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ERRATA.

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" 84, " 6 " " " 5 *read* 6.
" 88, " 2 " " " 4 " 5.
" 89, " 6 " " " † see * on next page.
" 90, " 7 " top, " * " † on preceding page.
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" 98, " 21 " bottom, after assume, *read* (as I did when I began this research.)
" 100, lines 2 and 9 from top, *for* Johnson *read* Johnston.
" 104, line 5 from top, *for* vertibuli *read* vestibuli.
" 109, " 3 " " *for* UT_8 *read* UT_3.
" 173, " 6 " bottom, *for* and *read* for,
" 174, " 8 " " *for* existing *read* exciting.
" 177, " 4 " top, *for* (7.) *read* 6
" 178, " 1 " bottom, *read* Resultant curve; formed by combining the curve of a musical note with that of its octave. A similar correction to the above, to be applied to figs. 5 and 6.
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THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. I.—*Results derived from an examination of the United States Weather Maps for* 1872 *and* 1873; by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College. With two Plates.

(Read before the National Academy of Sciences, Washington, April 24, 1874.)

THROUGH the kindness of Brigadier-General Albert J. Myer, Chief Signal Officer, U. S. A., I have received a weather map for 7½ A. M. daily, since Jan., 1872. This series of maps for the years 1872 and 1873 I have undertaken to discuss, in order to determine what information can be derived from them respecting the laws of storms. To prepare myself for this discussion, I ordered an engraver to make an outline map of the United States upon exactly the same scale as the U. S. weather maps, and provided myself with a large number of copies of this map. On one of these maps the tracks of all the storms for the month of January for the two years were delineated, whenever a storm center could be satisfactorily located for two successive days. In like manner, the storm paths for the month of February were delineated on a second map, and so on for each of the months of the year. These maps exhibited, in the aggregate, storm paths for 314 days, which was the whole number of cases obtained from the two years of observations. These results were then all reduced to a tabular form by measuring with a protractor the bearing of each storm path with reference to a meridian, and measuring the daily progress of the storm on a scale of inches. This table showed the date of each storm, together with the velocity of its progress and the direction of its path, and also a variety of other circumstances which it was supposed might have some connection with the preceding, viz: the height of the barometer at the center of the storm; the amount of the fall of the barometer in the preceding 24 hours; the amount of the rise of the barometer in the succeeding 24 hours; the change of pressure at the center of the storm in 24 hours, etc.

The following table shows the average direction and velocity of storms for each month of the year, as deduced from 314 cases.

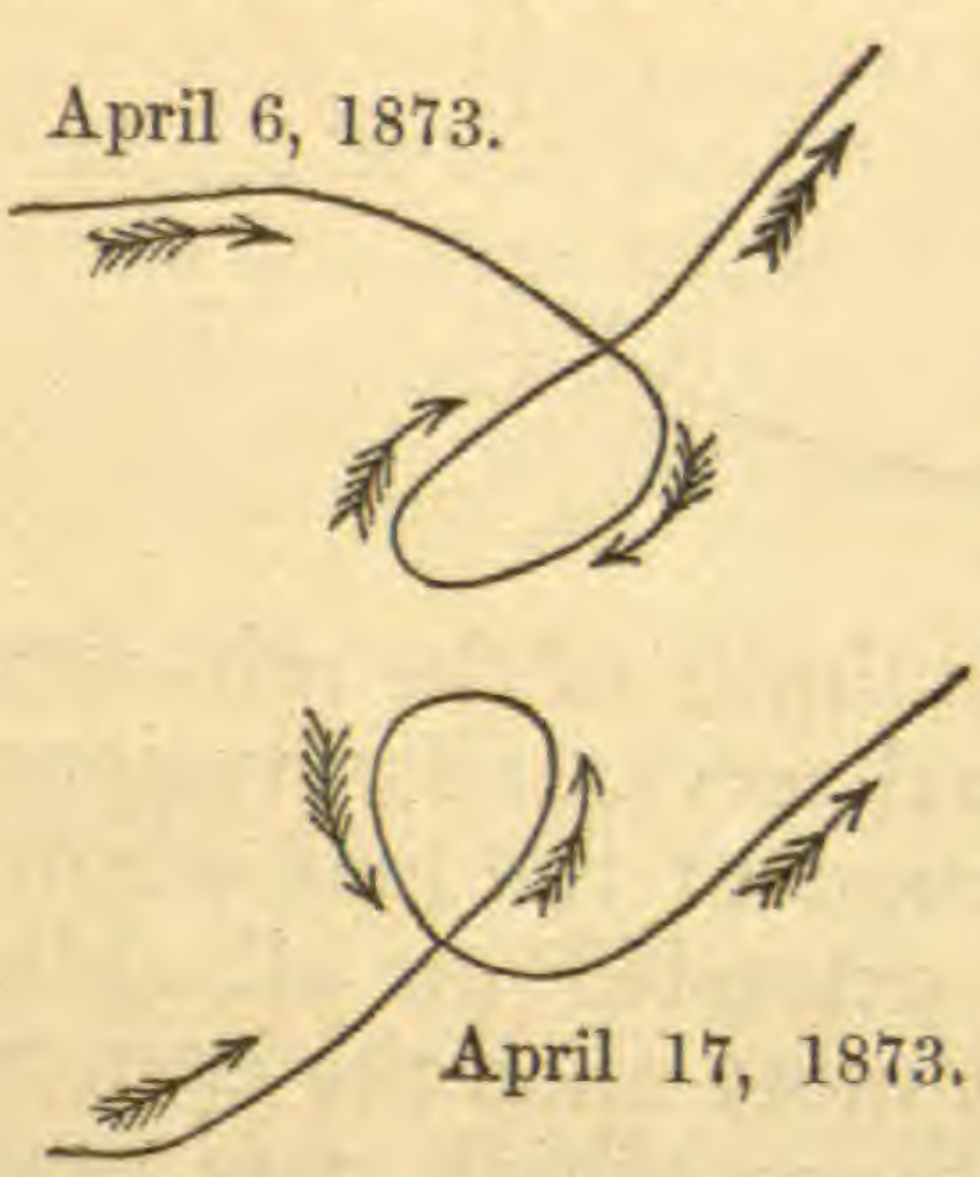
Months.	Course of storm.	Velocity in miles per hour.	Months.	Course of storm.	Velocity in miles per hour.
January,	N. 85° E.	28·5	July,	N. 102° E.	24·4
February,	N. 76 E.	31·0	August,	N. 85 E.	17·7
March,	N. 79 E.	30·8	September,	N. 78 E.	22·8
April,	N. 74 E.	25·6	October,	N. 69 E.	24·4
May,	N. 78 E.	24·2	November,	N. 80 E.	28·3
June,	N. 93 E.	21·2	December,	N. 84 E.	27·6
			Year,	N. 82 E.	25·6

The average direction of the storm paths for two years was N. 82° E., or 8° to the north of east, and the average velocity was 25·6 miles per hour; but there is a noticeable variation both in the direction and velocity, depending upon the season of the year. The course of storms is most southerly in summer, and it is a little less northerly in winter than it is in spring or autumn. July is the month in which the course is most southerly, and October is the month in which it is most northerly, the mean difference between these two months amounting to 33°. The velocity of progress is greatest in winter and least in summer. February is the month of greatest velocity and August the month of least velocity, the former exceeding the latter by 75 per cent.

The diversity in the direction and velocity of particular storms is much greater than this. In one instance, viz: Oct. 20, 1873, a storm traveled N. 44° W., and in another instance, Oct. 25, 1872, a storm traveled about due north. There have been ten cases in which the direction of the storm path was more than 60° north of east; and there have been three cases in which the direction was more than 60° south of east. In one case the direction of a storm path was 70° south of east, showing the entire range in the direction of storm paths to be over 180 degrees.

The diversity in respect to the velocity of progress of particular storms is still greater. In some cases, a storm center has remained sensibly stationary for 24 hours, and occasionally still longer, while in four cases a storm center has advanced over 1,200 miles in 24 hours, and in one case, May 15, 1873, the average velocity for 24 hours amounted to 57·5 miles per hour. Thus the velocity of progress ranges from zero to 57·5 miles per hour.

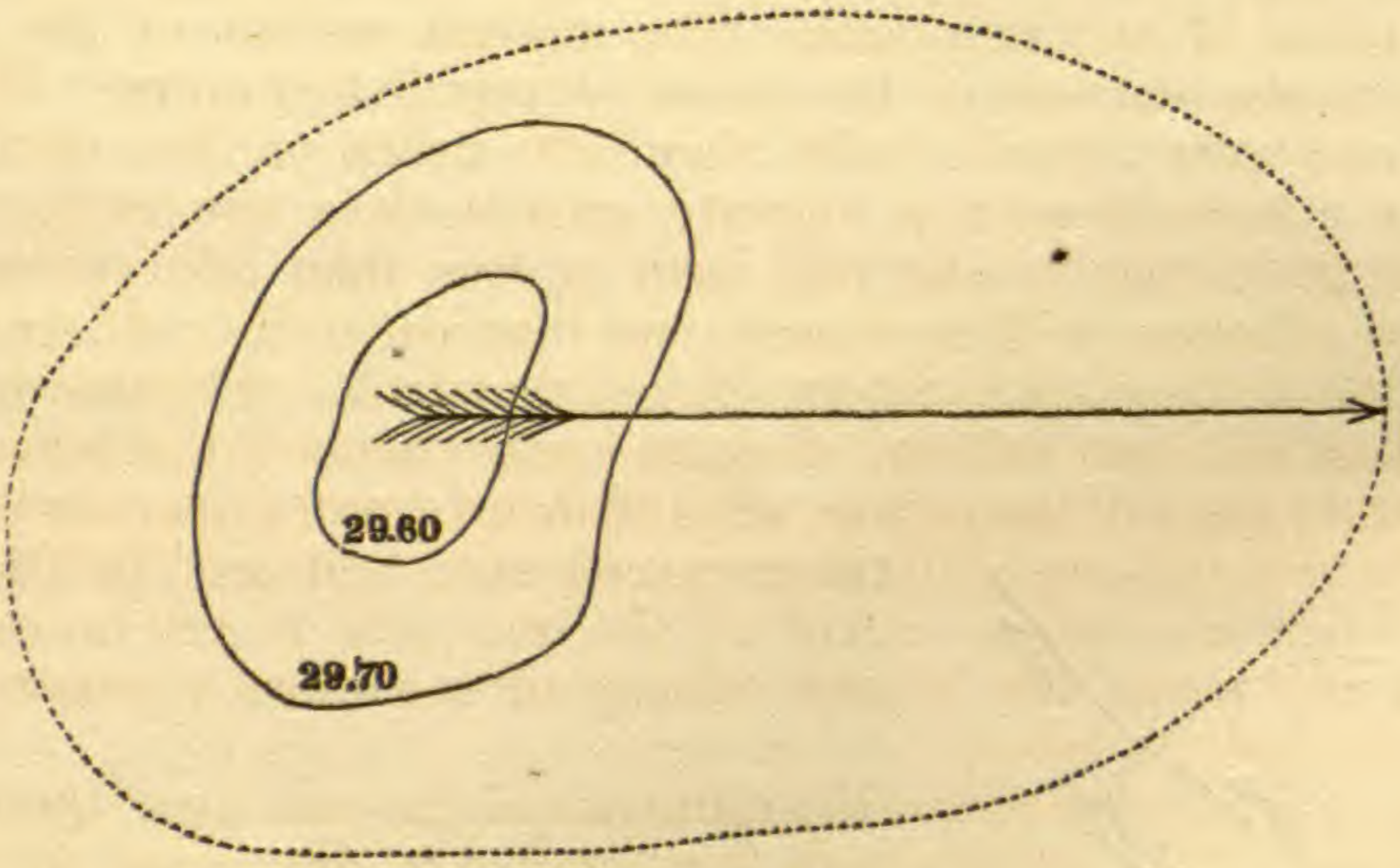
These results are derived from observations made at intervals of 24 hours. They represent, therefore, not the actual progress of storms from hour to hour, but the average progress for a period of 24 hours. The observations made under the direction of the Chief Signal Officer are made three times a day, and enable us to determine the change of position of a storm's center for every period of eight hours. A comparison of these observations shows much greater variations in respect both to the direction and velocity of storm paths than what is stated above. Some of these results are indicated in the Annual Report of the Chief Signal Officer for 1873. On the 6th of April, 1873, the path of a storm center near the Mississippi River, in lat. 40°, was such as is shown by the upper curve in the annexed cut; and on the 17th of April, 1873, the path of a storm center near Chicago was such as is shown by the lower curve. In the latter case the direction of progress changed 360° in a little more that 24 hours, and in both cases the actual motion of the center was for several hours westward, at the rate of from 10 to 15 miles per hour. If then we take into account the actual motion of a storm's center from hour to hour, we find that the storm path may have every possible direction, and the velocity of progress may vary from 15 miles per hour toward the west, to 60 miles per hour toward the east.



It thus appears that the inequality in the direction and velocity of storm paths is so great that a knowledge of their mean values affords but a very uncertain guide in predicting the progress of a storm from day to day. I have, therefore, sought to determine what are the most important disturbing causes which affect the velocity and direction of storm paths. For this purpose I have tabulated nearly all the materials afforded by the weather maps, and compared them with the velocity and direction of the storm paths.

Influence of rain-fall upon the course of storms.

One circumstance which appears to have a decided influence in modifying the course of a storm path is the fall of rain. Every considerable depression of the barometer is accompanied by a fall of rain, and the area of rain-fall usually extends further on the eastern side of a storm center than it does on the western side. The accompanying figure represents a portion of the area covered by a storm which prevailed April 25, 1874, near the Atlantic coast of the United States. The smallest oval on the left represents the isobaric line of 29·60; the oval immediately surrounding it is the isobaric line of 29·70; and the center of the storm was near the center of these two ovals.



The third and largest oval represents the limits of the rain area as far as can be determined from the signal service observations. The storm center advanced in the direction of the long arrow. It will be noticed that the rain area extended on all sides around the storm center, but spread out most upon the eastern side. The same is generally true of storms which pass over the United States.

In order to determine whether there exists any connection between the extent of this rain area and the velocity of the storm's progress, the rain-fall at each station for the preceding eight hours was copied from each of the weather maps in every case in which the storm path could be exactly traced for the next 24 hours; and the distance to which the rain area extended to the east of the storm center was measured upon the map. The whole number of storm paths was then divided into four equal classes; the first division embracing those cases in which the storm advanced with the greatest velocity; the second and third divisions embracing the cases next in order; and the fourth division embracing the cases of least velocity. The following are the average results deduced from a comparison of 152 cases, being all the cases in which the information required for this investigation could be derived from the maps.

Velocity in miles per hour.	Extent of rain area in miles.	Velocity in miles per hour.	Extent of rain area in miles.
38·8	590	21·6	503
28·5	548	14·5	365

These numbers clearly indicate that generally when a storm is advancing most rapidly, the rain area extends to an unusual distance on the eastern side of the storm, and the velocity of the storm's center appears to increase more rapidly than the extension of the rain area. The average extent of the rain area on the east side of the storm's center is 500 miles. When the rain area extends more than 500 miles on the east, the storm advances with a velocity greater than the mean; but when the extent of the rain area is less than 500 miles, the storm advances with a velocity less than the mean. If we confine the comparison to the two divisions which correspond to the greatest and least velocity, they lead us to conclude that when the eastern extent of the rain area is 100 miles greater than the mean, the hourly velocity of the storm's progress is increased 14·9 miles; but when the eastern extent of the rain area is 100 miles less than the mean, the hourly velocity of the storm's progress is diminished 8·1 miles.

In order to determine the influence of the rain area upon the direction of the storm's path, I attempted to determine not simply the limits of the rain area, but the direction in which the rain area was most extended. The rain area is usually of an oval form, as show in the preceding figure. For each rain area a line was drawn dividing the area symmetrically, so as to represent the longer axis of the oval, and the bearing of this line was measured with a protractor. I then selected those cases in which the direction of the storm paths was most northerly, and also those cases in which the direction was most southerly, and for each case the position of the axis of the rain area was determined. I then determined the average direction of the storm paths and the average direction of the axes of the rain areas for each class of cases, and obtained the following results.

Course of storm.	Axis of rain area.
N. 40° E.	N. 53° E.
N. 116 E.	N. 118 E.

According to these numbers, when the course of a storm is most northerly, the axis of the rain area is inclined to the storm's path 13° toward the south; but when the course of a storm is most southerly, the inclination of these two lines is only two degrees. Considering that there is a difference of 76° in the mean direction of the storm paths for the two classes of cases, and a difference of 65° in the position of the axes of the rain areas, we may conclude that the average course of the storm paths for 24 hours coincides very closely with the position of the axis of the rain area for the preceding eight hours. If the comparison could have been made with the direction of the storm paths for the succeeding *eight* hours, instead of 24 hours, it is presumed that the correspondence would have been still closer.

The connection here discovered between the progress of storms and the extent of the rain area cannot be regarded as accidental, and it is not difficult to discover, at least in part, the origin of this connection. The fall of rain, that is, the precipitation of the vapor of the atmosphere, is generally accompanied by a fall of the barometer. According to the theory advocated by the late Mr. Espy, when the vapor of the atmosphere is condensed, its latent heat is liberated, which raises the temperature of the surrounding air, causing it to expand and flow off laterally in all directions in the upper regions of the atmosphere, thus causing a diminished pressure over the region of precipitation, and an increased pressure on all sides beyond the area of the rain.

The progress of the storm eastward is not due wholly to a *drifting*, resulting from the influence of an upper current of the atmosphere from the west, but the storm works its own way eastward in consequence of the greater precipitation on the eastern side of the storm. Thus the barometer is continually falling on the east side of the storm and rising on the west side, in consequence of the flowing in of colder air upon that side, as will be more fully shown on a subsequent page.

Influence of the wind's velocity upon the progress of storms.

In order to determine whether there is any connection between the velocity of the storm's progress and the velocity of the wind upon the different sides of a storm, I selected all those cases in which a storm center was so situated that the velocity of the wind was given at a considerable number of stations both upon the east and west sides of the center. I then took two knitting needles and soldered them together at right angles so as to form a Greek cross. Then placing this cross upon one of the weather maps over a storm center, with the wires pointing northeast and southwest, the area surrounding the storm center was divided into four quadrants, which I designate as the north, south, east and west quadrants. The average velocity of the wind for the stations of observation in the different quadrants was then determined, including all stations within the influence of this storm center. The isobar 29·90 was generally taken as the limit of the storm, but sometimes it was necessary to reject observations within this distance when they were clearly under the influence of another storm center. Only 79 cases were found suitable for this comparison. In each of these cases the average velocity of the wind was determined for the east and west quadrants, and generally also for the south quadrant; but in a majority of the cases no observations could be obtained in the north quadrant, or the number of observations was too few to furnish a reliable mean. The following table shows the average velocity of the wind in the different quadrants, according to these observations.

W. quadrant.	S. quadrant.	E. quadrant.	N. quadrant.
10·1	8·8	8·3	7·6

It will be noticed that the velocity is greatest in the west quadrant, and that the velocity diminishes in the successive quadrants as we pass round the circle from west by south to north.

The observations were then divided into two classes, one containing those cases in which the rate of progress of the storm was greater than the mean, and the other containing the cases in which the rate was less than the mean, and the averages were taken both of the velocity of the storm's progress and the velocity of the wind in the east and west quadrants of the storm, when the following results were obtained.

Velocity of storm in miles per hour.	Velocity of wind in E. quadrant.	Velocity of wind in W. quadrant.
32·1	8·8	9·0
18·1	7·8	11·3

These numbers indicate that the stronger the wind on the west side of the storm, the less is the velocity of the storm's progress. The velocity of the wind in the west quadrant generally exceeds the velocity in the east quadrant by 22 per cent. When the velocity in the east quadrant is equal to that in the west quadrant, the velocity of the storm's progress is seven miles per hour *greater* than the mean; but when the velocity of the wind in the west quadrant exceeds that in the east quadrant by 44 per cent, the velocity of the storm's progress is seven miles per hour *less* than the mean.

Some persons might have anticipated that an increase in the velocity of the wind in the western quadrant of a storm would have the effect of driving the storm eastward more rapidly; that is, of increasing its velocity. But we shall see hereafter that upon each side of a storm's center the wind blows obliquely inward, and hence we must infer that in the central region of the storm there is an upward motion of the air: and this is the cause of the precipitation of vapor; that is, the cause of the rain-fall. An increase in the velocity of the wind in the western quadrant is accompanied by an increase in the velocity of the upward motion in this quadrant; that is, an increase of precipitation. This increased precipitation of vapor tends to depress the barometer on the western side of the storm; that is, tends to retard the eastward motion of the storm's center; and this cause may operate with sufficient force to carry the storm's center westward, as actually happened in several instances in the years 1872 and 1873. On the other hand, an increase in the velocity of the wind in the eastern quadrant tends to produce a greater precipitation on the eastern side of the storm's center; that is, tends to push the storm's center eastward, or increase the velocity of its progress.

Does the velocity of a storm's progress depend upon the rate at which the barometer falls when the storm is approaching, or upon the rate at which the barometer rises when the storm has passed?

In order to answer these questions, I compared the height of the barometer at the center of each storm with its height at the same place 24 hours before, and also 24 hours afterward. I thus obtained the fall of the barometer for 24 hours before the middle of the storm, and its rise for 24 hours after the middle. These numbers were incorporated in the same table which showed the velocity of the storm's progress for each month. The observations were then divided into two classes, one containing those cases in which the velocity of the storm's progress was greater than the mean, and the other containing those cases in which the velocity was less than the mean. It was found that the average fall of the barometer in front of the storm was nearly the same for both cases; but the average rise of the barometer for 24 hours in the rear of the storm was sensibly greatest in those cases when the velocity of progress was greatest. The averages for the two years indicated that when, after the center of the storm has passed, the barometer rises 20 per cent more rapidly than usual, the storm center advances seven miles per hour more rapidly than the mean; but when, after the storm, the barometer rises 20 per cent less rapidly than usual, the storm center advances seven miles per hour less rapidly than the mean.

By the same method of comparison it was ascertained that the velocity with which a storm advances is independent of the amount of the barometric depression at the center of the storm; and it is not sensibly affected by the circumstance whether the pressure at the center of the storm is increasing or diminishing. By the aid of the preceding principles, when we have given a weather map showing the position of a storm center for a certain hour, it seems possible to predict with considerable confidence where the storm center will be at the end of 24 hours. Nevertheless, it is admitted that while the preceding principles hold true for the average of a large number of examples, numerous and striking exceptions will be found when we attempt to apply these rules to particular cases. It is evident that the movements of the atmosphere are dependent upon a great variety of circumstances, and all of these circumstances have not been considered in the preceding deductions.

Relation between the velocity of the wind and the velocity of a storm's progress.

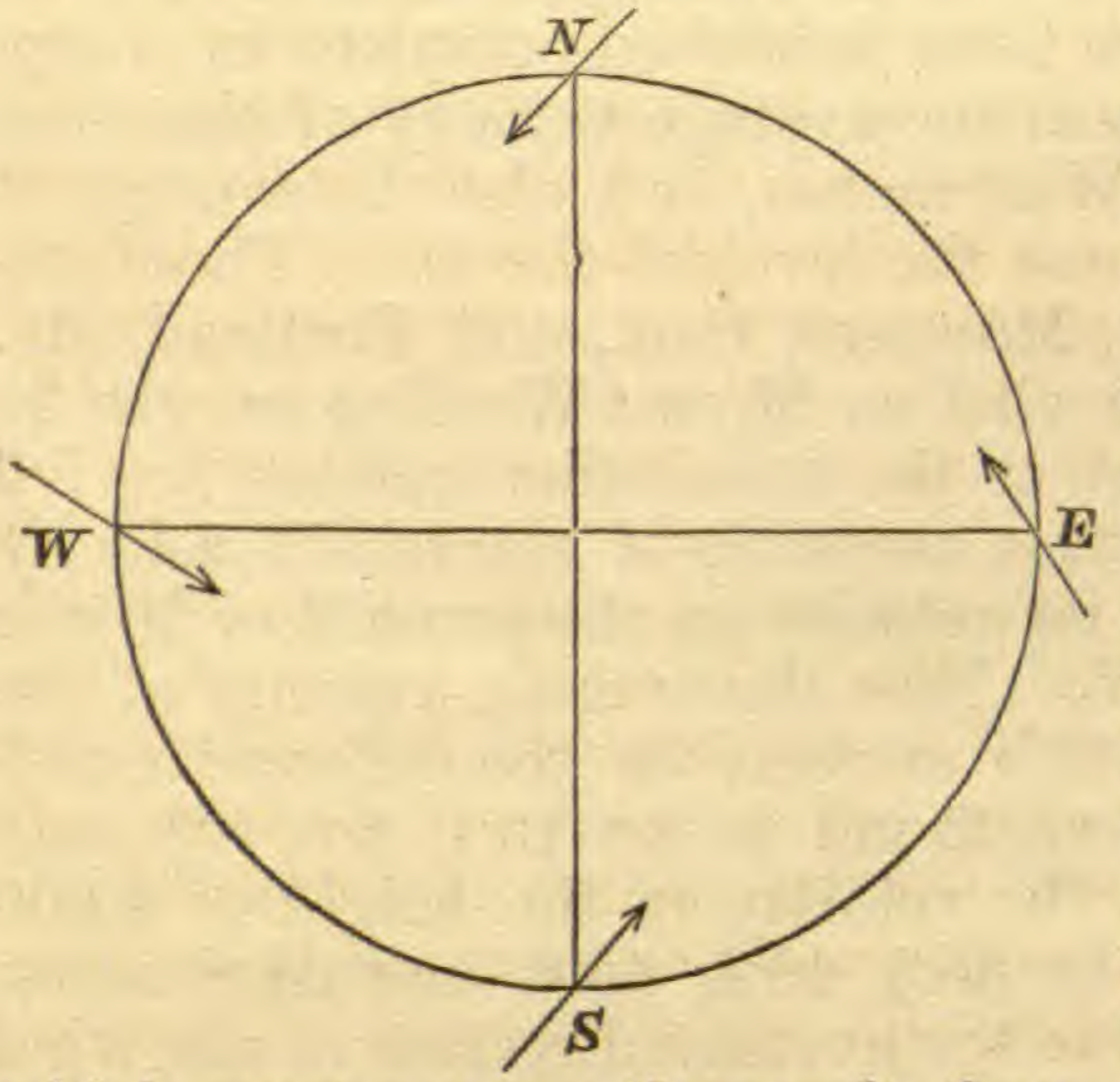
In order to determine the relation between the velocity of a storm's progress and the velocity of the wind in the different quadrants, it is important to know not simply the wind's velocity at the surface of the earth, but its velocity near the central region of the storm, which is presumed to be at an elevation of at least one or two miles. In order to form some estimate of this velocity, I took the observations at the signal service stations for Sept., 1872, which have been published complete in a separate volume, and determined the average velocity of the wind on the summit of Mount Washington, and also the average at the three nearest stations near the level of the sea. These stations were Burlington, Vt., Montreal, Can., and Portland, Me. The average velocity of the wind on Mount Washington was 29 miles per hour; the average at the three other stations was 5·3 miles; hence we conclude that the ratio of the wind's velocity near the level of the sea to its velocity on the summit of Mount Washington is as one to 5·5. Now the average velocity of the wind at stations on the earth's surface, for the different quadrants of the storm, has been found to be 10·1, 8·8, 8·3, and 7·6 miles per hour; hence the velocity at the height of 6,000 feet may be estimated to be 55·5, 48·4, 45·6, and 41·8 miles.

We wish now to determine the average direction of the wind in the different quadrants. For this purpose, each weather map which showed a storm center was crossed by two diagonal lines running northeast and southwest, marking off the four quadrants. Then, beginning with the west quadrant, I counted the number of stations at which the wind was reported from the north; also the number from the northwest, west, southwest, etc.; and in like manner for each of the four quadrants. The same was done with each of the weather maps, which furnished an example suited to this comparison. In determining the limits of each storm, the same rule was adopted as has been already stated in determining the velocities of the winds. I then found by addition the aggregate number of observations for each direction of the wind in the several quadrants, and from these numbers computed the average direction of the wind. The following table shows the resulting directions and also the velocities, as already stated, for an elevation of 6,000 feet.

	W. quadrant.	S. quadrant.	E. quadrant.	N. quadrant.
Velocity of wind,	55·5	48·4	45·6	41·8
Direction of wind,	N. 58° 48′ W.	S. 40° 25′ W.	S. 32° 6′ E.	N. 42° 33′ E.

In order to exhibit palpably to the eye the significance of these results, I have constructed the following figure, in which the four arrows show the average direction of the wind in the several quadrants, and the lengths of the arrows are proportioned to the velocity of the different winds. It is at once perceived that there is a strong tendency of the winds inward toward the central area of the storm. The following table shows the inclination of the several winds to a tangent to the circle; that is, it shows how much the average direction of the wind in each quadrant differs from what it would be if the wind revolved in a circle around the storm's center.

	W. quadrant.	S. quadrant.	E. quadrant.	N. quadrant.
Inclination,	58° 48′	49° 35′	32° 6′	47° 27′



The average departure of the winds from a tangent line is thus seen to be *more than forty-five degrees.* These numbers show how erroneous was Mr. Redfield's theory of storms when applied to the ordinary storms of the United States, and it appears that Mr. Espy was nearer the truth when he claimed for the winds a centripetal motion and entirely overlooked the rotary motion, which must necessarily result, from a general movement of the atmosphere from all quarters toward a central area. It is obvious that the centripetal motion is the primary motion, and the rotation is an effect resulting from this inward motion. It requires no argument to prove that when the wind is flowing from all quarters inward toward a central area, as shown in the last diagram, there is a rapid accumulation of air, which can only escape by an upward motion; that is, there must result a strong upward movement of the air near the center of the storm. As this air ascends, it comes into a region which is colder in consequence of its elevation, by which means its vapor is condensed, and thus by a direct comparison of observations we deduce a result which explains the cause of the rain.

If we compute the resolved portion of the wind's motion in the western quadrant, which acts in the same direction as that of the average progress of storms, we find it to be 43 miles. But the average progress of storms is only 25·6 miles; that is, at the height of 6,000 feet in the western quadrant of a storm, the velocity of the wind (estimated in the same direction as that of the storm's progress) is 68 per cent greater than the velocity with which the storm advances. This excess of motion of the wind affords a measure of the force of the upward movement in the center of a storm.

To determine whether a storm is increasing or diminishing in intensity.

In order to determine by what indications it may be known whether a storm is increasing or diminishing in intensity, that is, whether the barometric pressure at the center is diminishing or increasing, I have made a comparison of nearly all the concomitant phenomena which could be supposed to have any influence upon the result, and find the following rule to be well-nigh universal: when the barometer rises more rapidly than usual as the storm passes by, the pressure at the center of the storm is increasing; but when, in the rear of the storm, the barometer rises less rapidly than usual, the pressure at the center is diminishing, or the storm is increasing in intensity. A comparison of all the observations of two years indicates that when the rise of the barometer is 22 per cent greater than usual, the pressure at the center of the storm increases one-tenth of an inch in 24 hours; and when the rise of the barometer is 22 per cent less than usual, the pressure at the center decreases one-tenth of an inch in 24 hours. We thus see that a sudden rise of the barometer in the rear of a storm is usually followed by two different effects: the storm center advances more rapidly than usual, and the pressure at the center is diminishing. Sometimes one of these effects predominates, and sometimes the other; but generally, when the storm center is advancing with the greatest rapidity, the pressure of the center of the storm is decreasing, and when the storm's center is nearly stationary, the pressure at the center remains nearly stationary.

The rate at which the barometer falls in front of a storm appears to have very little influence upon the question whether the pressure at the center is increasing or diminishing.

The average of two years' observations indicates that when the winds on the eastern side of the storm's center are very much stronger than those on the western side, the pressure at the center of the storm is increasing; but when the winds on the western side are very much stronger than those on the eastern side, the pressure at the center of the storm is decreasing. This rule is, however, subject to numerous exceptions.

Form of the isobaric curves.

In order to determine the average form of the isobaric curves, and the position of their longer axes, I selected all those cases in which the center of a storm was distinctly indicated upon one of the weather maps, and in which at least one isobaric curve was shown for not less than one-half of the circuit. The number of cases found suitable for this kind of comparison was 203. Of these, nearly one-half were so situated that only half of one of the axes could be measured. These were generally cases in which the center of the storm was near the northern boundary of the United States, and the isobaric curve was incomplete on the northern side for want of observations. In nearly half the cases, the isobaric curve selected for measurement was the lowest isobar shown on the maps; but whenever there was a larger curve traced for at least one-half of its circuit, the latter curve was taken in preference. A line was then drawn to represent the longer axis of this curve, and the length of this line was measured; or when the curve was incomplete the length of half of this line was measured. The length of the line drawn through the center of the isobaric oval at right angles to its longer axis was also measured, and the direction of the longer axis was determined by a protractor. The result of these comparisons was as follows:

In 55 per cent of the whole number of cases, the major axis of the isobar exceeded the minor axis by one-half of its whole length. In 30 per cent of the cases, the major axis was more than double the minor axis; in nine per cent of the cases, the major axis was more than three times the minor axis: and in four per cent of the cases, the major axis was at least four times the minor axis.

These results appear to me to prove that the centrifugal force arising from the circulation of the wind around the storm center cannot be the principal cause of the fall of the barometer, for otherwise the shape of the storm would be more nearly circular.

With regard to the direction of the major axis of the isobars there is no uniformity. The major axis may have any position whatever; but the direction which is decidedly more frequent than any other is about N. 40° E. It will be readily perceived that this direction is almost identical with the general course of the Atlantic Coast, and also with the range of the Alleghany Mountains; and it might be supposed that one or both of these circumstances had some influence in determining the position of the isobaric curves. In order to decide this question, I divided the observations into two classes by a line drawn from Buffalo to Mobile; one class representing the storms of the Mississippi Valley, and the other representing the storms of the Atlantic Coast, and found the results in the two cases nearly identical. The easterly inclination of the storm axes is quite as decided in the Mississippi Valley as upon the Atlantic Coast. It may, therefore, be reasonably inferred that not only the elongated form of storms, but also the prevailing direction of their longer axes, is mainly dependent upon general, and not upon local causes.

Classification of Storms.

The storms which pass over the United States seem to be naturally divided into two distinct classes; the first class including those storms which come from the far west and northwest, the center of whose paths is generally north of lat. 40°; and the second class including those storms which come from the south and south west, and which generally appear to originate in Texas or the Gulf of Mexico. Storms of the second class are comparatively infrequent, forming only about one-sixth of the whole number of storms. They are almost unknown in the summer months, and are most frequent in the winter and spring. The majority of them have their origin west of the mouth of the Mississippi River, and their average course is N. 60° E., being 22° more northerly than the general average of storms, which would show the average direction of storms of the first class to be nearly due east. The average velocity of these storms does not differ much from that of storms of the first class. The majority of storms of the second class reach the Atlantic Coast before arriving at lat. 40°; and from thence their paths generally appear to be nearly parallel to the coast. One of these storms (Oct. 25, 1872) was diverted inland, and advanced northward nearly to Rochester, after which it turned eastward in the direction of Halifax. Another storm (Oct. 19, 1873), after passing near Cape May, was diverted northwestward as far as Mackinac, after which it turned again to the northeast. But these were rare exceptions to a very general rule. Nearly all the storms of this class, as soon as they reach the Atlantic, appear to advance northeastward in a direction nearly parallel to the coast.

Storms of the second class comprehend those violent movements which are commonly called *cyclones.* One of these passed over Punta Rassa, in Florida, Oct 6, 1873, where the barometer in 14 hours fell from 29·96 to 28·40, and afterward the storm continued its course with diminished severity, traveling northeastward nearly parallel to the Atlantic Coast.

The approach of storms of the first class is usually indicated upon the weather maps by the word *low,* at some point west of the Mississippi River north of lat. 40°, generally in Minnesota or Nebraska. Frequently we find the word *low* on the maps of two or three successive days, occupying nearly the same position, indicating either that the center of the storm was nearly stationary for 24 hours, or that the precise position of the storm's center was undetermined, and it could only be located at some distance westward. When the storm has advanced so far eastward that the exact position of its center can be assigned, it generally manifests a decided preference for the region of the northern lakes, particularly Lakes Superior and Huron; and from this region the average course of storms is almost exactly east.

Where do the storms which seem to come from the far west originate?

On the U. S. weather maps the isobaric lines are seldom extended westward beyond the meridian of twenty degrees west of Washington. According to these maps, about three-quarters of all our storms seem to originate in the neighborhood of Nebraska. In order to determine whether such is really their place of origin, I have taken the volume of signal service observations for Sept., 1872, and projected all the barometric observations in curves, arranging the stations in the order of longitude. From a comparison of these curves it appears that for the month of September, 1872, all the principal oscillations of the barometer at Portland, Oregon, can be traced at Fort Benton, Virginia City, Fort Sully, and at each of the other stations extending eastward to Lake Superior and Lake Michigan. In order to exhibit this fact more clearly, I have taken particular cases, and have drawn certain isobaric curves for several successive days. Plate I shows the isobaric curve of 29·8 for three successive days. On the 19th of September a slight fall of the barometer was observed in Montana, probably the result of a fall of rain on some of the mountains of that region. No rain was reported on that day from any of the signal service stations, but the weather was reported "threatening" at Fort Benton and Corinne, and "cloudy" at Virginia City, Cheyenne, etc. The first curve on the left shows the isobar 29·8 for 7½ A. M., Sept. 19th. The second oval shows the isobar 29·8 for 7½ A. M., Sept. 20th. On this day slight rain fell at Cheyenne; the weather was reported "threatening" at Denver, Omaha and Leavenworth; and "cloudy" at Breckenridge, Fort Sully, St. Paul, etc. The oval on the right shows the isobar 29·8 for 7½ A. M., Sept. 21st. On this day, half an inch of rain was reported at Duluth, ⅖ inch at Marquette, and slight rain at a few other stations near Lake Michigan. In two days the center of these isobars traveled eastward about 980 miles, or at an average rate of 20 miles per hour. The next day the storm advanced toward the northeast, and passed beyond the range of our stations of observation.

About three days later, another and more considerable depression of the barometer followed nearly in the track of the preceding. Plate II exhibits the isobar 29·7 for four successive days. On the 22d of Sept. a considerable fall of the barometer was observed from Oregon to Dacota. At 7½ A. M. the isobar of 29·7 was an oval extending westward nearly to the Pacific Ocean, and eastward to the Missouri River, a distance of about 1100 miles. On the same day "heavy rain" was reported at Portland, Oregon; at Fort Benton over a foot of snow fell on the 22d and 23d; snow and rain fell at Virginia City; and it was cloudy at Corinne, Fort Sully, etc. The second oval on Plate II shows the isobar 29·7 at 7½ A. M., Sept 23d. On this day rain or snow fell at Fort Benton, Virginia City, Cheyenne, Fort Sully, Breckenridge, and several places further east. The third oval shows the isobar of 29·7 at 7½ A. M., Sept. 24th. The longer axis of this oval, instead of being turned east and west as on the two preceding days, was now turned nearly northeast toward Lake Superior. Near the center of this oval the barometer stood at 29.36. On this day over half an inch of rain fell at St. Louis, Cairo, Davenport and Duluth, and a less amount at other places.

On the morning of Sept. 25th, the center of the isobar 29·7 was nearly over the middle of Lake Superior. On the north, this isobar extended beyond the range of our observations, so that only the southern portion of the curve could be definitely located. At Duluth the barometer stood at 29·33. On this day considerable rain fell at Duluth, Escanaba, Oswego, Kingston, etc. During the next 24 hours the center of this storm remained nearly stationary over Lake Superior, but on the following day it advanced northeastward, and passed beyond the range of our observations. In three days the center of the storm traveled eastward about 1280 miles, being at an average rate of 18 miles per hour.

It seems probable that this storm originated, or at least was first developed into a storm of considerable magnitude, through the collision of moist air from the Pacific Ocean with some of the high mountain peaks in Oregon, resulting in a heavy fall of rain or snow. The fact which is of special interest is that this storm when once organized, traveled over all the mountain ranges between the Pacific Ocean and Lake Superior without sensible obstruction. The same fact is noticeable in the storm of Sept. 19–22d, and several other storms of the same month. We hence infer that those storms which come to us from Nebraska sometimes originate in the mountains of Oregon, and probably sometimes come from the Pacific Ocean, whence they travel eastward, occasionally passing entirely across the continent to the Atlantic Ocean.

The reductions described in the preceding article have all been made under my direction, but the chief labor has been performed by Mr. Edward S. Cowles, a graduate of Yale College of the class of 1873.

ART II.—*Preparation of Photographic Dry-Plates by Daylight, by desensitizing and re-sensitizing the silver compounds;* by Prof. C. F HIMES, Ph.D., Dickinson College, Carlisle, Pa.

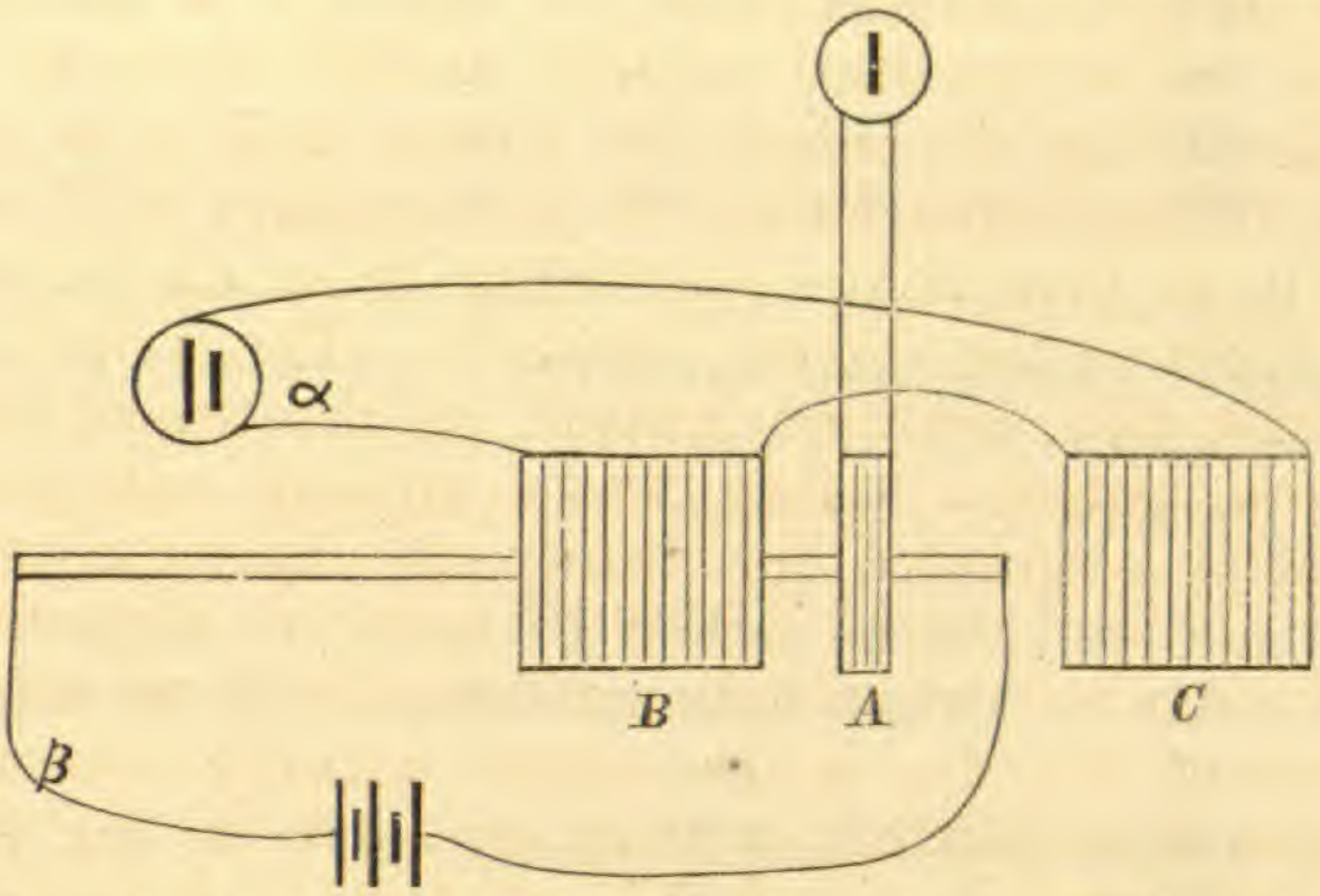
THE inconveniences, and the unpleasant, prolonged confinement in the dark room, connected with the preparation of photographic dry-plates have, perhaps, done more to render their employment uninviting than any feeling of uncertainty as to the results with them, although in many cases they are a matter of necessity rather than of convenience. Among the results of a series of experiments on the desensitizing and re-sensitizing of the haloid salts of silver, published several years ago, a decided simplification of the method for the preparation of dry-plates, more particularly by the tannin process, was obtained, which was adopted in the commercial preparation of dry-plates, and by some individuals. Its apparent conflict, however, with what might almost be termed photographic instincts, has doubtless prevented its more general adoption. Some recent experiments indicating the direction for further improvement, and the recent publication of a process by Krone, embodying, in a measure, a similar principle proposed to be employed in photographing the transit of Venus, render further allusion to it at this time in place. Glass plates coated with ordinary bromo-iodized negative collodion are sensitized in the usual argentic nitrate bath, in bright daylight, and are then thoroughly rinsed, or washed, in a tray of distilled water to remove the larger portion of the adhering silver solution. Any effect of the light upon them during these operations is removed at once, and, as far at least as practical photography is concerned, entirely, by flowing them once or twice with about a five per cent, or even weaker, solution of potassic iodide, or bromide; or the removal of the effect of light, or at least of any developable effect, can be just as effectually accomplished by simply rinsing the plates well, on their removal from the silver bath, and exposing them to the prolonged action, say for several hours, of direct sunlight, or bright diffused daylight. Plates in this condition, insensitive to light, are simply allowed to dry, and can then be kept indefinitely, and are again rendered sensitive to light, in a few minutes, by immersing them, in the dark, in a solution of tannin of 15 grains to the ounce of water, or by flowing such a solution back and forth over them for half a minute, after they have been moistened with distilled water. Being then dried they will retain their sensitiveness unimpaired for months, and, the writer has reason to believe, for years. The time of the exposure in the camera, as well as of development with the pyrogallic acid developer, is about the same as that of ordinary tannin plates, whilst the results are perhaps more certain, and the negatives cleaner.

The principal advantages of the preceding method lie in the complete division of the operations. The plates can be brought to the tanninizing stage leisurely, and comfortably, at any time, in an ordinary well-lighted and well-ventilated room; and the confinement to the dark room, with its damp and unhealthy atmosphere, when all the operations are conducted in it at the same time, is thus reduced to a minimum, since the tanninizing of a dozen plates will require but a few minutes at night. The washing of the plates is also not only accomplished with less trouble, but more perfectly, and the possibility of accidental stains, so great when solutions of such photographic incompatibles as argentic nitrate and tannin must be used in close succession, is entirely eliminated.

Other substances, as different soluble haloid salts, may be employed to desensitize the plates, and other sensitizing agents may be employed, a solution of argentic nitrate being in many respects the best. The sensitizer may also be applied before the plate has been allowed to dry; the exposure in the camera may also be made whilst it is yet wet; and in the latter case the time required for both exposure and development is less; but attempts to reduce it to that required for ordinary wet plates, and thus to obviate the necessity for a dipping bath of argentic nitrate in field photography, were not successful. Experiments upon dried desensitized plates, by "fuming" them with ammonia, as in the ordinary paper printing process, show that it possesses a decided sensitizing effect, but that the degree is dependent on a proper regulation of the time of fuming, since portions of the same plate exposed for different lengths of time to its action manifested differences in sensitiveness under development with the pyrogallic acid developer, after exposure in the camera; and those fumed for the longest time, as well as those for the shortest time, were less sensitive than those acted on for an intermediate length of time. In the process recently suggested by Krone, the operations are also conducted in daylight, the effect of which is *prevented* instead of *removed.* This is accomplished by taking advantage of the well known fact, that argentic iodide, formed in the presence of excess of potassic iodide, is insensitive to light, but may be rendered sensitive. The plates are coated with a collodion containing argentic nitrate, and immersed in a bath of potassic iodide, or in some cases of potassic iodide and bromide, and are then washed, and dried, and subsequently sensitized with argentic nitrate solution. Organic matter is added to all the solutions, including the collodion, in the form of an alcoholic solution of shellac and sandarach, and this may play a very important part in the process. Unless, however, the results obtained are superior in rapidity, or quality, the inconvenience, involved in the preparation of special collodion and special baths, will prevent its replacing the former method, in which all the solutions employed are such as are in common use. The employment of argentic iodide alone is suggested for photographing the transit of Venus, and it is also stated that with bromo-iodized films, it is necessary to protect them from the light, even before sensitizing them, on account of the effect of light on the argentic bromide. The writer in his first experiments also confined himself to the use of iodized collodion and potassic iodide, for a similar reason, but was subsequently convinced by numerous experiments, that this direct, or photo-chemical, action of light on argentic bromide, when a bromo-iodized collodion is employed, as shown in the bluish cast of the film, is also either entirely removed in the subsequent desensitizing, or plays no part in the subsequent development, and is too feeble of itself, if it is not entirely removed in fixing, to merit practical consideration. Neither was it found necessary to prolong the immersion of plates in the silver bath when coated with a collodion containing a bromide, as it seems it is necessary in Krone's process to prolong the immersion of plates coated with the silver collodion, when the bath contains potassic bromide with the iodide.

ART. III.—*Brief Contributions from the Physical Laboratory of Harvard College.* No. IX.—*On a Molecular Change produced by the passage of Electrical Currents through Iron and Steel bars;* by JOHN TROWBRIDGE.

IN the following experiments the magnetism induced in soft iron and in steel bars is employed to show the molecular change produced by the instantaneous passage of electrical currents in an axial direction. The apparatus employed was the following: Two coils, B and C, of thick copper wire 12 cm. long, 9 cm. in diameter, were connected with the poles of a Daniell's cell. The soft iron or steel bars, 2 meters in length, 12 mm. in diameter, were introduced as cores into one of the coils, B. A coil, A, of fine copper wire was slipped upon the core to a distance of 25 cm. from the face of the magnetizing helix B. This coil of fine wire was connected with a Thomson's reflecting galvanometer. The second magnetizing helix of coarse wire, C, was so arranged that its action neutralized the inducing effect of the electric currrent circulating through the helix B. When the circuit, therefore, was made through B and C, an induced current passed through the helix A and the galvanometer, which was due to the magnetism of the core at the distance of 25 cm. from the face of the helix B. The iron core was then made a



portion of an independent electrical current. Thus it was possible to magnetize the iron core by the helix B, and to send a current through it in the axial direction by the independent circuit. We shall call the magnetizing circuit circuit α, and the axial circuit circuit β.

The circuit α was first made and the deflection of the galvanometer noted; it was then broken. The following table shows the results obtained: δ, represents the deflection produced by the magnetism of the bar, δ', after the circuit β was made, δ'', after the breaking of the circuit β. Two Grove cells were used upon the circuit β.

TABLE I.

δ	δ'	δ''
110	150	155
110	145	150
110	150	155
110	150	155

This table shows that the instantaneous passage of the axial current through the iron bar gave rise to an increase in its magnetism. This phenomenon was apparent both at making and breaking the axial circuit. The increase of magnetism was greater on the breaking of the circuit β than on the making. This increase of magnetism disappeared on the second making of the magnetizing circuit α.

The magnetism of the core was then reversed, and the same effects were noted. An instantaneous passage of the axial current β was sufficient to produce the increase of magnetism. The following table shows the effects produced by allowing the axial current β to be made permanently. D and D′ represent the deflections produced by repeatedly breaking the magnetizing circuit α.

TABLE II.

δ	δ'	D.	D′.
90	120	90	90
90	120	90	90
90	120	90	90
90	120	90	90

It will be seen that the same effects were produced as on instantaneously making and breaking; there was no increase or diminution noticeable. Table III shows the effect of rapidly reversing the current β through the iron bar.

TABLE III.

δ	Current in one direction.	Current in two directions.
120	190	240
120	180	230
120	190	240
120	190	240

In all cases the rapid reversal of the current through the iron bar produced a momentary increase of magnetism, which disappeared on the magnetization of the core by the circuit α.

Experiments were then made upon the permanence of the effect of the instantaneous passage of an electrical current through an iron bar. Observations were taken at periods of three hours after the passage of the current through the iron. The results are embodied in Table IV.

TABLE IV.

δ	δ'	D.
160	190	160
150	180	150
137	167	137

Allowance having been made for variations in the batteries used, it was found that the iron bar maintained the molecular state imparted to it by the axial current during the period of observation, viz: three hours.

Table V. shows the effect upon a steel bar of the same dimensions as the soft iron bar previously used. The conditions of the experiments were the same.

TABLE V.

δ	δ'	D.
160	180	160
160	185	160
160	180	160
160	184	160

The increased effect is less marked in steel than in iron. The following table shows the effect of rapid reversals of the current.

TABLE VI.

δ	δ'	D.
160	180	190
160	180	190
160	185	189
160	180	190

It will be seen that the effects in this case are much less than in the case of soft iron. The current was passed through different portions of the iron bar outside of the portion covered by the helix A, which was connected with the galvanometer, but no effect was produced. The effect of heating the iron bar by the passage of the axial current β was infinitesimal on account of the large size of the bar and the instantaneous duration of the current.

The conclusions from the foregoing experiments appear to be as follows:

1. The passage of an electric current through an iron or steel bar produces a molecular change in it, which is apparent both at the closing and breaking of the circuit.

2. The rapid change of direction of a current through iron or steel bars produces a molecular disturbance which is greater than that imparted by a current sent in one direction alone.

3. Magnetization of the iron or steel bar is sufficient to restore it to the normal magnetic state which is imparted by the magnetizing helix.

4. The molecular action increases with the strength of the electric current.

No. X.—*Magnetism of Soft Iron;* by DAVID SEARS.

The Comptes Rendus of Jan. 12th, 1874, contains an article by M. J. Jamin on the magnetism of soft iron. M. Jamin investigated the magnetic condition of an iron bar which formed the core of two helices. The poles could be made opposite or the same at will. The strength of the magnetization of the iron was tested by moving a piece of iron along the bar and ascertaining the amount of force necessary to detach it. M. Jamin's experiments showed that the magnetization was greatest when two north or two south poles were opposed, and least when a north and south pole were directed toward each other. These results seem to him to require a modification in the received theory of solenoids.

In the following experiments the soft iron bar, instead of forming the core common to both magnetizing helices A and B, formed their armature. A coil of fine wire was slipped upon the iron bar, which was graduated in millimeters. This coil, which we shall term C, was connected with a reflecting galvanometer. The coils of the bobbin C being thus at right angles to those of the magnetizing helices A and B, the effects of the induction by the current circulating through A and B were obviated and the induced current which arose in C on making the magnetizing circuit was due to the magnetism of the iron bar alone. This method has great advantages over that of the proof bar. The observations can be taken readily and quickly; and it was found that the charge of magnetism through a space of a millimeter could be readily measured. In the first experiment the two magnetizing helices A and B were placed directly opposite each other, with their cores touching the middle of the iron bar. Table I. represents the results obtained when a north pole was opposed to a south pole.

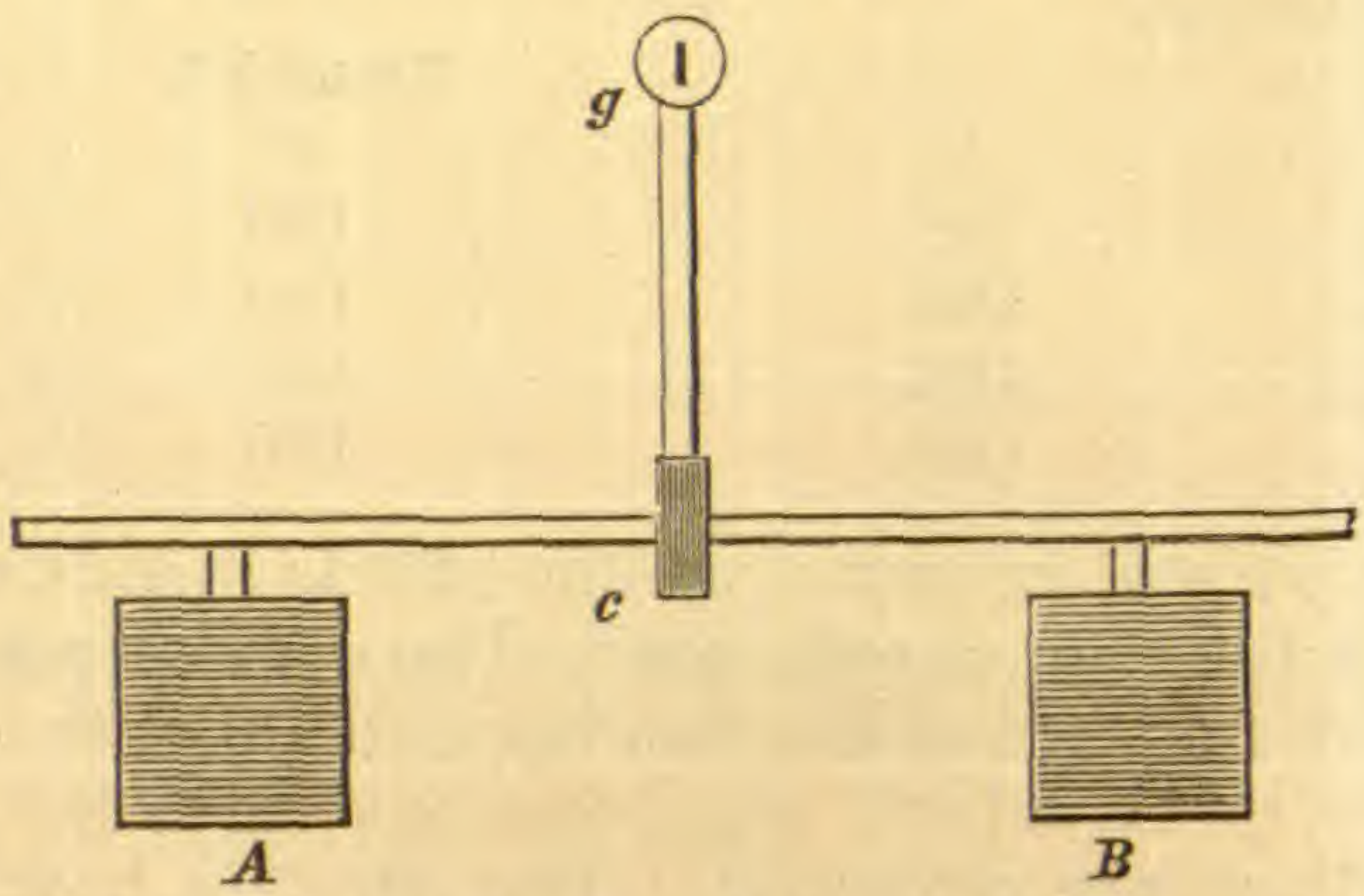


TABLE I.

