The theory was more fully elaborated in a paper read before the Illinois Natural History Society, at Springfield, in June, 1863. As this paper has not as yet been published, I embrace the opportunity of presenting a recast of my views in the present form.

cause exists. The first fact is brought into consideration under the first of the following propositions; the other is discussed under the propositions which follow the first.

1. *The soil of the Prairies is a Lacustrine Formation.*

Some of the older writers on the prairies, confining their attention to the so-called "wet prairies," so common in Ohio and Michigan—now usually termed "marshes," "swales" and "bogs"—found little difficulty in discovering the true origin of this class of prairies, and in proving that the humidity and sourness of the soil were the real causes of the absence of ordinary upland trees from their surface. Other writers, whose observations were made upon the dry and rolling prairies of Illinois, saw no immediate evidence of the aqueous origin of the soil, and knew no cause but the annual burning of the grass, for the remarkable absence of arboreal vegetation. It is this class of prairies to which the present discussion applies. They are conceived to have their origin in more general causes than the marshes and swales before mentioned. The latter have not had a simultaneous origin, and the causes which have brought them into existence have been local and limited in their influence. Being produced by the filling of ancient lakes, one has become a prairie at one epoch, another at another; and the work of filling lakes and forming wet prairies of this class is still in progress. For these reasons, a distinction should be carefully made between the wide, rolling prairies of Illinois and contiguous states, and the local swales of that or other states.

The lacustrine origin of the prairie soil is shown, *first*, by its physical characters. Not only has it the fineness, color and vegetable constitution which characterizes such soil, but we actually discover in it abundant remains of lacustrine shells, disseminated hundreds of miles from the present limits of the lakes. If, among older formations, we are permitted to infer the origin of the sediments from the nature of the included organisms, the evidence from testaceous remains is not less conclusive as to the nature of the prairie sediments.

The lacustrine origin of the soil is shown, *secondly*, by the necessary effect of geological changes of level which are generally admitted to have taken place. From the head of lake Michigan, all the way around the lakes to Niagara river, exist the well known evidences of a former higher level of the waters.[2] Even the increased elevation depending on the position of the falls of Niagara at Queenston—that is to say, the level of lake Erie at the time when the falls began to excavate their great

[2] Hall, *Geol. Rep. 4th Dist. New York*, pp. 348, 383; Lyell, *Travels in N. A., 1st Visit*, i, 29, and ii, 85; Desor, *Foster and Whitney's Rep. L. Sup.*, i, 204, 212, ii, 248, 253; Hubbard, *Mich. Geol. Rep.*, 1840, p. 102; Whittlesey, this Journal, [2], x, 31; Logan, *Geology of Canada*, p. 910, &c.

gorge—setting back through the chain of the lakes, would cause a rise in lake Michigan, above its present level, of 25 feet. This small elevation of lake Michigan would probably open an outlet toward the Illinois river. But it is highly probable that the escarpment at Queenston, by extending further north, attained, in consequence, a somewhat higher elevation, at the epoch under consideration. It could hardly be presumed, however, that this was the barrier which dammed the waters of the lakes to the much higher level, of which we have equally the indisputable records. We need but refer to the well known proofs of aqueous erosion along the shores of the lakes, extending from their present levels to the altitude of 200 and 300 feet. Mark them in the escarpments of the south shore of lake Erie; in the lake ridges of Ohio and Michigan;[3] in the caverns and arches and purgatories of Mackinac island[4]—especially in the side of "Sugar Loaf," whose base is now inland and elevated 150 feet above the surface of the water. Whatever may have been the barrier which dammed the waters to these heights, the evidences of their former presence are incontestable. But the moment we grant this ancient level to the waters, they inevitably escape from us toward the south, through the valleys of the Illinois and Mississippi rivers.[5] Turning our attention in this direction we find corroboration of the suggestion. The broad and deep, bluff-lined valley of the Illinois was never excavated by the present inconsiderable river. The deserted river valley discoverable at intervals further north, indicates the former southward flow of a large volume of water. At Lamont, this valley is distinct, with its bounding bluffs and its "pot holes" worn in the solid rock of the ancient river bed. But with the waters of lake Michigan standing one or two hundred feet above their present level, how much of the region south and west of Chicago must have been submerged? The ancient lake must have reached its arms into Iowa, northern Indiana and southwestern Michigan. These, the writer is convinced, were the relative levels of the land and water

[3] We are aware that Col. Whittlesey has attributed the higher ridges to a submarine origin, and that Sir Charles Lyell has advanced the same opinion in reference to the ridges of lake Ontario. In regard to the latter, it will be remembered that lake Ontario is 330 feet lower than lake Erie, and may easily be surrounded by ridges of marine origin, whose level is entirely below the ridges of lake Erie. Further, in reference to the latter, it will be remembered that they have often been found to enclose lacustrine shells. To say the least, even if we do not insist upon the lacustrine origin of the higher ridges, the lower ones, which blend with the terraces of late formation, establish a former altitude of the lakes which is quite sufficient for our present purpose.

[4] Foster and Whitney, *Rep. L. Sup.*, ii, pp. 164–6; Winchell, *Mich. Geol. Rep.*, p. 128.

[5] If the earlier portion of the gorge of the Niagara was undergoing excavation while a large portion of the waters of the lakes was being drained through the valley of the Illinois river, the force and rate of erosion must have been materially diminished below the present standard by the diminution of the volume of water.

for a considerable period immediately following the last great submergence of the continent. This conviction was first reached in the study of the prairies of Alabama, in the years 1851–2 and 3. Shells of *Unio*, *Melania*, &c., are here incorporated with the soil, as in Illinois, but in much greater abundance; and the ancient water-line can be distinctly traced around the bases of the knolls of white limestone, which rise like chalk islands from the bosom of a dark and heaving sea. The aqueous origin of the Alabama prairies was announced by R. W. Withers,[6] and W. W. McGuire,[7] but they both adopted the evidence of *marine* fossils, so abundant in the soil, as proof of the former presence of the sea; and were not at all aware that the submergence of which they saw the proofs had nothing to do with the formation of the prairie soil.[8]

The aqueous origin of the soil of the northwestern prairies was intimated by George Jones in 1836,[9] who compares the prairies and barrens of Illinois to the marshes, dykes and sand flats of Holland. Lesquereux, in 1856,[10] ascribed the general formation of prairies to water, and in 1861[11] reaffirmed his position in reference to the prairies of the Mississippi valley. Prof. J. D. Whitney has distinctly asserted a lacustrine origin for the prairies of the northwest,[12] and Dr. J. S. Newberry[13] has recognized the evidences of a former efflux of the lake waters over the Kankakee ridge in northern Illinois. The indications, indeed, seem to be sufficiently patent to induce the general assent of living geologists to the doctrine of the lacustrine origin of the soil of the prairies.

2. *Lacustrine sediments inclose but few living germs.*

Of the seeds which find their way to a body of fresh water, one portion—embracing the seeds of the grasses and sedges—will float upon the surface, and eventually lodge upon the lee shore. Another portion—embracing the fruits of most arboreal vegetation—will sink to the bottom, and undergo a speedy decomposition. Whenever a lake or a pond has been drained, the bottom remains a naked waste till the germs of vegetation have been gradually introduced *ab extra*. The gradual encroachment of vegetation upon the ancient domain of a lake during the period of its *gradual* drainage or gradual filling up, depends, of course, upon a supply of germs from the main land.

[6] This Journal, xxiv, 187. [7] Ib., xxvi, 93.

[8] So far as the writer is aware, he was the first to assert the lacustrine origin of the Alabama prairies and to maintain it—even in opposition to views then held by Prof. Tuomey.

[9] This Journal, xxxiii, p. 225.

[10] *Bullet. Soc. Nat. Sci. Neuchatel.* [11] See 2d *Geol. Rep. Ark.*

[12] Hall's *Geol. of Iowa*, i, p. 25.

[13] *Proc. Bost. Soc. Nat. Hist.*, vol. ix, May, 1862.

3. *Diluvial deposits, on the contrary, are found everywhere replete with living germs.*

Many of the facts upon which this proposition rests, are matters of common observation, but the broad conclusion does not seem to have impressed itself upon our attention. Nothing is a more common observation than to see plants making their appearance in situations where the same species was previously unknown, or for a long time unknown and under circumstances such that the supposition of a recent distribution of seeds is quite precluded. The following are some of the circumstances under which the sudden appearance of unwonted species occurs.

1st. When a change is produced in the physical condition of the soil. Left to nature, certain perennial grasses secure almost exclusive foothold in our fields, and form a sod in which the ordinary annuals are unable to flourish. Break up the sod, after any number of years, and subdue the perennial grasses, and we shall have a crop of annuals the first season—Veronicas, Chenopodiums, Euphorbias, Portulacas, Ambrosias, Crab-grasses, Fox-tails, Panicums, &c., &c. Cease cultivation, and the Poas and Glycerias will immediately resume possession. Similarly, the pertinacity with which the common Knot-grass (*Polygonum aviculare*) seizes and maintains its position only along the hardest beaten foot-paths is notorious; while the greater Plantain (*Plantago major*) renders itself no less conspicuous growing alongside. Earth thrown out of cellars and wells is generally known to send up a ready crop of weeds, and not unfrequently of species previously unknown in that spot. In all these cases, after allowing for all known possibilities of the distribution of seeds by winds, birds and waters, it still seems probable that germs must have previously existed in the soil.

2d. When a change is produced in the chemical nature of the soil. Illustrations are familiar to every agriculturist. How soon does a dressing of undecomposed muck or peat develop a crop of acid-loving sorrel—and how readily it is again repressed by a dressing of some alkaline manure. Let the waters of a brine well saturate a meadow, and how long before we witness the appearance of *Scirpus maritima*, *Triglochin maritimum* or some other salt-loving plant, whose germs, unless spontaneously developed, must have lain dormant in the soil at a greater or less depth.

3d. The disappearance of dominant species. It is well known that the clearing of a piece of forest and the burning of the brush is almost always followed by the appearance of certain unwonted plants known as "fire-weeds." In many cases it would seem highly improbable that the seeds of such plants have just been transported to such situations, at the moment when the disappearing forest admits the introduction of the conditions essential to their growth. It can hardly be doubted that the germs existed in the soil, ready to germinate whenever free sunlight, warmth and atmospheric air should be permitted to rouse their latent vital energy. Of the same nature is the recurrence of particular forest growths upon the same soil. Not unfrequently the second growth is of a very different nature from the first. In the "old fields" of Virginia and other southern states, the soil, cleared originally of deciduous trees, and then abandoned, after years of continuous cropping, sends up a growth of pines instead of deciduous trees. Many similar examples will suggest themselves to the mind of the reader.

4. *The living germs of the diluvial deposits were buried during the glacial epoch.*

Whence come the germs of that vegetation which is everywhere springing up in situations to which recent seeds could not have been distributed? This question has agitated the mind of many an inquirer who would have shrunk from the proposition which we here venture to enunciate. Let us examine the facts.

(1.) The vegetation which characterized the close of the Tertiary epoch was probably nearly identical with that existing at the present day under the same climatic conditions. Even in the older Tertiary Lignites, we have, according to the investigation of Lesquereux and Newberry, the remains of plants belonging to the following American genera, viz: *Quercus*, *Carya*, *Populus*, *Acer*, *Morus*, *Carpinus*, *Negundo*, *Laurus*, *Persea*, *Cornus*, *Rhus*, *Olea*, *Rhamnus*, *Magnolia*, *Smilax*, *Thuja*, *Sequoia*, *Taxodium* and *Sabal*—identifications made from scanty and defective material, and we may fairly presume that further investigations will greatly increase the number. Yet these plants, probably older than the Claiborne sands, show, according to Lesquereux, "the greatest affinity with species of our own time." From other beds of the middle or earlier Tertiary, we have still other existing genera, such as *Diospyros*, *Fagus*, *Nyssa*, *Aristolochia*, &c. The facts in our possession relative to the vegetation of the middle and later Tertiary epochs, show a most decided approximation to the existing Flora. From a pleiocene deposit near Somerville, Tennessee, Lesquereux identified the following recent species, viz: *Laurus Carolinensis*, *Prunus Caroliniana*, *Quercus myrtifolia*, *Fagus ferruginea*.[14] From the chalky banks of the Mississippi river, near Columbus, Kentucky, a collection was made of which all the species are recent, viz: *Quercus virens*, *Castanea nana?*, *Ulmus alata?*, *Planera Gmelini*, *Prinos integrifolia*, *Ceanothus Americanus?*, *Carya olivæformis*, *Gleditschia triacanthos*, *Acorus calamus*.[15] It is true that Dr. D. D. Owen has assigned the deposit containing these remains to the Quaternary

[14] This Journal, [2], xxvii, 363. [15] This Journal, [2], xxvii, 364.

period;[16] but as their position is 120 feet below the ferruginous sands containing *Megalonyx Jeffersoni*, and as the nature of these species is incompatible with such a climate as we universally associate with the glacial epoch, it is quite likely this assemblage of vegetable remains represents the general nature of the arboreal flora in existence near the close of the Tertiary period.

Although our positive knowledge of the vegetation of the period immediately preceding the advent of the reign of ice is confessedly meagre, it is certain that all the facts in our possession point to close specific correspondence with the modern vegetation of the same regions—modified certainly by the fact that, even in the latest Tertiary, the climate was considerably warmer than in the same latitudes at the present day.

(2.) The general effect of the events which ushered in and marked the progress of the reign of ice was, to destroy the vegegation flourishing over all the northern portion of the continent and mingle its forms with the cubic miles of *debris* detached from the underlying rocks. We find the trunks and limbs of trees buried 50 and 100 feet deep in this diluvial rubbish. It is impossible that myriads of vegetable germs should not also have been stored away. The drift deposits became the vast granery in which nature preserved her store of seeds through the long rigors of a geological winter.

(3.) But what evidences have we that the seeds of plants are capable of retaining their vitality through a geological period?

(a.) The ordinary process of destruction of vegetable tissues is merely an oxydation of the carbon and hydrogen entering into their constitution. It is seriously doubted whether the requisite conditions for such oxydation exist at considerable depths in the soil. It is stated that the piles sustaining the London bridge have been driven 500 years, and are still comparatively sound. Old Savoy Place, in the city of London, is sustained on piles driven 650 years ago, and they are yet perfectly sound. One of the piles taken up from the bridge built by the emperor Trajan across the Danube, was found petrified to the depth of three-quarters of an inch, while the remainder of the substance was unchanged after an interval of 1,600 years. The buried tree trunks already alluded to must have lain since the time of the last great geological revolution. Nor are these rare cases, for the encroachments of the waves upon the shores of the great lakes reveal whole forests of the buried trunks of the White Cedar, (*Thuja occidentalis*), bearing scarcely a trace of the work of destructive agencies upon them. Indeed it is known that well preserved woody tissue has been frequently exhumed from deposits of Tertiary, and even of greater age. The writer has pieces of drift-wood from the Cretaceous sands of Alabama, in

[16] *Kentucky Rep.*, vol. i, p. 22.

which the ligneous tissue is so fully preserved as to be capable of ignition, like recent wood. Even from the coal measures of Michigan the writer has made preparations of the delicate tissue of Jungermannia-like fronds; and from the coal mines of Lasalle in Illinois, he has specimens of exogenous wood of a brown color and not yet carbonized, though partially pyritized. All these examples tend to show the extreme slowness of the process of decay in ordinary vegetable tissues when excluded from the usual conditions of decay by burial in the earth.

(b.) The oily tissues of which seeds are composed are still more capable of resisting the tendency to dissolution; and ought certainly to remain unchanged, under circumstances which permit such perfect preservation of ordinary ligneous fibre. The evidences are very conclusive, that the seeds of ordinary vegetation may lie dormant in the surface soil for half a dozen or a dozen years. The seeds of *Erechthites* and other "fire-weeds" must have reposed in a latent state during the existence of the forest, whose disappearance is the signal for the resumption of their vital activity. The same is true of the seeds of the "old field pines," which have probably lain for an age or more, awaiting the maturity and destruction of the deciduous forest which usurped the soil. How *many* ages may they have lain there? How many more might they have lain and still been found ready for the first opportunity to seize a foothold?

There are some facts in our possession still more specific. It is well known that Dr. Lindley raised three raspberry plants from seeds discovered in the stomach of a man whose skeleton was found thirty feet below the surface of the earth, at the bottom of a barrow, or burial mound, which was opened near Dorchester, England. With the body had been buried some coins of the emperor Hadrian, from which we are justified in assuming that these seeds had retained their vitality for the space of 1,600 or 1,700 years. If they remained undamaged that length of time, their condition was practically fixed; and who shall say that 10,000 years would have produced a greater effect? Again, Lord Lindsay states that in the course of his wanderings amid the pyramids of Egypt, he stumbled on a mummy, proved by its hieroglyphics to be at least 2,000 years of age. On examining the mummy, after it was unwrapped, he found in one of its closed hands, a bulb which, when planted in a suitable situation, grew and bloomed in a beautiful dahlia. The credibility of this story may be questioned, as the real dahlia is a tuberous-rooted, Mexican genus, not known to botanists till the year 1789. That a bulb of some sort germinated under the circumstances alledged is not wholly incredible. It is further asserted, and generally believed, that wheat is now growing in England, which was derived from grains folded in the wrappings of Egyptian mummies, where they must have lain for two or three thousand years. Prof. Gray does not fully credit the account, but Dr. Carpenter, the eminent physiologist, gives it his full endorsement. Dr. Carpenter even goes so far as to give utterance to the following observations, which happen to be extremely pertinent to our present argument.

"These facts make it evident," he says, "that there is really no limit to the duration of this condition, [latent vitality], and that when a seed has been preserved for ten years, it may be for a hundred, a thousand or ten thousand, provided that no change of circumstances either exposes it to decay or calls its vital properties into activity. Hence, where seeds have been buried deep in the earth, not by human agency, but by some geological change, it is impossible to say how long anteriorly to the creation of man they may have been produced and buried, as in the following curious instance: Some well-diggers in a town on the Penobscot river, in the state of Maine, about 40 miles from the sea, came, at the depth of about 20 feet, upon a stratum of sand. This strongly excited their curiosity and interest, from the circumstance that no similar sand was to be found anywhere in the neighborhood, and that none like it was nearer than the sea-beach. As it was drawn up from the well it was placed in a pile by itself, an unwillingness having been felt to mix it with the stones and gravel which were also drawn up. But when the work was about to be finished, and the pile of stones and gravel to be removed, it was necessary also to remove the sand heap. This, therefore, was scattered about the spot on which it had been formed, and was for some time scarcely remembered. In a year or two, however, it was perceived that a number of small trees had sprung from the ground over which the heap of sand had been strewn. These trees became, in their turn, objects of strong interest, and care was taken that no injury should come to them. At length it was ascertained that they were Beach-plum trees, and they actually bore the Beach-plum, which had never before been seen except immediately upon the sea-shore. The trees had therefore sprung from seeds which were in the stratum of sea-sand that had been pierced by the well-diggers."[17] It cannot be doubted, as Carpenter concludes, that the seeds of the Beech-plum had lain buried since the remote period when that part of the state was the shore of the slowly receding sea.

Such a fact, so striking and so circumstantially recorded, is only of the same nature as others less critically noted, which daily pass before our eyes, in the upspringing of vegetable forms from the diluvial materials thrown out of wells, cellars and other excavations.

[17] Carpenter's *Elements of Physiology*, Am. ed., p. 41.

It must be confessed that the crucial observation is yet to be made. If vegetable germs exist in the drift, they can be discovered beforehand. The writer is not aware that any thorough search has ever been made for them; but until they have been actually detected, it is probable that even the convincing facts cited above will fail to secure universal assent to our proposition involving the prolonged vitality of the seeds of preglacial vegetation. While, however, the case is far from demonstrated, it may fairly be submitted that the explanation of certain facts, afforded by our theory, is less presumptuous and improbable than the supposition of spontaneous generation, the fortuitous distribution of seeds by any modern agency, or any other explanation which can be reasonably offered.[18]

5. *In proportion as the diluvial surface became exposed, the Flora of the preglacial epoch was reproduced.*

As the continent slowly rose from its last sea-burial, every portion of its surface, inch by inch, passed under the action of the ocean's surges. Even if the vegetable germs inclosed in the more superficial portions of the drift deposit had yielded to the destructive agencies of a geological period, the action of the sea would have uncovered and brought to light some of the more deeply seated and better protected seeds. If, then, our reasonings are correct, returning spring time vivified into activity the myriads of germs stored away by Nature from before the reign of ice; and the continent was again clothed with those forms of verdure which had adorned it at the close of the Tertiary period. But at this moment in the world's history, the retreating waters paused to brood over the wide region destined to become the garden of the west; perpetual dilution converted them into a vast inland sea of fresh water, upon whose bottom gathered the lifeless sediments that were to be the soil of the prairies. Then, when, in the progress of events, either through the removal of barriers, or the further upheaval of the land, the fresh waters were poured from the wide prairie region, there remained a naked and lifeless expanse of vegetable slime. From the bosom of the slime no plant could start, for the germ was not there. From beneath the load of slime, in the diluvial deposits below, no plant could raise its head, for it was sealed hermetically from air and light and warmth. A shining coat of

[18] With reference to the effect of sea water on the vitality of seeds during the epoch of submergence of the continent, we have not overlooked Darwin's experiments recorded in the London *Gardeners' Chronicle* for May 26th, 1855. While the experiments show a wonderful power of resisting the destructive influence of sea water, it is still apparent that the conditions of the experiments were such as to throw no light on the fate of seeds buried deeply in a submarine sand bed. It will be remembered further, that the filtration of sea water through a mass of sand, deprives it of its saltness, so that this agency in the destruction of vegetable germs embraced in a submarine soil becomes to a great extent eliminated. Compare Cabot, *Proceedings Bos. Soc. Nat. Hist.*, vol. iii, pp. 92, 103.

verdure clothed everywhere the more ancient surface of the drift; and here and there in the abandoned lake bottom, rose a knoll crowned with its emerald crest—an island perhaps in the former lake. Thus the prairies were at first a naked and herbless waste.

6. *The vegetation which finally appeared on the drained lacustrine areas was extra-limital, and was more likely to be herbaceous than arboreal.*

The natural agencies in the introduction of vegetation from beyond the limits of the prairie region would be winds, running water and animals—especially granivorous birds. In a region so nearly level, the agency of running water would be but feebly exerted. Winds would exert a more important influence in the dispersion of the lighter, and especially the feathered seeds; but granivorous birds, it is believed, would exert a still more important influence. Yet it will be noticed that none of these agencies, and especially the two more important ones, would effect the distribution of any except the smaller and lighter seeds. Numerous quadrupeds, it is true, engage in the transportation of nuts and acorns, but no suitable storage place for such fruits would be found upon the prairies, not to mention the fact that they are transported and stored for consumption rather than for seed. It can hardly be doubted that the humble forms of vegetation producing the lighter seeds would be the first to secure possession of the soil. Sedges and marsh-loving grasses, especially, would eagerly occupy the ground, until the chances of germination of any of the larger fruits, would become exceedingly diminished. Thus the prairie became covered with herbaceous vegetation exclusively, while all around the margins was arrayed a shining fringe of forest trees, and every island knoll stood crowned with its cluster of oaks. Around the borders of the prairie were the ancient sand dunes, blown up while yet the prairie was a lake bottom. A peculiar vegetation would suit itself to so purely arenaceous a soil; and an occasional tree would be able to plant itself along the belt thus destined to become the "barrens."

Thus the prairies were treeless because the grasses first gained foothold and then maintained it. The Indian, perhaps, made his appearance at this time, and formed an alliance with the grasses in their contest against the trees; and thus decided the question in favor of the grasses.

This is our theory of the origin of the prairies, and the absence of trees from their surface. Fatal objections may rest against it, but it is certain that all other theories are untenable.

1. The old and popular belief that the treelessness of the prairies was caused by the annual burning of the grasses by the Indians,[20] is now generally admitted to be inadequate.

2. The supposition that trees have been choked out by the tangled roots of cane, which in turn has disappeared under the influence of a burning sun,[21] has no applicability in a region visited annually by frosts too severe for cane to survive.

3. The supposition that the absence of trees is due to too great dryness of the soil during the summer, is disproved by the fact that trees flourish naturally in drier soils in the same vicinity, while, on being introduced, they flourish equally well in the prairie. The treeless and almost herbless deserts of the far west may have originated in extreme aridity of the atmosphere[22]—as others have from the highly saline character of the soil—but all our discussions have had reference to the prairies of the Mississippi valley.

4. A theory often urged is the considerable humidity of the soil of certain prairies,[23] and especially the wetness of the subsoil in contrast with the dryness of the soil during summer.[24] 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, the cottonwood, the beach, the ash, the alder, the cypress, the tupelo, the water-oak, the tamarack, the American arbor-vitæ or some other tree—some of them standing joyously half the year, if need be, in stagnant water. It is well known that swales are generally devoid of trees; but the reason for this is to be found in the fact that since a soil assumed the place of the ancient lake, the germs of trees have never been introduced; while the introduction of such germs is delayed by the circumstance that neighboring forests are generally such as are adapted to drier situations. Has it been found that a green willow or poplar twig will not root and thrive in a wet prairie? But further than this, large portions of the treeless prairies are *not wet.* Is there a different cause for treelessness here?

5. Prof. J. D. Whitney[25] has advanced the opinion that the extreme fineness of the prairie soil is the cause of the absence of trees; and the author of the article on "Plains," in the New American Cyclopedia, seems to have adopted this view. Against this theory we see several weighty objections. Many alluvial soils, as pulverulent as that of the prairies, are densely

[20] This Journal, vol. i, p. 331. [21] This Journal, vol. xxiii, p. 40.

[22] Does Prof. Dana allude to the prairies of the Mississippi valley when he says, (*Manual of Geol.*, p. 46), "and where the moisture is not sufficient for forests, she [America] has her great prairies and pampas?" See also Cooper, *Smithson. Rep.*, 1858, p. 276; Newberry, *Ohio Agric. Rep.*, 1859; Lambert, *Pacific R. R. Rep.*, vol. i, p. 166.

[23] Atwater, this *Journal*, i, 116; Bourne, *Ib.*, ii, 30; Lesquereux, 2*d Ark. Geol. Rep.*; *Western Monthly Magazine*, Feb., 1836

[24] Engelmann, this *Journal*, [2], xxxvi, 384. [25] *Iowa Geol. Rep.*, vol. i, p. 24.

wooded, and that in the same latitudes and under the same meteorological conditions. Again, partial or complete destitution of trees is observed on the coarser, sandy borders of the prairies, and on all recent sand dunes, even where no lack of vegetable sustenance exists. But the fatal objection to this theory, and all theories which look to the physical or chemical condition of the soil, or even to climatic peculiarities, for an explanation of the treeless character of the prairies, is discovered in the fact that *trees will grow* on them when once introduced—not water-loving trees exclusively, but evergreens, deciduous forest trees, and fruit trees—such as flourish in all the arable and habitable portions of our country. Everybody now knows that trees flourish well on the prairies; and the prairie farmers are actively engaged in their introduction.[26] It seems to us that this fact alone militates fatally against the views advanced by Whitney as well as those of Engelmann, Bourne, Atwater and others, who have attributed the distinctive character of the northwestern prairies to an excessive humidity of the soil.

University of Michigan, Aug. 30, 1864.

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

THE following paper on the nebular hypothesis differs from any other with which I am acquainted in several particulars, the principal of which are the arrangement of the parts treated, and the mathematical discussions introduced. It treats the theory of the subject first, and then the phenomena of Nature are compared with the theoretical conclusions.

The only apology which I have to offer for going over so much of the subject is that I attempted to prepare some articles on detached portions of the hypothesis, and found the explanations, which would be necessary to render the subject intelligible to those who had not made the matter a study, so many that the space demanded would be nearly equal to that occupied by the present paper.

That there is a growing interest in the subject, is known to every astronomer that has paid any attention to what has been written. It is a great problem yet to be completely resolved. The complete analytical treatment of the nebular hypothesis presents many difficulties; and without such aid it seems impossible to ascend to the origin of the solar system. I now submit what follows to the candid judgment of my readers.

[26] Compare Wells, this Journal, i, 331, where the forest is said to be encroaching on the prairies about St. Louis; Engelmann, Ibid., [2], xxxvi, 389; Edwards, *Rep. Dep. of Agric.*, 1862, p. 495.

## THE THEORY.

1. Geology has revealed the fact that it took immense ages of time to form the earth, and fit it for the habitation of man. The same science also points, somewhat definitely, to a time when the earth was in a highly heated condition. Mathematical science, applied to the problem of the earth's conformation, teaches us that the earth has that form—the asperities of its surface not considered—which it ought to have if it were in a fluid state when it assumed its present form. These facts—to which we might add the condition of Saturn's rings—seem to teach that the earth, and in short the whole *Solar System*, were once in an aëriform state. An additional argument in favor of this view, is derived from the physical constitution of *Comets*.

2. With these preliminary facts and considerations, perhaps we may be justified, in treating of a hypothesis—in assuming that the Solar System was once but a single body, and in a gaseous condition; and that this single solar body was but a detached portion of a still larger and more extensive mass, that being in a still more primitive state. If we ask what is the cause that made the materials from which worlds have since been made assume that primitive gaseous condition, we must look for our answer to the operations of caloric. Philosophers, in their investigations, have arrived at this general conclusion respecting the operation of heat, namely, that mechanical action develops it, and the greater the action the greater the heat; and that as soon as heat becomes sensible, it tends to change the condition of bodies. This, then, reduces the cause of the primitive gaseous state of the stellar and planetary worlds to mechanical action.[1] As the mechanical action becomes less and less, the operations of heat become less and less potent.

### I. *Of Stellar Systems.*

3. Let us assume that there once existed an immense and widely extended nebulous mass, from which solar and planetary bodies were afterward developed. It is extremely doubtful that the astronomer will ever obtain, with the telescope, any information respecting the existence of any such nebulous mass, even if such a body now exists within the range of telescopic vision, as I have attempted to show in a former paper in this Journal.[2]

4. We may now ask what was the probable nature of this supposed nebulous body? Was it homogeneous in structure, or was it heterogeneous? Let us try to determine this point. But first, what was the form of this great nebulous mass? Here

[1] Chemical action should not be left out of the consideration, but at present we cannot define its operations so well.

[2] Vol. xxxvii, [2], p. 210.

again we are reduced to conjecture. We cannot determine, with any approximation to truth, what the original form was. We may suppose it to have had no definite form, according to our conceptions, but rather to have been an amorphous mass, like the clouds of vapor which float in our atmosphere. If we could suppose this great nebulous mass to have been symmetrical in form and homogeneous in structure, in the process of cooling and condensation there would be little probability of its generating a rotatory motion on an axis; and under such conditions the matter would condense into a sphere. But we have not the remotest evidence—nay, all our evidence is to the contrary—that the original nebulous mass was homogeneous. It is a very difficult thing to find even a small amount of matter perfectly homogeneous. We must, therefore, conclude that the probability against the homogeneity of the original gaseous mass, is many millions to one in favor of it. Even if it were originally perfectly homogeneous, but of irregular shape, the attraction of gravitation, and the other forces of nature which acted upon it, would soon[3] cause it to become heterogeneous in its constitution.

5. Again, if we suppose the original fluid mass to have been symmetrical in form (at least one of some particular forms), and arranged in strata that were homogeneous in themselves, the process of cooling and condensing would not generate a rotatory motion, and the mass would ultimately become a sphere. But the chances against such an arrangement of the nebulous mass would be equally as great as against the condition previously described. These are the only cases in which it seems possible for the mass to condense without generating a rotatory motion.

6. The science of mechanics teaches us that if a fluid mass be not so constituted as to be arranged in strata, homogeneous in themselves—the strata being of different densities or otherwise—it will not be in equilibrium;[4] and in this case the attraction of gravitation, radiation of heat, and whatever other active and modifying causes might operate upon the nebulous body, would cause the fluid mass to change its parts so as to seek a condition of equilibrium;[5] and such a change would generate a motion of rotation about one or more axes. It is highly probable—caused by the form of the body, and the heterogeneous nature

[3] In saying that such a result would be *soon* reached, it must not be supposed that we mean "soon" according to our ideas of time. A million of years might be "soon" for such a change to take place. In speaking of such changes as are described in the text, we must extend our ideas of periods of time.

[4] Courtenay's Mechanics, pp. 355–6; and Airy's Mathematical Tracts, pp. 163–6; and other works on the subject.

[5] It must not be supposed that because the conditions of equilibrium are not satisfied that there will be a "breaking up" and a separation into distinct masses, but only a change in the distribution of the material composing the original fluid mass, so that it may ultimately attain the conditions of equilibrium. This change will generate a motion of rotation.

of the materials composing it, and perhaps other things—that the fluid would cool unequally in different parts, and in such places it would condense unequally, and thus, if they did not already exist, different centres of attraction would be produced. Around these different centres, matter would accumulate and condense, and these nuclei, so formed, would revolve around their common centre of gravity. As soon as a rotatory motion had commenced, centrifugal forces would begin to act; and as the process of cooling continued, the attraction of gravitation would have a greater control, (for the tendency of heat is to expand all bodies, and thus to operate against the attraction of cohesion, and also of gravitation in the case which we are considering,) and thus the mass would be condensed, and the rotatory motion thereby increased. But an increase of rotatory velocity would also increase the action of the centrifugal force, and this process would continue until in some parts the centrifugal force might equal the force of gravity, and a separation would take place; and in this way each nucleus might ultimately be separated from all the others, when each would pursue its own course around the common centre of gravity of the whole.

7. Each nucleus would itself be in a condition very similar to that which at first existed in the original great fluid mass. The same laws and forces acting on each nucleal mass, would generate a motion of rotation, if the previous separation had not already given it an initial rotatory velocity,[*] and this motion would generate a centrifugal force, as before, and each of these masses would separate into parts. This process of separation would continue until such a result could no longer ensue. During all this time, it must be recollected that the original nucleal parts would continue their revolutions around their common centre of gravity, while the parts into which each original nucleus was divided would continue *their* revolutions around *their* common centre of gravity. In this way each division, or system, would continue its own independent revolution, and at the same time it would be carried around the common centre of gravity of the greater system of which it formed a part; just as the satellites revolve around their primaries, while the primaries are at the same time carried around the sun, and the sun around some more distant centre. It is in this way that we would account for the existence of *clusters* of stars.

8. Notwithstanding the probability that the detached parts would have a motion of rotation impressed upon them, yet we cannot so easily infer that this motion would be in the same direction

[*] It is difficult to see how one of these supposed nucleal masses could be detached from the parent mass through the influence of a rotatory motion, without impressing on the detached part, at the time of separation, a motion of rotation. From the fact that the whole system is yet supposed to be fluid, such a result of the separation would seem to be still more probable than if it were solid.

as that of the original mass.[7] Yet, if the primary masses *should* give the satellite portions, in the act of separation, a tendency in the same direction as themselves, every system would probably revolve in the same direction. As these systems, or nebulous masses, became reduced in size, from the number of separations, they would ultimately reach a limit, after which no such separations as we have supposed would take place, and the body would thus be reduced to that size and condition only fitted to form a *solar system.* In a few cases compared with the whole, the final separation which we have supposed might be into nearly equal parts. Where there were but two centres of condensation, which separated as we have just supposed, a *binary* star would be produced;[8] and where there were three, a *ternary* system would result; and where there were more nuclei so related to each other, *multiple* stars, or systems, would be produced.

9. According to the view of the sidereal heavens which we have just explained, we conclude that the suns of the universe should be found to exist in clusters, and that these clusters are but parts of a *cluster* of clusters, and so on, each forming, as it were, an island universe. All the suns that make up these clusters must be in motion,[9] each pursuing its own individual course, while, at the same time, it will be carried onward in its greater orbit around the centre of gravity of the cluster of which it forms a part, and so on to the great centre of the whole. So far as observation as yet enables us to judge, it is concluded that the stars *are* arranged in clusters, but how nearly the grander conception, that the clusters are themselves arranged into systems, is true, we cannot at present decide.

10. As each sun forms the centre of a planetary system (in all probability), there must have been as many centres of condensation as there are suns, or fixed (?) stars as in common language we call them.

11. Now, although we have been unable to tell just how a motion of rotation commenced, yet we have shown what conditions must be fulfilled in order for the matter to condense into a sphere without generating a rotatory motion; and as those con-

[7] If several centres of condensation existed, as we have supposed, and any one of them should separate from the others, it would seem to be more likely that the detached portion would receive an impulse that would cause it to rotate in the opposite direction, just as two wheels turning on their axes, and revolving in contact, run in opposite directions. The direction of the motion of binary stars might enlighten us on this point.

[8] See this Journal, vol. xxxvii, 1864, p. 233, where Prof. Kirkwood attempts to account for the great eccentricity of the orbits of double stars, as compared with those of the planets.

[9] Whether the suns of the universe have been formed by any such process as I have described in the text or not, since they exist, the principles of Mechanics teach us that they *must* be in motion, and as the telescope detects separate clusters, the same principles teach us that the clusters must be in motion. These motions are probably such as to produce *dynamical equilibrium.*

ditions are almost impossible,—perhaps quite so in nature,—we see that a motion of rotation would result from the cooling down of the nebulous mass, the attraction of gravitation, and other forces of nature which might operate.

12. The preceding general considerations may be regarded as applying with more force to the development of the sidereal systems, such as the Milky Way, and other starry clusters, than to the formation of the solar system, or to any single system of a sun and the planets which revolve around it. We have now given a sufficiently lucid exposition of the development of the starry systems—the island universes—for our present purpose.[10] We shall now descend from these general considerations in reference to the structure of the universe, to a view of the principles which governed the formation of the solar system, according to the nebular hypothesis, and thus to see how far we are able to go in accounting for the general structure of that system of which we more immediately form a part. It is mainly in this part of our subject that we must look for that proof of the truth of the nebular hypothesis which will be in any great degree satisfactory.