Distance of the bobbin C from A and B in centimeters.	Deflections.
1	14
2	13
3	10.5
4	9

TABLE II.

Distance of the bobbin C from A and B in centimeters.	Deflections.
8	360
9	328
10	308
11	278

Table II. represents the results obtained when two north or two south poles were opposed.

In the latter case observations for the first four centimeters could not be taken, for the spot of light from the reflecting galvanometer was thrown off the scale. It will be seen that the magnetization of the bar is far greater in the latter case than in the former. at points remote from the opposed poles. Between the poles, however, the magnetization was greater when poles of the opposite name were opposed. In the above experiments the bobbin C was placed beyond the magnetizing helices. In the experiments which follow it was placed between them. In Table III. the distances of the bobbin C from the point midway between the two helices A and B are denoted by x, and the corresponding deflections by y.

Fig. 2.

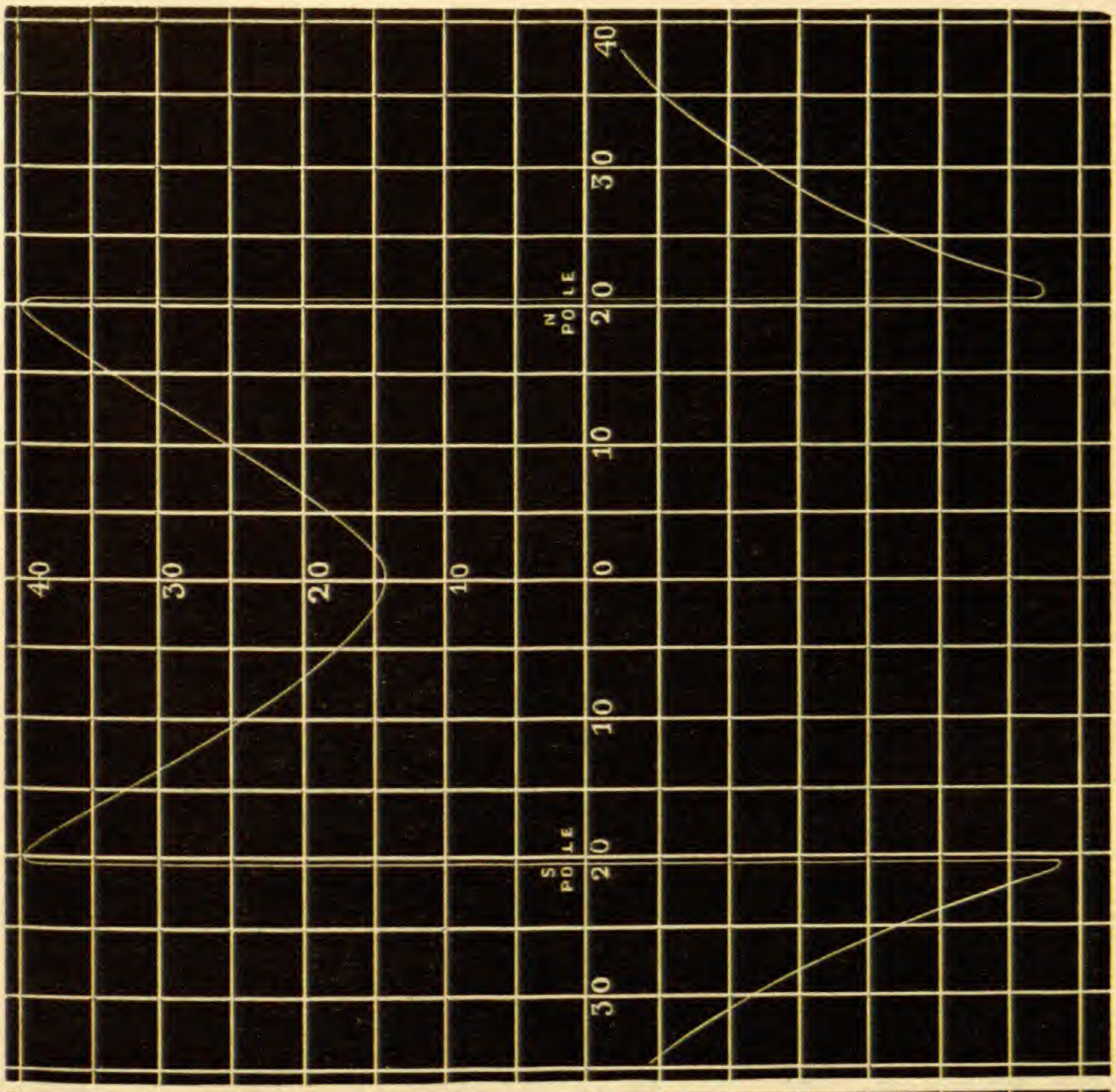

40
30
20
10
0
10
20
30
40
N POLE
S POLE

Fig. 1.

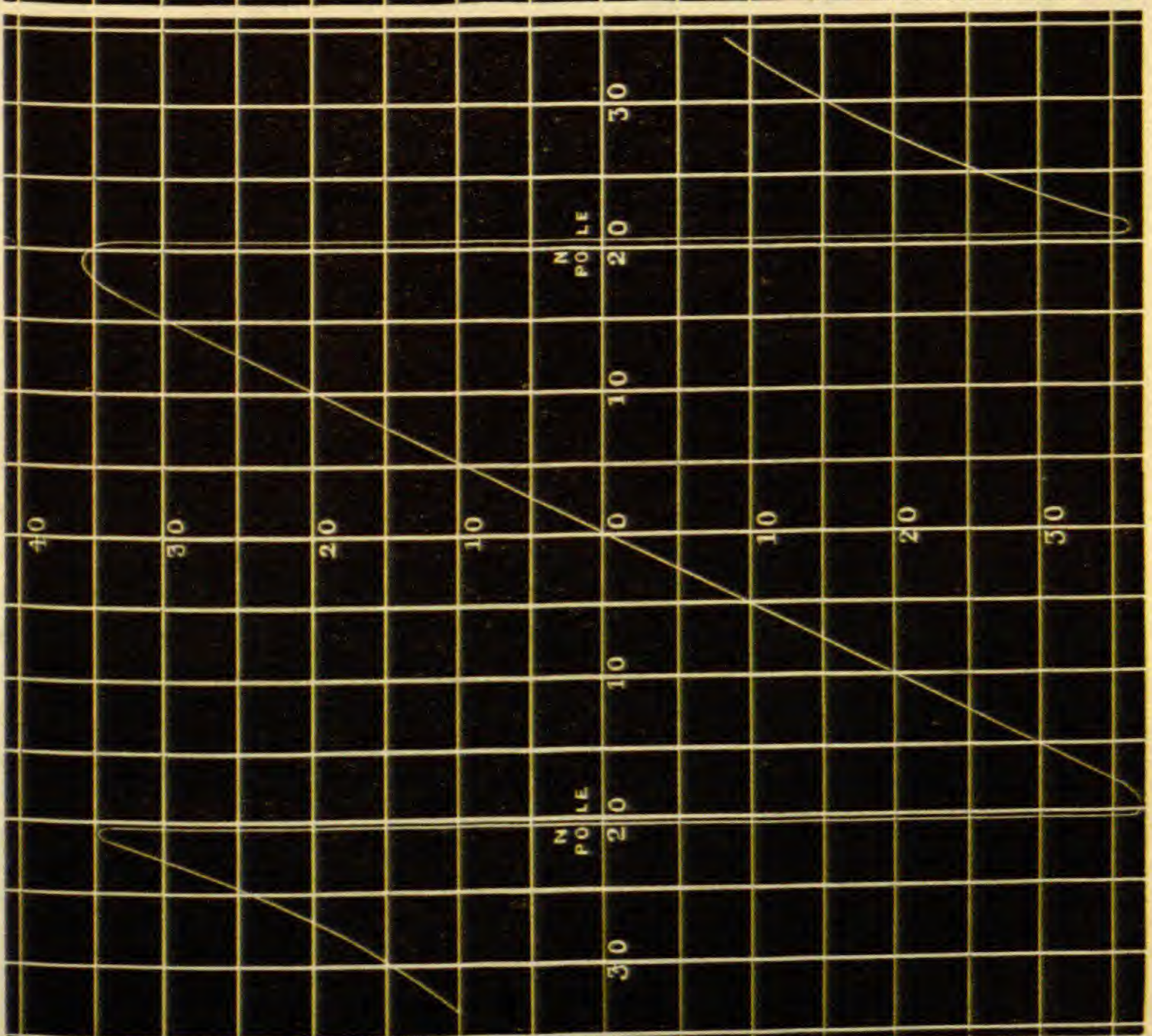

40
30
20
10
0
10
20
30
N POLE
N POLE

TABLE III.

Values of x.	Positive values of y.	Values of x.	Positive values of y.	Values of x.	Negative values of y.
0 cm.	0	14	137	21	220
1 "	5	15	150	22	210
2 "	15	16	164	23	197
3 "	22	17	180	24	185
4 "	30	18	190	25	170
5 "	37	20	204	26	156
6 "	46	21	220	27	142
7 "	55			28	130
8 "	65			29	120
9 "	76			30	112
10 "	86				
11 "	87				
12 "	110				
13 "	125				

These results are expressed by the curve in fig. 1. The positions of the two north poles were $x=+20$ and $x=-20$.

Table IV. represents the values obtained when a north pole was placed at $x=+20$ and a south pole at $x=-20$.

TABLE IV.

Values of x.	Positive values of y.	Values of x.	Positive values of y.	Values of x.	Negative values of y.
0 cm.	85	13 cm.	194	17 cm.	230
1 "	90	14 "	205	18 "	220
2 "	95	15 "	220	19 "	205
3 "	100	16 "	230	20 "	200
4 "	105			21 "	185
5 "	110			22 "	172
6 "	120			23 "	160
7 "	126			24 "	145
8 "	136			25 "	134
9 "	145			26 "	120
10 "	160			27 "	105
11 "	170				
12 "	180				

These values are expressed by the curves in fig. 2. The results obtained when the bar whose magnetism is tested forms the armature of two electromagnets are the reverse of those got by making the bar the core common to both the magnetizing helices A and B, which was the case investigated by M. Jamin. While his experiments tends to invalidate the theory of solenoids, ours tend to confirm it.

The following appear to be the conclusions from the preceding experiments.

1. With poles of the same name opposed to each other the magnetization of an iron bar forming the armature of the two

poles is greater on a part of the armature beyond the two poles than it is when poles of opposite signs are opposed.

2. On points of the armature between the two poles the magnetization is greatest when poles of the opposite names are opposed. A north and south pole attract an armature, therefore, with much greater force than two north or two south poles.

3. M. Jamin's conclusions from the experiments upon an iron bar forming a core to the enveloping helices are as follows:

3°. "If the theory of solenoids is admitted, the action of parallel currents should be to increase the intensity of magnetiza tion; on the contrary, it is diminished.

4°. When the currents in the magnetizing helices run in opposite directions, they should act opposed to each other on the currents circulating around the particles of the iron, and should diminish each other's action; on the contrary, it is increased.

5°. The action of the helices should be annulled at the middle of the bar. It is not."

When the bar to be experimented on forms not the core, but the armature of two electromagnets, the effects obtained are the reverse of those obtained by M. Jamin, and tend to confirm the theory of solenoids.

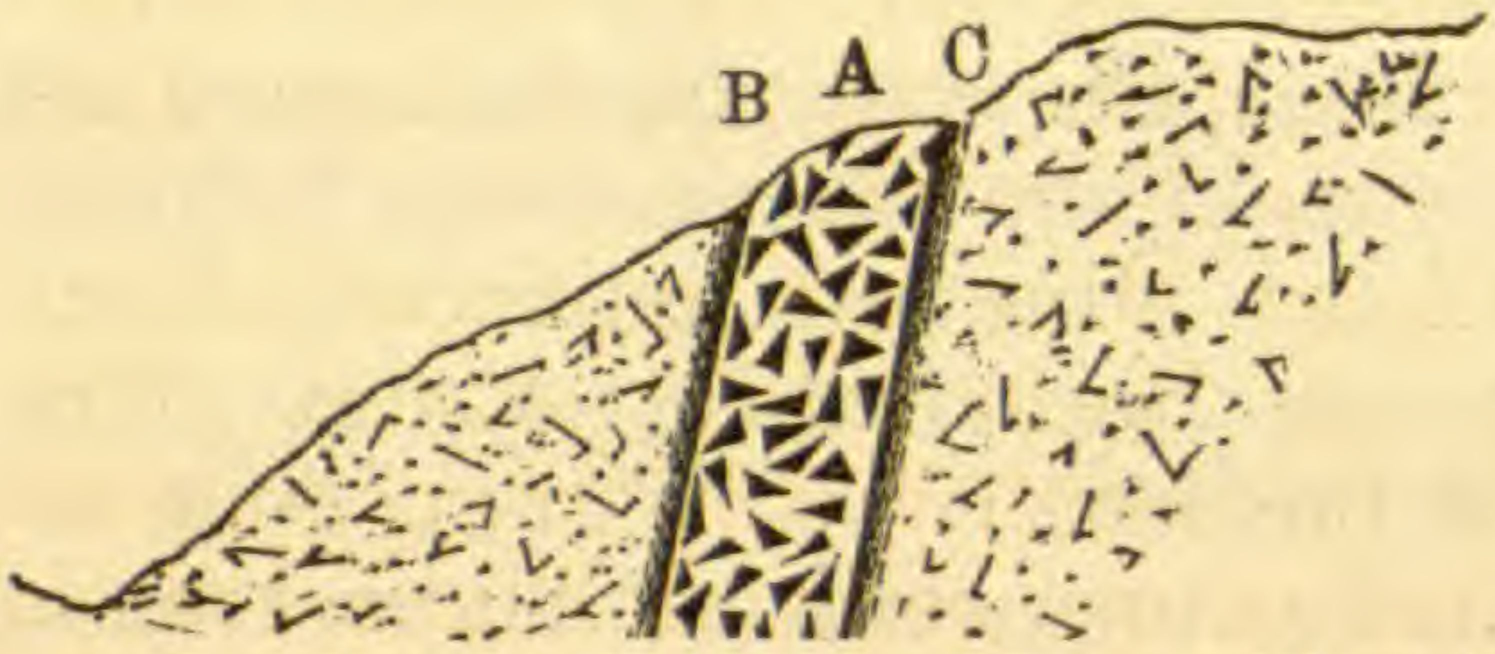
ART. IV.—*Mineralogical Notes; Tellurium Ores of Colorado;* by B. SILLIMAN.* *With a note by* ARCH. P. MARVINE, *on the Position and Geology of the Gold Hill Mining Region.*

IN May, 1873, I briefly announced the discovery of tellurium gold ores at the Red Cloud Mine in Colorado, and stated that Prof. N. P. Hill, of Blackhawk, had proposed to send me specimens of these ores.† The specimens sent by Prof. Hill were long in reaching me, and it is only recently that I have examined them. The observations made in the summer of 1873 by the officers of Dr. Hayden's Expedition have supplied the data needful to understand the mode of occurrence of these ores, for the details of which reference is made to Mr. Marvine's notes and map, which form part of this paper.

* The substance of this paper was communicated at the April session (1874) of the National Academy of Sciences at Washington. Since then, Mr. Marvine, of Dr. Hayden's Expedition, has kindly furnished me with the accompanying map and section, showing the position of the Gold Hill Mining Region, in advance of his forthcoming report, with a brief statement of the geological structure of the district, which I am happy to be able to quote as a substitute for the less detailed statement made orally by me to the Academy.

† This Journal, III, vol. v, 286.

It appears from them, in general, that near the mining hamlet of Gold Hill, about twenty-five miles northwest of Denver City, and at an elevation of almost 8,000 feet above tide, is a wide dike of prophyry cutting the metamorphic rocks, probably of Archæan age, about six miles west of where the Triassic rocks die out at the base of the mountains. A section of this dike, A furnished by Mr. Marvine, is annexed, showing the tellurium-bearing veins B and C on its sides. The porphyry of which it is composed has distinct crystals of feldspar implanted in a purplish-gray paste. These crystals have a greenish-white color, and are evidently partly decomposed. As seen in a microscopic section, it shows the usual obscurely crystalline ground-mass of felsite, with crystals of quartz, and sections of feldspar crystals showing the parallel bands of a triclinic species. A glance at the map shows the position and course of this dike, and also the existence of other dikes of porphyry in the same region. The porphyry from the "7·30" and "Central" Mines closely resembles that from the "Red Cloud," while that from a dike (No. 136) between the "7·30" and the "Americus" is distinctly trachytic, and that from the "Niwot" Mine, at the west margin of the map (No. 181), is a quartz-porphyry, with distinct crystals of biaxial mica. Those from the dikes at Jim Town (specimens No. 147), on the north border of the district, are distinct sanadin-trachyte.



A. Porphyry dyke. B, C. Veins with gold and tellurium ores.

The tellurium ores have been explored, so far, only in connection with the dike near Gold Hill, shown in the section, although they exist with the dike at "7·30" and the "Central." These ores are found along the line of contact of the walls of the dike, in a quartz gangue, associated chiefly with pyrite in small, brilliant, highly modified crystals, and rarely with chalcopyrite and sphalerite. Prof. Hill speaks (loc. cit.) of lead; but I have found no salts of this metal in the specimens received. The quartz is chiefly hornstone and uncrystalline quartz, and, on the side of the country rock, it is mixed with feldspar. Native gold is not visible in any of the specimens I have seen of this ore from below the surface; but where the surface is weathered, it exhibits free gold, arising from the decomposition of the tellurets.

On the sides of the dike the line of division is clearly defined, but not so on the side adjacent to the metamorphic rocks, it blending on this side with the granitic materials. The thickness of the veins varies from four or six feet to a few inches, but the rich tellurium ores form a comparatively narrow seam near the center of the vein. The Red Cloud Mine, which is found on the under side of the dike, has been explored to a depth of about seventy feet. The Cold Spring Mine is explored on the upper side of the dike. The tellurium ores are not found in the body of the dike, but have (owing probably to the long continued high temperature of the dike) found lodgement in the granite outside of the walls, and not in immediate contact with them.

The species at the Red Cloud Mine are native tellurium, sylvanite and hessite (which has been called petzite). The simplicity of the mineralogy of this locality is in strong contrast with what is found in the tellurium veins of Transylvania, which are mentioned more particularly farther on.

Native Tellurium.—The occurence of this rare species in the United States, in California, was mentioned by Dr. Genth with a query, in his Contributions to Mineralogy, No. vii (this Journal II, xlv, 313). Its existence in the Red Cloud Mine is unequivocal. It was simultanteously, yet independently, detected by Dr. Endlich and myself, in a small specimen from the collection made at the mine last summer, and now forming part of the Smithsonian Collection in Washington. It did not exist in the collection of those ores sent to me by Prof. Hill. The hexagonal cleavages are perfect, and one small and very perfect crystal was found, which has been measured by Mr. E. S. Dana. Its reactions before the blowpipe are perfectly in accordance with those of the species. It contains no selenium and only a trace of gold.

Auriferous Hessite.—This mineral has been spoken of as petzite; but it contains much too little gold for this latter species.* Its sp. gr. is 8·6; luster splendent metallic when freshly broken; fracture conchoidal, brittle, but somewhat malleable; under the pestle laminates into thin scales, and is with difficulty reduced to fine powder, leaving on the agate surfaces metallic streaks of plumbago-like color. Color telluric, tarnishes blackish on exposure, sometimes iridescent. Cleavage none.

Before the blowpipe in the closed tube, the pure mineral (with no trace of pyrite) decipitates, fuses to a globule adhering to the glass, and exhales a white sublimate, fusing into clear colorless globules. Alone on coal in both flames it gives a globule, coats the coal with the characteristic areola of tellurium and tellurous acid; it does not exhale any odor of selenium, nor show any trace of lead. The globule is non-magnetic if pyrite is absent, and does not vegetate with silver as hessite does;

* Mr. A. Eilers, M.E., in a notice of the Red Cloud Mine, in the Transactions of the American Institute of Mining Engineers, vol. i, p. 315, considers it petzite.

with soda it gives a large bead of silver, which dissolves in nitric acid, leaving gold in powder.

Cupellation gave gold 6·40 per cent, and silver 50·90. By a partial analysis I found, in the wet way, gold 7·131; silver 51·061 per cent. Understanding from Dr. Genth that he is engaged in the chemical investigation of this species, as well as of the other tellurium minerals of the Red Cloud Mine, with abundant material, I have willingly abandoned this work to him, satisfied that it cannot be in better hands.

Sylvanite.—This species from the Red Cloud Mine yields in the open tube a faint odor of selenium, and the gray ring of tellurium is preceded by a slight reddish ring of selenium. In the closed tube the ring of tellurium is more distinct, and the deep yellow-brown vapor of the metal is clearly seen, but the selenium is not evident.

Alone on the coal it fuses with exhalation of the odor of selenium and its well characterized blue flame. The first touch of the outer flame causes a liquid fusion, coating the coal, like argentic nitrate, with a silver film, and a yellow areola appears before the white film of tellurium oxide. Continuous flaming in the reducing flame produces a well marked yellow brown areola within the tellurium ring, becoming as it cools much more brown. It probably contains lead and antimony. Its reactions are not those given by Berzelius for sylvanite. It contains by assay gold and silver in the proportion 1·7 to 1. In the formula (Au. 28·5 Ag. 15·7) the ratio of the gold and silver is 1·8 : 1. My stock of this species was not sufficient to permit a determination of the specific gravity.

Prof. Hill, who has smelted large quantities of the ores of the Red Cloud Mine, informs me that "these minerals exist in this ore as minute particles, or so finely divided as to produce the effect of a stain in the rock. One of the specimens sent—the darkest colored—assayed here, was found to contain 1890 ounces of gold and 5300 ounces of silver to the ton of 2000 lbs."—about $50,000 in value.

Comparing what is known of the mineral associates of the tellurium ores of Colorado with those of Transylvania, as described by Von Cotta, the great simplicity of the mineralogy of the Colorado veins becomes very conspicuous. The age of the porphyry dikes which cut the Archæan rocks of Colorado has not been determined; but it is probable that they are more recent than the Triassic rocks which flank the base of the mountains. The tellurium veins of Offenbánya are accompanied by igneous rocks of more recent date than the Eocene sandstones, and those of Nagyag exist only in connection with igneous rocks, also of probable Eocene age (called by Von Hingenau "greenstone porphyry"), and composed of feldspar and amphibole, which have broken through sandstones and argillaceous shales. In Offenbanya, the tellurium ores occur under very peculiar geological conditions; that is, in veins in igneous rocks, and in segregated masses in granular limestone. The veins occupy thin clefts, fifteen of which on one property are tolerably parallel to each other (E. and W., dip 30°–40° N.), with an average width of about an inch, and they carry chiefly sylvanite and nagyagite sparingly distributed, and more rarely native gold. The chief matrix is quartz and diallogite, associated with pyrite, galenite, sphalerite, stibnite, native silver and pyrargyrite.

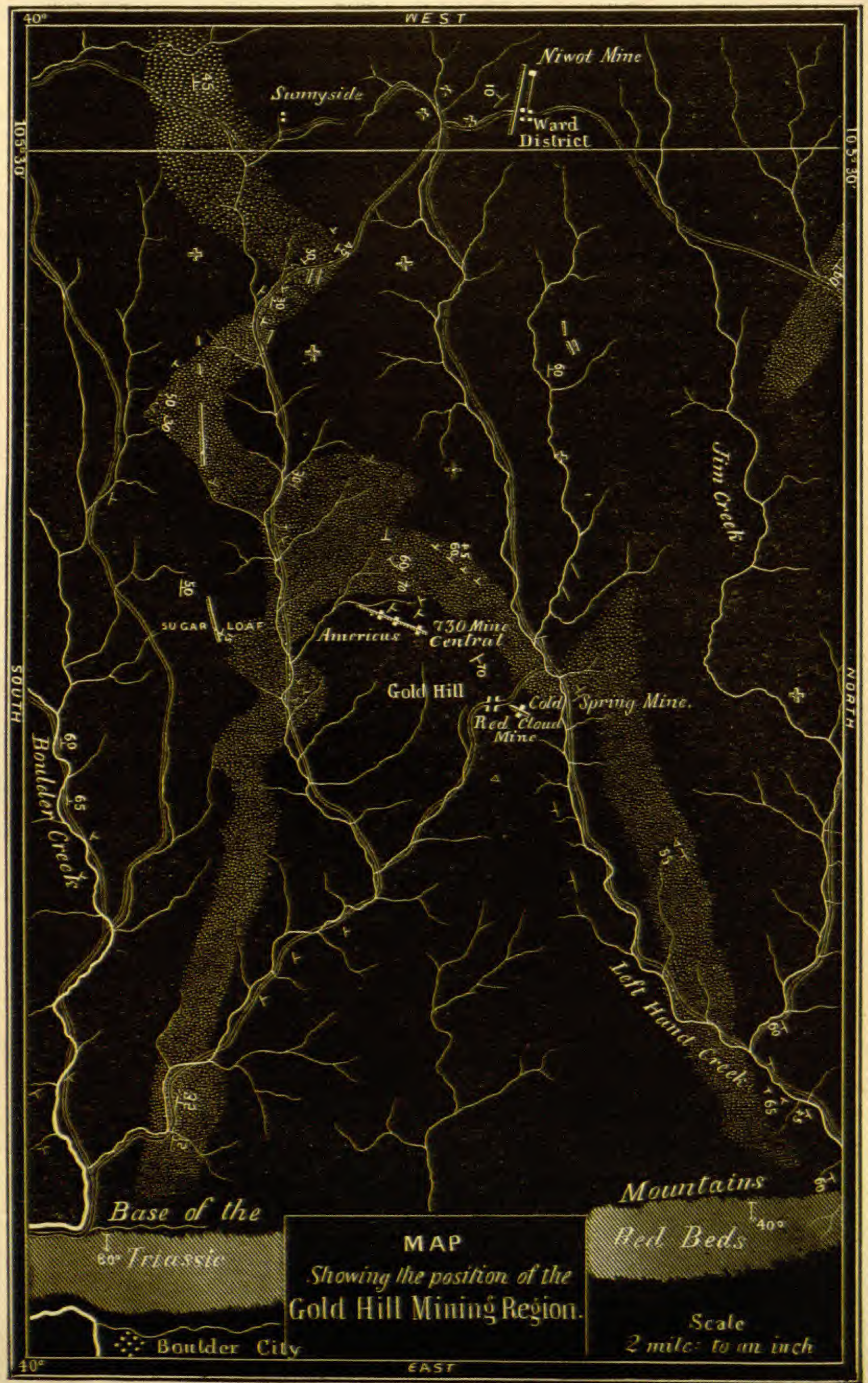
The gangue of the Nagyag lodes is diallogite, or brown spar, or calcite, or hornstone and quartz, it varying in the different lodes and in different parts of the same lode. The gold-bearing tellurium ores are scattered through this gangue with manganblende and pyrite. The chief ores worked are nagyagite, sylvanite, gold, auriferous iron pyrites, argentiferous tetrahedrite, native silver and galenite. Associated with these are hessite, bournonite, jamesonite, barite, sphalerite, stibnite, native arsenic, realgar, orpiment, silver glance, chalcopyrite, marcasite, native copper, malachite, pyrrhotite, sulphur, &c., with various epigene species. In all, over forty mineral species are enumerated as found in the veins of Nagyag. Compared with this abundance, we find at the Red Cloud Mine only native tellurium, sylvanite, hessite, pyrite, chalcopyrite, and more rarely galenite and sphalerite, with native gold as an epigene species at surface. The gangue stone is hornstone or chalcedonic quartz, with feldspar.

Possibly explorations at greater depths may develop other species; but this result has not followed the deep workings of the silver mines in Nevada, where, at the depth of 1500 feet, the number of species found is not greater than it was at the surface. A like paucity of species characterizes the metamorphic and volcanic rocks of the Sierra Nevada in California.

Position and General Geology of the Gold Hill Mining Region; by ARCH. R. MARVINE, Geologist of the Geological Survey of the Territories in 1873.*

THE accompanying map has been prepared to show the general outlines of the country in the neighborhood of the Gold Hill and Ward mining districts, in the mountains of Colorado, while the following remarks explain briefly the position of the region in relation to the surrounding country, as well as its general geology, as determined during the explorations of the summer of 1873.

* These notes form part of Mr. Marvine's Report, in the volume of Reports of the Expedition for 1873.


WEST
EAST
SOUTH
NORTH
40°
105°30'
Niwot Mine
Sunnyside
Ward District
Jim Creek
SUGAR LOAF
Americus
730 Mine
Central
Gold Hill
Cold Spring Mine.
Red Cloud Mine
Boulder Creek
Left Hand Creek
Base of the
Mountains
Triassic
Red Beds
Boulder City
MAP
Showing the position of the
Gold Hill Mining Region.
Scale
2 miles to an inch

Nearly parallel with, and a few miles west of, the western border of the country represented on the map, rises the main continental "divide" in a north-and-south crest, which here reaches an altitude of nearly thirteen thousand feet above the sea level. From the base of the main crest a zone of mountainous country extends eastward, which is cut through by the streams in a general east-and-west direction.

The intervening ridges are not sharp, but of a massive character, often with undulating surfaces, their higher points usually reaching in general a pretty uniform level, the ruggedness of the country being produced by the deep cañons of the stream. It is a portion of this region that is represented on the map.

At the east (near the border of the map) the region abruptly ends along a nearly north-and-south line, the massive spurs falling to the zone of "Hog Backs," or ridges of upturned sedimentary rocks, which lie all along the base of the range.

The "red beds," probably of Triassic age, form the innermost ridge, lying directly on the Archæan rocks of the mountains. These, in going eastward, are followed by the upturned edges of Jurassic shales, the Cretaceous groups, and the great Lignitic formation, of as yet disputed Cretaceous or Eocene age, which stretches eastward, and forms the beds directly underlying the Great Plains.

Boulder City is on the border between the mountains and plains, and is reached by railroad, Denver City being but twenty-five miles to the south and east. From Boulder City, wagon roads up the various cañons give access to the mines in the mountains. South of the region represented on the map, from fifteen to twenty miles, is Clear Creek, much like the Boulder in general character, on the tributaries of which the better known mining regions of Georgetown, Idaho, Empire, Black Hawk, and Central City are situated.

The rocks of the mountains, as a whole, may be considered as being composed of a great series of metamorphic rocks of Pre-silurian age. Quartzites, siliceous, micaceous, some hornblendic and garnetiferous schists, gneisses and granites, all occur; the gneiss, with possibly granite, in the greater proportion. While large areas of structureless granite abound, apparently of so-called plutonic or eruptive origin, search seldom fails in finding spots or areas more or less large of gneissic or even distinct schistose structure. The fact that these usually merge imperceptibly into the surrounding granite, as well as conform in their strikes and dips to the general system of folds, as more plainly indicated perhaps in adjacent schistose regions, show that such granites have been metamorphosed *in situ* and are indigenous rocks. At the same time sharp lines of demarcation, and the occurrence of dikes and allied features, show that the conditions of extreme metamorphism have probably been accompanied by a great softening of the rock, allowing ready molecular rearrangement into structureless forms, and producing plutonic and other appearances indicative of an exotic character.

The same granite mass, approached from opposite sides, might convey entirely different impressions as to its origin ; a metamorphic indigenous nature being indicated upon the one hand ; an eruptive, exotic origin upon the other.

I doubt if any of the large granite masses of the mountains are of true intrusive character, and even if those smaller ones which are clearly intrusive have come from great distances below, or are other than of the same series of rocks melted by the heat accompanying the metamorphism of the mass.

Along the south side of the map, and exposed by the cañon of the Boulder Creek, are massive gray granites, with but few points where any structure was observed.

All along this half of the map the general strike is approximately east and west, with a northern dip. This is the case also along its west border. Near the north and east sides, however, the dip is south, indicating a synclinal structure running through the middle of the eastern portion of the map.

A horizon in which a definite schistose structure tends to occur is indicated by the dotted area running through the center of the map. Some of the rocks here are distinct schists, but little changed, and include very irregular red and black banded mica schists, garnetiferous, and some handsome, fine and evenly banded, gray gneissic schists. In places, this zone may be lost in granite, but opportunity did not offer to carefully follow it throughout. Most of the granite on the north edge of the map contains little if any mica, tending to a reddish granular aplite. The schist zone shows the folds of the formation very well, some of them being very abrupt, and regions of great contortion. All the observed strikes and dips are indicated on the map, but the general structure of so small an area cannot be well shown separated from the surrounding country.

These schists and granites are pierced at many points by a number of dikes of felsite porphyry, which are also indicated on the map. Usually these form hills or ridges, and while some are quite long, the prophyry has apparently often found vent through less extended openings, now showing as sugar-loaf formed hills, without the direction of the dike being clearly indicated. Such forms are shown by a cross. The porphyries vary considerably in character, but no careful comparative examination of them has as yet been made. Some contain remarkably handsome crystals of fieldspar, often of the form of the Carlsbad twins.

The tellurium ores of Gold Hill occur in connection with one of these dikes. See the section on page 26. This dike varies from forty-five to thirty-five feet in width, trends about N. 30° E., and dips approximately 80° to the northwest. On the east side is the Red Cloud, on the west, the Cold Spring mines. The former, upon a casual examination, showed a well defined hanging wall or that on the side of the porphyry, a vein usually three or four feet in width, but in places pinching out to a few inches, with at one point a clay-like "gouge," and an indefinite foot wall on the side of the coarse gneissic granite, which is here the country rock. Some of the vein matter appeared as if a decomposed granite.

Pyrite is the most frequently occurring mineral in the dull quartzose gangue of the vein; with it there is some zinc blende. I had no opportunity to examine into any paragenetic or other feature of the tellurium ores.

ART. V.—*Notes on Diffraction Gratings;* by JOHN M. BLAKE.

DURING the past year I have had an opportunity to photograph some diffraction gratings. For this I have to thank Prof. George F. Barker, and also Mr. D. C. Chapman; who sent for the purpose a ruling on glass 6480 lines to the inch, by Mr. L. M. Rutherfurd.

In reproducing these lines by photography, it is necessary to employ contact printing; for no lens would give the required definition over so large a surface, either for copying a ruling, or for originating a grating, by reducing from a large drawing on paper. A ruling one inch square, containing 6480 lines, would be represented by a drawing 27 feet square, in which the lines would have a separation of one-twentieth of an inch. The sensitive plates used in these trials had a hard and very perfect albumen surface, which admitted of close contact with the ruled plate. The impression was made by a beam of sunlight. The resulting photographs were negatives, having white lines on a dark ground; the dark spaces being from once to twice the width of the lines. They gave more brilliant spectra than the original ruling, and this was doubtless owing to the greater intensity and contrast of the lines. But it was found, on examination, that the photographs had an unexpected defect; and in investigating the cause of this some interesting phenomena were noticed.

When a ruled plate is laid upon a film and lighted by a window, a series of rings or irregular bands can be seen on the grating, and similar appearances are reproduced in the photograph. These were at first thought to be Newton's rings. They varied on pressure, but could not be made to disappear by this means in every case; as the slightest irregularity of the film prevented absolute contact. It was found that a separation of a thickness of paper between the surfaces did not cause these bands to disappear, but increased their number. They were therefore not due to the "colors of thin plates." It will readily occur that these bands are of the same character as the effect seen on viewing one picket fence through another or the shadow of a piece of wire cloth through the cloth itself. The two sets of lines coincide in the light spaces, while in the dark bands the lines of the two series alternate. In the case we have to deal with there are one or more reflections between the brilliant surface of the film and the ruling. The precaution was always taken to stop reflection from the back of the sensitive plate by backing in optical contact. It may be interesting to note in this connection that a series of spurious bands can be seen crossing the spectra of a lamp when looking in one oblique direction through a glass ruling, the ruled surface being next the eye. When this position is found, the bands can be made to cross the first and second spectra, either parallel to true spectral lines, or obliquely, by rotating the glass plate in its own plane. These bands are evidently of the same character as those that have been described.

If we take a photograph which was made with imperfect contact, and hold it away from the eye toward a lamp, so as to catch the second spectrum on its surface, we will often observe one or two irregular bands. If the grating is now moved further to one side, so that the third spectrum begins to cover it, a number of bands will make their appearance, and in the fourth spectrum the number is still further increased. As a general rule, the light space between the dark bands in one spectrum is cut through by one or more dark bands in the next higher spectrum, and the dark band previously existing becomes a light space. Thus it happens in the higher spectra, which overlap, we may see two colors in alternate bands and not the two colors combined, as would be the case in a perfect grating. In some photographs the contact has been so perfect that no dark bands appear until the third or fourth spectrum is reached. The first spectrum is little impaired in any of the photographs of the 6480 line grating. The most brilliant spectrum observed was through a grating which showed numerous dark bands in the second and third spectra. It is believed that, notwithstanding the defect causing these bands, there is not produced a radical change in the accuracy of position of the lines. As proof of this may be mentioned the constancy of obtaining certain results now to be described. The following appearances were produced without any visible connection with the accidental bands on the photographs themselves, provided they were not viewed by light more oblique than was required to show the first spectrum.

If two photographs are superposed with the films in contact and the lines brought nearly parallel, the inaccuracies in the ruling, though exceedingly small, are brought out in a striking manner. When the lines are brought exactly parallel, in the copies of the 6480 grating, by Mr. Rutherfurd, there is presented an almost unbroken shaded surface, excepting that a periodic error is shown by shaded bands parallel to the lines 72 to the inch. If the plates are now turned a very little on one another, dark bands make their appearance, crossing the rulings at right angles, but subject to irregularities. First, the periodic error shows itself in the zigzag character given to the bands, and this is strikingly obvious. A displacement of the bands occurs at one place in the ruling, but after a short space they again return to their position.

The first or periodic error must be due to the screw of the ruling apparatus. The second may have been due to a slight lateral displacement of the diamond point, by some means, which continued for a short interval. As the plates are further turned upon one another, the bands rapidly increase in number and become nearly straight, and when the lines are inclined at an angle of five degrees, the cross bands become too close and fine for the best eye to detect.

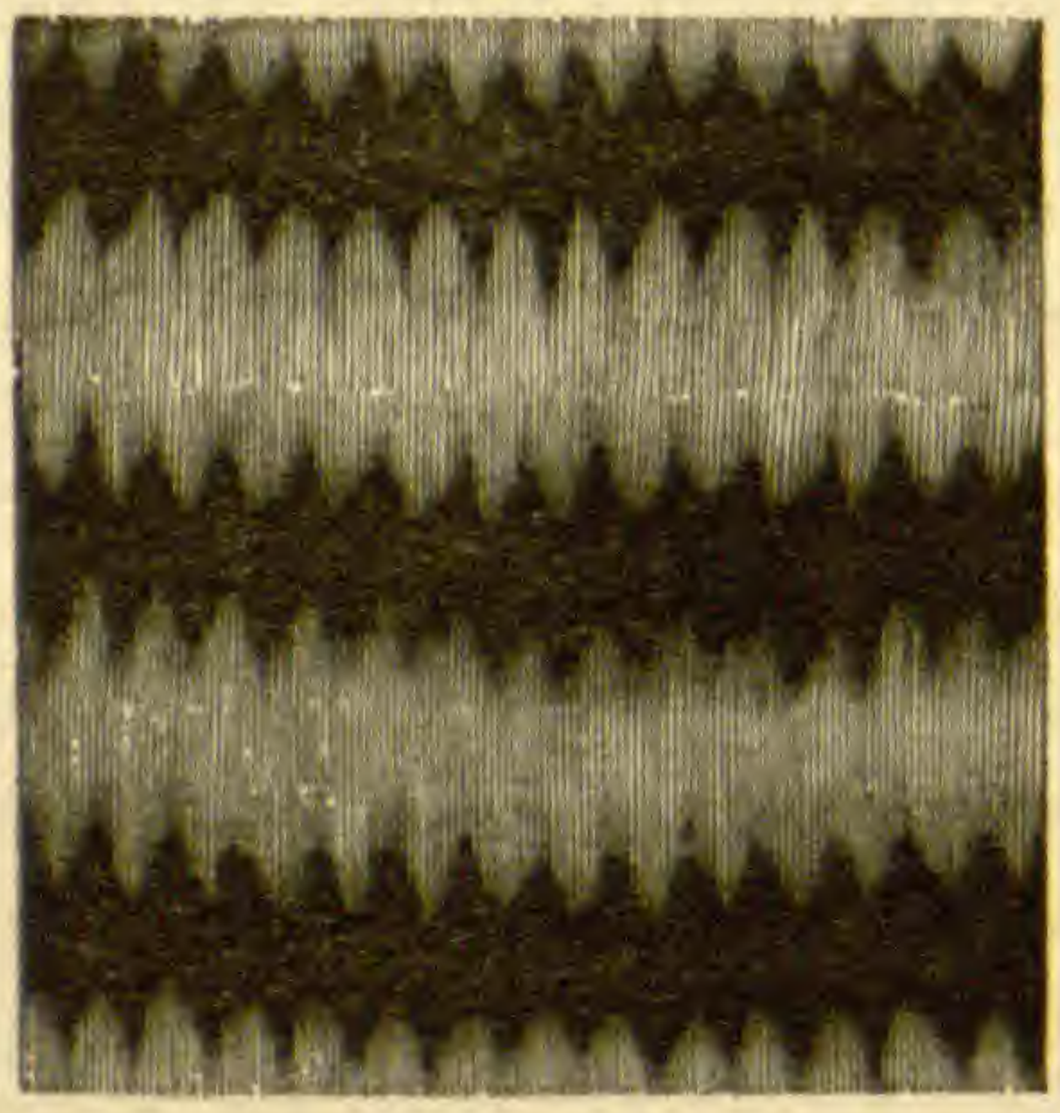
In order to get a clear understanding of these appearances, closely superpose and hold two sheets of ruled letter paper to the light. By crossing the lines more or less a diamond-shaped lattice work will be developed, and the cause of the secondary lines will be obvious.

These secondary lines give spectra according to their distance apart, and we have here a means of producing a *variable grating* which may find application in some investigations. By crossing the lines at nearly a right angle a number of additional secondary spectra can be seen. First, and most prominent after those due to the original lines, we have the diagonals 1–1; next 2–1, 3–1 and 4–1, &c; then 2–3. The four latter have been observed and there should be eight of each. The only second secondary spectrum observed by lamp-light was that of 1–1. This observation refers to the 6480 line grating.

The bands developed by superposition were photographed. This was first attempted by diffused light, and the best results were obtained when the light was oblique. But it was found difficult to prevent the doubling and splitting up of the bands at one end, where the light that entered the lens became more oblique to the grating, and the irregular bands of the individual photographs began to make their appearance. To remedy the last named difficulty, a lens of longer focus, about twenty inches solar, was tried, and then by using sun-light a much better result was secured. It was found altogether best with the 6480 line grating to move camera and grating attached, so as to make an angle with the direct beam, and into the proper direction to have the blue and violet of the first spectrum cover the image. Then the bands came out with a distinctness not before obtained, and the actinic effect was sufficiently uniform over the whole surface. The resulting photograph was transferred to the wood block for engraving, and the result is given in fig. 1.

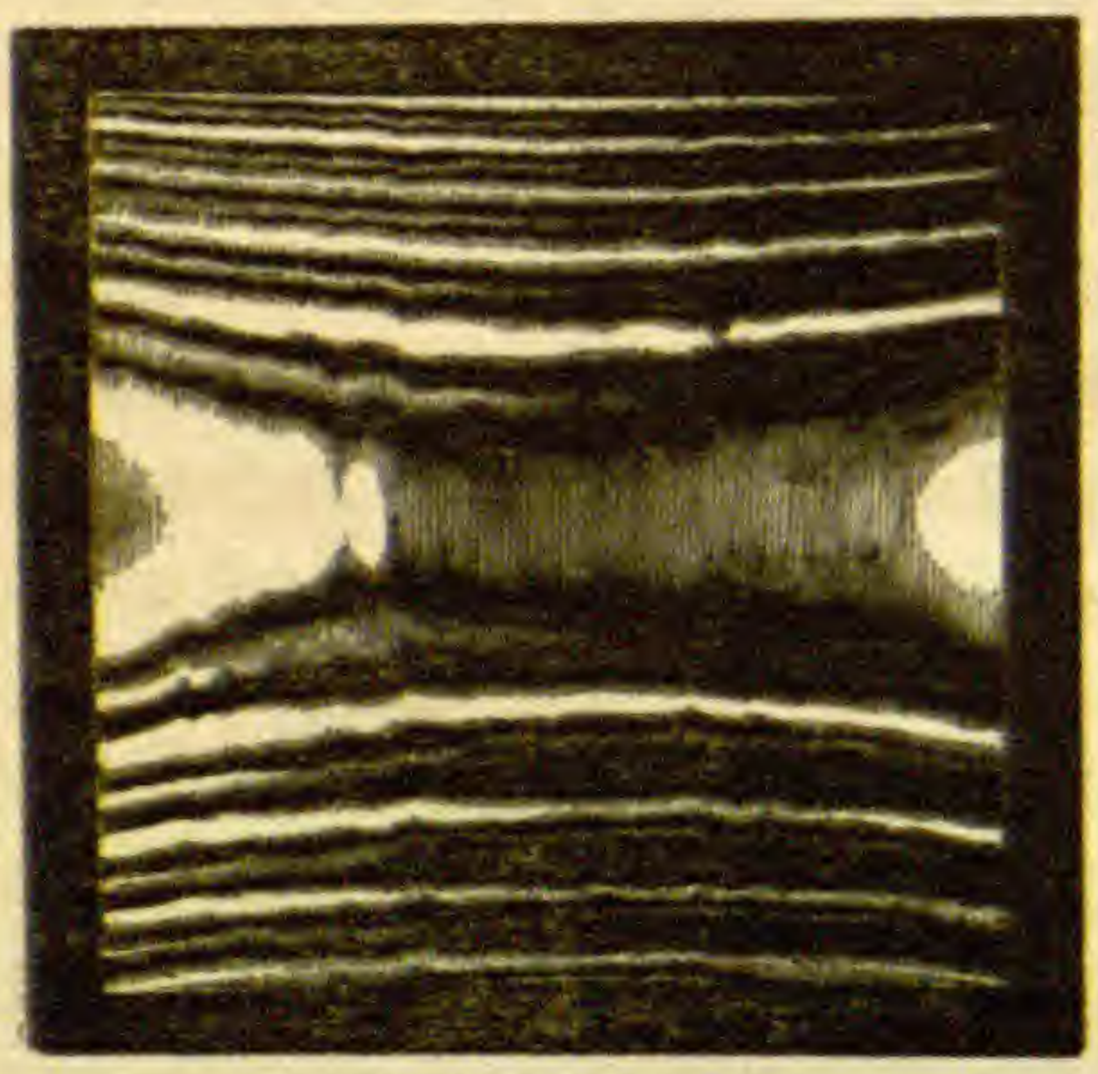
1.

2.



The same process was repeated with the 2000 line grating, excepting that it was found best to bring the image into the space between the direct beam and the first spectrum. The result is given in fig. 3; but unfortunately the minute irregularities so well shown in the photograph could not be reproduced in the wood cut. The first spectrum was impaired to some extent in these particular gratings, which were probably the third reproduction from the original. The splitting up of the bands could not be altogether prevented, and a difference in the actinic illumination at the two ends seemed unavoidable. Next micro-photographs were tried with the same illumination. The result with the Nobert grating did not possess sufficient interest to reproduce here. The original lines were shown. The Rutherfurd grating gave the result repre-

3.



sented in fig. 2. It was found, by comparing the depth of the serratures with the distance of two bands apart, that the periodic error amounted to one-fourth of the distance of the lines, or approximately the twenty-six thousandth of an inch.

To obtain the results given in figs. 1 and 3, the gratings were so disposed as to develop the greatest amount of error. The regularity of the former ruling is brought out by contrast; but the periodic error so plainly exhibited in figs. 1 and 2 is detected with great difficulty in Nobert's grating. This is perhaps due to the greater perfection of the screw employed; but to a large extent to the abundance of other errors which have no regularity of recurrence. However, three consecutive serratures were found, which were as sharp and apparently of the same character as those from the Rutherfurd grating.

If we place two whole copies of a grating, as would occur in respect to parallelism if one was bent on itself, making a folded edge parallel to the ruled lines, the resulting bands with the irregularities will be symmetrical about the central line of the ruling. The same position will also give symmetry about a line at right angles to the original lines, provided these lines are arcs of circles. This will be seen to be the case in fig. 3, and on counting the whole bands, above and below the point where the arcs are in effect parallel for a short distance, we find nine which represent a curvature of two and one-quarter lines The distance of the lines being known, we obtain the versed sine and from this the radius of curvature, which would be nine feet. A count under the microscope, with two gratings slightly overlapping, gave the curvature two and one-half lines. Rutherfurd's ruling gave no evidence of curvature in the lines when the latter were brought parallel. A curvature of the bands occurs in both cases, which indicates a gradual increase in the distance of the lines in going from one side of the ruling to the other. The inference from fig. 1 would be that this error may interfere with the definition of the spectrum as much as the periodic error due to the unequal thrust of the screw during each revolution; for, even granting that it appears doubled in actual amount in fig. 1, it seems to point to a displacement of one-quarter of a line. This curvature in the bands changes in direction when one plate is rotated to the other side of a parallel position of the lines. In fig. 3 we have both phases represented, as would be expected from the curvature of the lines and their consequent reverse crossing above and below the center. If we consider the greater curving of the bands in fig. 3 as compensated for by the width of the ruling, exceeding by one-third that represented in fig. 1, we have yet to take into account that the lines in the former ruling are three times as far apart, and hence it follows that the error due to an increment in the distance of the lines, in passing from one side of the grating to the other, is three times as great in the Nobert ruling. In each case the bands become straight in general direction, when one of each pair of gratings is rotated in the plane of the ruling 180 degrees.

Since writing the above, with the exception of a revision of the part relating to the photographing of the bands developed by superposition, I find a paper on this same subject by Lord Rayleigh, in Phil. Mag. for Feb., 1874. He has anticipated me on several points. He mentions, on superposing two photographed gratings, "the appearance of a system of parallel bars, whose direction bisects the external angle between the directions of the original lines." And suggests "the irregularity of the bars, due to imperfection in the ruling when parallelism is very closely approached, might be made useful as a test." He speaks also of irregular bands seen on individual photographs, and says that "the disappearance of the first spectrum is very unusual; but that it is common for bands to appear in the fourth and higher spectra."

His explanation of the cause originating these bands is different from mine. He says "when examined under the microscope, the opaque bar on the copy, which corresponds to the shadow of the groove (ruled line) of the original, is seen to be composite; being not unfrequently traversed along its length by several fine lines of transparency." And again, "in the process of copying, the groove of the original is widened into a bar, whose width depends on closeness of contact, an element which necessarily varies in different parts of the plate." In regard to the definition of a 3000 line grating by Nobert, he found that "in many cases the definition may be considerably improved by the use of a diaphragm so placed that only the central parts of the lines are operative." This observation appears to show that the Nobert ruling which he examined also had curved lines, and that this curvature injured definition.

He made copies "not perceptibly inferior to the original," and with copies of the 3000 line grating, with a high magnifying power, could "make out nearly, but not quite, all that is shown in Angström's map."

In a later paper in the March number of the same journal, he speaks of the inferiority of a 6000 line Nobert ruling to the one of 3000 lines by the same maker. It appears to me that an important point, which is apparently involved, should be borne in mind in the manufacture of gratings; since we have here an indication, that with 6000 lines to the inch, the maximum of defining power has been passed; for the reason that by increasing the number of lines, a point would be reached where the error in each turn of the screw, or the gain in twist of the same, would, in the width of the grating, equal the distance between two individual lines. Take, for instance, the 2000 line Nobert, the errors of which are pointed out in fig. 3. It would seem that to treble the number of lines would, in this case, pretty much destroy the effect of the grating; since the two errors remain independent of the number of lines.

It is not impossible that an application may be made of this principle of superposition, in studying the markings of diatoms. In carrying out this plan, besides superposing the shells themselves, it may be found useful to employ fine closely-ruled lines of known value; as in Nobert's microscopic test plate. Of course, it is to be expected that the higher powers would be dispensed with; a much lower power being used than would be required to separate the lines themselves.

New Haven, Conn., March, 1874.

ART. VI.—*On the Spectrum of the Zodiacal Light;* by ARTHUR W. WRIGHT.

THE observations, of which an account is here given, were made at various times during the year past, with a view to determine the nature of the zodiacal light, so far as it could be effected by a study of its spectrum, and as supplementary to the investigations upon its polarization published in this Journal in May last. Certain of the statements there made were based upon evidence derived from these observations, which it is the purpose of this article to set forth more fully.

As the object studied is one of the faintest among those upon which the spectroscope may be employed, some modifications were found necessary, both in the instrument and in the mode of observation. A Duboscq spectroscope with a single prism was employed, the telescope and collimator of which have a clear aperture of 2·4 centimeters. The magnifying power of the former is nine diameters. The instrument is thus well fitted to ensure the first requisite for the purpose in view, namely that of admitting a large beam of light, without too great dispersion. The positions of the spectral lines are ordinarily measured by means of an illuminated scale, which is of such a size that about 165 divisions are covered by the solar spectrum ordinarily visible. The image of the slit, when the latter is one millimeter wide, covers 8·2 divisions; when it covers a single scale division, the breadth of the slit is 0·122 millimeter. As it was found that an illumination sufficient to render the scale visible would greatly weaken, if not obliterate, the faint spectrum to be measured, it was necessary to employ some method of fixing scale-positions without recourse to the use of a light, and, in fact, it was found indispensable to exclude all artificial lights from the neighborhood of the instrument during the observations, and for some time previous, on account of their effect in blunting the sensibility of the retina.

The means employed in securing these desiderata constitute the principal modification introduced, and were as follows. The short joint connecting the eye-piece with the sliding tube of the telescope was removed, and another substituted for it, which was pierced on each side with a narrow opening. In this, and perpendicular to the axis of the tube, was firmly fixed a small rectangular frame, in which are two slides, or diaphragms, moved by fine screws passing through the ends of the frame. The latter is of sufficient length to allow each slide to traverse the entire field. The inner ends of the slides terminate in sharp, straight edges perpendicular to their line of motion. When in place, they are so adjusted as to be accurately in the focus of the eye-piece. They are thus seen projected upon the scale, and sharply defined, when the latter is illuminated. By turning the tube the terminal edges are made accurately parallel with the lines of the spectrum. As these are somewhat curved in the passage of the rays through the prism, the scale was placed in the middle of the field, and the measurements made from the middle point of the slides, or, in some instances, the ends of the slit were covered with bits of paper, reducing the spectrum to such a breadth that the curvature was small enough to be neglected.

Before making an observation, the instrument was carefully adjusted, and the scale so placed that the division-mark 50 coincided with the more refrangible edge of the sodium line, inasmuch as only one of the sides of the slit is movable, and on opening it the image widens toward the red end only. For the same reason, in fixing the limits of the spectrum, the breadth of the slit must be added to the scale-number on the less refrangible side, in order to reduce the dimensions of the spectrum to what they would be with a linear slit. The situation of any point in the spectrum, however faint, could be determined with a good degree of accuracy by moving the diaphragms up to it, and then, on illuminating the scale, reading its position by the scale-numbers. In the case of well-defined lines, like that of the auroral spectrum, the error is not greater than half a division of the scale, and, with carefully repeated determinations, may be made considerably less than this.

In making the observations upon the zodiacal light, the same method was pursued as in the investigation upon its polarization, previously mentioned. The room was not lighted, except by the diffuse illumination from the sky, and care was taken to keep the eye secluded from all bright lights for a considerable time before each observation. The central portion of the spectroscope was wrapped round with black cloth to prevent the entrance of any stray light. During the progress of the work attention was specially directed to three points, which it seemed important to determine. These were, first, to find the limits of the continuous spectrum which is always seen, and to obtain data for comparison with ordinary sunlight and with twilight; second, to ascertain whether or not the bright line, which is sometimes visible, is constantly present and belongs to the zodiacal spectrum; third, to discover whether there is any connection between the zodiacal light and the polar aurora. A record was kept of the circumstances of the different observations, and of the state of the sky at the time they were made.

With regard to the first of these questions, repeated examination showed, as has been found by other observers, that the proper spectrum of the light is a continuous one. A bright line is, indeed, sometimes seen, but, for reasons given below, it cannot be regarded as belonging to the zodiacal light. In its general appearance the spectrum is not different from that of faint daylight or of twilight. It extends from somewhat below D to near G. In order to fix its dimensions more definitely, an extended series of observations was made, and the limits in both directions were determined with great care. The slides were usually moved so as to cut off the visible part of the spectrum by degrees, until it could no longer be seen, by any effort of vision, and the positions of the limits of visibility were afterward read off on the scale. These varied somewhat with the state of the atmosphere, but the determinations made on the best nights agreed very well with each other. For the purpose of comparison, those were selected which were found on a few of the clearest nights. The width of the slit, in all these cases, was ten divisions of the scale, or 1·22$^{mm.}$ Adding ten to the readings at the lower or red end, as a correction for the breadth of the slit, the numbers are as follows:

Lower limit, 54, 52·5, 57·7, 57·5, 50; mean, 54·35.
Upper limit, 123·5, 119·5, 120, 124, 120; mean, 121·4.

Since the intensity near the limits declines by almost insensible gradations, and the circumstances of the observations at different times, as atmospheric conditions, sensibility of the eye, and the like, are subject to slight variations, too great weight must not be attached to the mean values. They do not indicate the absolute bounds of the spectrum, but simply the points beyond which, in general, the light is too feeble to produce a definite and constant visual impression. They may serve for comparison with those of other spectra measured in the same way. It could plainly be seen, on slowly moving the slides toward the end of the spectrum, and just before it ceased to be visible, that the light extended a considerable distance beyond them, as much perhaps as ten or fifteen divisions of the scale. The position of maximum brightness was merely estimated, and was not perceptibly different from that of the twilight spectrum. The extent and general form of the spectrum are shown in the accompanying plate (Plate III), where it is marked I. The appearance of the spectrum, as seen when the slit had a breadth of only two scale-divisions, is represented in No. V, but the limits there indicated are less certain than those obtained in the other cases.

A number of measurements were made, in the same manner, of the spectrum obtained with light from the sky. It was admitted through a circular opening, 1·75$^{mm.}$ in diameter, in the shutter of a darkened room, and fell upon a piece of white card-board some eight feet distant, a few inches from which was placed the slit of the spectroscope. After a somewhat prolonged stay in the darkness, the spectrum was seen with sufficient distinctness to allow of good determinations. Its brightness was very nearly the same as that of the zodiacal light, apparently a little greater. The breadth of the slit was as before ten divisions. The mean of five experiments gave, for the upper limit, the scale-number 132·5; for the lower, as observed, 45·6, or, corrected for the breadth of the slit, 55·6. The lower limit corresponds very nearly with that of the zodiacal spectrum, but the upper one is slightly higher. The difference is undoubtedly due to the fact that the light reflected from the sky contains a relatively larger proportion of the blue rays, which would somewhat increase the comparative intensity at that end of the spectrum. It is what we should expect from the fact that the zodiacal light traverses the atmosphere, while the other is reflected from it. The discrepancy is very slight, however, and would hardly be detected in the ordinary method of observing the spectra. The maximal point was not easy to fix, on account of the very gradual variation of intensity, but was between 70 and 80, apparently somewhat nearer the former. It is placed at 74 on the plate, spectrum IV.

Observations on light from the moon, and upon twilight, showed that their spectra correspond even more closely with that of the zodiacal light. In the case of the former, the lunar rays were received upon a white unglazed card placed a few inches in front of the slit. The latter being narrowed to two scale-divisions, the limits were found to be 47 and 136·6, the point of greatest intensity lying between 70 and 80, and evidently nearer the former. Similar results were obtained with twilight, the lower and upper limits, obtained with a slit covering two divisions, being 50 and 134·6, respectively. The lower limits in each case were corrected by adding the breadth of the slit.

The accompanying plate exhibits the general character of the zodiacal spectrum, and its relation to those with which comparison was made. The corresponding portion of the solar spectrum, with a few of the principal Fraunhofer lines, is represented at the bottom, as a standard of reference, and to show the position and value of the divisions of the scale used in the instrument, the numbers of which are placed above the lines. The band between 50 and 60, marked δ, is atmospheric, and its place is determined from Prof. Ångström's chart, and from Dr. Janssen's diagram of the telluric lines. It is the band seen on several occasions when a very narrow slit was used, as mentioned below. The line marked α is the principal line of the auroral spectrum, sometimes referred to the zodiacal spectrum, as stated in a previous paragraph. The wave-lengths, corresponding to points in the spectroscopic scale, for intervals of five divisions, are given in the following table.

Scale No.	Wave-length.	Scale No.	Wave-length.	Scale No.	Wave-length.
40	6255	75	5216	110	4606
45	6064	80	5111	115	4538
50	5889	85	5012	120	4472
55	5726	90	4919	125	4409
60	5583	95	4831	130	4352
65	5451	100	4751	135	4298
70	5329	105	4677	140	4246

The curves represent the relative extent and intensity of the observed spectra, in their different parts, by the length of the corresponding ordinates, in the usual way, the shaded portion being that which was actually measured, and the unshaded prolongations roughly indicating the apparent extent, as estimated before the light had fully disappeared. No. I. represents the spectrum of the zodiacal light; No. II. that of twilight; No. III. that of moonlight; No. IV. that of light from the sky; No. V. that of the zodiacal light with narrow slit No. VI. is copied from Prof. Piazzi-Smyth's chart,* and represents the extent of the zodiacal spectrum, as found in his observations, with a broad slit, and with a reference spectrum in the field.

In attempting to fix the brightest points of the spectra it was observed, that when diffused sunlight is directly observed with the spectroscope, and simply weakened by narrowing the slit, the maximum is near 50 of the scale. The apparent ex-

* Monthly Notices of the Royal Ast. Soc., June, 1872, p. 277.

tent of the spectrum is not much lessened, and the different colors are still perfectly distinct. In the case previously mentioned, where light from the sky was received into a darkened room, and rendered extremely feeble, the result was different. The distinction of colors was nearly or quite impossible, and the extent of the spectrum much diminished. The red rays had become almost too faint to affect the eye sensibly, and the apparent point of greatest intensity had perceptibly moved upward, an effect, which is, of course, chiefly subjective. Its position was found in various ways, as by moving the slides over the faint spectrum till their edges were most distinctly seen, or by observing the highest point of the curve which is formed by the boundary of the spectrum, when produced with a wedge-shaped slit, or when a thin hollow wedge of glass filled with ink was placed before the slit. Repeated determinations of its place showed that it was between 70 and 80, but nearer the former, and in the plate it is placed at 74. This is perhaps a little too high, but it is the best approximation the difficulty of the determination allowed. Quite the same result was found with moonlight and twilight, and the maximum was sensibly at the same point as with light from the sky. In some evenings during the early portion of the investigation, observations were begun before the close of twilight, and continued upon the zodiacal light until it was certain that the former had entirely ceased. If any considerable difference in the maxima of the two sources of light exists, it could not have been overlooked, but none was ever perceptible.

With respect to the second point of the inquiry, evidence was obtained which leaves little doubt that the bright line which has been occasionally seen, does not belong to the spectrum of the zodiacal light. If the negative evidence of scores of observations in which it could not be detected is insufficient, there are facts which satisfactorily explain its presence on the few occasions when it has appeared. Both Prof. Smyth * and M. Liais,† who have made careful observations, deny that it belongs to the zodiacal spectrum, and the former gives a conclusive explanation of its supposed existence there. It is sufficient to recall the fact that the light which gives the auroral line is essentially monochromatic, and would be visible in the spectroscope even when with the naked eye it could not be detected in the general illumination of the sky, as it would not be weakened by dispersion like the latter, and would hence be relatively intensified. In the course of the many observations made by the writer, the line was visible on three evenings, but on each of these occasions there was an aurora, which on one of the evenings had a considerable intensity. The

* Monthly Notices of Royal Ast. Soc., loc. cit.
† Comptes Rendus, Tom. 74, 1872, p. 262.

whole sky was more or less luminous, either by direct auroral action, or by reflection of the light thus produced. The bright line could be plainly seen wherever the instrument was pointed, and its position was found to be exactly the same, whether the light was derived from the zodiacal region, or directly from the aurora. On the other hand, the bright line was never seen when there was no aurora.

It remains to consider the question whether there is a connection of any kind between the zodiacal light and the polar aurora. The considerations mentioned above do not absolutely exclude the possibility that, simultaneously with the aurora, and with a certain dependence upon it, some luminific process may be going on in space, which would cause the bright line to appear in the zodiacal spectrum, however improbable such a supposition may be. But if there were no better reason, the general invariability of the zodiacal light from night to night, and the constancy of its presence throughout all the months of the year, sufficiently indicate for it a different cause. Moreover, on at least two of the evenings when only a continuous spectrum was seen, there was an aurora, moderately bright, though not extending to any great distance from the horizon. If the two were in any way related, we should expect some variation in the zodiacal light coincident with the appearance of remarkable auroral displays, but it does not appear that any such thing has ever been observed. There seems to be no evidence that they are not entirely independent phenomena.

One or two additional observations may be mentioned here as of interest. On several occasions when the slit of the spectroscope had a breadth of only two or three divisions, the spectrum at the first glance appeared to end suddenly at about 54 of the scale. Further examination showed light beyond, and that the apparent abruptness of its termination was caused by a dark band of about the width of the slit, which was found to occupy, as nearly as could be estimated, the exact position of the band marked δ in the plate. It is doubtless identical with it, and caused by atmospheric absorption.

The spectrum on the most favorable nights was still visible when the slit was narrowed to one division of the scale, and on one occasion the opening was reduced to 0·6 of a division. that is, to an actual breadth of $0{\cdot}073^{mm.}$ before the light became imperceptible. As the principal Fraunhofer lines are distinctly seen with sunlight when the slit is even wider than one division, it appears very probable that these lines may yet be detected in the zodiacal spectrum. Especially is this to be hoped for, if observations should be made, in the manner described, in lower latitudes, where the intensity of the light is known to be much greater.

The statements made in the previous article, in reference to the zodiacal spectrum, are satisfactorily established by the results of these observations, from which we may draw the following conclusions.

1. The spectrum of the zodiacal light is continuous and is sensibly the same as that of faint sunlight or twilight.

2. No bright line or band can be recognized as belonging to this spectrum.

3. There is no evidence of any connection between the zodiacal light and the polar aurora.

The deduction, drawn from the fact of its polarization, that the zodiacal light is derived from the sun, and is reflected from solid matter, is thus strengthened and confirmed by the identity of its spectrum with that of solar light. A discussion of the distribution of the reflecting matter in space is reserved for another article.

Yale College, June 5, 1874.

ART. VII.—*On the Age of the Copper-bearing Rocks of Lake Superior; and on the Westward Continuation of the Lake Superior Synclinal;* by ROLAND IRVING.

DURING the summer and fall of 1873, I was in charge of a geological exploration of northern Wisconsin, including the three counties which border on Lake Superior, on behalf of the State Geological Survey. This is a region but little known, it being for the most part unbroken forest, and without inhabitants except in three or four small towns immediately on the lake shore, the whole coast line, exclusive of islands, having a length not far from one hundred and twenty miles. It has been very little examined geologically. Portions were visited and described by members of the various corps under Dr. D. D. Owen, 1848–50; the easternmost of the three counties, that of Ashland, having received the most attention. Its general geological and topographical features are very briefly described by Colonel Chas. Whittlesey, in the final report of Dr. Owen. In 1860, Colonel Whittlesey again visited Ashland County on behalf of the Geological Survey of Wisconsin, then organized under Mr. James Hall. The results of his investigations were never published by the State, although some of them have appeared in pamphlet form and in transactions of scientific societies. Since that time no further examinations of the region have been made.

I. *Age of the Copper-bearing Series.*

Recent observations by Messrs. Brooks and Pumpelly * in the Upper Peninsula of Michigan, and by members of the Canada Geological Survey on the Canada side of Lake Superior, go far toward proving a dissimilarity in age between the "Copper-bearing Series" of those regions and the accompanying sandstones known to be Lower Silurian, two groups always before supposed to be identical in age. My own investigations in northern Wisconsin, and those of my assistant, Mr. E. T. Sweet, have developed facts which are very interesting in this connection, and it would seem not unlikely that in this region is to be found the key to the whole problem.

Since these facts are not to be published officially for some time to come, and since there are still investigations going on in the adjoining States and in Canada, it has been thought proper to make public the most important of them. It is not proposed to attempt, in this paper, any general discussion of the interesting question of the age of the copper-bearing rocks, but merely to give the facts that have been obtained, and to draw the conclusions toward which they seem to point.

Four grand groups of rock are found in northern Wisconsin: *Laurentian* granites, gneiss and schists; *Huronian* slates, schists, diorites, etc.; the traps, sandstones, conglomerates, melaphyres and slates of the *Copper-bearing Series;* and *Lower Silurian sandstones.* Besides these there are immensely thick deposits of Quaternary clays, and of Bowlder Drift.

1. *The Laurentian Rocks* (underlying the area marked I on the map), are always farthest removed frem the lake, never being nearer to it than eighteen miles, in Wisconsin, and generally being much more distant than this. So far as yet observed, these Laurentian rocks are granitic, gneissoid and syenitic in character, although without doubt schistose beds occur, since these are found just east of the eastern line of the district within the area of the Upper Peninsula of Michigan.† In northern Wisconsin the rocks of this group are everywhere overlaid by enormous accumulations of Drift material, showing through this covering in but very few places. Amongst these Drift heaps, and in the swamps which everywhere cover the surface of the country between them, the streams flowing northward into Lake Superior, and those flowing southward into the Mississippi, interlock in an intricate manner, the former—in a distance often not more than thirty miles from their sources to the lake—falling as much as 600 or 700 feet. From this it can be seen that their currents must be broken by numerous chutes and falls, which is the case; many of the single falls reaching a

* This Journal, June, 1872.

† T. B. Brooks, "Geology of Upper Peninsula," 1873.

height of 60 or 70 feet, whilst in one case, that of Black River, in Douglass County, the height of a single fall is 160 feet.

2. *The Huronian Rocks* (II on the map and section), which directly overlie the Laurentian—and unconformably, as shown by Brooks and Pumpelly, from observations made by them, in Michigan, just east of the Wisconsin line—constitute in Ashland County a continuous narrow belt, whose central portion is the well known "Penokie Iron Range," and whose width never exceeds two miles, being usually much less than this. This belt extends without break into Michigan almost as far as Lake Gogebic, where the rocks are lost sight of, being covered by accumulations of drift, and finally by horizontal Silurian rocks. They do not re-appear until, one hundred miles farther east, the Marquette iron region is reached. Here they are found again with some important changes, and covering a much wider extent of territory. Toward its western extremity the Huronian belt appears to come to an abrupt ending, the underlying Laurentian and overlying Copper-bearing Series coming together. Farther west, however, just on the west side of Ashland County, are two isolated belts of these rocks, in every way similar to the main area, each having in the same manner its central ridge. As to the continuation still farther westward of the Huronian, nothing whatever is known, as is indicated on the map by an interrogation point. The rocks of this group in northern Wisconsin are siliceous schists, talco-siliceous schists, black slates of undetermined composition, white quartz rocks, quartzites, magnetic and specular schists of various kinds, magnetic and specular iron ores, diorites, diorite slates and diorite schists. The beds of one portion of the group, about five hundred feet in actual thickness and continuous for over thirty miles, are impregnated throughout with the specular and magnetic oxides of iron in proportions varying from one or two per cent up to sixty and eighty per cent of the whole. The entire series has a nearly uniform dip west of north, generally at a very high angle. The thickness never varies far from 4,000 feet, a figure obtained by actual measurement.

3. *The Copper-bearing Rocks* (marked III *a*, *b*, *c*, *d*, *e*, *f*, on the accompanying map and sections).—In Ashland County, next north of and immediately overlying the Huronian, come the enormously thick beds of the Copper-bearing Series, which have—including all subdivisions—an apparent thickness of as much as four miles, and even more than this on the extreme east. These rocks form a broad belt in Ashland County, which is widest at its eastern end, narrowing toward the west, and at the same time receding from the lake shore. The most westerly known portion of the belt is at Long Lake, in the southern end

of Bayfield County. Eastward of the Montreal River, in Michigan, the belt separates into two: the main area, which continues without interruption to the end of Keweenaw Point; and the "South Mineral Range," which lies to the southward of the main belt, and follows the Huronian rocks eastward. The latter belt runs out in its eastern extension. Between it and the main area lie horizontal Silurian sandstones.

The southernmost (III *a*) portion of the group in Ashland County covers the broadest area. It includes rocks always highly crystalline, generally very coarsely so, and of such variation in mineral constituents, texture, etc., that I have not yet attempted to name all the varieties, nor even a considerable portion of them. Nearly all of them can, however, be included in two or three general kinds; labradorite, orthoclase, feldspar, hornblende, and some varieties of pyroxene, seeming to be the chief ingredients. The indications of bedding in this portion of the series are seldom to be seen, but where they are apparent, are marked, and point toward entire conformability with the underlying Huronian.

Next north of, and immediately overlying, the rocks just described are the beds of that portion of the group which I have designated III *b*, *c*, *d*, on the map. These are a series of alternating beds of trap, mostly melaphyres, but of very varying characters, being both amygdaloidal and compact, black, dark green, brownish red, brick-red and light pink in color, and always crypto-crystalline; and in the upper portions, beds of a very remarkable conglomerate (III *c*), together with a great thickness of sandstone and shale (III *d*). These sandstone and conglomerate beds do not altogether overlie the trappean beds, but are, near the junction, directly and unmistakably interstratified with them. There is a sort of gradation from the trap to the sandstones, the layers of the latter nearest the trap being made up of coarse trap-sand, whilst on receding from it they become more and more like the horizontal aluminous red sandstones of the Apostle Islands. The entire series, traps, conglomerates, sandstone and shales, have a very high dip to the northward, which is never less than 85°, and often reaches the perpendicular.

The sandstone, conglomerate, and shale beds have not as yet been seen west of Bad River, in Ashland County, but the traps can be traced uninterruptedly westward as far as Long Lake in Bayfield County. The traps of this group are also largely developed in Douglas County, and there are sandstone beds believed to belong to the Copper-bearing Series in another part of Ashland County, as will be mentioned farther on.

4. *Lower Silurian Sandstones* (IV on the map and section).—North of the north line of that portion of the Copper-bearing

Group just described—which line is also the northerly edge of the mountainous portion of Ashland County—is a country having the general character of a plain, though much worn into gullies near the main water-courses. This plain extends to the shores of Lake Superior, being widest on the west, and narrowing to a point on the east at the mouth of the Montreal River, where the elevated country reaches the water's edge. This plain is everywhere underlaid by a very thick mass—200 feet or more—of a peculiar red marly clay, which carries a few rounded boulders and pebbles of erratic rocks, and which appears to be overlaid by the regular boulder drift wherever the two come into contact. The rivers and their branches cut deep valleys into these clays, often showing banks as much as 100 feet high; but only in four places have any rocks in place been seen within this area in Ashland County. At A, on a small stream called Silver Creek, there is an exposure of horizontal red sandstone and shale, having the usual appearance of the sandstone and shale of the Apostle Islands. This locality of horizontal sandstone is only three-quarters of a mile north of the trap exposures on the same stream. The dipping sandstones and shales do not show in this vicinity; but four miles east they are apparent in great force, having a dip of ninety degrees, and a thickness of hundreds of feet in sight. No other exposure of horizontal sandstone has yet been seen on the mainland of Ashland County, either on its coast or inland. On the coast of Bayfield County, however, along its entire length, exposures are numerous, as also on the Apostle Islands, of which the horizontal sandstones are always the substructure, and where the exposures are often bold and precipitous. These sandstones are everywhere the same, alternating heavy-bedded and thin bedded, of a prevailing red or reddish brown color, with here and there lighter-colored portions in patches; *but always much lighter in color than the tilted sandstones associated with the traps.*

That these horizontal sandstones are the same as those of the Pictured Rocks, the eastern side of Keweenaw Point, and the Silurian basin which extends inland from Keweenaw Bay nearly as far west as the Montreal River, is not absolutely proven; the two areas of horizontal sandstones of east and west Lake Superior being nowhere connected, but always separated by rocks of the Copper-bearing Group.* That they are, however, the same, is extremely probably for the following reasons: (1) Because showing precisely the same relations to the Copper-bearing Series. (2) Because, on tracing them westward and southward to the St. Croix River in western Wis-

* The distance between the western end of the *eastern area*, near the Montreal River, and the easternmost outcrop of the *western*, is about thirty-six miles.

consin, *they appear to dip* underneath* the lighter-colored Lower Silurian sandstones of the Mississippi Valley, of which they are probably merely the downward continuation; thus showing the same relation to newer rocks as do the sandstones of eastern Lake Superior. (3) Similar lithological characters, an admissible test, I suppose, for regions so close to one another, and for entirely undisturbed rocks.

At the two points, marked III *e*, in Ashland County, are exposures of dark-red sandstone, and sandy shale *dipping southward*, as shown on the section. At one of these points, the easternmost, there is exposed a thickness of at least 2,000 feet of sandsone, dipping southward, at an angle of 38°. This great thickness is actual and not due to faulting. The impression that these sandstones, exposed at points more than ten miles apart, are portions of the northerly edge of a synclinal, of which the vertical sandstones to the south form the southerly, is almost irresistible. This opinion is strengthened by (1) the difficulty of accounting for so great a thickness with so uniform a disturbance, at points more than ten miles apart, by attributing this disturbance to a mere dislocation of the Silurian sandstones, which occur horizontal only a few miles off; (2) the occurrence of horizontal sandstones within the jaws of the supposed synclinal; (3) the very great thickness of these beds, which allies them closely to the vertical beds on the southward, whose total thickness cannot be less than 10,000 feet; (4) the occurrence of trap at the point marked III *e*, a little to the north of the line of the southward dipping sandstones; and (5) the probability, alluded to farther on, that there is somewhere in this region of Wisconsin a synclinal, representing the westward continuation of that existing between Keweenaw Point and Isle Royale.

Following the shore of Lake Superior westward from the Apostle Islands, the horizontal sandstones can be traced without break until Douglas County is reached, when they disappear from the coast—which is here altogether of red clay marls—but appear constantly in the beds of the many streams flowing northward into the lake. On ascending these streams to the southward, the red sandstones can be traced to their junction with the trappean beds of the Copper-bearing Series, which here dip, wherever the dip is observable, at a comparatively low angle *to the south*. The sandstone beds continue horizontal, or with a very slight dip northward, to within a short distance of the trap, when, in most instances, they show a remarkable change. In one case, however, they continue to within twenty feet of the trap without change, the exact junction being concealed by an eroded gulley. At Black River, for three hundred feet from the trap, the sandstone is broken up,

* D. D. Owen, Report on the Geology of Iowa, Wisconsin, and Minnesota.

in every conceivable manner, the misplaced layers dipping in all directions, and in its immediate vicinity making a sort of brecciated mass of fragments of trap and sandstone. This appearance is presented along the perpendicular side of a gorge, whose depth is more than a hundred feet, and is said by my assistant Mr. Sweet, who made all the observations in Douglas County, to be caused, beyond question, by no mere surface displacement, but by the general disturbed condition of the sandstone. On passing down the gorge to the westward, the sandstone layers become gradually less irregular, taking a prevailing northerly dip, until, 300 feet from the trap, they grade imperceptibly into the ordinary horizontal beds. Passing up the gorge to where the river falls over the trap, with a fall in all over 160 feet in height, the beds of melaphyre are found dipping *southward* at a low angle. On all of the other streams examined, the horizontal undisturbed layers of sandstone were found coming much closer to the trap, but in all except the one already cited, showing the same peculiar disturbance, although on a smaller scale.

The trappean beds of the Copper-bearing Group in Douglas County have been examined over the area marked III *f* on the map, where they compose a well marked ridge known locally as the "Copper Range." This ridge, with its constituent traps, can be traced some distance into Bayfield County on the east, but exactly how far is not yet known. These trappean beds carry here *no intercalated beds of sandstone and conglomerate*, and have in appearance, texture, &c., more similarity to the rocks of III *a* in Ashland County than to the more northerly beds of III *b*. They appear, on the whole, to be rather more similar to the traps of Isle Royale than to those of Keweenaw Point, and other places on the southerly shore of the lake. South of the "Copper Range," the county has not been explored geologically except on the line of the Brulé River. It is described as swampy and comparatively level, without many rock-exposures. The line of the Brulé River was explored by members of Dr. D. D. Owen's corps, and trappean beds found much farther up the stream (at a point marked III *x* on the map). Still farther south the horizontal red sandstones are again found (at points marked IV), and can be traced southward until they disappear beneath the lighter-colored layers of the Mississippi Valley.

Having stated, as briefly as possible, the main facts thus far known of the geology of northern Wisconsin, it becomes necessary, in order to show their bearing on the question of the age of the Copper-bearing Series, to draw attention to the following points. The great belt of rocks of this series, which extends southwestward from Keweenaw Point to Long Lake in

Wisconsin, forming the lake shore as far as Montreal River and then receding therefrom, includes alternating beds of melaphyres, basalts, porphyries, sandstones and conglomerates, which have always a northerly dip, increasing gradually in amount toward the west, from a low angle on Keweenaw Point to a vertical position on Montreal and Bad Rivers. In Ashland County the most northerly portions are exclusively sandstones, the most southerly exclusively traps, the whole group having a thickness measured by miles, the vertical sandstones alone in the uppermost portion having a thickness but little short of 10,000 feet. Inasmuch as the traps, sandstones and conglomerates are directly and conformably interstratified, and tilted together at various angles, it follows that the first named, whether ejected in a molten condition or not, must have been originally in a horizontal position; that the uppermost sandstones must have been formed after the last outflow of trap; and hence, that the present tilted position of the sandstones was not caused by trappean ejections, but that the traps themselves were tilted by the same force that tilted the sandstones and at the same time. In other words, *this whole group has undergone an ordinary regional disturbance, and the finding of horizontal beds in close proximity is just as much proof that these latter are altogether newer than the tilted beds, as it would be in any other case of unconformability.*

The conclusions, then, that I would draw are these:

1st. The beds of the Copper-bearing Series, and those of the Huronian Series, were once spread out horizontally over one another, including the whole series of tilted sandstones of the Montreal River; that they were disturbed by the same force, and received their present tilted positions at the same time, as evinced by the conformability of the two series throughout.

2d. That the horizontal sandstones of the Apostle Islands and the western end of Lake Superior were laid down subsequently to this tilting, and also to an immense amount of erosion, and that the sandstones of eastern Lake Superior were formed at the same time.*

As evinced by (1) the occurrence of horizontal sandstones in immediate proximity of tilted sandstone and traps, in Ashland County; (2) the occurrence of the same on the Apostle Islands, within but a few miles of the tilted beds of the Montreal; (3) the direct actual contact of the horizontal sandstones in Douglas County with the melaphyres of the Copper-bearing Series, here dipping southward, and (5) various other proofs cited by Brooks and Pumpelly in the article previously alluded to.

3d. That hence the Copper-bearing Series should rather be classed with the Archæan than with the Silurian.

*As already shown by Brooks and Pumpelly.

The only difficulty in the way of these conclusions lies in the remarkable phenomena presented at those points in Douglas County where the horizontal sandstones are found joining the traps. The first explanation that offers itself is naturally that these remarkable disturbances were caused by the protrusion of the trappean beds through the already formed sandstones. In answer to this it may be said, (1) that it is exceedingly doubtful whether the protrusion of molten matter through horizontal layers of sandstone would produce any such effect,—at least I cannot conceive how it could; (2) that this remarkable disturbance is unaccompanied by any hardening of the rock, appearance of baking, or other evidence of great heat; (3) that if these trappean beds are of the same Copper-bearing Series as those in Ashland County, then the proof of unconformability there found is conclusive of the greater age of the traps as compared with the horizontal sandstones; and (4) that hence we must find some other explanation of these phenomena. The only one that appears at all acceptable to me is that the traps, being deep-seated and non-continuous with the comparatively superficial sandstones, would, if impelled to move by any force, tend to move, to some extent, independently of the sandstones, and would, by an exceedingly slight motion northward against the latter, produce precisely the effect observed.

II. *Westward continuation of the Lake Superior Synclinal.*

Foster and Whitney long since pointed out the fact that the Keweenaw Point beds and those of Isle Royale formed the opposite side of a synclinal in which the lake lies, not improbably without other filling than its waters. On a diagram showing the "Systems of dykes," accompanying Agassiz's "Lake Superior," issued in 1850, I find a dotted line with a query made along the center of the peninsula of La Pointe, or Bayfield, indicating that possibly it has a trappean core and owes its projection into the lake to that core. I think there is reason to believe that this is true, and that the core of that peninsula, with the Douglas County traps, forms the westward continuation of the Isle Royale or northerly side of the synclinal just alluded to; that hence this synclinal trough, in its westward extension, does not hold the lake, but passes entirely on to its southerly shore, and that it is not improbable that the line of southward-dipping sandstones, in Ashland County, marks the southerly edge of the northern side of this synclinal.

However, inasmuch as there is no direct proof that the trough between Keweenaw Point and Isle Royale is a simple synclinal without subordinate folds, these southward-dipping sandstones may represent one of these subordinate bends rather than the northerly edge of the trough, and this seems the most likely supposition.

The arguments in favor of these views are as follows:

1st. The belt of rocks forming the southerly side of this synclinal is known to continue far westward into Wisconsin. It is therefore to be suspected that the northerly side has a similar extent.

2nd. If this northerly side does continue westward, it must be entirely under the waters of the lake, or on the southern shore, since on the northern shore the rocks are altogether different.

3d. This westerly continuation of the northerly side would preserve some sort of parallelism to the southerly side.

4th. The peninsula of Bayfield, Isle Royale, and Keweenaw Point have the same general trend.

5th. The copper-bearing melaphyres, &c., of Douglas and Bayfield Counties dip, wherever the dip is observable, to the southward.

6th. The northerly dip of the beds of the Keweenaw Point belt gradually increases in degree as the rocks are traced westward from Keweenaw Point, where it is low, until the Wisconsin line is reached, where it is vertical. This being the case, it would be expected that the two sides of the synclinal would approach one another in their western extension, which is in accordance with the facts, whether the southward-dipping sandstones be regarded as marking the northerly edge or not.

7th. If the supposition be true, then, on the northerly side where all the *observed* dips are from 25° to 38°, the area of country immediately underlaid by the outcropping edges would be much broader than on the southerly side where the dip is vertical. That this is the case is very evident from facts already stated. If the synclinal be regarded as simple, and the southward dipping sandstones of Ashland County be taken as the southerly edge of its northern side, then an average dip of as little as 25° to 30° would be required to spread the whole thickness from the line of the southward-dipping standstones as far as the middle of the peninsula of Bayfield. If, however, the synclinal be regarded as having a subordinate fold, then the dips may be of much greater amount. For many reasons, not necessary to enumerate here, I regard the latter of these as most probably the case.*

8th. The strange absence of intercalated beds of sandstone and conglomerate with the traps of Douglas County is thus explained, since the most northerly portions in that county correspond to the most southerly of Ashland County. Hence the dipping sandstones would be looked for in Douglas County in the unexplored region south of the Copper Range.

* The low dips—25°—observed in some places, would go to confirm the first of these views.

9th. The greater similarity of the Douglas County traps to those of Isle Royale, than to those of Ashland County, goes to confirm my views.

The dotted lines, then, on the accompanying section give my explanation of the structure of this region, in accordance with the foregoing facts and views.

It should be said that the trappean beds of Douglas County have been traced for some miles into the Bayfield peninsula, and can without doubt be traced still farther, but that they can certainly never be actually seen toward its extremity, since the covering of drift there is enormous, and since at some point they must pass beneath the horizontal sandstones of the Apostle Islands, and of the coast of the peninsula. It should also be said that much of the area south of the Copper Range in Douglas County, between the explored region and the line A B, which marks the supposed southerly edge of the northern side of the great synclinal, or else the line of the axis of the subordinate fold—which area I have supposed to be immediately underlaid by beds of the Copper-bearing Series—may be found to show horizontal sandstones, since there is no telling how far these may have been laid down on the upturned beds of the former mentioned series.

One more point suggests itself as affording strong proof of the greater age of the Copper-bearing Series as compared with the horizontal sandstones. I refer to the ferruginous and aluminous character of the Lower Silurian sandstones of Lake Superior, as compared with the light-colored, quartzose sandstones of the Mississippi Valley, which are either approximately or identically of the same age. If the Copper-bearing traps are older than the horizontal red sandstones, then the latter derived their material from the wear of these traps, which are remarkable for their low amount of silica, considerable percentage of alumina (feldspar) and prevailing content of iron (as magnetic iron). Many of these sandstones are almost a trap sand. The beds of the Mississippi Valley, however, derived their material from the wear of the Huronian quartzites, and Laurentian quartzose granites.

University of Wisconsin, March 21st, 1874.

ART. VIII.—*On the Parallelism of Coal-Seams;* by E. B. ANDREWS.

IN the April number of this Journal, Dr. Newberry calls in question my views of the general parallelism of coal-seams, derived from the study of our Ohio Coal-measures, and thinks them not only "untrue" but "calculated to do positive and practical harm." The only point of difference between us, of any interest to science, is whether the ancient shore lines, with their coal-marshes, subsided in an even and uniform way, or very unevenly, so as to bring each marsh to greatly varying depths below the water level. I am led to hold the former opinion, while he strenuously maintains the latter. In my district, and in the portions of his district—i. e., the one under his special supervision—that I have examined, and also in the bordering States of West Virginia and Kentucky, I find a general parallelism of the seams of coal, implying an even and uniform subsidence. This makes system possible and stratigraphy useful in our Coal-measures. If, on the other hand, the subsidence were uneven and irregular, no coal-seam can have its proper and exact horizon, and all things are in confusion. If, for example—and I quote one of the cases given by Dr. Newberry in his article—coals No. 8 and No. 9 are, at one place, 150 feet apart, and have three coal seams, *8a*, *8b* and *8c*, intercalated between them, and a few miles away they are only 50 feet apart, with no intercalated seams, the mind is left in confusion and perplexity, and the practical identification of coal-seams is well nigh impossible. The theory of unequal subsidences, of "very local subsidences," of "warped and folded strata," is itself very confusing, for it requires us to believe that the old shore-areas held themselves in statical equilibrium near the water's edge during the long periods in which the vegetable matter of the coal-marshes was accumulating, and then settled below the water level with all sorts of pitches and irregularities. That there could be such alternations of perfect rest and equipoise, with irregular and lawless subsidence, in a region never disturbed by igneous or other forces which have left any traces of themselves, appears theoretically highly improbable. According to Dr. Newberry's published sections, while his coal seam No. 5 settled down 20 feet in one place, it settled in another 130 feet, and farther on in the same direction only 32 feet. Among the illustrations and proofs of this confusion-theory adduced by Dr. Newberry, is the varying distance between his coal seams No. 1 and No 2. That the Briar Hill, or lowest seam in Mahoning and Trumbull Counties, is very irregularly bedded, I readily concede. It was laid down in patches and strings in the depressions of an uneven surface of Waverly or conglomerate, a surface which had probably been long subjected to sub-aerial erosion. When this very undulating surface, with the coal-swamps filling its basins and winding hollows, subsided below the ocean, the introduction of the proper Coal-measure stratification began, and then occur horizontally arranged sediments. Hence, the next seam of coal above is, according to Mr. Reed, in a "perfectly horizontal position" over areas where No. 1 shows great irregularities. Now, what I should expect in northeastern Ohio would be this: That all the subsequently formed seams of coal would be formed under the conditions of No. 2, and not after the manner of No. 1, whose conditions are entirely exceptional. The lowest seam of coal in Jackson County, in my district, is similarly uneven; but the next seam above—the Anthony seam—is perfectly regular and uniform, and in parallelism with all the seams above it. This parallelism is turned to practical account in the sinking of shafts and other explorations for seams of coal. The position of one being known, that of the rest is known. With the other alleged proofs of irregular subsidences I have no personal knowledge, excepting of the case drawn from the region of Belmont and Guernsey Counties, directly bordering my district. This is the case already referred to where coals No. 8 and No. 9 approximate, with a loss of three intercalated seams, viz: 8*a*, 8*b*, 8*c*. I have specially investigated this case, and find no tendency to convergence between No. 8 and No. 9. Everything proceeds westward regularly. For example, the (so-called) intercalated seam No. 8*c* is one of the great and continuous seams of my district, and sweeps through county after county, always 85 to 100 feet above No. 8, or the Pomeroy seam. Now, if the plane of seam No. 9, starting on the Ohio River, near Wheeling, at an elevation of 150 feet above No. 8, gradually dips to within 50 feet of No. 8, cutting the plane of the great seam, 8*c*, in its passage, we have in this the most wonderful fact in all the stratigraphy of coal fields. The faith of scientific men in this fact will be livelier when Dr. Newberry points out the line of intersection of the two coal planes. I have used his numbering of the coal-seams. There is very great difficulty in accurately numbering our Ohio coal-seams, if we begin at the base of the series, since such are the inequalities of the surface of the Waverly that coal No. 1 in one place is not coal No. 1 in another, and consequently, sections taken at different points, and, especially if so taken by different men, working without concert and not collating their work in the field, will be very conflicting. The make-shift of "intercalated seams" only adds to the confusion.

Dr. Newberry's method, of condemning my views on the authority of names, is one often used. but it has no place in science. I could adduce many and great names in favor of my theory of general parallelism. Of course, I do not claim parallelism in any absolute or mathematical sense, for no marsh would constitute a perfectly even plane; and in the subsequent compression of the sediments between seams of coal, the oozy mud in one place would be more compressed than the sands of another. This would give a little undulation to the planes of the coal-seams. I have recently found some interesting illustrations of this. But I hold to a general and well-marked parallelism, such as makes the stratigraphy of our coal-fields a system of symmetry and beauty, and of the highest practical value to all who wish to know the location and range of coal-seams.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Acetylene and its Determination.*—BLOCHMANN has subjected the metallic compounds of acetylene to exact analysis and derives the formula $C_2H_2Cu_2O$ for the copper salt, either:

$$\begin{array}{l} CH \\ \| \! | \\ C(Cu_2OH) \end{array} \quad \text{or} \quad \begin{array}{l} C \\ \| \! | \\ C \end{array} \!\! > Cu_2 + H_2O;$$

and the similar formula $C_2H_2Ag_2O$ for the silver compound. Its formation is thus given:

$$Cu_2Cl_2(NH_3)_2 + H_2O + C_2H_2 = C_2H_2Cu_2O + (NH_4Cl)_2,$$

and its decomposition by acids thus:

$$C_2H_2Cu_2O + (HCl)_2 = C_2H_2 + Cu_2Cl_2 + H_2O.$$

The acetylene used for the preparation of these compounds was prepared from ethylene bromide by treatment with alcoholic potash. For the estimation of acetylene in gaseous mixtures, he proposes to determine the content in copper of the precipitate obtained by passing the gas through an ammoniacal solution of cuprous chloride. This precipitate should be washed with warm ammonia water so long as the wash-water remains blue or contains chlorine. It is then dissolved in nitric acid, evaporated to dryness in a platinum dish, ignited, and weighed. The Königsberg gas examined in this way, ten liters being drawn through the solution, afforded in two parallel determinations ·063 and ·064 per cent of acetylene by volume. The production of acetylene by a Bunsen burner burning at the bottom of its tube was noticed by Rieth. The author finds that a gas, which gave in its combustion-products when burned as usual in such a burner $0·120^{m}$, or ·08 per cent by volume of acetylene, afforded when burned below, by ordinary gasometric analysis 0·96, and by the copper method 0·80 per cent of acetylene by volume, twelve times the original quantity.—*Ber. Berl. Chem. Ges.*, vii, 274, March, 1874. G. F. B.

2. *On Eucalyptol.*—FAUST and HOMEYER have critically examined the hydrocarbon called eucalyptol by Cloez, which constitutes the principal portion of the ethereal oil of *Eucalyptus globulus*. It was prepared by fractional distillation from this oil, three kilograms yielding 600 grams, boiling between 174° and

180°. Fractionated over sodium, a liquid was obtained boiling constantly from 171° to 174°, entirely soluble in absolute alchohol, ether and chloroform in all proportions, and in fifteen parts of 90 per cent alcohol. Its behavior generally is that of a terpene, which its odor resembles. It absorbs oxygen with avidity and resinifies, which fact explains its variable boiling point. Sulphuric acid turns it brown and dissolves it; water sets it again free. Nitric acid of sp. gr. 1·4, diluted with two parts of water, converts it into paratoluic and terephthalic acids. Elementary analysis gave 88·74 of carbon, and 11·48 of hydrogen, nearly corresponding to the formula $C_{10}H_{16}$. Suspecting an associated hydrocarbon poorer in hydrogen, the authors polymerized the eucalyptol and after dilution distilled. An oil was obtained boiling constantly at 173°–4°, and having the composition of cymol. Conversion into the barium salt of the sulpho-acid confirmed this conjecture. Hence the eucalyptol of Cloez is a mixture of cymol with a terpene, which may be called eucalyptene. It is probable that not over 30 per cent of cymol is present. The significance of these facts, in view of the prominence assigned to the leaves and ethereal oil of this plant in the medication of the day, is obvious.—*Ber. Berl. Chem. Ges.*, vii, 63, Jan., 1874. G. F. B.

3. *Compounds of Thallium with Alcohol-radicals.*—Though the atomic weight of thallium has been fixed from its specific heat, as determined by Regnault, yet its analogy with lead on the one hand and with potassium on the other, renders a determination of the vapor-density of some of its organic compounds desirable. HARTWIG has accordingly undertaken, in the laboratory of Professor Carius in Marburg, the solution of the question. By the action of a solution of thallic chloride in ether upon zinc-ethyl in slight excess, mixed with twice its volume of ether, the chloride of thallium-diethyl is obtained, according to the following equation:

$$TlCl_3+Zn(C_2H_5)_2=Tl(C_2H_5)_2Cl+ZnCl_2.$$

The chloride separates as a cream-like mass which is purified by recrystallization from hot water. It forms beautiful satin-like radiated crystals, difficultly soluble in water, and alterable by light. It may be heated to 190° without change; but then it is decomposed with a slight explosion into thallous chloride and a combustible gas. Analysis fixed its composition as above. By double composition, the sulphate, phosphate, nitrate, acetate, and iodide of thallium-diethyl were obtained. These thallium-ethyl compounds are distinguished clearly from those which tin, mercury or lead forms with the alcohol radicals by the fact that they undergo no decomposition on treatment with silver oxide. By decomposing the sulphate with barium hydrate, thallium-diethyl hydrate was prepared. It is twice as soluble in cold as in hot water, crystallizes in fine silky needles, and decomposes with explosion at 211°. Its solution blues red litmus paper strongly, but is remarkable from the fact that a current of carbonic gas may be passed through it without the formation of a trace of carbonate. All attempts to form from this body thallium-triethyl, though its existence is eminently probable, resulted negatively. Neither it, nor thallic chloride itself, when heated with zinc-ethyl in excess, to 110° or 150°, gave any thallium-triethyl, the products being metallic thallium, zinc chloride and gas (C_2H_4 and C_2H_6).

Subsequently, CARIUS himself, in connection with FRONMÜLLER, undertook the preparation of thallium-triethyl. Thallium-diethyl chloride, heated in sealed tubes with mercury-ethyl to 150° or 160°, afforded a colorless liquid about equal in volume to the mercury-ethyl used, which was butane. The reaction is:

$$Tl(C_2H_5)_2Cl+Hg(C_2H_5)_2=(C_4H_{10})_2+TlCl+Hg.$$

Metallic thallium heated with mercury-ethyl in sealed tubes to 160°–170° gave no more satisfactory results.—*Ber. Berl. Chem. Ges.*, vii, 298, 302, March, 1874. G. F. B.

4. *Taurin not Isethionamide.*—SEYBERTH has sought to repeat, under Ladenburg's direction, Strecker's synthesis of taurin. The ammonium isethionate employed fused at 135° instead of 130° as stated by Strecker. Heated in an oil bath to 210°-230° for several days, it underwent no perceptible change. The temperature was then raised to 230°–240°; decomposition took place and the mass foamed up. After eight hours, the syrupy mass was dissolved in water, boiled with animal charcoal, the solution evaporated, and the solid residue recrystallized from alcohol. The warty crystalline masses thus obtained fused at 147°, and became thereby almost insoluble in alcohol. After repeated extraction with boiling alcohol, the fusing point became constant at 190–193°. Potassium hydrate evolved ammonia from it, and it was readily soluble in water. Analysis gave the formula of isethionamide, $C_2H_7NSO_3$. Since it differs entirely from taurin in its properties, it follows that taurin cannot be the amide of isethionic acid.—*Ber. Berl. Chem. Ges.*, vii, 391, April, 1874. G. F. B.

5. *Condensation of Acetylene by the Silent Electric Discharge.*—P. and A. THENARD have succeeded in condensing acetylene gas to the liquid and even to the solid condition by submitting it to the silent electric discharge or "effluve," in an apparatus contrived by the latter. In one experiment, the gas condensed at the rate of four or five cubic centimeters per minute, there being deposited upon the sides of the tube a solid body, of the appearance of horn, very hard and of a wine color. On analysis, this substance afforded the composition of acetylene gas, of which, therefore, it is a polymer. It is not acted on by the most energetic solvents, fuming nitric-acid, in the cold, being without immediate effect on it. They have not yet succeeded in distilling it. Operating in a different manner, the authors have obtained a liquid body, also having the composition of gaseous acetylene. Owing to the minute quantities produced, it has not been fully examined. BERTHELOT, in the discussion which followed, called the attention of the Academy to the active condensing power of this silent discharge. Acetylene, however, is one of the more easily condensible gases, benzol being only tri-acetylene. By means of zinc chloride he had

obtained a solid product of condensation from benzol, not volatile at the temperature at which glass fuses. He had called it bitumene; Thenard's product was undoubtedly of the same kind, perhaps more condensed.—*C. R.*, lxxviii, 219. *Moniteur Scientifique*, III, iv, 265, March, 1874. G. F. B.

6. *Nitro-compounds of the Allyl series.*—Brackebusch has continued his researches on the production of nitro-derivatives by means of bromides. When bromallyl is treated with silver nitrite, a violent action takes place, and upon distillation there passes over two liquids, the lower of which is a heavy oily substance of a yellow color, which boils at 96° and gives on analysis numbers agreeing with those required by nitro-propylene, $C_3H_5NO_2$. Treated with alcoholic soda solution, the compound $C_3H_4NaNO_2$, sodium-nitro-propylene, is produced; in complete analogy with the nitro-derivatives of the fatty series. Potassium acts similarly. On reduction with acetic acid and iron-filings, saturation with sodium hydrate and distillation in a current of steam, allylamine, $C_3H_5NH_2$, was obtained. The lighter of the two liquids, forming the distillate in the preparation of nitro-propylene, gave similar percentage numbers on analysis. Nascent hydrogen evolved from it ammonia and it boiled at 85°. It was separated from the nitro-propylene by solution in water. It must therefore have been allyl nitrite, C_3H_5.O.NO.—*Ber. Berl. Chem. Ges.*, vii, 225, March, 1874. G. F. B.

7. *Repulsion due to Heat.*—Professor Crookes has recently communicated to the Royal Society the result of some experiments made with a balance for weighing bodies at a different temperature and atmospheric pressure from surrounding objects, and with which he found that at high temperatures the weight appeared to diminish. A straw beam carried masses of pith-ball at each end, and was enclosed in a glass tube connected with a Sprengel pump. The tube being full of air, a spirit flame was passed across the lower part, when the pith-ball descended slightly, and then rose to above its original position, as if there was first an attraction, instantly overcome by the air-current. The same effect was obtained with a glass bulb containing water at 70°, the pith-ball rising whether the heated body was above or below it, though less rapidly in the latter case. The pump was now set to work, when the action became less and less, until with a pressure of 7 mms. it ceased entirely. It seemed evident that the effect was due to air currents, and that in a vacuum no action would ensue. Continuing the exhaustive power, it was found that with a pressure of 3 mms. the pith rose as much as in common air. With a further exhaustion a much feebler source of heat, as the finger, produced a similar effect. A piece of ice caused the ball to descend. Numerous experiments were tried, which proved that the action was not due to electricity. A platinum spiral rendered incandescent by an electric current was next enclosed in the tube, and brass balls were used instead of pith. The tendency seemed to be to bring the center of gravity of the brass ball as

near as possible to the source of heat, when air of ordinary density, or even rarefied, surrounded the balance. When the pressure was 5 mms., the attraction was still strong. With a pressure of ½ mm., the attraction was still perceptible, but only feeble. The working of the pump was continued for some time, when the attraction became nothing. The pump being kept at work for an hour longer, a faint but unmistakable repulsion was detected. This effect increased when the pump continued to work for several hours. The pump tubes were now washed with sulphuric acid, and at the end of an hour the spiral was found to be energetically repellant. The fingers, a spirit flame and other sources of heat produced the same effect. By chemical means, a vacuum was formed so perfect that it would not transmit an electric current, when the repulsion was decided and energetic. Projecting on a pith-ball balance the different portions of the solar spectrum, the repulsion was so worked as to enlarge the apparatus. On repeating Cavendish's experiment, he found that in air the large mass when colder than the ball, repelled it, and when hotter, attracted. The opposite effects were obtained in a vacuum. Similar forces showed themselves with a variety of substances, as metals, ivory, charcoal, etc.

After showing that air currents cannot be the cause of these motions, he refers to this repulsive action of heat in Nature, and states that it is not improbable that the attraction now shown to exist between a hot and cold body will equally prevail when for the temperature of melting ice is substituted the cold of space, for a pith-ball a celestial sphere, and for an artificial vacuum a stellar void. In the radiant molecular energy of cosmical masses may at last be found that "agent acting constantly according to known laws" which Newton held to be the cause of gravity.—*Quart. Jour. Sci.*, xlii, 274. E. C. P.

8. *Double refraction of a Viscous Fluid in motion.*—Professor MAXWELL, in a paper before the Royal Society, states that a viscous fluid, according to Poisson, behaves as an elastic solid periodically liquefied for an instant, and solidified again, so that at each fresh start, it becomes for an instant like an elastic solid free from strain. The state of strain of certain transparent bodies may be investigated by this action on polarized light. This action, observed by Brenslet, was shown by Fresenel to be an instance of double refraction.

To ascertain whether the state of strain in a viscous fluid in motion could be detected by its action on polarized light, a cylindrical box was made with a glass bottom. In this a solid cylinder could be made to rotate, and the fluid to be examined placed in the annular space between the cylinder and the sides of the box. Polarized light was thrown up through the fluid parallel to the axis, while the cylinder was made to rotate. No effect was, however, obtained with solution of gum or syrup. Canada balsam, which had been very thick and almost solid in a bottle, affected the light when compressed.

It is easy, however, to observe the effect in Canada balsam, which is so fluid that it rapidly assumes a level surface after being disturbed. Put some Canada balsam in a wide-mouthed square bottle; let light polarized in a vertical plane be transmitted through the fluid; observe the light through a Nicol's prism, and turn the prism so as to cut off the light; insert a spatula in the balsam in a vertical plane passing through the eye. Whenever the spatula is moved up or down in the fluid, the light reappears on both sides of the spatula; this continues only so long as the spatula is in motion. As soon as the motion stops the light disappears, and that so quickly as to prevent, hitherto, the determination of the rate of relaxation of that strain which the light indicates.

If the motion of the spatula in its own plane, instead of being the plane of polarization, is inclined 45° to it, no effect is observed, showing that the axes of strain are inclined 45° to the plane of sheering, as indicated by the theory. Among transparent bodies there is considerable diversity in their action on polarized light. If a small portion is cut from a piece of annealed glass at a place where the strain is uniform, the effect on polarized light ceases as soon as the glass is relieved from the stress caused by the unequal contraction of the parts surrounding it. But if a plate of gelatine is allowed to dry under longitudinal tension, a small piece cut out of it exhibits the same effect on light as it did before, showing that a state of strain can exist without the action of stress. A film of gutta percha, which has been stretched in one direction, has a similar action on light. If a circular piece is cut out of such a stretched film and warmed, it contracts in the direction in which the stretching took place. The body of a sea-nettle has all the appearance of a transparent jelly, and it seemed as if spontaneous contraction of the living animal might be rendered visible by means of light transmitted through its body. It proved, however, that even a considerable pressure might be applied without affecting the light, showing, as may be seen by dissection, that the sea-nettle is not a true jelly, but consists of cells filled with fluid.

These instances suggest a means of studying the various liquids so as to determine whether they differ from solids in having a very small rigidity, or in having a small time of relaxation, or in both ways. These, which like Canada balsam, act strongly on polarized light, have probably a small rigidity, but a sensible time of relaxation. Those which do not show this action are probably much more rigid and owe their fluidity to the smallness of their time of relaxation.—*Phil. Mag.*, xlvii, 390. E. C. P.

9. *Spectroscope with Fluorescent Eyepiece.*—M. Lovet has employed a spectroscope in which a plate of some transparent and fluorescent body is placed at the point of the observing telescope, and then viewed through the eyepiece, which is inclined at an angle. The fluorescent plate may be made of uranium glass, or of thin pieces of glass a short distance apart containing any fluorescent liquid between them Two lines drawn at right angles on the glass take the place of cross-hairs. It is a good plan to inter-

pose a plate of cobalt blue glass to cut off the more brilliant portion of the spectrum. If the eyepiece is not inclined, the presence of the fluorescent plate does not prevent our observing the lines of the luminous part of the spectrum with accuracy, but the fluorescent spectrum produced by the plate is then seen but poorly. On inclining it, however, the luminous spectrum can no longer be seen, but the fluorescent spectrum then appears very clearly, of a uniform tint traversed by dark lines. These lines may be brought to coincide with the lines drawn on the glass and their deviation measured.

With uranium glass the fluorescent spectrum is well seen from *G;* it is very intense near *H;* the four rays *M* are also visible, but beyond that, little can be seen.

With bisulphate of quinine, the spectrum is very beautiful and bright. It extends but little into the visible part, about to *h*. We see very clearly the rays as far as *N*, and even a little beyond.

Esculine appears to give the most brilliant spectrum; we distinguish very clearly the lines *N*, and even *O*. The spectrum extends into the violet, a little beyond that of quinine.

Rose-napthaline (Magdala) gives good results for the ultra-violet portion as far as *M;* but the appearance of the fluorescent spectrum is curious in the less refrangible portions. From near *D* to *M* we distinguish all the rays with perfect clearness.—*Bib. Univ.*, No. 196, p. 338. E. C. P.

10. *Polarization of Metallic Surfaces.*—M. G. QUINCKE has determined the prime angle of incidence and prime azimuth of the Fraunhofer lines *C*, *D*, *E*, *F* and *G*, for various substances. Through a collimator with a vertical slit a beam of parallel rays falls on a polarizing prism standing in the azimuth 45°, and then on an observing telescope whose cross-lines can be illuminated by a plane glass mirror. A thin wire runs across the slit of the collimator to which the cross-thread of the telescope is adjusted. Between the collimator and observing telescope is a stand in which are placed two parallel mirrors of the substance to be examined. The analyzing prism is placed on a vertical circle, and between it and the eye is inserted a flint-glass prism. The light from the slit is thus spread out, giving a horizontal spectrum with Fraunhofer lines of from 1° to 3° breadth. If from a heliostat sunlight be let through the slit of the collimator and polarized in the azimuth 45°, a dark streak appears in the spectrum, which may be made to fall on any desired Fraunhofer line by changing the angle of incidence and the azimuth of the analyzer. The difference of phase and ratio of the amplitudes of the components polarized parallel and perpendicular to the plane of reflection, or the intensity of the components of the reflected light, can thence be calculated for any angle of incidence. The most perfect surfaces are possessed by nickel, silver, gold and selenium; the latter being melted and pressed with a cold plate of glass. With the exception of gold, in the case of all the metals here mentioned, the prime angle of incidence (polarization angle) diminishes with the diminution of the wave-

length, the reverse of what takes place with transparent bodies. The diminution, however, is very different with different metals.

The prime azimuths partly increase and partly diminish as the wave-lengths become less. Platinum shows a maximum value for the line *D*, cobalt and bismuth for the line *E*, and tin for the line *F*.

Observations were also made with thin transparent layers of gold, silver and platinum of thicknesses varying from 8 to 75 millionths of a millimeter. They show that the prime angle of incidence and the prime azimuths increase as the thickness increases, but in different degrees for different colors. With silver the prime azimuth exhibits a maximum value for a certain Fraunhofer line, which maximum moves toward the red end of the spectrum as the thickness is augmented. Gold, in transparent as in non-transparent layers, has a minimum angle of incidence at Fraunhofer's line *F*. These values are not altered by magnetizing or electrifying the plates.—*Pogg. Jubelband*, 336 ; *Phil. Mag.*, xlvii, 321. E. C. P.