II. *Of the Solar System.*

13. Heretofore we have not supposed the nebulous mass to have had any definite form; but we have now arrived at a point in our considerations where it is necessary to suppose the fluid body to have taken some definite form. Even if the larger parts of the original nebulous mass broke up before they took that form—necessarily symmetrical—which the conditions of equilibrium require, we have evidence that the smaller division, the single nucleus from which the solar system was developed, had so far changed the distribution of its materials, that it approximated to a symmetrical form. Mathematical investigation informs us what that form may have been.

14. A fluid mass which does not rotate on an axis must ultimately become spherical in form, whatever be the law of attraction.[11] But if it have a rotatory motion, it can never become a sphere, although it may approximate to one in form. A fluid body which rotates on an axis will be swelled out at the equator; that is, the particles of matter will be thrown from the axis of rotation, and there will consequently be a depression about the poles. Ultimately, however, the mass must assume a condi-

[10] For a more complete discussion of that part of our subject which relates to the formation of the starry clusters, see a very able treatise by Stephen Alexander, LL.D., Professor of Natural Philosophy and Astronomy in the College of New Jersey, published in Gould's Astronomical Journal, vol. ii, and entitled, *On the Origin of the Forms and the Present Condition of some of the Clusters of Stars and several of the Nebulæ.* It occupies 29 pages 4to.

[11] Courtenay's Mechanics, p. 355.

tion of equilibrium. The conditions of equilibrium are these: the fluid matter must be arranged in strata the density of which throughout must be the same. These strata may differ from each other in density, or the density of each one may be the same—that is, the body may be homogeneous. Also the resultant of all the forces acting on any one of these strata, must be directed towards the interior of the mass, and be normal to all the strata.[12]

15. In the case of a homogeneous spheroid there are, with a slow rotation, two forms of equilibrium, one of which is an oblate spheroid of *small* ellipticity, and the other is an oblate spheroid of *great* ellipticity. In the case where the density, equatorial gravity, and period of rotation, are the same as in the case of the earth, the axes will, in the first form, be as 230 to 231; and in the second, as 1 to 681.[13] If by any means the angular velocity of rotation becomes accelerated, the two forms of equilibrium will approach each other; and when they unite the time of rotation will be reduced to $2^h\ 25^m\ 26^s$.[14] If the period of rotation becomes still less, the fluid mass can no longer hold together; in other words, the equilibrium will no longer exist.

16. It has never been proved, so far as I am aware, that when the body is heterogeneous, there are more than one form of equilibrium. If the rotatory motion be slow, the demonstrated form is one of small ellipticity.[15] But if it never has been proved that there are more forms of equilibrium of a revolving heterogeneous mass, than one, it is, perhaps, nearly certain that, at least, *two* forms exist for the same period of rotation. Is it more reasonable to assume, as has been done, that the very oblate form is a little less oblate than the corresponding form of a homogeneous spheroid? Since the form of small ellipticity is *less* oblate in a heterogeneous spheroid than the corresponding form of a homogeneous one, does it not seem probable that the very oblate form of the former, is still more so than the similar one of the latter?

17. We shall now assume that the primitive rotating solar mass was, at least, mostly an aëriform fluid body, and that it approximated to a very oblate spheroid. Before proceeding

[12] In other words, the *potential* of the fluid mass, when expressed in terms of any arc drawn on the surface of any one of the strata, must be a constant for that stratum. Thus, let V be the potential expressed in terms of $s$, any arc drawn on the surface of a stratum, then, because a particle on this surface has no tendency to move in any direction, we must have $D_s V = 0 =$ the force acting on the particle. Integrating and $V = B =$ constant.

[13] Airy's Tracts, 4th ed., p. 148.

[14] Ib., p. 149.

[15] Ib., pp. 150–175; Pratt's Fig. of the Earth, pp. 63–79; Math. Monthly, vol. iii, pp. 166–182. The problem of the figure of a heterogeneous earth is one of the most difficult in physical astronomy, and it has been solved only for the case of small ellipticity.

further with our theory, however, it will be advisable to enquire into the nature of the constitution of the primitive spheroid. For that purpose let us suppose the spheroid already separated into rings by the processes of Nature. That being done we have the means of arriving, approximately, at the principal radii of gyration of the primitive spheroid at the time the several planetary rings were abandoned. To do this we shall proceed as follows:—

The principle of the Conservation of Areas,[16] teaches us that if a body rotate on an axis, every point of which has the same angular velocity, the angular velocity multiplied by the moment of inertia with respect to the axis of rotation, will give a product that is constant for the same body. Let

$M =$ the mass of the primitive spheroid;
$k =$ principal radius of gyration;
$T =$ time of rotation;
$A =$ a constant;
$\pi = 3{\cdot}14159$, &c.

Then

$$2\pi M k^2 = AT. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

So long as M remains the same, A will, also; but if M vary, so will A. Hence, as soon as the solar spheroid has thrown off a ring, the spheroidal mass, M, will be changed, and therefore A will be no longer the same quantity as before the separation. But, according to Dr. Galle, the mass of the sun is 738 times as great as the mass of all the other bodies of the system together.[17] We hence see if we entirely neglect the mass of all the planets in comparison with the mass of the sun, we shall neglect only such a quantity as would be between the second and third orders in the lunar theory. By making such a supposition, our results will still be approximately correct. We can hence call M and A constants. Let T and $k$ apply to any one of the planets; that is, T, its period of revolution, being the same as that of the ring at the time the ring was abandoned by the solar spheroid; and $k$ the principal radius of gyration of the solar spheroid at the same time. Now let $T_0$ represent the time of rotation of the sun, and $k_0$ his principal radius of gyration; $T_1$ and $k_1$, similar quantities when the solar spheroid extended to Mercury's orbit; $T_2$ and $k_2$, for Venus; &c. Equation (1) will give

$$\frac{k^2}{k_0^2}=\frac{T}{T_0}, \qquad \text{or} \qquad k=k_0\sqrt{\frac{T}{T_0}} \quad . \quad . \quad . \quad . \quad (2)$$

Let the mean distance of the earth from the sun be 1, and the mean distance of any other planet be $a$; then by Kepler's third law we have

[16] The reader will find this *principle* demonstrated in treatises on analytic mechanics.
[17] Cosmos, vol. iv, p. 362.

$$1^3 : a^3 :: T_3^2 : T_1^2, \quad \text{or} \quad T = T_3 a^{\frac{3}{2}}. \quad . \quad . \quad . \quad . \quad (3)$$

This value of T in (2) gives

$$k = k_0 \sqrt{\frac{T_3}{T_0}} \cdot a^{\frac{3}{4}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

18. To find the value of $k_0$, we may consider the ratio of the radius of gyration of the sun to his radius the same as the ratio of the similar quantities in the earth, without material error. If we take, for the present, $a$ for the equatorial radius of the earth, and $\varepsilon$ for the ellipticity of the surface; then if the earth be homogeneous, the moment of inertia with respect to the axis of rotation will be

$$\frac{8}{15}\pi\varrho a^5(1-\varepsilon),$$

in which $\varrho$ is the density; and if the earth be heterogeneous, the moment of inertia will be

$$Mk^2 = \frac{8}{15}\pi\int_0^a \varrho' \, D_{a'}[a'^5(1-\varepsilon')]da'; \quad . \quad . \quad . \quad . \quad (5)$$

in which $\varrho'$, $a'$ and $\varepsilon'$ apply to *any* stratum below the surface. Since the earth is a solid of equilibrium, we easily find the value of the integral of the second member of (5), by the methods pursued in works on the Figure of the Earth.[18] In that way we find

$$k^2 = 0{\cdot}3232a^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

The time of rotation of the sun is approximately $25^d{\cdot}34$, and his radius, 441,000 miles. The time of revolution of the earth around the sun is $365^d{\cdot}256$. With these numbers and the coefficient of $a^2$ in (6), we easily find the logarithm of the coëfficient of $a^{\frac{3}{4}}$ in (4) to be 8·000845, 10 being added to render the index positive. By enclosing the logarithm in brackets, (4) becomes

$$k = [8{\cdot}000845]\, a^{\frac{3}{4}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

By means of (7) we can easily compute the value of $k$ for each of the planets.

19. Let $a_1$ represent the mean distance of Mercury from the sun, $a_2$ that of Venus, and so on to $a_9$ for Neptune, $a_5$ being the mean distance of all the asteroids; then we have

[18] See Pratt's Figure of the Earth, pp. 72–74. It would have been sufficient for our purpose to consider the sun homogeneous, but for the fact that I afterwards employ equation (5) in a calculation to find the law of density in the solar spheroid.

TABLE I.

| | | | |
|---|---|---|---|
| $\log a_1 = 9{\cdot}587822$, | $\frac{3}{4} \log a_1 +$ | $8{\cdot}000845$ | $= 7{\cdot}691710 = \log k_1$ |
| " $a_2 = 9{\cdot}859338$, | " $a_2 +$ | " | $= 7{\cdot}895347 =$ " $k_2$ |
| " $a_3 = 0{\cdot}000000$, | " $a_3 +$ | " | $= 8{\cdot}000845 =$ " $k_3$ |
| " $a_4 = 0{\cdot}182897$, | " $a_4 +$ | " | $= 8{\cdot}138017 =$ " $k_4$ |
| " $a_5 = 0{\cdot}486951$, | " $a_5 +$ | " | $= 8{\cdot}366059 =$ " $k_5$ |
| " $a_6 = 0{\cdot}716237$, | " $a_6 +$ | " | $= 8{\cdot}538022 =$ " $k_6$ |
| " $a_7 = 0{\cdot}979496$, | " $a_7 +$ | " | $= 8{\cdot}735467 =$ " $k_7$ |
| " $a_8 = 1{\cdot}282929$, | " $a_8 +$ | " | $= 8{\cdot}963041 =$ " $k_8$ |
| " $a_9 = 1{\cdot}477654$, | " $a_9 +$ | " | $= 9{\cdot}109084 =$ " $k_9$ |

The logarithms in the last column of numbers in the above table, give for the corresponding numbers those in Table II.

TABLE II.

| | | |
|---|---|---|
| $k_1 = 0{\cdot}004917 =$ | 468,900 | miles. |
| $k_2 = 0{\cdot}007859 =$ | 749,300 | " |
| $k_3 = 0{\cdot}010019 =$ | 955,500 | " |
| $k_4 = 0{\cdot}013741 =$ | 1,311,000 | " |
| $k_5 = 0{\cdot}023231 =$ | 2,216,000 | " |
| $k_6 = 0{\cdot}034516 =$ | 3,292,000 | " |
| $k_7 = 0{\cdot}054383 =$ | 5,186,000 | " |
| $k_8 = 0{\cdot}091842 =$ | 8,759,000 | " |
| $k_9 = 0{\cdot}128554 =$ | 12,260,000 | " |

20. The above values of the principal radii of gyration of the primitive solar spheroid, at the time it abandoned the several rings from which the planets were formed, show that even when the Neptunian ring was abandoned, the solar spheroid was very much condensed about the centre; and that probably more than half the mass was within the orbit of the earth as it now exists, and the major part of half of it was within the present orbit of Mercury.

21. We may obtain a rough approximation to the law of density of the primitive solar spheroid, as follows:—Let A represent the semi-equatorial-diameter of the spheroid at the time any one of the planetary rings was about to be abandoned (A being the semi-equatorial-diameter of the spheroid before the Neptunian ring was separated, and no discontinuity in the spheroid being considered; or in other words, the spheroid is supposed to be continuous, and still the centrifugal force is considered as equal to the force of gravity in all parts—a very improbable supposition, I admit;) and $a$ the mean distance of any one of the rings from the centre; then for the spheroid *within* the outer diameter of the ring $a$, equation (7) will give for the moment of inertia, calling B the coëfficient of $a^{\frac{3}{2}}$,

$$\mathrm{M}k^2 = \mathrm{MB}a^{\frac{3}{2}};$$

and that of the ring, or shell between $a$ and A, will be

$$\mathrm{M}'k'^2 - \mathrm{M}k^2 = \mathrm{B}[\mathrm{M}'\mathrm{A}^{\frac{3}{2}} - \mathrm{M}a^{\frac{3}{2}}] \quad . \; . \; . \; . \; . \; . \quad (8)$$

But equation (5) gives

$$M'k'^2 - Mk^2 = \frac{8}{15}\pi\int_a^A \varrho\, D^a[a^5(1-\varepsilon)]da. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

From (8) and (9) we have

$$B[M'A^{\frac{3}{2}} - Ma^{\frac{3}{2}}] = \frac{8}{15}\pi\int_a^A \varrho\, D_a[a^5(1-\varepsilon)]da. \quad . \quad . \quad . \quad (10)$$

Differentiating (10), A being a constant, we have

$$\frac{3}{2}BMa^{\frac{1}{2}} = \frac{8}{15}\pi\varrho D_a[a^5(1-\varepsilon)]. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

We do not know the relation between $a$ and $\varepsilon$, and we might assume any probable relation that would cause $\varepsilon$ to become 0 at the centre; but it will be sufficient for our purpose to suppose $\varepsilon$ constant. This supposition will cause (11) to assume the form

$$\varrho = Ha^{-\frac{7}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$ [19]

From this equation, we see that the spheroid increased in density very rapidly near the centre. If the density at the mean distance of the earth from the sun be called unity, then $\varrho = a = H = 1$; and the density at the mean distance of the several planets will be as given in the following table.

TABLE III.

| Name. | Density. |
|---|---|
| Mercury, | 27·10000000 |
| Venus, | 3·10700000 |
| Earth, | 1·00000000 |
| Mars, | 0·23440000 |
| Asteroids, | 0·01976000 |
| Jupiter, | 0·00311300 |
| Saturn, | 0·00037310 |
| Uranus, | 0·00003234 |
| Neptune, | 0·00001485 |

The numbers in the above table may be taken to represent the density about the outer equatorial parts of the primitive solar spheroid at the time when each of the planetary rings was abandoned from the equatorial parts of the spheroid. The above numbers, however, must be considered as only a very rough approximation to the true density.

22. If we suppose the angular motion of all parts of the solar spheroid to be the same ($\omega$ suppose) at any given time, the principle of the conservation of areas gives

$$\omega Mk^2 = A = \text{constant, or } \omega^2 Mk^2 = \omega A. \quad . \quad . \quad . \quad (13)$$

The principle of the *vis viva* gives

$$\Sigma(mv^2) = \omega^2\Sigma(my^2) = \omega^2 Mk^2 = W \text{ suppose}; \quad . \quad . \quad . \quad (14)$$

[19] It is not pretended that the investigation in the text is, in all parts, legitimate; but I have introduced it as affording, perhaps, a better result than mere *guess work*. The complete solution is one of considerable difficulty.

in which $y$ is the distance of any particle from the axis of rotation. From (13) and (14) we have

$$W = A\omega. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$ [20]

This equation shows us that the living force of the body varies with the angular velocity; and as the angular velocity increases with the loss of heat, we see that as the solar spheroid gradually cools down, the living force of the mass gradually increases. Or in other words, as the body loses its heat, the attraction of gravitation has a greater influence and increases the living force of the body.

Even if the angular velocity of all the parts is not the same, we may arrive at an approximate result, provided the different strata differ but little in angular velocity from the mean. Let $\omega$ represent the mean angular velocity of the body, then the true angular velocity may be represented by $\omega+\omega'$, $\omega+\omega''$, &c., in which we shall suppose $\omega'$, $\omega''$, $\omega'''$, &c., so small that we may neglect their squares in comparison with $\omega^2$. We hence have

$$\Sigma(mv^2)=\Sigma my^2(\omega+\omega')^2=\Sigma(m\omega^2+2m\omega\omega')y^2=\omega^2 Mk^2+2\omega\Sigma(m\omega'y^2)=W$$

and $$\Sigma m(\omega+\omega')y^2=\omega Mk^2+\Sigma(m\omega'y^2)=A.$$

Therefore

$$W=2A\omega-\omega^2 Mk^2=\omega[2A-\omega Mk^2]. \quad . \quad . \quad . \quad . \quad (16)$$

From this equation we also see that W increases with $\omega$. We may also have

$$W=A\omega+\omega\Sigma(m\,a'y^2).$$

Either of these expressions for W will approximate to $A\omega$ as $\omega'$, &c., diminish without limit. From this we conclude that as soon as a rotation has commenced in a nebulous mass, its living force will increase as the mass cools.

23. The primitive solar spheroid could have only approximated to a symmetrical form, and to a symmetrical disposition of its materials, especially in the outer parts. If it were so constituted, it is difficult to see how it could separate into rings of much width; but the materials being somewhat heterogeneously distributed, a ring of considerable width might be thrown off, or rather abandoned.

24. The *invariable plane* of the solar system must be the invariable plane of the primitive solar spheroid, and that must

[20] If different strata have different angular velocities, A will no longer be a constant. To find an expression for the angular velocity in this case, of any one stratum, let W and A be functions of $r$, such that $\delta r$ is the thickness of the stratum whose angular velocity is $\omega$. The *vis viva* of the part immediately within the stratum of thickness $\delta r$ will be $W-D_r W\delta r$, and the sum of the areas will be $A-D_r A\delta r$. We hence have $W-D_r W\delta r=\omega A-\omega D_r A\delta r$; or by (15) $D_r W=\omega D_r A$, $\omega=\frac{D_r W}{D_r A}=D_A W$. This result is obtained by supposing the spheroid within the stratum $\delta r$ to have the same angular velocity in all its parts; and that it ultimately is the same as that of the stratum $\delta r$.

have coincided approximately with the plane of the equator. The first planetary ring abandoned would have an inclination to the plane of the ecliptic nearly the same as that of the principal plane; and thus the outermost planet of the solar system should move in an orbit whose inclination is nearly the same as that of the principal plane of the solar system. Grant[21] has given 1° 35′ 31″ for the inclination of the principal plane, for the year 1750. It is very difficult to find the exact inclination of the invariable plane, since it is partly dependent on the moment of inertia of the sun and planets, and that, at present, can only be determined approximately. By making as exact a determination as possible, M. Lespiault[22] has found the inclination of the invariable plane to be 1° 41′. The inclination of the orbit of Neptune is 1° 46′ 59″. The correspondence of these two numbers is rather remarkable. A nearer agreement could not be expected, when we reflect that the inclination of the planes of the planetary orbits to the invariable plane, is constantly varying from planetary perturbations, and has been for immense ages.[23]

25. In the same manner as that in which the first ring was abandoned by the primitive solar spheroid, in its process of cooling and contracting and consequent increase of rotatory velocity, other rings would successively be detached, and this process would continue until the solar body would become so far condensed, and the cooling process would go on so slowly, that more planetary rings could not be separated. It has been suggested by Prof. Daniel Kirkwood, that several rings might be separated at the same, or about the same time; and an investigation, which will be given in a *note*, seems to strengthen the opinion.[24]

[21] History of Phys. Astron., p. 101. [22] Smithsonian Report for 1861, p. 210.

[23] I will make this prediction, that if more planets beyond Neptune be discovered, the inclination of their orbits will differ but little from that of the *invariable plane.*

[24] See this Journal, [2], xxxviii, 5. The investigation referred to in the text, is as follows: The attraction of a homogeneous oblate spheroid on a particle within its mass is, parallel to the axes of $x$, $y$, and $z$,

$$2\pi x\rho\left\{\frac{\sqrt{1-e^2}}{e^3}\sin^{-1}e-\frac{1-e^2}{e^2}\right\}=2\pi\rho x\varphi e \text{ (suppose)},\ 2\pi\rho y\varphi e, \text{ and}$$

$$4\pi\rho z\left\{\frac{1}{e^2}-\frac{\sqrt{1-e^2}}{e^3}\sin^{-1}e\right\}=4\pi\rho z\psi e,$$

respectively. If $u$ be the distance of the attracted particle from the axis of rotation, $z$, then

$$u^2=x^2+y^2, \quad \text{and} \quad U=2\pi\rho u\varphi e$$

will be the attraction perpendicular to the axis of rotations. (See Todhunter's Analytical Statics, p. 265.) Call the centrifugal force $F=\omega^2 u$, $\omega=$ angular velocity of rotation. Call the ratio of F to U, R, then $R=\frac{F}{U}=\frac{\omega^2}{2\pi\rho\varphi e}$. From this we see that R is independent of $u$, the distance from the axis of rotation. Hence

26. The density of the external equatorial parts of the solar spheroid, before any ring was separated, must have been very small in comparison with the internal parts, as can be seen by Table III, Art. 21, which makes the ratio as 1 to 2,000,000. The consequence of this extreme rarity would be a difference in the angular velocity of rotation of the several strata situated at different distances from the axis of rotation. As the solar body, then, became smaller from a loss of heat, and its angular velocity thereby increased, the angular velocity of the external parts would not be much increased, except by friction. Such particles, or perhaps we should say the outermost equatorial strata, would tend to move under the influence of Kepler's third law. The result of this would be that the outer strata would not be separated at once, but as the solar spheroid quickened its rotatory velocity, the several outer strata, whose centrifugal force equaled the force of gravity of the internal spheroid, would hang together, each moving partly under the influence of the angular motion of the internal spheroid, and partly under the influence of Kepler's third law. Such strata would hang together until they had accumulated to such an amount, that, by their own attractive influence combined with the considerable difference of angular velocity of the internal and external parts, the whole of the strata whose centrifugal force equaled the force of gravity—and perhaps some more, in consequence of friction and cohesion—would be separated from the solar body. In consequence of the extreme rarity of the external parts of the solar spheroid about the equator, when the first ring was

if it were possible for R to become 1 for a point on the equator, it would be 1 for all points of the spheroid. If the revolving body be a homogeneous spheroid of equilibrium, of the mean density of the earth, R reaches the limit 0·853, as the spheroid quickens its rotatory velocity, beyond which it cannot increase and the spheroid remain in equilibrium. If it were possible for the spheroid to exist and R=1, it would spread out and be resolved into elementary rings, or rather strata. But as the spheroid ceases to exist before R=1, is it not probable that the whole spheroid would be resolved into a ring, similar to the case in Mr. Plateau's experiments? If the spheroid be not homogeneous, is it not possible for it to approximate to the conditions above supposed, and for several rings to be separated simultaneously, as suggested by Prof. Kirkwood? The conditions of equilibrium give us

$$(2\pi\varrho\varphi e-\omega^2)(xdx+ydy)+4\pi\varrho\psi e\,zdz=0, \quad \text{or}$$

$$\left(1-\frac{\omega^2}{2\pi\varrho\varphi e}\right)(xdx+ydy)+\frac{2\psi e}{\varphi e}zdz=(1-\mathrm{R})(xdx+ydy)+\frac{2\psi e}{\varphi e}zdz=0.$$

If R=1, then $\psi e=0=e^{\frac{1}{2}}-\frac{\sqrt{1-e^2}}{e^3}\sin^{-1}e$. Hence

$\frac{e}{\sqrt{1-e^2}}=\sin^{-1}e$. Put $e=\sin\lambda$, then $\lambda=\tan\lambda$. There are an infinite number of roots to this equation. Since every value of $e$ except $e=0$, is greater than that of the spheroid of swiftest rotation, we see that the equation $\psi e=0$ does not apply to a spheroid: but it indicates that there is a discontinuity.

abandoned, such ring would be of considerable width, greater, very probably, than that of any other ring. We are thus able to account, in a very satisfactory way, for Prof. Kirkwood's "spheres of attraction," in his beautiful "Analogy."

27. When the second planetary ring was separated, its density would be greater than that of the first, both in consequence of the continued condensation of the solar atmosphere—if we may so term it—and also that it naturally existed nearer the centre of the solar spheroid. In consequence of this greater mean density, the second ring would not be so wide as the first, since the friction and cohesion of the internal parts of the ring would be so much greater, that it would increase the centrifugal force to such an extent that the ring would necessarily separate.

28. For similar reasons, the width of the third ring would, in general, be less than that of the second; the fourth less than the third; and so on. In consequence of the attractive influence of the rings already separated, and also the probability that the centrifugal force of the inner parts of the abandoned ring would not equal the force of gravity, the third ring might equal in width the second, but we have not sufficient reason to suppose that this would frequently be the case.

29. In consequence of the extreme rarity of the external rings, it might easily happen that their several masses might be greater as the rings are nearer the center of the solar spheroid. But since the diameter of the ring is less the nearer the ring is to the centre of the spheroid, and the width of the ring less, as we have attempted to show in the preceding article, the increase of the masses of the rings, as they are nearer to the centre of the spheroid, would reach a limit. Although this reasoning is by no means conclusive, yet we see from it that the planet having the greatest mass might be found somewhere between the nearest and most distant planet. But in consequence of the want of a perfectly symmetrical distribution of the materials composing the primitive spheroid, we should not look for an obvious law regulating the distribution of the masses of the several planets thus formed. It is sufficient to find that the planets gradually increase in mass as they are situated farther from the sun, until we arrive at the greatest mass, and thence a gradual decrease.

30. If the materials composing the rings were so distributed as to cause a greater amount to be detached from one side of the plane of the equator of the spheroid, than from the other side, it would cause a change in the direction of the axis of rotation of the revolving body, and thus the second ring might be somewhat inclined to the first. For a similar reason the third might differ in inclination from the first and second; and so on

to the last.[25] But since the mass of the largest of the rings in our system is but a small fraction of the mass of the sun, the separation of any one ring would change the axis of rotation but little; and thus, as is really the case, we should expect to find the planets confined, in their motions, to a narrow zone in the heavens.

31. When the primitive solar spheroid was assuming approximate conditions of equilibrium, the more dense the material, the nearer to the centre of the body would it descend to seek its position of equilibrium. Is it not possible that the metals would in this way sink, leaving the outer planets with comparatively less of such materials than the inner ones? According to this view, the sun should possess comparatively more metals than any of the planets. Would not this great abundance of metals in the sun enable him to retain his heat for a greater length of time? and would he not be more intensely luminous in consequence?[26] That metals exist in the sun has recently been demonstrated experimentally by Messrs. Bunsen and Kirchoff.

32. The orbit of a planet, formed from a ring which was abandoned, as described above, must be very approximately circular. It might differ from a circle in any one or all of four ways: 1st, by the separation of the ring *before* the centrifugal force equaled the force of gravity; 2d, by the ring's remaining attached for some time *after* the centrifugal force equaled the force of gravity, and the revolution of the ring being influenced by the increased rotatory velocity of the central spheroid; 3d, by the modification of the force of attraction by the contraction of the central spheroid; and 4th, by the breaking up of the ring to form the planet. The third and fourth causes would, perhaps, be the most potent. Of course all such planets must move around the sun in the same direction.

33. The distance from the central body (and what is very essential to the planetary worlds, every system so formed *must* have remaining a central body of comparatively large dimensions,) of the planets thus formed, would, perhaps, follow some approximately simple law; but since the *real* law of the distances would probably be very complicated, owing to the want of a symmetrical arrangement of the materials composing the solar spheroid, it might depart in either extreme of distance (very far from the sun, or very near to him,) very considerably from the approximate law. Such, at least, seems probable.

[25] I am unable to see sufficient evidence of the synchronous formation of the planetary rings, as advocated by Prof. Kirkwood, and, I believe, by Prof. Alexander. The inclination of the planetary orbits to each other, and to the solar equator, I think furnish evidence against it. Prof. Hinrichs finds evidence of the successive formation of the rings.

[26] See Prof. Roscoe's lecture before the Royal Institution, Eng., May 6th, 1864, as copied into the Daily N. Y. Tribune of July 18th, 1864.

In the process of ring-making, and the breaking-up of the rings to form planets, the general principle of the conservation of areas would hold good, but the *vis viva* of the system would, as we have attempted to show, gradually increase, owing to the greater influence of the force of gravity in consequence of the loss of caloric, which acts as a repulsive force, from the system."[37]

[To be continued.]

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ART. XXXV.—*Note on a Colored Derivative of Naphthaline;* by M. CAREY LEA, Philadelphia.

IN the course of an examination of the compounds of naphthaline, the following observation was made, and, as at the present day, every colored reaction belonging to the products of coal-distillation is a matter of interest, I publish it.

While preparing some sub-chlorid of naphthaline $C_{20}H_8Cl_3$, by passing chlorine over naphthaline, I washed the crude product with ether, and separated the etherial liquid by filtration. By exposure to the atmosphere, the ether passed off; there remained a small quantity of a pale yellow transparent watery acid liquid, which separated itself from the denser and more colored portions. Placed by itself in a small capsule, it deposited after a time a bright blue film. The liquid was poured off from this film into another capsule, when it gradually deposited a further portion.

The quantity of this blue substance obtained was exceedingly small. It exhibited the following properties. It was insoluble in water, in alcohol, and in ether. Exposed to an ammoniacal atmosphere, it passed quickly to a full deep purple: vapor of chlorhydric acid restored the blue shade. Ammonia in the form of solution wholly destroyed the color, nor was it then restored by chlorhydric acid as when it had been rendered purple by ammoniacal vapor.

This reaction of a blue substance changed to purple by a small quantity of ammonia, and wholly decomposed by excess, is something quite new, and it is to be regretted that the substance is only obtainable in infinitesimal quantities as a by-product.

[37] Prof. Gustavus Hinrichs, of Iowa State University, is engaged, I believe, on an analytical investigation of the nature of the primitive spheroid and the separation of rings. We hope Prof. H. will soon complete his investigations, and publish his results.

## ART. XXXVI.—*On the study of the Electric Spark by the aid of Photography*; by Prof. OGDEN N. ROOD, of Columbia College.

### II. ON THE FORM OF THE FIGURE PRODUCED BY THE INDUCTION COIL.*

IN the March number of this Journal for 1862, I described a new method of studying the electric discharge by allowing it to fall directly on a sensitive photographic plate, where it produced latent images of certain forms, which, under the action of the developer, yielded very fine and sharp characteristic pictures. It was shown at that time that there was a very marked difference between the positive and negative figures, the former consisting essentially of one or more stars and rings in combination, while the latter was made up, for short discharges, of a collection of dots or minute circles, which, by the use of a greater length of discharge, became converted into two or more thick concentric rings. This difference, which could only be fully manifested by the use of a large number of wood-cuts, was always so great that it was possible at a glance to decide whether the impression had been produced by positive or negative electricity. It was also shown that while the positive electric brush is characterized by the production of one or more very minute rings arranged concentrically, the negative brush merely blackened the plate; further, that the negative figure is in general much larger than the positive, this being the reverse of what Riess has shown to be the case with the dust figures of Lichtenberg.

In my first paper, the term *positive* figure is applied to the one produced when the prime conductor charged with positive electricity is made the positive electrode, and the sensitive plate the negative electrode; a mode of speaking which is common enough, though it probably would be as well to speak only of the figures as produced on the positive or negative electrode. By the term positive figure, then, I have and shall indicate the one which is traced on the negative electrode by the discharge, and by negative figure, the one produced on the positive electrode.

The usual mode of speaking implies the idea that the positive electricity travels over to the sensitive plate (negative electrode), and there traces its peculiar figure, and, correspondingly, that the negative electricity travels over to the sensitive plate (positive electrode), and there acts to generate a figure of a different kind. Now the electric discharge is rendered possible by the presence of material particles of some kind between the electrodes; in the act of discharge these particles are heated till they become luminous; if they consist of air, the discharge through them can be traced as a blue or violet line, while at or nearly at the same

* For No. 1, see volume xxxiii, page 219, 1862.

instant of time, metallic particles are torn off from each electrode, as shown by Feddersen, and, in an ignited state, projected in the general direction of the other electrode. The source of the light, then, is twofold: 1st, from the heated gas, and 2d, from the finely divided heated particles of metal. It therefore becomes of interest to inquire by what particular portion of the luminous matter these photographic figures are traced.

Making the sensitive plate the negative electrode, for example, it might be supposed that the figure formed on that electrode was produced, 1st, by the ignited particles charged with negative electricity in their act of departure toward the positive electrode, and 2d, that it was partly due to the arrival of luminous metallic particles from the positive electrode, and 3d, that a portion of it was due to the line of heated gas which extends from one electrode to the other. That the figure is produced only by the 1st and 3d of these causes the following considerations will render probable. Feddersen found during the discharge of a Leyden jar that when a resistance was introduced into the circuit sufficient to reduce the discharge to a single one, viz: so that in one act the positive electricity flowed toward the negative electrode and the negative electricity toward the positive electrode, that metallic particles were torn off from each electrode, and projected merely in the general direction of the opposite electrode, being often thrown sidewise wide of the mark, or in other words, that very often the luminous finely divided metallic particles did not seem to bridge over the chasm between the electrodes, which were joined sometimes only by a fine line.

Fig. 1 is from two photographs obtained by him under such circumstances, enlarged two diameters.

1.

I have made some experiments on the spark from an electric machine, operating with very different apparatus, but still have obtained corresponding results. A glass plate, 4 inches square, was coated with collodion and sensitized as usual, then washed with water and flowed with a solution of tannin and dried. Its surface was of course sensitive to light, and to all appearance as poor a conductor of electricity as glass itself. Next, two strips of tin foil were pasted on a plain glass plate, so that the electric spark would pass from one to the other. The two plates were then firmly pressed together, the tin foil being next to the sensitive surface, and a spark from an electric machine discharged through the arrangement: the tin foil plate was then shifted half an inch, and a second spark discharged, &c. Afterward, the sensitive plate was developed with pyrogallic acid as usual, when it was found that pictures of the path of the spark had been traced. This path was very often not continuous from one electrode to the other, sometimes there were two or even three breaks in it, while it often happened that the discharge from the positive was not in line with that from the negative electrode. See figures 2 and 3, which are enlarged 7 and 5 diameters.

2.

3.

With a microscope magnifying 20 diameters I have observed similar interruptions in the electric spark when discharged freely in the air.

We are therefore led to conclude that it is probable that the photographic figures are traced mainly by the luminous particles just before their departure for the opposite electrode, and if any effect is to be ascribed to luminous particles coming from the opposite electrode, it is only that of a diffused and faint blackening of the plate. Luminous material particles are here spoken of because it is held, (as Plücker very properly insists,)[1] that electric light existent by itself is a fiction.

In all his experiments with the Leyden jar, Feddersen found that the discharge proceeded simultaneously at each electrode, positive electricity starting from the positive and traveling toward the negative electrode, while at the same exact instant of time negative electricity started for the positive electrode. This is also known to hold good with voltaic electricity, and there seems to be no reason why it should not also be true with the induction coil, so as to render applicable to the induction spark the results obtained with the Leyden jar or electric machine.

The following set of experiments on the induction spark was undertaken at the suggestion of Dr. W. Gibbs.

*Apparatus employed.*—A large induction coil by Ritchie was used, which was however set in action only by a feeble battery power, so that the extreme length of the spark, when the sensitive plate was the negative electrode, was only $1\frac{1}{10}$ inches. This was in order to make a comparison of the figures obtained with those previously secured in using frictional electricity. The plates were sensitized as described in my first communication on this subject, and the same form of apparatus for the projection of the spark was retained. In addition, a metallic point was sometimes substituted for the brass ball, and in some cases two insulated metallic points were fastened at varying distances above the sensitive plate, in such a manner that the positive current, after leaving the first point, reached the wet sensitive sur-

[1] Pogg. Ann., Bd. cxiii, 274.

face and traveled along it for about an inch and a half, and then passed from the sensitive surface through the air up to the second point. Under the first point a figure was generated corresponding to that which in my previous article I termed the positive figure, and under the second point a figure corresponding to that described as the negative. This is in accordance with the results previously obtained by placing the sensitive plate in connection with the prime conductor charged with positive or negative electricity, and drawing a spark from its surface. The advantage of this mode of experimenting is, that positive and negative figures are obtained on the same plate, side by side, and consequently are generated under like conditions. Finally, in all these experiments, as in those before published, a portion of the wet film always necessarily formed a portion of the circuit, introducing thus a considerable degree of resistance.

When the poles of the induction coil are united by a metallic or liquid conductor, on completing the voltaic circuit, a current is induced in the induction coil which circulates in the opposite direction from that possessed by the primary galvanic current, and on breaking this latter circuit, we have a second induced current circulating in the opposite direction from the first.

In this case, the poles of the coil are alternately positive and negative. The matter is however quite different when, as in the experiments detailed below, a portion of the induction circuit consists of air, so that a spark is generated. Here only, the current, induced by breaking the voltaic circuit, is able to travel through the intervening portion of air, and the poles become fixed, and remain steadily positive and negative, as with a galvanic battery. This has been proved during the last ten years by a variety of experiments, and is further confirmed, were it necessary, by the constant production of pure positive and negative photographic figures under answering circumstances. I have analyzed the induction spark between brass electrodes by a rapidly revolving mirror, and find it made up of a very considerable number of discharges, but according to what has been above stated, they are not to be regarded as oscillatory, but as always retaining their original direction. This is of course a great advantage in studying the electric discharge by the photographic method, as we obtain an unmixed figure, comparatively simple in its constitution, and the information thus gained, (it is hoped,) can hereafter be applied to the elucidation of more complicated electrical phenomena.

*Form of the Positive Figure,*

viz: that which is formed on the sensitive plate used as negative electrode.

Length of the discharge $\frac{1}{18}$ of an inch.

(*a*.) When a brass ball is used as positive electrode and the moist plate is connected with the negative pole of the coil, the figure consists of a central thick and irregularly shaped ring, and an external portion formed like an irregular star; see fig. 4, which is *enlarged* three diameters, as also are all the other drawings of the spark in this number, except where the contrary is expressly stated. The central portion within the first mentioned ring is more or less blackened, sometimes being quite dark, so that the ring can hardly be distinguished. In one case 14 sparks generated under the above mentioned circumstances were allowed to fall on the sensitive plate, which was then somewhat washed with pure water before the developing solution was applied. This operation it is well known reduces the intensity of the picture obtained, and thus often allows details to be made out which otherwise would be obscure. It will be indicated hereafter simply by the expression "washed plate." In all these fourteen cases, the interior of the above mentioned ring was uniformly darkened, and of a purplish color by transmitted light. It often happens that two of the external starlike figures are produced and superimposed; this was very beautifully shown in this washed plate, as well as in some which were unwashed. This, and other indications, obtained in positive and negative figures,[*] point out that it is a matter of chance whether single, double, or triple sparks are obtained by the ordinary manipulation.