II. Geology and Natural History.

1. *Small size of the Brain in Tertiary Mammals;* by Prof. O. C. Marsh.—At the last meeting of the Connecticut Academy of Arts and Sciences, June 17th, Prof. Marsh of Yale College made a communication on the size of the brain in Tertiary Mammals. His researches on this subject have been mainly confined to the larger extinct mammals which he had obtained in the Rocky Mountain region, and the results are of peculiar interest. The Eocene mammals all appear to have had small brains, and in some of them the brain cavity was hardly more capacious than in the higher reptiles. The largest Eocene mammals are the *Dinocerata*, which were but little inferior to the elephant in bulk. In *Dinoceras* Marsh, the type genus, the brain cavity is not more than one-eighth the average size of that in existing Rhinoceroses. In the other genera of this order, *Tinoceras* Marsh and *Uintatherium* Leidy, the smallness of the brain was quite as remarkable. The gigantic mammals of the American Miocene are the *Brontotheridæ*, which equalled the *Dinocerata* in size. In *Brontotherium* Marsh, the only genus of the family in which the skull is known, the brain cavity is very much larger than in the Eocene *Dinoceras*, being about the size of the brain in the Indian *Rhinoceros*. In the Pliocene strata of the West, a species of *Mastodon* is the largest mammal, and although but little superior in absolute size to *Brontotherium*, it had a very much larger brain, but not equal to that of existing Proboscidians. The Tapiroid ungulates of the Eocene had small brain cavities, much smaller than their allies, the Miocene *Rhinocerotidæ*. The Pliocene representatives of the latter group had well developed brains, but proportionally smaller than living species. A similar progression in brain capacity seems to be well marked in the equine mammals, especially from the Eocene *Orohippus*, through *Miohippus* and *Anchitherium* of the Miocene,

Pliohippus and *Hipparion* of the Pliocene, to the recent *Equus*. In other groups of mammals, likewise, so far as observed, the size of the brain shows a corresponding increase in the successive subdivisions of the Tertiary. These facts have a very important bearing on the evolution of mammals, and open an interesting field for further investigation.

2. *Coal (or Lignite) in the Cretaceous of Minnesota.*—Prof. N. H. Winchell, in his Report for 1873, announces the existence of coaly layers at several points in the Cretaceous of Minnesota, especially in the banks of Crow Creek and those also of the Redwood; but states that all explorations thus far made have proved fruitless. The coal found on the Redwood was earthy, passing sometimes into good Cannel coal, or into a bituminous clay; the compact Cannel coal is in detached lumps, and occurs throughout a band about four feet in thickness. At another outcrop, the Lignitic band is in the form of a black bedded clay or shale, five or six feet thick, containing some coaly or charcoal-like fragments. Mr. Winchell adds that "so far as discovered, there is not enough coal embraced in the Cretaceous of this State to warrant sanguine expectations of its becoming economically useful."

3. *Geological Survey of Pennsylvania.*—Professor J. P. Lesley, of the University of Pennsylvania, has been chosen by the Commissioners for the charge of the new geological survey of Pennsylvania, to which the Legislature have devoted $35,000 for three years. Professor Lesley, as is well known, was one of the original corps of Professor H. D. Rogers on the former survey, and no man has probably so thorough a knowledge of the geology of the State as he. It is proposed to devote the re-survey especially to its economic geology.

4. *Dana's Manual of Geology.*—A new edition of Dana's Manual has just been issued by Ivison, Blakeman, Taylor & Co., New York. The work has been much enlarged and to a great extent rewritten by the author, this having been required, as the preface states, by the progress which the science has made during the twelve years past. American geology, especially, has been greatly advanced during that interval through the many State surveys, and also through the independent researches of Leidy, Marsh, Cope, and other paleontologists and geologists.

Many new illustrations have been introduced, and, among them, an excellent engraving of the human skeleton of the cave of Mentone forms a frontispiece to the volume. Besides the additions to the text, which the new facts and the progress in the principles of the science required, the author has introduced a chapter on one of the departments of chemical geology, giving the destructive and formative effects that take place through the agency of water and the atmosphere, and also another chapter, at the close, presenting a brief review of geological dynamics, under the title of "Effects referred to their causes." The work is printed in excellent style; and although containing one-sixth more matter than the former edition, the volume, owing to its larger page of text, is but little increased in actual size.

5. *Report on the Exploration of Brixham Cave, conducted by a Committee of the Geological Society*, and under the superintendence of WILLIAM PENGELLY, Esq., F.R.S., aided by a local committee; with Descriptions of the Animal Remains, by GEORGE BUSK, F.R.S., and of the Flint Implements, by JOHN EVANS, Esq., F.R.S. By JOSEPH PRESTWICH, F.R.S., F.G.S., &c., Reporter. 100 pp. 4to.—This detailed and well illustrated memoir bears evidence that no pains in the exploration of the cave was spared that could add to the exactness and completeness of the series of facts afforded by it. The principal conclusions are already in the last edition of Lyell's Antiquity of Man, and need not be here repeated.

6. *Helderberg Rocks in New Hampshire.*—A note from Professor C. H. Hitchcock states that Mr. Huntington has obtained specimens of *Halysites* or chain coral on Fitch Hill, in Littleton.

7. *Das Gebirge um Hallstatt, eine geologische, paläontologische Studie aus den Alpen;* von EDMUND MOJSISOVICS VON MOJSVAR. 1. Theil. Die Mollusken-Faunen der Zlambach- und-Hallstätter Schichten, 1 Heft. From the Abhandlungen d. k. k. geol. Reichsanstalt, vol. 6, 4to. Vienna, 1873.—This is one of the ablest and most complete of the publications issued by the Geological Survey of Austria, and exhibits in all its parts the care and skill of the author.

Two new genera of Ammonitoids are described, Pinnacoceras and Sageceras. The former is a portion of the old genus Arcestes of Suess, with *Arcestes galeiformis* as the type, and the latter is new, with *Goniatites Haidingeri* as the type. These genera are most carefully described, but the main merit of the work consists in the apparently successful attempts to classify the Hallstatt beds, which have been subjects of discussion for many years among European paleontologists. For accomplishing this task in one of the most difficult localities in Europe, the author deserves and will doubtless win the highest praise. The species so far as published confirm the view that Hallstatt presents a peculiar fauna, but one which contains also a few species that are partly Jurassic, and others that are partly Carboniferous in their aspect. These are sufficient in number to show closer relations between the Carboniferous and Jurassic forms of Mollusca than are elsewhere known.

G. H.

8. *On Datolite;* by E. S. DANA, (*Tschermak's Min. Mittheilungen.* Vienna, 1874.)—This paper contains the results of a crystallographic study of the datolite of the principal European localities, and is illustrated by a plate containing figures of crystals from Tuscany, Arendal, and Andreasberg, with also, for comparison, three of the forms described and figured by the author in his Bergen Hill paper.* In addition, a catalogue is given of all the known planes of datolite crystals, now numbering 71, of which 16 were added by Mr. Dana from the Bergen Hill crystals, and 10 from those of foreign localities. A table contains the principal angles of all the forms, for the most part recalculated by the

* This Jour., III, iv, 16.

author, and a diagram presents a map-projection, after Miller's method, of all known planes in their zones. The crystals studied were from the Royal Mineralogical Museum of Vienna, of which Professor Tschermak is Director.

9. *On Atacamite;* by E. S. DANA, (*Tschermak's Min. Mittheilungen.* Vienna, 1874.)—The results of a large number of measurements of crystals of Atacamite, from Wallarro, South Australia, are here presented. They prove that the species is, as hitherto supposed, orthorhombic, but show further some irregularities in the planes of the vertical series, which can be explained only by the assumption of a dome 40-*ĭ* taking the place of *i*-*ĭ*, and a corresponding pyramid, in place of the prism I. The crystals under examination were placed in the hands of the author by Dr. A. Schrauf of the Vienna Mineralogical Museum.

10. *Changes in the character of Vegetation produced by Sheep-grazing.*—Dr. SHAW, of the Cape of Good Hope, sent to the Linnean Society an interesting communication (published in its Journal, No. 75) upon the ill effects of over stocking the dry grazing districts of Southern Africa with merino sheep. The sheep have introduced one obnoxious bur, that of *Xanthium spinosum*, which came in their wool. and which has spread to such an extent that it is only by the enforcement of severe, but almost too late, legislative enactments " that it is kept from being a sweeping curse to the wool-producers. In the Orange River Republic it has so affected the wool in some parts of the country as to make it nearly unremunerative as a staple product.

The principal changes, however, are not in the introduction of foreign plants, but in the alteration of the range and relative abundance of those that belong to the country. When first introduced into the midland or pasturage region, the sheep fed mainly on the grass. This, in a country with only periodical rain and under a high sun, soon began to fail. " Suffruticose plants and shrubs could alone stand against the sheep and such a climate; and at first, and as long as the grass was prominent, these could enjoy immunity from the sheep. But the grass vanished very rapidly, and the brush and scrub came to be the main resource of the flocks, and the ground was left to them and to obnoxious and poisonous herbs, . . . and to the intoxicating *Melicæ*, the 'dronk' grass of the Dutch colonists." It used to be thought that grasses were everywhere salubrious and bland; but it appears that in South Africa there are certain species of *Melica* of very ill repute; that these have increased incredibly in the last few years; and that the transport riders with their oxen (the only carrying power) have to travel over a wide stretch without stopping, because of these grasses, " on eating which cattle become afflicted with intoxication to an alarming extent." As the edible grasses and herbs, and then the less inviting scrubby plants, are consumed and destroyed, the bitter and nauseous plants of the drier Karoo region adjacent come in and contribute their energies to the extirpation of the indigenous and proper flora of the region, and render it more and more unfit for sheep pasturage.

It may be said that this same region used to support millions of antelopes; but then, these were always on the move, in their migration following the rains and the young fresh herbage; whereas the sheep are kept upon the limited area, where they cause as much destruction by their tread as by eating. The rain-fall is said to be affected, becoming more precarious and in thunder torrents, and so does not to the same extent soak into the ground, is carried off by the water-courses, and fails to supply the springs, which become weaker and less perennial year after year. Moreover, the *Chrysocoma* and other disagreeable *Compositæ*, at first eschewed by the sheep, but upon which they have to feed from necessity, greatly injure the mutton, which tastes and smells of them. Truly was it said by the veteran Fries, that the appointed lord of creation is apt to do his utmost to ruin it; that thorns and thistles, ill-favored and poisonous plants, mark the track which man has traversed through the earth.

The introduction of sheep into the foot-hills and higher portions of the Sierra Nevada in California is beginning to make havoc of its proper flora; from which it is not botany only that will suffer.

A. G.

11. *The Perigynium and occasional Seta in Carex* have recently been studied in England by Professor McNab and Professor Dyer; and the results of an investigation of the early development are recorded in two short papers in the Linnean Society's Journal, No. 75 (April). Contrary to Mr. Bentham's suggestion, the seta is proved to be of axial nature; and the perigynium answers to a single bract, according to Professor McNab—of probably one or possibly two bracts, according to Professor Dyer.

A. G.

21. Maximowicz, *Diagnoses Plantarum Japoniæ, etc.*—The 15th and 16th decades, September and October, 1873, contain some generally interesting matters. For instance, a review of the species of *Sanguisobta*, from which it appears that *S. Canadensis* and *S. alpina* develop their flowers from the base of the spike upward, while in *S. officinalis* and *S. obtusa* the flowering proceeds from the apex of the spike toward the base. There is a good account of the Japanese *Pomaceæ*, and another of *Ribes*, with revised characters. Dr. Maximowicz has some important remarks on the generic value of the number of *vittæ* in *Umbelliferæ*, upon the strength of which he declines to separate *Archangelica* from *Angelica*, and to divide *Conioselinum* between *Selinum* and *Ligusticum*.

A. G.

13. *Botanical Contributions*, by Asa Gray. From *Proceedings of the American Academy of Arts and Sciences*, May, 1874, vol. ix.—This paper is wholly, as the preceding one was mainly, devoted to *Compositæ*, and it contains several North American genera, such as *Madia*, *Hemizonia* (18 species), *Bæria*, *Actinolepis* (8 species), *Schkuhria* (with a very peculiar new species), *Helenium* (now of 20 species, including Mexican ones), *Microseris*, including *Calais* (17 species), *Malacothrix* (11 species), *Troximon*, including *Macrorhynchus* and *Lygodesmia*.

The present volume of the Proceedings indicates an increased activity of the American Academy, at least in the way of publication. This ninth volume is filled with the work of the year, ending with the meeting in May, and is actually issued early in June. The papers are separated from the proceedings, the latter, with the notices of members deceased during the two past years, forming the latter part of the volume. An activity upon which the Academy (now approaching its centenary) may pride itself, and which is highly creditable to the two secretaries. A. G.

14. *Influence of Climate and Topography on Trees around San Francisco Bay;* by I. G. COOPER, M.D. A paper in Proc. California Acad. Sci., March, 1874.—The comparative scarcity of trees and small number of species around the bay, as compared with the districts immediately north and south, arrested Dr. Cooper's attention. He concludes that the main cause of the deficiency is the prevalence of the sea-winds from the northwest, throughout the dry season; these sweeping in through the Golden Gate and through the depression of the coast range between Petaluma and Tomales Bay. The character of the soil and elevation above the sea are of comparatively little consequence. "Since the general course of the mountain ranges is nearly northwest in this region, and the wind strikes their southwest slopes obliquely, and the sun in its daily course shines most intensely and longest upon the same exposure, it follows that this slope is almost everywhere destitute of trees, although along the coast exposed to the greatest rainfall and the most fog. The opposite or northeast slopes, therefore, usually have the greatest tree growth." . . . "These winds seem to act in two ways. First, by their drying power, as seen in the absence of trees on slopes of hills exposed to them, while trees may abound on the opposite slope, though facing the south and more exposed to the sun. Second, by their coolness not permitting the sun's heat to produce the growth, even where moisture is abundant." The grouping of the trees of the district at different distances around San Francisco is then given, in more detail. A. G.

15. *Catalogue of Plants growing without Cultivation in the State of New Jersey, etc.;* by OLIVER R. WILLIS, Instructor of Natural Science in the Alexander Institute. New York (Schemerhorn & Co.), 1874. Besides the 70 pages of Catalogue, Mr. Willis, having the needs of botanical students and collectors in view, has usefully filled several pages with practical suggestions to teachers, and directions for drying and preserving plants. The latter are to be commended, except as to the liquid for poisoning specimens. "To preserve specimens from the depredations of insects, make a saturated solution of corrosive sublimate in fourth-proof alcohol, then add an equal bulk of water, and with this solution wet the parts of the plants attacked." A solution of this sort will not penetrate into the specimens promptly, and the weak spirit will evaporate too slowly. Use 95 per cent alcohol, and dilute with the same, or, better, dissolve an ounce of corrosive

sublimate in a quart of the strongest alcohol, and it will be of the proper strength. The poison will thus promptly penetrate the interior of the specimen, and be left there, while the quick evaporation of the alcohol avoids the injury which water or weak spirit may give rise to. Mr. Willis has also added to his Catalogue a list of the botanists of this country, taken from the Botanical Directory of the Torrey Club. The Catalogue itself is unusually full of details, popular accounts of the more remarkable plants being given, and of many that are not particularly so. One item is especially gratifying to farmers, viz., that the author has never seen a Canada Thistle in the State, "except in the Presbyterian church-yard, Freehold." Among the wild plants omitted we will mention the charming *Linnœa.* A. G.

16. Professor C. F. MEISSNER of Basle, an excellent man and distinguished botanist, who has long been infirm, died on the second of May, ult. His large herbarium had become the property of Columbia College, New York, through the liberality of a generous lover of botany, and is joined with that of the lamented Dr. Torrey. A. G.

17. *Illustrated Catalogue of the Museum of Comparative Zoology*, No. VII. *Revision of the Echini*, Part IV; by ALEXANDER AGASSIZ.—This part completes the Revision of the Echini, upon which Mr. Agassiz has been engaged for several years, and is devoted to their Anatomy and Embryology. Like the previous parts, it is accompanied by many excellent plates, done by the new photographic processes, increasing the total number of plates to ninety-four, with sixty-nine wood-cuts. Notwithstanding the very serious loss, in the Boston fire, of manuscripts, drawings and plates intended for this volume, the author has produced a very useful and valuable work, in which the entire anatomy and embryology of the Echini, so far as known, are reviewed in detail. There are also chapters on the habits, geological succession, and affinities of the Echini. Under the last head the author maintains the view, held quite generally by American naturalists, that the Echinoderms are closely related to the Acalephs and Polyps, in opposition to the opinions of many prominent European zoologists, that they are allied to the worms, or form by themselves a separate branch of the Animal Kingdom. V.

18. *Illustrated Catalogue of the Museum of Comparative Zoology.* No. VIII. *Zoological Results of the Hassler Expedition.* I. *Echini, Crinoids, and Corals*; by ALEX. AGASSIZ and L. F. DE POURTALES. 49 pp. 4to, with 10 plates.—The first part, relating to the Echini, is by Mr. Agassiz. It contains description of, and notes upon, many of the more interesting species collected by the expedition, and is accompanied by four plates, upon which eleven species are well figured by the "heliotype" process. Many of the more important species were dredged in 100 fathoms, off Barbadoes; others are from Patagonia, the west coast of America, and the Galapagos Islands. Mr. Pourtales describes and figures a new species of *Rhizocrinus* (*R. Rawsonii*) dredged in 80 to 120

fathoms, off Barbadoes. He also describes and figures a specimen of *Holopus Rangii* D'Orb., from off Barbadoes, loaned to Professor Agassiz by Governor Rawson, and upon the study of which Professor Agassiz was engaged during his last days at the Museum. In the part relating to the Corals, Mr. Pourtales has described and figured many interesting deep-sea species, many of which are new, and has given additional localities for others previously described.* Most of them are from the rich locality off Barbadoes, referred to above, but a few are from Brazil and the Pacific. Among the more interesting forms is a new genus (*Duncania Barbadensis*) which is referred to the Rugosa. V.

19. *On the Habits of the California Wood-rat;* by A. W. CHASE, Assistant U. S. Coast Survey. (From a letter to B. Silliman, dated Anaheim, California, May 27, 1874.)—While on the northern coast I noticed a fact in natural history to me quite curious, regarding the habits of the so-called California *wood-rat.* I am not sufficiently versed in such matters to give you the name of this interesting creature. It is a little larger than an ordinary Norway rat, dark-brown in color, with large lustrous eyes, and a tail covered with thin hairs. I should call it intermediate between the squirrel and rat. This creature builds its nest in the woods, sometimes on the ground, more frequently in the lower branches of trees. They accumulate a surprising quantity of dried twigs, which they interlace to form a dome-shaped structure, often ten or twelve feet high and six or eight feet in diameter.

Openings in the mass lead to the center, where is found the nest, consisting of the finely-divided inner bark of trees, dried grass, etc. But it is to the peculiar thievish propensity of this little creature that I wish to call attention.

To make my story intelligible, I would first state that I am partial owner of some property on the Oregon coast, on which a saw mill had been placed, but which, owing to various causes, has never been in operation. On this property was a dwelling house for the hands, in which, on work being discontinued, were stored a quantity of stuff, tools, packing for the engine, six or seven kegs of large spikes; in the closets, knives, forks, spoons, etc. A large cooking stove was left in one of the rooms.

This house was left uninhabited for two years, and, being at some distance from the little settlement it was frequently broken into by tramps who sought a shelter for the night. When I entered this house I was astonished to see an immense rat's nest on the empty stove. On examining this nest, which was about five feet in height, and occupied the whole top of the stove (a large range), I found the outside to be composed entirely of *spikes*, all laid with symmetry so as to present the points of the nails outward. In the center of this mass was the nest, composed of finely divided fibers of the hemp packing. Interlaced with the

* In a recent letter Mr. Pourtales desires us to make the following correction: He now considers the species described as *Flabellum Braziliense* (p. 38, pl. VI, figs. 16, 17) identical with the *Euphyllia spinulosa* Dana, and therefore proposes to call it *Flabellum spinulosum.*

spikes, we found the following: About three dozen knives, forks and spoons, all the butcher knives, three in number, a large carving knife, fork and steel; several large plugs of *tobacco;* the outside casing of a silver watch was disposed of in one part of the pile, the glass of the same watch in another, and the works in still another; an old purse containing some silver, matches and tobacco; nearly all the small tools from the tool closets, among them several large augers. Altogether, it was a very curious mixture of different articles, all of which must have been transported some distance, as they were originally stored in different parts of the house.

The ingenuity and skill displayed in the construction of this nest and the curious taste for articles of iron, many of them heavy, for component parts, struck me with surprise. The articles of value were I think stolen from the men who had broken into the house for temporary lodging. I have preserved a sketch of this *iron-clad* nest, which I think unique in natural history.

Many curious facts have since been related me, concerning the habits of this little creature. A miner told me the following: He once, during the mining excitement in Siskyiou County, became in California parlance "dead broke," and applied for and obtained employment in a mining camp, where the owners, hands and all slept in the same cabin. Shortly after his arrival small articles commenced to disappear; if a whole plug of tobacco were left on the table, it would be gone in the morning. Finally a bag, containing one hundred or more dollars in gold dust, was taken from a small table at the head of a "bunk," in which one of the proprietors of the claim slept. Suspicion fell on the new comer, and he would perhaps have fared hardly; for, with those rough miners, punishment is short and sharp; but, just in time, a large rat's nest was discovered in the garret of the cabin, and in it was found the missing money, as well as the tobacco and other articles supposed to have been stolen.

20. *The Doctrine of Evolution: its data, its principles, its speculations, and its Theistic bearings.* By ALEXANDER WINCHELL, LL.D., Chancellor of Syracuse University, &c. 148 pp. 12mo. New York, 1874. (Harper & Brothers.)—This volume is an argument against the derivation of species by gradual variation. The author sustains the idea of a system of evolution in the organic kingdoms, but with this qualification, that "the evolution, while a real evolution in its main features," has "many facts of a strongly discordant character" in its details. He observes that the theories of evolution admitting of progress by considerable leaps "escape measurably from the embarrassments" which those encounter that make the progress gradual. In his concluding remarks, after a recapitulation of his deductions from known facts, the author says that there exists no "*a priori* ground for denying that some phase of the doctrine of filiative evolution in the organic world may yet become proven and established," and then rightly adds, that if so established there will be in this "no proof of the absence of immediate divine agency from any of the operations of life."

III. Astronomy.

1. *On the Motions of some of the Nebulæ toward or from the Earth;* by William Huggins.—The observations on the motions of some of the stars toward and from the earth, which I had the honor to present to the Royal Society in 1872, appeared to show, from the position in the heavens of the approaching and receding stars, as well as from the relative velocities of their approach and recession, that the sun's motion in space could not be regarded as the sole cause of these motions. "There can be little doubt but that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars and also a motion peculiar to each star."*

It then presented itself to me as a matter of some importance to endeavor to extend this inquiry to the nebulæ, as it seemed possible that some light might be thrown on the cosmical relations of the gaseous nebulæ to the stars and to our stellar system by observations of their motions of recession and approach.

Since the date of the paper to which I have referred, I have availed myself of the nights sufficiently fine (unusually few even for our unfavorable climate) to make observations on this point. The inquiry was found to be one of great difficulty, from the faintness of the objects and the very minute alteration in position in the spectrum which had to be observed.

At first the inquiry appeared hopeless, from the circumstance that the brightest line in the nebular spectrum is not sufficiently coincident in character and position with the brightest line in the spectrum of nitrogen to permit this line to be used as a fiducial line of comparison. The line in the spectrum of the nebulæ is narrow and defined, while the line of nitrogen is double, and each component is nebulous and broader than the line of the nebulæ. The nebular line is apparently coincident with the middle of the less refrangible line of the double line of nitrogen.†

The third and fourth lines of the nebular spectrum are undoubtedly those of hydrogen, but their great faintness makes it impossible to use them as lines of comparison under the necessary conditions of great dispersive power, except in the case of the brightest nebulæ.

The second line, as I showed in the paper to which I have referred, is sensibly coincident with an iron line, wave-length 495·7; but this line is inconveniently faint, except in the brightest nebulæ.

In the course of some other experiments my attention was directed to a line in the spectrum of lead which falls upon the less refrangible of the components of the double line of nitrogen. This line appeared to meet the requirements of the case, as it is narrow, of width corresponding to the slit, defined at both edges, and in the position in the spectrum of the brightest of the lines of the nebulæ.

* Proceedings of the Royal Society, vol. xx, p. 392. † Ib., p. 380.

In December, 1872, I compared this line directly with the first line in the spectrum of the Great Nebulæ in Orion. I was delighted to find this line sufficiently coincident in position to serve as a fiducial line of comparison.

I am not prepared to say that the coincidence is perfect; on the contrary, I believe that if greater prism power could be brought to bear upon the nebulæ, the line in the lead spectrum would be found to be in a small degree more refrangible than the line in the nebulæ.

The spectroscope employed in these observations contains two compound prisms, each giving a dispersion of 9° 6′ from A to H. A magnifying power of 16 diameters was used.

In the simultaneous observation of the two lines it was found that if the lead line was made rather less bright than the nebular line, the small excess of apparent breadth of this latter line, from its greater brightness, appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible side of the two lines appeared to be in a straight line across the spectrum. This line could be therefore conveniently employed as a fiducial line in the observations I had in view.

In my own map of the spectrum of lead this line is not given. In Thalén's map (1868) this line is represented by a short line to show that, under the conditions of spark under which Thalén observed, this line was emitted by those portions only of the vapor of lead which are close to the electrodes.

I find that by alterations of the character of the spark this line becomes long and reaches from electrode to electrode. As some of those conditions (such as the absence of the Leyden jars, or the close approximation of the electrodes when the Leyden jars are in circuit) are those in which the lines of nitrogen of the air in which the spark is taken are faint or absent, the circumstance of the line becoming bright and long, or faint and short inversely, as the line of nitrogen suggested to me the possibility that the line might be due not to the vapor of lead but to some combination of nitrogen under the presence of lead vapor. As, however, this line is bright under similar conditions when the spark is taken in a current of hydrogen, this supposition cannot be correct.

A condition of the spark may be obtained in which the strongest lines of the ordinary lead spectrum are scarcely visible, and the line under consideration becomes the strongest in the spectrum, with the exception of the bright line in the extreme violet.

I need scarcely remark that the circumstance of making use of this line for the purpose of a standard line of comparison is not to be taken as affording any evidence in favor of the existence of lead in the nebulæ.

Each nebula was observed on several nights, so that the whole observing time of the past year was devoted to this inquiry. In no instance was any change of relative position of the nebular line and the lead line detected.

It follows that none of the nebulæ observed shows a motion of translation so great as 25 miles per second, including the earth's motion at the time. The motion must be considered in the results to be drawn from the observations; for if the earth's motion be, say, 10 miles per second from the nebulæ, then the nebula would not be receding with a velocity greater than 15 per second; but the nebula might be approaching with velocity as great as 35 miles per second, because 10 miles of this velocity would be destroyed by the earth's motion in the contrary direction.

The observations seem to show that the gaseous nebulæ as a class of bodies have not proper motions so great as many of the bright stars. It may be remarked that two other kinds of motion may exist in the nebulæ, and if sufficiently rapid, may be detected by the spectroscope. 1. A motion of rotation in the planetary nebulæ, which might be discovered by placing the slit of the instrument on opposite limbs of the nebulæ. 2. A motion of translation in the visual direction of some portions of the nebulous matter within the nebula, which might be found by comparing the different parts of a large and bright nebula.

Sir William Herschell states that "nebulæ were generally detected in certain directions rather than in others, and the spaces preceding them were generally quite deprived of stars; that the nebulæ appeared some time after among stars of a certain considerable size, and but seldom among very small stars; that when I came to one nebula I found several more in the same neighborhood, and afterward a considerable time passed before I came to another parcel."*

Since the existence of real nebulæ has been established by the use of the spectroscope, Mr. Proctor† and Prof. D'Arrest‡ have called attention to the relation of position which the gaseous nebulæ hold to the Milky Way and the sideral system.

It was with the hope of adding to our information on this point that these observations of the motions of the nebulæ were undertaken.

In the following list the numbers are taken from Sir J. Herschel's "General Catalogue of Nebulæ." The earth's motion given is the mean of the motions of the different days of observation.

No.	h.	H.	Others.	Earth's motion from Nebulæ.
1179	360	--	M. 42	7 miles per second.
4234	1970	--	Σ. 5	12 " "
4373	--	IV. 37.	--	1 " "
4390	2000	--	Σ. 6	2 " "
4447	2023	--	M. 57	3 " "
4510	2047	IV. 51.	--	14 " "
4964	2241	IV. 18.	--	13 " "

—*Proc. Roy. Soc.*, *March*, 1874.

* Philosophical Transactions, 1784, p. 448.

† "Other Worlds than Ours," pp. 280–290.

‡ "Astronomische Nachrichten," No. 1908, p. 190.

2. *Astronomical Observatory in the Sierra Nevada.*—Mr. James Lick, of San Francisco, already renowned for his munificent gift to the California Academy of Sciences, has recently devoted to public purposes $2,000,000 ; and of this sum $700,000 are set aside for the equipment of an Astronomical Observatory on some elevated point in the Sierra Nevada. Land on the borders of Lake Tahoe has been given by him. But in case this site is not thought the best, the trustees, in conjunction with Messrs. Alvan Clark & Sons, the well-known opticians, are to appoint a person who shall determine on a site. The $700,000 are to be applied to the making of a telescope larger and better than any now in existence; and the surplus left, after paying for the telescope, is to be invested for the maintenance of the observatory, or, " be made useful in promoting science."

3. *Cordoba Observatory.*—The report of Dr. Gould of the work done at this observatory during the past year is received. From this we learn that at the date of the report, 538 zones had been observed, comprising over 70,000 stars. He estimates that about 115 zones more will cover the southern heavens according to the plan which he has adopted for this catalogue. Rejecting duplicates, he estimates the whole number of independent stars in it as likely to exceed 65,000. He states that the middle of the present year may see the zone observations completed.

4. *Coggia's Comet.*—Dr. Tietjen gives the elements and an ephemeris of the Coggia comet in the *Astronomische Nachrichten*, No. 1993. The following are the places:

Berlin Time.	α	δ	Light-factor.
July 4.	7^h $36{\cdot}6^m$	$+63°$ $1'$	40
7.	40·3	58 36	58
11.	42·9	51 25	84
15.	44·7	37 27	128
19.	46·3	+23 6	150
23.	48·0	− 0 5	149
27.	50·1	−16 25	112
Aug. 4.	55·2	−39 55	45

These elements bring the comet within about 25,000,000 miles of us in the fourth week in July, with a brilliancy ten-fold that which it has in the middle of June. It will remain below Ursa Major till near the middle of July, when it will pass rapidly south, a little west of the sun. It will be almost directly between us and the sun July 20th.

5. *New Planet.*—A new planet was discovered by Mr. Perrotin at Toulouse on the 19th of May. It was then of the 11·5 magnitude.

IV. Miscellaneous Scientific Intelligence.

1. *The American Museum of Natural History, New York.*—The corner-stone of the first section of this institution, in the Cenf tral Park of the city of New York, was laid by the President of the United States, on the 2d of June, in the presence of a con-

course of distinguished people from various parts of the country. On behalf of the museum, Mr. Robert L. Stuart, the President of the Commission, briefly recounted its history, its incorporation in 1869 by the Legislature of New York, the award of Manhattan square, adjacent to the Park, for its use and that of the Metropolitan Art Museum, with the donation of five hundred thousand dollars to each for the commencement of the buildings. The plans, of which the present portion form only the commencement, contemplate an expenditure of $6,000,000. Addresses were made also by Messrs. Wales and Stebbins on behalf of the Park Commissioners, and by General Dix as Governor of the State, all fully adopting and commending the undertaking.

Professor Joseph Henry, Secretary of the Smithsonian Institution, made the concluding address, setting forth the true aims and purposes of such a museum as a means of diffusing a knowledge of science, and, by proper instructors and endowments, fostering a spirit of scientific research and promoting the general advancement of science. Under a wise administration of its affairs, the American Museum of Natural History will not fail, for the want of means, to carry out its proper purposes, and will thus become a source of delight and of advancement to the great community about it, and an honor to American science in the eyes of the civilized world.

B. S.

21. *Note on the Recent Earthquakes of Bald Mountain, in Rutherford County, North Carolina;* by Professor F. H. Bradley.—So far as *direct observation* upon the disturbances there in progress was concerned, my trip to the Bald Mountain region was a failure, for there was not the least disturbance during our visit. But from an examination of the present condition of the mountain, on both sides and at the top, we were able to come to an understanding of the phenomena reported to us by residents of the region. And here I would remark that the reports heretofore published have grown rapidly in proportion to their distance from home, there having been, apparently, *no foundation* for the stories of yawning crevices and smoking pits. The phenomena actually observed seem to have consisted simply of noises within the mountains, more or less loud, and shakings of the surface.

The sounds are variously described as resembling continuous musket firing, or heavy cannonading, or "the rumble of a heavy iron-axled wagon rolling rapidly over a rocky road." The shakings were wave-like vibrations of the surface, moving laterally from the center of disturbance, and causing cracks in walls and chimneys, and occasionally throwing down loose articles.

These are the common phenomena of earthquakes, but they were here much more local and less violent than usual. There was nothing properly volcanic about them; and the region shows no volcanic rock. Bald Mountain forms one of the southeastern spurs of the easternmost range of the Blue Ridge proper, and consists of micaceous and hornblendic schists and gneisses, having the variable southeasterly dip characteristic of the whole range.

2. *Fluctuations in the Great Lakes.*—In the number of *Nature* for April 30th, Mr. G. M. Dawson endeavors to show, by a comparison of observations, that there is probably a connection between the fluctuations of the American Lakes and the development of sun-spots. More extended observations are required to give probability to the conclusion.

3. *Chemical Centennial, August 1st,* 1874.—Northumberland, in Pennsylvania, on the Susquehannah, the place where Dr. Priestley was buried, has been selected as the spot at which all chemists are invited to gather on the first of August next, the hundreth anniversary of the discovery of oxygen by the illustrious philosopher. An address is to be delivered over his grave. This proposition of Dr. Bolton, to which we have referred already, has met with a cordial response from a large number of chemists. Professor Henry, of the Smithsonian, proposes to be present with some of the original apparatus of Priestley from the Smithsonian collections. The first of August falling on Saturday, the meeting will be called for the day previous. A programme will be soon issued by the committee in charge.

4. *Topographical Atlas, projected to illustrate explorations of surveys west of the* 100*th meridian;* under the direction of Hon. Wm. W. BELKNAP, Secretary of War, by the Corps of Engineers, U. S. Army, Brigadier-General A. A. HUMPHREYS, Chief of Engineers: embracing results of the different expeditions under 1st Lieut. GEO. M. WHEELER, Corps of Engineers.—The expeditions referred to in this title are those of 1869, 1871, 1872 and 1873. The maps here issued, six in number, are in large folio, and handsomely printed. The first gives in colors a general view of the drainage areas of the country west of the Mississippi; the second, a subdivision of the region west of the 100th meridian to the Pacific, into equal areas, 94 in all, for each of which a separate map is projected; and the remaining four are maps number 50, 58, 59, 66, of the Atlas. On these maps the topography is represented with much detail and clearness. The completed atlas will be a very important contribution to American geography. Lieutenant Wheeler takes the field this season for the continuation of his surveys.

5. *Annual Record of Science and Industry for* 1873. Edited by SPENCER F. BAIRD, with the assistance of eminent men of science. 12mo, pp. 714. New York (Harper & Brothers), 1874. —This is the third volume of Prof. Baird's Annual. A general summary of scientific and industrial progress during the year 1873 occupies an introductory chapter of 132 pages, covering a wide range of subjects from astronomy to technology. The whole volume is rich in varied and useful facts.

6. *Mountain Sculpture in the Sierra Nevada.*—The article on Mountain Sculpture, cited from in the preceding volume, at page 515, (for a copy of which we are indebted to Prof. E. S. Carr,) was written, as Prof. Carr has recently informed us, by Mr. John Muir.

Critical Essays on Physics, Metaphysics and Ethics, by Lawrence S. Benson. Vol. I. Physics. 164 pp. 8vo. New York, 1874. (James S. Burnton.)

The Psychology of Scepticism and Phenomenalism, by James Andrews. 60 pp 12mo. Glasgow, 1874. (James Maclehose.)

CHART SHOWING THE PROGRESS OF THE STORM OF SEPT 19-21 1872 PLATE I.

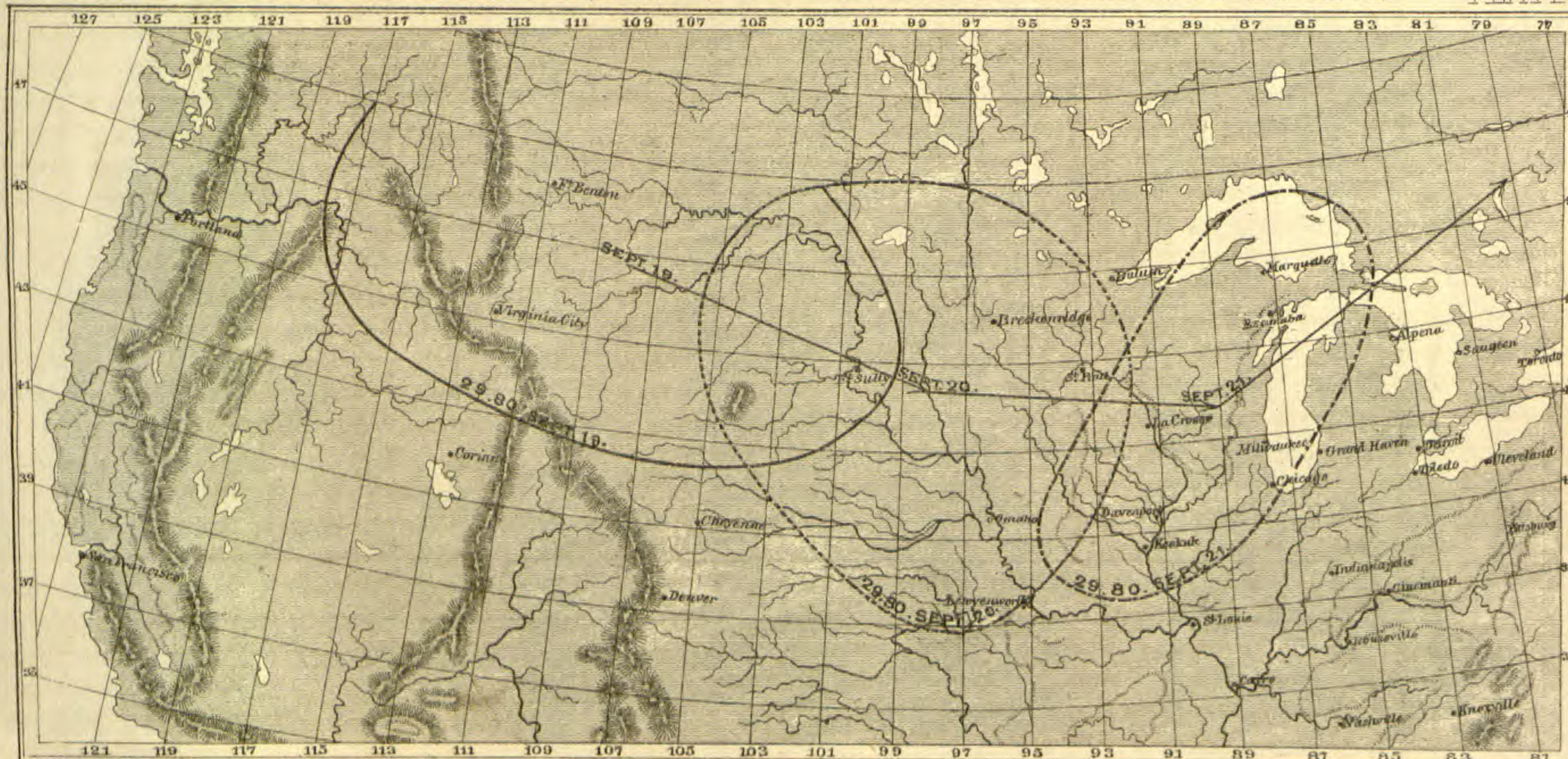


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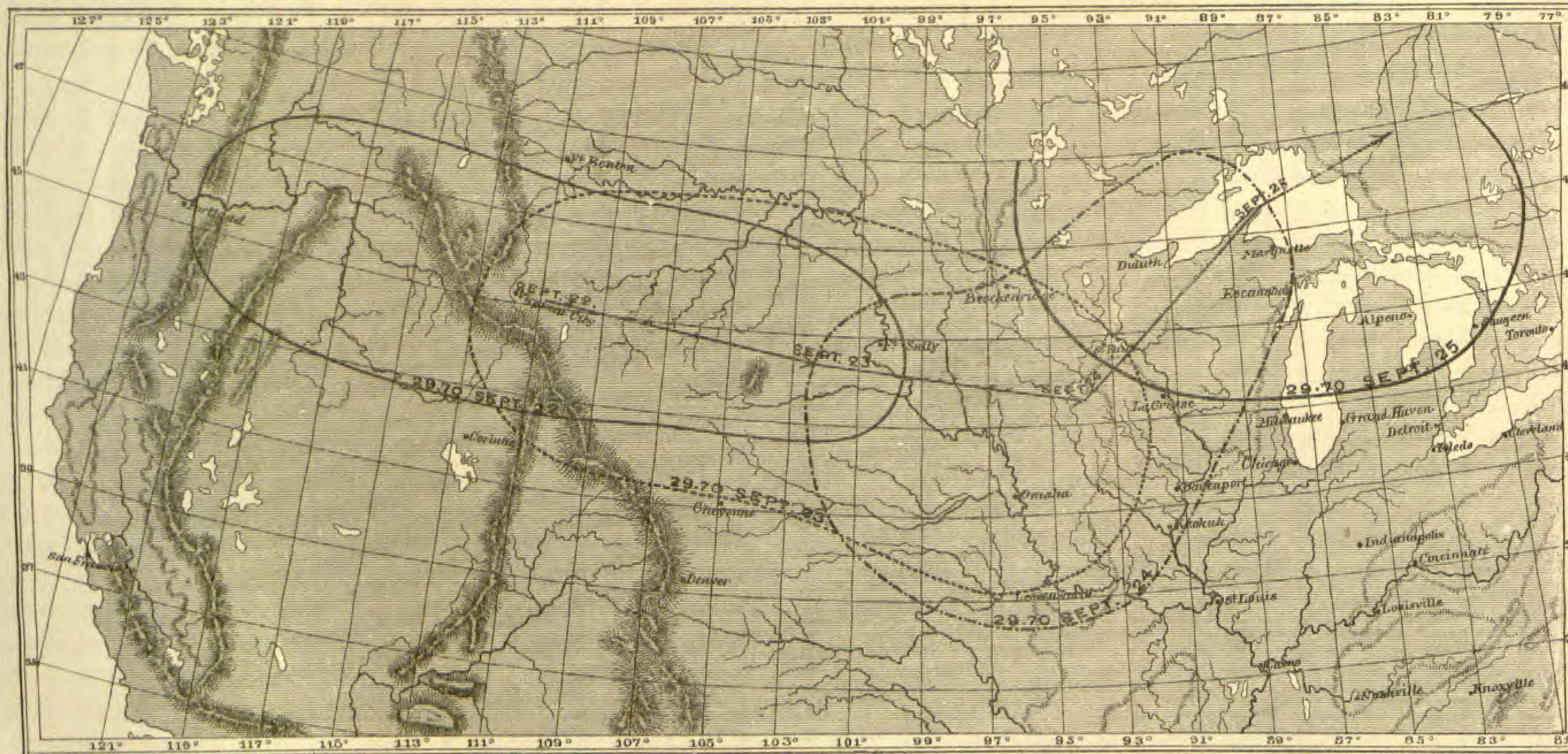
CHART SHOWING THE PROGRESS OF THE STORM OF SEPT. 22-25 1872. PLATE II.



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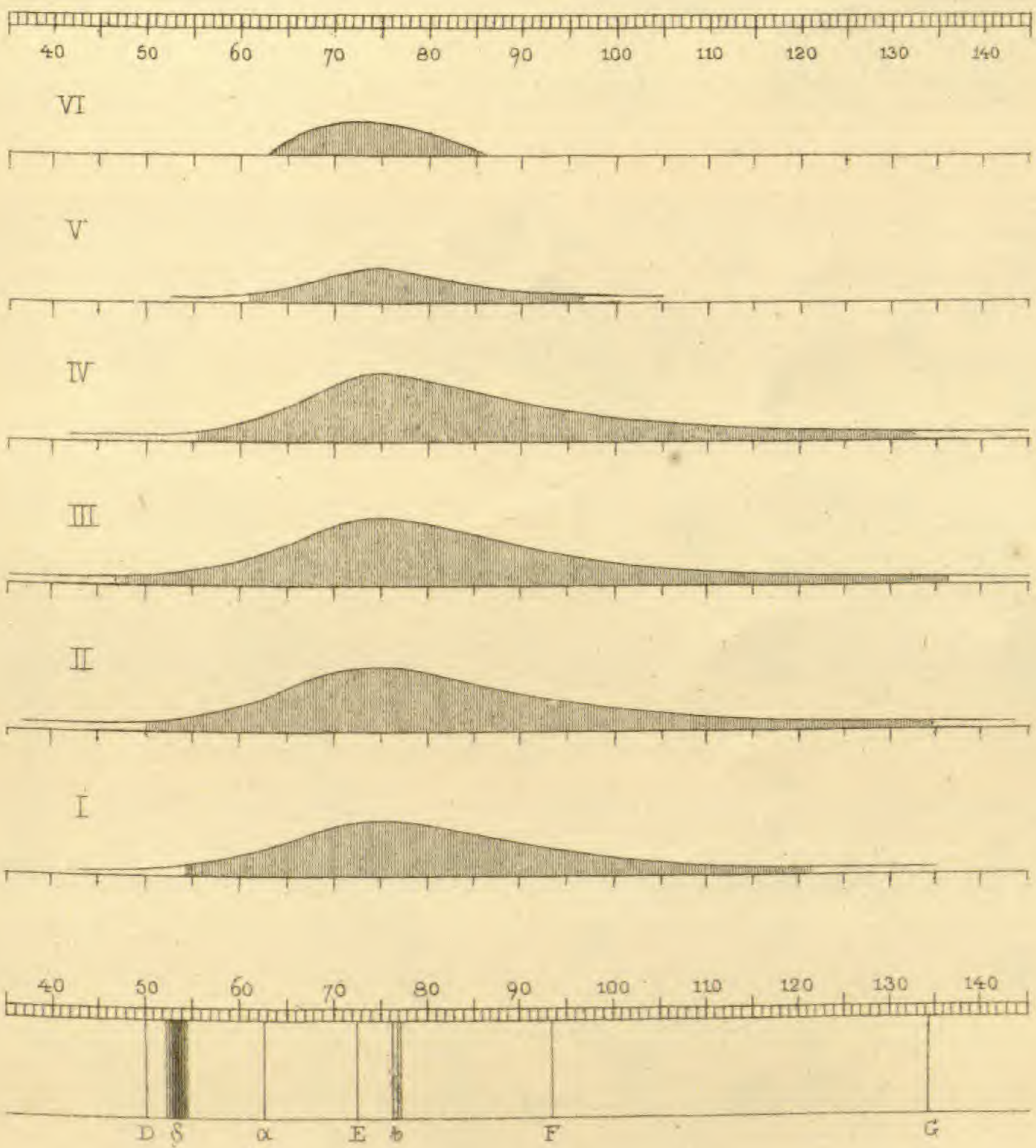

A. W. W. Del.

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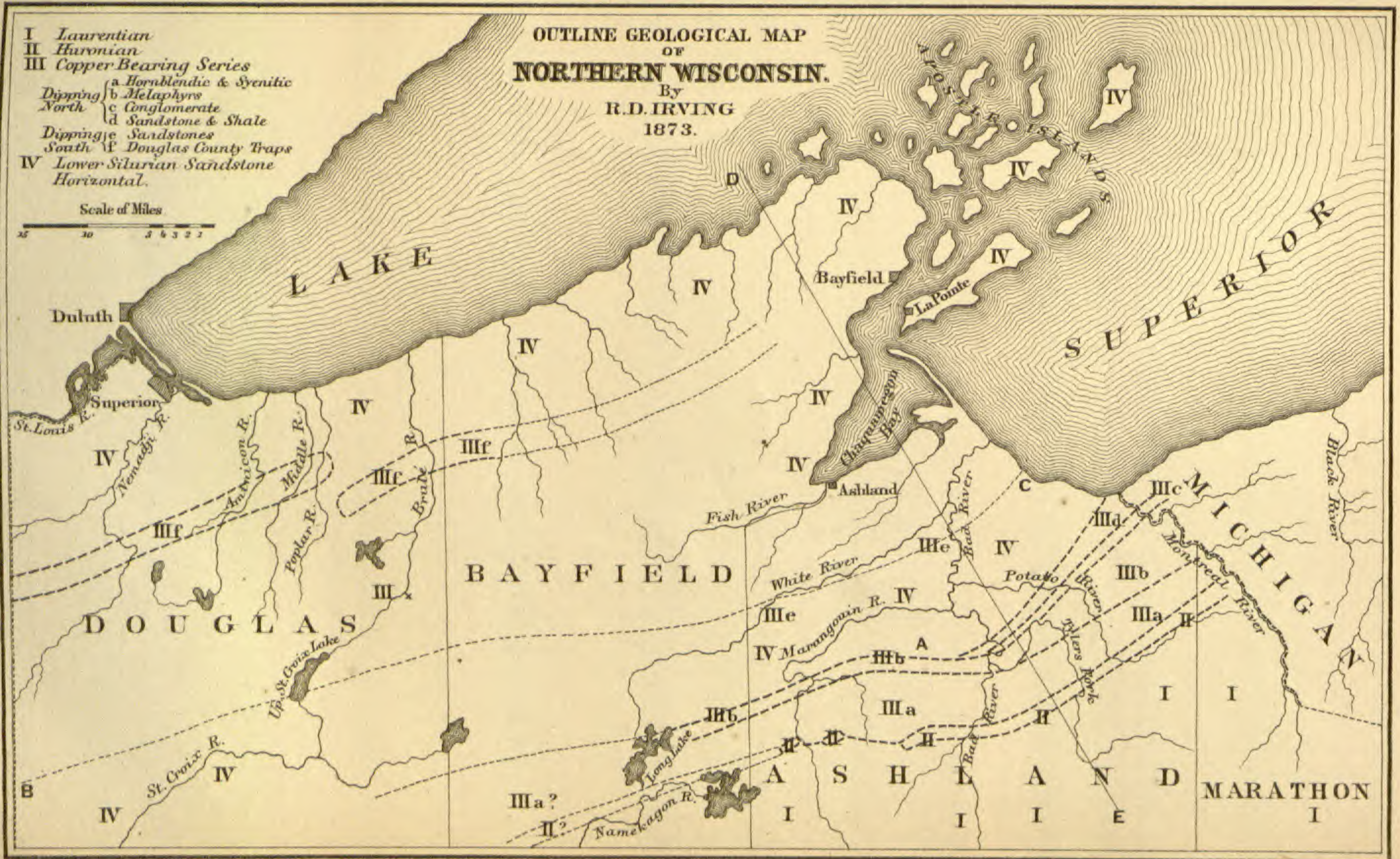
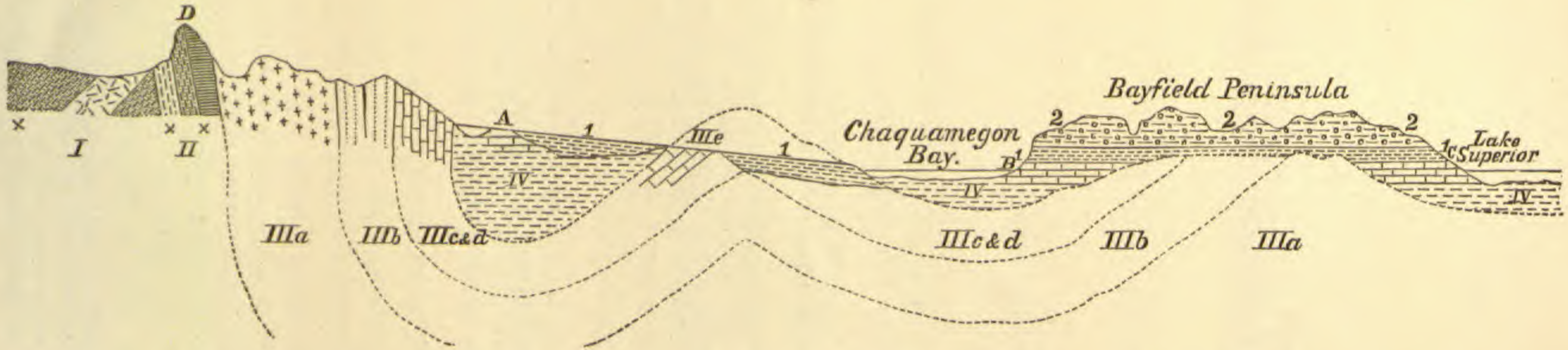
SPECTRUM OF THE ZODIACAL LIGHT.


OUTLINE GEOLOGICAL MAP
OF
NORTHERN WISCONSIN.
By
R.D. IRVING
1873.
I Laurentian
II Huronian
III Copper Bearing Series
Dipping North
a Hornblendic & Syenitic
b Melaphyrs
c Conglomerate
d Sandstone & Shale
Dipping South
e Sandstones
f Douglas County Traps
IV Lower Silurian Sandstone Horizontal.
Scale of Miles
LAKE
SUPERIOR
APOSTLE ISLANDS
Duluth
Superior
St. Louis R.
Nemadji R.
Amnicon R.
Middle R.
Poplar R.
Brule R.
Bayfield
La Pointe
Chaquamegon Bay
Ashland
Fish River
White River
Bad River
Potato River
Montreal River
Black River
Marangoin R.
Tylers Fork
Up. St. Croix Lake
St. Croix R.
Long Lake
Namekagon R.
DOUGLAS
BAYFIELD
ASHLAND
MARATHON
MICHIGAN
A
B
C
D
E



SECTION, TO ILLUSTRATE ARTICLE VII, BY R. IRVING, ALONG LINE DE OF MAP (PLATE IV). Full lined and dark portions from actual observation. Dotted lines ideal. A, B, C, horizontal Silurian sandstone; D, Penokie Iron Range. 1, Red marly clays; 2, Boulder Drift. For further explanation, see map.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. IX.—*Researches in Acoustics;* by ALFRED M. MAYER. Paper No. 5,* containing:

1. An Experimental Confirmation of Fourier's Theorem as applied to the decomposition of the vibrations of a composite sonorous wave into its elementary pendulum-vibrations.
2. An Experimental Illustration of Helmholtz's Hypothesis of Audition.
3. Experiments on the supposed Auditory Apparatus of the Culex Mosquito.
4. Suggestions as to the function of the spiral scalæ of the Cochlea, leading to an Hypothesis of the Mechanism of Audition.
5. Seven Experimental Methods of Sonorous Analysis described and discussed.
6. The Curve of a Musical Note, formed by combining the sinusoids of its first six harmonics; and the curves formed by combining the curves corresponding to various consonant intervals.
7. Experiments in which are produced from the above (sec. 6) curves the Motions of a Molecule of Air when it is animated with the resultant action of the six elementary vibrations forming a musical note; or is set in motion by the combined action of sonorous vibrations forming various consonant intervals.

1. *An Experimental Confirmation of Fourier's Theorem as applied to the decomposition of the vibrations of a composite sonorous wave into its elementary pendulum-vibrations.*

A simple sound is a sound which has only one pitch. Such a sound is produced when, with a bow, we gently vibrate the prongs of a tuning-fork and bring them near a cavity which resounds to the fork's fundamental tone. An almost pure simple sound can be obtained by softly blowing a closed organ-pipe. On examining the nature of the vibratory motions of the

* This paper is the fifth, in the series of those on Acoustics, already published in this Journal. The preceding papers, however, were not numbered.