4.

(*b*) When the positive electrode is a metallic *point*, a figure like 5 is obtained, its interior being often more or less shaded; it is also generally surrounded by a diffused faint mass of shade.

5.

(*c*.) When two points are used, as before described, the figure remains essentially the same as with a single point.

It will be seen that while there is some difference between the positive induction figure, and that produced under similar circumstances by frictional electricity, there is yet some analogy between them. This could be made more evident by wood-cuts representing forms obtained two years ago, which approached in shape more nearly to the induction figure than those published at that time. The induction spark, however, as is known, consists of two distinct portions, *i. e.*, the spark proper, and the so-called luminous "atmosphere," and it will be shown at the conclusion of this article, that when the effects produced by the two different portions are separated, the resemblance to the figure generated by frictional electricity is much closer than it here appears.

[*] Compare figure 22.

Length of discharge $\frac{2}{10}$ inch. The forms are as above described.

$\frac{3}{10}$ of an inch. (*a.*) When the positive electrode is the brass ball, the figure remains the same as with $\frac{1}{10}$ inch, except that rather faint starlike rays begin to be thrown out from the external ring.

(*b.*) When the point is used the figure is like fig. 5, except that the ring manifests a disposition to take a starlike border.

$\frac{4}{10}$ and $\frac{5}{10}$ in. The figures are much the same as the last.

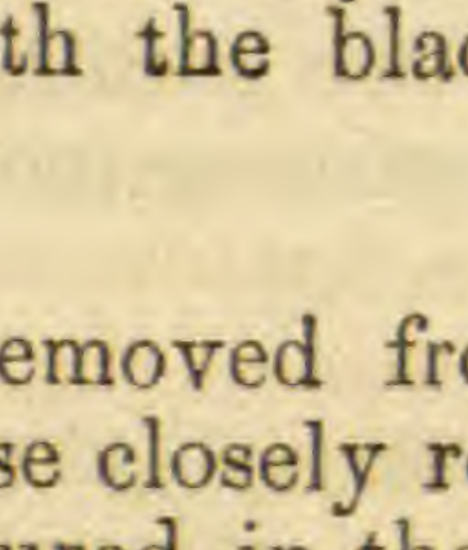
$\frac{7}{10}$ in. From a brass ball. The form is often like fig. 6, the irregular ring showing a tendency to become starlike.

6.

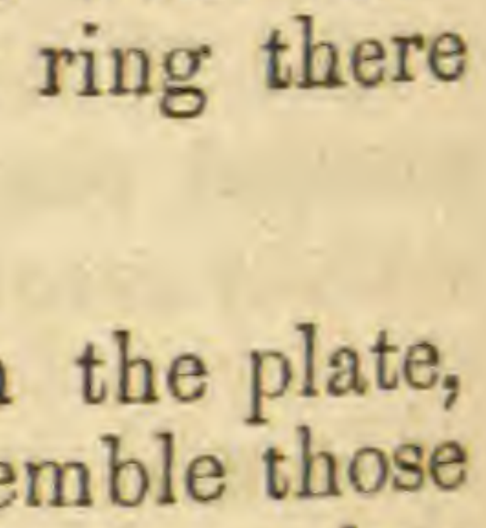
1 inch. Often like the last; sometimes the star becomes fully developed, resembling much the starlike figures produced by frictional electricity. On one plate, for instance, occurs a figure like 1, (in the first No. of this memoir), except that the central portion of the disc is occupied by a dark disc; others resemble the figure marked .5 in the same wood-cut, except that the two fine rings are not reproduced in the induction figure.

$1\frac{1}{10}$ inches. A more or less irregular star, as seen in fig. 7, resembling in many respects those produced by frictional electricity; the fine rings however being absent. It seems to consist of two parts, a star with a sharp outline, and a circular mass of shade, which, when unsymmetrically placed, is seen to be distinct from the star as in fig. 8. The starlike portion corresponds with that shown in fig. 4, and the black disc with the black ring there figured.

7.



8.



If the positive electrode is farther removed from the plate, only partial sparks are produced. These closely resemble those obtained by frictional electricity and figured in the first number of this memoir; it is to be remarked, however, that the lines making up the figure are much *thicker*, viewed under the microscope, as if they had been traced by a blunt quill instead of a fine steel pen.

*The Induction Brush.*—Here the same peculiarity is to be noticed; for while the forms of the figures (often very beautiful) are quite like those obtained from a common electric machine, the lines making them up are far thicker, so that the difference is easily seen with the naked eye. In consequence of this, the smaller figures are not rings, but minute round discs, either uniformly black, or with the central portion only slightly lighter than the rest of the disc. It is true that uniformly shaded discs of this kind can be detected with the microscope on plates prepared with frictional electricity, but then they are much smaller than when the coil is used. Forms like V (in my first article on this subject) are also constantly produced.

*Form of the Negative Figure,*

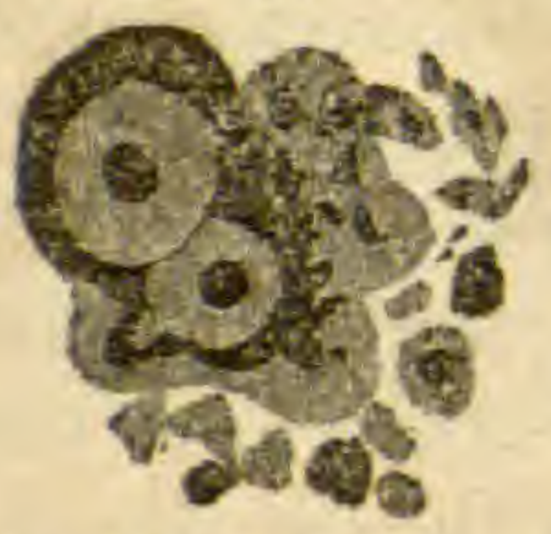
viz: that which is generated on the sensitive plate used as the positive electrode.

Length of discharge $\frac{1}{10}$ inch.

(*a.*) When the brass ball is used as negative electrode, a form like fig. 9 is produced, consisting of a number of dots, and generally two circular discs with a central nucleus each. There are often many more dots than are indicated in the wood-cut. It will be seen that this figure closely corresponds with that produced by frictional electricity, with the exception of the two discs and nuclei, and these, as will be shown, are generated by the "luminous atmosphere."

(*b.*) When the negative electrode is a metallic point, we have a central nucleus, and a uniform disc of shade, fig. 10. This, with the exception of the central points, corresponds with what was often obtained in using frictional electricity.

9.



10.

11.

(*c.*) With two points, the figure remains as in *b.* When a *washed* plate is employed, many markings of a peculiar, but hardly starlike nature are revealed in the nucleus, see fig. 11.

It will be noticed that the size of the *negative* induction figure is nearly the same as that of the positive, whereas with frictional electricity, the negative figure was much larger.

$\frac{2}{10}$ in. The forms are about the same as above indicated.

$\frac{3}{10}$ in. (*a.*) When the negative electrode is the brass ball, the figure remains as above.

(*b.*) When it is a point, a new figure is produced, see figs. 12, which are magnified six diameters. These resemble, to some extent, negative figures produced by frictional electricity. The central disc is owing to the atmosphere.

12.

$\frac{4}{10}$ in. Much like the last.

$\frac{5}{10}$ in. Also much like fig. 13, or like fig. 10, the circumference of the circle being a dark ring.

$\frac{6}{10}$ and $\frac{7}{10}$ in. Like fig. 12, except that the figure is enclosed in a ring.

13.

$\frac{8}{10}$ in. Like fig. 13, the white ring being often shaded uniformly with the exterior ring. Using a point, figures similar to 12 are obtained, their outline being quite circular. These correspond with those obtained by frictional electricity.

This is the limit of the negative spark; and on separating the electrodes farther, the negative brush is obtained. This produces, as with frictional electricity, only a general diffused blackening of the plate, which is, however, slight in intensity when compared to that obtained by machine electricity.

*Form of the figures produced by the "Luminous Atmosphere."*

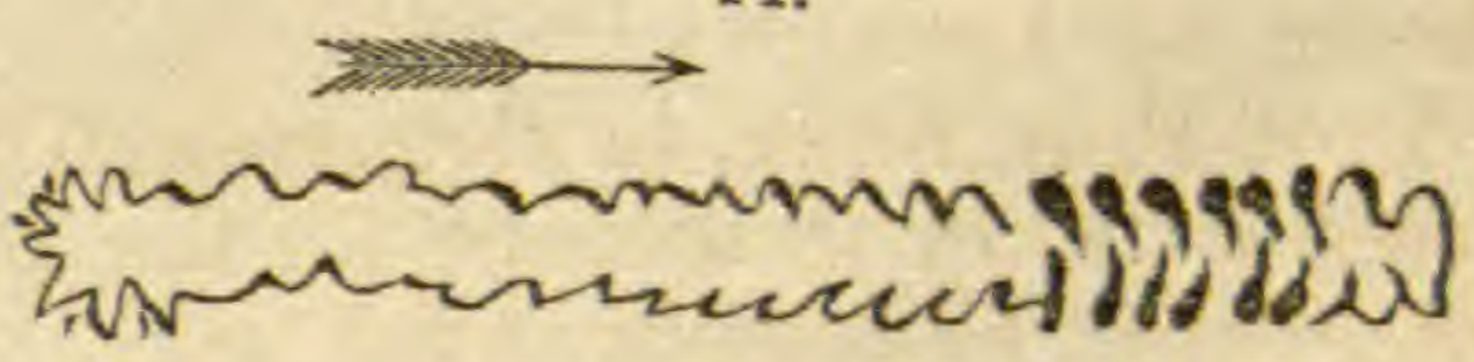
In the year 1855, Du Moncel showed that the induction spark is essentially different from that obtained from a Leyden jar or the prime conductor of an electric machine; and it is now understood to consist of a line of light, supposed to correspond to the common electric spark, and of a portion termed a luminous atmosphere. This latter can be moved by a current of air so as to be detached from the line of light; its heating power is greater, and its duration longer than that of the line of light as shown by Lissajous.

Prof. P. L. Rijke, in Poggendorff's *Annalen*, cxi, 612, has proposed a theory to account for the presence of this luminous atmosphere; he conceives the spark proper to be formed by the union of the electricities near the two ends of the induction coil, while the atmosphere, he thinks, is generated by the electricities from portions nearer the middle of the coil, the latter traversing a much greater length of wire and experiencing consequently much more resistance. According to this theory, then, the movable atmosphere consists of *true electric discharges*, the series of positive discharges being directed toward the negative pole, and the negative discharges toward the positive pole. This theory of course excludes the idea of the so-called "atmosphere" being in reality simply an atmosphere of heated particles of matter. We have seen thus far, that, in all cases, positive and negative discharges produce each their peculiar figures on the sensitive plate, and if Prof. Rijke's theory be correct, we should of course expect to find that the so-called atmosphere would produce on the sensitive plate definite figures analogous to those produced by the common spark, and, further, that the positive would differ from the negative figure. On the other hand, if the "atmosphere" consists simply of heated particles, we should expect merely a general and ill-defined blackening of the sensitive plate, which would probably be much alike at the two electrodes.

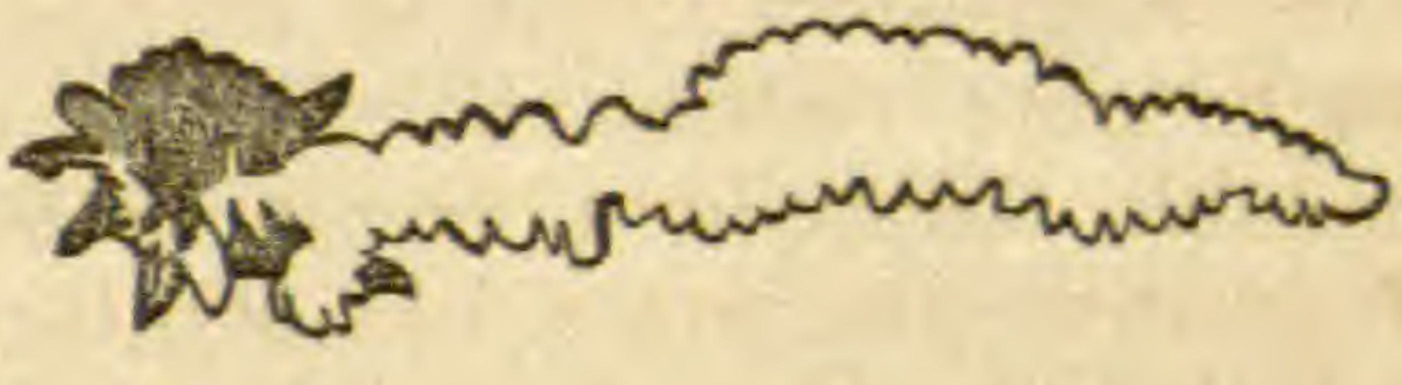
The following experiments will serve to show that in point of fact the most remarkable difference does indeed exist between the positive and negative figures generated by the movable atmosphere; that these figures are for the most part definite and delicate in outline, and filled with details that strongly suggest the idea that they are produced either by many discharges of varying intensity, or by a continuous discharge subject to constant and often tolerably regular fluctuations. These results serve therefore to a considerable extent to confirm the views set forth by Mr. Rijke.

*Mode of Experimenting.*—The apparatus was arranged as before, a metallic point or brass ball being used as electrode, and the spark was generated while a current of air traversed the surface of the sensitive plate. It was found in practice that blowing from the mouth across, or at a small angle with, the sensitive plate, answered quite well. The "atmosphere" was then drawn out into a streak of light on the surface of the sensitive plate an inch or more in length, and thus photographed itself, showing a mass of details quite invisible before. When the brass ball is used as positive electrode, figures similar to fig. 14, are produced, consisting of a star which is generated under the electrode, while the atmosphere produces the long tail, which is often filled with complicated cross markings, as it were interwoven. Fig. 15 was produced by removing the electrode to $\frac{2}{10}$ in., and here it can be seen that the movable atmosphere starts from a central star corresponding to the inner ring in fig. 4, while the external star of fig. 15 also corresponds to that seen in fig. 4. With a spark $\frac{4}{10}$ in. long, figures like 16 (only partly drawn,) are produced. Here we have a star corresponding to that which is produced by frictional electricity, with the luminous atmosphere emanating from near its centre. The same may be said of fig. 17, obtained with a spark one inch in length. Fig. 18 was produced by a point, the spark being $\frac{1}{10}$ inch.

14.



15.



16.

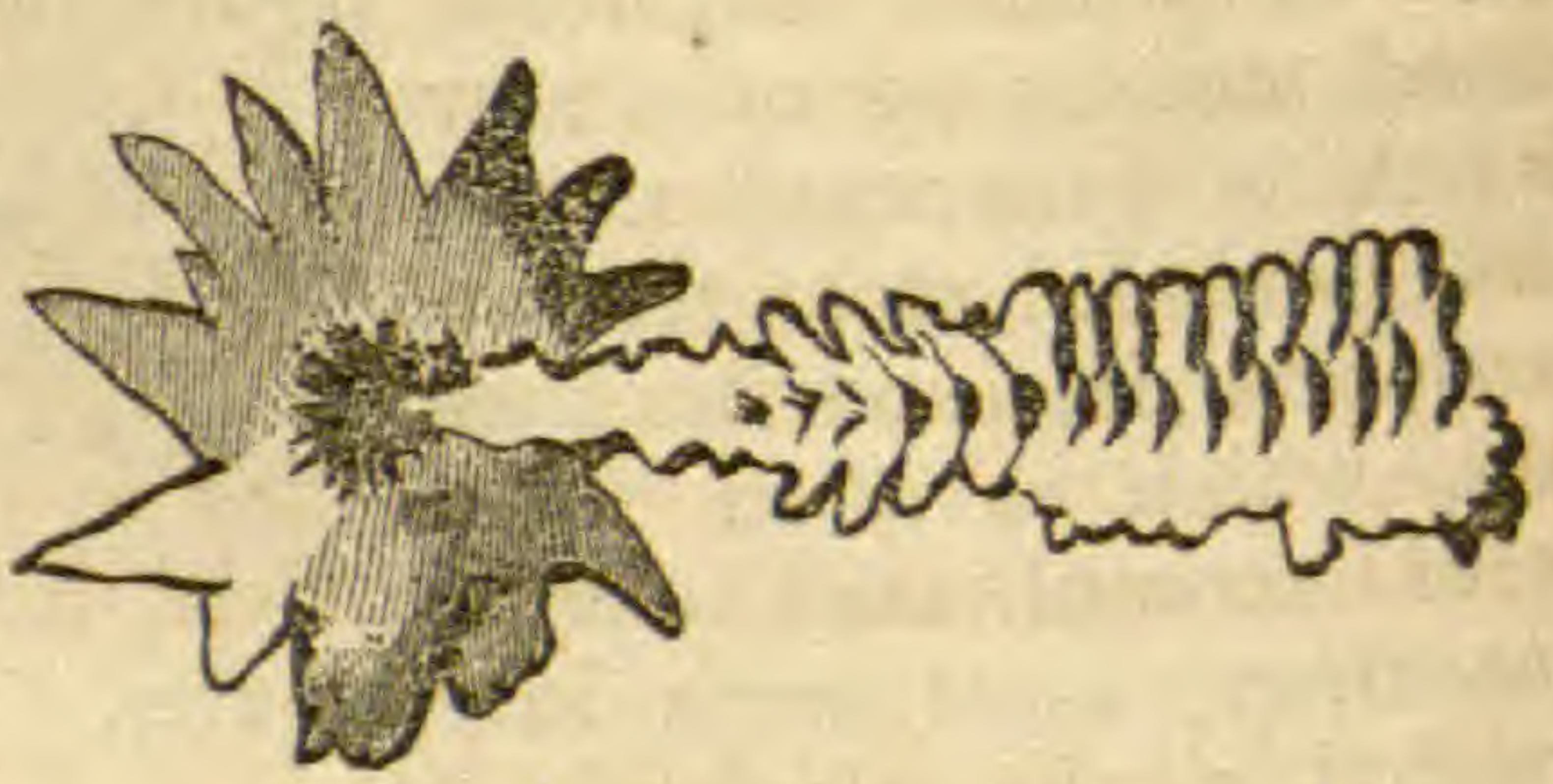
17.

18.

Prof. Rijke, in order to test his theory, caused the discharge of a large induction coil to take place between insulated brass points, which were connected with the poles of the coil by two strings wet with distilled water; the diameter of these strings was 5$^{mm}$, their length $\frac{7}{10}$ of a meter. Under these circumstances, *all* of the electricity developed is subjected to a very considerable resistance in passing through the water, and he found that in this way, in conformity with his theory, the bright line supposed to correspond with the common spark entirely disappeared, and only the atmosphere remained. Experimenting in the same way, I used two wet strings, each 2 feet long, and $\frac{1}{10}$ of an inch in diameter. It was found that the intensity of the bright line was greatly diminished, though it did not entirely vanish. Using this arrangement with a spark $\frac{1}{8}$ inch long, figures like 19 were produced; while, on removing the strings, the same current produced pictures like fig. 20. In the latter figure, attention is called to the symmetrical manner in which the markings in a portion of the tail are disposed. These wood cuts cannot of course do *justice* either to the peculiarity of the markings, or their exact disposition. It seems possible that the study of markings like these, and others quite distinct, which will be noticed hereafter, may throw some light on the mode in which electricity progresses over imperfectly conducting surfaces. When the electrode is removed to a distance of $\frac{3}{10}$ in. and the wet strings still employed, figures similar to 19 are still obtained.

19.

20.



These experiments explain to a considerable extent the difference between the positive figures as obtained by frictional electricity and by the coil, showing them in great part to be due to the presence ef the luminous atmosphere.

*Form of the Negative figure produced by the Luminous Atmosphere.*

When the negative electrode is the brass ball, and its distance $\frac{1}{10}$ inch from the sensitive plate, the "atmosphere" is blown out, producing figures like 21, consisting of an external heavy black streak, irregularly shaped, and often not over well defined at its edges, its

21.

interior being occupied by a canal with sharp edges, along which are disposed sharp round dots, &c. The portion of the spark not moved by the current of air consists of a mass of minute, rather ill-defined dots and rings, or simply of a uniformly shaded circular disc. Both of these latter forms seem identical with those produced by frictional electricity, and we are therefore led by the inspection of the negative figure to the same conclusions arrived at by the study of the positive. It often happens that from the collection of dots and rings, two or three of the black tails have their origin, showing that a double or triple spark had been generated; see fig. 22, magnified 4 diameters. It will be noticed that the actinic power resides mainly in the "atmosphere," the collection of dots and rings being comparatively faint.

22.

23.

When the length of the discharge is increased, the figure remains unaltered in character, except that the tail takes its origin from figures like 12; see fig. 23 (only partly drawn), length of spark $\frac{4}{10}$ inch.

When two wet strings are used as before indicated, the length of the negative tail is much diminished, and for sparks $\frac{1}{10}$ inch long the character remains as in fig. 21. With sparks $\frac{2}{10}$ inch long, the movable atmosphere still produced strong figures as before; but the series of rings and dots, corresponding to the negative figure obtained by frictional electricity, often vanishes entirely, a result quite in accordance with the theory of Prof. Rijke.

*On certain fine Markings which often accompany the Negative Figure.*

When the sensitive plate is used as positive electrode, the negative figures, obtained without the use of a current of air, are often accompanied by peculiar fine markings, which usually extend outward toward the edge of the plate. The details of these markings are generally about as fine as the structure of the collodion will allow, and they consequently bear a much higher magnifying power than the negative figure itself. An example of this kind is seen in fig. 24, magnified seven diameters, the distance of the electrode from the sensitive

24.

plate being $\frac{7}{10}$ of an inch. Another is represented in fig. 25, magnified 12 diameters; the length of the spark was $\frac{5}{10}$ of an inch. A large number of these markings were obtained on different plates, and their agreement in character is so decided that they cannot be attributed to accident.

25.

Further, their connexion can always be traced to certain negative figures, and often into their interior. When two metallic points are used as before described, and positive and negative figures generated under them, it would be expected that these lines, marking the passage of electricity, would extend from one figure to the other. This is not the case, the markings being directed in just the opposite way. They are caused no doubt by the passage, over the sensitive surface, of comparatively feeble currents of electricity, and show in a remarkable manner the minute and often regular fluctuations which attend the development of the electric light under such circumstances. We should suppose that electricity would not produce much more than a streak more or less continuous under these circumstances, and these markings I consider among the most curious of the results obtained, suggesting as they often do the perspective view of a cylinder wound with a right and left-handed spiral. No markings of this kind have in any case been observed in connexion with positive figures, and I have not obtained similar tracings by the use of frictional electricity. They are probably connected with the movable atmosphere, as they can often be traced directly to the impression left by this constituent of the spark.

When the sensitive plate forms either the positive or negative electrode, and the length of the spark is $\frac{1}{10}$ of an inch, on removing the plate into a dark room, it is found before development that a very small dark ring about $\frac{1}{100}$ of an inch in diameter has been produced. It is always much fainter when the plate is used as negative electrode. If the iodid of silver be removed from the film by hyposulphite of soda, the intensity of both positive and negative rings is considerably reduced, and then it becomes quite difficult to distinguish the positive figure. I cannot say at present whether these rings are produced by light, heat, or galvanic action, though the relative intensity of them, and their occurrence on both electrodes, render the last supposition improbable.

August 31st, 1864.

ART. XXXVII.—*Dependence of Terrestrial Magnetism on Atmospheric Currents;* by PLINY EARLE CHASE, M.A., S.P.A.S.*

ON account of the mutual dependence of all the forces of nature, and the reasonableness of Prof. Faraday's conjecture, that they are often, if not always, convertible more or less into each other,[1] it seems probable that the disturbances of the magnetic needle may be as closely connected with the earth's rotation, and the continually changing position of each point relatively to the sun, as those of the barometer and thermometer. Ampère held that the earth is an electro-magnet, magnetized by an electric current from east to west, the current being excited by the action of the sun's heat successively on different parts of the earth's surface as it revolves toward the east. The friction of trade-winds and ocean currents, and the variations of light and temperature that are produced by rotation and orbital revolution, must exert an influence upon the magnetic needle, and besides these indirect effects, Arago has shown that simple rotation, in some unknown way, produces magnetism in bodies of every description. Many have supposed that this magnetism is derived from the earth by induction; but on account of the impossibility of escaping from the influence of terrestrial magnetism, it is difficult to obtain any conclusive evidence on the subject.[2] A similar impossibility has interfered with Prof. Faraday's endeavors to connect gravity and magnetic or electric action by experimental results. The probability of such a connection has been shown by the electricity developed in the dry pile of De Luc, and by Gen. Sabine's observation, that when the sun and moon were on the meridian, the magnetic variation reached 5°, but when they were in quadrature, it fell as low as 20′.[3]

The great forces of nature can be measured only by their disturbances or their deviations from uniformity. The action of gravity is so nearly uniform at all times and in all parts of the globe, that it is difficult to imagine any crucial experiment that could demonstrate its relations to magnetism. Perhaps a needle, hinged at its point of support, with the two extremities nicely balanced, might help us toward such a demonstration, if careful experiments were tried, to show the relative influence of gravity upon each extremity, both before and after magnetizing, and when subjected to artificial magnetism, so as to produce various amounts of deviation from the normal dip and declination. Or, centrifugal force, so applied as alternately to assist and oppose the effects of gravity, as in large fly-wheels revolving with va-

* From the Proceedings of the Am. Phil. Soc.

[1] Phil. Mag., [4], i, 68.

[2] See correspondence of M. J. Nicklès, this Journal, [2], xvii, 117, &c.

[3] This Journal, [2], xix, 424.

rious degrees of rapidity, may indicate variations of magnetic influence, that can be explained only by the conversion of gravity into magnetism, or the reverse.

Prof. Faraday, in a lecture before the Royal Institution in the year 1857, endeavored to show that our usual conception of gravity is not in harmony with the principle of "conservation of force." Prof. Brücke[4] and others have tried to remove the difficulties in which the question is involved, but I believe none of the proposed solutions have been satisfactory to the learned philosopher who first started the discussion.

It has even been questioned whether gravity can be properly called a force, or whether it is anything more than a simple "tendency." Prof. Brücke has shown conclusively, that it is subject, like heat and other recognized forces, to all the laws which regulate the interchange of *actual* and *potential* energy; and our barometrical investigations furnish a beautiful illustration of the manner in which its tension is balanced by opposing forces.

We speak, indeed, of weight, as if it could be predicated only of bodies at rest, and as if it were so entirely distinct from momentum that no comparison could be properly instituted between the two. Precisely the reverse is true. Absolute rest is apparently an impossible condition of matter, for, to whatever extent the action of opposing forces may be relatively neutralized, the inconceivable rapidity of æthereal, planetary, and stellar motions produces a constant change of place. Even if we confine our attention to the earth alone, in each instant ($dt$) every particle has a tangential motion ($\sin d\theta$), and a central motion of gravity ($\text{vers}\, d\theta$) that constitutes a *vis viva* which we call its weight, and which is in equilibrium with the elasticity of the molecular æther. The sum of all the instantaneous energies is the same, whether the particle fall freely for any given time, or remain apparently at rest. All the potential energy which is transformed in one case into the actual energy of motion,[5] in the other is counteracted by an equivalent and opposite central energy of elasticity. Therefore, when we compare the relative effects of rotation and gravity, it is immaterial whether we use as the measure of force, the integral of the *vires vivæ*, or the re-

[4] Phil. Mag., [4], xv, 81.

[5] The potential energy of gravity is represented by $g = 32$ ft. per second. The earth's rotation allowing only about $\frac{1}{289}$ of this amount, or ·1107 ft. per second, to be converted into actual energy, the remainder must be employed in overcoming molecular elasticity. The formula $a = \left(\frac{gt^2r^2}{4\pi^2}\right)^{\frac{1}{3}}$ gives 26,221 miles as the radius of the sphere of attraction that is in equilibrium with the molecular elasticity at the earth's surface. These opposing forces must produce constant oscillations, and, by the study of these oscillations, it may perhaps be possible to reconcile the several hypotheses of Newton, Faraday, Mossotti, and Challis, respecting the nature of gravitation. See Phil. Mag., [4], xiii, 231–7, and xviii, 447, sqq.

spective amounts of motion that the two forces would produce, if they were able to act freely for the same time. The difficulty of determining the repulsion of molecular elasticity precluding any satisfactory use of the former measure, I employed the other;[6] and the precise accordance of the results, thus obtained, with the results of observation justified the correctness of the hypothesis, in the same manner as the accurate computation of planetary motions has confirmed the Newtonian theory of gravity.

Gravity, therefore, with the same propriety as heat, may be considered as a "mode of motion," whether acting merely as "dead weight," or as an accelerating or a deflecting force. If it can be shown that magnetism also originates in motion, we may be able to demonstrate the mutual convertibility that Faraday suspected.

The earliest hypothesis with regard to terrestrial magnetism looked for its cause to a powerful magnet, lying nearly in the line of the earth's axis. Subsequent discoveries led to a modification of this view by the supposition of another magnet, pointing toward the Siberian pole. Mr. Barlow's idea, that the magnetism is superficial and induced,[7] has now been generally adopted, and if it could be shown that solar or rotary action is capable of developing magnetism in particles such as those which are known to constitute our globe, the great difficulty in the way of a satisfactory explanation would be removed.[8] Ampère's, Barlow's, and Christie's experiments showed that simple rotation is sufficient to affect the magnetism of a compass-needle,[9] and in the oxygen of the atmosphere, which, as Faraday discovered, has a specific magnetism, variously estimated at from $\frac{1}{882}$* to $\frac{1}{287}$,† of that of iron, we have a medium through which any induced magnetism may be distributed over the entire surface of the earth. Some simple experiments, that can be easily repeated, seem to confirm Ampère's views, and to indicate the manner in which the circulating electric current is excited.

There is a species of mechanical polarity, of which I have never seen any notice, that is apparently produced by motions resembling those to which the air is continually subjected. It may be exhibited in the following ways:

1. In the middle of a basin of water lay a long strip of any substance (floating it by corks or otherwise, if it is heavier than water). After the water has become still, lift the basin carefully by one hand, and hold it at arm's length. The intermittent muscular action produces longitudinal vibrations, which tend to

[6] Proc. Am. Phil. Soc., ix, 284.
[7] Phil. Trans., 1831.
[8] Enc. Brit., Art. "Magnetism."
[9] The effect of rotation on the magnetic needle may be shown in a rough way, by causing an ordinary grindstone to revolve rapidly, and bringing a compass near its edge.
* By Becquerel.
† Plücker.

bring the floating strip into a line with the outstretched arm, and the tendency may be increased by moving the basin gently up and down.

2. Hold the gimbals of a binnacle compass so that it can swing only in one direction, and cause it to move like a pendulum in that direction. The needle will tend toward the line of oscillation. Vessels may have been lost from ignorance of this fact, for it is not unusual for compass pivots to become so worn that the needle moves sluggishly, and, in order to start it, the compass-box is shaken. If one of the gimbal hinges should be rusty, the shaking would bring the needle toward a line perpendicular to the axis of the free gimbal, and the captain might easily suppose that he was sailing north, when his course was due east or west.

3. Take an ordinary pocket compass, grasp it firmly between the thumb and finger of one hand, and move it quickly up and down through a small arc. The needle, as in the last instance, will tend toward the plane of motion. This experiment may be variously modified, according to the length and directive energy of the needle, the steadiness of the operator's nerves, &c. Sometimes a simple grasp, with a powerful muscular contraction, will bring the needle into line, without any other vibration than that which arises from the irresistible nervous tremor. Sometimes the momentum acquired by each pole in its approach to the operator, carries it forward so as to bring the other pole under the wave-influence, and the needle is thus made to rotate so rapidly as to become nearly invisible.

The polarity in each of the three cases here enumerated, is easily explained upon purely mechanical principles, but there are some indications that seem to show a close connection between the mechanical vibrations and those of nervous electricity. There appears to be a great difference in the control of different individuals over the needle. Some can bring it into line at once, with scarcely any perceptible motion, while others are obliged to use considerable effort; the needle does not seem at all times equally susceptible; it often appears more easy to produce rotation in one direction, than in the other. There may, therefore, be a natural connection between these experiments and those of M. Du Bois Raymond, who attached two strips of platinum to a very delicate galvanometer, and caused them to dip into two cups of salt water. Dipping the fingers of each hand into the cups, and alternately bracing the muscles of each arm, he produced a perceptible deflection of the needle. MM. Becquerel and Despretz repeated the experiment without obtaining very satisfactory results, but M. Humboldt was more successful.[10] Add to these phenomena the well-known evidences of a constant current, circulating around magnets, and if we

[10] This Journal, [2], viii, 405.

suppose that electricity consists simply of vibrations, it will seem perfectly natural that the magnet should obey the strongest vibrations.

Barlow's and Lecount's laws for the distribution of the induced magnetism in masses of iron, are precisely the same as would follow from the relative centrifugal motions of different portions of the earth, provided the magnetic axis coincided with the axis of rotation. It is therefore reasonable to presume that they accurately represent the superficial motions or currents on which the magnetism depends, and to hope that a careful study will enable us to detect the cause of the oscillations that polarize the air and all other bodies that are capable of vibrating in harmony with it.

If the earth were stationary, the sun's heat would produce a constant ascending current over the whole meridian, which would be supplied by colder lateral currents from each side. The effect of these several currents would be a mechanical atmospheric polarity, precisely analogous to that which was indicated by our experiments upon the control of the magnetic needle by mechanical vibrations.

In consequence of the earth's rotation, this condition exists only at the instant of noon. At all other times, the flow of the cool air toward the equator, and of the warm air to the coldest portions of the globe, is modified by the earth's motion. The warm air rises at the equator, and flows toward the point of greatest cold, until it becomes sufficiently cooled to sink to the earth. Still flowing onward, it absorbs the heat of the earth, until it is so rarefied as to rise again. This process of alternate rise and fall, is continued until the air reaches the pole, and then returns by the same law, and in a similar manner, to the equator.[11] These currents, which are flowing at all hours, and in all portions of the earth, produce an atmospherical directive energy toward the poles of maximum cold, which appear, according to Sir David Brewster, to coincide with the magnetic poles.

Now, if we consider that in addition to these permanent currents, there is a continual motion of silent convection, the warm air rising, and the cold air descending in parallel columns, like the particles in a vessel of boiling water,[12] and if we remember

[11] Halley, in 1686 (Phil. Trans., No. 183), explained the trade wind, and the necessity of a reverse upper current, but he found it "very hard to conceive why the limits of the trade wind should be fixt about the 30th degree of latitude all around the globe." The problem has been solved by Prof. Ferrel. I am not aware that any one has ever pointed out the combined effects of convection, absorption of heat from the earth, and the daily superposition of the rotation- and temperature-currents.

[12] It is very probable that this motion of convection is a more important agency than has generally been supposed. If we close the lower drafts of a common air-tight stove, and open a register immediately over the fire, the cold air does not rush directly to the draft pipe; but it falls with great velocity to the surface of the fuel, as may be shown by dropping pieces of paper through the register.

that the warm air is charged with moisture which is condensed as it ascends, parting thereby with much of its heat and electricity, we can hardly deem it necessary to adopt Dr. Dalton's hypothesis that ferruginous matter is the source of atmospheric magnetism. Still, the existence of vaporized iron in the air undoubtedly contributes an increased intensity to the magnetic currents, and it may probably be an important agent in the production of magnetic storms.

The two vibratory systems above mentioned are conjoined during the hours when the sun is above the horizon, and the laws of motion applicable to the first system correspond precisely, as I propose to show hereafter, with the laws of the solar-diurnal magnetic variation deduced from General Sabine's admirable discussions of the St. Helena observations. It is not so easy to explain in its minute details the comparatively insignificant lunar-diurnal variation, but I am convinced that the aërial currents produced by lunar attraction will sufficiently account for all the magnetic influence that is due to the moon exclusively. The changing barometric pressure, and the deposition of dew during the night, modify these currents in such a way as to disguise the simple effect of any slight disturbing cause; nevertheless, there is a manifest tendency, underneath all the disguise, to maxima and minima at the precise hours when they ought to occur in consequence of the moon's attraction.

In the influence of the violet rays upon magnets, the connection of the violet rays with the tension of brass in the polariscope, the excitement of magnetic vibrations in iron by percussion and torsion, the increase of magnetism by cold and its diminution by heat, and the general correspondence between Challis's laws of molecular action and the laws of attraction and rotation, we may find interesting evidences of the unity of force which all modern discovery tends to demonstrate, and in that unity a sufficient explanation of the observed annual and secular variations of the magnetic needle, the disturbing magnetic effects of auroras and solar spots, the changes of the wind, and storms of every kind. Some of the well-known phenomena of storms furnish a ready test of the principles I have attempted to establish.

Although I have described the general tendency of a particle of air, it is not probable that all the atmosphere, or even perhaps any considerable portion of it, follows so regular a path. In the upper regions, where the air is not so much affected by the radiation of the earth, it may oscillate, as suggested by Redfield, "from centrifugal action toward the equator, and gravitation toward the poles,"[13] and between the points of decussation there are undoubtedly eddies which have a general movement east-

[13] This Journal, xxv, 130.

ward or westward, in accordance with the theory of M. Dové. The disturbance of the æther, dependent upon the relative attractions of the earth and sun, probably produce tides corresponding in time with those of the barometer, which must modify the atmospheric currents.

Having thus ascertained the causes and directions of the principal normal currents, the ordinary theory of winds enables us to understand the effect of mountain peaks, deserts, forests, rivers, and ocean streams. Every point of the earth's surface that accumulates or radiates an undue amount of heat, becomes a centre of polarity with an attractive energy that disturbs the atmospheric equilibrium, tending to produce wind and rain. If the disturbance is confined to a limited area, there is a well-known cyclonic tendency, the portion of the eddy which is nearest the equator *generally* flowing eastward. Mr. Galton[14] has ingeniously shown that, in descending cyclones, the direction may be reversed, and I should expect a similar reversal to be of frequent occurrence in the neighborhood of some of the powerful ocean-currents, at points where they tend to produce backward eddies. Such points are found midway between the Sandwich Islands and California, about 35° west of Chili, near the west coast of New Holland, in the Indian Ocean, northeast of Madagascar, and in other places.

The effect of ocean-currents in producing cyclones, and directing their course, is well illustrated by the repeated observations that have been made in the Gulf Stream. Prof. Lesley's interesting account of the series of storms encountered by the Canada on her one hundredth voyage,[15] exhibits the natural consequences of the friction of two belts of air at different temperatures, moving in opposite directions. The warm air over the Gulf Stream, and the cold air over the Arctic currents that flow nearer to the American continent, are both borne very nearly in their normal directions, but with the approach of winter their parallelism becomes almost vertical, the cold belt becomes wider from its encroachment upon the land, and the vortices that arise from their concurrence are frequently brought down to the surface of the ocean, instead of taking place in the higher regions of the air, as they usually do during summer.

While sudden, violent tempests that are occasioned by local disturbances over a limited area, are almost necessarily cyclonic, I am inclined to adopt Espy's theory with regard to long storms, that usually "the wind will blow in toward a line rather than toward a point;" and in favor of this hypothesis, as well as of the periodicity of weather-changes, I would suggest the following explanation.

[14] Phil. Mag., Sept., 1863.
[15] Proc. Amer. Philos. Soc., ix, 361.

The normal currents of the atmosphere are subject, as we have seen, to a daily disturbance by the sun's action. This disturbance, like the moon's tidal action, is cumulative, and has a constantly increasing tendency to overcome the aërial polarity. The gathering wave follows the sun until it is saturated with vapor, and as soon as it becomes powerful enough to influence the normal current, it must produce a shifting of the wind and a deposition of moisture. The equilibrium of temperature is then restored, to be subjected anew to the same constant disturbance and the same stormy culmination.

[My attention has been called by the *Journal of the Franklin Institute*, to some extracts from the *London Athenæum* for January, announcing a paper on Magnetic Storms, which was read by Mr. Airy before the Royal Society, in which the Astronomer Royal appears, in some measure, to have anticipated my views upon the sources of terrestrial magnetism.

As I have not yet seen the paper in question, I do not know how far the priority may extend; whatever may be its limits, it will give me pleasure to yield my claims to so distinguished and cautious an investigator, and to find that my own independent conclusions have been so ably corroborated. And I believe I have good grounds for hoping that in the specific solar action which I have pointed out, Mr. Airy will find the precise "occasional currents produced by some action or cessation of action of the sun," for which he is looking.]

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ART. XXXVIII.—*On the Principal Causes of Barometric Fluctuations;*[1] by PLINY EARLE CHASE, M.A., S.P.A.S.

THE powerful and prejudicial influence of an inveterate scientific error, is shown in the following dogmatical statement of Mr. Joseph John Murphy, an investigator who has lent useful aid to meteorological science.[2]

In the *Edinburgh New Philosophical Journal* for April, 1864, p. 183, he says: "Were the atmosphere not acted on by heat, it would be everywhere at rest, and every level surface, at whatever height, would be an isobarometric surface. .... The earth's

[1] From the Proceedings of the American Philosophical Society.

[2] Mr. Murphy was an early and independent advocate of so much of Mr. William Ferrel's theory, as explains the polar depression of the barometer by centrifugal force and friction. Mr. Ferrel's paper, which appears to have been the first publication that contained a true explanation of the equatorial as well as the polar barometric depression, of the maxima near the parallels of 30°, and of the cause of the rotatory motion of storms, was printed in the Nashville Journal of Medicine and Surgery, and afterward in pamphlet form, in the summer of 1856. The subject was treated at greater length in his essay on "the motion of fluids and solids relative to the earth's surface," which was published in the Mathematical Monthly for 1859, vol. i, p. 140, sqq.

rotation cannot produce currents, but it modifies them when they are produced by the action of heat."

There can be no doubt that heat is one of the causes, and in most places it is, perhaps, the principal cause, of those atmospheric disturbances which are modified by rotation, but the assumption that the atmosphere "would be everywhere at rest," except for differences of temperature, leads to palpable absurdities.[3]

It may be freely admitted that Galileo, in attributing the ocean tides exclusively to "the rotation of the earth, combined with its revolution about the sun," attached too much importance to the simple combination of the motions of rotation and orbital translation, but his mistake is no greater than the opposite belief, which is now too prevalent, that there is only a single influence which can produce any important tidal effects in the atmosphere.

In the last number of this *Journal*, I stated that if M is the barometric mean for any given day and place, and $\theta$ is the moon's altitude, observation and theory concur in demonstrating that the lunar tide may be expressed by M C ($\sin\theta\cos\theta$),[4] C being a constant to be determined for each station, the principal elements of which are functions of the latitude, of gravity, and of time. I subjoin, in illustration, a

*Table of the average daily lunar barometric tides.*

| Lunar Hours. | Station. | | Lunar Hours. | Station. | |
|---|---|---|---|---|---|
| | St. Helena. | Girard College. | | St. Helena. | Girard College. |
| | in. | in. | | in. | in |
| 0 | −·00006 | +·00313 | 6 | −·00276 | −·00308 |
| 1 | −·00051 | +·00341 | 7 | −·00242 | −·00339 |
| 2 | −·00172 | +·00291 | 8 | −·00121 | −·00290 |
| 3 | −·00253 | +·00214 | 9 | −·00046 | −·00206 |
| 4 | −·00315 | −·00011 | 10 | +·00021 | +·00013 |
| 5 | −·00330 | −·00144 | 11 | +·00035 | +·00149 |

The existence of the tidal law, which, as we have seen, should produce differences in the respective ratios of ·5, ·866, and 1, at 1, 2, and 3 hours from the mean tide, is shown in the following

[3] See Proc. Am. Philos. Soc., ix, 283–4.

[4] This is evidently only another form of a single element in La Place's law of the tides. I present it in this shape, both because I obtained it independently, and because it makes the resemblance to my rotation formula more striking.

*Table of tidal differences and ratios.*

| Stations. | Lunar time. | Differences of Barometer. | | | Ratios | | |
|---|---|---|---|---|---|---|---|
| | | 1h. | 2h. | 3h. | 1h. | 2h. | 3h. |
| St. Helena, 1844–46. | Before 2h, | ·00121 | ·00166 | ·00207 | ·585 | ·802 | 1 |
| | After 2h, | ·00081 | ·00143 | ·00158 | ·501 | ·905 | 1 |
| | Before 8h, | ·00121 | ·00155 | ·00209 | ·579 | ·742 | 1 |
| | After 8h, | ·00075 | ·00142 | ·00156 | ·481 | ·917 | 1 |
| | MEAN, . . . | ·00099 | ·00151 | ·00182 | ·545 | ·830 | 1* |
| | Mean ratios, | | | | ·536 | ·841 | 1 |
| Girard Coll., 1842–44. | Before 4h, | ·00225 | ·00302 | ·00352 | ·639 | ·858 | 1 |
| | After 4h, | ·00133 | ·00297 | ·00328 | ·405 | ·905 | 1 |
| | Before 10h, | ·00219 | ·00303 | ·00352 | ·602 | ·861 | 1 |
| | After 10h, | ·00136 | ·00300 | ·00328 | ·415 | ·915 | 1 |
| | MEAN, . . . | ·00178 | ·00300 | ·00340 | ·524 | ·884 | 1 |
| | Mean ratios, | | | | ·515 | ·885 | 1 |
| Grand Mean, or average of Mean ratios, . . | | | | | ·525 | ·863 | 1 |

By a partial interpolation for the true time of mean tide at St. Helena, I obtain for the ratios of the means ·557, ·866, and 1, corresponding precisely with theory at 2h from mean tide. The tables furnish suggestive evidences of the effect of declination, the varying tidal influence of attraction, when acting with and against rotation, and the resistance of gravity to the tidal flow of air.

There are, therefore, manifestly four important causes of barometric disturbance: 1, rotation, with its quarter-daily phases of alternate aid and opposition to the attraction and temperature-currents, and of shifting the aërial particles to levels of greater or less density; 2, variations of temperature and vapor; 3, lunar attraction; 4, solar attraction. Among the subordinate causes, perhaps the next in order of importance is, 5, resistance of the æther, which, according to Fresnel's theory,[5] is subject to the laws of inertia and attraction, as well as to those of elasticity. If his theory is correct, the terrestrial æther (or the portion which partakes of the earth's rotation) may be so modified by the planetary æther (or the portion which revolves about the sun), as to produce a resistance varying at different hours, and a consequently varying atmospheric compression, which may some time enable us to measure its own density. The solar attraction may be constantly tending to accumulate the terrestrial æther, as well as the atmosphere, in a spheroid with a major axis in the line of the radius vector, and the position of the axes, as in the

* Major-General Sabine's table of the lunar tides at St. Helena, from October, 1843, to September, 1845 (Phil. Trans., 1847, p. 48), gives for the ratios of the MEAN, ·497, ·832, and 1, which, if averaged with the mean at Girard College, gives a general mean of ·512, ·858, and 1. The GRAND MEAN for the entire periods of observation at the two stations, is ·500, ·849, and 1.

[5] It is, perhaps, hardly proper to call this "Fresnel's theory," since it follows necessarily from the conception of an extremely tenuous and elastic material fluid, such as the æther is generally supposed to be. But I believe M. Fresnel has done more than any one else to show the agreement of the hypothesis with observed phenomena, and his labors deserve to be kept in honorable remembrance.

case of the ocean and aërial spheroids, may be modified by rotation. It appears to me that one of the most probable results of the rotation of the earth with its atmosphere, in an æthereal medium, would be the production of two systems of oscillations, moving with the rapidity of light, one in the line of the earth's orbit, and the other in the line of its radius vector, and that those systems would be constantly so related that while one tended to retard, the other would tend to accelerate the earth's motion.

The influences of rotation and attraction can be calculated, and, after deducting their amount, the problem of accounting for the residual disturbance will be simplified. Or, by taking the average of a long series of observations made at each hour of the solar day, the effects of lunar attraction may be so far eliminated, that they can be safely disregarded in attempting to fix the approximate value of the other principal disturbances.[6] The formula for the rotation tide has already been given, and observation appears to indicate that it is retarded about an hour by inertia; next in order of importance are the temperature and vapor tide, and the solar tide. It would be presumptuous in the present stage of our investigations, to attempt to fix the precise amount of disturbance which is attributable to each of these two tides, but from the following considerations we may derive conjectural results, which appear to me to be more satisfactory and philosophical than any that have been heretofore obtained.

The theoretical maxima of the rotation tide, allowing an hour for the lagging of inertia, occur at $4^h$ and $16^h$; the minima, at $10^h$ and $22^h$. The solar attraction maxima, with the same allowance, should be found at $1^h$ and $13^h$; the minima, at $7^h$ and $19^h$. If we assume that the attraction tidal curve is symmetrical, and regard all the deviations from symmetry as occasioned by differences of temperature and vapor, we may readily construct the following approximate daily barometric tidal table (p. 384).

Imperfect as these first approximations confessedly are, and probable, nay, almost certain, though it be, that a large portion of the residual tide should be transferred to the temperature and vapor column,[7] yet I think the above table will be found sug-

[6] The absence of any long series of observations at each hour of the lunar day, prevents our eliminating the effects of solar attraction in a similar way. Nevertheless, I propose at some future time to attempt the elimination, so far as practicable with the tables at my command, in the hope of thereby effecting a more accurate determination of the temperature and vapor tide.

[7] I can see no good reason, at present, for supposing the existence of a solar tide greater than ·002 in., which would be equivalent to ·0005, ·0009, and ·001, at 1, 2, and 3 hours from the mean tide. This would reduce the quarter-daily residual tide at St. Helena, to the following form:

| 1h. | 2h. | 3h. | 4h. | 5h. | 6h. | 7h. |
|---|---|---|---|---|---|---|
| −·0033 | −·0029 | −·0012 | +·0008 | +·0019 | +·0019 | +·0020 |

If this residual be added to the preceding column, it gives a result accordant with the 6th inference, except two disturbances, which, I think, can be easily explained, one at midnight, and the other in the hottest part of the day.

TABLE.

| Astronomical time. | Girard College, 1842–44. Mean height, 29·938 inches.* Observed height 29 inches +. | Rotation tide. | Temperature and vapor tide. | Solar and Residual tide. | St. Helena, 1844–46. Mean height, 28·2821 inches. Observed height 28 inches +. | Rotation tide. | Temperature and vapor tide. | Solar and Residual tide. |
|---|---|---|---|---|---|---|---|---|
| h. | | in. | in. | in. | | in. | in. | in. |
| 0 | ·943 | +·0126 | −·0031 | −·0045 | ·2985 | +·0149 | +·0035 | −·0020 |
| 1 | ·927 | | −·0055 | −·0055 | ·2819 | | +·0021 | −·0023 |
| 2 | ·915 | −·0126 | −·0059 | −·0045 | ·2660 | −·0149 | +·0008 | −·0020 |
| 3 | ·909 | −·0217 | −·0058 | −·0015 | ·2553 | −·0258 | −·0003 | −·0007 |
| 4 | ·908 | −·0252 | −·0053 | +·0005 | ·2521 | −·0298 | −·0010 | +·0008 |
| 5 | ·911 | −·0217 | −·0075 | +·0022 | ·2562 | −·0258 | −·0015 | +·0014 |
| 6 | ·917 | −·0126 | −·0124 | +·0040 | ·2642 | −·0149 | −·0040 | +·0010 |
| 7 | ·925 | | −·0170 | +·0040 | ·2764 | | −·0067 | +·0010 |
| 8 | ·935 | +·0126 | −·0196 | +·0040 | ·2899 | +·0149 | −·0081 | +·0010 |
| 9 | ·942 | +·0217 | −·0199 | +·0022 | ·3003 | +·0258 | −·0090 | +·0014 |
| 10 | ·945 | +·0252 | −·0187 | +·0005 | ·3061 | +·0298 | −·0066 | +·0008 |
| 11 | ·946 | +·0217 | −·0122 | −·0015 | ·3025 | +·0258 | −·0047 | −·0007 |
| 12 | ·941 | +·0126 | −·0051 | −·0045 | ·2913 | +·0149 | −·0037 | −·0020 |
| 13 | ·938 | | +·0055 | −·0055 | ·2777 | | −·0021 | −·0023 |
| 14 | ·935 | −·0126 | +·0141 | −·0045 | ·2646 | −·0149 | −·0006 | −·0020 |
| 15 | ·933 | −·0217 | +·0182 | −·0015 | ·2562 | −·0258 | +·0006 | −·0007 |
| 16 | ·934 | −·0252 | +·0207 | +·0005 | ·2550 | −·0298 | +·0019 | +·0008 |
| 17 | ·940 | −·0217 | +·0215 | +·0022 | ·2611 | −·0258 | +·0031 | +·0014 |
| 18 | ·950 | −·0126 | +·0206 | +·0040 | ·2737 | −·0149 | +·0055 | +·0010 |
| 19 | ·959 | | +·0170 | +·0040 | ·2898 | | +·0067 | +·0010 |
| 20 | ·966 | +·0126 | +·0114 | +·0040 | ·3048 | +·0149 | +·0068 | +·0010 |
| 21 | ·968 | +·0217 | +·0061 | +·0022 | ·3163 | +·0258 | +·0070 | +·0014 |
| 22 | ·967 | +·0252 | +·0033 | +·0005 | ·3184 | +·0298 | +·o057 | +·0008 |
| 23 | ·958 | +·0217 | −·0002 | −·0015 | ·3117 | +·0258 | +·0045 | −·0007 |

gestive of valuable inferences, of which the following are perhaps among the most important.

1. That the apparent osculation of the solar and residual curve near the hours of high barometer may perhaps be owing to æthereal resistance.

2. That the cumulative action of the sun upon the air and æther may possibly render the disturbing influence of its attraction upon the atmosphere even greater than that of the moon.

3. That the paradoxical assumption of those who advocate the temperature theory of the quarter-daily tides, that a dependent relation can exist between the barometrical changes and the changes of temperature, which "appears to be *direct* during the morning hours, and *inverse* during those of the day and evening,"[8] is unnecessary, useless, and unphilosophical.

4. That in intertropical and medium latitudes, the average daily barometric tide which is attributable to variations of temperature is smaller than the rotation tide.

5. That there is but one high and one low temperature tide in twenty-four hours.

* The sum of the tides + the mean height = observed height.

[8] James Hudson, Phil. Trans., 1832.

6. That the effects of temperature upon atmospheric pressure reach their maximum in the evening, when the aërial absorption of heat from the sun ceases to be in excess of its radiation, and their minimum in the morning, when radiation ceases to be greater than absorption.

7. That the daily temperature tide increases, while the rotation tide diminishes, as we approach the poles.

8. That, in consequence of rotation, there should be a slight tendency to vertical ascending currents at $4^h$ and $16^h$, and descending currents at $10^h$ and $22^h$.

9. That, whatever modifications the table may require, there can be no doubt of the existence of the three tides, with maxima and minima near the times specified, or of the possibility and desirability of accurately determining their magnitude.

The phenomena on which these inferences are based are all susceptible of a simple and obvious explanation, and thus, by reasoning alternately *à priori* and *à posteriori*, we elicit, from a scheme of seemingly lawless confusion, the beauty of a most marvellous order.

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ART. XXXIX.—*A new Meteoric Iron from Wayne County, Ohio.* —*Some remarks on the recently described Meteorite from Atacama, Chili;* by J. LAWRENCE SMITH, Prof. Chem. Med. Dep. University of Louisville.

### *Meteoric Iron of Wayne Co., Ohio.*

THE existence of a mass of meteoric iron from Wayne county, Ohio, has been known to me for some years; but I have delayed noticing its existence, hoping to obtain the mass and thus give a more complete description of it than I am able to do.

My attention was first called to it by Prof. James C. Booth, of the U. S. Mint at Philadelphia, it having been brought to him by Peter Williams, of Wooster, Wayne county, Ohio, who supposed it to be a mass of silver or some other precious metal. Prof. Booth saw at once that it was meteoric iron, and tried to procure it from Mr. Williams; but from some notion of its possessing considerable intrinsic value, he retained it, and since that time both the iron and Mr. Williams have been lost sight of.

Prof. Booth detached a small portion of it, part of which specimen he placed at my disposal, with the following memorandum: "Meteoric Iron, given me in 1858 by Peter Williams, of Wooster, Wayne county, Ohio. It was a rounded mass, weighing about 50 ℔s., and found by him in a woods near the above place, while gathering boulders to pave a town. It exhibits the usual figures, on application of acid to a smooth surface."

As it is a well authenticated meteorite, it is proper to make a record of it. Its specific gravity is 7·901, and it is composed of—

| | |
|---|---|
| Iron, | 93·61 |
| Nickel, | 6·01 |
| Cobalt, | ·73 |
| Copper, | very minute, not estimated. |
| Phosphorus, | ·13 |
| | 100·48 |

There was a very small quantity of manganese, that has been estimated along with the nickel.

*The new Atacama Meteorite.*

A fragment of the meteorite lately described by Prof. Joy, (this *Journal*, March, 1864, p. 243,) has been sent to me by Prof. C. F. Chandler, and I have thus been afforded an opportunity of carefully examining it. I had at first supposed that it might be in some way related to the well known Atacama iron; but it is very clear, by the most casual inspection, that it has no connection with that iron; at the same time it resembles so closely another meteoric mass from that region, in fact, is so identical with it in all particulars, that if it had not hailed from another locality, it would be pronounced a portion of the meteorite from *Sierra de Chaco*, *Atacama*, described in 1863 by Prof. Rose (see p. 131, Buchner, *Geschichte der Meteoriten*).

Prof. Joy omitted to mention in his paper that the meteorite was said to have been found in the *Janacera* pass.

The meteorite from Sierra de Chaco was, at the time it was described, unique in its physical characteristics; the close resemblance to it, therefore, of the one under notice, and its coming from Atacama has induced me to investigate as far as possible the relative position of *Sierra de Chaco* and *Janacera* pass.

The best authority on the geography of Chili in this country, is doubtless Capt. Gilliss, of the U. S. Observatory at Washington; in answer to my enquiries on the subject, he gives the following information:

"I do not know any pass in Chili named Janacera; there is a river Jarquera, which has its origin near one of the passes in Atacama, and very probably there may be a pass of the same name. The river Jarquera is to the northward and eastward of Chaco, the former being within the chain of the Andes, and Chaco most probably is in the western or coast range. They are from 120 to 150 miles apart."

As it is important to locate this meteorite correctly, I have written to Prof. Domeyko on the subject. The village of Chaco is situated near latitude 25° 20′ S., and longitude 69° 20′ W. from Greenwich; and its height above the sea is 8,778 feet.

The meteorite in question is so intimate a mixture of metallic and stony matter, that it is difficult to say whether to rank it among the stony or metallic meteorites. Treated with a mixture of nitric and chlorhydric acids, and slightly warmed, the metallic portion is rapidly dissolved, without the form of the mass being altered. Its mineral constituents are readily separated by the combined aid of chemical and mechanical means; and, besides the iron, I have been able to separate small but distinct particles of chromic iron, small spherical masses of olivine as beautiful in color and as transparent as that from the Pallas meteoric iron, and also a pyroxenic mineral; and perhaps with a larger amount of material to work upon, other minerals might have been recognized.

I have nothing to add to the careful chemical examination by Prof. Joy, having detached mechanically most of the minerals that he deduced from analysis.

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## ART. XL.—*On a Process of Organic Elementary Analysis, by Combustion in a Stream of Oxygen Gas;* by C. M. WARREN.*

THE process in general use for ultimate organic analysis had seemed to me so nearly perfect as to leave little room for any very marked improvement. Like all other processes of analysis, however, it has its own peculiar sources of error, inherent in the nature of the substances employed, and in the manipulations to be performed. But these appear to have been reduced to a minimum; so that, with great care and the necessary skill, there can be no reason to doubt that that process, with the various modifications which have been proposed to meet special cases, is capable of affording as accurate results, in a majority of instances, as can, perhaps, be claimed for any other analytical process. Nevertheless, there are instances, and they are doubtless numerous, where so satisfactory a solution of the question which may be under consideration as would be highly desirable cannot be attained by that process. It was after repeated unsuccessful efforts, in a case which appeared to be of this kind, that I was led to conceive the idea of making the combustion in oxygen gas alone; and to devise the method which I am about to describe.

Were it not for the danger of explosions in the combustion tube, the occurrence of which would, at least, render its use fruitless of good results, pure oxygen, as a combustion agent in analysis, would seem, of all substances, the one most naturally suggested. This apparent difficulty is probably the chief reason why it has not long ago been brought into general use; its

* From the Proceedings of the Amer. Acad., Boston, March 8, 1864, vi, 251.

employment since the time of Prout, so far as I am informed, having been mostly confined to the combustion of the residual carbon of highly carbonaceous substances, after the other decomposition products, containing the hydrogen, had been burnt at the expense of oxyd of copper.

By a very simple device I entirely obviate the danger of explosion; viz: the combustion tube is closely packed with asbestus, or other inert substance,[1] and yet so loosely as to leave free passage for gases through the interstices. The packing of the tube requires some care. This, however, may be readily accomplished, giving great uniformity to the mass of asbestus, by having the latter carefully broken into small, loose pieces, which are gradually added to the tube, and arranged in position by means of a stiff iron wire. Little attention need be had to the packing of the centre of the tube, as this will come right of itself, if the packing against the sides is properly done. It will be found convenient and expeditious to turn the tube continuously in the hand, and cause the end of the iron rod to follow around against the sides of the tube, placing the asbestus, by gentle taps, alongside its inner surface, so that only very small open spaces may be seen.

In the experiments which I have made, I have generally had about ten or twelve inches in length of the tube filled with asbestus. As the combustion takes place within a very short space, it seemed at first that the tube might be reduced considerably below the ordinary length; it was found, however, that shortening of the tube below a certain limit made it difficult to control the distillation of volatile substances and prevent too rapid combustion; it being essential in this, as in other processes, that the combustion should proceed slowly, and with a good degree of regularity; otherwise it would be difficult to regulate the supply of oxygen to meet the demand of the burning substance. By having the column of asbestus of considerable length, the anterior end of which only is ignited, the substance, if volatile, becomes diffused through a large space, and the distillation thereby easily controlled, as only a small portion of the substance need then be heated at a time. Doubtless a

[1] I have used only asbestus in my experiments thus far, and in every instance with perfect success. Quartz sand, selected with care as to the size of the grains, had suggested itself, on account of greater convenience in filling the tube, as it would properly arrange itself on simply being poured into the tube. It would, however, be liable to the objection, that any jarring of the tube, while lying upon the combustion furnace, would be likely to settle the particles more closely together and form a channel along the top, in which an explosion might take place which would spoil the analysis. I therefore prefer asbestus to any thing which I have thought of; and any apparent inconvenience in preparing a tube with this substance will pass into insignificance, if the precautions which I advise for protecting the tube from breakage are observed, as then the same tube may be made to last for a long time.

shorter tube would answer equally well for many non-volatile substances. It will be observed that the asbestus packing is but another application of the principle involved in the use of wire gauze in Davy's safety-lamp.[2]

In order to obtain perfect control of the analysis, and to be always certain that the requisite quantity of oxygen is being admitted, I have adopted certain simple expedients, enumerated below, which have been found fully adequate to that end.

1. The distillation of the substance, if volatile, is effected by means of a bar of copper, placed over and attached to one of Bunsen's burners, as shown at *a*, in the following figure.

This bar, having first been brought to the maximum temperature which the lamp is capable of producing, is placed near or under the bulb containing the substance; applying that part of the bar nearest or most remote from the flame, or an intermediate point, according to the temperature required.

The steadiness of the heat thus applied, and the facility with which it may be regulated by simply moving the bar, render it decidedly preferable to any other means which I have employed for that purpose. I had for a long time used such a bar for the same purpose in the old process, with extreme satisfaction. In some cases a bar of copper laid on the combustion furnace,[3] one end projecting into the flame by which the tube is being heated, and the other end raised and extending toward the substance, has been found to answer a good purpose.

2. In the case of volatile bodies (I have not yet analyzed any others by this process), I have found the combustion to proceed most satisfactorily when, having first heated about four or five inches of the anterior portion of the tube, which includes the oxyd of copper, and started the flow of oxygen, I apply the heated bar to the bulb containing the substance, and immediately expel the whole of the liquid,—which becomes at once

[2] It has occurred to me that my safety-tube may serve as the basis of a more simple and equally accurate process for the analysis of gases by gradual combustion instead of explosion, in which weighing would take the place of measurement. I propose, at an early day, to study this question by a series of experiments.

[3] As there are those, and probably they are many, who still persist in the use of charcoal in place of the more modern gas furnaces for generating heat for combustions, I desire here to say that I have in use one of Baumhauer's gas furnaces, procured a few years ago from Luhme & Co., in Berlin, which seems to me to have no fault. It is impossible for me to conceive what objection one could have to it, unless it be that a naked tube might become overheated along the bottom; and this would be a valid objection if the remedy were not so simple. If the tube be laid in a trough of sheet iron (brass is objectionable, in my process, on account of its obscuring the tube with oxyd of zinc), with a thin layer of asbestus between, and fastened together with wire, no harm could ever occur from overheating. A tube of Bohemian glass, thus protected, may be used for a large number of analyses; and, indeed, become almost a permanent fixture upon the furnace. The asbestus prevents the glass and metal from adhering together,—which is probably the chief or only cause of breakage of wrapped tubes,—so that sudden cooling and re-heating may take place with perfect security. It is important that the iron trough should

absorbed by the asbestus,—and then, if necessary, gradually move the heated bar forward, driving the substance toward the ignited portion of the tube, until it shall have reached that point in the tube where the temperature is just sufficient to cause the oxygen to take up the vapor in suitable proportion; indicated by the bubbles of oxygen and carbonic acid, as will be described below:—a point as easily found as, in the old process, that of the requisite temperature for proper distillation of the substance. When this is accomplished, which will occupy but a short time, the heat in front of and behind the substance being constant and uniform, no further manipulation of the heat is required—the supply of oxygen only requiring attention. In the ordinary way, on the contrary, in which the heat is applied only on one side of the substance, the latter, if volatile, is constantly changing position backward in the tube, necessitating a corresponding movement of the heat in the same direction, which requires constant care and considerable skill.

This proceedure—referring to the immediate expulsion of liquid from the bulb, etc.—implies that that portion of the tube immediately forward of the bulb should not already be too warm, which might easily be the case with a body of very low boiling-point. It would then be necessary to expel the substance from the bulb no faster than the oxygen would absorb it in the proper proportion; which, as experience has shown, may be easily accomplished.

With a body of extreme volatility it may be necessary also to place a dish containing pieces of ice under the bulb; as even the temperature of the surrounding air might in such a case cause the substance to pass forward too rapidly.

3. The oxgen is admitted through Liebig's potash bulbs containing sulphuric acid; and the carbonic acid formed is absorbed by similar bulbs with potash; to which is attached a tube filled

not extend much backward of that part of the tube where it is desired that the combustion should take place, so that the temperature of the principal part of the column of asbestus may remain under the control of the operator, by means of the heated copper bar, or otherwise.

Independent of the use of a metallic bar, as described above, or any novel appliance, the heat can be regulated by this furnace with a nicety as great as, or even greater, than, by the use of coal. The partitions in this furnace, between the cocks, are two inches apart; so that the gas from one of the jets ignites about two inches of the tube. To rely, therefore, alone upon the cocks for regulating the heat in burning the substance, would doubtless often lead to bad results; but the heat may be made to approach the substance in the most gradual manner,—next to that of conduction by a metallic bar,—by making use of a piece of thin brass plate, about two inches long, and half an inch wider than the top of the furnace, the edges of which are turned down against the sides.

If this plate is laid on the wire gauze covering the furnace, and pressed down so as to fit closely enough to prevent the gas from igniting under it, the gas escaping from the cock underneath may all be made to burn at one end of the plate, and to extend the heat along the tube as gradually as the plate itself is capable of being moved.

with soda-lime and chlorid of calcium, as recommended by Mulder,[4] to take up any traces of carbonic acid which may escape absorption in the bulbs, and the trace of moisture which is invariably carried forward from the latter. Special care should be taken to select both sets of bulbs with the view to have the openings in the one as nearly as may be of the same size as those of the other, so that the bubbles of oxygen, considered as representing volumes, entering the sulphuric acid bulbs, may be readily compared with the bubbles or volumes of carbonic acid entering the potash bulbs; these bubbles may then serve as a valuable index by which to regulate the supply of oxygen. Especially is this true in cases where the composition of the body to be analyzed is pretty nearly known, as then the number of bubbles of oxygen required for every bubble of carbonic acid produced may be readily calculated.

But as it is, in any case, advisable to conduct the experiment so that there shall always be an excess of oxygen passing unabsorbed through the potash bulbs, and as this excess would seldom be large even if a sufficiency of oxygen were admitted to burn the most richly hydrogenized body known, it may generally be well to admit enough for such a case.

The volume of oxygen actually consumed in burning the lightest liquid known—probably of the formula $C_8 H_{10}$—which I have separated from petroleum, and which contains a larger percentage of hydrogen than any other non-gaseous body, as compared with the volume of carbonic acid formed, is as 1·62 : 1; the fraction representing the oxygen which is taken up by the hydrogen of the body, and which of course becomes condensed and disappears from the volume of carbonic acid. In burning this body with just the equivalent quantity of oxygen,—assuming that the combustion would be complete under such circumstances,—we should have one bubble or volume of carbonic acid entering the potash bulbs, for every 1·62 bubbles or volumes of oxygen entering the sulphuric acid bulbs. A sufficient excess of oxygen would be secured in this case, and a simple ratio obtained, if 2 bubbles of oxygen were to be admitted for 1 bubble of carbonic acid appearing in the potash bulbs. The case would then be further simplified by having the openings in the sulphuric acid bulbs of such size as would give bubbles twice as large as those from the potash bulbs; as then, when the bubbling should be equally rapid in both, the relation between the volumes of the gases would still be maintained, viz: 2 vols. of oxygen to 1 vol. of carbonic acid. Such bulbs would be highly desirable, but would probably have to be made expressly for the purpose.

4. As an additional control over the supply of oxygen, and

[4] Liebig and Kopp's Jahresbericht, 1858, p. 588.

D
d
e
e
b
B
C
c
f
A
a
D.T. SMITH, SC

serving also as a temporary safeguard against the escape of incompletely oxydized substance, in case of too rapid distillation, with an insufficient flow of oxygen, I have two or three inches in length of the tube filled with coarse, strongly ignited oxyd of copper, placed in front of the asbestus, and this followed by a plug of the latter substance to keep it in place, and prevent the formation of a channel along the upper surface of the oxyd of copper. The tube itself being laid in an iron trough, as above described, with the upper half of the tube exposed, the oxyd of copper is heated in such a manner that any reduction would be readily observed at the end in contact with the asbestus. In this manner it serves as a valuable indicator, by which to determine, at a glance, whether the flow of oxygen is sufficient. It will rarely happen that any reduction of the oxyd of copper will take place. I have, however, in some of my earlier experiments, with too short a column of asbestus, and ill-adapted bulbs, had so much of the oxyd of copper reduced that combustible gases passed through the absorbing apparatus; and in one instance, when the unabsorbed gases were collected, the quantity of combustible gas was so considerable as to form, with the oxygen collected with it,—which of course came forward at an earlier or later stage of the process,—an explosive mixture. Having seen no indication that any other than gaseous bodies escaped the combustion tube in such a case, it occurred to me that such an analysis might be saved by collecting the gas over mercury, and, at the close of the combustion, before detaching the absorbing apparatus, conducting it a second time through the combustion tube.[6] As a matter of economy, also, in the saving of the excess of oxygen, when a considerable number of analyses are to be made, this idea seemed to recommend itself; as the oxygen would, at the same time, become purified from any traces of combustible matter which might be present, and could then safely be collected as pure oxygen, and finally transferred to the oxygen gasometer.

I therefore constructed for this purpose the apparatus which is represented in the background of the preceding figure as attached to the anterior end of the absorption apparatus. At the close of the combustion, when only pure oxygen appears to enter the potash bulbs, the flow of oxygen is interrupted; the communication with that portion of the drying apparatus which is back of the short U tube, A, is closed at *b*; and the tube B—

[6] As the time consumed in an experiment is so short, and the quantity of combustible gas present, if any, so very small, and that mixed with a very large quantity of oxygen, it is not improbable that the gas might as well be collected over water; as the quantity which could be absorbed by the water in so short a space of time would probably be inappreciable.

which is movable in the cork—turned up.[6] The joint at *c* is then disconnected; the end leading to the receiver C tightly closed with a piece of glass rod; and a communication established between the absorption apparatus and another receiver containing water,—not shown in the figure,—for collecting the pure oxygen. On opening the spring-clip *d* (the more modern form, which is provided with a fine screw, is excellently well adapted for this purpose), the mercury will flow from the reservoir D into the receiver C, and force the gas through the capillary tube *e e*; thence through the short U tube A, containing chlorid of calcium, to the combustion tube and absorption apparatus; and the gas is finally collected over water in the receiver provided for that purpose.

The introduction of a longer column of oxyd of copper would probably accomplish the same purpose with less expense; but neither expedient can be regarded as essential to the process. As the saving of an analysis by the use of a longer column of oxyd of copper would only be occasional, the additional heat required, and consequent discomfort occasioned by its continued use, would hardly be compensated for. So that, while I would not, therefore, recommend the use of an additional quantity of oxyd of copper, I would also discard the other expedient of collecting the gas over mercury, or water, etc., unless the saving of the surplus oxygen, together with the additional security afforded, should be considered of sufficient importance to recommend it. As the passing through of the gas the second time requires no attention after it is once started, and occupies but a short time, during which the operator may attend to anything else, I much prefer, for myself, to retain in use that part of the process.

5. Some other less important peculiarities in the construction and use of the apparatus will now be noticed, in connection with some remarks on the performance of the analysis.

The posterior end of the combustion tube, as seen in the figure, is bent obliquely upward, as in the common form, except that, instead of being drawn out to a point, it is left of the full size of the tube. The object of this form is to prevent, in a great measure, the escape of oxygen during the time occupied in introducing the substance for analysis; and also for greater convenience and security from loss in the performance of this operation; especially in the case of volatile liquids. In the latter case, the neck of the bulb—which has previously been provided with one or more scratches on its side near the end—

[6] That this tube may not operate as a siphon, the outer limb is formed by attaching, near the bend, a flexible tube, of larger bore than that of the glass tube. This flexible tube is preferable to glass, on account of the readiness with which it adapts itself to any change of position of the glass tube, by which it may always project into the receiver underneath, and prevent waste of mercury.

is introduced into the end of the combustion tube, and broken off by pressure against the side of the tube; the bulb itself is then allowed to drop in, and the end of the tube immediately closed with a perforated cork containing a glass tube, *f*, connecting it with the drying apparatus. This connecting tube is constructed of hard Bohemian glass; the anterior end of which is drawn out to a short, blunt point, and the opening nearly closed in the blowpipe flame, to the size of a small needle; the object of which is, to increase the rapidity of the flow of oxygen at that point, and thereby diminish the liability to loss from diffusion of gases or vapor backward into the drying apparatus, which is always too liable to occur when the posterior end of the combustion tube is not sealed.