Sections 1, 2, 3, 5, 6 and 7 of this paper were read before the National Academy of Sciences during the session of November, 1873. Section 4 was read before the Academy on April 21, 1874.

prongs of the fork* and of the molecules of air in the resounding cavity† and in the closed organ-pipe,‡ we find that each of these vibrations follows the same law of reciprocating motion as governs the vibrations of a freely-swinging pendulum. But other bodies, for instance, the free-reeds of organ-pipes and of melodeons,§ vibrate like the pendulum, yet we can decompose the vibrations they produce in the air into many separate pendulum-vibrations, each of which produces in the ear a simple sound of a definite pitch; thus, we see that a pendulum-vibrating body, when placed in certain relations to the air on which it acts, may give rise to highly composite sounds. It is, therefore, evident that we cannot always decide as to the simple or composite character of a vibration reaching the ear solely from the determination of the motion of the body originating the sound, but we are obliged to investigate the character of the molecular motions of the air near the ear, or of the motion of a point on the drum of the ear itself, in order to draw conclusions as to the simple or composite character of the sensation which may be produced by any given vibratory motion. Although we cannot often detect in the ascertained form of an aerial vibration all the elementary pendulum-vibrations, and thus predetermine the composite sensation connected with it; yet, if we find that the aerial vibration is that of a simple pendulum, we may surely decide that we will receive from it only

* In my course of lectures on Acoustics, I thus show to my students that the prong of a tuning-fork vibrates like a pendulum. I take two of Lissajous' reflecting forks, giving, say, the major third interval, and with them I obtain on a screen the curve of this interval in electric light. On a glass plate I have photographed the above curve of the major third passing through a set of rectangular coördinates formed of the sines of two circles whose circumferences are respectively divided into 20 and 25 equal parts. I now place this plate over the condensing-lens of a vertical lantern and obtain on the screen the curve, the circles and their net of coördinates. Suspended over the lantern is a Blackburn's compound pendulum, which is so constructed that its "bob" cannot rotate around its axis. The bob is hollow and a curved pipe leads from its bottom to one side of the pendulum. The pendulum is now deflected into a plane at 45° with its two rectangular planes of vibration so that the end of the curved pipe coincides with the beginning of the curve over the lantern. The bob of the pendulum is fastened with a fine cord in this position and fine hour-glass sand is poured into it; the cord is now burned and the sand is delivered from the pipe, as the swinging pendulum gives the resultant of its motions in the two planes of vibration, while the photographed curve on the lantern is progressively covered with the sand if the tines of the two vibrations of the pendulum are to each other as 4 to 5.

† Helmholtz, Tonempfindungen, 1857, p. 75. Crelle's Jour. für Math. Bd. lvii.

‡ See Mach's Optisch-Akustische Versuche, Prag., 1873, p. 91. Die Stroboskopische Darstellung der Luftschwingungen.

§ The Rev. S. B. Dod, one of the trustees of the Stevens Institute, has recently made an experiment which neatly shows this. He silvered the tips of two melodeon reeds and then vibrated them in planes at right angles to each other, while a beam of light was reflected from them. The resultant figure of their vibrations is the same as that obtained by two Lissajous' forks placed in the same circumstances and having the same musical interval between them as that existing between the reeds.

the sensation of a simple sound. Thus, if we arm the prong of a tuning-fork with a point, and draw this point on a lamp-blackened surface with a uniform motion, and in a direction parallel to the axis of the fork, we shall obtain on the surface a sinusoidal or harmonic curve;* and this curve can only be produced by the prongs of the fork vibrating with the same kind of motion as that of a freely-swinging pendulum. If we now bring this vibrating fork near the mouth of a glass vessel whose mass of air responds to the tone of the fork and, by the method of Mach, examine the vibratory-motions of the air, we shall see it swinging backward and forward; and by combining these vibrations with the rectangular vibrations of forks placed outside of the vessel we shall obtain the curves of Lissajous. If the membrane of the drum of the ear be placed in connection with the resounding cavity, it must necessarily partake of the motion of the air which touches it, and ultimately the auditory nerve fibrillæ are shaken in the same manner, and we receive the sensation† of a simple sound. Here the mind naturally inquires the reason of this connection existing between the sensation of a simple sound and the pendulum-vibration. It has always appeared to me that the explanation of this invariable connection is that the pendulum-vibration is the simplest vibratory motion that the molecules of elastic matter can partake of, and that the connection of the sensation with the mode of vibration is the connection between the simplest sensation perceived through the intervention of the trembling nerves, and the simplest vibration which they can experience. Indeed, the pendulum-vibration is the only one which produces the sensation of sound, for if any other recurring vibration enters the ear it is decomposed by the ear into its elementary pendulum-vibrations, and if it cannot be so decomposed then the given vibration is not recurring and does not produce in us the sensation of sound, but causes that which we denominate as noise. This remarkable connection between a simple sound and the pendulum, or harmonic, vibration, together with the fact of the power of the ear to decompose the motions of a composite sonorous wave into its vibratory elements, was thus distinctly enunciated by Ohm. *The ear has the sensation of a simple sound only when it receives a pendulum-vibration, and it decomposes any other periodic motion of the air into a series of pendulum-vibrations, each of which corresponds to the sensation of a simple sound.*

* The equation of this curve is $y=a \sin\left(\frac{2\pi x}{\lambda} + \alpha\right)$. The length on the axis of one recurring period of the curve is λ; the constant a is the maximum ordinate, or amplitude. The form of the curve is not affected by α, but any change in its value slides the whole curve along the axis of x. It is interesting to observe that this curve expresses the annual variation of temperature in the temperate zones.

† See Helmholtz on the distinction between a sensation and a perception. Tonempfindungen, p. 101.

We have seen that the harmonic curve is the curve which corresponds to the motion which causes the sensation of a simple sound, but a molecule of vibrating air or a point on the tympanic membrane may be actuated by vibratory motions, which, when projected on a surface moving near them, will develop curves which depart greatly from the simplicity of the harmonic, or curve of sines;* but nevertheless these curves will alway be periodic if the sensation corresponding to their generating motions is that of sound. Now Fourier has shown, and states in his theorem, that any periodic curve can always be reproduced by corresponding harmonic curves (often infinite in number) having the same axis as the given curve and having the lengths of their recurring periods as 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., of the given curve; and the only limitation to its irregularity is that its ordinates must be finite, and that the projection on the axis, of a point moving in the curve, must always progress in the same direction. Fourier demonstrates that the given curve can only be reproduced by one special combination, and shows that by means of definite integrals one can assign the definite sinusoids with their amplitudes and differences of phase. Now Helmholtz † has shown that differences of phase in the constituent elementary sounds do not alter the character of the composite sound, and therefore, that although the forms of the curve corresponding to one and the same composite sound may be infinite in variety (by reason of differences in phase and amplitude in the component curves), yet the composite sound is always resolved into the same elements. This experimental result of Helmholtz also conforms to the theorem of Fourier in reference to the curves projected by such motions; for he has shown that only one series of sinusoidal resolution is possible.

Fourier's theorem can be expressed as follows: The constants C, C_1, C_2, &c., and a_1, a_2, &c., can be determined so that a period of the curve can be defined by the following equation:‡

$$y = C + C_1 \sin\left(\frac{2\pi x}{\lambda} + a_1\right) + C_2 \sin\left(2\frac{2\pi x}{\lambda} + a_2\right) + \cdots$$

But Fourier's theorem is the statement of a mathematical possibility, and it does not necessarily follow that it can be immediately translated into the language of dynamics without experimental confirmation, for, as Helmholtz remarks, "That mode of decomposition of vibratory forms, such as the theorem of Fourier describes and renders possible—is it only a

* In section 5 of this paper, I have constructed several important curves corresponding to composite vibrations.

† Tonempfindungen, p. 190 *et seq.*

‡ For other and more convenient forms of expression of this theorem, as well as for a demonstration of it, see pp. 52 and 60 of Donkin's Acoustics—the most admirable work ever written on the mathematical theory of sound.

mathematical fiction, admirable because it renders computation facile, but not corresponding necessarily to anything in reality? Why consider the pendulum-vibration as the irreducible element of all vibratory motion? We can imagine a whole, divided in a multitude of different ways; in a calculation we may find it convenient to replace the number 12 by 8+4, in order to bring 8 into view; but it does not necessarily follow that 12 should always and necessarily be considered as the sum of 8+4. In other cases it may be more advantageous to consider the number as the sum of 7+5."

The mathematical possibility, established by Fourier, of decomposing any sonorous motion into simple vibrations, cannot authorize us to conclude that this is the only admissible mode of decomposition, if we cannot prove that it has a signification essentially real. The fact, that the ear effects that decomposition, induces one, nevertheless, to believe that this analysis has a signification, independent of all hypothesis, in the exterior world. This opinion is also confirmed precisely by the fact stated above, that this mode of decomposition is more advantageous than any other in mathematical researches. For the methods of demonstration which comport with the intimate nature of things, are naturally those which lead to theoretic results the most convenient and the most clear."

The theorem of Fourier translated into the language of dynamics would read as follows: "*Every periodic vibratory motion can always, and always in one manner, be regarded as the sum of a certain number of pendulum-vibrations.*"

Now we have seen that any periodic vibratory motion, which has the proper velocity, will cause the sensation of a musical note, and that a pendulum-vibration gives the sensation of a simple sound;* therefore, if Fourier's theorem is applicable to the composition and decomposition of a composite sonorous wave, it will be thus related to the phenomena of sound: "*Every vibratory motion in the auditory canal, corresponding to a musical sound, can always, and always in one manner, be considered as the sum of a certain number of pendulum-vibrations, corresponding to the elementary sounds of the given musical note.*"

Heretofore we have called in the aid of the sensations,—assumed to be received through the motions of the co-vibrating

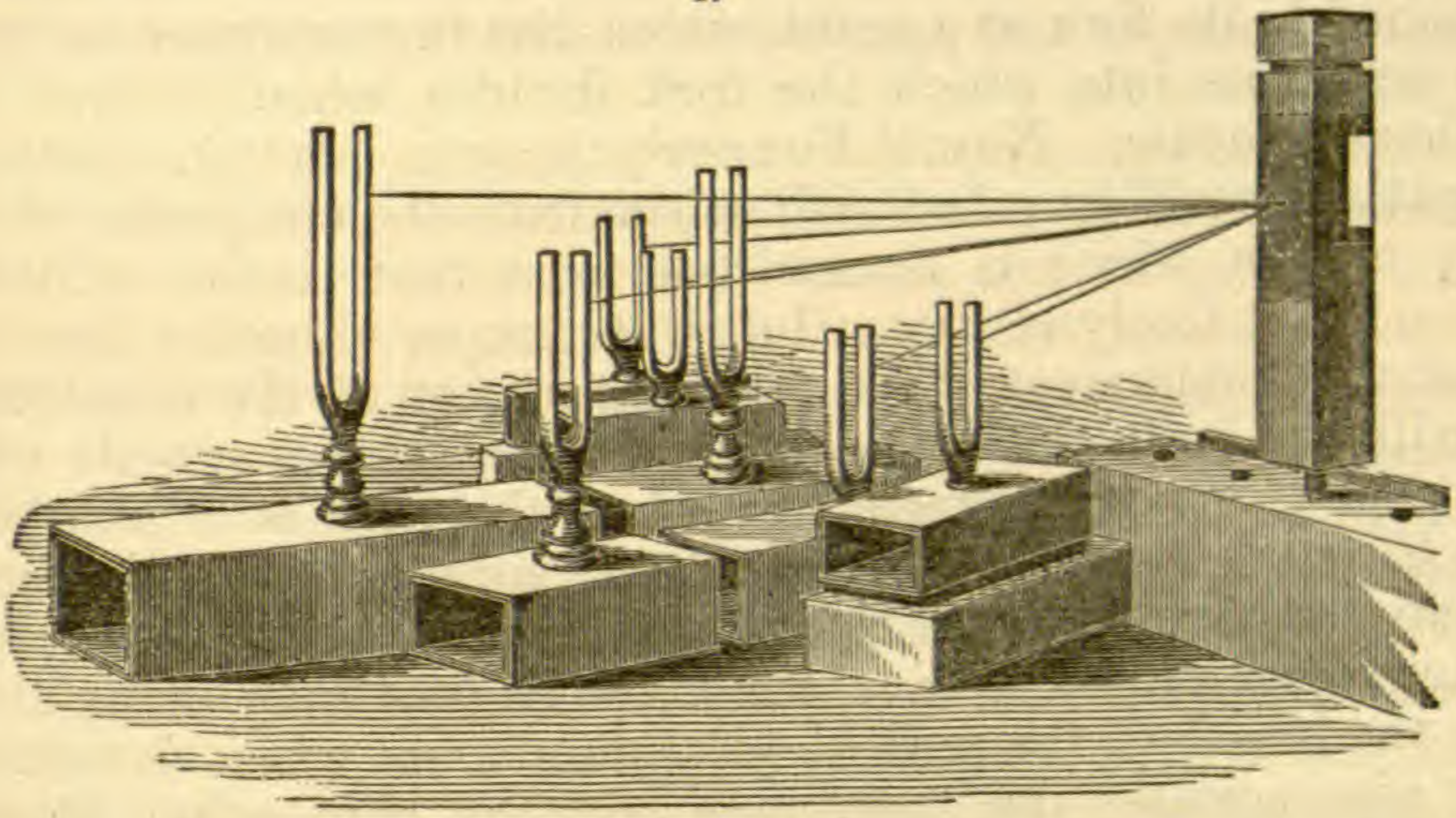
* Professor Donkin, in his Acoustics, Oxford, 1870, p. 11, advises the use of *tone* to designate a simple sound, and the word *note* to distinguish a composite sound. His reasons are "that *tone* (Gr. τόνος) really means *tension*, and the effect of tension is to determine the *pitch* of the sound of a string;" while a musical *note* is generally a composite sound. Professor Donkin further states: "Helmholtz uses the words *klang* and *ton* to signify compound and simple musical sounds. We have followed him in adopting the latter term. But such a sound as that of the human voice could hardly in English be called a *clang*, without doing too much violence to established usage."

parts of the ear,—to help us in our determination of the simple or composite character of a given vibratory motion; but Fourier's theorem does not refer to the subjective effects on the organ of hearing,—the dynamic function of whose parts are yet very imperfectly understood. Ohm's theorem, on the other hand, refers entirely to these subjective phenomena of the ear's analysis of a complex sensation into its simple elements. As Fourier's theorem refers only to the decomposition of a composite recurring vibration into its elementary pendulum-vibrations, it has nothing to do with the physiological fact of the co-relation of the pendulum-vibration and the simplest auditory sensation; though this well ascertained relation gives us the privilege of using this sensation as an indicator of the existence of an aerial pendulum-vibration. Hence, as Fourier's theorem is entirely independent of our sensations, we must endeavor to verify it directly by experiments, which must perform *the actual* decomposition of the composite periodic motion of *a point* into its elementary pendulum-vibrations. But many difficulties present themselves when we would bring to the test of experiment the dynamic signification of Fourier's theorem. For example, the composite sound-vibration, on which we would experiment, emanates from a multitude of vibrating points; parts of the resultant wave surface differ in their amplitudes of vibration; while points equally removed from one and the same point of the body originating the vibrations, may differ in their phases of vibration; so that when such a wave falls upon co-vibrating bodies which present any surface, the effects produced are the results of extremely complex motions. The mind sees at once the difference between this complicated conception and the simple one embodied in the statement of the dynamic application of Fourier's theorem.

As the mathematician decomposes *seriatim* every point of the recurring curve into its harmonic elements, so the physicist, in confirming the dynamic application of Fourier's theorem, should decompose into its simple pendulum-vibrations the composite vibratory motion which such a curve represents, and indeed *reproduces* when it is drawn with a uniform motion under a slit in a diaphragm which exposes to view only a point of the curve at once. Therefore, only one vibrating point of the composite sonorous wave should be experimented on, and the composite vibratory motion of this point should be conveyed along lines to points of elastic bodies which can only partake of simple pendulum-vibrations. All of these essential conditions I have succeeding in securing in the following arrangement of apparatus.

A loose inelastic membrane—(thin morocco-leather does well)—was mounted in a frame and placed near a reed-pipe; or, as in other experiments, the membrane was placed over an opening in the front of the wooden chamber of a Grenié's free-reed pipe. The ends of several fine fibers from a silk-worm's cocoon were brought neatly together and cemented to one and the same point of the membrane, while the other ends of these fibers were attached to tuning-forks mounted on their resonant boxes, as shown in fig 1. In the experiment which I shall now

1.



describe, eight forks were thus connected with one point of the membrane. The fundamental tone of the pipe was Ut_2, of 128 vibrations per second; and the pipe was brought into accurate unison with a fork giving this sound.* The forks connected with the membrane were the harmonic series of Ut_2, Ut_3, Sol_3, Ut_4, Mi_4, Sol_4, $B^{\flat}_4$–, Ut_5. In the first stage of the experiment we will suppose that the fibers are but slightly stretched; then, on sounding the pipe, all the fibers at once break up into exquisite combinations of ventral segments. If the sunshine fall upon a vibrating fiber and we look on it obliquely, in the direction of its length, we shall see ventral segments superimposed on ventral segment in beautiful and changing combinations. On gradually tightening the fibers, we diminish the number of their nodes, and on reaching a certain degree of tension with fibers 1 m. long, I have seen them all vibrating with single ventral segments. On increasing the tension, the amplitudes of these single segments gradually diminish and at last disappear entirely, so far as the unaided eye can discern, and then we have reached the conditions required in our experimental confirmation.

* Since the number of beats per second given by any harmonic (of a pipe out of tune with its harmonic series of forks) will be as the order of the harmonic, it is better to tune a reed to unison with a fork giving one of its higher harmonics. I generally used the Sol_3 fork, or the 3d harmonic.

The point of the membrane to which the fibers are attached is actuated by a motion which is the resultant of all of the elementary pendulum-vibrations existing in the composite sonorous wave, and the composite vibrations of this point are sent through each of the fibers to its respective fork. Thus, each fiber transmits to its fork the same composite vibratory motion, while each fork can only vibrate so as to give the simple pendulum-vibration of a simple sound, for each fiber is attached to its fork at a point which lies in the upper node of the segments into which the fork divides when it gives its higher harmonic. Now if Fourier's theorem has "an existence essentially real," any fork will select from the composite vibratory motion, which is transmitted to it, that motion which it has when it freely vibrates; but if its proper vibration does not exist as a component of the resultant motion of the membrane, it will not be in the least affected. Now this is exactly what happens in our experiment, for when the pipe is in tune with the harmonic series of forks, the latter sing out when the membrane is vibrated; but if the forks be even slightly thrown out of tune with the membrane, either by loading them, or by altering the length of the reed, they remain silent when the sounding pipe agitates the membrane and the connecting fibers.* Thus have I shown that the dynamic application of Fourier's theorem has "an existence essentially real."

It is indeed very interesting and instructive thus to observe in one experiment the analysis and synthesis of a composite sound. On sounding the reed it sets in vibration all the forks of the harmonic series of its fundamental note, and after the reed has ceased to sound, the forks continue to vibrate and their elementary simple sounds blend into a note which approximately reproduces the timbre of the reed-pipe. If we could by any means obtain all of the elementary vibrations, and have them with their relative intensities correctly preserved, we should have an echo of the sound of the reed after the latter had ceased to vibrate; but the impossibility of thus obtaining the highest components of the reed, and the difficulty of reproducing the relative intensities of the harmonics in the co-vibrating forks, allow us but partially to accomplish this effect.

2. *An Experimental Illustration of Helmholtz's Hypothesis of Audition.*

The experiment, which we have just described, beautifully illustrates the hypothesis of audition framed by Helmholtz to account for this—among other facts,—that the ear can decompose a composite sound into its sonorous elements. Helmholtz

* See section 4 of this paper for an account of the degree of precision of this method of sonorous analysis.

founds his hypothesis on the supposition that the rods of Corti, in the ductus cochlearis, are bodies which co-vibrate to simple sounds; somewhat, I imagine, as loaded strings* of graded lengths and diameters would act in similar circumstances. The vibrations of the composite wave fall upon the membrane placed near the reed as they fall upon the membrane of the tympanum; and these vibrations are sent through the stretched fibers, (or delicate splints of rye-straw, which I have sometimes used,) from the membrane to the tuned forks, as they are sent from the membrana tympani through the ossicles and fluids of the ear to the rods of Corti. The composite vibration is decomposed into its vibratory elements by the co-vibration of those forks whose vibratory periods exist as elements of the composite wave motion; so the composite sound is decomposed into its sonorous elements by the co-vibrations of the rods of Corti, which are tuned to the elementary sounds which exist in the composite sonorous vibration. The analogy can be carried yet further by placing the forks in line and in order of ascending pitch, and attaching to each fork a sharply-pointed steel filament. If the arm be now stretched near the forks, so that the points of the filaments nearly touch it at points along its length, then any fork will indicate its co-vibration by the fact of its pricking the skin of the arm, and the localization of this pricking will tell us which of the series of forks entered into vibration. The rods of Corti shake the nerve filaments attached to them, and thus specialize the position in the musical scale of the elements of a composite sonorous vibration. Thus a complete analogy is brought into view between our experiment and Helmholtz's comprehensive hypothesis of the mode of audition.

3. *Experiments on the supposed Auditory Apparatus of the Culex Mosquito.*

Ohm states in his proposition that the ear experiences a simple sound only when it receives a pendulum-vibration, and that it decomposes any other periodic motion of the air into a series of pendulum-vibrations, to each of which corresponds the sensation of a simple sound. Helmholtz, fully persuaded of the truth of this proposition, and seeing its intimate connection with the theorem of Fourier, reasoned that there must be a cause for it in the very dynamic constitution of the ear; and the previous discovery by the Marquis of Corti of several thousand† rods of graded sizes in the *ductus cochlearis* indicated

* For discussions of the vibratory phenomena of loaded strings, see Donkin's Acoustics, p. 139; and Helmholtz's Tonempfindungen, p. 267.

† "But all of the propositions on which we have based the theory of consonance and dissonance rests solely on a minute analysis of the sensations of the ear. This analysis could have been made by any cultivated ear, without the aid of

to Helmholtz that these were suitable bodies to effect the decomposition of a composite sonorous wave by their co-vibrating with its simple harmonic elements. This supposed function of the Corti organ gave a rational explanation of the theorem of Ohm, and furnished "a leading thread" which conducted Helmholtz to the discoveries contained in his renowned work, "Die Lehre von den Tonempfindungen."* In this book he first gave the true explanation of timbre, and revealed the hidden cause of musical harmony, which, since the days of Pythagoras, had remained a mystery to musicians and a problem to philosophers.

It may, perhaps, never be possible to bring Helmholtz's hypothesis of the mode of audition in the higher vertebrates to the test of direct observation, from the apparent hopelessness of ever being able to experiment on the functions of the parts of the inner ear of mammalia. The cochlea, tunneled in the hard temporal bone, is necessarily difficult to dissect, and even when a view is obtained of the organ of Corti, its parts are rarely *in situ;* and, moreover, they have already had their natural structure altered by the acid with which the bone has been saturated to render it soft enough for dissection and for the cutting of sections for the microscope.

As we descend in the scale of development, from the higher vertebrates, we observe the parts of the outer and middle ear disappearing, while at the same time we see the inner ear gradually advancing toward the surface of the head. The external ear, the auditory canal, the tympanic membrane, and with the latter the now useless ossicles, have disappeared in the lower vertebrates, and there remains but a rudimentary labyrinth.

Although the homological connections existing between the vertebrates and articulates, even when advocated by naturalists, are certainly admitted to be imperfect, yet we can hardly suppose that the organs of hearing in the articulates will remain stationary or retrograde, but rather that the essential parts of their apparatus of audition, and especially that part which receives the aerial vibrations, will be more exposed than in higher organisms. Indeed, the very minuteness of the greater part of the articulates would indicate this, for a tympanic membrane placed in vibratory communication with a modified

theory, but the leading-thread of theory, and the employment of appropriate means of observation, have facilitated it in an extraordinary degree.

"Above all things I beg the reader to remark that the hypothesis on the co-vibration of the organs of Corti has no immediate relation with the explanation of consonance and dissonance, which rests solely on the facts of observation, on the beats of harmonics and of resultant sounds."—Helmholtz, Tonempfindungen, p. 342.

* According to Waldeyer, there are 6,500 inner and 4,500 outer pillars in the organ of Corti.

labyrinth, or even an auditory capsule with an outer flexible covering, would be useless to the greater number of insects for several reasons; first, such an apparatus, unless occupying a large proportion of the volume of an insect, would not present surface enough for this kind of receptor of vibrations; and secondly, the minuteness of such a membrane would render it impossible to co-vibrate with those sounds which generally occur in nature, and which the insects themselves can produce; similarly, all non-aquatic vertebrates have an inner ear formed so as to bring the aerial vibrations, which strike the tympanic membrane, to bear with the greatest effect on the auditory nerve filaments,* and the minuteness of insects also precludes this condition. Finally, the hard test, characteristic of the articulates, sets aside the idea that they receive the aerial vibrations through the covering of their bodies, like fishes, whose bodies are generally not only larger and far more yielding, but are also immersed in water which transmits vibrations with 4¼ times the velocity of the same pulses in air and with a yet greater increase in intensity. For these reasons, I imagine that those articulates which are sensitive to sound, and also emit characteristic sounds, will prove to possess receptors of vibrations external to the general surface of their bodies, and that the proportions and situation of these organs will comport with the physical conditions necessary for them to receive and transmit vibrations to the interior ganglia.

Naturalists, in their surmises as to the positions and forms of the organ of hearing in insects, have rarely kept in view the important consideration of those physical relations which the organ must bear to the aerial vibrations producing sound, and which we have already pointed out. The mere descriptive anatomist of former years could be satisfied with his artistic faculty for the perception of form, but the student of these days can only make progress by constantly studying the close relations which necessarily exist between the minute structure of the organs of an animal and the forces which are acting in the animal, and which traverse the medium in which the animal lives. The want of appreciation of these relations, together with the fact that many naturalists are more desirous to describe many new forms than to ascertain the function of one well known form, which may exist in all animals of a class, has tended to keep many departments of natural history in the condition of mere descriptive science. Those who are not professed naturalists appreciate this perhaps more than the naturalists themselves, who are imbued with that enthusiasm which always comes with the earnest study of any one department of nature; for the perusal of those long and laboriously precise descriptions of forms of organs without the slightest

* See section 4 of this paper.

attempt, or even suggestion, as to their uses, affects a physicist with feelings analogous to those experienced by one who peruses a well classified catalogue descriptive of physical instruments, while of the uses of these instruments he is utterly ignorant.

The following views, taken from the "Anatomy of the Invertebrata by C. Th. v. Siebold," will show how various are the opinions of naturalists as to the location and form of the organs of hearing in the *Insecta.* "There is the same uncertainty concerning the organs of audition (as concerning the olfactory organs). Experience having long shown that most insects perceive sounds, this sense has been located sometimes in this and sometimes in that organ. But in their opinion, it often seems to have been forgotten, or unthought of, that there can be no auditory organ without a special auditory nerve, which connects directly with an acoustic apparatus capable of receiving, conducting and concentrating the sonorous undulations. (The author who has erred most widely in this respect is L. W. Clarke, in Mag. Nat. Hist., Sept., 1838, who has described at the base of the antennæ of *Carabus nemoralis* Illig. an auditive apparatus composed of an *Auricula*, a *Meatus auditorius externus* and *internus*, a *Tympanum* and *Labyrinthus*, of all of which there is not the least trace. The two white convex spots at the base of the antennæ of *Blatta orientalis*, and which Treviranus has described as auditory organs, are, as Burmeister has correctly stated, only rudimentary accessory eyes. Newport and Goureau think that the antennæ serve both as tactile and as auditory organs. But this view is inadmissible, as Erichson has already stated, except in the sense that the antennæ, like all solid bodies, may conduct sonorous vibrations of the air; but, even admitting this view, where is the auditory nerve? for it is not at all supposable that the antennal nerve can serve at the same time the function of two distinct senses.)

"Certain Orthoptera are the only Insecta with which there has been discovered, in these later times, a single organ having the conditions essential to an auditory apparatus. This organ consists, with the Acrididæ, of two fossæ or conchs, surrounded by a projecting horny ring, and at the base of which is attached a membrane resembling a tympanum. On the internal surface of this membrane are two horny processes, to which is attached an extremely delicate vesicle filled with a transparent fluid and representing a membranous labyrinth. This vesicle is in connection with an auditory nerve which arises from the third thoracic ganglion, forms a ganglion on the tympanum, and terminates in the immediate neighborhood of the labyrinth by a collection of cuneiform, staff-like bodies with very finely-pointed extremities (primitive nerve-fibers?), which are surrounded by loosely-aggregated, ganglionic globules. (This organ has been taken for a soniferous apparatus by Latreille. J. Müller was the first who fortunately conceived that with *Gryllus hieroglyphus* this was an auditory organ. He gave, however, the interpretation only as hypothetical; but I have placed it beyond all doubt by careful researches made on *Gomphoceros*, *Oedipoda*, *Podisma*, *Caloptenus* and *Truxalis*.")

"The Locustidæ and Achetidæ have a similar organ, situated in the fore-legs directly below the coxo-tibial articulation. With a part of the Locustidæ (*Meconema*, *Barbitistes*, *Phancroptera*, *Phylloptera*), there is on each side of this point a fossa, while with another portion of this family there are, at this same place, two more or less spacious cavities (auditory capsules) provided with orifices opening forward. These fossæ and these cavities have each, on their internal surface, a long-oval tympanum. The principal trachean trunk of the leg passes between two tympanums, and dilates, at this point, into a vesicle whose upper extremity is in connection with a ganglion of the auditory nerve. This last arises from the first thoracic ganglion, and accompanies the principal nerve of the leg. From this ganglion in question passes off a band of nervous substance, which stretches along the slightly excavated anterior side of the trachean vesicle. Upon this band is situated a row of transparent vesicles containing the same kind of cuneiform, staff-like bodies, mentioned as occurring with the Acrididæ. The two large trachean trunks of the fore-legs open by two wide, infundibuliform orifices on the posterior border of the prothorax, so that here, as with the Acrididæ, a part of this trachean apparatus may be compared to a *Tuba Eustachii*. With the Achetidæ, there is, on the external side of the tibia of the fore-legs, an orifice closed by a white, silvery membrane (tympanum), behind which is an auditory organ like that just described. (With *Acheta achatina* and *italica*, there is a tympanum of the same size, on the internal surface of the legs in question; but it is scarcely observable with *Acheta sylvestris*, *A. domestica* and *A. campestris*.)"

Other naturalists have placed the auditory apparatus of diurnal lepidoptera in their club-shaped antennæ; of bees at the root of their maxillæ; of *Melolontha* in their antennal plates; of *Locusta viridissima* in the membranes which unite the antenna with the head.

I think that Siebold assumes too much when he states that the existence of a tympanic membrane is the only test of the existence of an auditory apparatus. It is true that such a test would apply to the non-aquatic vertebrates, but their homologies do not extend to the articulates; and besides, any physicist can not only conceive of, but can actually construct other receptors of aerial vibrations, as I will soon show by conclusive experiments. Neither can I agree with him in supposing that the antennæ are only tactile organs, for very often their position and limited motion would exclude them from this function;* and, moreover, it has never been proved that the antennæ, which differ so much in their forms in different insects, are always tactile organs. They may be used as such in some insects; in others, they may be organs of audition; while in other insects they may, as Newport and Goureau surmise, have both functions; for, even granting that Müller's law of the specific energy of the senses extends to the insects, yet the anatomy of their nervous system is not sufficiently known to prevent the supposition that there may be two distinct sets of nerve fibers in the antennæ or in connection with their bases; so that the antennæ may serve both as tactile and as auditory organs; just as the hand, which receives at the same time the impression of the character of the surface of a body and of its temperature; or, like the tongue, which at the same time distinguishes the surface, the form, the temperature and the taste of a body. Finally, I take objection to this statement: "Newport and Goureau think that the antennæ serve both as tactile and auditory organs. But this view is inadmissible, as Erichson has already stated, except in the sense that the antennæ, like all solid bodies, may conduct sonorous vibrations of the air." Here, evidently, Siebold had not in his mind the physical relations which exist between two bodies which give exactly the same number of vibrations; for it is well known that when one of them vibrates, the other will be set into vibration by the impacts sent to it through the intervening air. Thus, if the fibrillæ on the antennæ of an insect should be tuned to the different notes of the sound emitted by the same insect, then when these sounds fell upon the antennal fibrils, the latter would enter into vibration with those notes of the sound to which they were severally tuned; and so it is evident that not only could a properly constructed antenna serve as a receptor of sound, but it would also have a function not possible in a membrane; that is, it would have the power of analyzing a composite sound by the co-vibration of its various fibrillæ to the elementary tones of the sound.

The fact that the existence of such an antenna is not only supposable but even highly probable, taken in connection with an observation I have often made in looking over entomological collections; viz: that fibrillæ on the antennæ of noctural insects are highly developed, while on the antennæ of diurnal insects they are either entirely absent or reduced to mere rudimentary filaments, caused me to entertain the hope that I should

* Indeed, they are often highly developed in themselves while accompanied by *palpi*, which are properly placed, adequately organized and endowed with a range of motion suitable to an organ intended for purposes of touch.

be able to confirm my surmises by actual experiments on the effects of sonorous vibrations on the antennal fibrillæ; also, the well known observations of Hensen encouraged me to seek in aerial insects for phenomena similar to those he had found in the decapod, the *Mysis*, and thus to discover in nature an apparatus whose functions are the counterpart of those of the apparatus with which I gave the experimental confirmation of Fourier's theorem, and similar to the supposed functions of the rods of the organ of Corti.

The beautiful structure of the plumose antennæ of the male *Culex Musquito* is well known to all microscopists; and these organs at once recurred to me as suitable objects on which to begin my experiments. The antennæ of these insects are twelve-jointed and from each joint radiates a whorl of fibrils, and the latter gradually decrease in their lengths as we proceed from those of the second joint from the base of the antenna to those of the second joint from the tip. These fibrils are highly elastic and so slender that their lengths are over three hundred times their diameters. They taper slightly, so that their diameter at the base is to the diameter near the tip as 3 to 2.

I cemented a live male mosquito with shellac to a glass slide and brought to bear on various fibrils a $\frac{1}{5}$th objective. I then sounded successively, near the stage of the microscope, a series of tuning-forks with the openings of their resonant boxes turned toward the fibrils. On my first trials with an Ut_4 fork, of 512 v. per sec., I was delighted with the results of the experiments, for I saw certain of the fibrils enter into vigorous vibration, while others remained comparatively at rest.

The table of experiments which I have given is characteristic of all of the many series which I have made. In the first column (A) I have given the notes of the forks in the French notation, which König stamps upon his forks. In the second (B) are the amplitudes of the vibrations of the end of the fibril in divisions of the micrometer scale; and in column (C) are the values of these divisions in fractions of a millimeter.

A.	B.	C.
Ut_2	·5 div.	·0042 mm.
Ut_3	2·5	·0200
Mi_3	1·75	·0147
Sol_3	2·0	·0168
Ut_4	6·0	·0504
Mi_4	1·5	·0126
Sol_4	1·5	·0126
$B^{\flat}_4-$	1·5	·0126
Ut_5	2·0	·0168

The superior effect of the vibrations of the Ut_4 fork on the fibril is marked, but thinking that the differences in the observed amplitudes of the vibrations might be owing to differences in the intensities of the various sounds, I repeated the experiment, but vibrated the forks which gave the greater amplitudes of co-vibration with the lowest intensities; and although I observed an approach toward equality of amplitude, yet the fibre gave the maximum swings when Ut_4 was sounded, and I was persuaded that this special fibril was tuned to unison with Ut_4 or to some other note within a semitone of it. The differences of amplitude given by Ut_4 and Sol_3 and Mi_4 are considerable, and the table also brings out the interesting observation that the lower (Ut_3) and the higher (Ut_5) harmonics of Ut_4 cause greater amplitudes of vibration than any intermediate notes. As long as a universal method for the determination of the relative intensities of sounds of different pitch remains undiscovered, so long will the science of acoustics remain in its present vague qualitative condition.* Now, not having the means of equalizing the intensities of the vibrations issuing from the various resonant boxes, I adopted the plan of sounding, with a bow, each fork with the greatest intensity I could obtain. I think that it is to be regretted that König did not adhere to the form of fork, with *inclined prongs*, as formerly made by Marloye; for with such forks one can always reproduce the same initial intensity of vibration by separating the prongs by means of the same cylindrical rod which is drawn between them. Experiments similar to those already given revealed a fibril tuned to such perfect unison with Ut_3 that it vibrated through 18 divisions of the micrometer or ·15 mm., while its amplitude of vibration was only 3 div. when Ut_4 was sounded. Other fibrils responded to other notes, so that I infer from my experiments on about a dozen mosquitos that their fibrils are tuned to sounds extending through the middle and next higher octave of the piano.

* I have recently made some experiments in this direction, which show the possibility of eventually being able to express the intensity of an aerial vibration directly in fraction of Joule's Dynamical Unit, by measuring the heat developed in a slip of sheet rubber stretched between the prongs of a fork and enclosed in a compound thermo-battery. The relative intensities of the aerial vibration produced by the fork when engaged in heating the rubber and when the rubber is removed, can be measured by the method I described in the Amer. Jour. Sci., Feb., 1873. Of course if we can determine the amount of heat produced per second by a known fraction of the intensity, we have the amount produced by the vibration with its entire intensity. Then means can be devised by which the aerial vibration produced by this fork can always be reproduced with the same intensity. This intensity, expressed in fraction of Joule's unit, is stamped upon the apparatus, which ever afterward serves as a true measure for obtaining the intensities of the vibrations of all simple sounds having the same pitch as itself. The same operation can be performed on other forks of different pitch, and so a series of intensities of different periods of vibration is obtained expressed in a corresponding series of fractions of Joule's unit. Recent experiments have given $\frac{1}{100000}$th of a Joule's unit as the approximate dynamic equivalent of ten seconds of aerial vibrations produced by an Ut_3 fork, set in motion by intermittent electro-magnetic action and placed before a resonator.

To subject to a severe test the supposition I now entertained, that the fibrils were tuned to various periods of vibration, I measured with great care the lengths and diameters of two fibrils, one of which vibrated strongly to Ut_3, the other as powerfully to Ut_4; and from these measures I constructed in homogeneous pine wood two gigantic models of the fibrils; the one corresponding to the Ut_3 fibril being about one meter long. After a little practice I succeeded in counting readily the number of vibrations they gave when they were clamped at one end and drawn from a horizontal position. On obtaining the ratio of these numbers, I found that it coincided with the ratio existing between the numbers of vibrations of the forks to which co-vibrated the fibrils of which these pine rods were models.

The consideration of the relations which these slender, tapering, and pointed fibrils must have to the aerial pulses acting on them, led me to discoveries in the physiology of audition which I imagine are entirely new. If a sonorous wave falls upon one of these fibrils so that its wave-front is at right angles to the fibril, and hence the direction of the pulses in the wave are in the direction of the fibril's length, the latter cannot be set in vibration; but if the vibrations in the wave are brought more and more to bear athwart the fibril it will vibrate with amplitudes increasing until it reaches its maximum swing of co-vibration, when the wave-front is parallel to its length and therefore the direction of the impulses on the wave are at right angles to the fibril. These curious surmises I have confirmed by many experiments made in the following manner. A fork which causes a strong co-vibration in a certain fibril is brought near the microscope, so that the axis of the resonant box is perpendicular to the fibril and its opening is toward the microscope. The fibril, in these circumstances, enters into vigorous vibration on sounding the fork; but, on moving the box around the stage of the microscope so that the axis of the box always points toward the fibril, the amplitudes of vibration of the fibril gradually diminish, and when the axis of the box coincides with the length of the fibril, and therefore the sonorous pulses act on the fibril in the direction of its length, the fibril is absolutely stationary and even remains so when the fork, in this position, is brought quite close to the microscope. These observations at once revealed to me a new function of these organs; for if, for the moment, we assume that the antennæ are really the organs which receive aerial vibrations and transmit them to an auditory capsule, or rudimentary labyrinth, then these insects must have the faculty of the perception of the direction sound more highly developed than in any other class of animals. The following experiments will show the force of this statement and at the same time illustrate the manner in which these insects determine the direction of a sonorous center. I placed under the microscope a live mosquito, and kept my attention fixed upon a fibril which co-vibrated to the sound of a tuning-fork, which an assistant placed in unknown positions around the microscope. I then rotated the stage of the instrument until the fibril ceased to vibrate, and then drew a line on a piece of paper, under the microscope, in the direction of the fibril. On extending this line, I found that it always cut within 5° of the position of the source of the sound. The antennæ of the male mosquito have a range of motion in a horizontal direction, so that the angle included between them can vary considerably inside and outside of 40°,* and I conceive that this is the manner in which these insects during night direct their flight toward the female. The song of the female vibrates the fibrillæ of one of the antennæ more forcibly than those of the other. The insect spreads the angle between his antennæ, and thus, as I have observed, brings the fibrillæ, situate within the angle formed by the antennæ, in a direction approximately parallel to the axis of the body. The mosquito now turns his body in the direction of that antenna whose fibrils are most affected, and thus gives greater intensity to the vibrations of the fibrils of the other antenna. When he has thus brought the vibrations of the antennæ to equality of intensity, he has placed his body in the direction of the radiation of the sound, and he directs his flight accordingly; and from my experiments it would appear that he can thus guide himself to within 5° of the direction of the female.

Some may assume from the fact of the co-vibration of these fibrils to sounds of different pitch, that the mosquito has the power of decomposing the sensation of a composite sound into its simple components, as is done by the higher vertebrates; but I do not hold this view, but believe that the range of co-vibration of the fibrils of the mosquito is to enable it to apprehend the ranging pitch of the sounds of the female. In other words, the want of definite and fixed pitch to the female's song demands for the receiving apparatus of her sounds a corresponding range of co-vibration, so that instead of indicating a high order of auditory development it is really the lowest, except in its power of determining the direction of a sonorous center, in which respect it surpasses by far our own ear.†

* The shafts of the antennæ include an angle of about 40°. The basal fibrils of the antennæ form an angle of about 90°, and the terminal fibrils an angle of about 30°, with the axis of the insect.

† Some physiologists, attempting to explain the function of the semicircular canals, assume, because these canals are in three planes at right angles to each other, that they serve to fix in space a sonorous center, just as the geometrician by his three coördinate planes determines the position of a point in space. But this assumption is fanciful and entirely devoid of reason; for the semicircular canals are always in

The auditory apparatus we have just described does not in the least confirm Helmholtz's hypothesis of the functions of the organ of Corti; for the supposed power of that organ to decompose a sonorous sensation depends upon the existence of an auditory nerve differentiated as highly as the co-vibrating apparatus, and in the case of the mosquito there is no known anatomical basis for such an opinion. In other words, my researches show external co-vibrating organs whose functions replace those of the tympanic membrane and chain of ossicles in receiving and transmitting vibrations; while Helmholtz's discoveries point to the existence of internal co-vibrating organs which have no analogy to those of the mosquito, because the functions of the former are not to receive and transmit vibrations to the sensory apparatus of the ear, but to give the sensation of pitch and to decompose a composite sonorous sensation into its elements; and this they can only do by their connection with a nervous development whose parts are as numerous as those of the co-vibrating mechanism. Now as such a nervous organization does not exist in insects, it follows that neither anatomical nor functional relations exist between the co-vibrating fibrils on the antennæ and the co-vibrating rods in the organ of Corti, and therefore, that neither Hensen's observations on the *Mysis* (assumed by Helmholtz to confirm his hypothesis), nor mine on the mosquito, can be adduced in support of Helmholtz's hypothesis of audition.*

The above described experiments were made with care, and I think that I am authorized to hold the opinion that I have established a physical connection existing between the sounds emitted by the female and the co-vibrations of the antennal fibrillæ of the male mosquito; but only a well established physiological relation between these co-vibrating parts of the animal and the development of its nervous system will authorize us to state that these are really the auditory organs of the insect. At this stage of the investigation I began a search through the zoological journals, and found nearly all that I could desire in a paper, in vol. iii, 1855, of the Quarterly Journal of the Micro-

the same dynamic relation to the tympanic membrane, which receives the vibration to be transmitted always in one way through the ossicles to the inner ear. Really, we determine the direction of a sound by the difference in the intensities of the effects produced in the two ears, and this determination is aided by the form of the outer ear and by the fact that man can turn his head around a vertical axis. Other mammalia, however, have the power of facilitating the determination of motion by moving the axis of their outer ears into different directions. It is also a fact that when one ear is slightly deaf, that the person unconsciously so affected always supposes a sound to come from the side on which is his good ear.

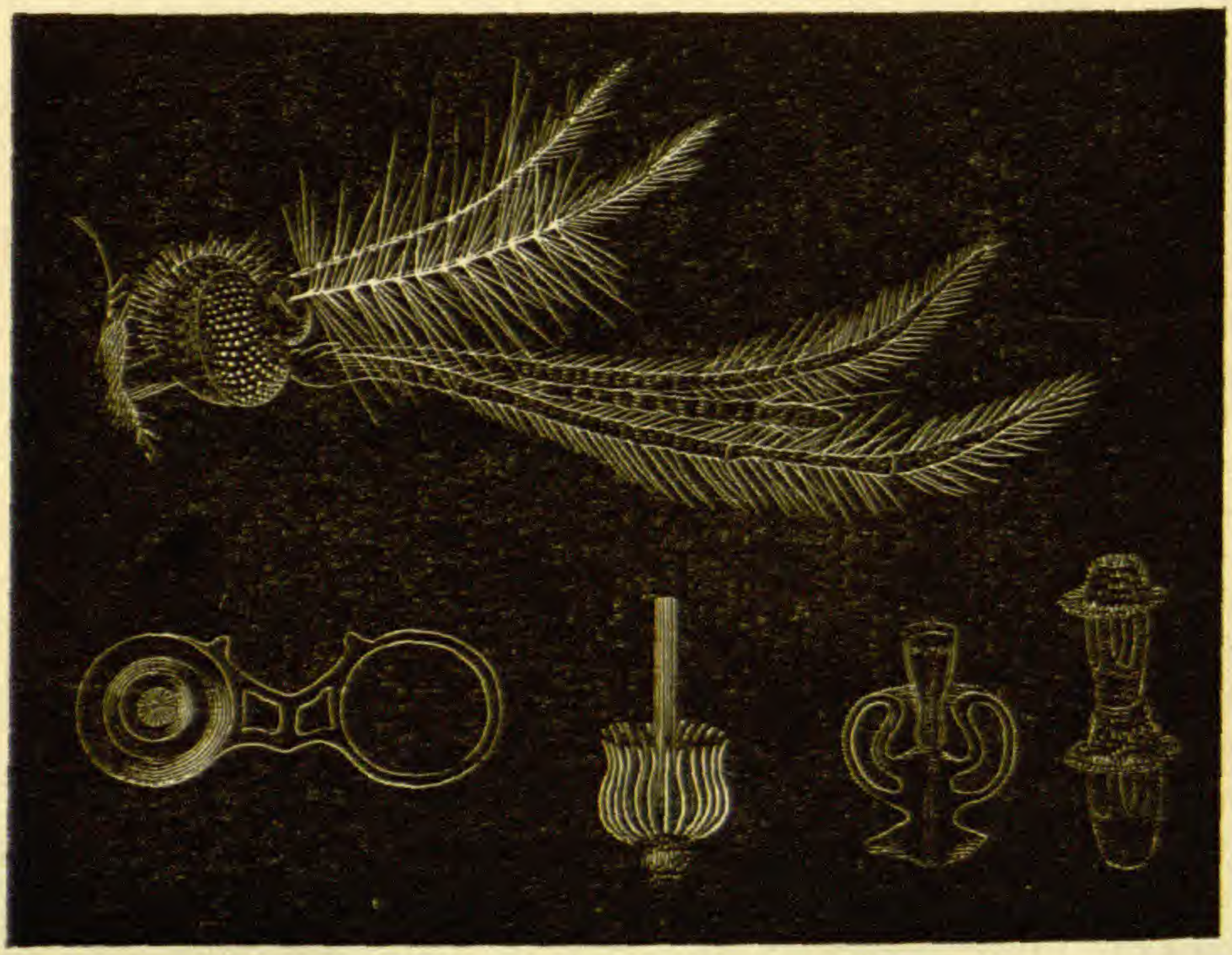
* Also, the organ of Corti having disappeared in the lower vertebrates, it is not likely that it would reappear in the articulata; and especially will this opinion have weight when we consider that the peculiar function of the organ of Corti is the appreciation of those composite sounds, whose signification mammals are constantly called upon to interpret.

scopical Society, entitled "*Auditory Apparatus of the Culex Mosquito*, by Christopher Johnson, M.D., Baltimore, U. S."

In this excellent paper I found clear statements showing that its talented author had surmised the existence of some of the physical facts which my experiments and observations have confirmed. To show that anatomical facts conform to the hypothesis that the antennal fibrils are the auditory organs of the mosquito,* I cannot do better than quote the following from Dr. Johnson's paper:

"While bearing in mind the difference between *feeling a noise* and *perceiving a vibration*, we may safely assume with Carus—for a great number of insects, at least,—that whenever true auditory organs are developed in them, their seat is to be found in the neighborhood of the *antennæ*. That these parts themselves are, in some instances, concerned in collecting and transmitting sonorous vibrations, we hold as established by the observations we have made, particularly upon the *Culex mosquito;* while we believe, as Newport has asserted in general terms, that they serve also as tactile organs.

2.



"The male mosquito differs considerably, as is well known, from the female; his body being smaller and of a darker color,

* A short time before the death of my friend, Prof. Agassiz, he wrote me these words: "I can hardly express my delight at reading your letter. I feel you have hit upon one of the most fertile mines for the elucidation of a problem which to this day is a puzzle to naturalists, the seat of the organ of hearing in Articulates."

and his head furnished with *antennæ* and *palpi* in a state of greater development. (Fig. 2.) Notwithstanding the fitness of his organs for predatory purposes, he is timid, seldom entering dwellings or annoying man, but restricts himself to damp and foul places, especially sinks and privies. The female, on the other hand, gives greater extension to her flight, and attacking our race, is the occasion of no inconsiderable disturbance and vexation during the summer and autumn months.

"The head of the male mosquito, about 0·67 mm. wide, is provided with lunate eyes, between which in front superiorly are found two pyriform capsules nearly touching each other, and having implanted into them the very remarkable antennæ.

"The capsule, measuring about 0·21 mm., is composed of a horny substance, and is attached posteriorly by its pedicle, while anteriorly it rests upon a horny ring, united with its fellow by a transverse fenestrated band, and to which it is joined by a thin elastic membrane. Externally it has a rounded form, but internally it resembles a certain sort of lamp shade with a constriction near its middle; and between this inner cup and outer globe there exists a space, except at the bottom or proximal end, where both are united.

"The antennæ are of nearly equal length in the male and the female.

"In the male, the antenna is about 1·75 mm. in length, and consists of fourteen joints, twelve short and nearly equal, and two long and equal terminal ones, the latter measuring (together) 0·70 mm. Each of the shorter joints has a fenestrated skeleton with an external investment, and terminates simply posteriorly, but is encircled anteriorly with about forty *papillæ*, upon which are implanted long and stiff hairs, the proximal sets being about 0·79 mm. and the distal ones 0·70 mm. in length; and it is beset with minute bristles in front of each whorl.

"The two last joints have each a whorl of about twenty short hairs near the base.

"In the female the joints are nearly equal, number but thirteen, and have each a whorl of about a dozen small hairs around the base. Here, as well as in the male, the parts of the antennæ enjoy a limited motion upon each other, except the basal joint, which, being fixed, moves with the capsule upon which it is implanted.

"The space between the inner and outer walls of the capsule, which we term confidently the auditory capsule,* is filled with a fluid of moderate consistency, opalescent and containing minute spherical corpuscles, and which probably bears the same relation to the nerve as does the lymph in the scalæ of the cochlea of higher animals. The nerve itself, of the antenna, proceeds from the first or cerebral ganglion, advances toward

* See fig. 2.

the pedicle of the capsule in company with the large trachea, which sends its ramifications throughout the entire apparatus, and, penetrating the pedicle, its filaments divide into two portions. The central threads continue forward into the antenna, and are lost there; the peripheral ones, on the contrary, radiate outward in every direction, enter the capsular space, and are lodged there for more than half their length in *sulci* wrought in the inner wall or cup of the capsule.

"In the female the disposition of parts is observed to be nearly the same, excepting that the capsule is smaller, and that the last distal antennal joint is rudimental.

"The proboscis does not differ materially in the two sexes; but the palpi, although consisting in both instances of the same number of pieces, are very unlike. In the female they are extremely short, but in the male attain the length of 2·73 mm.; while the proboscis measures but 2·16 mm. They are curved upward at the extremity.

"* * * The position of the capsules strikes us as extremely favorable for the performance of the function which we assign to them; besides which there present themselves in the same light the anatomical arrangement of the capsules, the disposition and lodgment of the nerves, the fitness of the expanded whorls for receiving, and of the jointed antennæ fixed by the immoveable basal joint for transmitting, vibrations created by sonorous undulations. The intra-capsular fluid is impressed by the shock, the expanded nerve appreciates the effect of the sound, by the quantity of the impression; of the pitch, or quality by the consonance of particular whorls of stiff hairs, according to their lengths; and of the direction in which the undulations travel, by the manner in which they strike upon the antennæ, or may be made to meet either antenna in consequence of an opposite movement of that part.

"That the male should be endowed with superior acuteness of the sense of hearing, appears from the fact, that he must seek the female for sexual union either in the dim twilight or in the dark night, when nothing but her sharp humming noise can serve him as a guide. The necessity for an equal perfection of hearing does not exist in the female; and, accordingly, we find that the organs of the one attain a development which the others never reach. In these views we believe ourselves to be borne out by direct experiment, in connection with which we may allude to the greater difficulty of catching the male mosquito.

"In the course of our observations we have arrived at the conclusion, that the antennæ serve to a considerable extent as organs of touch in the female; for the palpi are extremely short, while the antennæ are very moveable, and nearly equal the proboscis in length. In the male, however, the length and perfect development of the palpi would lead us to look for the seat of the tactile sense elsewhere, and, in fact, we find the two apical antennal joints to be long, moveable, and comparatively free from hairs; and the relative motion of the remaining joints very much more limited."

My experiments on the mosquito began late in the fall, and therefore I was not able to extend them to other insects. This spring I purpose to resume the research, and will experiment especially on those orthoptera and hemiptera which voluntarily emit distinct and characteristic sounds.

4. *Suggestions as to the function of the Spiral Scalæ of the Cochlea, leading to an Hypothesis of the Mechanism of Audition.*

As the auditory nerve has by far its highest development in the cochlea, it is a natural inference that this part of the ear is chiefly concerned in audition, and that the very peculiar form of the cochlea fulfills some important function; yet the relations of this form to the mode of audition has occupied but little the attention of physiologists. The only suggestion as to the uses of its form with which I am acquainted, is that given by Dr. J. W. Draper in his Physiology, N. Y., 1855. This distinguished scientist states that "it may be imagined how it is that a sound passing through the auditory canal, the bones of the tympanum, the membrane of the fenestra ovalis, and thus affecting its destined portion of the lamina, does not give rise to an idea in the mind of repetition or reverberation by moving back and forth through the two scalæ and affecting its proper nerve fibril at each passage. Is there not a necessity for the exertion of some mechanism of interference which shall destroy the wave after it has once done its work?" Dr. Draper then reasons that this reverberation is prevented by the scalæ being of different lengths and by the fact of their junction in the helicotrema. These two circumstances give rise to interferences, in the helicotrema, of the waves which have proceeded from the stapes up the scala vestibuli with the waves which passed from the membrana tympani across the tympanum to the fenestra rotunda, and thence up the scala tympani to the helicotrema. Dr. Draper also states that when the stapes is pushed in by the contractions of the tensor-tympani and stapedius muscle, the relative length of the scalæ is changed, and thus the proper adjustment for an interference is effected. But even granting that "reverberations or repercussions" take place in a body like the whole apparatus of audition, whose heterogeneous structure must make all of its vibrations, *taken as a mass*, forced oscillations, I do not agree with my distinguished friend in thinking that the difference in the lengths of the scalæ could bring about any interference except of the most minute and inefficient amount; even if we could agree with Dr. Draper that the intensity of the pulses sent from the fenestra rotunda nearly equal the intensity of those sent up the scala vertibuli from the stapes. The following considerations will make clear our objections to the hypothesis of Dr. Draper. If we take the mean wave-length of the sounds which fall upon the ear as that of the treble G of 440 vibrations per second, it follows that this wave-length will be one meter. But the velocity of sound in the fluid of the scalæ is, at least, $4\frac{1}{4}$ times what it is in air of the same temperature; therefore the average length of the sonorous waves which traverse the scalæ is $4\frac{1}{4}$ meters, and hence for two such waves, meeting in the helicotrema, to completely interfere, one scala would have to exceed the other in length by 2·12 meters. But the entire length of a scala is at the highest only 29 mm., and the difference in their length, taken at its maximum, is so slight that the diminution in the intensity of the resultant wave produced in the helicotrema is inappreciable; and especially will it be so considered when we take into account the relatively feeble intensity of the wave which is sent from the tympanic membrane across the air of the drum on to the membrane of the fenestra rotunda, where two sudden changes in density occur before it passes up the scala tympani.

The following attempt at an explanation of the functions of the spiral stairways of the cochlea is given merely as a suggestion, and with the hope that I may thereby call the attention of students of physiological acoustics to the consideration of the uses of these peculiar forms. Recent studies in embryology and comparative anatomy have shown that the ductus cochlearis is the essential part of the ear, and that the forms of the scalæ are determined by it; for "the original soft parts of the cochlea are distinct from their osseous capsule, which belongs to the petrous bone; the scalæ are secondary formations around the principal canal of the cochlea, the ductus cochlearis, whose epithelial lining proves eventually to be the germ center, so to speak, of the entire apparatus." (*Waldeyer, On the Auditory Nerve and Cochlea;* in Stricker's Histology.) The fact that the ductus controls the form of the scalæ, and not *vice versa*, shows that the scalæ must bear some very important functional relation to the ductus. This relation will become evident on considering the actions which must take place when a sound-wave traverses the scalæ.