As an additional precaution against loss from this source, this connecting tube is packed with asbestus in the same manner as the combustion tube, and during the combustion is heated with one of Bunsen's burners. In case vapor of the substance should reach this tube, notwithstanding the above precaution against it, it could not reach the drying apparatus as such; but would be immediately decomposed, and the carbonic acid formed would at least stand a good chance of being carried forward, and prevent a loss in the determination of the carbon. The heating of this connecting tube may be superfluous for the object above described (a point which I have not yet taken the time to determine); but it certainly has the good effect of heating the oxygen, and thus preventing the condensation of liquid at the cork in the end of the combustion tube.

In the performance of an analysis, the first step should be to expel the moisture from the combustion tube, while hot, by passing through it, for some time, a stream of dry air from the gasometer.[7] The tube should then be filled with oxygen, before the substance, if volatile, is added; as otherwise particles of unburnt substance might escape during the displacement of the air, and occasion loss. The absorbing apparatus, having been previously weighed, is then attached, and, if the excess of oxygen employed is to be saved, the oxygen again admitted to expel

[7] The necessity for this may be entirely obviated, after the first analysis, and much time saved and uncertainty avoided, by connecting the anterior end of the combustion tube, at the close of a day's operations, with a set of stationary drying tubes of ample capacity, which may stand back of the furnace out of the way, communication with which is established by means of a flexible tube. Or, better, a movable tube may be attached by means of a screw to the opening in the top of the gasometer, extending to the top of the upper reservoir, so that water cannot enter, and then, by simply turning the cock underneath, communication would be opened between the surrounding air and the combustion tube, through the intervening drying apparatus. At the close of work, the anterior end of the combustion tube should then be tightly corked, the fire extinguished, and the tube allowed to cool in dry air. It would thus be always ready for immediate use.

the air from the absorbing apparatus. The connection is then made with the receiver C, if used, and the tightness of the joints tested by turning down the tube B, so as to partially exhaust the apparatus. If found tight, as indicated by the liquid in the potash bulbs, the tube B is again turned up, and the substance then introduced in the manner above described. A very slow stream of oxygen is now admitted; the tube B again turned down till the level of mercury in this tube shall be half an inch to an inch below the level of mercury in the receiver C; and from time to time during the combustion the position of this tube is adjusted so as to preserve about this difference between the levels of the mercury, or at least so as to prevent the mercury in the tube from ever rising above that in the receiver.

In this manner the mercury, instead of offering resistance to the passage of gas from the combustion apparatus, and thus increasing the internal pressure upon the joints, which would be objectionable, actually operates advantageously by producing partial exhaustion, and thus diminishing the internal pressure upon the joints, and consequently the liability to leakage. The distillation of the substance is now commenced, and conducted as previously detailed above. So soon as condensation of moisture appears in the neck of the chlorid of calcium tube, indicating that combustion has commenced, the flow of oxygen may be gradually accelerated to keep pace with the progress of the combustion, as indicated by the bubbles in the potash bulbs. When the burning of the substance seems to have been completed, heat is gradually applied, for a short time, along the whole length of the column of asbestus, to obviate the possibility of any loss from unburnt substance.

The absorbing apparatus may be weighed filled with either oxygen or air; for myself, I prefer the latter, as, on the whole, more convenient and less liable to lead to error. At the close of the analysis, therefore, I expel the oxygen from the apparatus by admitting air from the air-gasometer,[8] saving for further use the oxygen which is expelled during the first five or six minutes. Thus far I have applied this process only in the analysis

[8] The oxygen-gasometer and the air-gasometer each having a separate drying apparatus, the time consumed in changing from one to the other is very much shortened, as the necessity for displacement of the oxygen or air—as the case may be—which is contained in the drying apparatus is avoided. Each drying apparatus consists, 1st, of Liebig's bulbs, containing sulphuric acid; 2d, of a U tube, 15 inches high (nearly 3 feet of tube), filled with soda-lime for carbonic acid; and 3d, of two such U tubes (5 to 6 feet of tube), filled with chlorid of calcium. The object in using drying tubes of such large dimensions is to avoid the necessity of too frequent renewal. The gasometers stand in a pan of copper, which is provided with an outlet to the sink, so that they may be filled without disconnecting from the drying apparatus; thus giving a degree of permanence to the apparatus and saving some labor.

of volatile hydrocarbons of the formula $C_n H_{n-6}$; $C_n H_{n+2}$, etc.[9] As a mixture of the vapors of these bodies with oxygen is highly explosive, a more severe test of the safety of the process could not be applied.

In *every* experiment which I have made, the combustion has proceeded as quietly as if burning in the open air. The results obtained are extremely accurate and uniform.

Although the bodies which I have analyzed represent but a single class of organic substances, I can see no reason to doubt that the process will apply equally well in the generality of cases.

If this view be corroborated by actual experiment, the process can hardly fail to supplant the common methods, if for no other reasons than its greater convenience, economy of time, avoidance of excessive heat, neatness, etc.; while, as regards accuracy of results, it will at least not be found inferior to the other methods; but, on the contrary, I think, preferable, as affording greater security against failures and errors from accidental causes.

Having obtained such satisfactory results in the cases referred to, and being prevented by other important work, to which the study of this process is only incidental, from pursuing the subject further at present (except so far as I shall have occasion to use the process in my other investigations), I have thought it advisable to present the process to the Academy as it now stands. I hope, however, to be able to resume the work before long, with the view to determine, by experiment, the extent of its applicability as a general method, and will report the results to the Academy.

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## ART. XLI.—*On Celestial Dynamics;* by J. R. MAYER.*

[Concluded from p. 243.]

### VIII. *The Tidal Wave.*

IN almost every case the forces and motions on the surface of the earth may be traced back to the rays of the sun. Some processes, however, form a remarkable exception.

One of these is the tides. Beautiful, and in some respects exhaustive researches on this phenomenon, have been made by Newton, Laplace, and others. The tides are caused by the attraction exercised by the sun and the moon on the movable

[9] In an analysis of amyl-alcohol, made in my laboratory by my friend Mr. Storer, for the sake of familiarizing himself with the process,—it being his first analysis by this apparatus,—the following result was obtained.

| | Experiment. | Theory. |
|---|---|---|
| Carbon, . . . . . . | 68·53 | 68·18 |
| Hydrogen, . . . . . | 13·63 | 13·64 |

* From L. E. & D. Phil. Mag., [4], vol. xxxv.

parts of the earth's surface, and by the axial rotation of our globe.

The alternate rising and falling of the level of the sea may be compared to the ascent and descent of a pendulum oscillating under the influence of the earth's attraction.

The continual resistance, however weak it may be, which an instrument of this nature (a physical pendulum) suffers, constantly shortens the amplitude of the oscillations which it performs; and if the pendulum be required to continue in uniform motion, it must receive a constant supply of *vis viva* corresponding to the resistance it has to overcome.

Clocks regulated by a pendulum obtain such a supply, either from a raised weight or a bent spring. The power consumed in raising the weight or in bending the spring, which power is represented by the raised weight or the bent spring, overcomes for a time the resistance, and thus secures the uniform motion of the pendulum and clock. In doing so, the weight sinks down or the spring uncoils, and therefore force must be expended in winding the clock up again, or it would stop moving.

Essentially the same holds good for the tidal wave. The moving waters rub against each other, against the shore, and against the atmosphere, and thus, meeting constantly with resistance, would soon come to rest if a *vis viva* did not exist competent to overcome these obstacles. This *vis viva* is the rotation of the earth on its axis, and the diminution and final exhaustion thereof will be a consequence of such an action.

*The tidal wave causes a diminution of the velocity of the rotation of the earth.*

This important conclusion can be proved in different ways.

The attraction of the sun and the moon disturbs the equilibrium of the movable parts of the earth's surface, so as to move the waters of the sea toward the point or meridian above and below which the moon culminates. If the waters could move without resistance, the elevated parts of the tidal wave would exactly coincide with the moon's meridian, and under such conditions no consumption of *vis viva* could take place. In reality, however, the moving waters experience resistance, in consequence of which the flow of the tidal wave is delayed, and high water occurs in the open sea on the average about 2½ hours after the transit of the moon through the meridian of the place.

The waters of the ocean move from west and east toward the meridian of the moon, and the more elevated wave is, for the reason above stated, always to the east of the moon's meridian; hence the sea must press and flow more powerfully from east to west than from west to east. The ebb and flow of the tidal wave therefore consists not only in an alternate rising and falling of the waters, but also in a slow progressive motion from east to west. The tidal wave produces a general westerly current in the ocean.

This current is opposite in direction to the earth's rotation, and therefore its friction against and collision with the bed and shores of the ocean must offer everywhere resistance to the axial rotation of the earth, and diminish the *vis viva* of its motion. The earth here plays the part of a fly-wheel. The movable parts of its surface adhere, so to speak, to the relatively fixed moon, and are dragged in a direction opposite to that of the earth's rotation, in consequence of which, action takes place between the solid and liquid parts of this fly-wheel, resistance is overcome, and the given rotatory effect diminished.

Water-mills have been turned by the action of the tides; the effects produced by such an arrangement are distinguished in a remarkable manner from those of a mill turned by a mountain-stream. The one obtains the *vis viva* with which it works from the earth's rotation, the other from the sun's radiation.

Various causes combine to incessantly maintain, partly in an undulatory, partly in a progressive motion, the waters of the ocean. Besides the influence of the sun and the moon on the rotating earth, mention must be made of the influence of the movement of the lower strata of the atmosphere on the surface of the ocean, and of the different temperatures of the sea in various climates; the configuration of the shores and the bed of the ocean likewise exercise a manifold influence on the velocity, direction, and extent of the oceanic currents.

The motions in our atmosphere, as well as those of the ocean, presuppose the existence and consumption of *vis viva* to overcome the continual resistances, and to prevent a state of rest or equilibrium. Generally speaking, the power necessary for the production of aërial currents may be of threefold origin. Either the radiation of the sun, the heat derived from a store in the interior of the earth, or, lastly, the rotatory effect of the earth may be the source.

As far as quantity is concerned, the sun is by far the most important of the above. According to Pouillet's measurements, a square metre of the earth's surface receives on the average 4·408 units of heat from the sun per minute. Since one unit of heat is equivalent to 367 Km, it follows that one square metre of the surface of our globe receives per minute an addition of *vis viva* equal to 1620 Km, or the whole of the earth's surface in the same time 825,000 billions of Km. A power of 75 Km per second is called a horse-power. According to this, the effect of the solar radiation in mechanical work on one square metre of the earth's surface would be equal to 0·36, and the total effect for the whole globe 180 billions of horse-powers. A not inconsiderable portion of this enormous quantity of *vis viva* is consumed in the production of atmospheric actions, in consequence of which numerous motions are set up in the earth's atmosphere.

In spite of their great variety, the atmospheric currents may be reduced to a single type. In consequence of the unequal heating of the earth in different degrees of latitude, the colder and heavier air of the polar regions passes in an under current toward the equator; whereas the heated air of the tropics ascends to the higher parts of the atmosphere, and flows from thence toward the poles. In this manner the air of each hemisphere performs a circuitous motion.

It is known that these currents are essentially modified by the motion of the earth on its axis. The polar currents, with their smaller rotatory velocity, receive a motion from east to west contrary to the earth's rotation, and the equatorial currents one from west to east in advance of the axial rotation of the earth. The former of these currents, the easterly winds, must diminish the rotatory effect of the globe, the latter, the westerly winds, must increase the same power. The final result of the action of these opposed influences is, as regards the rotation of the earth, according to well-known mechanical principles, $= 0$; for these currents counteract each other, and therefore cannot exert the least influence on the axial rotation of the earth. This important conclusion was proved by Laplace.

The same law holds good for every imaginable action which is caused either by the radiant heat of the sun, or by the heat which reaches the surface from the earth's interior, whether the action be in the air, in the water, or on the land. The effect of every single motion produced by these means on the rotation of the globe, is exactly compensated by the effect of another motion in an opposite direction; so that the resultant of all these motions is, as far as the axial rotation of the globe is concerned, $= 0$.

In those actions known as the tides, such compensation, however, does not take place; for the pressure or pull by which they are produced is always stronger from east to west than from west to east. The currents caused by this pull may ebb and flow in different directions, but their motion predominates in that which is opposed to the earth's rotation.

The velocity of the currents caused by the tide of the atmosphere amounts, according to Laplace's calculation, to not more than 75 millimetres in a second, or nearly a geographical mile in twenty-four hours; it is clear that much more powerful effects produced by the sun's heat would hide this action from observation. The influence of these air-currents, however, on the rotatory effect of the earth, is, according to the laws of mechanics, exactly the same as it would be were the atmosphere undisturbed by the sun's radiant heat.

The combined motions of air and water are to be regarded from the same point of view. If we imagine the influence of the sun and that of the interior of our globe not to exist, the motion of the air and ocean from east to west is still left as an obstacle to the axial rotation of the earth.

The motion of the waters of the ocean from east to west was long ago verified by observation, and it is certain that the tides are the most effectual of the causes to which this great westerly current is to be referred.

Besides the tidal wave, the lower air-currents moving in the same direction, the trade-winds of the tropics especially, may be assigned as causes of this general movement of the waters. The westerly direction of the latter, however, is not confined to the region of easterly winds; it is met with in the region of perpetual calms, where it possesses a velocity of several miles a day; it is observed far away from the tropics both north and south, in regions where westerly winds prevail, near the Cape of Good Hope, the Straits of Magellan, the Arctic regions, &c.

A third cause for the production of a general motion of translation of the waters of the ocean is the unequal heating of the sea in different zones. According to the laws of hydrostatics, the colder water of the higher degrees of latitude is compelled to flow toward the equator, and the warmer water of the tropics toward the poles, in consequence of which, similar movements are produced in the ocean to those in the atmosphere. This is the cause of the cold under current from the poles to the equator, and of the warm surface-current from the equator to the poles. The waters of the latter, by virtue of the greater velocity of rotation at the equator, assume in their onward progress a direction from west to east. It is a striking proof of the preponderating influence of the tidal wave that, in spite of this, the motion of the ocean is on the whole in an opposite direction.

Theory and experience thus agree in the result that the influence of the moon on the rotating earth causes a motion of translation from east to west in both atmosphere and ocean. This motion must continually diminish the rotatory effect of the earth, for want of an opposite and compensating influence.

The continual pressure of the tidal wave against the axial rotation of the earth may also be deduced from statical laws.

The gravitation of the moon affects without exception all parts of the globe. Let the earth be divided by the plane of the meridian in which the moon happens to be, into two hemispheres, one to the east, the other to the west of this meridian. It is clear that the moon, by its attraction of the eastern hemisphere, tends to retard the motion of the earth, and by its attraction of the western hemisphere, to accelerate the same rotation.

Under certain conditions, these tendencies compensate each other, and then the action of the moon on the earth's rotation becomes zero. This happens when both hemispheres are arranged in a certain manner symmetrically, or when no parts of the earth can change their relative position; in the latter case a sort of symmetry is produced by the rotation.

The form of the earth deviates from a perfectly symmetrical sphere on account of the three following causes:—(1) the flattening of the poles, (2) the mountains on the surface, and (3) the tidal wave. The first two causes do not change the velocity of the earth's axial rotation. In order to comprehend clearly the effect of the tidal wave, we shall imagine the earth to be a perfectly symmetrical sphere uniformly surrounded by water. The attraction of the sun and the moon disturbs the equilibrium of this mass, and two flat mountains of water are formed. The top of one of these is directed toward the moon, and the summit of the other is turned away from it. A straight line passing through the tops of these two mountains is called the major axis of this earth-spheroid.

In this state the earth may be imagined to be divided into three parts—a smaller sphere, and two spherical segments attached to the opposite sides of the latter, and representing the elevations of the tidal wave. The attraction of the moon on the small central sphere does not change the rotation, and we have therefore only to consider the influence of this attraction on the two tidal elevations. The upper elevation, or mountain, the one nearest the moon, is attracted toward the west because its mass is principally situated to the east of the moon, and the opposite mountain, which is to the west of the moon, is attracted toward the east. The upper tidal elevation is not only more powerfully attracted because it is nearer to the moon, but also because the angle under which it is pulled aside is more favorable for lateral deflection than in the case of the opposite protuberance. The pressure from east to west of the upper elevation preponderates therefore over the pressure from west to east of the opposite mountain; according to calculation, these quantities stand to each other nearly as 14 to 13. From the relative position of these two tidal protuberances and the moon, or the unchangeable position of the major axis of the earth-spheroid toward the centre of gravity of the moon, a pressure results, which preponderates from east to west, and offers an obstacle to the earth's rotation.

If gravitation were to be compared with magnetic attraction, the earth might be considered to be a large magnet, one pole of which, being more powerfully attracted, would represent the upper, and the other pole the lower tidal elevation. As the upper tidal wave tends to move toward the moon, the earth would act like a galvanometer, whose needle has been deflected from the magnetic meridian, and which, while tending to return thereto, exerts a constant lateral pressure.

The foregoing discussion may suffice to demonstrate the influence of the moon on the earth's rotation. The retarding pressure of the tidal wave may quantitatively be determined in the same manner as that employed in computing the precession of the equinoxes and the nutation of the earth's axis. The varied distribution of land and water, the unequal and unknown depth of the ocean, and the as yet imperfectly ascertained mean difference between the time of the moon's culmination and that of high water in the open sea, enter, however, as elements into such a calculation, and render the desired result an uncertain quantity.

In the mean time, this retarding pressure, if imagined to act at the equator, cannot be assumed to be less than 1000 millions of kilogrammes. In order to start with a definite conception, we may be allowed to use this round number as a basis for the following calculations.

The rotatory velocity of the earth at the equator is 464 metres, and the consumption of mechanical work, therefore, for the maintenance of the tides, 464,000 millions of Km, or 6000 millions of horse-powers per second. The effect of the tides may consequently be estimated at $\frac{1}{30,000}$th of the effect received by the earth from the sun.

The rotatory effect which the earth at present possesses, may be calculated from its mass, volume, and velocity of rotation. The volume of the earth is 2,650,686,000 cubic miles, and its specific gravity, according to Reich, = 5·44. If, for the sake of simplicity, we assume the density of the earth to be uniform throughout its mass, we obtain from the above premises, and the known velocity of rotation, 25,840 quadrillions of kilogrammetres as the rotatory effect of the earth. If, during every second in 2500 years, 464,000 millions of Km of this effect were consumed by the ebb and flow of the tidal wave, it would suffer a diminution of 36,600 trillions of Km, or about $\frac{1}{700,000}$th of its quantity.

The velocities of rotation of a sphere stand to each other in the same ratio as the square roots of the rotatory effects, when the volume of the sphere remains constant. From this it follows that, in the assumed time of 2500 years, the length of a day has increased $\frac{1}{1,400,000}$th; or if a day be taken equal to 86,400 seconds, it has lengthened $\frac{1}{16}$th of a second, if the volume of the earth has not changed. Whether this supposition be correct or not, depends on the temperature of our planet, and will be discussed in the next chapter.

The tides also react on the motion of the moon. The stronger attraction of the elevation nearest to, and to the east of the moon, increases with the tangential velocity of our satellite; the mean distance of the earth and the moon, and the time of revolution of the latter, are consequently augmented. The effect of this action, however, is insignificant, and, according to calculation, does not amount to more than a fraction of a second in the course of centuries.

## IX. *The Heat of the Interior of the Earth.*

Without doubt there was once a time when our globe had not assumed its present magnitude. According to this, by aid of this simple assumption, the origin of our planet may be reduced to the union of once separated masses.

To the mechanical combinations of masses of the second order, with masses of the second and third order, &c., the same laws as those enunciated for the sun apply. The collision of such masses must always generate an amount of heat proportional to the squares of their velocities, or to their mechanical effect.

Although we are not in a position to affirm anything certain respecting the primordial conditions under which the constituent parts of the earth existed, it is nevertheless of the greatest interest to estimate the quantities of heat generated by the collision and combination of these parts by a standard based on the simplest assumptions.

Accordingly we shall for the present consider the earth to have been formed by the union of two parts, which obtained their relative motions by their mutual attraction only. Let the whole mass of the present earth, expressed in kilogrammes, be T, and the masses of the two portions $T-x$ and $x$. The ratio of these two quantities may be imagined to assume various values. The two extreme cases are, when $x$ is considered infinitely small in comparison with T, and when $x=T-x=\frac{1}{2}T$. These form the limits of all imaginable ratios of the parts $T-x$ and $x$, and will now be more closely examined.

Terrestrial heights are of course excluded from the following consideration. In the first place let $x$, in comparison with $T-x$, be infinitely small. The final velocity with which $x$ arrives on the surface of the large mass, after having passed through a great space in a straight line, or after previous central motion round it, is, according to the laws developed in relation to the sun in Chapter IV, confined within the limits of 7908 and 11,183 metres. The heat generated by this process may amount to from $8685 \times x$ to $17,370 \times x$ units, according to the value of the major axis of the orbit of $x$. This heat, however, vanishes by its distribution through the greater mass, because $x$ is, according to supposition, infinitely small in comparison with T.

The quantity of heat generated increases with $x$, and amounts in the second case, when $x=\frac{1}{2}T$, to from $6000 \times T$ to $8685 \times T$ units.

If we assume the earth to possess a very great capacity for heat, equal in fact to that of its volume of water, which when calculated for equal weights = 0·184, the above discussion leads to the conclusion that the difference of temperature of the constituent parts, and of the earth after their union, or, in other words, the heat generated by the collision of these parts, may range, according to their relative magnitude, from 0° to 32,000°, or even to 47,000°!

With the number of parts which thus mechanically combine, the quantity of heat developed increases. Far greater still would have been the generation of heat if the constituent parts had moved in separate orbits round the sun before their union, and had accidentally approached and met each other. For various reasons, however, this latter supposition is not very probable.

Several facts indicate that our earth was once a fiery liquid mass, which has since cooled gradually, down to a comparatively inconsiderable depth from the surface, to its present temperature. The first proof of this is the form of the earth. "The form of the earth is its history." According to the most careful measurements, the flattening at the poles is exactly such as a liquid mass rotating on its axis with the velocity of the earth would possess; from this we may conclude that the earth, at the time it received its rotatory motion, was in a liquid state; and, after much controversy, it may be considered as settled that this liquid condition was not that of an aqueous solution, but of a mass melted by a high temperature.

The temperature of the crust of the globe likewise furnishes proof of the existence of a store of heat in its interior. Many exact experiments and measurements show that the temperature of the earth increases with the depth to which we penetrate. In boring the artesian well at Grenelle, which is 546 metres deep, it was observed that the temperature augmented at the rate of 1° for every 30 metres. The same result was obtained by observations in the artesian well at Mondorf in Luxembourg: this well is 671 metres in depth, and its water 34° warm.

Thermal springs furnish a striking proof of the high temperature existing in the interior of the earth. Scientific men are agreed that the aqueous deposits from the atmosphere, rain, hail, dew, and snow, are the sole causes of the formation of springs. The water, obeying the laws of gravity, percolates through the earth wherever it can, and reappears at the surface in places of a lower situation. When water sinks to considerable depths through vertical crevices in the rocks, it acquires the temperature of the surrounding strata, and returns as a thermal spring to the surface.

Such waters are frequently distinguished from the water of ordinary springs merely by their possessing a higher temperature. If, however, the water in its course meets with mineral or organic substances which it can dissolve and retain, it then reappears as a mineral spring. Examples of such are met with at Aachen, Carlsbad, &c.

In a far more decided manner than by the high temperature of the water of certain springs, the interior heat of our globe is made manifest by those fiery fluid masses which sometimes rise from considerable depths. The temperature of the earth's crust increases at the rate of 1° for every 30 metres we descend from the surface toward the centre. Although it is incredible that this augmentation can continue at the same rate till the centre be reached, we may nevertheless assume with certainty that it does continue to a considerable depth. Calculation based on this assumption shows that at a depth of a few miles a temperature must exist sufficiently powerful to fuse most substances. Such molten masses penetrate the cold crust of the globe in many places, and make their appearance as lava.

A distinguished scientific man has lately expressed himself on the origin of the interior heat of the earth as follows:—"No one of course can explain the final causes of things. This much, however, is clear to every thinking man, that there is just as much reason that a body, like the earth for example, should be warm, warmer than ice or human blood, as there is that it should be cold or colder than the latter. A particular cause for this absolute heat is as little necessary as a cause for motion or rest. Change—that is to say, transition from one state of things to another—alone requires and admits of explanation."

It is evident that this reflection is not fitted to suppress the desire for an explanation of the phenomenon in question. As all matter has the tendency to assume the same temperature as that possessed by the substances by which it happens to be surrounded, and to remain in a quiescent state as soon as equilibrium has been established, we must conclude that, whenever we meet with a body warmer than its neighbors, such body must have received at a (relatively speaking) not far distant time, a certain degree of heat,—a process which certainly allows of, and requires explanation.

Newton's theory of gravitation, whilst it enables us to determine, from its present form, the earth's state of aggregation in ages past, at the same time points out to us a source of heat powerful enough to produce such a state of aggregation, powerful enough to melt worlds; it teaches us to consider the molten state of a planet as the result of the mechanical union of cosmical masses, and thus to derive the radiation of the sun and the heat in the bowels of the earth from a common origin.

The rotatory effect of the earth also may be readily explained by the collision of its constituent parts; and we must accordingly subtract the *vis viva* of the axial rotation from the whole effect of the collision and mechanical combination, in order to obtain the quantity of heat generated. The rotatory effect, however, is only a small quantity in comparison with the interior heat of the earth. It amounts to about $4400 \times T$ kilogrammetres, (T being the weight of the earth in kilograms) which is equivalent to $12 \times T$ units of heat, if we assume the density of the earth to be uniform throughout.

If we imagine the moon in the course of time, either in consequence of the action of a resisting medium, or from some other cause, to unite herself with our earth, two principal effects are to be discerned. A result of the collision would be, that the whole mass of the moon and the cold crust of the earth would be raised some thousands of degrees in temperature, and consequently the surface of the earth would be converted into a fiery ocean. At the same time, the velocity of the earth's axial rotation would be somewhat accelerated, and the position of its axis with regard to the heavens, and to its own surface, slightly altered. If the earth had been a cold body without axial rotation, the process of its combining with the moon would have imparted to it both heat and rotation.

It is probable that such processes of combination between different parts of our globe may have repeatedly happened before the earth attained its present magnitude, and that luxuriant vegetation may have at different times been buried under the fiery debris resulting from the conflict of these masses.

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As long as the surface of our globe was in an incandescent state, it must have lost heat at a very rapid rate; gradually this process became slower; and although it has not yet entirely ceased, the rate of cooling must have diminished to a comparatively small magnitude.

Two phenomena are caused by the cooling of the earth, which, on account of their common origin, are intimately related. The decrease of temperature, and consequent contraction of the earth's crust, must have caused frequent disturbances and revolutions on its surface, accompanied by the ejection of molten masses and the formation of protuberances; on the other hand, according to the laws of mechanics, the velocity of rotation must have increased with the diminution of the volume of the sphere, or, in other words, the cooling of the earth must have shortened the length of the day.

As the intensity of such disturbances and the velocity of rotation are closely connected, it is clear that the youth of our planet must have been distinguished by continual violent transformations of its crust, and a perceptible acceleration of the velocity of its axial rotation; while in the present time the metamorphoses of its surface are much slower, and the acceleration of its axial revolution diminished to a very small amount.

If we imagine the times when the Alps, the chain of the Andes, and the Peak of Teneriffe were upheaved from the deep, and compare with such changes the earthquakes and volcanic eruptions of historic times, we perceive in these modern transformations but weak images of the analogous processes of bygone ages.

While we are surrounded on every side by the monuments of violent volcanic convulsions, we possess no record of the velocity of the axial rotation of our planet in antediluvian times. It is of the greatest importance that we should have an exact knowledge of a change in this velocity, or in the length of the day during historic times. The investigation of this subject by the great Laplace forms a bright monument in the department of exact science.

These calculations are essentially conducted in the following manner:—In the first place, the time between two eclipses of the sun, widely apart from each other, is as accurately as possible expressed in days, and from this the ratio of the time of the earth's rotation to the mean time of the moon's revolution determined. If, now, the observations of ancient astronomers be compared with those of our present time, the least alteration in the absolute length of a day may be detected by a change in this ratio, or in a disturbance in the lunar revolution. The most perfect agreement of ancient records on the movements of the moon and the planets, on the eclipses of the sun, &c., revealed to Laplace the remarkable fact that in the course of twenty-five centuries, the time in which our earth revolves on its axis has not altered $\frac{1}{500}$th part of a sexagesimal second; and the length of a day therefore may be considered to have been constant during historic times.

This result, as important as it was convenient for astronomy, was nevertheless of a nature to create some difficulties for the physicist. With apparently good reason it was concluded that, if the velocity of rotation had remained constant, the volume of the earth could have undergone no change. The earth completes one revolution on its axis in 86,400 sidereal seconds; it consequently appears, if this time has not altered during 2500 years to the extent of $\frac{1}{500}$th of a second, or $\frac{1}{43,000,000}$th part of a day, that during this long space of time the radius of the earth also cannot have altered more than this fraction of its length. The earth's radius measures 6,369,800 metres, and therefore its length ought not to have diminished more than 15 centimetres in twenty-five centuries.

The diminution in volume, as a result of the cooling-process, is however, closely connected with the changes on the earth's surface. When we consider that scarcely a day passes without the occurrence of an earthquake or shock in one place or another, and that of the three hundred active volcanos some are always in action, it would appear that such a lively reaction of the interior of the earth against the crust is incompatible with the constancy of its volume.

This apparent discrepancy between Cordier's theory of the connexion between the cooling of the earth and the reaction of the interior on the exterior parts, and Laplace's calculation showing the constancy of the length of the day, a calculation which is undoubtedly correct, has induced most scientific men to abandon Cordier's theory, and thus to deprive themselves of any tenable explanation of volcanic activity.

The continued cooling of the earth cannot be denied, for it takes place according to the laws of nature; in this respect the earth cannot comport itself differently from any other mass, however small it be. In spite of the heat which it receives from the sun, the earth will have a tendency to cool so long as the temperature of its interior is higher than the mean temperature of its surface. Between the tropics the mean temperature produced by the sun is about 28°, and the sun therefore is as little able to stop the cooling-tendency of the earth as the moderate warmth of the air can prevent the cooling of a red-hot ball suspended in a room.

Many phenomena—for instance, the melting of the glaciers near the bed on which they rest—show the uninterrupted emission of heat from the interior toward the exterior of the earth; and the question is, Has the earth in twenty-five centuries actually lost no more heat than that which is requisite to shorten a radius of more than six millions of metres only 15 centimetres?

In answering this question, three points enter into our calculation:—(1) the absolute amount of heat lost by the earth in a certain time, say one day; (2) the earth's capacity for heat; and (3) the coëfficient of expansion of the mass of the earth.

As none of these quantities can be determined by direct measurements, we are obliged to content ourselves with probable estimates; these estimates will carry the more weight the less they are formed in favor of some preconceived opinion.

Considering what is known about the expansion and contraction of solids and liquids by heat and cold, we arrive at the conclusion that for a diminution of 1° in temperature, the linear contraction of the earth cannot well be less than $\frac{1}{100,000}$th part, a number which we all the more readily adopt because it has been used by Laplace, Arago, and others.

If we compare the capacity for heat of all solid and liquid bodies which have been examined, we find that, both as regards volume and weight, the capacity of water is the greatest. Even the gases come under this rule; hydrogen, however, forms an exception, it having the greatest capacity for heat of all bodies when compared with an equal weight of water. In order not to take the capacity for heat of the mass of the earth too small, we shall consider it to be equal to that of its volume of water, which, when calculated for equal weights, amounts to 0·184.[1]

If we accept Laplace's result, that the length of a day has remained constant during the last 2500 years, and conclude that the earth's radius has not diminished $1\frac{1}{2}$ decimetre in consequence of cooling, we are obliged to assume, according to the premises stated, that the mean temperature of our planet cannot have decreased $\frac{1}{430}°$ in the same period of time.

The volume of the earth amounts to 2650 millions of cubic miles. A loss of heat sufficient to cool this mass $\frac{1}{430}°$ would be equal to the heat given off when the temperature of 6,150,000 cubic miles of water decreases 1°; hence the loss for one day would be equal to 6·74 cubic miles of heat.

Fourier has investigated the loss of heat sustained by the earth. Taking the observation that the temperature of the earth increases at the rate of 1° for every 30 metres as the basis of his calculations, this celebrated mathematician finds the heat which the globe loses by conduction through its crust in the space of 100 years to be capable of melting a layer of ice 3 metres in thickness and covering the whole surface of the globe; this corresponds in one day to 7·7 cubic miles of heat, and in 2500 years to a decrease of 17 centimetres in the length of the radius.

According to this, the cooling of the globe would be sufficiently great to require attention when the earth's velocity of rotation is considered.

At the same time it is clear that the method employed by Fourier can bring to our knowledge only one part of the heat which is annually lost by the earth; for simple conduction through *terra firma* is not the only way by which heat escapes from our globe.

In the first place, we may make mention of the aqueous deposits of our atmosphere, which, as far as they penetrate our

[1] The capacity for heat, as well as the coëfficient of expansion of matter, as a rule, increases at higher temperatures. As, however, these two quantities act in opposite ways in our calculations, we may be allowed to dispense with the influence which the high temperature of the interior of the earth must exercise on these numbers. Even if, in consequence of the high temperature of the interior, the earth's mass could have a capacity two or three times as great as that which it has from 0° to 100°, it is to be considered, on the other hand, that the coëfficient of expansion, $\frac{1}{100,000}$. only holds good for solids, and is even small for them, whilst in the case of liquids we have to assume a much greater coëfficient: for mercury between 0° and 100°, it is about six times as great. Especially great is the contraction and expansion of bodies when they change their state of aggregation; and this should be taken into account when considering the formation of the earth's crust.

earth, wash away, so to speak, a portion of the heat, and thus accelerate the cooling of the globe. The whole quantity of water which falls from the atmosphere upon the land in one day, however, cannot be assumed to be much more than half a cubic mile in volume, hence the cooling effect produced by this water may be neglected in our calculation. The heat carried off by all the thermal springs in the world is very small in comparison with the quantities which we have to consider here.

Much more important is the effect produced by active volcanos. As the heat which accompanies the molten matter to the surface is derived from the store in the interior of the earth, their action must influence considerably the diminution of the earth's heat. And we have not only to consider here actual eruptions which take place in succession or simultaneously at different parts of the earth's surface, but also volcanos in a quiescent state, which continually radiate large quantities of heat abstracted from the interior of the globe. If we compare the earth to an animal body, we may regard each volcano as a place where the epidermis has been torn off, leaving the interior exposed, and thus opening a door for the escape of heat.

Of the whole of the heat which passes away through these numerous outlets, too low an estimate must not be made. To have some basis for the estimation of this loss, we have to recollect that in 1783, Skaptar-Jokul, a volcano in Iceland, emitted sufficient lava in the space of six weeks to cover 60 square miles of country to an average depth of 200 metres, or, in other words, about 1½ cubic mile of lava. The amount of heat lost by this one eruption of one volcano must, when the high temperature of the lava is considered, be estimated to be more than 1000 cubic miles of heat; and the whole loss resulting from the action of all the volcanos amounts, therefore, in all probability, to thousands of cubic miles of heat per annum. This latter number, when added to Fourier's result, produces a sum which evidently does not agree with the assumption that the volume of our earth has remained unchanged.

In the investigation of the cooling of our globe, the influence of the water of the ocean has to be taken into account. Fourier's calculations are based on the observations of the increase of the temperature of the crust of our earth, from the surface toward the centre. But two-thirds of the surface of our globe are covered with water, and we cannot assume *à priori* that this large area loses heat at the same rate as the solid parts; on the contrary, various circumstances indicate that the cooling of our globe proceeds more quickly through the waters of the ocean resting on it than from the solid parts merely in contact with the atmosphere.

In the first place, we have to remark that the bottom of the ocean is, generally speaking, nearer to the store of heat in the interior of the earth than the dry land is, and hence that the temperature increases most probably in a greater ratio from the bottom of the sea toward the interior of the globe, than it does in our observations on the land. Secondly, we have to consider that the whole bottom of the sea is covered by a layer of ice-cold water, which moves constantly from the poles to the equator, and which, in its passage over sand-banks, causes, as Humboldt aptly remarks, the low temperatures which are generally observed in shallow places. That the water near the bottom of the sea, on account of its great specific heat and its low temperature, is better fitted than the atmosphere to withdraw the heat from the earth, is a point which requires no further discussion.

We have plenty of observations which prove that the earth suffers a great loss of heat through the waters of the ocean. Many investigations have demonstrated the existence of a large expanse of sea, much visited by whalers, situated between Iceland, Greenland, Norway, and Spitzbergen, and extending from lat. 76° to 80° N., and from long. 15° E. to 15° W. of Greenwich, where the temperature was observed to be higher in the deeper water than near the surface—an experience which neither accords with the general rule, nor agrees with the laws of hydrostatics. Franklin observed, in lat. 77° N. and long. 12° E., that the temperature of the sea near the surface was $-\frac{1}{2}°$, and at a depth of 700 fathoms $+6°$. Fisher, in lat. 80° N. and long. 11° E., noticed that the surface-water had a temperature of 0°, whilst at a depth of 140 fathoms it stood at $+8$.

As sea-water, unlike pure water, does not possess a point of greatest density at some distance above the freezing-point, and as the water in lat. 80° N. is found at some depth to be warmer than water at the same depth 10° southward, we can only explain this remarkable phenomenon of an increase of temperature with an increase of depth by the existence of a source of heat at the bottom of the sea. The heat, however, which is required to warm the water at the bottom of an expanse of ocean more than 1000 square miles in extent to a sensible degree, must amount, according to the lowest estimate, to some cubic miles of heat a day.

The same phenomenon has been observed in other parts of the world, such as the west coast of Australia, the Adriatic, the Lago Maggiore, &c. Especial mention should here be made of an observation by Horner, according to whom, the lead, when hauled up from a depth varying from 80 to 100 fathoms in the mighty Gulf-stream off the coast of America, used to be hotter than boiling water.

The facts above mentioned, and some others which might be added, clearly show that the loss of heat suffered by our globe during the last 2500 years is far too great to have been without sensible effect on the velocity of the earth's rotation. The reason why, in spite of this accelerating cause, the length of a day has nevertheless remained constant since the most ancient times, must be attributed to an opposite retarding action. This consists in the attraction of the sun and moon on the liquid parts of the earth's surface, as explained in the last chapter.

According to the calculations of the last chapter, the retarding pressure of the tides against the earth's rotation would cause, during the lapse of 2500 years, a sidereal day to be lengthened to the extent of $\frac{1}{16}$th of a second; as the length of a day, however, has remained constant, the cooling effect of the earth during the same period of time must have shortened the day $\frac{1}{16}$th of a second. A diminution of the earth's radius to the amount of $4\frac{1}{2}$ metres in 2500 years, and a daily loss of 200 cubic miles of heat, correspond to this effect. Hence, in the course of the last twenty-five centuries, the temperature of the whole mass of the earth must have decreased $\frac{1}{14}°$.

The not inconsiderable contraction of the earth resulting from such a loss of heat, agrees with the continual transformations of the earth's surface by earthquakes and volcanic eruptions; and we agree with Cordier, the industrious observer of volcanic processes, in considering these phenomena a necessary consequence of the continual cooling of an earth which is still in a molten state in its interior.

When our earth was in its youth, its velocity of rotation must have increased to a very sensible degree, on account of the rapid cooling of its then very hot mass. This accelerating cause gradually diminished, and as the retarding pressure of the tidal wave remains nearly constant, the latter must finally preponderate, and the velocity of rotation therefore continually decrease. Between these two states we have a period of equilibrium, a period when the influence of the cooling and that of the tidal pressure counterbalance each other; the whole life of the earth therefore may be divided into three periods—youth with increasing, middle age with uniform, and old age with decreasing velocity of rotation.

The time during which the two opposed influences on the rotation of the earth are in equilibrium can, strictly speaking, only be very short, inasmuch as in one moment the cooling, and in the next moment the pressure of the tides must prevail. In a physical sense, however, when measured by human standards, the influence of the cooling. and still more so that of the tidal wave, may for ages be considered constant, and there must consequently exist a period of many thousand years' duration during which these counteracting influences will appear to be equal. Within this period, a sidereal day attains its shortest length, and the velocity of the earth's rotation its maximum—

circumstances which, according to mathematical analysis, would tend to lengthen the duration of this period of the earth's existence.

The historical times of mankind are, according to Laplace's calculation, to be placed in this period. Whether we are at the present moment still near its commencement, its middle, or are approaching its conclusion, is a question which cannot be solved by our present data, and must be left to future generations.

The continual cooling of the earth cannot be without an influence on the temperature of its surface, and consequently on the climate; scientific men, led by Buffon, in fact, have advanced the supposition that the loss of heat sustained by our globe must at some time render it an unfit habitation for organic life. Such an apprehension has evidently no foundation, for the warmth of the earth's surface is even now much more dependent on the rays of the sun than on the heat which reaches us from the interior. According to Pouillet's measurements, mentioned in Chapter III, the earth receives 8000 cubic miles of heat a day from the sun, whereas the heat which reaches the surface from the earth's interior may be estimated at 200 cubic miles per diem. The heat therefore obtained from the latter source every day is but small in comparison to the diurnal heat received from the sun.

If we imagine the solar radiation to be constant, and the heat we receive from the store in the interior of the earth to be cut off, we should have as a consequence various changes in the physical constitution of the surface of our globe. The temperature of hot springs would gradually sink down to the mean temperature of the earth's crust, volcanic eruptions would cease, earthquakes would no longer be felt, and the temperature of the water of the ocean would be sensibly altered in many places—circumstances which would doubtless affect the climate in many parts of the world. Especially, it may be presumed that Western Europe, with its pleasant favorable climate, would become colder, and thus *perhaps* the seat of the power and culture of our race transferred to the milder parts of North America.

Be this as it may, for thousands of years to come we can predict no diminution of the temperature of the surface of our globe as a consequence of the cooling of its interior mass; and, so far as historic records teach, the climates, the temperatures of thermal springs, and the intensity and frequency of volcanic eruptions are now the same as they were in the far past.

It was different in prehistoric times, when for centuries the earth's surface was heated by internal fire, when mammoths lived in the now uninhabitable polar regions, and when the tree-ferns and the tropical shell-fish, whose fossil remains are now especially preserved in the coal-formation, were at home in all parts of the world.

ART. XLII.—*Notice of a new fossil Annelid (Helminthodes antiquus), from the Lithographic Slates of Solenhofen;* by O. C. MARSH, F.G.S., of New Haven, Ct.

DURING a geological excursion which I recently made through the south of Germany, I spent several days at the lithographic quarries of Solenhofen, in Bavaria, and was so fortunate as to obtain a rich suite of fossils from that well known locality.

One of the most interesting specimens in the collection is a new Annelid, which is so well preserved that not merely the outer form, but also the inner structure, can be determined with considerable certainty. The fossil is about $3\frac{1}{2}$ inches in length, and $\frac{3}{8}$ of an inch in breadth. The alimentary canal is straight, of nearly equal size throughout the body, and appears to be filled with its original contents.

This is, I believe, the first instance in which any part of an Annelid itself has been found preserved; the fossil remains hitherto referred to this class being either calcareous tubes allied to *Tubicola*, or certain impressions, tracks, and borings attributed to Annelids, but most of them more or less problematical as regards their origin.

At the last meeting of the Geological Society of Germany, held here on the 6th inst., I mentioned the discovery of this specimen; and, as it was evidently quite different from anything previously described, I proposed for the species the name *Helminthodes antiquus.*

A careful comparison with living forms will probably be necessary to determine the true position of this fossil among the Annelids, to which class it undoubtedly belongs, although some points in its structure seem to indicate other affinities. A full description, with illustrations, will soon be ready for publication in the American Journal of Science.

Berlin University, July 12, 1864.

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# SCIENTIFIC INTELLIGENCE.

## I. CHEMISTRY AND PHYSICS.

1. *On the wave-lengths of the luminous and ultra-violet rays.*—MASCART has presented to the French Academy a memoir on wave-lengths, containing some new determinations of considerable interest and apparently of much accuracy. The author employed a prism of iceland spar, which permits the ultra-violet rays to pass with even greater facility than quartz, while the high dispersive power for the ordinary ray allows a very large number of lines to be distinctly seen. The author has drawn about

seven hundred lines more refrangible than H, selecting from among them, for the sake of comparison, the lines L, M, N, O, P, Q, R, already employed by physicists, as well as two others, S and T, still more refrangible and not previously studied. The wave-lengths were determined by means of a ruled glass, executed by Nobert, and containing about 44 lines to the millimeter. The following table gives the results of the measurements in thousandths of a millimeter:

| | | | | | |
|---|---|---|---|---|---|
| B | 0·68667 | F | 0·48596 | N | 0·35802 |
| C | 0·65607 | G | 0·43075 | O | 0·34401 |
| D | 0·58880 | H | 0·39672 | P | 0·33602 |
| E | 0·52678 | L | 0·38190 | Q | 0·32856 |
| *b* | 0·51655 | M | 0·37288 | R | 0·31775 |

The numbers contained in this table are the means of ten series of experiments which closely agreed. The author believes them exact to at least half a unit in the fourth significant figure, except in the case of the ultra-violet rays where the observation is more difficult. The numbers are in general a little higher than those of Fraunhofer in his second series. —*Comptes Rendus*, lviii, 1111. W. G.

2. *On the determination of wave lengths by means of interference bands.* —BERNARD has re-invented and presented to the Academy of Sciences as new, the method employed by Esselbach for the determination of wave-lengths by means of a special application of the principle of interferences. As the method in question is likely to be extensively employed hereafter, and as it has been in some respects extended in its application by Bernard, we shall here give it in full. The author has applied it in two somewhat different forms. A plate of spar 1·022$^{mm}$ in thickness cut parallel to the axis, was placed perpendicularly to the incident beam between two Nichol's prisms. The axis of the plate being 45° from the principal sections of the prisms crossed at a right angle, the emergent beam possessed the maximum intensity. When this beam was analyzed by means of a spectroscope with four prisms, the spectrum was found to be traversed by bands, alternately light and dark, through the former of which the solar lines could be seen. If $m$ be the number of bands comprised between two rays corresponding to the wave-lengths $\lambda$ and $\lambda'$, $\delta$ and $\delta'$ the differences between the ordinary and extraordinary indices for these rays, and $e$ the thickness of the plate, the value of $\lambda$ may be deduced from the equation $\lambda = \dfrac{\delta}{\frac{\delta'}{\lambda'} + \frac{m}{e}}$, in which $m$ is to be taken as positive or negative according as $\lambda$ is smaller or greater than $\lambda'$. To determine the wave-length of any line in the spectrum, it is therefore only necessary to know a single wave-length, as for instance that of D, and the quantities $\delta$, $\delta'$, $e$, given directly by observation and determined once for all for the same plate. It is only necessary to count the number of included lines $m$. Another and simpler method of applying the same principle is the following. In front of the object-glass of the collimator, and adapted to it, is placed a screen having a rectangular opening about two centimeters in height and seven millimeters wide. The difference in path between the two interfering rays is produced by a plate of quartz 0·999$^{mm}$ in thickness. This plate is attached by wax to the screen so as to cover

half the width of the opening with a height of five millimeters; the inner border of the plate was parallel to the slit of the collimator. The very well defined and numerous bands obtained in this manner occupied only one-fourth of the spectrum in height, and formed a sort of ruled ribbon, the centre of which corresponded to the middle line of the spectrum. From A to H there were more than seven hundred bands, and the interval between the two principal rays in D was equal to the width of a band. By using a blue glass, sixty bands could be counted beyond A in the extreme red, and a still greater number seen indistinctly up to the last limit of the visible spectrum. To calculate the value of $\lambda$ we replace, in the formula already given, the values $\delta$ and $\delta'$ by $n-1$ and $n_1-1$, where $n$ and $n_1$ are the indices of refraction of quartz for the ordinary rays of wave-length $\lambda$ and $\lambda'$. In this manner the author obtained the following wave-lengths, taking Fraunhofer's value for D, namely, 5888:

| | | | |
|---|---|---|---|
| A 7602 | C 6557 | E 5266 | G 4305 |
| B 6865 | D —— | F 4858 | H { 3969 border<br>3967 centre. |

Bernard suggests—and this constitutes, we think, the chief merit and only original part of his paper—that the arrangement he proposes will give a simple and precise method of classifying the spectral lines, since it is easy to measure one-tenth of the interval between two successive bands by means of a micrometer screw, and thus with a standard plate of quartz, one millimeter in thickness, to divide the spectrum between A and H into 7000 equal parts. Moreover, by a simple method of interpolation, it would be easy to deduce the wave-length of any observed line. This would unquestionably be a great improvement in the spectroscope, and would give a fixed scale deserving of general adoption. —*Comptes Rendus*, lviii, 1153. W. G.

3. *On the atomic weight of Thorium and the formula of Thoria.*—Delafontaine has determined the equivalent of thorium from numerous analyses of the sulphate. The earth employed was obtained partly from orangeite from Aröe, and partly from thorite from Lövön, the mineral being in each case resolved by sulphuric acid in the manner recommended by Marignac for cerite. The sulphate of thoria is heavy, white, and caseous, and consists of extremely fine felted needles; if allowed to stand a few days, with less water than is required for complete solution, the mass changes to clear and colorless crystals, which, according to Marignac, belong to the oblique rhombic system. The sulphate was dried at 400°–450°; the anhydrous salt gave on ignition pure thoria. As a mean of fourteen experiments, the author obtained 52·51 per cent of thoria. Determinations of the acid and water in this and another sulphate were also made, and lead to the formulas $4(ThO, SO_3)+9$ aq. and $2(ThO, SO_3)+9$ aq., which are, to say the least, unusual. The author considers it most probable that the true formula of thoria is $ThO_2$ and not ThO, as assumed by Berzelius. In the former case the equivalent of thorium, according to the above-mentioned experiments, is 115·6. Delafontaine further adduces in support of his opinion the isomorphism of thoria with stannic and titanic acids observed by Nordenskiöld and Chydenius. Upon this view, the sulphates must be written $2(ThO_2,$

$2SO_3$)+9 aq. and $ThO_2$, $2SO_3$+9 aq. Finally, the formulas of the double fluorids of potassium and thorium become, upon Delafontaine's view, $KF+ThF_2+4$ aq. and $KF+2ThF_2+4$ aq. If we take O=16, the atomic weight of thorium considered as tetratomic must be doubled and becomes 231·2. The weight of evidence certainly appears to be in favor of the formula $ThO_2$ (O=8).—*Ann. der Chemie und Pharmacie*, cxxxi, 100; from *Archives des Sciences Physiques et Naturelles*, xviii, 343.

W. G.

4. *On Yttria.*—An investigation of this rare earth by Popp in Wöhler's laboratory leads to the not unwelcome result that the metals *erbium* and *terbium*, which have long figured in our books and taunted our ignorance, have no real existence; what has hitherto been considered as a mixture of the oxyds of yttrium, erbium and terbium being merely a mixture of yttria with the oxyds of the metals of the cerium group and with small portions of the alkalies and lime. In a note, the author further states that his investigations have rendered the existence of lanthanum very doubtful, so that it is possible that the metal may be stricken from the list of elements and go the way of *donarium*, *norium*, *wasium*, and *wodanum*. The separation of yttria from the oxyds of cerium and didymium may be effected, according to Popp, by carbonate of baryta, which precipitates the latter completely in the cold, while yttria remains in solution; for the success of this process it is necessary that the cerium should be in the form of sesquioxyd, as the protoxyd is not precipitated by carbonate of baryta. Freshly precipitated pure yttria resembles hydrate of alumina, and has a pure white color without a trace of red, when perfectly free from oxyd of cerium. After ignition, the oxyd is a heavy yellowish-white powder; when pure white it contains alkali and lime. The hydrate was found to have approximately the formula YO, 2HO. Yttria is a strong base which expels ammonia from its salts on boiling, and exhibits much analogy with magnesia; all of its crystalline hydrated salts have a distinct pale rose color which is peculiar to them and does not arise from manganese or didymium. Solutions of yttrium, when examined by the spectroscope, exhibit five distinct black absorption lines, which do not correspond with those of didymium, but are characteristic for yttrium. Of these lines, one lies in the extreme red, two between the green and yellow, one between the blue and green, and one far in the violet. The author determined the equivalent of yttria by four analyses of the sulphate which closely agreed, and gave 42·015 as the equivalent of the oxyd, so that we may consider that of the metal as 34, within the limits of the errors of the analyses: this is an arithmetical mean between the equivalents of calcium and didymium. Metallic yttrium was prepared by reducing the chlorid by means of sodium. It is an iron-black powder, which when dry is blackish-gray, resembling iron reduced by hydrogen, and does not oxydize at ordinary temperatures in the air. Water oxydizes it very slowly in the cold, more rapidly by boiling. Dilute acids dissolve it with great facility and with evolution of hydrogen. Heated upon platinum foil it burns with an intense light, and this is still more remarkable when the combustion takes place in oxygen. The author has analyzed and described a large number of salts, and rendered the chemical history of the metals tolerably

complete. For the descriptions of the salts we must refer to the original memoir.—*Ann. der Chemie und Pharmacie*, lv, 179. W. G.

5. *On the separation of Cerium from Lanthanum and Didymium.*—Popp has found that cerium may be easily and completely separated from lanthanum and didymium by adding acetate of soda to the solution of the mixed chlorids, passing chlorine into the liquid, and heating till the liquid boils. After boiling a short time, the whole of the cerium is separated as a bright yellow precipitate, perfectly free from lanthanum and didymium. When much free acetic acid is present the precipitate redissolves on cooling and is again precipitated when the solution is boiled. The boiling solution is to be filtered upon a water bath funnel, and the precipitate washed with boiling water. The filtrate must remain clear when heated, otherwise the cerium is not entirely separated. In place of chlorine, a solution of hypochlorite of sodium may be used, the solution being boiled as before. The author states that when the operation is carefully conducted a single treatment is sufficient, and every trace of cerium is precipitated free from lanthanum and didymium. The oxyd of cerium precipitated appears to be a superoxyd, $CeO_2$. It contains water, and dries to a brownish-yellow mass, which yields a bright yellow powder. Popp finds that pure protoxyd of cerium, obtained by igniting the oxalate and heating the oxyd in a current of hydrogen, is a purely white powder which takes a reddish color when exposed to the air. When the superoxyd is dissolved in warm concentrated nitric acid, and the solution is precipitated by ammonia, a flesh-red hydrate of the sesquioxyd is formed, which, after settling and in large masses, possesses a dirty violet-red color. The ignited hydrate gives a deep brown-red oxyd, which is insoluble in the strongest acids, and is only dissolved by long digestion with concentrated sulphuric acid. The solution has a golden-yellow color, and gives a bright yellow crystalline precipitate with sulphate of potash. Metallic cerium may be obtained by simply igniting the oxalate in a glass tube closed at one end and excluding the air as much as possible. It is a blackish-gray metallic powder, which, on gentle heating, burns to a red oxyd. Water appears not to oxydize it.—*Ann. der Chemie und Pharmacie*, cxxxi, 359. W. G.

6. *On Wasium.*—Popp and Delafontaine have, independently of each other, examined the so-called oxyd of wasium of Bahr. Both chemists agree that the supposed new element does not exist. While, however, Popp maintains that the oxyd of wasium is a mixture of the oxyds of yttrium, cerium and didymium, Delafontaine considers it to be only oxyd of cerium. In any case, the suspicions expressed by Nicklès and by the writer of these notices in regard to wasium appear to have been well founded.—*Ann. der Chem. und Pharm.*, cxxxi, 364, 368.

W. G.

7. *Preliminary notice of a new earth.*—Bischof has discovered in a calcareous mineral a new earth which exhibits the following reactions. It is precipitated by sulphid of ammonium, completely by potash, imperfectly by ammonia, as a gelatinous bluish-white precipitate. The last precipitation is not prevented by tartaric acid, and only partially by ammonia. The precipitate by potash or ammonia is somewhat soluble in water and may therefore be washed away. Carbonate of soda gives a

white flocky precipitate. Carbonate of ammonia dissolves the earth almost completely; the remainder, which is in very small quantity, appears to exhibit other reactions. With sulphuric acid the earth gives a salt which is soluble with difficulty and crystallizes easily. The most peculiar fact connected with the new earth is the behavior of the chlorid on heating. This gives a white sublimate which is precipitated by potash, while a basic oxyd (?) remains. If this be moistened with chlorhydric acid and heated, the sublimate is again formed. The earth gives no characteristic reactions either with the blowpipe or spectroscope. The author promises a more full examination as soon as the material can be obtained, meantime no name is proposed for the supposed new element, and it is to be hoped that none will be until its existence is demonstrated beyond the possibility of doubt.—*Pogg. Ann.*, cxxii, 646. W. G.

8. *Thallium.*—The presence of thallium in the lepidolite of Moravia and the mica of Zinnwald has been ascertained by Prof. Schrötter.

9. *Refractory character of alumina and silica;* C. BISCHOF.—Pure alumina chemically prepared is less refractory than chemically pure silica, but pure native alumina is more refractory than native silica (rose quartz of Norway, rock crystal or amethyst). Basic silicates of alumina are more refractory than acid, and clays are more refractory the more alumina they contain.—*J. für prakt. Chemie*, xci, 19.

10. *On the magnetic period depending on the Sun's rotation;* by Prof. GUSTAVUS HINRICHS, Iowa State University.—In the last number of this Journal (the number for September, p. 269) there is a synopsis of a paper of Baxendell on some remarkable changes in the magnetic condition of the earth having a *variable* period of from twenty-three to thirty-two days, and being closely connected with the period of solar spots. An explanation is at the same time advanced—but depending on a series of hypotheses equal in number to the changes observed, and apparently all equally daring. A ring of nebulous matter is assumed and referred to Leverrier's hypothesis; but again this ring is considered very different from common matter, being endowed with forces that make it oscillate through a distance of three millions of miles toward and from the sun. Indeed, these hypotheses seem much more in need of an explanation than the phenomena observed.

It may therefore not be amiss to show that the principal period may be accounted for by my theory of terrestrial magnetism, which is based upon only one single hypothesis, and which seems to account for all known phenomena of terrestrial magnetism. This hypothesis is that *an electrical current is a current of ether*—a hypothesis which is apparently the legitimate result of modern science, and from which I think the principal laws of static and dynamic electricity can be analytically deduced.

From this hypothesis it follows that all heavenly bodies, having both a translatory and rotatory motion, are magnets. (See my memoir entitled *Der Erdmagnetismus als Folge der Bewegung der Erde im Aether*, Copenhagen, 1860, § 19.) Hence *the sun is a magnet.* (*Der Erdmagnetismus*, § 25.)

The solar spots are certainly great changes in the atmosphere of the sun. Hence, the amount of ether condensed by translation, as well as the distribution thereof by rotation, must, whatever the spots themselves may be, change with the size, frequency, and velocity of these spots.

But any change in the ether-current is a change in the sun's magnetism, which, by induction in the earth-magnet, produces a similar change in the latter. From this it follows:

1st. That the elements of terrestrial magnetism change in harmony with the change in the solar spots. (*Der Erdmagnetismus*, §§ 37, 38.)

2d. As the sun rotates around an axis differing from his magnetic axis (*Erdm.*, § 19), there must be a period in terrestrial magnetism equal to the *apparent* time of the sun's rotation, i. e., 27¼ days (Olmsted, *Astronomy*, § 148). This evidently is Baxendell's period, for the mean of the shortest and longest period given by him (23 and 32 days) is 27½ days —as good as identical with the above.

It remains now only to be seen whether there is any cause for the variation of this new period. In the notice of Carrington's work on the Spots on the Sun, in this Journal, July, 1864, p. 142, it is stated that the spots travel at different rates according to their distance from the sun's equator, and that the limiting parallels between which the spots are found, contract and expand somewhat correspondingly to the periods of maximum and minimum of sun-spots. Therefore it appears to us *less* bold to expect from more complete investigations on the spots and on the magnetic periods in question a full accordance between both, than to assume a diamagnetic ring oscillating through a space of three millions of miles.

Baxendell's period of 18 months might be referred to the secular rotation of the magnetic axis of the sun, but for our earth this period amounts to from ten to fourteen centuries (*Erdm.*, § 61), so that the above period for the sun seems to be smaller than the less density of the solar sphere can account for. We would, therefore, not pronounce on this question.

11. *The Electric Discharge.*—Mr. FEDDERSEN, of Leipsic, has published (*Poggendorff's Ann.*, cxiii, 437, and cxvi, 132) an interesting account of his experiments on the discharge of the Leyden jar. The light caused by the explosion was analyzed by a revolving concave mirror, silvered by Liebig's process, its radius of curvature being 500 mm. The rates of revolution varied from 30 to 100 times per second. The image of the spark thus formed was received either on a ground glass plate and examined by the eye, or on a sensitive photographic plate, where it impressed its elongated image. These experiments have thrown much light on the nature of the discharge, and seem to have settled some disputed points. Previous to this, many electricians were accustomed to regard the discharge either as continuous, or as consisting of a series of partial discharges all directed the same way. Feddersen has shown that each of these kinds of discharges really does occur under certain circumstances; but that in most cases where the circuit is metallic, the discharge is *oscillatory*, the electricity flowing not merely in one direction, but alternately to and fro. The well-known experiment of Wheatstone had induced a belief that the electric discharge, with a short metallic circuit, lasted less than the one millionth of a second, while Feddersen has found that it occupies nearly $\frac{1}{10000}$ of a second. With a short circuit of good conducting power, Feddersen finds indications that lead him to the belief that the discharge is oscillatory; that is, one half of the positive electricity from the interior of the jar passes to the exterior coating, simultaneously

one half of the negative electricity of the external coating passing to the interior of the jar; the action now however does not cease, as would be expected; the interior of the jar becomes negatively electrified, and discharges itself, the two currents of positive and negative electricity being now reversed in direction; next, the interior of the jar becomes again positively charged, and discharges itself—and so on, until the force is spent, which takes place after 30, 20, 10, or a smaller number of oscillations. If a certain number of oscillations have been observed, and the resistance of the circuit be gradually increased, the number of observed oscillations will diminish, until finally only one remains. This is the point at which the discharge ceases to be oscillatory, and becomes *continuous.* In this kind of discharge the two currents never change their primary directions; they rapidly reach a maximum, and gradually diminish, ceasing altogether when one half of the positive electricity has reached the exterior coating, and one half of the negative the interior of the jar. If now the resistance of the circuit be further gradually increased, the discharge remains continuous, but its *duration* is increased, till a second point is reached, when the nature of the discharge again alters, and the continuous discharge is succeeded by a third kind of discharge, the "*partial*," when equilibrium is effected by a series of partial sparks, the positive and negative currents preserving their primary directions to the end. This kind of discharge Feddersen obtained by the introduction into the circuit of narrow tubes filled with distilled water.

*Duration of the discharge with a short circuit of good conducting power.*—The spark thus produced is drawn out by the revolving mirror into a band, whose length of course varies with the rate of rotation; with a rate of 52 revolutions per second, the band was $27\frac{1}{2}^{mm}$ in length, and consisted of a yellowish white portion, shading into a greenish white, and this again into a red tail, the last being caused by the fine metallic particles, in cooling, passing through a red heat.

| | | | | |
|---|---|---|---|---|
| Duration of | the | yellow white | portion, | 0·00003 sec. |
| " | " | green | " | 0·00004 " |
| " | " | red | " | 0·00006 " |
| | | Total, | | 0·00013 " |

In this experiment a single Leyden jar was used, the coating being = 0·2006 square meter. It was found that increasing the length of the spark, and the area of the electrical surface, each lengthened the duration of the total discharge.

It was also found that when two jars were used, each having a coated surface = 0·2006 square meter, a tube of sulphuric acid sp. gr. 1·25, $1^{mm}$ wide and $9^{mm}$ long, being introduced into the circuit, only one oscillation remained, and with a tube $12^{mm}$ long the continuous discharge generally began.

By the introduction into the circuit of 1300 meters of copper wire, whose resistance = that of a tube of sulphuric acid sp. gr. 1·25, $1^{mm}$ wide and long, the duration of the discharge was lengthened three or four fold; it was oscillatory in its nature, and the band on the ground glass was crossed by dark spaces. These bands, sometimes 9 inches in length, furnished splendid photographs where the indications of the alternate change in the direction of the currents were pretty plain. By the introduction of tubes of sulphuric acid, the number of these bands could be reduced to one, and then by continuing the process, the discharge passed into the *continuous* as before.

Feddersen found that the length of the spark, and the amount of the charge of electricity, had no sensible effect on the duration of a *single* oscillation; with ten jars of the above mentioned electrical capacity, and a tolerably short metallic circuit, he obtained,

| | For a 4mm. spark. | For 8mm. |
|---|---|---|
| Time of one oscl. | 0·00000304 sec. | 0·00000305 sec. |

With 16 jars and a very long circuit, he obtained,

| | For a 1½mm. spark. | For 9mm. |
|---|---|---|
| Time of one oscl. | 0·0000511 sec. | 0·0000514 sec. |

The alteration of the area of the electric surface, (number of jars,) exercised an influence according to the law

$$t = a\sqrt{s}$$

$t$, being the time of a single oscillation; $a$, a constant, dependent only on the nature of the circuit and the Leyden jars, while $s$ is the number of the jars. O. N. R.

12. *Interesting Electrical Phenomenon;* by C. PIAZZI SMYTH, Astronomer Royal for Scotland.—With reference to the notice on "Photographing Electric Light,"[1] on page 272 of the last number of *The British Journal of Photography*, I beg to send you in a dry-plate picture a similar case which occurred to me on the 21st ult. (July).

I was merely trying the qualities of some newly-prepared dry plates by Mr. Nicol, by taking a window view of house-tops, and was surprised to find every chimney top surmounted by a black streak or brush; i. e., black in the negative, and therefore indicating light. Nothing of the kind was visible to the naked eye in the scene itself, as a really existent fact, nor was any similar appearance visible on the ground-glass of the camera. The appearance, therefore, did not result from any bad action of the lens, which is a very good one. The stop employed was a small one (0·3 inch), and the definition of the developed picture was extremely sharp. Again: the appearance could not be caused by smoke coming from the chimneys, because that would hardly have been luminous; not one-tenth of the whole chimneys could have had fires below them, and either smoke or rarified air would have drifted with the wind, which was blowing sensibly at the time, whilst the dark rays went upward straight as arrows. Again: that the chimneys, as chimneys, had nothing to do with it, was shown by a similar brush or ray appearing at the top of a certain little ventilator in the roof of one of the houses shown, and not out of the parts emitting air, but from the ornamental spike at the top.

This circumstance convinced me at the time that the phenomenon was an electrical one, invisible to the eye, but abundantly visible or sensible to the photographic camera, and the occasion was perfectly agreeable thereto; for it was at the conclusion of a week of unusually hot, calm

[1] On the action of very weak electric light on the iodized plate, by Prof. O. N. Rood, this Journal, March, 1864, p. 207.

weather, and the sky had that morning become clouded with forms of clouds eminently electrical.

Happily the thunder storm did not break in this neighborhood, being wafted away elsewhere; but had it broken here, the photograph tells exactly *where* the lightning was preparing to come down; and there is one tall iron chimney in the view, with the strongest ray of the whole above it, showing that that would certainly have been struck in preference to its neighbors, and, if unprovided with metal communication to the earth and water, would infallibly have caused mischief to the house to which it is attached.

I have sent a second plate, taken six days afterward, when east wind and rain had disposed of all the electricity that had been brewing in the air; and it will be seen that, although it is the same view, taken with the same camera, and with the same sort of tannin dry plate, there are no electrical brushes, or black rays, surmounting the chimney pots.—*British Journal of Photography.*

## II. MINERALOGY AND GEOLOGY.

1. *On Meteoric Irons;* by H. HAIDINGER.—Haidinger presents good reasons for considering the metallic iron of Rohitzan, another found near Kremnitz in Hungary, and another from the vicinity of Cotta in Saxony, as probably not meteoric.

He next describes a *Meteoric iron from Copiapo.* Although iron predominates in it, it consists largely of stony material, and is actually a brecciform rock—an agglomeration of fragments, about and in the interstices of which the iron is spread as if it had been introduced in a liquid or pasty state. The stony pieces vary in size from that of a grain of sand to half an inch. Meteoric pyrrhotine (*troilite* of Haidinger) is mixed with the silicates in pieces sometimes a quarter of an inch in diameter. There is also some graphite. Nickel constitutes 6·4 p. c. of the metallic part.

From the writings of Philippi, Tschudi, and Domeyko, it appears that there are numerous blocks of meteoric iron over the Chilian territory and especially through the desert region of Atacama. Prof. Joy has analyzed one found in the Andes, 50 English miles from Copiapo. His results differ essentially from those obtained by Prof. G. Rose for a meteoric iron from the Sierra of Chaco, sent by Domeyko to the Berlin Museum (*Monatsb. Acad. Berlin*, Jan. 15, 1864). An abstract of the memoir of Domeyko on the meteoric irons of Chili is given in the *Comptes Rendus*, March 8, 1863.

The paper takes up next the *Iron of Sarepta*, Southern Russia. The surface of a plate cut from this iron, examined by reflected light, shows a structure distinctly crystalline-granular, like that of the meteoric iron of Arva (Northern Hungary). An analysis afforded Iron 95·937, schreibersite 1·315, tin 0·017, silicium 0·820.—*Les Mondes*, July 28, 1864, p. 583; from the *Ber. Wien. Akad.*, May 12, 1864.

2. *On artificial Anatase, Brookite, and Rutile;* by Mr. HAUTEFEUILLE.—The dry method of forming crystallized titanic acid adopted by Hautefeuille consisted in dissolving the titanic acid in an alkaline fluorid, or in fluorid of calcium, alone or mixed with silica, and submitting the solu-

tion to the action of a current of chlorhydric acid gas. As a further perfecting of the method, he makes the vapor of water to react directly on the gaseous fluorid of titanium in a reducing or oxydizing atmosphere.

For *Anatase* the fluorid of titanium is conducted along a platinum tube to the middle of a second platinum tube in which the vapor of water is passing. The tube is so heated that the fluorid and vapor of water meet at a temperature a little below that of the volatilization of cadmium. Octahedral crystals are formed, having the angles of anatase, and a density between 3·7 and 3·9.

The titanic acid takes the form and angles of *Brookite* in the presence of fluohydric acid when the temperature at which it is produced is between that required for the volatilization of cadmium and that for zinc. G.=4·1—4·2.

*Rutile* results when the fluorid of titanium and vapor of water are mixed at a bright red heat. The forms obtained are acicular square prisms with octahedral terminations. G.=4·3.

In these reactions the fluohydric acid acts the same part as the chlorhydric in the method of crystallizing adopted by H. St. Claire Deville; it is an ephemeral solvent of the titanic acid.—*Les Mondes*, July 28, p. 605.

3. *Bishopville meteorite; Chladnite.*—The Bishopville meteorite, of which an analysis by Prof. J. Lawrence Smith is given at page 225 of this volume, was chemically investigated by Rammelsberg in 1861 (*Monatsber. Berlin. Akad.*, Sept., 1861, p. 895). He first treated the mass with chlorhydric acid, then with carbonate of soda, and obtained a residue of 90·75 p. c.; and for the part decomposed, the composition $\ddot{Si}$ 2·29, $\overline{Fe}$ 0·97, $\dot{Mn}$ 0·20, $\dot{Mg}$ 3·51, $\dot{Ca}$ 0·58 = 7·55, besides 0·8 loss by ignition. An analysis of the residue afforded $\ddot{Si}$ 60·86, $\overline{\ddot{Al}}$ 3·00, $\overline{Fe}$ 0·31, $\dot{Mg}$ 34·48, $\dot{Ca}$ 0·11, $\dot{Na}$ 1·26, $\dot{K}$ 0·93 = 100·95. He concludes that in each case the material is only a mechanical mixture and not a chemical compound.

He next divided the powdered stone by elutriation into a *lighter* (A) and a *heavier* (B) part, and analyzed them separately, hoping thereby to prove a like, or different, composition for the two. His results are:

| | A. | B. |
|---|---|---|
| Silica, - - - - - - | 58·74 | 57·12 |
| Alumina, - - - - - | 6·16 | 2·13 |
| Sesquioxyd of iron and some $\overline{Mn}$, | 1·82 | 2·71 |
| Magnesia, - - - - - | 29·78 | 36·71 |
| Lime, - - - - - - | 1·70 | 1·48 |
| Loss (alkalies), - - - | 1·80 | —— |

Rammelsberg concludes that the so-called *chladnite* is not a tri-silicate of magnesia, as made by Prof. C. U. Shepard in his analyses, but does not further educe the nature of the species.

Dr. A. Kenngott, in his *Uebersicht der Resultate Mineralogischer Forschungen* for 1861, published in 1862, cites the above results, and shows, further, that the *heavier* portion consists mostly of *enstatite*, the rest including a little *olivine*, and, as he judges from the alumina found (2·13 p. c.), a few per cent of lime-feldspar, or *labradorite*. The *lighter* portion he also makes to consist largely of *enstatite*, with a little *oligoclase*.

[The difference between the chemical analyses by Rammelsberg and Smith appears to be owing to the fact that the latter, having a better specimen for examination, separated the *pure white* chladnite from the mass of the meteorites, as he states in his paper. In consequence of this, Prof. Smith obtained in his carefully made analysis no alumina and no iron, but only the ingredients of a true enstatite, as he himself has announced.—J. D. D.]

4. *Crystals of Rhombohedral and Dimetric species often optically biaxial.*—BREITHAUPT has published, in *Poggendorff's Annalen*, cxxi, 326, a notice of the quartz of Euba (near Chemnitz in Saxony), which Prince Salm-Horstmar had found to be optically biaxial (*Pogg. Ann.*, cxx, 334), and in it claims to have first made this observation. It occurs in crystals and also massive. Its hardness is but 6 to 6½, and its specific gravity 2·578–2·632. He states that while the plates are distinctly biaxial, there are, as Mr. Jenzsch has shown, both left-handed and right-handed crystals. This quartz weathers with remarkable readiness, although, according to a chemical examination by Reich, it contains no impurity except about ¼ per cent of oxyd of iron. It occurs in four narrow veins (1 in. to 2 ft. thick), associated with a feldspar which Breithaupt proposes to describe under the name of *paradoxite*,—a mineral which he had hitherto found only in tin-veins, and which, even in the Euba veins, afforded some tin ore on pulverization and washing. These tin-bearing veins of Euba occur in the Permian red sandstone (Rothliegende).

Breithaupt observes also that *chalcophyllite*, most *apatite* and *calcite*, *mimetene*, *phenacite*, *dioptase*, *nepheline*, *zincite*, *greenockite*, and other rhombohedral minerals, are optically more or less biaxial; and that the same biaxial condition characterizes most crystals under the dimetric system examined by him, as, for example, many of *scheelite*, *wulfenite*, *cerasine*, *idocrase* (especially the manganesian idocrase of St. Marcel in Piedmont), *meionite*, *zircon*, *mellite*, etc., as well as those of *apophyllite* long since so made known by Brewster.

He also states that a grossular garnet from Siberia is uniaxial along one tetragonal axis, and that the manganesian garnet, of high specific gravity, is optically isotropic.