All know that the organ of Corti is enclosed in the ductus cochlearis, a canal of triangular section bounded on two of its sides by the scalæ, and on its third by the membranes lining the outer wall of the cochlea. The upper wall of this canal is formed by the membrana Reissneri, which separates it from the scala vestibuli, and its lower wall is the lamina spiralis, and the elastic membrana basilaris, which separate it from the scala tympani. The ductus is closed at its upper end, and at its lower end it communicates with the sacculus hemisphericus by a fine duct. The arch of Corti rests upon the membrana basilaris, which extends beyond the base of the arch to the membranous outer wall of the cochlea, and over the arch spreads the membrana tectoria, covering the rods of Corti and the hair-cell cords as with a roof, but leaving the outer portion of the elastic membrana basilaris exposed. We will now show that the significance of these anatomical relations is to bring the sound vibrations to act with the greatest advantage on the co-vibrating parts of the ear, and to cause these parts to make one-half as many vibrations in a given time as the tympanic or basilar membranes.

The relations which the form of the scalæ bears to the sonorous waves traversing them, will be modified according to the existence or non-existence of a communication between the scalæ. On this point there seems to be some difference of opinion, and, therefore, I will attempt to explain the functions of the scalæ, first, on the supposition that they are continuous, and then on the assumption that they are not continuous, but closed at the place where the helicotrema is supposed by most anatomists to exist.

E. Weber was the first to point out the peculiar molecular actions which exist when the dimensions of a body are very small compared with the length of the sonorous waves which traverse it; and Helmholtz based his investigations on *the Mechanism of the Ossicles of the Ear* on the theory of Weber, which Helmholtz gives in these words: "The difference in displacement of two oscillating particles, whose distance from one another is infinitely small compared with the wave-length, is itself infinitely small compared with the entire amplitude of displacement." It is evident that the compressions and dilatations which may exist in any body, depend entirely on the differences in the phases of the vibrations constituting the sonorous wave, and when the body has a depth equal to half a wave-length it can embrace the maximum amount of condensation and rarefaction. But condensation and dilatation alone produce *lateral action* on the walls of a straight canal traversed by sonorous vibrations, and hence, if the length of the canal be but a small fraction of the wave, then there exists throughout the canal but little difference in phase, and therefore but little lateral action. Now the united lengths of the scalæ is but a small fraction of the mean length of the sonorous waves which traverse it; for if we take, as above, $4\frac{1}{2}$ meters as the mean length of the waves which are propagated through the scalæ, and 59 mm. as the length of the united scalæ, it follows that the latter is only $\frac{1}{72}$ of the mean wave-length. Now if we imagine the scalæ straightened and forming one continuous tube with a free communication existing at the helicotrema, then the mean wave traversing them will cause only $\frac{1}{22}$ of the lateral action which this same wave would produce if the scalæ had the length of half of the wave, and it follows that the whole liquid of the scalæ would vibrate forward and backward almost as an incompressible mass, approaching in character to the oscillations of a solid piston in a cylinder; therefore, the action against the walls of the ductus cochlearis would be very slight. But now consider the change in effect on the ductus which takes place when it, together with the scalæ, is wound up into such an ascending spiral as exists in the ear. The molecules of the liquid in the scalæ, thrown forward and backward by the vibrations of the stapes, tend to move in straight lines, but the now curved form of the scalæ causes them to press against the outer or peripheral part of the upper wall (membrana Reissneri) of the ductus cochlearis, and against the outer part of the lower wall (membrana basilaris) when the stapes moves inward, and when it moves outward this action of compression is relieved from the two opposite walls of the ductus. But these actions produced by the stapes on the two walls of the ductus are opposed to each other, and since they take place simultaneously and with about the same intensity, (by reason of our assumption of the free communication of the scalæ,) the rods of Corti and the hair-cells will not vibrate but will only experience compressions and dilatations like the fluid in which they are immersed. Therefore, there appears to me a physical basis for the opinion that either there is no communication between the scalæ, or, if the helicotrema exist, that it must be a very constricted passage. Indeed, if we adopt the latter view, then everything works to produce the maximum effect on the co-vibrating parts of the organ of Corti; for when the stapes moves inward the pressure is thrown on the outer border of the upper wall, or roof, of the ductus, thence across to the peripheral portion of the basilar membrane. This action, we may say, takes place simultaneously throughout the whole length of the ductus, moves downward the floor of the basilar membrane, and thus presses the fluid of the scala tympani against the fenestra rotunda and moves this membrane outward. When, however, the stapes moves outward, the pressure is relieved from the elastic basilar membrane, which is now moved upward, while the fenestra rotunda moves inward.*

* If we could examine, at the same time, vibrating points on the stapes and on the fenestra rotunda with a vibration-microscope, I imagine that these points would exhibit no difference in phase when the membrana tympani vibrated to a note below the treble.

There are also other anatomical facts besides the inclination of the membrana Reissneri to the plane of the membrana basilaris, and the inclination of both these membranes to the plane perpendicular to the axis of the cochlea, which favor an opinion that the outer or peripheral part of the basilar membrane receives the main part of the vibrations which enter the ductus. The auditory nerve fibrils are not attached to the Corti rods or pillars, as was formerly imagined; and, therefore, these bodies cannot be the co-vibrating parts of the ductus; but the Corti pillars appear to act, in conjunction with the cylindrical nerve-cells of Hensen, as supports for the lamina reticularis, between which and the basilar membrane are steadily and tensely stretched the hair-cell *cords* (as I will term them); and to these cords are attached the nerve-fibrils. Waldeyer says, on this point, that "The outer radial fibers direct their course, as Gottstein has found, toward the tunnel of Corti, passing between the inner pillars and traversing the tunnel about midway between the summit and base of the arch; in a profile view these fibers appear like stretched harp-springs. On leaving the arched space they pass between the outer pillars and direct their course —rising a little toward the scala vestibuli—straight to the hair-cells, with which they become completely fused. In several preparations from the dog and the bat I have seen this termination of the nerves in the most convincing manner, at least so far as the innermost row of hair cells is concerned; as to the other rows, we may pretty confidently assert that the termination of the nerves is the same, for we can frequently see several fibers passing at the same time between the outer pillars." The very fact that the number of these hair-cell cords increases with the higher development of the ear shows their important function; for, while in man they are arranged alternately in five rows and number 18,000, in other Mammalia there are only two or three rows.* These hair-cell cords are more perpendicular to the basilar membrane than the Corti rods, and are also different in their forms, having swellings in the middle of their lengths. These swellings must cause them to act like loaded strings, and each hair-cell cord is peculiarly well adapted to co-vibrate with only one special sound. Also, these hair-cell cords are placed in reference to the sound pulses, striking them somewhat in the relation which the antennal fibrils of the mosquito bear to a wave-surface to which their lengths are per-

* It is to be regretted that no accurate measures of the lengths and diameters of the rods and cords of the organ of Corti have been secured. The outer pillars of the arch of Corti certainly double their length in going from the base to the top of the ductus; but does this fact point them out as bodies suitably proportioned to co-vibrate to sounds extending through at least *eight octaves?* I known of no measures on the hair-cell cords. When their dimensions are determined, physiologists will be able to give more precision to their hypotheses.

pendicular. The hair-cell cords, therefore, will not be set in vibration by the action of the feeble pulses which may reach them directly through the membrana Reissneri from the scala vestibuli; and furthermore, the shielding influence of the membrana tectoria tends to prevent this direct action on the cords.

If my view be correct, that these cords receive their vibrations from the basilar membrane, and not directly from the impulses sent into the ductus, it necessarily follows that these cords bear to the membrane, to which they are attached, the same relation as stretched strings bear to the vibrating tuning-forks in Melde's experiments; and, therefore, *a cord in the ductus will vibrate only half as often in a second as the basilar membrane to which it is fastened.* Experiments, similar to those described in section 1 of this paper, illustrate very well our hypothesis of audition. Thus, the membrane, placed near the sounding reed, stands for the basilar membrane; strings, of various lengths and diameters and loaded at their centers, are fastened to the membrane and represent the hair-cell cords. On sounding the reed-pipe, only those strings in tune with the harmonics existing in the composite sound of the reed will enter into vibration; just as when the same sound vibrations enter the ear, and vibrate the basilar membrane, the only hair-cell cords which enter into vibration are those in tune with the elementary vibrations existing in the membrane. Also, it is to be observed that as the loaded string makes one vibration to two of the membrane, so the hair-cell cord makes only one vibration to two of the basilar membrane.

If it be true that when simple vibrations impinge on the ear, the tympanic and basilar membranes vibrate twice, while the co-vibrating body only vibrates once, then it follows that if the same simple vibrations can be sent directly to the co-vibrating parts of the ear, without the intervention of the basilar membrane, we should perceive a sound which is the octave of the one we experienced when the same simple vibrations entered the ear through the tympanic membrane. Hence it appears that our hypothesis can be brought to the test of experiment in the following manner: A tuning-fork held near the ear causes a sensation corresponding to the designated pitch of the fork. But the vibrations of this fork can be sent to the inner ear through the bones of the head; and although we cannot prevent the simultaneous vibration of the tympanic and basilar membranes, yet we can at the same time directly vibrate all the parts of the inner ear. Therefore, if we first hold this fork near the ear and note its pitch and the quality of its sound, and then press its foot firmly against the temporal bone, we should perceive a marked difference in the timbre of the fork when sounded in these two different positions; for when its foot is against the head we should hear the usual simple sound of the fork accompanied by its octave.

Thus, if we take an Ut_3 fork and vibrate it near the ear, and closely apprehend the character of its sound, we shall experience a sensation which certainly does not contain that corresponding to the higher octave of the fork. Now, press firmly the foot of the fork against the zygomatic process, close to the ear, directing the foot of the fork somewhat backward, and we shall distinctly hear the higher octave of the fork singing in concert with its real note. If the auditory canal be now closed by gently placing the tip of the finger over it, we shall perceive the higher octave with an intensity almost equal to that of the fundamental note. The same sensation, though less intense, may be obtained by placing the fork on any part of the temporal bone. One can also perceive distinctly the higher octave when the fork is placed on the parietal bone, about two inches in front of and an inch or so to the side of the foramen, and its foot directed toward the opposite inner ear, while the auditory canal of this ear is gently closed with the tip of the finger. But the higher octave sings out with the greatest intensity when the foot of the fork is placed on the tragus of the outer ear. A friend, who is a musician as well as a physicist, repeated these experiments, and he informs me that when the foot of the fork is placed against the tragus of his ear he hears the higher octave to the almost entire exclusion of the lower, and with a clearness that reminds him of the sensation perceived when an Ut_4 resonator, placed to the ear, reinforces its proper note. The higher octaves of several forks have been thus perceived, but the forks from Ut_3 to Ut_4 inclusive appear to give the best results.

The fact that sound pulses sent to the inner ear through the head give the sensation corresponding to the higher octave of that perceived when the fork vibrates the air outside the ear, and, therefore, that different co-vibrating parts of the ear are set in action by the vibrations reaching the ear by these two different routes, is a necessary consequence of my hypothesis of the mode of audition, and was not suspected until my hypothesis pointed it out to me, and was not known until I attempted to test the hypothesis by experiment. I know of no other hypothesis that accounts for this fact, which, while it is a necessary consequence of my own views, is directly opposed to those hypotheses hitherto formed on the mode of audition; for, according to the latter, the co-vibrating parts of the ear make as many oscillations in a given interval as the tympanic and basilar membranes.

[To be continued.]

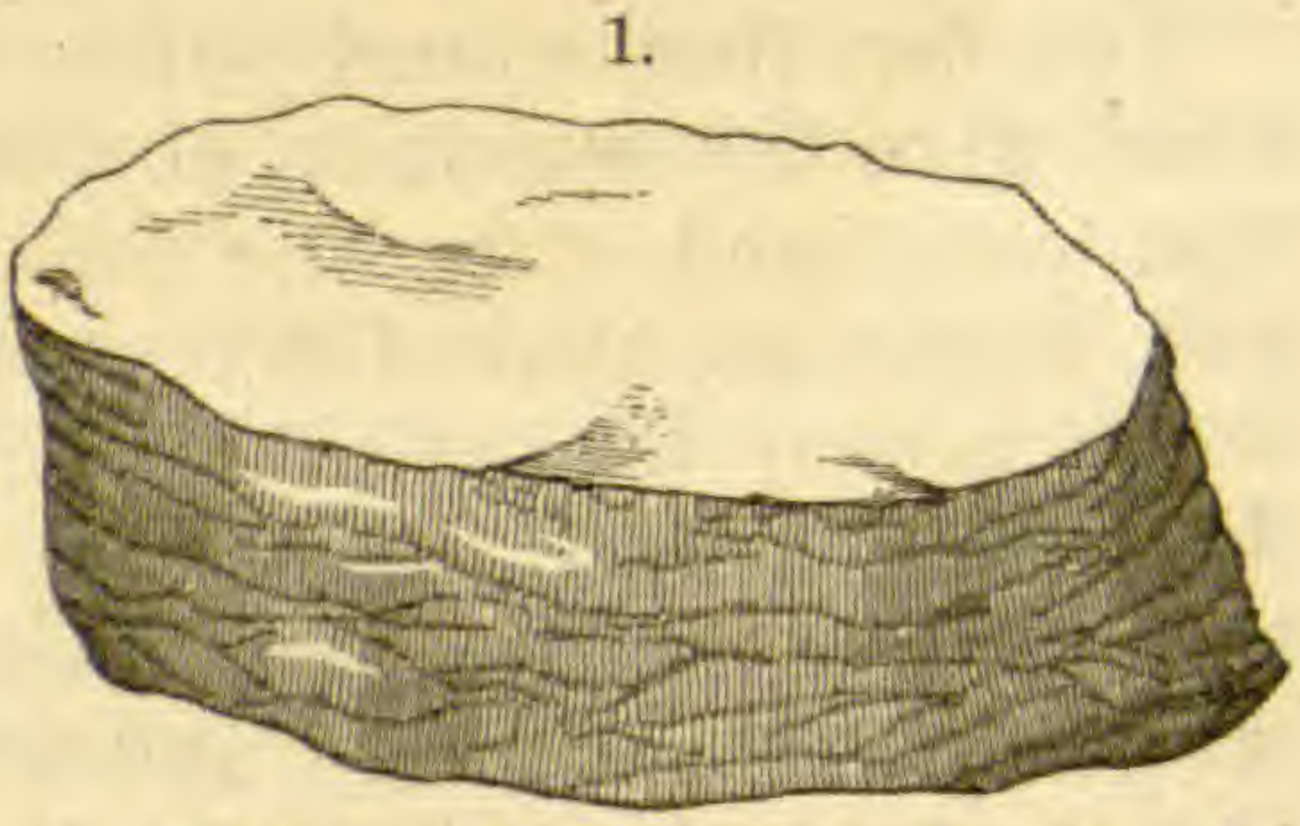
ART. X.—*On the so-called Land Plants from the Lower Silurian of Ohio;* by J. S. NEWBERRY.

[Read before the National Academy of Sciences, at the meeting in April, 1874.]

IN the January number of this Journal, Mr. Leo Lesquereux describes two fossils, found in the upper portion of the Cincinnati group, near Lebanon, Ohio. These he considers as the remains of land plants, and refers them to the genus *Sigillaria;* and this case is cited as the first instance where plants so highly organized have been met with in Lower Silurian rocks. Through the kindness of the Rev. H. Hertzer, to whom the specimens in question belong, they had been in my possession some time before the publication of Mr. Lesquereux's notice, and I had examined them with some care for the purpose of determining, if possible, their botanical relations. I had also made careful drawings of them, of which copies are herewith submitted. As the result of my examination, I am compelled to say that I fail to find, either in the external characters or internal structure of these specimens, any satisfactory evidence that they represent land plants; still less that they form species of the genus *Sigillaria.* Their external markings are fairly represented in the accompanying figures, and they exhibit no internal organic structure whatever. They are simply casts in earthy limestone without carbonaceous matter, or any traces of woody tissue.

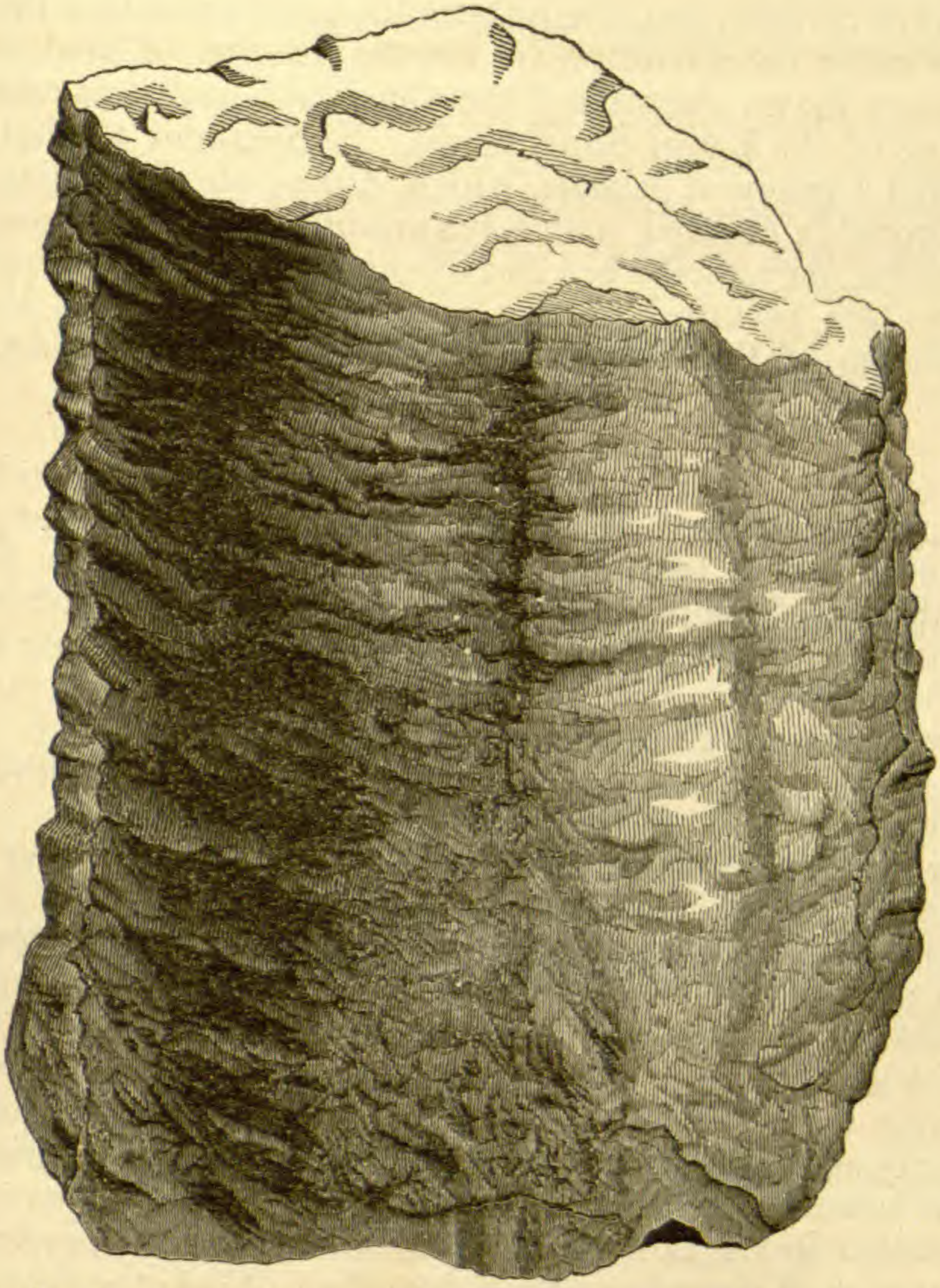
The smaller specimen (fig. 1) is a discoid section of a cylindrical trunk, of which the external surface is very smooth, but is marked by a reticulation not unlike that of one section of the genus *Sigillaria.* I did not discover, however, any dots or tubercles in the center of the meshes, such as are referred to by Mr. Lesquereux, and which, were they present, might be supposed to represent the place of the nutrient vessels of the leaves. Taken by itself, I should say that this specimen might be a sponge or some other low form of marine life, quite as well as a *Sigillaria.* Since it is so small and forms so little of the original organism, I think it would be unsafe to make it the base of any general and important conclusion.

1.



The larger specimen (fig. 2) is represented, like the other, of natural size. This is also a cast of a nearly cylindrical trunk, of which the external surface is roughened by irregularly disposed and unequally sized lenticular prominences. These resemble, in a rude way, the leaf scars borne by the trunks of some Lycopodiaceous or Cycadaceous plants, but they do not exhibit the spiral arrangement, nor the details of structure

2.



which the leaf-scars of such plants almost uniformly retain in the fossil state. In the interior of this trunk are seen a few of the irregularly scattered points of carbonaceous matter, but they are not continuous fibers, and to my eye show no traces of cell structure.

Taking all the characters of these interesting fossils into consideration, I am disposed to regard them as casts of the stems of fucoids. Had they been land plants, they would almost certainly exhibit more distinctness and regularity of surface-marking, some coating of carbonaceous matter, and some traces of organic structure. A large number of specimens of sea-floated land plants, which we have found in the Devonian limestones of Ohio, all assert their botanical affinities by these characters. The remains of fucoids, on the contrary, consist almost universally of mere casts of their external surface, carbonaceous matter and internal structure having both entirely disappeared.

The physical condition of the region about Cincinnati, during the Lower Silurian age, strengthens the conclusion that the specimens under consideration are not the remains of land plants. As I have shown elsewhere,* the Cincinnati arch was raised at the close of the Lower Silurian age. Subsequent to that time it formed a group of islands, which, during the Devonian age, were probably covered with a luxuriant terrestrial vegetation. But during the period when the Cincinnati group was deposited an open sea occupied all this part of the Mississippi Valley. The shores of this sea were formed by the Blue Ridge, the Adirondacks, the Canadian Highlands and the Eozoic area on the south shore of Lake Superior, nowhere nearer than 500 miles from the locality where these specimens were found. In these circumstances we must regard it as extremely improbable that specimens of two species of land plants should be floated from this far-off shore and should be deposited together in the calcareous sediment accumulating at the sea bottom near where Cincinnati now stands. That fucoids should be found there is, however, not at all strange, for they float to all parts of all oceans, and other fucoids are frequently met with in the Cincinnati group of this vicinity.

For the reasons given above, I should hesitate to rest upon these specimens so important a conclusion as that promulgated by Mr. Lesquereux. I would not be understood, however, to assert positively that they are *not* the remains of land plants, for they are too imperfect to be decisive of that question, but only this, that they do not afford characters which permit me to accept them as evidence of the existence of land plants, and certainly not of *Sigillariæ* in Ohio, during the Lower Silurian age.

The remains of what have been called land plants have been discovered in the Lower Cambrian sandstones of Sweden, and two species of these have been described (*Eophyton Linneanum* Torell, and *E. Torelli* Linnarsson). The specimens are said by algologists not to be the remains of algæ, but they are considered to be vascular cryptogams or monocotyledons. It is not certain, however, that they are not thallogens, as all traces of structure are lost and nothing is left but the impression of the external surface.†

The evidence of the existence of land plants during the Upper Silurian age is more satisfactory. Prof. J. W. Dawson of Montreal has announced the discovery of vascular cryptogams

* Geological Survey of Ohio, vol. i, part i, page 93.

† Geological Magazine, Sept., 1869. [In the Ofversigt af Kongl. Vetenskaps-Akademiens Förhandlingar, 1873, No. 9, Stockholm (the Bulletin of the Royal Swedish Academy) A. Nathorst has an article, illustrated by several plates, is which he shows a close resemblance between the forms of Eophyton and the trailn of drifting Fuci and other plants, and suggests this origin for them.—Eds.]

in the Upper Silurian strata of Gaspé, Canada.* Here, with a large number of fucoids, a few specimens have been found, which he refers to his genus *Psilophyton.* In these the scalariform axis and the outer fibrous bark both remain, and serve as guides in their classification.

With these exceptions, no land plants are reported below the Devonian. On this point, however, the evidence is all negative, and highly organized land plants may be at any time found in the Lower Silurian rocks. Indeed, the variety and high rank of the Devonian flora prepares us to expect such a result. Strict accuracy compels us to state, however, that up to the present time positive proof of the existence of land plants in the Lower Silurian has not been met with in other countries, nor is it furnished by the specimens under consideration.

ART. XI.—*A Criticism upon the Contractional Hypothesis;* by Captain C. E. DUTTON, U. S. A.†

THE hypotheses, which have been put forth to explain the part performed by hypogeal forces in the evolution of the surface features of the earth, are here referred to two types: 1st, those attributing them to the contraction resulting from secular loss of heat: this for the sake of convenience will here be called the *contractional hypothesis;* 2d, those arguments which have been resorted to in order to explain isolated facts, or groups of facts, by attributing them to the reaction of the interior to disturbances produced by external changes: this will be called the *reactional hypothesis.*

The argument for the contractional hypothesis presupposes that the earth-mass may be considered as consisting of two portions, a cooled exterior of undetermined (though probably comparatively small) depth, inclosing a hot nucleus. Although of some importance to the argument, it is not regarded as a vital question whether the nucleus be solid or fluid, nor whether the two portions be abruptly distinguished, or merely differentiated from each other. The secular loss of heat, it is assumed, would be greater from the hot nucleus than from the exterior, and the greater consequent contraction of the nucleus would therefore gradually withdraw the support of the exterior, which would collapse. The resulting strains upon the exterior would be mainly tangential. Owing to considerable inequalities in the ability of different portions to resist the strains thus devel-

* Dawson, Precarboniferous Plants of Canada, p. 66.

† This paper is one of several communications to the Washington Philosophical Society made during the winter of 1872–73, and has not hitherto been published except by title and brief abstract in the minutes of that society.

oped, the yielding would take place at the lines, or regions of least resistance, and the effects of the yielding would be manifested chiefly, or wholly, at those places, in the form of mountain chains, or belts of table-lands, and in the disturbances of stratification. The primary division of the surface into areas of land and water are attributed to the assumed smaller conductivity of materials underlying the land, which have been left behind in the general convergence of the surface toward the center. Regarding these as the main and underlying premises of the contractional argument, it is considered unnecessary to state the various subsidiary propositions which have been advanced to explain the determination of this action to particular phenomena, since the main proposition upon which they are based is considered untenable.

There can be no reasonable doubt that the earth-mass consists of a cooled exterior inclosing a hot nucleus, and a necessary corollary to this must be secular cooling, probably accompanied by contraction of the cooling portions. But when we apply the known laws of thermal physics to ascertain the rate of this cooling, and its distribution through the mass, the objectionable character of the contractional hypothesis becomes obvious. As the process under discussion must have continued during an immense duration, we must select some starting point at which the mean temperature of the mass was materially greater than at present. Assuming the degradation of temperature to have been continuous, we have only to assign a duration of sufficient length to arrive at a period when the whole mass, or a considerable portion of it, was fluid. The selection of this starting point is not altogether one of choice, but the one pointed to by the only obvious mode of reasoning, and in itself apparently not inconsistent with present facts. It is quite foreign to the purpose to inquire how the earth originally received its store of heat, since the fact of possession covers all grounds of present inquiry. As was indicated by Sir W. Thomson, the distribution, both of materials and heat, under the condition of fluidity would be an approach toward homogeneity, resulting from movements of convection taking place through the liquid mass. The first stage of evolution resulting from loss of heat would apparently be that of consolidation. The argument of Hopkins is here accepted, that consolidation must begin at the center, as a consequence of the fact that pressure elevates the congealing point; and temperatures being kept nearly uniform, the maximum pressure would determine the primary point of congelation. The solidification of materials at the surface would result in sinking by their increased density until the central solidification had proceeded so near the surface as to leave only an imperfectly liquid mass in which such movements, becoming more and more retarded, at length ceased, leaving a globe, departing from uniform temperature by differences not greater than the differences in congealing temperatures due to differences in pressure. The result would be a solid globe, with, perhaps, isolated reservoirs of liquid matter, which may have separated in the transition stage from fluid to solid by reason of a higher melting point.

This assumption of the genesis of the earth, though regarded as preferable to all others that have been proposed, is by no means insisted on. It is selected because it gives to the contractional argument the fullest scope and widest range of conditions consistent with known physical laws. There is apparently no supposition which can reasonably allow a higher interior temperature consistently with the formation of a stable surface. To assume a lower temperature for the interior would take away from that argument *pro rata* a portion of the possible diminution of volume upon which it must rely to account for surface corrugation. To assume a higher one would be a violation of physical laws as we now understand them, and virtually constitute an inadmissible appeal to mysteries. Starting, therefore, from a globe possessing the highest degree of temperature which can properly be conceded consistently with a condition in which the evolution of surface features can begin, it remains to inquire how far the cooling has progressed at the present time; what portions have been sensibly affected by it; and to what extent each portion has been affected. Difficult as this problem may seem, it is not beyond the reach of a general solution; and a particular solution will become possible immediately upon the determination of certain data not wholly beyond the reach of experiment.

Sir W. Thomson has very happily called Fourier's solutions of this problem a "mathematical poem;" and the discussion of one of them * by that preëminent philosopher is here summarized. Fourier's problem was that of "finding at any time the rate of variation of temperature from point to point, and the actual temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane."

Let V denote half the difference of the two initial temperatures.
- v half their sum.
- t the time.
- x the distance of any point from the plane.
- T the temperature of the point x at the time t.
- $\varkappa$ the conductivity of the material in terms of its own thermal capacity.

* Transactions Roy. Soc. Edinburgh, vol. xxiii.

Assuming temperature to be dependent upon the value of x, the rate of variation would be expressed by the first differential coefficient $\frac{dT}{dx}$, whose value, acccording to Fourier, is

$$\frac{dT}{dx}=\frac{\mathrm{V}}{\sqrt{\pi\kappa t}}\times\varepsilon^{-\frac{x^2}{4\kappa t}},$$

while the actual temperature at that point would be

$$\mathrm{T}=v+\frac{2\mathrm{V}}{\sqrt{\pi}}\int_0^{\frac{x}{2\sqrt{\kappa t}}}dz\varepsilon^{-z^2}.$$

These formulæ for the supposed infinite solid are obviously applicable for a limited period of time, and without sensible error, to the cooling earth. Beyond that period it would be necessary, in order to preserve their applicability, to introduce the proper modifications rendered necessary by the spherical form of the earth. It will soon appear, however, that the present, and consequently all past, geological epochs lie far within this limiting period, and hence we need not concern ourselves with it; in other words, the cooling of the earth, comparatively speaking, has made but very little progress up to the present day. To obtain from these expressions determinate values of $\frac{d\mathrm{T}}{dx}$ and T, it is first necessary to assign some value to κ, the coefficient of conductivity. To find this Thomson and Forbes instituted a series of experiments upon three different qualities of rock material, by burying thermometers to depths varying from 3 to 25 feet, and observing the effects of the variations of atmospheric temperature upon them. The observations, extending through fourteen years, were carefully reduced for each to the sum of a number of terms, each of which expressed a "simple harmonic," or vibration of temperature. By comparing the amplitudes of the annual vibrations at different depths, the value of the conductivity was determined * for the materials experimented upon. For a mean value of κ, Thomson took 400 as the most probable one; the units being the foot, the degree F°, and the year. This value, substituted in the first equation, gives

$$\frac{d\mathrm{T}}{dx}=\frac{\mathrm{V}}{35{\cdot}4}\cdot\frac{1}{\sqrt{t}}\cdot\varepsilon^{-\frac{x^2}{1600t}}.$$

It is also necessary to find some value for V, a matter of some difficulty. In the present case this will represent the maximum temperature of the interior at the beginning of the

* That is, the value in terms of its specific heat. The specific heat was determined by Regnault from blocks sent to him for that purpose.

cooling, and its value must be hypothetical. We are concerned, however, only with such values of it as may have undergone change, and we may take it to be the melting point of the more refractory materials which constitute the chief bulk of the nucleus. Presuming these to be anhydrous silicates for at least 500 to 800 miles in depth, and paying due regard to the effect of pressure upon the congealing point, we may accept Sir William's estimate of this temperature, which he takes to be, at a maximum, 7,000° F—a most abundant estimate. This reduces the expression to a relation between three unknown quantities, x, t, and $\frac{dT}{dx}$. If we desire to ascertain the rate of variation of temperature per foot of depth, at the distance x, after the lapse of the time t, (subject to data already given,) we have merely to substitute the numerical values taken for those quantities. Thus the variation of temperature at the depth of 2,000 feet, after the lapse of 100 million years, would be $\frac{dT}{dx} = \frac{V}{354000}$ * $= \frac{1}{50{\cdot}6}$ of a degree F per foot of descent. For the same value of t, the rate of increase of temperature at 400,000 feet of depth would have diminished to $\frac{1}{141}$ of a degree per foot; at 800,000 feet, to less than $\frac{1}{2550}$ of a degree per foot; and below 150 miles the increase of temperature would not be sensible. For points very near the surface—say ten miles or less—the exponential factor becoming sensibly equal to unity, the equation shows that the increase of temperature would be inversely as the square root of the duration of the cooling. If it be possible, therefore, to determine a true mean rate of increase of temperature per foot of descent at any point near the surface, the time required to elapse from the epoch of the first establishment of the cooling to the present will become known. This mean is placed by some investigators at $\frac{1}{50}$ of a degree F per foot, and by others at $\frac{1}{60}$; the former giving about 100 million, and the latter about 130 million, years.

The accompanying graphical representation† exhibits the law of increase of geothermal temperatures in accordance with Sir W. Thomson's discussion of Fourier's theorem.

Of the general correctness of this theorem there can be no doubt. We may, however, for the moment qualify this assertion by an inquiry as to the nature of one of the quantities entering the expression of it. The coefficient of conductivity $\varkappa$ is regarded as a true constant. Whether it is so in reality is questionable. Experiments upon the conductivity of some materials show that there is probably a variation,‡ which is

* The exponential factor having become so near $\varepsilon^0 = 1$ that it may be omitted.

† From the paper of Sir W. Thomson referred to.

‡ This has been shown by Principal J. D. Forbes to be the case with iron, the conductivity diminishing with the temperature.

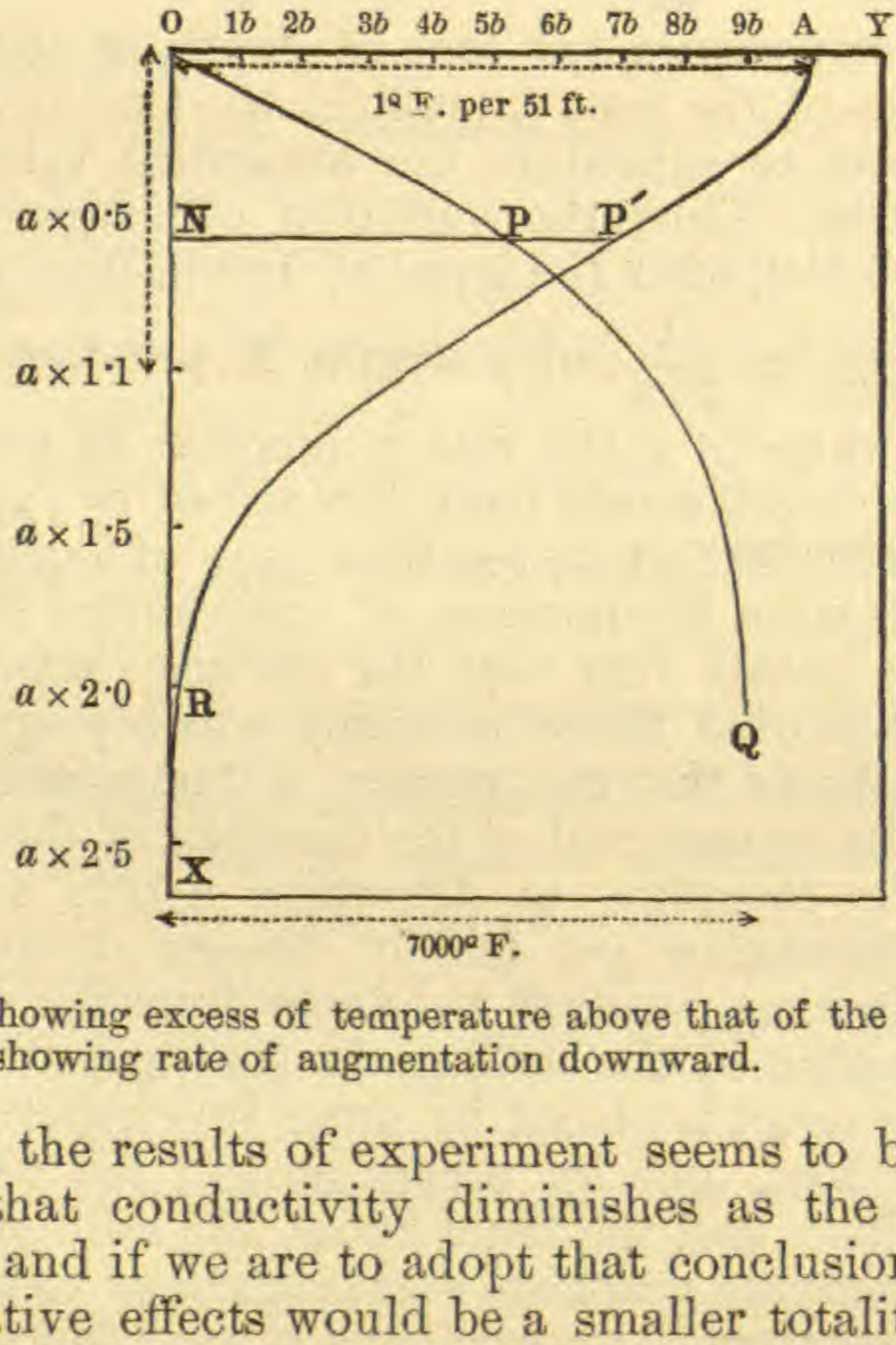
some function of the temperature. But if a function of the temperature, it must also be a function of the time and depth, and hence would alter the general form of the law, and affect all quantitative evaluations derived from it.* The general

Graphical representation of increase of temperature downward in the earth. (*Sir W. Thomson.*)

$ON = x$ $\qquad a = 2\sqrt{\kappa t}.$

$NP' = b\,\varepsilon^{-\frac{x^2}{a^2}} = y'.$ $\qquad \frac{dT}{dx} = \frac{V}{a} \cdot \frac{NP}{b\frac{1}{2}\sqrt{\pi}}.$

$NP = \text{area } ONP'A \div a = \frac{1}{a}\int_0^x y'dx.$



O P Q showing excess of temperature above that of the surface.
A P′R showing rate of augmentation downward.

tendency of the results of experiment seems to be toward the conclusion that conductivity diminishes as the fluid state is approached, and if we are to adopt that conclusion in this case, the quantitative effects would be a smaller totality of dissipation of heat, and a more rapid transition from cold to hot, thus bringing the nucleus nearer the surface. The time required to establish an increase of $\frac{1}{51\cdot6}$° F per foot of descent would also be increased, and probably in no small degree, depending of course upon the amount of variation which the conductivity undergoes with change of temperature. Such a modification

* The identical curve here given might still be used with a slight modification in the axis of abscissas. If instead of equal divisions of that axis, we were to take unequal ones with values determined by the equation $\kappa = f(T) = f'(x)$, the curve would be totally unchanged. To reduce it to a scale of equal parts, stretch out or shrink up, as the case may be, the whole projecting plane in the direction of x until the divisions become equal.

of Fourier's law would still further reduce the basis of the contractional hypothesis by reducing the total dissipation, and the amount of contraction which could be inferred from it.

Again, the value given to $\varkappa$ by Sir W. Thomson has been questioned, and it has been argued that rocks, porous and saturated with water, are much worse conductors of heat than those experimented upon by that philosopher, and there may be good reason for placing this value considerably lower—say at 250 instead of 400. The effect of this modification would be to move surfaceward the positions of the isogeotherms, as determined by the value of the surface rate of increase, and to extend the duration of the cooling.

Another serious quantitative modification will appear possible when we inquire as to the value of $\frac{dT}{dx}$ for places near the surface, i. e., the rate of increase of temperature per foot of descent. This is the yard-stick, by which everything else is measured. Its value is known to vary widely, being $\frac{1}{15}$ in some places, and not more than $\frac{1}{110}$ in others. Is it safe, or even proper, to take the average of all observed rates as the true and most probable one? It is a most natural inference that the higher ones are attributable to the proximity of exotic masses, are therefore accidental and should be excluded from averages. The extreme slowness with which heat is dissipated from such masses ought to prepare us for the possibility that proximity to exotic igneous masses may vitiate any result. It would seem most proper to select such values as may have been determined in places which show the minimum disturbance throughout their whole geological history. But there is a difficulty even here. The aqueous circulation below the surface, everywhere so abundant, must in some cases affect the normal temperature produced by secular cooling. While a preference is here expressed for the smaller rates, it is yet immaterial, so far as the present argument is concerned, which of the extremes be taken.

1. Given then this world of ours, once of nearly uniform temperature, 7,000° F, now exhibiting an increase of $\frac{1}{100}$ of a degree per foot of descent near the surface. What is the present thermal distribution, and what has been the period of its evolution? The epoch would be about 625 millions of years. At a depth of 300 miles the increase of temperature would be about $\frac{1}{5460}$ of a degree per foot of descent. Thence inward the total amount of cooling since the beginning would be inconsiderable: outward it would show an increasing amount, very gradual at first, but becoming more and more rapid, till it reaches the present mean temperature at the surface.

2. Or take the present surface rate at $\frac{1}{50.6}$ of a degree per foot, the other conditions being unchanged. The epoch would be about 160 millions of years, and below 140 miles the rate of increase would be inconsiderable.

3. Taking Sir W. Thomson's valuation of $\varkappa$ at 400, instead of 250, and of the surface rate at $\frac{1}{50.6}$, the epoch becomes about 98 million years, and below 150 miles the rate of increase would be less than $\frac{1}{2700}$.

4. Take $\varkappa$ at 250, and $\frac{dT}{dx}$ at $\frac{1}{200}$ at the surface: the epoch would be 2,500 millions of years, and below 600 miles the cooling may be disregarded.

That Fourier's theorem, under the general conditions given, expresses the normal law of cooling, is admitted by all mathematicians who have examined it. The only ground of controversy must be upon the values to be assigned to the constants. But there seem to be no values consistent with probability which can be of help to the contractional hypothesis. The application of the theorem shows that below 200 or 300 miles the cooling has, up to the present time, been extremely little: were it otherwise, the present rate of increase of heat per foot of descent would be lower than the lowest reasonable estimate, unless indeed new evidence can be brought up to show that this ratio is much less than $\frac{1}{100}$, and that the present accepted mean of $\frac{1}{50}$ to $\frac{1}{60}$ is the result of unknown perturbations, tending to exaggerate its value many times. At present, however, the unavoidable deduction from this theorem is that the greatest possible contraction due to secular cooling is insufficient in amount to account for the phenomena attributed to it by the contractional hypothesis.

So far the discussion has taken no account of such inequalities in the process of cooling as have occurred in the form of Plutonic action. Our knowledge of this subject, especially of its history, is so obscure that any treatment we might propose to give it would be purely speculative, and none but the simplest and apparently most necessary inferences from it could be justified. It is certain that such action would accelerate the dissipation of heat, but at the expense of what regions can be known only when we are able to locate the seat of its causation. But in any event it is not apparent that the conclusion just drawn would be sensibly affected in this quarter.

There is, however, a possible source of diminution of volume, other than the contraction directly due to the fall of temperature, which may be alluded to. A change of temperature is, in most cases, followed by changes in chemical relative potentials developing new affinities. In localities where molecular mobility is possible, these changes may give rise to new compounds of higher average density. But it is not clear how such changes could take place at depths greater than those assigned as the limits of sensible cooling, and such an assumption must appear gratuitous until supported by evidence. The want of such evidence compels us to confine possible changes of density (so far as strict reasoning is concerned) to horizons not lower than two or three hundred miles. Although no estimate can be made of the contraction of this portion, it is probably safe to say that its volume cannot have diminished so much as one-tenth; and if we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem. By far the larger portion of this contraction must have taken place before the commencement of the Paleozoic age. By far the larger portion of the residue must have occurred before the beginning of the Tertiary; and yet the whole of this contraction would not be sufficient to account for the disturbances which have occurred since the close of the Cretaceous. In all mountain regions the disturbances of the strata, which are supposed to be due to tangential compression, are so great, that, in order to account for their plication by this hypothesis, we should be compelled to assume a contraction of some circles of latitude, since the commencement of the Permian epoch, amounting to many hundreds of miles. But when we examine the Laurentian rocks wherever found, their excessively disturbed condition must utterly prohibit the belief that it is the result of secular contraction of the interior. Bearing in mind that a shrinkage of one-fifth of linear dimensions implies an increase of 95 per cent in mean density, and that according to this hypothesis such increase is zero at the surface, it puzzles the imagination to conceive what must have been the condition of the earth mass while the Laurentian sediments were accumulating, if we are to assume that their present distortion is due merely to secular contraction.

The determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated. It has been assumed that if a contraction of the interior were to occur, the yielding of the outer crust would take place at localities of least resistance. But this could be true only on the assumption that the crust could have a horizontal movement in which the nucleus does not necessarily share. A vertical section through the Appalachian region and westward to the 100th meridian shows a surface highly disturbed for about two hundred and fifty miles, and comparatively undisturbed for more than a thousand. No one would seriously argue that the contraction of the nucleus had been confined to portions underlying the disturbed regions: yet if the contraction was general, there must have been a large amount of slip of some portion of the undisturbed segment over the nucleus. Such a proposition would be very difficult to defend, even if the premises were granted. It seems as if the friction and adhesion of the crust upon the nucleus had been overlooked. Nor could this be small, even though the crust rested upon liquid lava. The attempts which some eminent geologists have recently made to explain surface corrugation by this method clearly show a neglect on their part to analyze carefully the system of forces which a contraction of the nucleus would generate in the crust. Their discussions have been argumentative and not analytical. The latter method of examination would have shown them certain difficulties irreconcilable with their knowledge of facts. Adopting the argumentative mode, and in conformity with their view regarding the exterior as a shell of insufficient coherence to sustain itself when its support is sensibly diminished, the tendency of corrugation to occur mainly along certain belts, with series of parallel folds, is not explained by assuming that these localities are regions of weakness. For a shrinkage of the nucleus would throw each elementary portion of the crust into a state of strain by the action of forces in all directions within its own tangent plane. A relief by a horizontal yielding in one direction would by no means be a general relief. We may conceive these forces to be resolved into two sets of components respectively parallel to the two coördinate axes. In the case under consideration the diminution of the intensity of one set of components by weakening the supports from which it thrusts would have the effect of increasing the intensity of the other set of components at right angles to the weakened set. No relief could take place unless it be a relief in all directions. The case in question is not that of the cylindric arch, but nearly that of the dome; and if a collapse is to occur, every terrestrial great circle must contract equally and simultaneously; otherwise great deformations of the earth's normal figure would result. The plications of the Paleozoic rocks do not conform, either in Europe or America, to the consequences here affirmed. These disturbances are localized in long and rather narrow belts, and if they truly represent contraction of certain great circles, then such contraction must have been enormous in arcs perpendicular to the axes of plication, and very little in arcs parallel thereto. Still more discordant is the contractional hypothesis with the Tertiary plications. From Cape Horn to the Behrings Sea is a continuous belt, very narrow for most of the distance, but extremely disturbed throughout. If the parallels of latitude perpendicular to this mighty range have alone contracted so greatly at this axis, how can we reconcile such an assumption with the fact that the earth's present figure is so nearly an ellipsoid of revolution with an eccentricity due to its mean density and angular velocity. It is here that the analogy of the withered apple fails. If it is corrugated irregularly by shrinkage, it fails to preserve its original figure; and conversely, if it preserves its original figure, it must be corrugated uniformly.

A comparison of this hypothesis with details of surface structure would involve an interminable discussion. The diversity of details is so great that only the most prominent and persistent ones could be properly selected; and as it is intended to bring these into relation with other propositions, their discussion will be omitted here.

ART. XII.—*Descriptions of two new Species of Fishes from the Bermuda Islands;* by G. BROWN GOODE.

IN a collection of fishes, including some seventy species, made at the Bermudas in the spring of 1872, I find two forms apparently undescribed, descriptions of which are given below. As the marine life of the Bermuda group is essentially West Indian in its character, these species may be regarded as additions to the icthyological fauna of the West Indies.

1. *Diapterus Lefroyi*, sp. nov.

This species belongs to the genus *Gerres* as defined by Dr. Günther. It is distinguished from all other members of the genus and family by its relatively greatly elongated form. The body is fusiform, compressed, its greatest height, at the thoracic region, being a little less than one-fourth (·23) of the total length and a little more than one-fourth (·27) of the length without caudal (·89): in *Diapterus aprion*, the most elongated of the species hitherto described, the greatest height is but one-third of the length. The height of the body is uniform under the spinous portion of the dorsal, sloping gently and at a nearly uniform angle above and below to the middle of the caudal peduncle. The height of the body behind the dorsal (·10) is less than one-half, that of the least height of the tail (·06) is one-fourth of the greatest height of the body.

The scales are large, measuring ·03 and ·04 in height and ·02 and ·03 in length: they form about forty-five oblique transverse rows between the head and the caudal, four and one-half longitudinal rows between the back and the lateral line and ten between the lateral line and the belly.

The length of the head (·22) equals the greatest height of the body and is double the greatest width of the head (·11): the height at the pupil (·14) is double the width of the interorbital space (·07). The length of the snout (·06) equals the length of the operculum (·06); when the mouth is protruded the length of the snout is doubled (·12) and when retracted the posterior extremity of the intermaxillary process extends to the vertical through the center of the pupil. The nasals are very prominent and the nostrils are nearer to the orbit than to the extremity of the jaw.

The orbit is circular, its diameter (·08) one-third the length of the head. The origin of the dorsal is slightly behind that of the ventrals, its distance from the snout (·31) twice the length of its base (·16). The dorsal spines are graduated nearly in the proportion (I=·02; II=·12; III=·11; IV=·10; V=·09; VI=·085; VII=·0725; VIII=·05; IX=04). The notch between the spinous and soft portions is very deep and the connecting membrane barely perceptible. In the soft dorsal the fifth ray is the longest (·09) and equals the fifth spine, the succeeding rays diminishing regularly to the last, which equals the ultimate spine (·04); the length of its base (·20) is greater than that of the spinous dorsal. The anal begins behind the center of the body (·56); the first spine is very short (·01), one-fifth the length (·05) of the second, which is slender; the first ray is the longest (·08), the succeeding rays regularly diminishing in length to the last (·03). The lobes of the caudal are equal, the outer rays in length (·21) five times the inner ones (·04). The extremity of the pectoral reaches the vertical from the last dorsal spine: its distance from the snout at the axilla (·25) is nearly equal to the height of the body. The ventral spine resembles the fifth dorsal spine in shape and size; the length of the longest ray (·11) slightly exceeds one-third of the distance from the snout to the ventral axilla (·30); the axillary appendage consists of four lanceolate scales, the first and longest as long as the last ventral ray.

Color: silvery, with a bluish tint above; axils of the pectorals and extremity of snout brownish.

Radial formula, D. IX, 10. A. II, 8. P. 12. V. I, 5. C. 3, 9, 9, 3.

The unit of measurement used above is one-hundredth of the total length, which in an average specimen is 7·29 inches (M. O. ·185). The species is common in the protected inlets about the islands in company with the "shad" (*Diapterus gula*), from which it is distinguished by the name "long-boned shad:" they are in demand for bait and are easily seized in large quantities. I take pleasure in dedicating the species to his Excellency, Maj.-Gen. J. H. Lefroy, F.R.S., Governor of the Bermudas, who while doing so much for the social and political welfare of the islands, is taking an active part in adding to our knowledge of their natural history.

2. *Engraulis chœrostomus*, sp. nov.

This species closely resembles *Engraulis surinamensis* (Blkr.) Gthr. differing from it, however, in several respects.

The height of the body (·16) is a little more than two-thirds of the length of the head and is contained six times in the total length and a little more than four times in the length to end of middle caudal rays (·90): the height at the ventrals is less (·13). The scales are large, in thirty-eight oblique rows between the head and the caudal.

The length of the head (·22) is less than one-fourth of the total and is double its height at the pupil (·11): its greatest width (·08) is about one-third of its length. The orbit is nearly circular and its diameter (·05) equals the length of the snout (·05) and the width of the interorbital area (·05). The snout projects far beyond the lower jaw, whose extremity just passes the vertical from the anterior margin of the orbit. The maxillary is dilated above the mandibular joint, rather tapering behind, and extends to the gill opening. The gill-rakers are fine, setiform, not longer than the eye (·05), about 25 on the lower branch of the outer branchial arch.

The origin of the dorsal fin is in front of the middle of the body (·45 from snout), and directly above the extremities of the ventrals: the length of the first ray (·06) is half that of the second (·12), which nearly equals the length of the base (.11).

The origin of the anal is at the middle of the body (·51 from snout) and below the posterior dorsal rays: its greatest height (·11) nearly equals that of the dorsal.

The length of the middle caudal rays (·08) is two-fifths of the outer rays (·20). The length of the pectorals (·11) equals the length of base of dorsal (·11), the extremities reaching to the origin of the ventrals. Length of ventrals (·09): distance from snout (·35).

Color: back and sides brownish, belly white; a broad, clearly defined lateral band of silver as wide as the diameter of the orbit (·05).

Radial formula D. 13–14. A. 23–24. Length 2·68 inches (M. O ·068).

Common in schools in Hamilton Harbor, where it is taken for bait in cast nets. Its enormous mouth has given it the name of "hog-mouth fry."

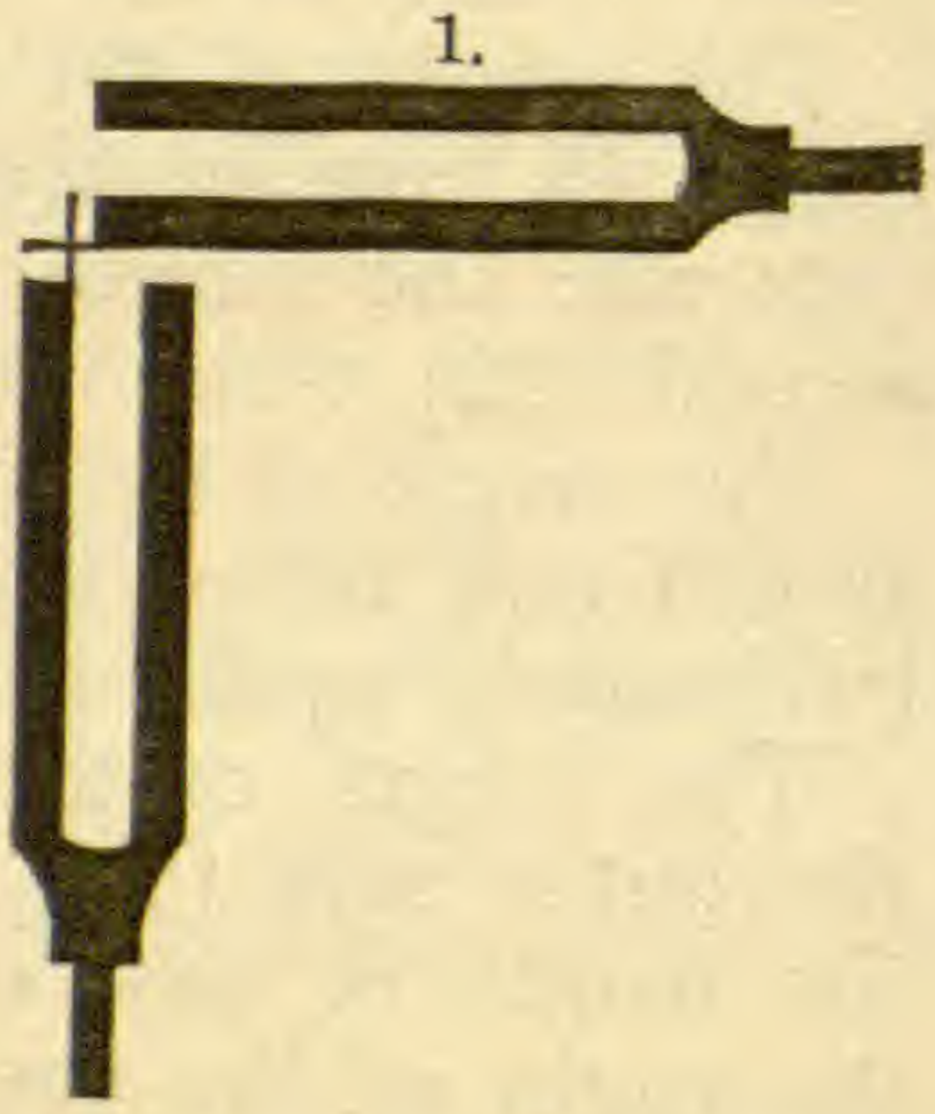
The types of these descriptions are preserved in the U. S. National Museum in Washington and the University Museum in Middletown, Conn.

ART. XIII.—*On an optical method of studying the Vibrations of Solid Bodies;* by OGDEN N. ROOD, Professor of Physics in Columbia College.

IN the year 1855 Lissajous described a beautiful mode of bringing two tuning-forks into exact unison, which since that time has been of great use for the production of exact copies of standard forks executing a known number of vibrations in a second. This method is now so familiar that farther allusion to it would be superfluous, and I pass on to the description of an analagous proceeding, which if it falls a little short of that of Lissajous in point of exactitude, is, on the other hand, more easy of execution and more generally applicable to the study of the vibrations of solid bodies of very different forms. The nature of the method referred to will best be illustrated by a few examples.