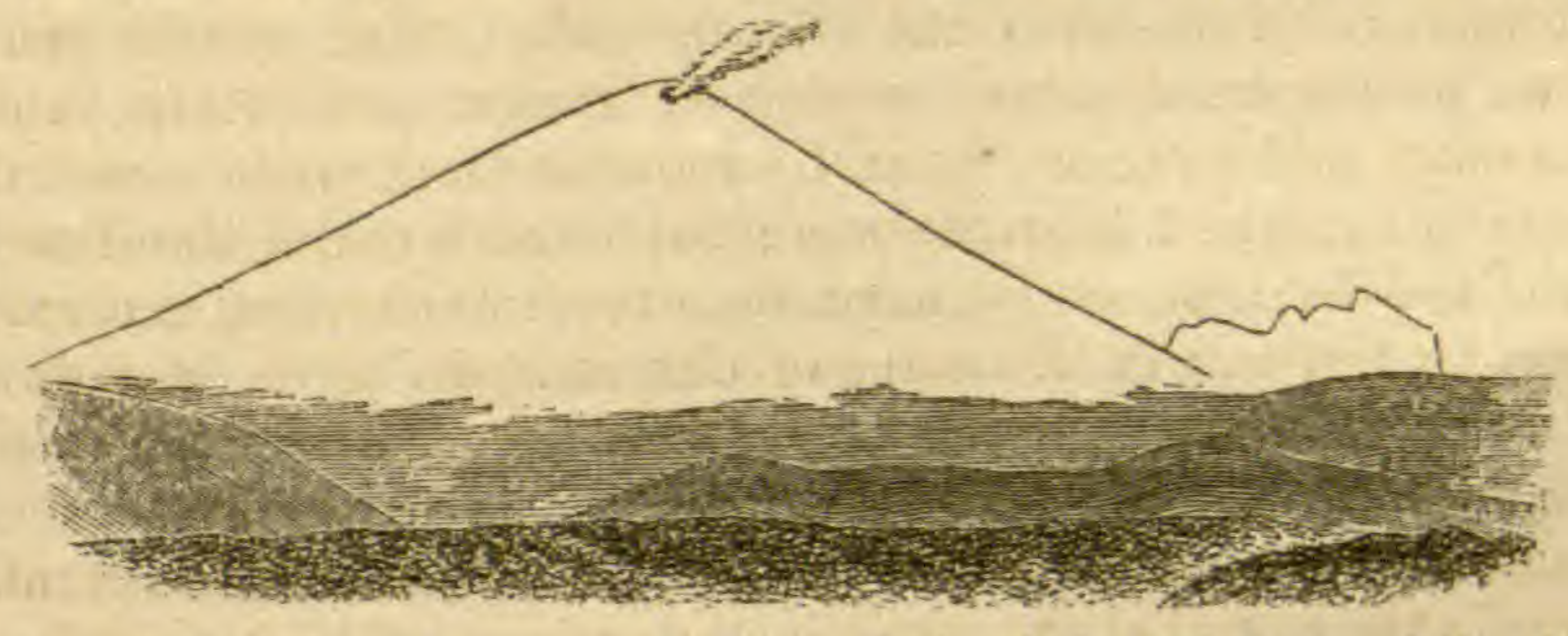
[These variations from the normal *uniaxial* condition under the Dimetric and Hexagonal systems, are like the variations from the normal which occur in all physical characters, and they have been to some extent before observed. The amount of variation, which is the point of greatest interest, is not mentioned by Breithaupt.—J. D. D.]

5. *Geschichte der Mineralogie von* 1650–1860 (History of Mineralogy from 1650 to 1860); by FRANZ VON KOBELL. 704 pp. 8vo, with 50 woodcuts and 1 lithographic table. Munich, 1864: J. G. Cotta.—The science of Mineralogy could not have found a better historian among its living votaries than Prof. von Kobell. He is a man of profound learning —a scholar in every sense of the term (as well as a poet)—an original investigator—and a thorough mineralogist. His work is correspondingly philosophical and complete. He divides the period of the history into three sections, the *first* including the century from 1650 to 1750, when the science existed only in its elements or first beginning; the *second*, the half century from 1750 to 1800, when it was taking shape, under the combined influences of the progressing sciences of chemistry and crystallography; and the *third*, the remaining time, from 1800 to 1860.

Under each of these sections, the author takes up separately the physical, chemical, and taxonomic divisions of the science; and under the third, these divisions are further subdivided for separate treatment. The work closes with a history of mineral species, in which the time and author of original discovery, and many additional details, are given.

6. *Mineralogische Notizen;* by FRIEDRICH HESSENBERG. No. 6 (*fünfte Fortsetzung*), 42 pp. 4to, with 3 plates. From the Transactions of the Senckenburg naturf. Gesellschaft at Frankfort, v, 233.—This continuation of Hessenberg's admirable crystallographic papers includes articles on crystals of Hematite, Blende, Malachite, Cassiterite, Sphene, Linarite, and Chalcolite.

7. *Note on the volcanic peaks of Cotapaxi and Arequipa;* by J. D. DANA.—In the sketch of the peak of Cotapaxi published by Humboldt, the slopes, as deduced from its profile or outline, are 52° on the right and 50° on the left. De la Beche copied this figure in his Geological Observer, with the inclination a little more reduced, viz: to 48° and 45°. In photographs of this volcano taken from near La Tacunga, by Camilius Farrand, the average angle on the right is 27° 15′, and the steepest 29° 30′; while on the left, the slope is almost uniformly 30° 50′. In another view (see the following figure), from nearly the same direction, but taken from the base of the mountain, the greatest slope of the right out-



line in the profile is 32° 50′, and that of the left 30° 10′, while the average for the former is 30° 45′, and that for the latter 26° 45′. Thus the facts are, as the writer has long believed, very widely different from what would be inferred from the published sketches. On account of this belief, he hesitated much before inserting the copy of De la Beche's figure in his Manual of Geology, (fig. 966, p. 686). Having issued that figure, he would now refer the reader to it in order that he may appreciate the contrast between the true slope and its caricature.

Arequipa, seen from the Carmen Alto, as shown by a photograph published at Lima by the "Sociedad Fotografica," has a slope of 32° 50′ in its outline or profile on the right side, and 27° 45′ on the left side. The slopes of this volcanic peak are therefore very nearly the same with those of Cotapaxi.

An angle of 45° in a volcanic cone (such as Humboldt gave in his views), could have been made only by ejections of cinders; while slopes below 34°, as are these here referred to, may be a result of ejections of tufa, or of alternations of cinders, tufa, and lava.

8. *The Dinotherium an Elephantine Marsupial.*—In the department of the Haute-Garonne in France, a pelvis of a Dinotherium has been found. It is of immense size, being 1·8 metres (5 ft. 11 in. English)

from one crest to the other of the iliac bones, 1·3 metres (4 ft. 3 in.) in height from the inferior symphysis of the pubis to the extremity of the superior spine of the iliac bones, and it indicates that the animal exceeded in size the largest of ancient elephants. It resembles most the pelvis of the elephant, but is peculiar in several respects, and most remarkably in the existence of a cavity, somewhat triangular in form, alongside of the cotyloid cavity (between this and the inferior spine of the ilium), which is evidently an articulating cavity; moreover, in the cavity on one side, a portion of the articulating bone was found; and, near the pelvis, another similar bone, much more perfect, was obtained, approximately triangular in shape like the cavity. These are, beyond question, *marsupial* bones. The writer states that this articulation of the marsupial bone with the ilium instead of with the pubis, is not surprising, considering the many other abnormal characteristics of the Dinotherium; and further that the dimensions of an abdominal pouch commencing at the pubis would have been hardly large enough for the young of a Dinotherium. The Dinotherium was then a Marsupial, like the opossum and kangaroo. It must have lived upon the branches and leaves of trees, which the reversed tusks would enable it to bring within reach. The trenchant ridges and deep channels of the teeth show that the food was of a kind requiring powerful mastication, and therefore that above stated rather than the roots of plants. The size of the Dinotherium is further evidence on this point; for an elephant eats 150 to 200 kilograms (330 to 440 lbs.) of food per day; and so many pounds of roots would have soon exhausted the supply about the lakes they were supposed by Buckland to inhabit. Its tusks, besides subserving the purpose mentioned, must have been also their principal, if not only, means of defense. Being turned downward, they were especially adapted to strike with heavy blows the smaller animals that would be likely to attack them. The writer also observes that while the neck of the huge animal was very short, the trunk must have been of great size, and that its use included, in all probability, the putting of the young into its abdominal pouch, as well as the feeding of itself.—From a letter from P. J. M. Sanna Solaro, of Toulouse, in *Les Mondes*, of Sept. 29.

9. *H. von Meyer's Palæontographia: Beiträge zur Naturgeschichte der Vorwelt;* edited by W. Dunker—has reached its 13th volume, parts one and two of which are just published.

## III. BOTANY AND ZOOLOGY.

1. *A new American station for Heather.*—The Newfoundland habitat of *Calluna* having been confirmed (*vide* this Journal, [2], xxxviii, 122), we have now the pleasure to announce that Professor Lawson,—late of King's College, Kingston, now of Dalhousie College, Halifax,—has had the good fortune to bring to light a new locality from the island of Cape Breton. The flowering specimen which Prof. Lawson sends us was collected, on the 30th of August last, "in a wet, springy place, among Spruce stumps, in peaty soil, overlying clay, on the farm of Mr. Robertson, St. Ann's, Inverness Co., Cape Breton Island." He states that "it has been known there for ten years, having been noticed by a Highlander when mowing

who immediately ran to his master, Mr. Robertson, exclaiming, 'I have found heather.' Full enquiry into the whole circumstances leads me to the belief that the *Calluna* has not been planted at St. Ann's, but is a genuine native. There is only a small patch of it, not much more than a yard across. . . . . . . . Its surroundings at St. Ann's are most appropriate. Both in scenery and vegetation there is striking resemblance to the Scotch Highlands. Gælic is the common language, and all the genuine manners and customs of the Highlanders are there." It is interesting to notice that the Heather appears to be even more restricted in this new station than in that at Tewksbury, Mass.,—the indigenous character of which it helps to establish. We may now fairly infer that the Heather once flourished throughout our eastern borders, from Massachusetts to Newfoundland, but is verging to extinction, not being able to compete here with the rival claimants of the boggy soil. A. G.

2. *Icones Muscorum, or Figures and Descriptions of most of those Mosses peculiar to Eastern North America which have not been heretofore figured;* by WILLIAM S. SULLIVANT, LL.D., etc., etc. With 129 copper plates. Cambridge, Mass.: Sever & Francis. London: Trübner & Co. 1864. Imp. 8vo.—We briefly announced this work in the September number of this Journal, in terms of unqualified admiration,—which were intended to apply as well to the scientific character of the volume as to the rare perfection of the typography and the plates. One hundred and thirty species are illustrated, a full plate (with one or two exceptions) and commonly two pages of letter-press being devoted to each. The detailed descriptions are in Latin, as also the explanation of the plates; the habitat and the general remarks are in English. The plates represent the Moss of the natural size, as magnified, and with an ample series of exquisite analyses; for the most part there are as many as twenty figures to each plate. The drawings are placed to the credit of Mr. August Schrader, who has had a long training for such work under Mr. Sullivant's direction. They were engraved by Mr. Wm. Dougal, of Washington, who executed the plates of the *Musci* of Wilkes's Pacific Exploring Expedition. Probably upon no work of the kind has an equal amount of labor, knowledge, and expense been lavished. Only a small edition has been printed, and it is published at a price ($10 in gold) which, however considerable at present, will, it is understood, be very far from covering the cost. A. G.

3. *Species Filicum;* by Sir W. J. HOOKER. Parts XV and XVI, completing the fourth volume, and parts XVII and XVIII, constituting the fifth and last volume.—The second half of the fourth volume and more than one hundred pages of the fifth, are devoted to the genus *Polypodium*, which embraces as sections *Phegopteris*, *Goniopteris*, *Cyrtomiphlebium*, *Phlebodium*, *Goniophlebium*, *Craspedaria*, *Campyloneurum*, *Niphobolus*, *Phymatodes*, *Drynaria*, *Dipteris*, and *Dictyopteris*. Of this great genus there are here described four hundred and nine species, those attributed to the United States being *P. vulgare*, *incanum*, *Phegopteris*, *hexagonopterum*, *Dryopteris*, *alpestre* (Northwest America, Dr. Lyall), *aureum*, *Californicum*, and *Scouleri* (Oregon). This last species is the *P. pachyphyllum*, described in this Journal ([2], xxii, 138), and has been recently again described, and figured, under the name of *P. car-*

*nosum*, by Dr. Kellogg of San Francisco. No mention is made of the Californian *P. falcatum* Kellogg, an older name than *P. Glycyrrhiza* Eaton, nor is the fact noted that besides *P. aureum* two other West Indian forms, *P. Plumula* or *P. pectinatum* and *P. Phyllitidis* have been collected in Southern Florida.

Following *Polypodium* (which as here arranged constitutes Suborder IX, *Polypodieæ*) is Suborder X, *Grammitideæ*, with eleven genera. *Jamesonia*, the first of them, is reduced to the original *J. imbricata* of the Andes. *Nothochlæna* has twenty-seven species, of which five, *N. sinuata, ferruginea, candida, dealbata*, and *Fendleri*, occur in the region extending from the Missouri river to Texas and Arizona. *Monogramme* with its section *Pleurogramme* includes ten species, none of them in the United States. *Diclidopteris angustissima* Brackenridge, is put into this genus under the name of *M. Junghuhnii*. *Gymnogramme* is extended so as to include several nominal genera, and not less than seventy-four species. This genus is most widely known through the gold and silver ferns of the conservatories, *G. sulphurea, calomelanos*, etc. *G. triangularis*, one of the most golden of them all, is, very appropriately, a denizen of California. *G. podophylla* Hook., a new species, is doubtfully identified with Mr. Charles Wright's No. 819 from New Mexico. *Brainea, Meniscium*, and *Antrophyum*, do not occur in the United States. *Vittaria lineata* is found in Florida. *Tænitis, Drymoglossum*, and *Hemionitis*, all of few species, are the remaining genera of this Suborder.

The highly diversified group constituting Suborder XI, *Acrosticheæ*, of which Fée has made nineteen genera, Moore seventeen, and even the careful Mettenius not less than six, is here condensed into two,—*Acrostichum*, of one hundred and sixty-seven species, and *Platycerium* of five. One species, *A. aureum*, is found on the coast of Florida, and in similar places throughout the tropical world. *Acrostichum* is divided into fifteen sections, corresponding pretty well with the genera of other authors. There is no group of Ferns more difficult to arrange in genera or sections than this; and the arrangement here adopted has the great merit of being intelligible and convenient.

In an appendix is given the Cyatheaceous genus *Matonia*, before omitted. The fifth volume contains the usual index of species and an index of tribes, suborders and genera to the whole work.

This great and useful work, which the venerable author commenced more than twenty years ago, is now happily concluded; but it will give pleasure to all students of Ferns to learn by a note at the end of the volume that—far from claiming a well-earned leisure—he "is preparing, if life and health be spared him to accomplish, a volume, to be entitled 'Synopsis Filicum,' with *brief* characters of the sections, genera, and species of Ferns (omitting all really dubious ones), with general habitats, and references, in every instance, for synonyms, more full localities and general information, figures, etc., to the pages of the present work; to which will be added all needful corrections and alterations, also the additional species (not a few) which have come into the author's possession during the twenty years this present work has been in progress; and, lastly, including the *Schizæaceæ, Osmundaceæ, Marattiaceæ*, and *Ophioglossaceæ;* thus constituting a needful supplementary volume to the

'Species Filicum,' and in itself constituting a handbook, especially useful to travelling pteridologists, who would find it impracticable to carry about with them so voluminous a work as the present." Such a volume is much needed, and we sincerely hope that he may be able to complete this important and spirited undertaking. D. C. E.

4. *On the Skeleton of the Gare-fowl (Alca impennis), and the probability of its being an extinct species;* by Prof. OWEN.—It is assumed that the extinction of a well-marked species of animal is a matter of very great rarity in the historical times, at least as compared with prehistoric geological periods; but nothing is known of the rate at which species did become extinct during those periods. In the class of Birds there was good evidence of several species having become extinct within the last two centuries. Of these, Prof. Owen instanced the Dodo (*Didus ineptus*), in the island of Mauritius; the Solitaire (Peyophaps), in the island of Rodriguez; several species of Dinornis and Palapteryx, with the Aptornis, in New Zealand; species of the nocturnal parrot, Nestor, in Philipp's Island; the Great Awk, or Gare-fowl (*Alca impennis*), in the Northern Seas, and probably the gigantic Epyornis, in the island of Madagascar. The Apteryx appears to be verging toward extinction in New Zealand. It is of peculiar interest to zoology to secure evidence of those species which are passing or have passed away. Under that impression, Prof. Owen had communicated his papers on the Dodo, Dinornis, Palapteryx, and Apteryx to the *Transactions of the Zoological Society*, and this evening gave an account of the skeleton—the first that had reached him—of the *Alca impennis.* After a minute and detailed account of the several bones, the author proceeded to compare their characters with those shown in the osteology of the awks, guillemots, penguins, and other birds, and showed that the resemblance of the gare-fowl to the penguin was merely that of adaptive relations of the wings as fins; and that, in every essential point, the great awk was more closely allied to the northern Alcadæ. The specimen which had afforded the materials for this paper was a mummified bird dug up from beneath four feet of deposit, in Penguin Island, off the coast of Newfoundland, such deposit, like the guano of the Peruvian islands, being valuable for its ammoniacal and nitrogenous principles, due to the excrement of many generations of the now extinct sea-birds that formerly bred on that island. A more complete survey of the coast of Greenland is requisite before the extinction of the Gare-fowl can be regarded as "an accomplished fact."—*Proc. Zool. Soc.*, in the *Athenæum*, July 9.

5. *Synopsis of the Bombycidæ of the United States;* by A. S. PACKARD, Jr.—Part I. From the Proceedings of the Entomological Society of Philadelphia, June, 1864. pp. from 97 to 130.—This paper is a commencement of a "synonymical list" of the Bombycidæ, with extended notes. The part here issued takes up the Lithosiidæ and Arctiadæ.

6. *Review of American Birds*, etc.; by Prof. SPENCER F. BAIRD.—Sheets 4, 5, 6, containing pages 49 to 96 of this important work, announced at page 303, have reached us. These finish the *Turdidæ*, and take up, in order, the *Cinclidæ*, *Saxicolidæ*, *Sylviidæ*, *Paridæ*, *Certhiadæ* and *Troglodytidæ*.

7. *Cryptochiton Stelleri.*—The Chiton described at page 185 of this volume, by Dr. Prescott, is the *Cryptochiton Stelleri* of Middendorf.

## IV. ASTRONOMY.

1. *Discovery of another minor Planet, Sappho,* (80).—Mr. Pogson, of the Observatory at Madras, announces the discovery by him of another small planet, on the 3d of May last. Its light was that of a star of the 10th–11th magnitude. Its position May 3d, $13^h\ 44^m\ 11^s$ Madras mean time, was, $\alpha = 16^h\ 12^m\ 3^s{\cdot}41$; $\delta = 16°\ 47'\ 10''{\cdot}8$. He has given to it the name Sappho.

2. *Comet* II, 1864. A very faint telescopic comet was discovered in Coma Berenices, on the 23d of July, by Donati at Florence. It had a small star-light nucleus. In a few days it was lost in the sun's light.

Mr. Krüger, of Bonn, gives the following elements, computed from the observations of July 28th, July 31st, and Aug. 2d.

Perihelion passage 1864, Oct. 11·088, mean Berlin time.

$$\pi = 159°\ 0'\ 26''$$
$$☊ = 31\ \ 59\ \ 12$$
$$i = 70\ \ 23\ \ 0$$
$$\log q = 9{\cdot}96400, \text{ motion retrograde.}$$

3. *Tempel's Comet.*—This comet was observed at Charleston, S. C., on the 5th of August, by Acting Master Tillinghast, of the U. S. iron-clad Catskill, at which time the head was very brilliant, and a tail, upward of 30° in length, was visible to the naked eye. On the evening of the 9th, a large nebulous object—the same comet—was observed by H. P. Tuttle, of the same vessel, near $\beta$ Virginis; as seen with a telescope, it had a bright nucleus and a very faint tail. On the two following evenings it was also visible; it had moved rapidly to the eastward and southward.—*Communication from H. P. Tuttle,* dated August 19, U. S. Iron-clad Catskill, off Charleston, S. C.

4. *Shooting stars on the night of Aug. 9–10th,* 1864.—At several places arrangements were made to watch for the annual return of the August meteors. But throughout New England and the Middle States the air was so smoky that, on the night of the 9th, but a few of the fixed stars near the zenith were visible.

At New Haven the sky was almost covered until half past eleven, when the clouds became so thin that Polaris was just visible. This star could be seen during the next three hours, and for a part of the time stars of the fourth magnitude might be seen in the zenith. Mr. C. G. Rockwood assisted me. Together we saw 44 meteors during these three hours, after which the clouds became too thick for us to observe.

At Belvidere, N. J., Rev. H. S. Osborn and Mr. G. H. Coursen saw between $10^h\ 20^m$ and one o'clock, 29 flights. There was the same smoky sky as in New Haven.

At Philadelphia, Mr. B. V. Marsh and Mr. R. M. Gummere saw through the haze 13, between $10^h\ 40^m$ and $12^h$, of which 12 were conformable.

In Chicago, Mr. F. Bradley had much better success. He with Mr. Wm. Dickinson and Mr. Theodore M. Slauson watched from half past ten, Aug. 9th, until half past three, Aug. 10th. They were at first assisted by a fourth person. The night was very favorable, except that there was a cloud in the N.W. for a short time. The following is the result of their count before one o'clock.

| | 10½h—11h. | 11h—12h. | 12h—1h. |
|---|---|---|---|
| North, - - | 13 | 38 | 60 |
| East, - - - | 9 | 38 | 31 |
| South, - - | 7 | 33 | 54 |
| West, - - - | 12 | 30 | 7 |
| | 41 | 139 | 152 |

Soon after midnight one observer ceased to watch. The following numbers were seen during the next two and a half hours by the other three, making in all 1026 shooting stars.

| | 1h—2h. | 2h—3h. | 3h—3½h. |
|---|---|---|---|
| North and West, | 92 | 112 | 40 |
| East and South, | 75 | 80 | 13 |
| South and West, | 93 | 130 | 59 |
| | 260 | 322 | 112 |

The small number seen by one observer during the last half hour was probably due to his fatigue and sleepiness. There seemed to be a large number of unusually fine meteors, apparently a larger proportion than usual. Only a small proportion of the whole were unconformable.

On the night of the 6th, during an hour and a half, ending near midnight, Mr. Bradley counted thirty, between one-half and two-thirds of which were conformable.

At Lawrence, in Kansas, Mr. Wm. H. R. Lykins counted between the setting of the moon (about half past ten) and one o'clock, over 300 meteors. The sky was beautifully clear and cloudless.

Mr. George Scarborough, in a letter from Riverside, Kansas, to the *Atchison Free Press*, states that on the same night from $10^h$ to $10\frac{1}{2}^h$, he counted *sixteen* meteors; from $10\frac{1}{2}^h$ to $11^h$, *seventeen*; the next half hour, from $12^h$ to $12\frac{1}{2}$ (*sic*), *twenty-five* were noted. At one o'clock, not being very well, he retired, but arose at three o'clock, and during one hour counted *fifty*. The nights of the 10th and 11th were unfavorable, but between $3^h$ and $4^h$ A. M., Aug. 13th, he counted but *six* meteors, though the sky was clear.

Mr. H. P. Tuttle, on board the Catskill off Charleston, reports that he sat up to watch from $9^h$ P. M. to $4^h$ A. M., and saw only seventeen (?) during that time. The sky was remarkably clear. H. A. N.

4. *Report on Luminous Meteors;* by Mr. J. GLAISHER.—The Report contained numerous observations of fireballs, or the largest class of meteors, contributed for the Catalogue presented. The largest fireball described was seen on the 5th of December, 1863, which produced the vivid impression of lightning over the whole of the British Isles. Fireballs described in Paris are greatly underrated, for meteors of the largest class are there rated as only six times brighter than Venus. Two small fireballs were seen in a short space of time on the 21st of January, and two of the largest size on the 4th of July, 1864. Two fireballs closely followed the observation of a large meteor at Athens by Dr. Schmidt, on the 19th of October, 1863; one in England, and the second on the coast of Spain. This preference of individual dates is now well known, and receives the attention of the Committee. Like the fireball of 1783, the

meteor was composed of large and smaller globes, recalling the showers of stones at L'Aigle and Stranraer. The mechanical theory of the heat, roughly estimated from the light of twenty shooting stars, doubly observed in August, 1863, proved the average weight of these to have been little more than two ounces. A similar estimate of the largest fireball of the present Catalogue would furnish very nearly a hundred weight of material substance. Dr. Haidinger supposes that non-productive fireballs and shooting stars are loosely compacted in their substance, and thus accounts for their want of penetrating power. Prof. Newton and Mr. Herschel have concluded independently, that shooting stars commence at seventy miles and disappear at fifty miles above the surface of the earth.[1] At sixty miles above the earth, shooting stars are far more frequent than at any other altitude, and they are considerably more between forty and eighty miles above the earth than in all other elevations put together. The region from forty to eighty miles above the earth is the "stable atmosphere" of Mr. Quetelet, as determined by the heights of shooting stars. It cannot, on the received law of decrease of density, comprise more than $\frac{1}{10000}$th part by weight of the total shell of the atmosphere; yet the 9999 parts of the remaining atmosphere are very seldom molested by their presence. It appears necessary on this account to retrench very greatly the weights of unproductive fireballs and shooting stars. Examples in the present Catalogue of suddenly collapsing and rekindling meteors appear to favor an hypothesis that chemical affinities, unknown at ordinary temperatures, produce in similar meteors a considerable portion of their unaccountable excess of light and heat. Ten meteors have been estimated in the past year by referring their apparent courses to the stars. The average heights and velocities of these are:—Height at first appearance, 103 miles; at disappearance, 68 miles; length of path, 79 miles; velocity, 49 miles per second. Frequent observations of the radiant points of shooting-stars are recorded in the present Catalogue. These have been observed on the 10th of August, the 30th of November, and the 6th of December, 1863, the 2d of January, the 10th and the 20th of April, and the 10th of August, 1864, by referring the meteors to twelve perspective charts representing the whole circuit of the constellations as they appear at intervals of two hours above the vapors of the horizon in the latitude of Greenwich. The longest paths on these maps can be traced correctly with an ordinary rule; and by their prolongation the intersection of their lines determines the radiant point in showers, such as those of the 10th of August, 1863, and the 2d of January and the 10th of April, 1864. Even solitary observations thus recorded, slowly accumulating from year to year, appeal more correctly to the eye by this means than a meteoric shower observed without the aid of maps; while the radiant points observed in the past year, it was believed, would have escaped attention had not maps been specially provided in advance. The observations of meteors on the 9th and 10th of August, 1864, indicate a display, ranking very nearly with the general average of the phenomena, which, in the clear sky and absence of the moon, amounts to between thirty and forty per hour for a single observer

[1] Prof. Newton informs us that he has not arrived at the conclusion here stated. See page 135 of this volume.—Eds. Am. J. Sci.

constantly regarding the sky near the zenith. In numbers there was not half, and in brilliancy not more than a small fraction, of the display of the previous year. It was less striking on the 10th than on the 9th of August, consistently with the ordinary conditions of leap-year. If any indication of periodicity can yet be traced in the fluctuations of this phenomenon, it is perhaps a minimum, at intervals of eight years, which has thrice occurred; and last in 1862. In the Appendices to the Catalogue, notices of aërolitic falls have been collected, as well as abstracts of several recent papers on meteoric subjects. Prof. H. A. Newton has constructed the elements of the November meteoric ring solely from historical data, in such a manner as to leave very little for further observations to supply. The orbit is almost circular, retrograde, and inclined 17° to the ecliptic; with a precession of 52″·60 from a fixed equinox nearly equal to that of the equator, but in an opposite direction. The meteoric cloud extends over one-tenth or one-fifteenth of the periphery of this ring, and the velocity with which particles of this cloud arrive on the atmosphere of the earth is 20·17 miles per second, allowing for the attraction by the earth. The velocity of their passage through the air is 38·7 miles, or nearly forty miles per second. The question of the radiant points of shooting stars has chiefly engaged the attention of the Committee during the past year. It is remarkable that a radiant point, which is the vanishing-point of straight lines seen in perspective, should not have been associated with the meteors of the 10th of April until the compiling of the present Report, for this date has long since been noticed by Baumhauer, in 1845, and again more recently by Wolf, while astronomers have been aware for more than thirty years that periodical meteors take their direction from definite vanishing-points among the stars. The number of such radiant points which remain yet to be discovered appears to be strictly measurable by the zeal of observers; nevertheless, Mr. Greg has been rewarded with very unexpected results, indicating, at present, between twenty and thirty radiant points as giving rise to the great majority of shooting stars observed throughout the year. The long-continued observations of Prof. Heis, of Münster, corroborate the results of Mr. Greg, and they are now receiving extensions at the hands of Dr. Schmidt, of Athens. As this inquiry, pursued without the use of maps specially provided for the purpose, is very nearly hopeless, and indeed more likely to be pernicious than profitable to the interest of meteoric astronomy, the Committee earnestly desire that a grant of £40 may be sanctioned for the purpose of lithographing the twelve charts now submitted and for printing copies for a selected number of competent observers.—*Proc. Brit. Assoc.*, from the *Athenæum*, Sept. 24, 1864.

6. *On the possible Connexion between the Ellipticity of Mars, and the general Appearance of its Surface;* by Prof. Hennessy.—The physical characters of Mars have attracted considerable notice, on account of the supposed resemblance of that planet to our earth, and at the same time one of the most prominent of these characters presents a striking contrast with its terrestrial counterpart, namely, its ellipticity, which is estimated by most astronomers at a higher value than mechanical theory would assign, if the planet had been originally in a fluid state. In accordance with hydrostatical laws, a planet similar to Mars, and rotatory

around its axis in the same period of time, should have an ellipticity very nearly approaching to that of our earth. Two observers of great eminence, Bessel and Johnson, seemed to have arrived at a similar conclusion. The observations made by the former were fully discussed by Mr. Oudemanns in the *Astronomische Nachrichten*, No. 838, p. 352. After combining the results of different observed diameters with various angles of position, by the method of least squares, Oudemanns came to the conclusion that the observations gave varied and uncertain values for the diameters; and therefore that it was permissible to regard the planet as approximately spherical. Johnson, in the Radcliffe Observations for 1850 and 1853, discussed the results of measurements made with the heliometer, and arrived at substantially the same result. Although the late Mr. Arago referred to some of the author's views regarding terrestrial physics, as probably affording explanation for the anomaly of the large ellipticity which he assigned to Mars, in his posthumous publication on the structure of the planet, the author had heard the same eminent person express views almost identical with those flowing from the observations of Bessel and Johnson. At the same time, the simplification which the author endeavored to introduce into the theory of the Earth's figure, will not, if applied to that of Mars, suffice to account for the usually received high ellipticity of that body. Abstaining, for the present, from any attempt at an explanation of this peculiarity, let us endeavor to trace out its consequences with reference to the configuration of that planet. It seems to be generally admitted that there is, in the neighborhood of one of the poles of Mars, a great mass of brilliant matter, analogous to a mass of terrestrial snow. This very substance is even supposed, with great probability, to seriously interfere with the accuracy of telescopic observations, owing to the optical disturbances arising from the irradiation of such an extremely bright object. It is also manifest that if this substance should be snow, the varying seasons of the planet would cause its dimensions to vary, and thus the power of the disturbing influence. These circumstances show that great caution should be used in accepting any results which are liable to be affected by the presence of this snowy patch, and they also necessarily imply the existence of a fluid like water in that part of the surface of Mars wherever the temperature is above the freezing point of the fluid. If this should be so, the generally assumed large ellipticity of Mars should be followed by another result. Several years ago, when controverting and disproving an erroneous theory of the Earth's figure, put forward by Playfair, and which has since acquired some importance by being reproduced by Sir John Herschel, in support of his general views, and appealed to by Sir Charles Lyell, the author obtained mathematical expressions for the equilibrium of a fluid like water spread over an exterior abraded spheroid such as this theory assumed the Earth to be. It follows from these expressions that if the Earth possessed a very small ellipticity, or were spherical, it would consist of two great circumpolar continents, with an intermediate belt of equatorial ocean. I have assigned the dimensions of these continents, supposing the ocean to have its present volume. It also immediately follows that if the Earth had a very great ellipticity, such, for example, as that so frequently assumed for Mars, the reverse would take place, and the dry land would form an equatorial belt, while the poles would be enveloped in water. The dimensions of these circumpolar oceans, with the assumed ellipticity of Mars, could be also assigned, and they should exist on its surface, unless there should be great irregularities in the density of the matter composing the planet. The mechanical theory on which these conclusions are based is simple, and therefore the attention of observers may be directed to the inquiry as to whether, compared with our Earth, a greater predominance of dry land exists at the equatorial parts of Mars compared to its polar regions. If the author might venture to draw any conclusion from the results hitherto observed, and especially from the drawings appended to Mr. Lockyer's paper, in the *Memoirs of the Astronomical Society*, he would say that no such predominance of equatorial land exists on the surface of Mars, and therefore if its appearances are partly due to the presence of a liquid on its surface, we must conclude that its ellipticity has been generally exaggerated, and that the results of Bessel and Johnson's observations are, upon the whole, nearer to the truth than those of other observers.—*Proc. Brit. Assoc.*, from the *Athenæum*, Sept. 24, 1864.

## V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Discovery of Lake-habitations in Bavaria; by Prof. Desor.*—The following account of the discovery of ancient lake-habitations in Bavaria, is from the *Journal de Genève* of June 9th and July 6th. Prof. Desor writes as follows, under the date of June 14th:

"You are aware that for many years it has been my intention to explore the lakes of Bavaria. In spite of the fruitless efforts made in many of these lakes, I could not believe that the lacustrine population had been arrested at Lake Constance, and that the lakes of the Bavarian plain, although so advantageously situated, had remained unknown to these primitive people. I have availed myself of some weeks of vacation to accept an invitation from my friend Prof. Liebig, to visit that region. The morning after our arrival, after having discussed the advantages of different lakes in the neighborhood of Munich, I dispatched Benz, our fisherman, to lake Wurm or Starnberg, to make a preliminary examination. I joined him the next day, with Prof. v. Siebold, the celebrated zoologist. Prof. Liebig was, to his great regret, unable to accompany us. We began our explorations on the right bank of the lake; this offered us only some Roman remains of little importance. We reached a little island situated on the left bank, in face of the new summer palace of King Max, now in process of erection. Here seemed to be a place where we might reasonably expect to find the remains of those ancient structures, if any had ever existed about the lake; and in fact we had not made the tour of the island before we discovered many heads of piles; the first were not very distinct, yet they were not to be mistaken. Soon we found others, better defined, cut off close at the bottom, as in our stone stations at Hauterive and Auvernier. Some, of fir, were so distinct that we could count through the water, here three feet deep, the rings of growth of the trunk. The wearing of the piles at this place is to be attributed to the action of ice. Prof. v. Siebold was delighted at the sight of these foundations, counting back thousands of years; for every thing indicated that here, as at Lake Constance, we had come upon the age of stone. What is most remarkable is that these piles, which are almost a foot in diameter, frequently with a projection at the top, have the appearance of being continued in a line into the island, exactly as we had observed the year before at Isoletta, on Lake Varese; so that it is very probable that the island of Starnberg, which bears the name of Isle of Roses (Roseninsel), from the beautiful roses which are cultivated there, is (like that of Lake Varèse, and that of the little Lake of Inkwyl) an artificial island.

But piles alone did not satisfy us. Remains more characteristic must be found if we would demonstrate that there existed here, as in Switzerland, constructions made and inhabited by men, not by beavers. Benz was not long in discovering fragments of pottery. These included, as in Switzerland, vases of a black color, imperfectly burned, fashioned with the hand, but yet rudely ornamented near the edge, sometimes with a groove, sometimes with impressions made by the fingers, or with a ring of impressions made by both the fore finger and thumb applied to the neck of the vase.

Prof. von Siebold, our zoologist, looked earnestly for bones among the piles; and in less than two hours, we had found remains of the horse, the stag, the ox, the wild boar, and the wolf. That which was not least significant was that the greater part of the bones, especially of the long bones, had been broken, another proof that they had been accumulated by the lake-dwellers, who, as we know, were in the habit of breaking the bones of animals to extract the marrow.

You will understand, after an expedition so successful, how eager we were to return to Munich and make known our discoveries to our friends. It is the great news of the day; Bavaria has its lake-habitations. Now that the impulse has been given, all will press to the work. Prof. von Siebold starts to-morrow morning with Benz for the lake of Chiem. Orders have already been given to drag around the island of Roses, and I do not doubt that, below the superficial layer of mud, hatchets and knives of flint will be discovered. Next we must examine the Austrian lakes, for I am convinced that they too are not wanting in piles."

[The editor of the *Journal de Genève* of July 6th, adds the following account of later explorations.]

"We complete, from information received from Professor Desor, what we have already published on the discoveries of lacustrine antiquities in Bavaria. The commission appointed by order of the King, and which includes, among its most active members, Profs. v. Siebold and Moritz Wagner, has already obtained interesting results. Already the number of *stations* examined by them, with the assistance of Mr. Desor's fisherman, amounts to eight, distributed among six lakes; two in lake Starnberg, one in lake Chiem, two in lake Schlier, one in lake Seeon, one in lake Ammer, and yet one more in another little lake. At lake Chiem, as well as at lakes Schlier and Seeon, the lacustrine vestiges are found concentrated around the islands; and what is most remarkable, the island in lake Seeon appears to be artificial, like the island of Roses in lake Starnberg; so that a fact which, at the commencement of lacustrine studies, seemed so strange as to put it in doubt, is now decidedly established.

The greater part of these points have not yet been explored in detail, and consequently up to this time have furnished only the more common and abundant objects, particularly bones broken to extract the marrow, and fragments of pottery. From their general character, they would be referred to the age of stone; but, before deciding positively, it is important to wait the result of more careful investigations. Already, since the first examination, the piles of the island of Roses have furnished some objects in bronze, especially a hair-pin of large size, and quite similar to those of the Swiss lakes. As that island contains also Roman remains, and others belonging to the early centuries of the Christian era, it affords an example of a continuous occupation, such as has been observed in many of the Swiss lakes, particularly at Steinberg, on Lake Bienne, near Nidau. It is evident that some of these points have never been without inhabitants since the age of stone.

A considerable number of ancient relics were found about a quarter of a century since, at the laying of the foundations of the little chateau on the Island of Roses. These objects were carried to Munich, where they figured for a long time in the collection of antiquities. Later, by order of the king, they were transferred to the chateau on the Isle of Roses, where Prof. Desor with some difficulty obtained permission to examine them. They were found collected together in a glass case, in the king's cabinet. The Roman and French objects are correctly labeled, while the fragments of lake pottery are designated as objects of great antiquity, anterior to the invention of wheel pottery, and consequently before the Romans.

Since the discovery by Prof. Desor of a lake station on the Island of Roses, it has been noticed that the rampart which surrounds the island and which has been constructed of materials taken from the beach of the island, includes many fragments of the same lacustrine pottery, which have passed unheeded till now—another proof that in order to see, one must look with experienced eyes. Now that the way has been opened, we do not doubt that the lakes of Bavaria will prove as rich a mine as those of Switzerland."

*Lake-habitations* have been since found at Olmutz, in Austria, of the most ancient kind; and at the southern end of Lake Garda there are others abounding in curious bronzes.

2. *On Spontaneous Generation and semi-organized bodies;* by E. Fremy.—At the meeting of the Academy of Sciences of France on the 27th of June last, Mr. Fremy presented the following note on Spontaneous Generation:

"My name having been often cited in connection with the subject of spontaneous generation, I believe it my duty to give a precise statement of opinions which I have always presented, on this important question, in my lectures, and in my works treating of fermentation.

I need hardly say here that I repel without hesitation the idea of spontaneous generation, if it be applied to the production of an organized being, even the most simple, from elements not possessed of the vital force. Chemical synthesis enables us, beyond doubt, to reproduce a large number of proximate principles of vegetable and animal origin; but organization, in my view, puts an impassable barrier to these synthetic reproductions.

Alongside of the definite proximate principles which synthesis can form, such as glucose, oxalic acid and urea, there exist other substances much less stable, and at the same time of much more complex constitution. They contain all the elements of the organs of organized beings; we find in them carbon, hydrogen, oxygen, nitrogen, even phosphorus and sulphur, and often lime and alkalies. These substances are albumen, fibrine, osseine, the vitelline substances, etc. It is not possible, as I think, to regard them as organic proximate principles; I designate them by the general name of semi-organized bodies.

They are in something of the same state in relation to organization, the formation of tissues, the production of ferments and putrefaction, as a dry seed which continues on for years without presenting the phenomena of vegetation, and which germinates when submitted to the influences of the atmosphere, moisture and heat.

These semi-organized bodies, which contain all the elements of the organs, can, like a dry seed, remain for a long time in a state of organic immobility; but they may also pass out of this state and afford, at the expense of their own substance, all the elements of organization when the circumstances become favorable for organic development.

I will not undertake to state at this time all the conditions which may cause the semi-organized bodies to participate in the actual phenomena of organization; I mention only the one which, in my view, is the most important: that of organic impulse (*entrainement organique*).

It is well known in chemistry with what readiness a substance undergoing alteration can draw another into a similar condition of things (*peut en entrainer un autre*). It is thus in nitrification; different phenomena of oxydation determine and force along the oxydation of the ammonia, azotized substances, and even that of azote, as Chevreul has recently demonstrated.

The semi-organized bodies may feel the vital impulse, and thus become involved in the process of organization by living beings under whose influence they may be; they then form membranes, tissues, ferments; they can then organize themselves and decompose themselves; in a word they will be themselves living.

It is thus that I understand the part played by albuminous substances in the phenomena of organic development and decomposition.

I do not consider them as simply supplying food to animals or plants, and as only in this way a means of fermentation; I attribute to them a direct agency, and I admit that, under the influences which I have mentioned above, they may take or experience actual and complete organization.

If the ideas here submitted are accepted, they will explain, on one side, the part which organic beings take, incontestibly, in the phenomena of fermentation and disorganization, and on the other, the constitutive part, equally evident, in my opinion, of the albuminous media in which fermentation, moulds and infusoria are developed.

I content myself at this time with an announcement of these first principles. In another communication I will take up the question whether or not the semi organized bodies can present the phenomena of organization under other influences than that of organic beings.—*L'Institut*, June 29, 1864.

3. *Charcoal having the solidity and texture of mineral coal formed under pressure.*—We have received from Mr. Robert Safely, of Cohoes, N. Y., an account of the conversion of a portion of the wooden step of a turbine water-wheel into a very compact coal resembling closely in texture and appearance ordinary mineral coal, along with a specimen of the coal. The step was of oak, and about 10 inches through; and when taken out, the whole surface was covered with a layer of coal. The charring was a consequence of the water pipe which lubricated it becoming clogged with dirt. Mr. Safely states further that the fall of water to which the wood was subjected when it was converted into coal, was exactly 25 feet; and as the diameter of the wheel is 5 feet 7 inches, the pressure on the wheel would be measured by a column 5 ft. 7 in. in diameter and 25 ft. high, less what is due to the water striking the bucket at a small angle to the plane of the wheel. The gearing, wheel, shaft, etc. weigh about 3 tons, which would give for the pressure upon the step, if the whole weight of the water was reckoned, about 20 tons.

The facts exemplify the formation of coal under pressure, combined with moisture and a moderate heat, and with very slow motion.

4. *On the field of Vision in Man;* by F. Foucou.—Mr. Foucou gives the following results of his measurements of the amplitude of the field of vision in the case of two persons, Mr. Leboucher and Mr. Puchot.

| | Mr. Leboucher. | | Mr. Puchot. | |
|---|---|---|---|---|
| | Right eye. | Left eye. | Right eye. | Left eye. |
| Superior limit of the field, | 63° 12′ | 63° 14′ | 52° 48′ | 54° |
| Inferior limit " | 74° 57′ | 74° 41′ | 76° 46′ | 74° |
| Internal limit " | 60° 5′ | 67° 25′ | 59° 54′ | 64° |
| External limit " | 101° 23′ | 97° 03′ | 107° 52′ | 99° |
| Horizontal diameter of field, | 138° 9′ | 137° 55′ | 129° 34′ | 128° |
| Vertical " " | 161° 28′ | 164° 28′ | 167° 46′ | 163° |

The following conclusions are stated:

That in the same individuals, the field of vision has approximately the same breadth in the two eyes along the same diameter, while the horizontal and vertical diameters in the same eye differ widely.

That in both eyes, in the case of both persons examined, the angle for the outer limit of visibility is greater than a right angle, so that a ray of light making an obtuse angle with the axis of the eye can nevertheless pass by the pupil, traverse the crystalline and vitreous humor, and produce an image on the retina.

That the angular limits of vision are not the same in different persons; this difference for the superior limit of visibility amounts to 10° at least, but is almost nothing for the inferior limit; for the outer limit it may be 6°; for the inner but slight.

Adding the angle for the external limit of vision of the left eye to that for the external of the right, it gives about 200° for the total breadth of the field for the two eyes.—*Les Mondes,* Aug. 18, 1864, p. 733.

5. *Petrifaction of Animal Substances.*—About twenty-five years ago the scientific world was surprised by an announcement of the fact that a Venetian, named Girolama Segato, had discovered a means of reducing dead bodies to a state of hardness closely approaching to that of stone,

except at the joints, where he had succeeded in maintaining a certain degree of pliancy. The results obtained by Mr. Segato in this direction were altogether wonderful, and many strangers used to visit his collection at Florence, where he had settled. Nevertheless he was not encouraged, first, on account of his political principles, and, secondly, because the clerical party, which was then all powerful, got up a cry of impiety against him. His secret found no purchasers, and he died in consequence of a complaint which he had contracted in visiting some of the wildest parts of Africa. A short time after his death, the late Abbé Francesco Baldacconi, director of the Museum of Natural History at Sienna, obtained certain results which led to very strong hopes that Segato's secret might be re-discovered. Mr. Baldacconi's process consisted in steeping the anatomical specimen for several weeks in a solution of equal parts of corrosive sublimate and salammoniac, a mixture which by the earlier chemists was called *sal alembroth;* and in 1844 a liver thus prepared was sent over by him to the Academy of Sciences here. This specimen had acquired the consistency of steatite, or of serpentine, and was perfectly incorruptible. The Italian papers now state that a Sardinian naturalist, Professor Marini, has re-discovered Segato's secret. His process is also kept a secret, but from the description it appears that he obtains still more remarkable results than his predecessor. He has constructed a small table entirely composed of petrified animal substances, viz: brain, blood and gall, and having quite the appearance and consistency of breccia. His preparations are incorruptible, they preserve their natural color, and will resume their original state on being immersed in water for some time. Professor Marini intends to exhibit his preparations in Paris.

6. *Manna-lichen of Pallas.*—A recent fall of the Manna-lichen, (*Lichen esculenta* of Pallas, *Parmelia esculenta* of Sprengel,) has been made known by Dr. Haidinger. of Vienna, (*The Reader*, Aug. 6). It fell at Charput, northwest of Diarbekir, in Asia Minor, during a gust of rain, and a portion was sent to Vienna on the 6th of July by Baron Prokesch-Osten, the Austrian Internuncio at Constantinople. Pallas long since observed this manna in the steppes of the Kirghis; and these falls are now known to extend far to the west, crossing the Caspian Sea to Van, Diarbekir, Malatia and Ienischehir. A considerable fall occurred in Oroomiah, to the southwest of the Caspian, in 1829. It is common also in Northern Africa over Sahara. The manna is ground to flour and made into bread. Mr. J. Hogg suggests, in *The Reader* of Aug. 13, that the wind probably takes up and carries along the minute seeds of this lichen from the southeast or northeast, which, falling with the rain, quickly vegetate.

Mr. Hogg refers to an article of his on this manna and the manna of the Israelites, in vol. iii, pp. 183–236, of the *Transactions of the Royal Society of Literature.* B. Seemann, in the same Journal, observes that the manna of the Scriptures has been regarded, and with better reason, the substance called manna which exudes from the *Tamarix Gallica* var. *Mannifera*, after its bark has been punctured by an insect (the *Coccus manniparus* of Ehrenberg); and he observes that a recent communication for the *Journal of Botany*, from Dr. Landerer, of Athens, supplies additional evidence in favor of this opinion. He argues that the wind and rain required to supply the lichen could hardly have been a daily occurrence during the sojourn of the Israelites in the wilderness.

7. *On an ancient Factory of Flint Implements;* by Abbé C. CHEVALIER.—Doctor Léveillé, a physician at Grand Pressigny (Department of Indre-et-Loire in France), has recently discovered near that village, upon the lands of Claisière and of the Doucetterie, an ancient manufacturing place of flint implements, exceeding in importance and interest any thing of the kind before known. Vast quantities of chips of siliceous stones, of arrow-heads, hatchets, knives 15 to 20 centimetres long, of lance-heads, &c. have been obtained from it. Cut stones (*noyaux taillés*) of prismatic shape and about 20 centimetres long, are especially abundant; they occur by thousands over an extent of 5 or 6 hectars. The collections from Abbeville and various caverns, which have excited so much attention, are nothing compared with the accumulations at the workshop of Pressigny. Only a few polished objects have been met with. Dr. Léveillé has found, however, a hatchet polisher. It is a block of sandstone, 40 to 50 centimetres long and 25 to 30 broad, marked throughout with furrows, angular in section, in which the hatchets were polished by friction, after they had been rudely shaped by hammering. It is certainly one of the most curious of the implements of this primitive branch of industry.—*Acad. des Sci. Paris,* from *Les Mondes,* Aug. 25, 1864, p. 770.

8. *Discovery of Fossil Stone Implements in India.*—At a recent meeting of the Royal Asiatic Society of Bengal, Professor Oldham exhibited a small collection of stone implements which had very recently been discovered by Messrs. King and Foote, of the Geological Survey of India, near Madras. These were all of the ruder forms, so well known as characterizing the flint implements which have excited so much attention within the last few years in Europe. They were all formed of dense semivitreous quartzite—a rock which occurred in immense abundance in districts close to where these implements had been found, and which formed a very good substitute for the flints of north Europe. This was the first instance in which, so far as he knew, such stone implements had been found in India *in situ.* True celts, of a totally different type and much higher finish, and in every respect identical with those found in Scotland and Ireland, had been met with in large numbers in Central India, but never actually imbedded in any deposits. They were invariably found under holy trees or in sacred places, and were objects of reverence and worship to the people, who could give no information as to the source from which they had been originally gathered together. A single and very doubtful fragment of a stone implement had been found by Mr. W. Theobald, Jr., in examining the deposits of the Gangetic plains near the Soane river. This occurred in the Kunkurry clay of that district; but, with this exception, he was not aware of any stone implements of any kind having previously been noticed *in situ* anywhere in India. Those now on the table had been collected partly by himself, from a ferruginous lateritic gravel-bed, which extended irregularly over a very large area west of Madras. In places, this was at least fifteen feet below the surface, cut through by streams, and in one such place, from which some of the specimens on the table were procured, there stood an

old ruined pagoda on the surface, evidencing that, at least at the time of its construction, that surface was a permanent one. This bed of gravel was in many places exposed on the surface, and had been partially denuded; and it was in such localities, where these implements had been washed out of the bed, and lay strewed on the surface, that they were found most plentifully.

Mr. Oldham remarked on the great interest attaching to such a discovery, and on the probable age of the deposit in which they occurred. Another point of interest connected with the history of such implements was the remarkable fact that while, scattered in abundance over the districts where they occurred, were noble remains of what would by many be called Druidical character-circles of large standing stones, cromlechs, kistvaens, often of large size and well preserved, all of which were traditionally referred to the Karumbers, a race of which there yet existed traces in the hills, still all the weapons and implements of every kind found in these stone structures were invariably of iron. No information whatever regarding these stone implements could be obtained from the peasantry, who had been quite unaware of their existence.—*Jour. of the Asiatic Society of Bengal*, No. I (1864); from *Ann. Mag. Nat. Hist.*, [3], xiv, 155.

9. *Postscript to Prof. Winchell's article on the Origin of Prairies* (p. 332); by the Author.—In my article on Prairies, the belief is expressed that the assumption of the possibility of the almost indefinite suspension of the vitality of seeds, required by my theory, would present the greatest obstacle to its reception. It seems excusable, therefore, to crowd into a postscript, a reference to evidences temporarily overlooked, and especially to testimony and facts collected by Mr. Marsh in his learned work, "*Man and Nature*," p. 285, *et seq.* This work has but just fallen into my hands. Mr. Marsh thinks, with Dr. Carpenter, that the vitality of seeds "seems almost imperishable while they remain in the situations in which nature deposits them." He cites numerous instances in which one crop of plants has disappeared on a change of conditions, and another, of different nature, has promptly assumed its place, originating, evidently, from seeds preëxisting for ages in the soil. He says "earth brought up from wells or other excavations soon produces a harvest of plants, often very unlike those of the local flora." He expresses the opinion that earth ejected from considerable depths by a certain earthquake convulsion, to which reference is made and which soon became covered with vegetation "never observed in that region before," must have brought up with it the seeds from which the novel vegetation sprang, under "the influence of the air and sun, from depths where a previous convulsion had buried them ages before." In the same connexion may be quoted a statement by Darwin (*Origin of Species*, Am. ed., p. 69), to the effect that in the midst of a very large and very sterile heath in Staffordshire, some hundreds of acres were planted with the Scotch fir, and, after twenty-five years, not less than twelve species of plants (not counting grasses and sedges) had made their appearance in the plantation of firs, "which could not be found in the heath"—and this, though the fir forest seems to have been visited only by insectivorous birds.

Mr. Marsh quotes from Dwight's *Travels* his account of the appearance of a fine growth of hickory [*Carya glabra* Torr.] on lands in Vermont which had been permitted to lie waste, when no such trees were known in the primitive forest within a distance of fifty miles; also, Dr. Dwight's account of the appearance of a field of white pines, on suspension of cultivation, in the midst of a region where the native growth was *exclusively* of angiospermous trees. "The fact that these white pines covered the field exactly, so as to preserve both its extent and figure," says Dr. Dwight, "and that there were none in the neighborhood, are decisive proofs that cultivation brought up the seeds of a former forest within the limits of vegetation, and gave them an opportunity to germinate."

The existence of a succession of forests of different prevailing species has been satisfactorily established in Denmark by the researches of Steenstrup on the *Skovmose*, or Forest-bogs, of that country (*Mem. Acad. Sci. Copenhagen*, ix, 1842). These bogs are from twenty to thirty feet in depth, and the remains of forest trees in successive layers, prove that there have been three distinct periods of arborescent vegetation in Denmark—first, a period of the pine (*Pinus sylvestris*)—secondly, a period of the oak (*Quercus robur sessiflora*)—lastly, a period of the beech (*Fagus sylvatica*), not yet arrived at its culmination. The dominant species of each period flourished to the entire exclusion of the other two species, (see *Smithsonian Report*, 1860, p. 305, *et seq.*) Cæsar affirms that the *Fagus* and *Abies* were, in his time, wanting in England, but the beech (*Fagus*) is now plentiful, and Harrison tells us in his "*Historicall Description of the Iland of Britaine*" (*Holingshed's Chronicles*, 1807, i, 359), that "a great store of firre" is found lying "at their whole lengths" in the "fens and marises" of Lancashire and other counties, where not even bushes grew in his time. (See further, Marsh's *Man and Nature*, p. 222.) No doubt such extinct forests have flourished in America, even since the Glacial epoch, and have stocked the accumulating soils with their stores of vitalized fruitage, awaiting some future resurrection; and no doubt the "fens and marises" of Lancashire, under suitable circumstances, would reproduce from their granaries of forest fruit, the arboreal vegetation which had flourished and disappeared before the Roman conquest.

Ann Arbor, Mich., Oct. 15, 1864.

10. *A jet d'eau made by means of the heat which air, when confined under glass, derives from the solar rays.*—It is well known that the air confined under glass, if it receive the direct rays of the sun, will become much heated, far beyond the temperature of the rays, owing to the action of the glass in absorbing these rays and conveying the absorbed heat to the air within. Prof. Mouchot, of Alençon, has made the following application of the heat thus acquired. He takes a bell of silver, very thin and covered with lamp-black, and places over it two bells of glass, and exposes the whole to the rays of the sun. Two curved tubes furnished with stopcocks pass under the black bell, one of them to supply water when it is required, the other to give exit to the water; the latter terminating outside in an ordinary jet d'eau orifice. Being now exposed to the solar rays—whose heat is transformed into non-luminous

heat in its passage through the walls of the bells, an effect that goes on accumulating without cessation—the air situated above the water dilates, and by its pressure causes a jet to rise, attaining sometimes in Mouchot's trials a height of nearly 33 feet. When the water is exhausted, a screen placed before the sun will cool the interior and cause the water to return, or a new supply may be introduced through the supply-pipe. Many times the shade thrown over the apparatus by spectators caused it to stop, much to their surprise.—*Les Mondes*, Sept. 22.

11. *Method of destroying the larves of Weevils.*—Mr. Marsaux has used naphthaline with success for exterminating the larves which for some years have destroyed the plants in the nursery of Versailles. The poisoning of a hectar (about 2½ acres) has required 250 kilograms of napthaline, costing 250 francs (it being introduced to a depth of nearly 10 inches). But on waiting until the time when the larves have reached the superficial layer of the soil, the quantity required was much less, and the cost but 80 to 125 francs the hectar. The naphthaline, as it is a fatty solid, has first to be reduced to powder and mixed with fine, dry sand. It is powdered by means of an iron cylinder, or a wooden pestle armed with iron.—*Les Mondes*, Sept. 8, p. 53.

12. *British Association.*—The British Association met at Bath on Wednesday, the 14th of September. In the accounts of the meeting which have reached us from England, the only American name mentioned among those of "distinguished Foreigners" is "Commodore Maury of the Confederate States." Sir Charles Lyell delivered his inaugural address in the theater of Bath. Besides the usual scientific meetings, there were several geological and archeological excursions; a *conversazione* in the "Assembly-rooms," Thursday night; a discourse in the theater, Friday evening, by Professor Roscoe; another by Dr. Livingstone, Monday; and a *Microscopical soirée*, Tuesday. The session closed on Wednesday. The receipts for the past year amounted to £4505, and after paying all expenses there was a balance in hand of nearly £365.

At the recent meeting 2788 tickets were sold, making for the income from this source £2964. The balance in hand is stated to be £3622, 17s. The Association has, also, funds in Consols to the amount of £8500.

The researches in science, carried forward under the auspices of the Association, cost, in 1834, £20; while for the present year the large sum of £2037 has just been voted.

The Report for the meeting in 1863 has been issued, making a volume of more than a thousand pages.

The British Association will meet next year at Birmingham, when Sir Charles Lyell will surrender his presidency to Prof. Phillips, of Oxford.

13. *Report to the British Association, at Bath, on a uniform system of weights and measures.*—The Committee of the British Association reported in favor of adopting the metric system of France, and the report was unanimously adopted, yet with some discussion as to whether the unit should be the French meter or not. This committee consisted of Lord Wrottesley, Mr. Adderley, Sir W. Armstrong, the Astronomer Royal, Mr. Samuel Brown, Mr. W. Evart, M.P. (who was chairman of the committee in the House of Commons, and who conducted the measure with reference to the metric system through Parliament), the Master of

the Mint, Sir John Hay, Prof. Hennessey, Mr. James Heywood, Dr. Lee, Prof. Levi, Professor Miller, Prof. Rankine, Rev. Dr. Robinson, Colonel Sykes, M.P., Mr. Tite, M.P., Professor Williamson, Mr. Purdy, and Mr. Yates.

Among the recommendations of the committee are the following:

(4.) That it be recommended to the government, in all cases in which statistical documents issued by them relate to questions of international interest, to give the metric equivalents to English weights and measures.

(5.) That in communications respecting weights and measures presented to foreign countries which have adopted the metric system, equivalents in the metric system be given for the ordinary English expressions for length, capacity, bulk, and weight.

(6.) That it be recommended to the authors of scientific communications, in all cases where the expense or labor involved would not be too great, to give the metric equivalents of the weights and measures mentioned.

(8.) That treatises explaining the metric system, with diagrams, should be forthwith laid before the public. That works on arithmetic should contain metric tables of weights and measures, with suitable exercises on those tables; and that inspectors of schools should examine candidates for pupil teachers in the metric system.

(9.) That in reports made to the British Association, degrees of heat or cold be given according to both the Centigrade and Fahrenheit thermometers, and that the scales of thermometers constructed for scientific purposes be divided both according to the Centigrade and Fahrenheit scales, and that barometric scales be divided into fractions of the meter, as well as into those of the foot and inch.

14. *Analysis of a Hot Spring containing Lithium and Cæsium, in Wheal Clifford;* by Dr. W. A. Miller, V.P.R.S.—This hot spring is the most abundant source of lithia at present known. The quantity of chlorid of lithium furnished by it may be stated to be 800 lbs. in twenty-four hours. The existence of cæsium in quantity somewhat considerable for an element hitherto so rare adds to the interest with which this water will be regarded. Both chemists and medical men will hail this supply of lithium. The temperature of the spring is 122°—125° at the 230 fathom level; average yield 150 gallons per minute; specific gravity at 60° F. 1007. 646·1 grains of fixed salts per imperial gallon are obtained on evaporation. These consist of

| | Grains. |
|---|---|
| Chlorid of potassium with a little chlorid of cæsium, | 14·84 |
| Chlorid of lithium, - - - - - - | 26·05 |
| Chlorid of sodium, - - - - - - | 363·61 |
| Chlorid of magnesium, - - - - - | 8·86 |
| Chlorid of calcium, - - - - - - | 216·17 |
| Sulphate of calcium, - - - - - - | 12·27 |
| Oxyds of iron, manganese and aluminum, - - | *trace* |
| Silica, - - - - - - - - | 3·65 |
| | 645·45 |

| | | Cubic Inches. |
|---|---|---|
| In 1 imp. gallon the gases amounted to | - - | 8·91 |
| Consisting of— | | |
| Carbonic acid, | - - - - - - - - - | 1·89 |
| Oxygen, | - - - - - - - - - | 1·72 |
| Nitrogen, | - - - - - - - - - | 5·30 |
| Ratio of oxygen to nitrogen gas, | - - - | 1 : 3 |

*Proc. Brit. Assoc.*, from *The Reader*, Oct. 8.

15. *On the Temperature of the Sexes;* by Dr. DAVY.—The theory of Aristotle that a man possessed more warmth than a woman, had been disputed; and it had been held by some, as the result of modern research, that the temperature of women was slightly superior to that of men. The author considered the early opinion the more correct. Taking the average, the temperature of males and females was as 10·58 to 10·13. The result of some elaborate experiments recently instituted was that the temperature in the case of the men varied between 99 and 99½, that of the women was between 97¾ and 98. An examination of other animals gave still a somewhat higher temperature for the male than the female, six fowls showing the proportion of 108·33 for the former to 107·79 for the latter.—*Proc. Brit. Assoc.*, from *The Reader*, Oct. 8.

16. *On Crude Paraffin Oil;* by Dr. B. H. PAUL.—The author remarked that very little attention had hitherto been paid to that portion of crude paraffin oil which was heavier than water, and its existence had been denied. He found, however, that the oil obtained from coal, or any similar material, by distillation at a moderate heat not exceeding low redness, always contains oils heavier than water, and that these oils are precisely the same as the oils heavier than water, which are contained in the ordinary coal-tar of gas-works, consisting in both cases chiefly of carbonic acid and a thick pitchy substance. It was also shown that the product obtained by distilling different varieties of bituminous coal at a low heat differs very considerably in its character, according to the kind of coal it is obtained from, and that this difference is mainly due to the relative proportions of oil lighter than water and of oil heavier than water. In the case of the oil obtained from the kind of coal commonly used as fuel, the proportion of heavy oil is so large that the product closely resembles the coal-tar of gas works in all its outward characters, although the oils lighter than water which it contains are identical with those contained in crude paraffin oil, as it is usually manufactured from particular kinds of coal and other bituminous minerals, which are exceptional in so far as they yield by distillation a product containing the light oils in much larger proportions than the heavy oils.—*Proc. Brit. Assoc.*, from *The Reader*, Oct. 8.

17. *German Association.*—The twenty-ninth meeting of the German Association, held this year at Giessen, was brought to a conclusion on the 23d ult., after a most successful meeting, more than a thousand members and associates being present. Spontaneous generation and some geological theories were among the matters discussed at some length.—*The Reader*, Oct. 8.

17. *White Fish of the Great Lakes of N. America.*—A writer in the *Athenæum* urges the introduction of the "celebrated White Fish of the Canadian Lakes" into the "lakes of Cumberland and Scotland, now almost valueless."

OBITUARY.

FRANCIS ALGER.—Francis Alger was born in Bridgewater, Massachusetts, March 8, 1807, and died suddenly at Washington, of typhoid pneumonia, Nov. 27, 1863. His taste for Mineralogy and other branches of science was first awakened in 1824. In 1826, he went to Nova Scotia with his father, to erect a furnace, and collected the minerals of Digby Neck and of the trap rocks of Granville, a catalogue of which he published in the *Boston Journal of Philosophy and the Arts*, and also in this Journal (xii, 227). In the following year he undertook an exploration of the mineral region of the peninsula of Nova Scotia, along with Dr. C. T. Jackson; and their results were jointly published in volumes xiv and xv of this Journal (1828 and 1829). Again, in 1829, another exploration was made with Dr. Jackson, and an extended paper on the subject published in the *Memoirs of the Academy of Arts and Sciences.* Soon after the publication of this paper, he was elected a member of the Academy; and in 1849, he received the honorary degree of A.M. from Harvard College.

Mr. Alger was one of the original members of the Boston Society of Natural History, and for many years Curator of Mineralogy. His mineralogical explorations and collections during this time and afterward were numerous and important, as shown both by his rich cabinet and his occasional publications. At the Beryl locality of Grafton, N. H., he uncovered and worked out a crystal of beryl weighing five tons. In 1844, he published an edition of Phillips's Mineralogy, which, by great labor, he had adapted to American science by the addition of lists of localities, and various other facts relating to American minerals. In 1845, he published in this Journal a paper on the Zinc mines of Franklin, N. J., in which he had become greatly interested; in 1846, notes on various minerals; in 1850, on crystallized gold from California, and on quartz containing rutile; in 1852, on the identity of lincolnite and beaumontite with heulandite. The last paper appeared also in the *Journal of the Boston Society of Natural History*, which Journal also contains others of his mineralogical papers.

Capt. JOHN HANNING SPEKE, the African explorer, died at Bath, on the 16th of September, from the accidental discharge of a musket in his own hand. He had gone to that place to attend the meeting of the British Association; and its members were discussing the advisability of recommending to the Government that some positive recognition of his services should be made, when the sad tidings reached them.

## VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Canadian Naturalist and Geologist.*—This valuable bi-monthly scientific Journal, published at Montreal, under the auspices of the Natural History Society of Montreal and containing its Proceedings, closed its first series with the end of the 8th volume, and began its second with

February of the present year. The editors for the year 1864 are Dr. J. W. Dawson, Prof. T. Sterry Hunt, E. Billings, Prof. S. P. Robbins, and Rev. A. F. Kemp, with David A. P. Watt as General Editor.

2. *Organic Philosophy, or Man's true place in Nature;* by HUGH DOHERTY, M.D. Vol. I, Epicosmology. London; Trübner & Co.—Mr. Doherty introduces his work on Man's true place in Nature with the statement that "Man is not merely a skeleton, nor is external nature a congeries of bones." "There are in man a body and a soul, and both must be well understood before we can discover his true place. The human skeleton is but a fragment of the body; and though, to those who are well versed in comparative anatomy, a part of any physical organism may show the nature of the whole, still a fragment of the body gives no adequate idea of the living soul, which is the man." He proceeds from this as his basis, and bringing to bear upon his great subject a wide range of philosophical knowledge, has produced a work that will be read with interest and profit by those interested in the great question of the day. His views of classification of the various departments of nature are to a considerable extent novel, and to us they appear artificial. They occupy a large part of the volume, and no analysis can be given without devoting more space to the subject than seems desirable.

3. *The Geological Magazine, a Monthly Journal of Geology:* edited by T. Rupert Jones, F.G.S., Prof. Geol. Roy. Military College, Sandhurst, assisted by Henry Woodward, F.G.S., F.Z.S., British Museum.—Numbers I. to IV. have been issued of this Monthly Geological Journal, the first having appeared on the 1st of July. Price 1*s*. 6*d*. per number.

4. *Introduction to "The Alpine Guide;"* by JOHN BALL, M.R.I.A., F.L.S., etc., late President of the Alpine Club. 113 pp., 12mo. London, 1864: Longman, Green, Longman, Roberts and Green. Price 1*s*.—Contains in a brief compass the information most important to the Alpine traveller—as, that pertaining to custom house regulations, money, modes of travelling, guides, inns, expenses, maps, etc., together with general advice on mountaineering, and also sketches of the climate, vegetation, geology, glaciers, &c., of the Alps. It is the introductory portion of "The Alpine Guide," in which full instruction is given with regard to the various passes in the Alps, etc.

5. *Revision of the Polyps of the Eastern Coast of the United States;* by A. E. VERRILL. 46 pp., 4to, with a lithographic plate. From the Memoirs of the Boston Society of Natural History, second series, vol. i, No. 1, July, 1864.—This memoir of Professor Verrill is a thorough review of the Polyps of the Atlantic coast of the United States, embracing a careful revision of earlier writings and investigations, notes on the species hitherto described, and descriptions of several new species. The names of some of these last are from the manuscripts of Professor Agassiz and Mr. Wm. Stimpson. The author expresses his indebtedness to Professor Agassiz for the free use of the collections in the Cambridge Museum of Comparative Zoology, and also of Prof. A.'s magnificent series of drawings made from life by Mr. J. Burkhardt. The plate contains figures of five species—*Bunodes Stella* Verrill, *Rhodactinia Davisii* Agassiz, *Halcampa producta* Stimpson, *Edwardsia sipunculoides* Stimpson, and *Bicidium parasiticum* Agassiz.

Annual Report of the Geological Survey of India, for the year 1862-3.

Memoirs of the Geological Survey of India; Palæontologica Indica. Vol. II. Part 6. The Fossil Flora of the Rajmahal Series, Bengal; by T. OLDHAM and JOHN MORRIS. Vol. III, Part 1, the Fossil Cephalopoda of the Cretaceous Rocks of India (Ammonitidæ); by F. STOLICZKA. 1863.

Paleontologie Française, ou Description des animaux invertébrés fossiles de la France. Tome VII. Terrain Crétacé; Livraisons 8, 9, 11, 13, 14; by G. COTTEAU. 1862, 1863.

Jahresbericht über die Fortschritte der Chemie, etc., for 1863. First Heft. Giessen, August, 1864.—The Jahresbericht is now edited by Heinrich Will, aided by C. Bohn, Th. Engelbach, and A. Knop.

Rambles in the Rocky Mountains, with a visit to the Gold Fields of Colorado; by MAURICE O'CONNOR MORRIS. London. 1864. Smith, Elder & Co.

On the Right Ascension of the Pole Star as determined from observation; by T. H. SAFFORD, Assistant at the Observatory of Harvard College. 13 pp., 8vo. From the Proceedings of the American Academy of Arts and Sciences, vol. VI. Printed for the Observatory of Harvard College.

A Register of Earthquakes in California from 1800 to 1863; by Dr. JOHN B. TRASK. 26 pp., 8vo. From the Proceedings of the California Acad. Nat. Sci. San Francisco, 1864.

PROCEEDINGS OF THE ACAD. NAT. SCI. PHILADELPHIA. No. 3, May, June, July and August, 1864.—Page 145, Critical remarks on the genera Sebastes and Sebastodes of Ayres; *T. Gill.*—p. 147, Second Contribution to the Selachology of California; *T. Gill.*—p. 151, Notes on some points in Ichthyology (a Kansas species of Percopsis; history of the name Gymnotus; genus Melantho = Campelamo of Rafinesque); *T. Gill.*—p. 153, On new species of Invertebrata from Puget Sound, collected by the Naturalist of the N.W. Boundary Commission, A. H. Campbell, Esq., Commissioner (species of *Crustacea* of the genera Eupagurus, Hippolyte, Idothea, Æga, Bopyroides, Caprella, Amphithoe, Anonyx, Gammarus, Amphithonotus, Ampelisca, Ammothea; of *Gephyrea* of the genera Phascolosomum, Sternaspis; of *Tunicata* of the genera Cynthia, Chelysoma; of *Holothuridæ* of the genus Pentacta); *W. Stimpson.*—p. 161, The influence of the Earth's atmosphere on the color of the Stars; *J. Ennis.*—p. 166, Contributions to the Herpetology of tropical America (species of Caudisoma, Tomodon, Toluca, Chamæleolis, Eupristis, Xiphosurus, Anolis, Læmanctus and Uta); *E. D. Cope.*

PROCEEDINGS OF THE ESSEX INSTITUTE, Vol. IV, No. 2.—Notes on the family Zygænidæ, with two plates—concluded; *A. S. Packard, Jr.*—Catalogue of Birds found at Springfield, Mass., with notes on their Migrations, Habits, &c., together with a list of the Birds found in the State and not yet observed at Springfield; *J. A. Allen.*

PROCEEDINGS OF THE AMER. PHIL. SOC., Vol. IX, No. 71.—Page 291, Height of tides; *P. E. Chase.*—p. 294, Danish element in England; *H. Coppee.*—p. 330, Obituary of W. Darlington; *T. P. James.*—p. 345, Barometric fluctuations and temperature; *P. E. Chase.*—p. 355, Heat and muscular energy; *P. E. Chase.*—p. 360, Manufacture of paper pulp from wood; *R. Briggs.*—p. 360, New flax-fibre machine; *Emerson.*—p. 367, Magnetic currents; *P. E. Chase.*—p. 371, On Stone implements; *F. Peale.*—p. 372, Letter of Prof. Zantedeschi on spectral analysis.—Synopsis of a paper on the influence of Ether in the Solar System; *A. Wilcocks.*—p. 388, On the Abbeville quarries; *J. P. Lesley.*—p. 395, Effects of rotation on the barometer; *P. E. Chase.*—p. 399, On the ancient sea-level; *J. P. Lesley.*

*Announced by Messrs. Williams & Norgate:—*

Comparative Osteology, an Elementary Atlas of Comparative Osteology, to consist of 12 plates in folio, drawn on stone by B. Waterhouse Hawkins, Esq., the figures selected and arranged by Prof. T. H. HUXLEY, F.R.S.

Prehistoric Archæology, or Essays on the Primitive Condition of Man in Europe and America; by JOHN LUBBOCK, F.R.S., President of the Ethnological Society. 1 vol. 8vo, with numerous woodcut illustrations.

Subscriptions are solicited for the following work, to be issued by the K. K. zoologisch-botanischen Gesellschaft of Vienna:—*Nouveau Système des Blattaires, par M. Charles Brunner de Wattenwyl.* Subscription price in Vienna, 4 Austrian florins. It will contain descriptions of about 400 species of the Blatta family, many of them new, with 13 copper-plates.

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