Tuning-forks.—Let us suppose that it is required to ascertain whether two forks are in unison, or to determine the difference in the number of vibrations executed by them in a second. For this purpose a short piece of fine steel wire is attached to each of the forks and they are supported in positions so that their vibrations shall be at right angles to each other, as indicated in fig. 1. The wires may have a diameter of one or two-tenths of a millimeter, or even less, and are to be attached with the least possible amount of soft wax or varnish. They may be brought quite near to each other, or may, if necessary, be several inches apart. If the forks are now set into vibration and the intersection of the wires viewed against a bright background with a small telescope, it will be seen that an optical figure is developed, which is partly due to the same well known conditions that give rise to the figures of Lissajous, and partly to the circumstance that the wires move with less velocity when near their maximum deviation from the line of rest. Hence, if the difference in phase is 0, an appearance like fig. 2 is produced, which changes into fig. 3 when the difference in phase has increased to one-half a complete vibration. Fainter indications of the same figures are shown in all cases, except when the difference in phase is one-fourth, three-fourths, &c., of a vibration, or nearly so. This figure, then, is characteristic

1.



2.

3.

of forks in unison, and by proper arrangement of the light and telescope can be made tolerably sharp and distinct. If the forks are actually in unison, the above figure is pretty certain to be produced after one or two trials, and the fact of its constancy will then, on the other hand, be the evidence of perfect unison. If the forks are not exactly in unison, fig. 1 will, as stated, after some time change into fig. 2, and the number of seconds necessary for this change will measure the interval required by one of the forks in gaining or losing half of a complete vibration.

When the two wires are only a few millimeters apart, it is evident that the telescope will furnish distinct images of them both without any especial contrivance, but as a general thing in the application of this method, the intervening distance between the vibrating bodies will be greater, giving rise to an obstacle in making the focal adjustment. This is readily met by limiting the aperture of the object-glass. The focal length of the object-glass of the telescope used by me was 120 millimeters for parallel rays, and when the aperture was reduced to two millimeters, sufficiently distinct vision of both the wires could be obtained, even when their distance apart was several centimeters, and the telescope yet so near to them as to admit of the performance of all the manipulations by the observer without rising. With this limited aperture, the light from a white cloud answered quite well.

If the forks differ by an interval of an octave, an almost equally distinct and well marked figure will be produced, such as is seen in figs. 4 and 5, which represent the characteristic appearances in this case. This figure is quite as useful for purposes of investigation as that of unison. Somewhat less distinct and more complicated figures are given by the quint, the duodecime and the double octave. It is a little more difficult to distinguish them from each other by a glance, and they are less sharply defined than those of unison or of the octave, which should always have the precedence for experimental determinations; still the other figures admit of being employed when necessary. The relation of all these to the corresponding figures of Lissajous is evident on inspection.

4. 5.

Tuning-forks and vibrating cords.—From the foregoing it evidently is easy with this method to bring a vibrating string into unison with a given tuning-fork, or to adjust it so that the interval shall be a quint, octave, twelfth or double octave, above or below. It is also easy to ascertain the number of vibrations made by a string in a given case, by the aid of a bridge and a properly selected fork making a known number of vibrations, the string being shortened till it furnishes one of the above mentioned figures, and executes hence a known number of vibrations, after which the number of vibrations made by its whole length can readily be calculated by a well known law.

Vibrating cords.—To bring two cords into unison, or to produce one of the above mentioned intervals, it is not at all necessary that they should actually vibrate at right angles to each other: in my experiments I simply placed between the strings on the monochord a cork cut at an angle of 45°, and supporting at this angle a small piece of looking-glass of good quality. The reflected and vertical image of the farther string was then seen in the telescope crossed by the horizontal image of the nearer string, and the mirror being turned so as to reflect at the same time light from the sky, all the conditions were fulfilled. It is evident that this arrangement furnishes an excellent experimental mode of studying the laws of vibrating strings, of comparing them with theory, and examining into the deviations caused by stiffness and unequal caliber.

In all the experiments with strings I employed the differential *sonomètre* of Marloye as constructed by König, so well known for his excellent workmanship, merely adding a clamp to one of the bridges for the purpose of holding down the cord. But this instrument, although admirable when the ear is used as a test, was found not to possess a delicacy sufficiently great to enable the experimenter to take full advantage of the sensitiveness of the method here described. The arrangement for varying the tension was hardly sufficiently fine, and a screw movement attached to the bridge would have been a great advantage. It was also found difficult to maintain a given tension with perfect exactitude for any length of time. Probably some modification of the older arrangements of Weber or Fischer would be found to answer better.

Vibrations of rods, bars and plates.—It is evident that rods or bars supported at one extremity or at two nodes, and provided with fine terminal wires, can by this method be brought into unison, or have one of the above mentioned intervals established between them. A preferable mode, however, is to study them in connection with the monochord and a tuning-fork. The entire string of the monochord is first brought into unison with a tuning-fork, or some definite interval established; the cord and rod or bar are then combined at right angles, and the bridge moved till unison is again effected, when it is possible to calculate the number of vibrations actually executed by the bar or plate. As an example, I give the result of two rough experiments, which would have been rendered more accurate by the aid of an assistant.

The cord, one meter in length, was brought into unison with an Ut_1 fork, and hence executed 64 double vibrations per second. It was afterward combined with a plate of glass 330 millimeters long and supported at the two nodes. Five determinations with the bridge were made, and after bringing the cord a second time into unison with the fork, repeated.

847·	843·
846·9	846·5
847·7	846·9
847·1	845·7
847·7	843·
847·28	845·02

The result then in the first case was 75·535, in the second 75·738 vibrations per second.

An experiment with another piece of glass cut from the same plate and of nearly the same length gave, with two determinations, 77·811 and 77·717 vibrations per second.

Vibrations of bells.—If the fine wire is attached to one side of a bell, the number of vibrations executed by the bell can readily be obtained with the monochord in the manner already indicated. A bell-glass was set in vibration by a bow, and combined with a string one meter in length, which was an octave lower than a Sol_2 fork, and hence made 96 double vibrations per second. The bridge was adjusted till the string was an octave lower than the bell-glass when sounding its fundamental note. The results are given below:

802·	803·
802·7	803·5
804·5	804·
804·2	801·7
803·5	802·2
803·38	802·88

The number of vibrations obtained in first case then was 238·99, in the second 239·14, with a difference of ·15 of a single vibration.

In experiments of this kind it is plain that the accuracy attainable depends to a great extent on the time during which the vibration of the two bodies can be maintained; still it is not admissible to maintain the cord in vibration by the help of the *bow*, as the slightest variation in the pressure causes the figure at once to change. On the other hand, the bow is useful in bringing a short string into unison with a fork, &c., merely for the purpose of assuring the experimenter that the figure actually to be used, and furnished by a *greater length* of the string, is really that of the lower octave, twelfth or double octave. In the experiments with the monochord the string was simply drawn aside with the feather-end of a quill, and then left to itself. If this and the vibration of the plate, &c., be effected by the aid of an assistant, the time for observation at the telescope is increased and more accurate results than those above given can be obtained. The changes in the plane of vibration of the string are hardly a source of embarrassment when the unison or octave figures are employed, but become so with the others which are more complicated, in the case where it is desired to count the changes occurring in the figure during a certain number of seconds.

Vibrating membranes I find can readily be studied in this way by attaching to them a small piece of fine wire bent with two right angles, and using them in connection with the monochord or a tuning-fork.

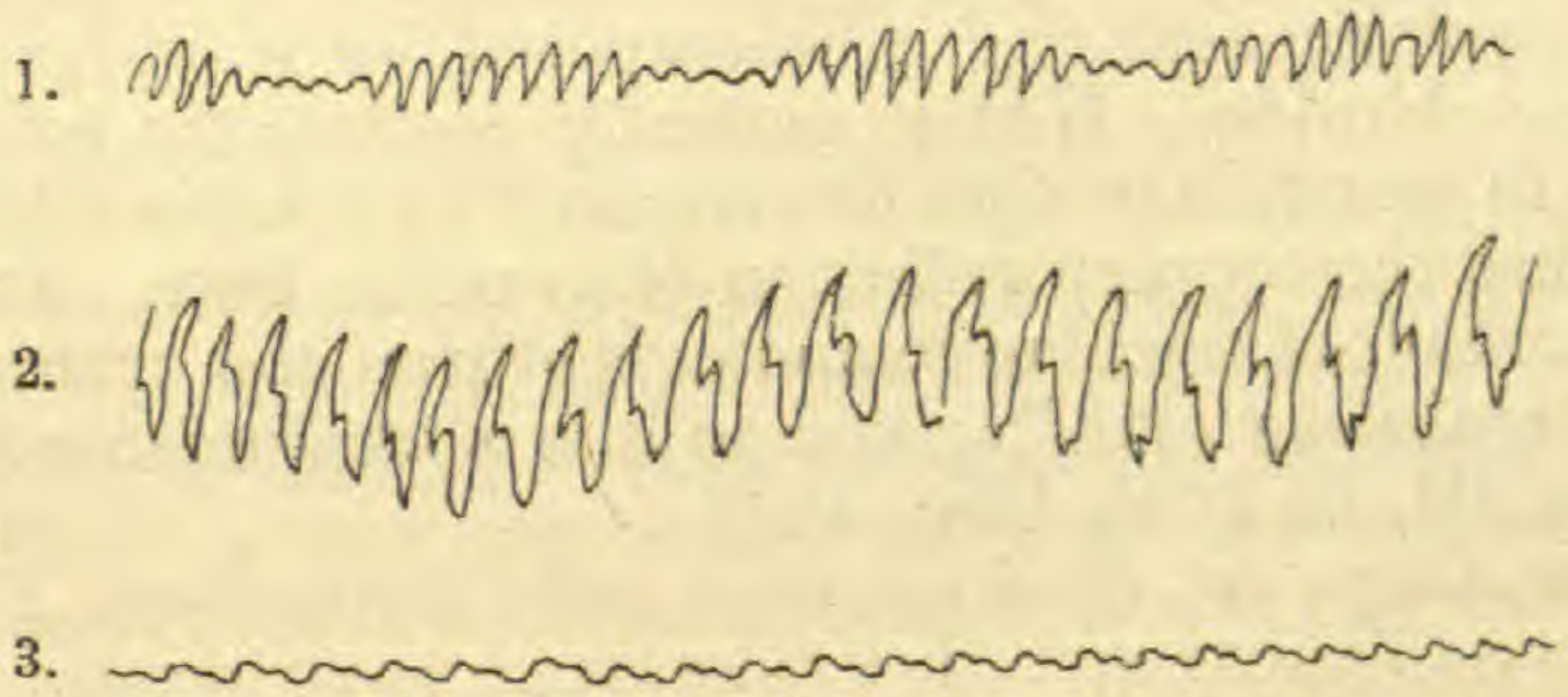
Finally, I may add that the more important of these figures may easily be rendered visible to a large audience. Wires about a millimeter thick are attached to two tuning-forks placed in front of a magic lantern; an image is formed on the screen with the aid of a lens of about eighty millimeters focal length; the figures are then well shown, along with certain of their details not particularly mentioned in this article.

New York, May 21st, 1874.

ART. XIV.—*The Phonautograph;* by CHAS. A. MOREY.

ALMOST every collection of acoustical apparatus includes one of Leon Scott's phonautographs, but I think I am right in saying that very little use is ever made of them, other than for their explanation. The curves being drawn upon blackened paper cannot be projected, and in most cases they are of so small an amplitude that they require very close inspection for their analysis. As was found at the time of its invention, the great difficulty lies in the fact that the principal motion of the style is a longitudinal one instead of a lateral one. All these objections are obviated, and the instrument rendered extremely useful, by the following simple device, which may be readily applied to any one of them. Instead of attaching the style directly to the membrane, it is attached to the end of the long lever. This may be of any length (the one used was about twelve inches) and is made very slender and light, either of deal or of a stiff straw. This is attached at the end to the brass ring which holds the membrane by a simple hinge of goldbeater's skin, and also to the membrane itself by a short, flexible bristle, or piece of broom-corn. This is inserted into the lever, and is fixed with a drop of glue; the other end is fixed to the membrane as the style usually is, either by a drop of glue, or sealing wax, the former being preferable. Now it is evident that the former longitudinal motion of the style will impart a lateral motion to the lever, and thus to the style at the free end of it; and also that this motion will be greatly magnified. The curve can now be drawn upon a plate of smoked glass, as in the well known experiments with tuning forks, the plate being drawn under the style in the direction of the length of the lever. The whole attachment can readily be made so light as to encumber the membrane but very little.

The following curves will serve to show the advantage gained over the usual attachment, as well as to suggest the



numerous uses to which the instrument can be put. The first one is the toungue trill, or the German *r* prolonged; the second is the result of a note upon the trombone; and the third is the *oo* in mood. The fact that a difference in the intensity of the sound shows itself so very plainly in the curves (2), seems to suggest something in the way of quantitative work in that direction, but it is yet a question as to the delicacy of the instrument.

In each of the cases above, the sound was made in front of the parabolic condenser, but at some little distance from it—two or three feet.

Mass. Inst. Technology, Boston, May 24, 1874.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *The so-called Antimony blue.*—A year or more ago, a new coloring matter was introduced into commerce under the above name, prepared by precipitating a solution of antimony in aqua regia by potassium ferrocyanide. Inasmuch as this re-agent is commonly used by chemists to detect the presence of iron in antimony, it is obvious that the blue compound is not ordinarily thrown down by it in solutions of this metal. Krauss has therefore investigated the conditions of its production. He finds that on boiling a solution of tartar-emetic in concentrated hydrochloric acid, to which some ferrocyanide of potassium is added, the same blue substance is produced. On analysis no antimony could be detected in it. Believing therefore, that the antimony was in no wise essential, Krauss boiled the ferrocyanide in concentrated hydrochloric acid alone, and obtained the blue compound. In further proof of this assumption, he found that the antimony could be satisfactorily replaced by mercury. The use of antimony accelerates the formation of the precipitate. The blue substance is soluble in hydrochloric acid and is decomposed by it on boiling, producing ferric chloride. Water gradually restores the color. It is not soluble in water, is at once decomposed by caustic alkalies, and in powder has the coppery reflection of prussian blue. It is therefore only another of the blue compounds of iron and cyanogen, and the name "antimony blue," given to it is a misnomer.—*Moniteur Scientifique*, III, iii, 1095, Dec., 1873. G. F. B.

2. *On the Alloys of Hydrogenium with Palladium, Potassium and Sodium.*—Troost and Hautefeuille have sought to determine whether palladium-hydrogenium is a mechanical or a chemical combination, by studying the tension of the hydrogen evolved from it at various temperatures. Chemical compounds having a gaseous constituent, suffer, as is well known, a partial decomposition under the influence of heat, the tension of the gas evolved being invariable for each temperature, and having no relation to the amount of the undecomposed product; while on the other hand, solutions of gases in solids, as hydrogen in platinum black, for example, being mechanical mixtures, evolve, when heated, a gas whose tension, at the same temperature, is variable, being dependent on the degree of saturation of the absorbing substance. Palladium, charged with hydrogen electrolytically,* was placed in a tube of glass, connected at one end with a manometer, and at the other with a Sprengel pump. Operating at the temperature of 100° C., with palladium charged in different degrees, the results show: 1st, that when the volume of the absorbed hydrogen is

* Since palladium thus saturated loses hydrogen at the ordinary temperature, and since on exposure to the air, it heats, from the oxidation of this gas, the authors adopted the plan of placing it at once in water free from air, and heating this to boiling, the gas evolved being measured. It was then treated as above.

above 600 times that of the palladium, the tension decreases very rapidly with the decrease in the amount of hydrogen, a fact characteristic of a simple solution. 2d, that the tension becomes constant, the proof of chemical union, so soon as the volume of hydrogen reaches 600 times that of the palladium; a volume which corresponds to one-half an atom of hydrogen to an atom of palladium. From this limit, palladium-hydrogenium behaves like a definite compound, capable of true dissociation, the gaseous tension henceforth depending alone on the temperature. The authors have prepared a table of the tensions of dissociation at different temperatures, from which it appears that the compound Pd_2H evolves no gas at ordinary temperatures and that at 130°–140°, its tension is equal to the atmospheric pressure; hence the compound cannot be formed at temperatures superior to this, except under pressure. Moreover, the authors have proved that Pd_2H is capable of dissolving hydrogen, the quantity depending on its physical state.

In a second paper, Troost and Hautefeuille give the results of their investigations upon the definite compounds which potassium and sodium form with hydrogen. The potassium was placed in an iron boat, in a glass tube connecting by a T piece with a manometer on one side and a three-way cock on the other, by means of which the tube could be exhausted, or filled with pure and dry hydrogen. This tube could be heated to any desired temperature and kept there for any length of time. The authors found that no absorption took place when the potassium was melted, nor below 200°; at 290°, 2·5 grams required 250 hours for saturation; while at 350° to 400°, the absorption goes on much more rapidly. Potassium-hydrogenium thus prepared, is a brittle substance, having the crystalline grain and the luster of silver amalgam, and appearing like a true alloy. It may be fused in hydrogen or in a vacuum without change. In the air it inflames spontaneously. At 200° in a vacuum it begins to dissociate, the tensions being measurable between 330° and 430°, and increasing very rapidly with the temperature above 370°, becoming 760 mm. at 411°. Hence it cannot be formed at this temperature, except under pressure. The best temperature for its formation is 300°. It dissolves hydrogen, the quantity varying with the temperature and pressure; at 300° and 760 mm. it dissolves 40 volumes of this gas. On expelling the excess of gas, a point determined by the agreement of the tension of the hydrogen with its dissociation-tension for that temperature, the compound gave on analysis 126 volumes of hydrogen to one of potassium, the formula K_2H requiring 124·6. Sodium absorbs hydrogen actively between 300° and 421°, the tension at this latter point being 760 mm. At ordinary temperatures sodium-hydrogenium is soft, but becomes brittle and crystalline just before fusion, so that it may be pulverized. It has the luster of silver, is a little more fusible than sodium, loses no hydrogen on fusion and is only slowly alterable in the air. Its density taken in naphtha is 0·959, that of sodium being 0·970. Its dissociation follows the same laws as the potassium compound, the tensions being a little less for the same temperatures. It absorbs 3 or 4 volumes of hydrogen at 400° and 760 mm. Analysis gave 237 volumes of hydrogen; Na_2H requires 238. Lithium, heated to 500° in hydrogen under 760 mm., absorbs 17 times its volume of this gas. Thallium, under similar conditions, only three times.

In a third paper, Troost and Hautefeuille give the results of their determination of the density of this solidified hydrogen. Regarding, with Graham, these compounds as alloys of metallic hydrogen, and assuming that they, like the alloys examined by Matthiessen, are formed without condensation, it is easy to calculate the density of the hydrogen as contained in them. Graham's number 0·733, for the density of hydrogenium, is too high, since, as we have seen, he operated on a substance which disengaged hydrogen at ordinary temperatures and was not a definite compound. The authors, taking their compounds Pd_2H and Na_2H, determined accurately their densities. The density of the former they found to be 11·06, that of the palladium being 12. From these data, the density of hydrogenium, as calculated, is 0·62. The density of the Na_2H is 0·959, that of the sodium being 0·970. This gives 0·630 as the density of hydrogenium, a remarkably close accordance. The mean is 0·625, a density but a little superior to that of lithium. The atomic volume of hydrogenium, as deduced from the above data, is the smallest known, being 1·6.—*C. R.*, lxxviii, 686, 807, 968, March, April, 1874.

G. F. B.

3. *On a Lactic acid of the Allyl series.*—In the hope of reducing trichlor-lactic acid—obtained by heating chloral-cyan-hydrate with hydrochloric acid—to monochlor-lactic acid, and of effecting in this way the synthesis of glyceric acid, Pinner treated it first with zinc-dust and water, and subsequently with zinc and dilute hydrochloric acid. But the difficulty of separating the products from the large quantity of zinc chloride formed, rendered this method unfruitful. The acid was then converted into its ethyl-ether and this, treated with zinc and hydrochloric acid, acquired soon a peculiar odor and became converted into a volatile oil, which upon fractioning, boiled constantly at 145°–146°. Analysis showed that it was not monochlor-lactic acid, but that it was ethyl monochloracrylate; not only the two chlorine atoms being replaced by hydrogen but a molecule of water abstracted in addition, thus:

$$CH_2Cl.CHOH.COOC_2H_5 - H_2O = CHCl{=}CH{-}COOC_2H_5.$$

Ethyl monochloracrylate is a mobile colorless liquid, boiling at 146°, having the odor of the allyl series, and irritating the mucous surfaces. Boiled with barium hydrate, it dissolves gradually, exchanging its chlorine for hydroxyl, and yielding the well-crystallized barium salt of a new lactic acid which Pinner calls acryl-lactic acid, and which has the formula $C_3H_4O_3$ (or CHOH.CH.COOH); thus differing from lactic acid by H_2 less. The free acid was prepared from the barium salt, but for want of material was not fully examined. Ethyl chloracrylate, treated with alcoholic ammonia, gives rise to an alanin, crystallized in fine needles. It has the formula $CH(NH_2)$=CH--COOH, and differs from ordinary alanin not only by having H_2 less, but also by being nearer to ethylene-lactic acid in constitution than to ethylidene-lactic acid, from which alanin itself is derived.—*Ber. Berl. Chem. Ges.*, vii, 250, March, 1874. G. F. B.

4. *Occurrence of Leucin in the fresh juice of the Vetch.*—Both leucin and tyrosin stand in very intimate relations with the albuminates. Their frequent occurrence in the body, their appearance in the urine in certain diseases, their rapid production in the peptonizing of the albuminates, all go to show the importance of these relations. Now, since asparaginic acid occurs among the decomposition products of leucin, and since asparagin appears during the sprouting of the papilionaceæ and disappears later while protein bodies are forming, it occurred to GORUP BESANEZ to examine, for leucin, the juice of the common vetch, grown in rich earth and in the dark. This juice, freed from albuminates by boiling, was dialyzed, the diffusate being evaporated till the asparagin crystallized out. The mother liquor, on further concentration, deposited a granular substance, which formed crusts on the surface of the liquid, and which had all the appearance of leucin. Repeated recrystallization from boiling alcohol gave it pure, in the form of small spheres of sharp outline and radial in structure. It reactions proved it to be leucin. The author believes with Dragendorff, that the substance obtained by Reinsch from the juice of *Chenopodium album*, and called chenopodin, is really leucin.

In a second paper, Gorup Besanez shows that this occurrence of leucin in the juice of the vetch is constant. The shoots were grown in wet sand and in a dim light, and were examined after two weeks growth, when they were 12–15 cm. long—after three weeks growth, length 20–25 cm.—and after four weeks growth, length about 25 cm. The leucin appeared to exist in inverse ratio to the asparagin. In these experiments the juice, after separation of the protein bodies by coagulation, was mixed with 90 per cent alcohol in excess, thus precipitating, beside other matters, almost all the asparagin. The filtrate on concentration deposited first asparagin, then the leucin; a sugar-like substance, reducing the copper test, remaining in the mother liquor. Since leucin cannot be detected in the ripe seeds of the vetch, and legumin, which these contain, does not exist in the etiolated sprouts, the author believes that the leucin is an intermediate stage in the production of the albuminate. Althea roots and the roots of *Scorzonera hispan.*, were examined for leucin, but without result.—*Ber. Berl. Chem. Ges.*, vii, 146, 569, Feb., April, 1874. G. F. B.

5. *On Protamine, a new Base from the Spermatozoids of the Rhine salmon.*—MIESCHER has examined the chemical character of the spermatozoids of the Rhine salmon, which at the time of maturity, in November, may be obtained pure in considerable

quantity either from the milt itself or from the glands secreting it. The composition of these spermatozoids, though peculiar, is quantitatively quite constant, containing of lecithin 7·5 per cent, cholesterin 2·2 per cent, fat 3·5 per cent, albuminates 10·3 per cent, nuclein—the principal constituent, containing 9·6 per cent of phosphorus—48·7 per cent. This latter substance, now for the first time obtained pure, is not free in the sperm; it exists there as an insoluble compound of an organic base, protamine. To prepare this, the spermatozoids are extracted with hot alcohol to remove fat, lecithin, etc. The residue may then be treated with hydrochloric acid and precipitated with platinic chloride; or with nitric acid and thrown down by mercuric nitrate. On decomposing the precipitate with hydrogen sulphide, the hydrochlorate or nitrate of the base is obtained. Both these salts crystallize with difficulty on slow evaporation in prisms probably rhombic. They are easily soluble in water, difficulty so in alcohol, insoluble in ether. They have a peculiar taste, which is astringent, faintly sweet and at the same time bitter. Evaporated with nitric acid a yellow spot is left, which on adding sodium hydrate, becomes a beautiful red, passing into violet on warming. The free base reacts alkaline. Analysis fixes its formula as $C_9H_{20}N_5O_2(OH)$. It constitutes 26·8 per cent of the dry spermatozoids. From the testicles of a single large salmon in October, 20–30 grams of protamine may be obtained.—*Ber. Berl. Chem. Ges.*, vii, 376, April, 1874.

G. F. B.

6. *On the Aqueous lines in the Solar Spectrum at high altitudes.*—Crocé-Spinelli and Sivel, during their balloon-ascent of March 22d, were furnished by Janssen with a small spectroscope for the purpose of observing the solar spectrum at high altitudes. They ascended from Villette in a balloon of 2800 cubic meters capacity, at $11^h\ 34^m$ in the forenoon, and reached their highest point at $1^h\ 30^m$, the barometer standing at 30 cm., indicating a height of 7300 meters. The temperature, which at starting was 13°, fell to −22°. The descent was safely accomplished at $2^h\ 12^m$. The spectroscopic observations were to be directed specially to the two dark bands on either side of the D lines, produced, as is well known, by the vapor of water. Janssen attributed them to terrestrial absorption, and hence maintained that they ought to disappear at a high elevation. Secchi, on the contrary, believing that the aqueous vapor producing the absorption existed on the sun, argued that they must persist at any altitude. The balloon observations fully confirmed Janssen's view. At 5500 meters, the band on the right of the D line disappeared, and at 7000 meters, that on the left become invisible. The lines E and F were more decided than at the sea level. The red of the spectrum became darker, so that B and C were perceived with difficulty. Atmospheric glare was so much reduced that at 6000 meters, at 180° from the sun, only the yellow of the spectrum could be seen and that without lines. The observations given were made at an angle 5° to 7° from the sun.—*C. R.*, lxxviii, 946, April, 1874.

G. F. B.

7. *Apparent Adhesion.*—M. STEFAN designates by this name the phenomenon that when two plates are laid one upon the other they cannot be again separated without the expenditure of a force. This phenomenon has been hitherto conceived as conditioned by adhesion—that is, by the molecular forces between the particles of the two plates; and experiments have been made for the purpose of determining statically its amount.

In this phenomenon, however, we have not to do with a static, but with a dynamic problem. The experiments made by the author showed that the separation of the two plates can be effected by any force whatever; only the time in which the distance of the plates is changed a measurable quantity by the action of such a force is the greater the smaller the force.

Simultaneously with the commencement of the action of a separating force the distance of the plates commences also to increase; yet the motion is very slow, and grows ever quicker with increasing distance. The apparent adhesion is much greater when the plates are under water or another liquid instead of in air. The distance of two plates of 155 mm. diameter, immersed in water, amounting at first to 0·1 mm., increases, in consequence of the continuous pull of 1 gram, 0·01 mm. in $1\frac{1}{2}$ minutes, 0·1 mm. in 7 minutes. From this it is intelligible how, limiting the observation to a short time, one may be misled to the assumption of a static equilibrium. The author measured, in his experiments, the times which elapsed while a given initial distance measured by a wire placed between the plates increased by a certain quantity. Between these times and the other quantities, which varied with the experiments, the following relations were found. The times are inversely proportional to the separating force; they are nearly, but not exactly, inversely proportional to the squares of the initial distance; for plates of different sizes, they are to one another as the fourth powers of the radii of the plates; for different liquids, as the times in which, under equal pressure, equal volumes of the liquid flow through a capillary tube.

From this it evidently results that with this phenomenon the question is a problem of hydrodynamics; and it is never easy, at least in general, to describe it. When the separating force begins to act, the distance between the plates receives an infinitely small increment; thereby the space limited by the plates is augmented, and the fluid undergoes a dilatation, in consequence of which the hydrostatic pressure becomes less. The excess of pressure of the exterior fluid acts in opposition to the separating force. Nevertheless, equilibrium does not ensue, because the diminution of the hydrostatic pressure between the plates has for its result a flowing in of the exterior fluid and thereby, again, a diminution of the difference of the pressures. The distance of the plates can be again increased by the separating force, and the same process repeats itself in a continuous manner.

The author gives also an approximate theoretical solution of the problem, starting from the following consideration. The *vis viva* acquired by the plates through the separating force is, on account of the great slowness of the motion, vanishingly small in comparison with the work of that force. This work must consequently have its equivalent in another work; it has it in that which is necessary for maintaining the flow of the liquid from the outside into the space included between the plates.

The equation deduced from this assumption gives again all the different laws to which the experiments have conducted. It permits us to derive from the experiments the coefficients of internal friction for the experimental fluids. If the centimeter be chosen for the unit of length, the mass of one gram as the unit of mass, and the second as time-unit, it follows that for water of the temperature of 19° C. this coefficient = ·0108, for air = ·00183, which values almost exactly coincide with those deduced from the experiments of Poiseuille, Maxwell and O. E. Meyer.—*Royal Acad. of Vienna*, April, 1874; *Phil. Mag.*, xlvii, 465. E. C. P.

8. *Effect of Magnetism on an Electric discharge in rarefied gases.*—MM. DE LA RIVE and SARASIN have in a former memoir (Archiv. des Sci., xli, 5) studied the action of a magnet on a discharge perpendicular to its axis. Since then, they have modified the experiment as follows. The two bobbins of an electro-magnet were placed end to end, and the Geissler tube inserted so as to be acted on by only one of the magnetic poles. Two cylindrical tubes were used, one containing hydrogen, the other nitrogen, at a pressure of 1 mm. or less. The induced current was produced by a large Ruhmkorff coil and a very small portion of the current directed through a galvanometer far enough from the magnet to be uninfluenced by it. Ordinarily, the discharge exhibits around the negative electrode a beautiful blue aureola extending to the sides of the tube, beyond a long dark interval, and thence to the positive electrode streaks wide apart. This appearance is completely changed as soon as the magnet is excited. When the negative electrode is acted on by the magnet, the negative aureola, which previously had a diameter equal to that of the tube, or 32 mm., and length 35 mm., is transformed into a cylinder of only 8 or 9 mm. in diameter, very luminous, extending to the positive electrode, presenting an appearance similar to the narrow positive jet observed at about 8 or 10 mm. pressure.

The same effect was obtained with a large bell, in which a central negative electrode was encircled by a positive ring. The large spherical aureola developed at low pressures, being replaced by a narrow blue jet of vivid splendor, having sometimes the appearance of a brilliant blue flame escaping from the positive electrode.

The effect of the magnet on the resistance of the gas was also very marked. In the case of hydrogen the derived current through the galvanometer was increased from 20° to 40°, when the electro-magnet was excited by 25 Bunsen cells. The corresponding change in the case of nitrogen was from 20° to 30°. In another case, with 40 cells, the change was from 12° to 55° for hydrogen, and 10° to 35°

for nitrogen. When it is the positive electrode that is acted on there is scarcely any appreciable effect. But the kind of magnetism makes no difference.

When the circuit contains several consecutive Geissler tubes, all placed in the same way, each having its negative electrode next the magnet, the effect upon the strength of the current is still greater. But if, in addition to the tube or tubes placed under the action of the magnet, there is one in the circuit out of this action, no effect upon the intensity of its current is produced by the magnet, although the modification which the appearance of the discharge undergoes in the other tubes placed over the magnetic tube, remains the same. It seems, then, that it is a special and peculiarly intense resistance, having its seat at the issue from the negative electrode, which is thus overcome by the intervention of the magnet.

A final series of experiments support this view, and show that the dimensions of the negative electrode, which notably influence the dimensions of the aureola and the resistance to the passage of the electricity, influence also the augmentation of intensity produced by the magnet in the case of an axial discharge. Working with the large bell, a very great loss or almost no augmentation of intensity was obtained according as a platinum point of wire, a small ball, or a ball 4 cm. in diameter was employed as a negative electrode.—*Bib. Univ.*, l, 41; *Phil. Mag.*, xlvii, 462. E. C. P.

9. *Frictional Electricity.*—M. L. Joulin has investigated the laws of frictional electricity with a machine suggested by the electrical phenomena often observed in belts used for transmitting power. A very great quantity of electricity is thus produced, capable of giving long brushes and sparks, of deviating a galvanometer needle, of decomposing water, and in Geissler tubes of showing the stratification of the light. A new method of measuring electric tension has been employed, dependent on the greatest distance at which a brush is perceptible on a given sphere when brought near the electrified body. The experiment was varied by using pulleys of various metals or non-conductors and by changing the velocity tension and temperature. In arranging the results there seem to be three causes influencing the production of electricity. The velocity of separation of the parts of the belt and pulley, the complex mechanical action of bending the belt, and the temperature of the two materials.—*Ann. de Chim. et de Phys.*, ii, p. 5. E. C. P.

10. *Note to Prof. Cooke's monograph on the Vermiculites;* by the Author.—In a note to my monograph on the Vermiculites (Proceedings of the American Academy, vol. ix, and this Journal, vol. vii, page 427,) I described incidentally, certain phenomena of circular polarization obtained by superposing plates of muscovite mica, which have a wide optical angle. Quite recently my attention has for the first time been called to the fact that these phenomena had been previously observed by E. Reusch (Monatsberichte d. Akad. Berlin, July, 1869), and I take this early opportunity to acknowledge his priority.

The observations of Reusch do not appear to have received that notice which their importance deserved; and, by exhibiting the same results from a different point of view, I hope I have been able to bring them into greater prominence, and to show their important mineralogical bearing.

Cambridge, July 1, 1874.

11. *Will's Tables for Qualitative Chemical Analysis.* (2d American, from the 9th German Edition.) Edited by Professor C. F. HIMES, Ph.D. 8vo. Philadelphia, 1874. (H. Carey Baird.)—All chemists are familiar with Will's Tables, which have introduced multitudes into the mysteries of chemical analysis. Professor Himes has added to this edition two new tables of his own, embodying Bunsen's valuable flame reactions. He has also made valuable additions to the opening chapter on the "Course of Qualitative Analysis," and inserted a new chapter (the last) on the "Detection of Metallic Oxides."

12. *Milk Analyses;* by J. ALFRED WANKLYN, M.R.C.S., &c. New York (Van Nostrand), 1874. 12mo, pp. 73.—This is a practical treatise on the examination of milk and its derivatives, cream, butter and cheese, giving simple and exact methods of analysis, together with the results of the author's own researches in 1871, especially into the milk and butter supplied to the London metropolitan work-houses and hospitals. It is a valuable and timely contribution to an important department of sanitary science. The author has greatly simplified and improved the methods of milk analysis.

13. *Bunsen's Flame Reactions.* pp. 30, 8vo. Also translated by Professor HIMES, and published by Mr. Baird, form a proper supplement to Will's Tables.—This translation of Bunsen's Memoir was originally published in the Journal of the Franklin Institute in 1868.

II. GEOLOGY AND NATURAL HISTORY.

1. *Addition to the Paper, "Volcanic Energy: an attempt to develop its true Origin and Cosmical Relations;"** by ROBERT MALLET. (Abstract.)—Referring to his original paper (Phil. Trans., 1873), the author remarks here that, upon the basis of the heat annually dissipated from our globe being equal to that evolved by the melting of 777 cubic miles of ice at zero to water at the same temperature, and of the experimental data contained in his paper, he had demonstrated, in terms of mean crushed rock, the annual supply of heat derivable from the transformation of the mechanical work of contraction available for volcanic energy, and had also estimated the proportion of that amount of heat necessary to support the annual vulcanicity now active on our globe; but, from the want of necessary data, he had refrained from making any calculation as to what amount in volume of the solid shell of our earth *must* be crushed annually, in order to admit of the shell

* Read June 20, 1872; Phil. Trans. for 1873, p. 147.

following down after the more rapidly contracting nucleus. This calculation he now makes upon the basis of certain allowable suppositions, where the want of data requires such to be made, and for assumed thicknesses of solid shell of 100, 200, 400, and 800 miles respectively.

From the curve of total contraction (plate x, Phil. Trans., part i, 1873) obtained by his experiments on the contraction of slags, he has now deduced partial mean-coefficients of contraction for a reduction in temperature of 1° Fahr., for intervals generally of about 500° for the entire scale, between a temperature somewhat exceeding that of the blast-furnace and that of the atmosphere, or 53° Fahr. And applying the higher of these coefficients to the data of his former paper, and to the suppositions of the present, he has obtained the absolute contraction in volume of the nuclei appertaining to the respective thicknesses of solid shell above stated. In order that the shell may follow down and remain in contact with the contracted nucleus, either its thickness must be increased, its volume remaining constant, or the thickness being constant, a portion of the volume must be extruded. The former supposition is not admissible, as the epoch of mountain-building has apparently ceased; adopting the second, the author calculates the volume of matter that must be crushed and extruded from the shell in order that it may remain in contact with the nucleus. He tabulates these results for the four assumed thicknesses of shell, and shows that the amount of crushed and extruded rock necessary for the heat for the support of existing volcanic action is supplied by that extruded from the shell of between 600 and 800 miles thickness, and that the volume of material, heated or molten, annually blown out from all existing volcanic cones, as estimated in his former paper, could be supplied by the extruded matter from a shell of between 200 and 400 miles in thickness.

On data which seem tolerably reliable the author has further been enabled to calculate, as he believes for the first time, the actual amount of annual contraction of our globe, and to show that if that be assumed constant for the last 5000 years, it would amount to a little more than a reduction of about 3·5 inches on the earth's mean radius. This quantity, mighty as are the effects it produces as the efficient cause of volcanic action, is thus shown to be so small as to elude all direct astronomical observation, and, when viewed in reference to the increase of density due to refrigeration of the material of the shell, to be incapable of producing, during the last 2000 years, any sensible effect upon the length of the day. The author draws various other conclusions, showing the support given by the principal results of this entirely independent investigation to the verisimilitude of the views contained in his previous memoir.—*Pro. Roy. Soc.*, No. 152, 1874.

2. *Metamorphic products from the burning of coal-beds of the Lignitic Tertiary in Dakota and Montana.*—In this paper, published in the Proceedings of the Boston Society of Natural History, Jan., 1874 (vol. xvi), Mr. J. A. Allen describes metamorphosed

beds of clays and sands, accompanied by pumiceous and lava-like materials, looking like true volcanic products, occurring far from any volcanic region and due solely to the burning out of beds of brown coal. The thickness of the altered beds amounts in places to thirty or even fifty feet, but seldom exceeds eight to twelve feet. Generally the overlying clays and sands have been merely hardened and changed in color; but in some places, where the coal bed was thick, the deposit in immediate contact has been more or less fused, and has received occasionally a vitreous porcellanic or vesicular structure, and even the scoriaceous and pumice-like aspect of volcanic products. The lowermost of the burnt series consists of cinders and clinkers much like the residuum left in coal grates from the combustion of ordinary mineral coal.

The region of the Bad Lands, on the Little Missouri, is one of the largest areas of this Lignitic metamorphism, it covering a breadth of twenty to thirty miles for 200 miles in length; all the ridges and buttes are capped or bounded with the reddened and indurated shales. Other such areas occur along the Yellowstone near the mouth of Powder River, and along Powder River, and also on the Rosebud and Tongue Rivers. They were found by Dr. Hayden on the sources of Tongue River, within a few miles of the Big Horn Mountains and on those of North Fork of the Shyenne River and elsewhere; and by Dr. Hines as far south as the "foot slopes" of the same range on the Crazy Woman's Fork of Powder River. Nicollet long since described, but from report, the *pseudo-volcanoes*, or smoking hills of the west, which were made, evidently, from the burning of the subjacent coal beds, and he attributes the fire to the action of decomposing pyrites on lignites and other material of a combustible nature.

Mr. Allen states that the landscape is variously affected by the metamorphism The baked rocks, besides giving their red tints to the country, arrest or greatly retard the erosion of the buttes and ridges consisting of them. Over areas of thousands of square miles they thus in great measure determine the surface contours and protect the hills from rapid denudation. Fragments of pumice have been found on the Missouri as far south as Port Pierre, and the early explorers supposed them to be the products of unknown volcanoes, high up in the mountains.

3. *Lignitic formations north of the parallel of* 49°.—Mr. G. M. Dawson, in the last number of the Canadian Naturalist, and also in a report to the British North American Boundary Commission (Montreal, 1874), gives an account of his own and others observations on the region north of the parallel of 49° and west of the Red River. The boundary line strikes the Lignitic Tertiary about 250 miles west of Red River, in the valley of the Souris River. Few distinct plants were found in this region, but there were shells representing two species of *Paludina* or *Viviparus*, at least two of *Melania*, one *Corbula*, and others resembling those described by Meek from the Lignitic beds farther south. The coal is stated

to be of excellent quality. Scoria and clinkers from the burning of the beds in place are stated to be abundant; but Mr. Dawson says that pyrites cannot have been the source of the combustion, since it is of rare occurrence in the coal. To the west the beds occur on the upper parts of the northwestern tributaries of the Missouri, 345 miles west of Red River and beyond; at 400 miles from Red River, near Porcupine Creek, there are many plants in the beds, as leaves of *Thuja*, *Sequoia*, *Taxus*, *Populus*, *Salix*, *Ulmus*, *Platanus*, etc.; also the fern, *Onoclea sensibilis*, which is very abundant. The best of the brown coal afforded Mr. Dawson 42 to 47½ per cent of fixed carbon, 12 to 17 per cent of water, 32 to 40 per cent of volatile matter, and 2½ to 5 per cent of ash. None of those examined afforded a coherent coke.

In a following paper, Mr. Dawson shows that the Cretaceous at Manitoba contains numerous foraminifers, and the species resemble much those of the English chalk. Both abound in forms allied to Textularia and Rotalia. Various forms of coccoliths were found in the same rocks.

4. *Marine Champlain Deposits on lands north of Lake Superior.*—Dr. DAWSON, in his annual address before the Natural History Society of Montreal, May 18th, says that Prof. Bell, in the Report of the Canadian Geological Survey for 1870–71, states the occurrence of marine shells, similar to those of the Champlain deposits in the vicinity of Montreal, at a height of 547 feet above the sea. Dr. Dawson also remarks that in the hills behind Murray Bay and Les Eboulements, he has observed these shells at a height of at least 600 feet; and also that Mr. Kennedy has recently found marine shell deposits of the same era on Montreal Mountain, at a height of 534 feet above the sea.

5. *Climate of the Champlain Period.*—In the address referred to in the preceding article, Dr. DAWSON repeats his conclusion that the climate of the period when the land stood below the present level—"that which immediately preceded our own modern age"—was cold. The evidence to which he appeals proves the existence of a cold climate as regards the waters, that is, of cold currents on the coast, but no facts are mentioned which tell anything with regard to the climate over the land.

6. *Fossil Cockroaches from the Carboniferous of Cape Breton.* (Canadian Naturalist, vol. vii, No. 5.)—Mr. Scudder here describes two new species from two wings discovered by R. Brown, Esq. He names them *Blattina Bretonensis* and *B. Heeri*. The specimens are on dark shale, and are associated with leaves of *Sphenophyllum* and ferns.

7. *Fossil Elephant and Mastodon in California.*—According to a letter from Dr. L. G. Yates, of Centreville, Alameda County, California, read by Dr. Leidy before the Academy of Natural Sciences of Philadelphia, (and published in the Proceedings for 1874, pp. 18–26), remains of Elephants have been found in Alameda, Calaveras, Los Angelos, Placer and Solano Counties; and the Mastodon in Alameda, Amador, Calaveras, Contra Costa, El

Dorado, Mendocino, Placer, Santa Barbara, Stanislaus, Solano, Sonoma and Tuolumne Counties. The special localities in each county are mentioned in the letter; and Dr. Yates adds that the majority of them he has himself visited. He observes, that although he has looked for human remains or relics in the same beds, he has failed to discover any. He once found a human skull apparently in the same formation which had afforded elephant and mastodon remains; but he afterward discovered the body to which the head belonged, about eighteen inches below the surface, in the bank above where there had been an old Indian burial ground.

8. *Bulletin of the Cornell University.* (*Science.*) Volume i, number 1 and 2. 64 pp. 8vo, with 9 plates. Ithaca, 1874.—This first scientific Bulletin, from the Cornell University, contains two valuable papers: one, a reconnoissance of the Lower Tapajos, being a preliminary report of the Morgan Expedition to the Amazon in 1870, 1871; by Ch. Fred. Hartt; the other, descriptions and figures of the Carboniferous Brachiopoda of Itaituba, on the Rio Tapajos, Province of Para, by O. A. Derby. The latter paper shows, as stated in its conclusions, that the Carboniferous rocks of South America thus far made knows by their fossils, are true Carboniferous, and not Subcarboniferous; that of the 27 species described, 12 are also North American (these including the common kinds *Athyris subtilita*, *Spirifer cameratus*, *Chonetes glabra*, *Productus cora* and others); that several Brazilian and North American species are included among those described by D'Orbigny from Lake Titicaca and Yarbichambi, Bolivia; and among those from the province of Arque, a degree or more southeast of Lake Titicaca, by Col. Lloyd; and others from Cochabamba and Santa Cruz. Mr. Derby's paper appears to be worked up with great care. It is illustrated by nine photographic plates.

9. *Memoire per servire alla Descrizione della Carta Geologica d'Italia.*—The first volume of the series, in illustration of the geology and geological map of Italy, was published, at Florence, in 1871, and the first part of the second in 1873. They are quarto volumes, beautiful in style of execution, and admirable in the maps and engravings. The first has already been briefly noticed in this Journal. The second contains papers: on the Physical geography of the island of Ischia, by Dr. C. W. C. Fuchs; on the Alpine chain of the St. Gothard to be tunneled for the Italico Helvetic railway, by F. Giordano; on the Tertiary formations of the Sulphur-bearing zone of Sicily, by S. Mottura; Malacologia Pliocenica Italiana, by Dr. C. d'Ancona. Fasc. II, genera *Pisania*, *Ranella*, *Triton*, *Fasciolaria*, *Turbinella*, *Cancellaria*, *Fusus*.

10. *Note on Prof. J. Lawrence Smith's collection of his Memoirs;* by George J. Brush: addressed to the Editors.—My attention has been called to a volume entitled "Mineralogy and Chemistry, Original Researches by Professor J. Lawrence Smith," in which there appears, on page 109, in connection with an article on the "Reëxamination of American Minerals," the following footnote: "In the first half of this reëxamination I was assisted by my friend, George J. Brush." As this statement may lead to misap-

prehension in regard to the relations of Professor Smith and myself on the part of some readers, I desire in this place to give the facts.

The first three papers on the "Reëxamination of American Minerals" were published as a *joint* work by Professor Smith and myself, in volumes xv and xvi in the second series of this Journal. In the volume of researches above alluded to, these cover pages 109 to 141, to the end of the section on petalite; and for the matter therein contained I simply claim to be responsible equally with Professor Smith. This claim can be easily substantiated by reference to the original papers in this Journal, and by other evidence if necessary.

I have no doubt Professor Smith will be glad to have this correction made, and presume it was only an accidental inadvertence which led him to publish the foot-note without further explanation. I should not offer this correction had I not learned that Professor Smith's volume has had a wide circulation, both in this country and in Europe.

New Haven, July 8th, 1874.

11. *Cuarto Appendice al Reino Mineral de Chile i de las Republicas vecinas, publicado en la segunda edicion de la Mineralojia*, de DON IGNACIO DOMEYKO, Rector de la Universidad. 58 pp. 8vo. Santiago, Chile. 1874.—This fourth appendix to the second edition of Domeyko's Chilian Mineralogy contains notes on new localities, with descriptions of various minerals, the most of them metallic species. For a double chlorid of silver and mercury at Los Bordos, in the department of Copiapo, the name Bordosite is given by Señor Bertrand. Ulexite and Hayesite are stated to have been found at a locality on the river Loa in littoral Bolivia, and in Carmen Alto, fourteen leagues from Antofagasta, the old localities being in the desert of Atacama in Peru, and at Ascotan in Bolivia. In addition, Domeyko now adds another locality at Ola, about thirty leagues to the east of the mines of copper of Chañaral de las Animas, northeast of the range of Doña Ines; the place appears like a dried lake. The locality of borocalcite (Hayesite), in the dry lake of Maricunga, fifty-nine miles to the north of Puquios, is, according to Fonseca, of great extent, he estimating the amount at 14,000,000 tons. A memoir on the subject has been published by Fonseca in the Anales de la Universidad. It is mainly a *hydrated borate of lime*,—borocalcite, mixed with some common salt, but without any ulexite (boronatrocalcite).

12. *Veszelyite.*—A new mineral has recently been described by Prof. A. Schrauf, the eminent crystallographer of Vienna, under the name of *Veszelyite.* It is triclinic, resembling distorted liroconite, the crystals being bounded by the prism and dome ($100:001=101° 3'$). It has a bluish-green color, and the composition is expressed by the formula $4CuO\,P_2O_5\,5H_2O$. It occurs on garnet at Morawitza in Banat. It is named from the discoverer.

13. *On Livingstonite, a new mineral;* by MARIANO BARCENA. (El Minero Mexicano, May, 1874.)—Livingstonite much resembles in color and aspect stibnite or sulphid of antimony. It occurs in

prisms, apparently isomorphous with stibnite, and like it in thin columnar groups. Color, bright lead-gray; of powder red, instead of black like stibnite. Hardness, 2 on Breithaupt's scale. Density, at 16° C, 4·81. Fuses at the first touch of the blowpipe flame, and gives out abundant white fumes. Cold nitric acid does not sensibly attack it; but warm dissolves it and a white residuum falls. Sulphydric acid precipitates it, forming a yellow sulphid and another of a black color. Reactions show that it contains mercury as well as antimony. An analysis has not yet been completed, but an assay proved the presence of 10 per cent of mercury, showing that it is in all probability a sulphid of mercury and antimony.

It is from Huitzuco, in the State of Guerrero, Mexico. Mr. Barcena has named it in honor of the distinguished African traveler, Mr. Livingstone; with reference to which, he well says, "Al hacer esta dedicatoria he tenido presente que los bienhechores de la humanidad pertenecen á todas las naciones, y que la humanidad entera debe honrar su memoria."

14. *On the Plagopterinæ and the Ichthyology of Utah;* by E. D. Cope. 14 pp. 8vo. 1874.—A part of the Report of the Geographical and Geological Explorations and Surveys west of the 100th meridian; First Lieut. G. M. Wheeler, Corps of Engineers, U. S. A., in charge. From the Proceedings of the Amer. Phil. Soc. of Philadelphia.

15. *Annotated List of the Birds of Utah;* by H. W. Henshaw. 16 pp. 8vo. (Reprinted from the Ann. N. York Lyc. Nat. Hist., vol. xi, 1874, at Salem, Mass.)—Mr. Henshaw's personal observations were made in connection with the survey under Lieut. Wheeler, of which he is ornithologist.

16. *Bulletin of the Buffalo Society of Natural Sciences.*—Vol. ii, No. 1 (104 pp. 8vo), contains a list of the Noctuidæ of North America, by A. R. Grote; a catalogue of the Coleoptera from the region of Lake Pontchartrain, Louisiana, by S. V. Summers; and a catalogue of Boleti of New England, with descriptions of new species, by C. C. Frost.

17. *Anatomy of the Invertebrata;* by C. W. von Siebold. Translated from the German, with additions and notes, by Waldo I. Burnett, M.D. 470 pp. 8vo. Boston, 1874. (James Campbell.)—This is a reprint of Dr. Burnett's excellent translation of Von Siebold's well known text-book on the Anatomy of the Invertebrata. It is a standard work, and, although not presenting the latest results of researches, should be in the hands of all zoological students. The notes of Dr. Burnett are an important addition to the original work.

18. *Maps of the Geyser Basins, on Madison River*, after a reconnaissance by G. R. Belcher. U. S. Geological Survey of the Territories, F. V. Hayden in charge.—These two large maps give the exact positions and areas of the many hot springs, or lakes and geysers, of the Upper Madison. They are a most interesting study for the geologist. Dr. Hayden has the satisfaction of seeing great and valuable results flowing from the explorations under his charge.

19. Three Essays relating to Vegetable Paleontology are before us:

(1.) ALPH. DECANDOLLE, *on Physiological Groups in the Vegetable Kingdom: (Constistution dans le Règne Végétal de Groupes Physiologiques applicables a la Geographie Botanique Ancienne et Moderne)*; par M. ALPHONSE DECANDOLLE.—An article of 38 pages in the Archives des Sciences de la Bibliothèque Universelle, Geneva, of May, 1874, and issued separately. M. DeCandolle here makes an attempt to mark out groups of plants according to the climates which they affect.

From the grouping of plants according to their botanical characteristics, he says, come classes, families, genera, species, &c. From geographical delimitations come floras. For the study of geographical botany and of the vegetation of former periods, some kind of physiological arrangement, which may comport with constitutional characteristics and aptitudes, becomes desirable; and this want M. DeCandolle here undertakes to supply.

He sketches five groups. First, plants requiring a great amount of heat and moisture. A name expressive of both these requirements would be cumbrous; so he chooses for this group a name referring to the temperature only, and calls them *Megathermal* plants, or in short *Megatherms*, i. e., plants to which much heat is essential. These inhabit the rainy intertropical regions in the plains, and sultry valleys up to the 30th parallel.

Second, plants requiring about as much heat but far less moisture. These, taking the name from the latter characteristic, he terms *Xerophilous* plants, lovers of dryness. They are pretty widely distributed, but they especially affect the regions bordering the tropics and extending, say to the 35th parallel in both hemispheres.

The third group, *Mesothermal* plants, require, as the name denotes, moderate heat, also a fair supply of moisture, at least in the growing season, say a mean annual temperature of 59°–68° Fahr., that of the rather warm temperate zones.

The fourth group, *Microthermal* plants, i. e., those demanding little heat, say a mean annual temperature of 57° or less, down to that of 32°, with of course a good summer temperature. In this group would be included the vegetation of our Northern States and Canada.

The fifth group, *Hekistothermal* plants, those requiring least heat, such as make up arctic, antarctic, and alpine vegetation.

His sixth group, *Megistothermal* plants, those which require an exceptional amount of heat, or a mean of over 86° Fahr., are mentioned as having probably played a part among the earlier vegetations, but as now represented only by a few lowly organized plants of thermal waters.

The present distribution of plants over the globe depends, 1, upon the distribution in a preceding epoch, and 2, upon the physical conditions, mainly the distribution of heat and moisture, now existing. These conditions have varied greatly; and under them the corresponding sorts of plants have occupied stations widely different from those they now occupy. As illustrating this, DeCandolle mentions the fact that the vegetation of the borders of the Mediterranean extended to Paris at the commencement of the present period; that the arctic-alpine or hekistothermal vegetation, now belonging to the polar region and the summit of high mountains in Europe, occupied the plains in the glacial period after having been once before stationed nearly as at present; that the vegetation, say of the 35th parallel in the Atlantic United States, has once been as far north as the 60th parallel; and the intertropical flora had advanced to London at the commencement of the Tertiary period. In another connection he remarks that, "il est à peu près démontré que les flores actuelles des Etats-Unis méridionaux et du Japon se sont rapprochées un fois dans le nord, sous l'empire de climats tempérés, selon l'hypothèse émise, en 1859, par M. Asa Gray." Now when one endeavors to trace the probable connection between preceding and present vegetations, as it were historically, it becomes convenient, if not almost necessary, to have some name under which a particular *ensemble* of vegetation, such as that of a large part of the United States in our time, may be spoken of as a whole when recognized in far other times and places. Hence these proposed names, of Mesotherms, &c. If the grouping and the naming, as now struck out, are not wholly satisfactory, at least a fair beginning has been made and a practical convenience secured. Their utility becomes apparent when attention is turned to fossil floras.

In these the dominant character, megathermous, mesothermous, microthermous, or even xerophilous, is pretty clearly to be made out, in the Tertiary floras unmistakably. That similar forms of vegetation at different eras imply similar climates is an unavoidable hypothesis; that they imply similar antecedent plants is the natural presumption; and when, along with the general similarity, some of the species of consecutive floras appear to be identical or nearly so, this presumption approaches demonstration. In following up this line in the regions and through the times which we know most about, viz: in Europe and North America, and from the present to the Cretaceous period, DeCandolle brings to view various interesting considerations, and among them a corollary drawn by Heer, which our paleontological geologists should heed. Namely, that when two fossil floras or faunas are very similar, but in widely separated latitudes (say Spitzbergen and middle Europe for a marked instance), they cannot possibly have been contemporaneous. For difference of temperature according to latitude must have been as real as now within the periods referred to, however modified by geographical configuration; and of two such floras the northern must have been the more ancient, since the temperature has on the whole diminished in the course of ages, and especially during the Tertiary epochs. And not only very different fossil Tertiary floras must have been contemporaneous in different latitudes, but probably under the same parallels also, at least as different as the present floras of Europe and China, or of California and Pennsylvania.

How the adaptive diversification into these physiological groups came to pass, is a question of the same nature as that of the diversification into classes, genera and species, but far easier to form a conception of. As a specimen of the essay we translate a portion of DeCandolle's remarks upon this:

"Starting from the idea, admitted by physicists, of a nearly uniform and considerably elevated temperature of the earth at a remote period, followed by a very gradual cooling and distinction of climates during an incalculable series of ages, there must have been at first only one physiological group of plants, that which I have called megistothermous. To this succeeded the plants of the Carboniferous era, of little diversification, however, as compared with those of subsequent ages. They would seem to have been megatherms, or sometimes mesotherms, for the existing ferns and conifers correspond to those two categories. Among them there may have been species adapted to the long polar twilights, just as our ferns often inhabit the forests, and as some cultivated conifers, such as *Cryptomeria Japonica*, prefer the shade. The snow must have destroyed the primeval vegetation of the north, but the megatherms and mesotherms survived elsewhere. It is difficult to conceive what was passing during the immense period of the secondary formations, on account of the subsequent dispersion of the land surfaces, the extent of the seas which covered Europe, and the fewness of the fossil plants which have thus far been studied. At the commencement of the Tertiary period, the megatherms occupied all the exposed surface up to the 48th degree of north latitude, that is to say, all the present hot and temperate region. The other classes became detached little by little from the vegetation that preceded, and localized toward the north and upon the mountains, in proportion as the increased cold drove out the prior possessors. Gradually the megatherms lost territory and the others acquired it. This is the simple expression of the facts, irrespective of theory.

"How and by what means has this physiological grouping in accordance with the climates been effected? Here hypothesis begins. The question is the same as that which respects the evolution of forms, except that here there is a basis of known facts which in that case is wanting; and nobody can prove that there was originally only one kind of plant at a time when there was certainly only one climate. And unity of climate carries with it physiological unity for the vegetation of the epoch, whatever it may have been. There is nothing hypothetical therefore in saying that the actual physiological groups have succeeded to the single primary one. As to the cause of this evolution of groups, the hypothesis proposed by Darwin for that of forms is equally applicable, and equally finds support in the circumstances as they become more and more known. Thus the changes both in physiological character and in the forms have been very slow and very gradual. The study of the Tertiary floras, as pursued by M. de Saporta, following in the footsteps of Heer, furnishes abundant proofs of a slow and continual substitution of analogous forms, which is quite contrary to the hypothesis of abrupt renewals at long intervals, through unknown causes, as maintained by the eminent Zurich professor. But I need not now discuss the Darwinian hypothesis. At least we may wait until some other discussable hypothesis is propounded."

As we write this, the information comes to this country that M. DeCandolle has the distinguished honor of being elected one of the eight foreign members of the Institute of France (Academy of Sciences), to fill the vacancy made by the death of Agassiz.

(2.) W. C. WILLIAMSON: *Primeval Vegetation in its relation to the Doctrines of Natural Selection and Evolution.*—This is one of the fourteen able *Essays and Addresses by Professors and Lecturers of Owens College, Manchester*, published in commemoration of the opening of the new college buildings, Oct. 7, 1873 (Macmillan & Co., London, 1874), and forming a handsome octavo volume of 560 pages. Professor Williamson's lecture fills fifty-five pages, with a sketch of what is known of the vegetation of the preceding, mainly of the earlier, geological periods, and with evidently well considered and noteworthy remarks upon the bearings of our actual knowledge on the question referred to. It opens with a reference to the "wide-spread commotion in the scientific world" caused by "the allied doctrines of Darwin and Herbert Spencer in relation to the origin of species." The popular association of these names is natural enough, and Mr. Darwin has himself to some extent furthered it. But scientific men, it is to be hoped, keep up a distinction between natural history investigation pursued by evolutionary hypotheses and the *a priori* deductive natural history developed by Mr. Spencer. The former brings new aids to research and practical problems to be attempted: of the latter it is unnecessary now to speak, and we could not well speak of it so highly as Professor Williamson does. On the other hand Professor Williamson alludes to "the advocates of the new views" as "treating with ill-concealed contempt any one belonging to an older scientific school who is unable to travel along the new road with their vigorous steps." Surely this cannot be justly said of any who have treated of vegetation, recent or primeval, under the light of the "new views," nor of Mr. Darwin himself; indeed, this is so far from being true that it is fair to presume that the professor had some other order of persons in his mind. He concludes that "the geological record is less imperfect botanically than zoologically, since we practically catch the vegetable kingdom at a comparatively early stage of its history, and are able in some measure to trace its upward progress,"—propositions which, we suppose, are open to serious doubt. That "the extinct vegetable kingdom has been comparatively neglected in connexion with this subject" may be true; but a singular idea of what has been done and attempted is conveyed when Dr. Dawson's name alone is mentioned in this connexion, and Heer and Saporta, Newberry and Lesquereux, are left out of view—perhaps because they have not written polemically. These are all the faults we have to find with what appears to be a fair and faithful presentation of the topic, as respects the fossil botany of the Devonian and Carboniferous formations, with which the author is thoroughly familiar, and to which he mainly restricts his attention in this essay. His conclusions are that natural selection and evolution "unquestionably account in a satisfactory manner for many phenomena that have hitherto received no other adequate explanation," such, for instance, as the production of varieties, "while it is equally certain that many of these varieties are wholly undistinguishable from what we designate [as] species; it is also true that in many instances generic types appear to merge into each other in such a way as to make it difficult to define their boundaries." The longer he studies any group and the more numerous the specimens obtained from different localities, the more utterly does he distrust their specific and even generic distinctions; and he can account for this state of the case only on the supposition of "intermediate forms produced through the action of the varying external forces which lead to evolution." To the question whether variations are boundless or are confined within definite limits, while admitting that those limits may include genera, perhaps he would include families also, he replies that "the facts of paleophytology appear to me to favor the latter rather than the former conclusion. We have not much difficulty in distinguishing the great paleozoic types from one another." This, surely, is what would be expected, at least by Mr. Darwin.

(3.) J. W. Dawson: *Annual Address of the President of the Natural History Society of Montreal*, May, 1874.—A considerable portion of this address is devoted to vegetable paleontology and related matters. Substantially Principal Dawson's views and aims are not very unlike those of Prof. Williamson, although the mode of argument and the tone are. Both have occupied themselves mainly with the fossil botany of the earlier formations; and this may explain the contrast between their conclusions as to the abrupt introduction of types and forms, and those of Count Saporta, based upon the fuller records of Tertiary deposits, which DeCandolle has referred to in an extract given above. Professor Williamson probably thinks that evolutionary hypotheses in some form are not unlikely to prevail; Dr. Dawson, that they are already well nigh exploded. The former seems inclined to accept at least the probability of evolution for species and genera. Dr. Dawson restricts its sway "to varietal and race forms," which "constitute conventional as distinguished from natural species," maintaining that the latter are permanent and original. But, so far as we can make out, the difference between his "varietal forms or conventional species," which have arisen from "descent with modification," and the natural species which have not, is simply this: that actual evidence (such as that of known intermediate forms) has enforced the conclusion in certain cases and not in others. Whenever it does, or tends to do so, we have only to transfer these from the list of actual to that of conventional or spurious species; and so the doctrine of the "marvelous permanence of specific types" will remain unassailable, while the truths for which "modern theories of derivation have been chiefly valuable, so far as established, will remain as substantial results after these theories have been exploded." "Exploded" is hardly the proper term (except as suggestive of the "engineer hoist with his own petard,") for a process in which the doctrine of the derivation of species has all the life taken out of it as fast as it comes into existence. This method of confutation is more effectual than original. It was foreshadowed by Sir John Harrington in the 16th century; when he wrote:—

> "Treason doth never prosper, what's the reason?
> "Why, if it prosper, none dare call it treason.

This absolute distinction into varietal and derived as against specific forms assumed to be underived, along with the recognition that some of the latter have come down through the Tertiary or even from the Cretaceous ages, "while some species have disappeared without known successors, and others [like them?] have come in without known predecessors," seems to be the key to the interpretation of some points in the following passage. We quote it at length, being doubtful if we could do it justice in an abstract.

"Physically the change from the Cretaceous to the Tertiary was one of continental elevation—drying up the oceanic waters in which the marine animals of the Cretaceous lived, and affording constantly increasing scope for land animals and plants. Thus it must have happened that the marine Cretaceous animals disappeared first from the high lands and lingered longest in the valleys, while the life of the Tertiary came on first in the hills and was more tardily introduced on the plains. Hence it has arisen that many beds which Meek and Cope regard as Cretaceous on the evidence of animal fossils, Newberry and Lesquereux regard as Tertiary on the evidence of fossil plants. This depends on the general law that in times of continental elevation newer productions of the land are mixed with more antique inhabitants of the sea; while on the contrary in times of subsidence older land creatures are liable to be mixed with newer products of the sea. Thus in Vancouver's Island plants which Heer at first regarded as Miocene have been washed down into waters in which Cretaceous shell-fishes still swarmed. Thus Cope maintains that the lignite-bearing or Fort Union group contains remains of Cretaceous reptiles, while to the fossil botanist its plants appear to be unquestionably Tertiary. Hence also we are told that the skeleton of a Cretaceous Dinosaur has been found stuffed with leaves which Lesquereux regards as Eocene. At first these apparent anachronisms seem puzzling, and they interfere much with arbitrary classifications. Still they are perfectly natural, and to be expected where a true geological transition occurs. They afford, moreover, an opportunity of settling the question whether the introduction of living things is a slow and gradual evolution of new types by descent with modification, or whether, according to the law so ably illustrated by Barrande in the case of the Cephalopods and Trilobites, new forms are introduced abundantly and in perfection at once. The physical change was apparently of the most gradual character. Was it so with the organic change? That it was not is apparent from the fact that both Dr. Asa Gray and Mr. Cope, who try to press this transition into the service of evolution, are obliged in the last resort to admit that the new flora and fauna must have migrated into the region from some other place. Gray seems to think that the plants came from the north, Cope supposes the mammals came from the south; but whether they were landed from one of Sir William Thomson's meteors, or produced in some as yet unknown region of the earth, they cannot inform us. Neither seems to consider that if giant Sequoias and Dicotyledonous trees and large herbivorous Mammalia arose in the Cretaceous or early Tertiary, and have continued substantially unimproved ever since, they must have existed somewhere for periods far greater than that which intervenes between the Cretaceous and the present time, in order to give them time to be evolved from inferior types; and that we thus only push back the difficulty of their origin, with the additional disadvantage of having to admit a most portentous and fatal imperfection in our geological record." (pp. 9, 10.)

The latter portion of this paragraph conveys a singular idea of what one of the writers referred to by Principal Dawson was about. Dr. Gray had a problem before him, of which these are the salient points and the emphatic facts: The *Taxodium* or Bald Cypress of our Southern Atlantic States, or a tree so like it as to be now undistinguishable, was in the Miocene age an inhabitant of the arctic regions round the world. The Sequoias or Redwoods of California were represented in this same arctic Miocene flora by several species of similar trees, two of which were so much like the two present Californian Redwoods that, if we mistake not, Dr. Dawson is disposed to regard them as "varietal forms" of the same. Besides these peculiar types, the arctic Miocene forest contained, and apparently consisted of, trees and shrubs generically like those which compose the forests of New England, Virginia, etc., some of them generally resembling, others very closely resembling, and some seemingly specifically representing, our existing trees. What was the probable relation between these existing trees of our latitude and the arctic ones of ancient times? In answering this question, Dr. Gray took two postulates for granted, in common with the scientific world: 1, that the more recent individuals of a species are descendants of the earlier individuals of the same species; 2, that the temperate-climate vegetation which once flourished beyond the arctic circle was slowly driven southward by the cold. His moderate theoretical inference is, that the vegetation among which we live has, as a whole, descended from the arctic Miocene vegetation, most of it with modification, some of it without. This is what Dr. Dawson calls being "obliged in the

His death followed close upon that of his old and near friend Meissner, of Basel. His herbarium, we understand, is in part bequeathed to certain institutions and individuals, and in part will be sold. His library, said to be very rich in other branches of natural history, as well as in botany, is by the late owner's desire to be sold entire. This will afford an excellent opportunity for some of our colleges or other institutions. No particulars have as yet been received; but any institution wishing to secure a valuable library, such as it takes a long and devoted life to form and perfect, should address Madame Shuttleworth, at Berne, Switzerland. A. G.

III. Astronomy.

1. *Polariscopic Observations of Coggia's Comet* (1874, III).—During the greater portion of the time when the comet was favorably situated for observation, it was unfortunately hidden by clouds, or extinguished by thick haze. On one or two occasions, however, I was able to obtain evidence that its light was polarized. The instrument employed was the very sensitive polariscope described in the May number of this Journal.

On the evening of July 6, although the sky was generally full of drifting clouds, clear intervals appeared now and then, which allowed a distinct view of the comet. The polariscope showed the bands, both bright and dark, quite definitely, and they were seen with comparative ease. Observations repeated a number of times agreed in showing that the light was polarized in a plane passing through the axis of the tail, that is, as nearly as could be estimated, in a plane passing through it and the sun. Other observations made on the evening of July 14, when the sky was quite clear, gave the same result, though less satisfactorily, as the twilight had begun to interfere with the observations. After waiting until this had disappeared, it was possible to see the bands, though with some difficulty, and the degree of the polarization appeared to be decidedly less than on the previous occasion.

The circumstances were too unfavorable to admit of any determination of the percentage of light polarized, but it was certainly not large. The fact of polarization shows that a considerable portion of the light of the coma is derived from the sun by reflection. A. W. WRIGHT.

Yale College, July 24, 1874.

2. *Spectrum of Coggia's Comet* (1874, III).—In the Comptes Rendus for June 8, 1874, M. Rayet gives an account of spectroscopic observations, made early in June, which showed that the spectrum of the nucleus was continuous, resembling that of a star of the sixth magnitude, except that the color at the extremities could not be distinguished. The spectrum of the coma consisted of the usual three bands of cometary spectra, differing from them, however, in the fact that instead of being sharply terminated on the less refrangible side and fading gradually away toward the blue, they were definitely bounded on both sides, the central band especially terminating sharply on both edges as if by a line,

while the other two were slightly diffuse on both edges, thus approaching the ordinary type. They were less than half as bright as the central band. M. Rayet states that the three corresponded respectively with the yellow, green and blue parts of the spectrum, without giving more definitely their exact positions and dimensions. He speaks of the nuclear spectrum as very short. The same peculiarity was observed by Mr. Lockyer, who says (Nature, June 25, 1874), "It struck me that the spectrum was short, i. e., that it was deficient in blue rays; and as one saw in the telescope a fan-like structure above the nucleus (as seen in an inverting telescope), so also in the spectroscope, the continuous spectrum sparkled as if many short, bright lines or bands were superposed upon it." A. W. W.

3. *Observations of the Aurora Borealis made by* MINERVA E. WING, *at West Charlotte, Vt., lat.* 44° 19′, *long.* 73° 15′ *W. from Greenwich.*—The following observations were communicated to the Smithsonian Institution and have been forwarded to this Journal by Prof. Joseph Henry.

1872. Oct. 5 and 6, Aurora first observed at midnight; lasted until daylight; confined to a belt from W. to E. across the zenith. Oct. 28, 8 P. M., auroral lights low in the north; brightest near midnight. Oct. 29, 8 P. M., a low rim of bright light on the north horizon, threw up a few beams a little E. of N. Occasional auroral lights during the remainder of night.

Dec. 23, 8 P. M., Auroral lights lasted until midnight. Dec. 31, at times quite light in north.

1873. Jan. 5, Aurora at 2 A. M., brightest at 3 A. M., with a few streamers in N. E. Jan. 7, 8½ P. M., aurora in E. and W. sending beams to zenith. Jan. 26, 7½ P. M., aurora till 9 P. M., when it took the form of a low arch on the N. sky. Jan. 29, midnight, aurora similar to that of the 26th.

Feb. 4, 8 P. M., Appearance of a white cloud through which the stars shone, retained the same position all night till 4 A. M. Feb. 20, auroral lights very bright from midnight until morning. Feb. 20, 8 P. M., similar to aurora of Jan. 26th. Feb. 22, 8 P. M., aurora very white; at midnight very bright. Feb. 24, 8 P. M., bright; very white. Feb. 27, aurora from midnight till morning.

March 1, Aurora from 7½ P. M. to midnight. March 5, aurora from 9 P. M. March 10, 8 P. M., a few beams of aurora visible. March 23, faint aurora in north, at 9 P. M. March 24, 8 P. M., bright auroral light in north. March 27, 8 P. M., quite bright in N. W., lasted all night.

April 8, 7 P. M., Crimson aurora. April 11, at midnight red light on N. horizon. April 16, light on N. horizon. April 21, strong auroral light 10 P. M. April 29, faint aurora. April 30, faint aurora.

May 3, Faint aurora all night. May 6, Bright aurora, lasted all night. May 8, aurora all night. May 20, aurora all night. May 22, bright aurora from dark cloud bank in N. May 31, aurora all night.

June 18, Bright display of auroral lights. June 22, auroral lights during the night. June 23, aurora all night. June 25, most brilliant aurora of this year; form of an arch, with short beams playing on both sides up and down; very bright at 11 P. M., 1 A. M. and 3 A. M. June 27, active aurora, 10½ P. M.; beams shot past the zenith.

Aug. 14, Aurora. Aug. 16, arch low on N. horizon. Aug. 17, aurora near horizon from 9 P. M. to midnight. Aug. 19, aurora 10 P. M. in N., a few faint beams. Aug. 27, bright aurora 1 A. M. Aug. 30, Low aurora 2 A. M.

Sept. 3, Bright aurora in N. horizon. Sept. 20, bright auroral light on N. horizon. Sept. 26, aurora from midnight till morning. Sept. 30, bright but low aurora 3 A. M.

Oct. 22, Aurora at midnight.

IV. Miscellaneous Scientific Intelligence.

1. *Smithsonian Report for* 1872. 456 pp. 8vo. 1873. (Government Printing Office.)—This is the twenty-seventh of the series of annual Reports of the Smithsonian Institution. The Report appears more than a year after its date; such are the delays at the Government Printing Office. The Report of the Secretary, Prof. Henry, for the year 1872, covers fifty pages of this volume. In it the line of policy which has been steadily followed during a quarter of a century in the government of this institution, under the will of Smithson (of which the United States act only as trustees), is fully set forth, and its essential wisdom shown by its good results. With an income of only about $45,000, by the use of great economy, the Smithsonian conducts a world-wide system of exchanges of books and objects of natural history, having on its lists the names of nearly 3,000 correspondents in all countries where science is cultivated, to whom its publications are regularly sent, and from whom returns reach its archives. The Smithsonian Contributions (quarto) have now reached seventeen ponderous volumes, filled with original memoirs. Its Miscellaneous Contributions (8vo) are composed of (mostly) original memoirs on natural history, meteorology, and other departments of science designed to aid those engaged in original work. These objects, with the custody of its collections, the care of its buildings, and the payment of its staff, required a wise and frugal administration of its funds to avoid the shoals of insolvency. Under the conduct of its distinguished Secretary, and his Assistant Secretary, Prof. Baird, the Smithsonian Institution has attained a position of commanding influence for good in matters of science. A General Index of these annual Reports at the close of the thirtieth volume (that for 1875) would add greatly to their value.

2. *Seventh Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology.* 42 pp. 8vo. Cambridge, 1874.—The Report opens with an account of the Agassiz collection obtained during the voyage of the Hassler, with descriptions of the crania and other specimens. One microcephalic skull, from Ancon, of an individual not quite adult, has the internal capacity 33 cubic inches, or 44 per cent of the average Peruvian cranium, and much smaller than the crania of some Peruvian children not over seven years of age. Though probably idiotic, there are no marked signs of it. The closing article is on Human Remains in the Shell Heaps of the St. John River, East Florida, in which the author, Prof. Wyman, presents reasons for believing that cannibalism was practised by the Indians. In evidence of this, it is stated that the bones were not deposited as in ordinary burial, but scattered in a disorderly manner; secondly, they were broken, as if reduced to a size suitable for the vessels used in cooking; thirdly, there was a degree of method in the breaking of them, showing that it was not done by wild animals.

3. *Geological Survey of Hokkaido: Yesso Coals. A Report by* Henry S. Munroe, E.M. 39 pp. 12mo. Tokei. 1874. Pub-

lished by the Kaita Kushi.—This handsomely printed report (in English) reaches us through Mr. B. S. Lyman, chief geologist and mining engineer. Mr. Munroe has made thorough work of the Yesso coals, giving in a series of tables both proximate and ultimate analyses, and, with the latter, a calculation of the calorific power, evaporative power, and temperature of combustion. The geological horizon of the Yesso coals is not given, but we infer from the following remark by Mr. Munroe that they are probably of Tertiary age. "It will be seen on inspecting this table" (comparative table of coals from different parts of the world) "that these Japanese coals are widely removed by this composition from all coals of similar age, and can be compared with the best Carboniferous coals. They are therefore neither *lignites* nor *brown coals*, but true bituminous coals." This statement the analyses given fully sustains.

Mr. Lyman states the whole thickness of workable and probably good coal, in the Kayanoma field to be at least thirty-five feet, chiefly in beds of from three to seven and a half feet thick.

4. *Supplements aux Notes sur les Tremblements de Terre ressentis de* 1840 *a* 1868; pp. 70, Bruxelles, 1873; and *Note sur les Tremblements de Terre en* 1870 *avec Supplément pour* 1869; par M. Alexis Perrey, Professeur Honoraire a les Facultés des Sciences de Dijon; pp. 146, Bruxelles, 1874.—These are papers presented by Prof. Perrey to the Royal Academy of Belgium, and published in volumes xxiii and xxiv respectively of their *Memoires*. They bring down his published records to the close of 1870. The former adds many notices to those previously published for the years 1843–1868. In the latter, the supplement for 1869 occupies 60 of the 146 pages. In connection with a slight trembling reported at Pulkova, Sept. 20, 1867, the author notices several observations of slight earthquakes made known only by the unusual oscillation of the levels of astronomical instruments.

C. G. R.

5. *Blue Pigment of the Egyptians.*—Fifteen centuries before the Christian era, the Egyptians appear to have been acquainted with the preparation of three distinct kinds of blue pigment, prepared from mixtures of sand, soda, and lime, with oxide of copper. One of these fine colors has been lately examined by M. Henri de Fontenay, who contributes a paper on the subject to the June number of the *Annales de Chimie*. The investigation was conducted in Peligot's laboratory, at the Conservatoire des Arts et Metiers, and some examples of the blue frit were then made at the National Porcelain Factory at Sèvres, under M. Salvétat. The author publishes not only analyses of ancient specimens, but recipes for their imitation. A mixture of 70 parts of white sand, 25 of chalk, 15 of oxide of copper, and 6 of dry carbonate of soda, yielded, when fritted together, a blue material said to be equal in color, texture, and durability to the ancient examples.—*Athenæum*, June 20.

6. *Magnetic Observatory in China*—A magnetic observatory has been established at Zi-ka-Wei, in China, under the superintend-

ence of Father Dechevrens. A first Report of the diurnal variation and of the magnetic intensity for part of March and April last, has been received in this country, from which we gather that the mean of the declination is 1° 53′ 59·8″ W., of the inclination, 46° 13′ 13·7″, and of the intensity, 6·92833. A complete set of self-recording magnetographs, by Adie, has been forwarded to this new observatory; and as Father Dechevrens and his assistant have been well instructed in their use, we may hope that a series of valuable observations of the phenomena of magnetism will be obtained.—*Athenæum*, June 20.

7. *Festschrift zur Feier des hundertjährigen Bestehens der Gesellschaft Naturforschender Freunde zu Berlin.* 264 pp. 4to.—Contains papers presented at the centennial of this Society, July 9th, 1873. The authors are Ehrenberg (on the phosphorescence of the ocean), Rammelsberg (on the chemical nature of some minerals), Rose (on the meteoric iron of Equique), Gerstæcher (on the morphology of *Orthoptera amphibiotica*), Dr. Gustav Fritsch, L. Kny (on the axillary buds of the Florideæ), P. Magnus, E. v. Martens, W. C. H. Peters (on Dinomys, from Peru), Dr. P. Ascherson and Otto Müller. The volume is illustrated by twenty plates.

8. *American Association for the Advancement of Science.*—The twenty-third meeting opens at Hartford, Connecticut, on Wednesday, the 12th of August, at 10 o'clock. Members should report on arrival at the State House, where will be the headquarters of the Association.

9. *French Association for the Advancement of Science.*—The French Association for the Advancement of the Sciences is to hold its third meeting this year at Lille, the session commencing on the 20th, and closing on the 27th, of August. A local committee has organized scientific excursions, under the direction of M. Kuhlmann, corresponding member of the Academy of Sciences, assisted by the mayor of the town and other eminent persons. The President of the Association for the present year is M. Ad. Wurtz.—*Athenæum*, June 20.

10. *Academy of Sciences, Paris.* — A. DeCandolle has been elected Foreign Associate in place of Professor Agassiz. The foreign members of the Academy of this highest grade are entitled to take part in all elections, and enjoy all the privileges of the members in Paris. M. von Kokscharow, of St. Petersburg, has been made Corresponding Member, in place of Professor Sedgwick.

11. *University of Oxford.*—The chair of Geology at Oxford, made vacant by the death of Professor Phillips, has been well filled by the appointment of Mr. Joseph Prestwich, F.R.S., F.G.S.

12. *Half Hour Recreations in Popular Science.* (Estes & Lauriat, Boston.)—No. 11 of this series contains an article by Robert Hunt, F.R.S., on Coal as a Reservoir of Power, and another by Professor Clifford, on Atoms.

13. *On the so-called Land-Plants of the Lower Silurian of Ohio.*—On page 113 of this volume, six lines from the top, after the word exceptions, insert—and those of a very few foreign Lycopodiaceous plants: a clause inadvertently omitted. J. S. N.

THE

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[THIRD SERIES.]

ART. XV.—*On the possible Variability of the Earth's Axial Rotation, as investigated by Mr. Glasenapp*; by SIMON NEWCOMB.

IN the number of this Journal for September, 1870, I called attention to the fact that there were apparent inequalities of long period in the motion of the moon, which had not been accounted for by the gravitation of the known bodies of the solar system, and suggested that they must be accounted for in one of three ways: either (1) there are inequalities in the motion of the moon, due to the gravitation of the sun and planets, which have hitherto eluded mathematical computation; or (2) the motion of the moon is affected by the action of some other forces than that of gravitation; or (3) the time of the earth's rotation on its axis, and therefore the length of its sidereal day, is subject to irregular variations of long period. Of these three exhaustive hypotheses the second was shown to be improbable, because such forces would be likely to produce either regular inequalities of short period, or progressive secular variations, instead of the very slow and irregular changes actually observed. If, then, the first hypothesis were excluded, we should have to fall back on the third as the most probable explanation.

The investigation of the first hypothesis is a purely mathematical process, admitting in theory of being carried through with any degree of rigor. Since the publication of the paper in question I have been engaged in this investigation, and although the pressure of other engagements has prevented its completion, it is so far advanced as to make it quite improbable that there are any other inequalities of long period in the motion of the moon produced by the gravitation of the planets than the one due to the action of Venus, discovered by Hansen. Assuming provisionally that this result is correct, and that the gravitation of the known bodies of our system cannot produce the observed inequalities of long period, we are forced to accept the hypothesis of the variability of the sidereal day as a provisional theory. At the same time, so long as we have no independent proof of such variability, it cannot be accepted as an established fact. It therefore becomes very desirable to find some independent test of the invariability of the sidereal day. In the paper referred to it was remarked that observations of the interior planets, especially transits, might afford such a test. But it has since occurred to me that eclipses of the first satellite of Jupiter might afford a yet better and more decisive test. The definitive and exhaustive application of this test would require the complete re-investigation of both the theory and observations of Jupiter's satellites, a work for which I have collected some of the materials, but which I have not been able even to commence.

There is, however, one circumstance which rendered the satisfactory application of the test very easy. Granting that the inequalities were really to be accounted for by changes in the earth's rotation, the most extraordinary and sudden change of which we have knowledge occurred about 1860. The velocity of rotation, which for the ten or twenty years previous had been rather slower than the average, was then suddenly accelerated, so as to cause a subsequent gain of perhaps a second per annum, which continued at least till 1872. Collecting all the accessible observations of Jupiter's first satellite from 1850 to 1871, a similar change seemed to be indicated by them, but it was only about half as great as that indicated by the moon, and no greater than its possible error; so that the result did not in any considerable degree affect the probability of the hypothesis.

Last summer I learned that Mr. Glasenapp, of the Pulkowa Observatory, was engaged in an extended investigation of recent observations of the satellites of Jupiter; and, as it seemed desirable that the proposed test of the hypothesis should be made by another, I requested Mr. Glasenapp to try whether the times of the eclipses of the first satellite would be better represented when they were corrected for the hypothetical changes in the earth's rotation. Assuming the earth to be correct at the epochs 1840 and 1870, we should have *earth time slow* by the following amounts at certain other epochs:

Year.	s.	Year.	s.	Year.	s.
1850·5	+ 2	1862·5	+11	1868·5	+ 2
1855·5	+ 5	1864·5	+10	1870·5	0
1860·5	+10	1866·5	+ 6	1872·5	− 2

Mr. Glasenapp has just published his paper in the Russian language, the concluding part of which is devoted to this investigation. He has also sent me the following more extended account of his investigation, from which I omit the citation of the original observations.

"A great many preparations for the expedition of the transit of Venus, and the publishing of a Russian memoir on the observations of Jupiter's first satellite, have not allowed me to take up the question of the variability of the earth's axial rotation, which you proposed to me some time ago. But now I have investigated this very interesting question, and the result seems to me to be satisfactory, so that your hypothesis is very probable.

"In the investigation I have chosen the two following ways: (1) I have tried if the corrections of noon:

Year	1845	+ 1^s	(1)
	1850	+ 2	
	1855	+ 5	
	1860	+10	
	1862	+11	
	1864	+10	
	1866	+ 6	
	1868	+ 2	
	1870	0	
	1872	− 2	

you sent to me in your letter dated October 24, 1873, applied to the observed times of the eclipses of Jupiter's first satellite, will bring them in better agreement with the tables of Damoiseau than the uncorrected observations. The result I obtained is favorable, that is, the observations corrected by the quantities (1) are represented better than the uncorrected ones: thus your corrections seem to be real.

"(2.) I determined the corrections of Damoiseau's ecliptic tables of the first satellite for 44 different epochs (22 epochs from eclipse disappearing and 22 from eclipse reappearing), and it was evident that these corrections, and consequently the corrections of noon (when the corrections of Damoiseau's tables are considered as constant quantities) change in the same manner as the corrections (1) you obtained from the observations of the moon, and that their periodicity is very near the same you have found.

"Allow me to communicate to you the whole investigation of this interesting problem.

1. *First investigation.*

"All the observations of the eclipses of the first satellite (as disappearing, as reappearing) from 1848 till 1873, which I could find in astronomical literature, have been reduced to a homogeneous form, that is, they were corrected for the influence of the different dimensions of object glasses, for the influence of Jupiter's different zenith-distances at the time of observation, for the influence of different distances of Jupiter from the earth, and for the influence of all other circumstances which may change the apparent brightness of the satellite, and which can be taken into consideration. Each observation gives an equation of condition of the form:

$$x+\Delta\,.\,y+\rho\,.\,z+(C-O)=0, \qquad (2)$$

where x is the correction of the ecliptic tables, Δ the distance of Jupiter from the earth, y the correction of the adopted velocity of light ($493^s\cdot2$ by Delambre), and $\rho\,.\,z$ a correction to be applied to the observations for different distances of the satellite from the center of Jupiter. The member $\rho\,.\,z$ must be introduced in the equation of condition, because all the reductions of the observations are made by Bailly's formula * (plus a constant member C), which is probably imperfect.

"The solution of the equations (2) gives:

a. Eclipse disappearing.	*b.* Eclipse reappearing.	
$x_0=-60^s\cdot6\pm8^s\cdot0$	$x_1=-9^s\cdot0\pm8^s\cdot0$	
$y_0=+10\cdot52\pm1\cdot74$	$y_1=-1\cdot47\pm1\cdot26$	(3)
$z_0=-1\cdot97\pm3\cdot54$	$z_1=+1\cdot60\pm2\cdot56$	
$\gamma_0=\pm9^s\cdot89$	$\gamma_1=\pm9\cdot09$	

(γ_0 and γ_1 are the probable errors of an observation whose weight is unity.)

"Then the same observations were corrected by the quantities (1) and discussed in the same manner as the uncorrected observations; the result will enable us to see if the corrections (1) represent better the observations of the eclipses.

"Each corrected observation gives us an equation of condition of the same form as (2):

$$x+\Delta y+\rho\,.\,z+(C-O_1)=0, \qquad (4)$$

in which only the last member O_1 has another signification, because the observed times of the eclipses are all corrected by means of the table (1). The solution of these equations give the following results:

a. Eclipse disappearing.	*b.* Eclipse reappearing.	
$x'_0=-42^s\cdot3\pm7\cdot9$	$x'_1=-8^s\cdot7\pm6^s\cdot0$	
$y'_0=+6\cdot83\pm1\cdot72$	$y'_1=-1\cdot12\pm1\cdot24$	(5)
$z'_0=-0\cdot64\pm3\cdot50$	$z'_1=+1\cdot82\pm2\cdot52$	
$\gamma'_0=\pm9\cdot77$	$\gamma'_1=\pm8\cdot97$	

"If you compare the values of the probable errors γ_0, γ_1, γ'_0, γ'_1, in both cases, you will see that by means of the correc-

* Histoire de l'Académie, Paris, MDCCLXXI, p. 580.

tions (1) the observations of the eclipses of Jupiter's first satellite are represented somewhat better than the uncorrected observations. Therefore we can conclude that these corrections may be real, and that your hypothesis on the variability of the earth's axial rotation may be true.

2. *Second Investigation.*

"The second way I choose for the decision of the same question is to determine the corrections of the ecliptical tables of the first satellite for different epochs, and to deduce from these corrections the corresponding ones of mean noon for the same epochs.

"The values of x_0, y_0, z_0 and x_1, y_1, z_1 (3), which were deduced from the observations without the quantities (1) give the following corrections of Damoiseau's tables:

(1.) Correction of the tabular mean longitude of Jupiter's first satellite, ---- $=-27^s{\cdot}7\pm4^s{\cdot}8$

(2.) Correction of the tabular semi-duration of the eclipse, ------------------ $=-49^s{\cdot}1\pm4^s{\cdot}8$

(3.) Correction of Delambre's velocity of light (of the quantity of $493''{\cdot}2$) ---- $=+7^s{\cdot}64\pm1^s{\cdot}02$

(4.) Value of z, which is negative for disappearances of the satellite, and positive for his reappearance, --------- $=1^s{\cdot}73\pm2^s{\cdot}07$

and with these values we correct the moments of eclipses as given in the Nautical Almanac (Damoiseau's tables) and calculate the quantities (C−O)—that is: calculation—observation—for each eclipse observed since 1848.

"From these quantities we derive the following corrections of Damoiseau's ecliptic tables of the first satellite.

a. Eclipse disappearing.

	C—O	Weight.
1848·86	+ 6·9	4·7
49·22	+ 6·7	3·4
50·00	+22·7	2·4
51·10	+ 9·2	5·3
52·70	+22·2	1·6
55·50	+33·2	2·0
56·58	+20·9	4·7
57·72	+16·3	22·7
58·82	+11·5	7·7
59·90	+22·9	2·0
61·22	+14·5	3·3
62·17	+29·0	2·9
63·15	+22·0	3·5
64·28	+ 4·7	7·5
66·45	+22·6	1·8
67·63	+33·9	2·6

b. Eclipse reappearing.

	C—O	Weight.
1848·24	−16·7	7·7
49·27	− 4·5	8·5
50·32	−15·4	7·6
53·50	−30·7	1·9
55·70	−17·2	2·7
56·97	−11·5	3·6
58·10	−12·2	4·5
59·15	− 5·9	9·7
60·21	− 6·3	8·1
61·30	+ 4·4	0·9
62·32	−12·7	4·4
63·37	−28·3	3·6
64·60	−11·5	1·6
65·70	−27·8	0·7
66·72	−18·8	3·3
67·82	−16·9	4·9

a. Eclipse disappearing.	C—O	Weight.	*b.* Eclipse reappearing.	C—O	Weight.
1868·65	+33·8	2·0	1868·83	−16·5	6·7
69·76	+15·2	12·5	69·45	−23·3	2·0
70·84	+ 8·4	6·3	70·05	−24·2	1·8
71·90	− 2·6	9·6	71·14	−20·6	8·0
72·50	+ 5·3	8·2	72·21	−17·4	21·9
73·25	+15·0	10·2	73·24	−16·5	26·4

"To discuss these corrections by the method of least squares, let us adopt the following form for the equations of condition :

$$x+k(t-t_0)+m(t-t_0)^2+C-O=0,$$

where t is the year of observation, $t_0=1861{\cdot}00$, x the correction of the tables for 1861·00, k and m the co-efficients to be determined from the observations.

"From these equations we obtain the following normal equations :

a. Eclipse disappearing.

$$+126{\cdot}9x+259{\cdot}3k+8227m+1718=0$$
$$+259{\cdot}3x+8227k+35467m+1706=0$$
$$+8227x+35467k+923395m+85480=0$$

b. Eclipse reappearing.

$$+140{\cdot}5x+455{\cdot}6k+12239m-2111{\cdot}8=0$$
$$+455{\cdot}6x+12239k+55150m-9823=0$$
$$+1223{\cdot}9x+55150k+1558741m-196478=0$$

which give for x, k, and m the values:

a. Eclipse disappearing. *b.* Eclipse reappearing.

$$\left.\begin{array}{ll} x_0=-17{\cdot}76\ \pm1.25 & x_1=+12'69\ \pm0{\cdot}86 \\ k_0=+0{\cdot}08317\pm0{\cdot}1553 & k_1=+0{\cdot}25118\pm0{\cdot}09172 \\ m_0=+0{\cdot}06245\pm0{\cdot}02178 & m_1=+0{\cdot}01753\pm0{\cdot}01356 \end{array}\right\} \quad (7)$$

"If the reduced observations were quite homogeneous, no difference should be found between the quantities k and m, deduced respectively from the disappearances and reappearances of the first satellite; but the difference we have found between the two values of k and m is larger than their probable errors allow it, so that properly we have no right to take the mean of the values k_0 and k_1, m_0 and m_1, and to consider it as the most probable value of m and k. Still we take these means and consider them as the most probable values because we do not know the cause of this discordance, and cannot therefore make any other combination of the values k_0 with k_1, and m_0 with m_1.

"The combined values of x, k and m are

$$\left.\begin{array}{l} x=+3''{\cdot}52\ \pm0{\cdot}71 \\ k=+0{\cdot}2076\pm0{\cdot}0783 \\ m=+0{\cdot}3006\pm0{\cdot}01151 \end{array}\right\} \quad (8)$$

"The probable error of k is very large in comparison with its whole value, and this arises from the want of a sufficient number of observations of the eclipses. Indeed, if you consider the corrections of Damoiseau's tables, you can easily see that very few observations are made during many years, and during some not at all.

"By means of the quantities (8) we calculate the following corrections of the tables for the same epochs, for which you obtain the corrections (1):

1850·5	+4·6	(9)
55·5	+3·4	
60·5	+3·4	
62·5	+3·9	
64·5	+4·5	
66·5	+5·5	
68·5	+6·8	
70·5	+8·2	
72·5	+9·9	

And if the corrections of the tables may be considered as constant quantities, the values (9) with opposite signs will be the corrections of noon plus any constant quantity; thus when we change the signs in the quantities (9) and add to each of them the constant $+7^s·9$, so that at 1872·5 the correction of noon shall be the same as you have found (1), we obtain the following values for the correction of noon.

Correction of noon.		Correction by S. Newcomb.
1850·5	+3″	+ 2″
55·5	+4	+ 5
60·5	+5	+10
62·5	+4	+11
64·5	+3	+10
66·5	+2	+ 6
68·5	+1	+ 2
70·5	0	0
72·5	−2	− 2

"The comparison of these series shows:

1. That the change of the corrections is similar in both series.
2. That the periods are very near each other.
3. That the maxima coincide.

"Although the corrections themselves, in both series, differ much from each other, so that they alone cannot give us the right to make any conclusion on their reality, but as the disappearances of the first satellite, as the reappearances (*a*) give the same sign for the values of k and m, (*b*) that the change of the corrections (*g*) is similar in both series, (*c*) that the periods of these changes are very near the same, in both cases, and (*d*) that the maxima coincide,—it seems to me that we have the whole right to ascribe reality to your remarkable hypothesis on the variability of the earth's axial rotation."

On this paper of Mr. Glasenapp I remark that the case does not seem to me so well made out as he considers it. The hypothesis to be tested is not simply that the rotation of the earth is variable, but that the outstanding errors in the moon's place are due to this variation. The hypothesis can be made more probable only by showing that the eclipses of Jupiter's satellites are better represented when the hypothetical corrections are applied. Now, by some unfortunate concurrence of errors of theory or observation, the results from Jupiter's satellite seem to fall just half way between the two hypotheses, (1) of invariable sidereal day, and (2) of such variable sidereal day as will account for the apparent errors of lunar theory, and therefore leave the question undecided.

The question may, however, arise, whether the corrections of "earth time" already cited are the only ones which will represent the motion of the moon. On this I remark that there is one doubtful term of long period in Hansen's tables of the moon which I did not take out in making the comparison on which the above correction is founded. This term is that depending on the longitude of the moon's node, which Hansen has found more than a second larger than Airy and others have from observations. I shall, therefore, determine the outstanding mean errors in the lunar theory when we take from Hansen's tables; (1) the excess of his secular acceleration over theory, $5''\cdot 4T^2$; (2) the empirical term depending on the action of Venus; and (3) the above mentioned excess of his value of the 19 year term, assuming it to be 1″. The following table,

Year.	Correction given by		Concluded Correction	Errors of Hansen's Theory.			Corrections of pure theory.		Earth slow.
	Greenwich.	Washington.		(1.)	(2.)	(3.)	(1.)	(2.)	
	″	″	″	″	″	″	″	″	s
1850	+0·3	−1·3	0·0	+0·05	+0·56	+0·7	+1·3	−0·4	−0·8
51	+1·5	+0·1	+1·3	0·04	0·45	+1·4	+2·8	+1·4	+2·7
52	+0·9	----	+0·9	+0·03	0·36	+1·5	+2·3	+1·2	+2·3
56	+1·0	----	+1·0	0·00	0·09	+0.3	+1·4	+1·5	+2·8
57	+1·5	----	+1·5	----	0·02	−0·1	+1·4	+1·8	+3·4
58	+2·0	+1·5	+1·8	----	0·05	−0·4	+1·4	+2·1	+4·0
62	+2·4	+2·4	+2·4	----	0·05	−1·0	+1·4	+3·3	+6·3
63	+2·2	+1·2	+1·7	0.00	0·02	−0·8	+0·9	+3·1	+5·9
64	+0·1	−1·0	−0·4	+0·01	0·09	−0.6	−0·9	+1·6	+3·0
65	−1·1	−2·4	−1·7	0·01	0·14	−0·3	−1·5	+1·3	+2·5
66	−2·2	−2·5	−2·4	0·02	0·20	0·0	−2·2	+0·9	+1·7
67	−3·9	−4·1	−4·0	0·03	0·28	+0·3	−3·4	0·0	0·0
68	−4·4	−4·5	−4·5	0·03	0·36	+0·6	−3·5	+0·2	+0·4
69	−4·6	−5·5	−5·0	0·04	0·45	+0·8	−3·7	+0·3	+0·6
70	−5·0	−6·1	−5·5	0·05	0·56	+0·9	−4·0	+0·3	+0·6
71	−7·0	−7·2	−7·1	0·06	0·67	+1·0	−5·4	−0·8	−1·5
72		−7·8	−7·8	+0·07	+0·80	+1·0	−5·9	−1·0	−1·9

partly taken from my former paper, shows the correction of Hansen's tables given by the Greenwich and Washington observations, and the modification of the theory just mentioned. In the second column of "corrections to pure theory," the theoretical annual motion of the moon is supposed to be diminished by $0''\cdot30$.

The difference between the numbers in the last column and those formerly cited arises almost entirely from the change of $1''$ in the value of Hansen's nineteen-year coefficient. Let us now treat Mr. Glasenapp's residuals by a method intermediate between the two which he adopts. I have divided his residuals into ten groups, and in order to make the observations of ingress and egress comparable, have corrected them as follows:

The ingress residuals by $-15^s\cdot2+0^s\cdot08\ (t-1861\cdot0)$
The egress residuals by $+13\cdot4\ +0\cdot25\ (t-1861\cdot0)$

and have then taken the mean by weights of each group. The results are as follows:

Excess of Theory over observation.

Ingress.			Egress.			Combined.			
							C–O.		
Date.	C–O.	Wt.	Date.	C–O.	Wt.	Date.	Uncorrect'd	Corrected.	Wt.
	s.			s.			s.	s.	
1849·0	− 9·4	8	1848·8	+ 0·1	16	1848·9	−3·1	−2·1	24
50·7	− 2·6	8	50·3	− 4·7	8	50·5	−3·6	−3·6	16
52·7	+ 6·2	2	53·5	−19·2	2	53·1	−6·5	−9·0	4
56·3	+ 9·0	7	56·4	− 1·9	6	56·3	+3·9	+0·9	13
58·5	− 0·4	30	58·8	+ 4·9	14	58·6	+1·3	−2·7	44
61·1	+ 6·7	8	61·0	+ 5·7	13	61·1	+6·1	+0·1	21
63·7	− 1·1	16	63·7	− 8·8	5	63·7	−3·0	−8·5	21
67·6	+15·1	6	67·8	− 2·5	16	67·8	+2·3	+1·7	22
70·5	− 4·0	9	70·7	− 5·9	12	70·6	−5·2	−5·2	21
72·6	− 8·1	28	72·8	− 0·6	48	72·7	−3·4	−1·4	76

The columns C–O show the residuals, (1) when the length of the day is supposed uniform; (2) when the times are corrected for hypothetical "earth slow" from the last column of the last table. We have now to see which series can be best represented by an expression increasing uniformly with the time. Solving by least squares we find the expressions to be:

$$\text{Uncorrected residuals} = -0^s\cdot80 - 0^s\cdot100(t-1860\cdot0)$$
$$\text{Corrected residuals} = -2\cdot45+0\cdot039(t-1860\cdot0).$$

Subtracting the value of these expressions, we find the residuals still outstanding to be as follows:

Uncorrected.	Corrected.	Wt.
$-3^s.4$	$+0^s.8$	24
-3.7	-0.8	16
-6.4	-6.3	4
$+4.3$	$+3.5$	13
$+2.0$	-0.2	44
$+7.0$	$+2.5$	21
-1.8	-6.2	21
$+3.9$	-3.8	22
-3.3	-3.2	21
-1.2	$+0.6$	76

It will be seen that of the ten residuals, all but one are diminished by the application of the hypothetical corrections. Although the observations are too uncertain, and the residuals too irregular, to regard this result as proving the hypothesis, yet it seems to me to render it worthy of reception as being, in the present state of our knowledge, the most probable explanation of the outstanding differences of long period between the theoretical and observed longitude of the moon.

ART. XVI.—*Researches in Acoustics;* by ALFRED M. MAYER. Paper No. 5.

(Continued from page 109.)

5. *Six Experimental Methods of Sonorous Analysis described and discussed.*

THE remarkable discoveries in sound made in these later times by Helmholtz were owing, in great part, to his having early seen the necessity of obtaining precise knowledge of the composition of sounds,—by means of the methods of sonorous analysis which he had devised,—before one could attempt to give an explanation of the causes of timbre, of the mechanism of audition, and of the physiological causes of musical harmony; and furthermore, he reduced all of the analytic methods and explanations, contained in his classical work, to a harmonious system, by showing how they naturally flowed from the fertile theorem of Fourier. As Helmholtz distinctly states: "In letzter Instanz ist also der Grund der von Pythagoras aufgefundenen rationellen Verhältnisse in dem Satze von Fourier zu finden, und in gewissem Sinne ist diese Satz als die Urquelle des Generalbasses zu betrachten." (Tonempfindungen, p. 346.)

The evident importance of the subject of sonorous analysis will probably render interesting the few remarks I here venture to offer on this subject. I will describe in order six methods of sonorous analysis. Methods 1, 2 and 5 have been used by Helmholtz and König. Methods 3, 4 and 6, as far as I know, originated with me. To render comparable all of the experiments which I shall describe, I shall always use one composite sound of uniform intensity by sounding with a blast of constant pressure an Ut_2 Grenié free-reed pipe, from which has been removed its reinforcing pyramidal pipe.

(1.) *Analysis by means of Resonators applied directly to the ear.*

This method of Helmholtz is so well known that it need not here be described, but I will give some experiments which I have made on its degree of precision under the head of the sixth method of analysis, to be described.

(2.) *Analysis by means of Resonators connected with König's manometric flames.*

This is the least delicate and accurate of all methods of sonorous analysis, but it has a value in giving an objective, ocular illustration, which is sometimes of use. I do not, however, here refer to the use of a manometric flame placed in connection with a conical pipe, into which one sings, and which instrument in the hands of König has done such admirable work in the analysis of the vowel sounds; but I refer to the harmonic series of resonators connected with as many flames which burn near a long revolving mirror.

The following experiments will show the degree of delicacy of the above apparatus. I sounded the reed pipe with its maximum intensity when it was about one foot from the center of the series of resonators, and produced well marked serrations in the Ut_3 and Ut_4 flames, but the other flames showed only slightly indented top borders. It was only when I sang loudly Ut_3 or sounded this same note on a French horn that the edges of the flames became deeply serrated.

(3.) *Analysis by means of Resonators which are successively brought near the source of origin of a composite sound and thus successively reinforce all of its sonorous elements.*

If we take any two resonators separated by a known interval, and vibrate before them the forks of this interval, we can carry these sounds in the memory. Now close the nipples of the resonators with wax and successively bring their mouths near the origin of the composite sound. If the simple sounds to which the resonators are tuned exist as components of the sound, we shall hear them singing out clearly above the general chorus of the other harmonics. Thus I have often successfully shown to an audience the composition of a composite sound.

(4.) *Analysis by means of Resonant boxes carrying solid bodies tuned in unison with the sounds to which the air in these boxes resounds.*

This method is an excellent one when the composite sound can be obtained with intensity, when the boxes are accurately in tune with the solid bodies (forks or strings) attached to them, and when the boxes are in unison with the sonorous elements of the composite sound which we would analyze. The last mentioned condition is not always fulfilled in the boxes on which forks have been sometime mounted, for the former are apt to change their interior capacity by warping. This fact can readily be ascertained by partly introducing the hand into the mouth of the box, and noting the effect on the intensity of the sound.

This method of analysis is similar to the one previously described; for the resonant box of a fork acts like a resonator and can be used to intensify any harmonic of a composite sound; but there is an important difference in the methods, for the fork or the string being in unison with the proper note of the mass of air in the box, is set in vibration by the latter, so that after the box has been removed from the vicinity of the origin of the composite sound, and the latter has ceased, we find that the fork sings out alone, and thus shows that it has selected from a chorus of harmonics that one which is in unison with its own tone. I have thus been able, by placing one fork after another, of the series of the harmonics of the Ut_2 reed, to show the composition of its sound to a large audience with entire satisfaction. I have also succeeded with the following experiment. Forcibly sound the reed, and place around the opening of its "stump" all of the eight forks, of the harmonic series of Ut_2, with the mouths of their resonant boxes toward the reed. After the reed has sounded for a few seconds, stop it, and we shall find that all of the forks are in vibration; and thus singing together, they approximately reproduce the sound of the reed. This experiment to succeed requires the resonant boxes, the forks, and the harmonics of the reed to be in exquisite unison.

(5.) *Analysis by means of the beating of simple sounds of known pitch with the harmonics of the composite sound to be analyzed.*

If we use forks for the simple sounds, it will be better slightly to flatten or sharpen the note of the sound to be analyzed. Then knowing the number of beats that the fundamental of the note gives with its corresponding fork, we can designate the number of any other harmonic by the number of beats it makes with a fork of known pitch; for the number of beats observed, when referred to the number of beats of the fundamental as unity, will be directly as the number of the harmonic in the series. If we cannot well alter the pitch of the composite sound, then it will be well to use forks with sliding weights on graduated prongs, or loaded strings accurately tuned and provided with meter scales. In this system of analysis it is very important to guard against being led astray by the beating of resultant tones; therefore, the forks or strings should be gently vibrated and resonators used to assist the ear.

(6.) *Analysis by means of a loose membrane which receives the composite sonorous wave and transmits its vibrations through filaments or light rods to a series of forks mounted on their resonant cases.*

This method of analysis is the one we devised in our experimental confirmation of Fourier's theorem and described in sec. 1 (pp. 82–84). We here wish to call attention to the precision of this method of analysis, while at the same time acknowledging the delicate instrumental conditions required to use it successfully. The principal interest attached to it is that it shows in a vivid manner the operation of the instantaneous decomposition of a composite wave into its elementary pendulum-vibrations. The method has also peculiar interest as showing, in the most striking manner, the exaltation of the action of very feeble periodic impulses to such degrees of intensity as to set into synchronous vibration very large masses of matter; and it may be well before we discuss the subject proper of this section to call attention to this very interesting result, as set forth in the following experiment with our apparatus. To the membrane, covering the hole in the box of the reed-pipe, I attached one end of a fiber of silk-worm cocoon, one meter long and weighing one milligram. The other end of the fiber was cemented to the face of a prong of an Ut_2 fork. This fork weighed 1,500 grams, while the top of its resonant box and the air in the latter weighed respectively 102 and 22 grams. Therefore, the fiber set in motion 1,624,000 times its own weight by only a fraction of the force which traversed it, and even this force was a yet more minute fraction of the whole energy of the aerial vibrations produced by the reed.

An experiment like the above is instructive as an analogical illustration of the manner in which we may imagine an ætherial vibration to produce chemical decomposition by causing such powerful synchronous vibrations in the molecules of compounds as to shake their atoms asunder; and we have already seen how very feeble impulses sent through a medium of great tenuity can, when rapidly recurring and of the proper period, produce mechanical effects which at first sight appear incredible. Time is required in both cases to produce an appreciable action. The time required in the case of the sonorous vibrations decreases as their number per second increases, and in the case of the ætherial vibrations we have analogical phenomena. In the acoustical experiment, if the fork be much out of tune with the pulses transmitted by the fiber, no motion is produced in the fork; likewise, we may imagine that when the period of vibration of one or more of the constituent atoms of a certain molecule is far removed from unison with any of the ætherial vibrations falling upon it, no motion, or chemical decomposition, will ensue.

The analogy between the two classes of phenomena is yet more striking when we remember that the fork *selects*, from the composite vibratory motion which traverses the fiber, only that vibration which is in unison (or only slightly removed from unison) with its own proper periodic motion; so, likewise the molecule, or atoms of the molecule, select only those vibrations from the ray which are in unison with their own atomic periods; and on the tuning of the atom depends whether the result of the action of the ray will evince itself as heat, phosphorescence, chemical action, or fluorescence.

The following experiments show that the method of analysis we are now discussing surpasses in delicacy and sharpness of definition any other method in which sympathetically vibrating bodies are employed. As already shown, the forks select from the composite vibratory motion which strikes them only those simple vibrations which are in unison with their own vibratory periods. This remark, however, requires some modification, though the qualification necessary is less than is required when other similar methods of analysis are used. In all cases of co-vibration there is a certain range of pitch, above and below the sound which is in unison with the existing vibration, through which the co-vibrating body responds. The farther the remove from unison the weaker the response.* But in some cases a slight remove of the pitch from unison will cause a great diminution in the intensity of the co-vibrations, while in others the same departure from unison causes only a slight or even inappreciable change in their intensity. For example, I connected the Ut_3 fork—the second in the harmonic series of Ut_2—to the membrane on the Ut_2 reed, and sounded the latter during a few seconds. After the reed was silent, I heard the fork sounding with intensity.† But, on loading the fork with a

* See Helmholtz's Tonempfindungen, p. 217, for the law connecting the variation of intensity of co-vibration with the variation of pitch in the existing body.

† It is perhaps hardly necessary to state that in all of these experiments I first ascertained that when the fiber was detached from the fork, the latter did not emit a sound caused by the action of the intervening vibrating air. This condition is readily attained by screening the mouths of the resonant boxes, or by turning these mouths away from the sounding reed. Also, I should here state that the reed was tuned to the fork after the fiber was stretched between the latter and the membrane on the reed-pipe.

piece of wax, so that it gave *five* beats per second with the note of the fork when unloaded, I could not, by any variation in the tension of the fiber or of any other circumstances of the experiment, set in vibration the fork by means of the pulses sent from the reed through the fiber; yet, on placing the nipple of an Ut_3 resonator in my ear, I perceived that this flattened note of the fork produced a decided resonance, thus showing that although the fork could not respond to its own note flattened five beats per second, yet the resonator, under the same circumstances, did enter into sympathetic vibration. When the fork gave *four* beats per second it responded to the reed, but this response was only audible on placing the ear close to the mouth of the fork's resonant box. With *three* beats per second the sound of the fork was readily perceptible, while the resonator reinforced it very decidedly. When the fork was out of unison *two* beats per second, its sound was slightly increased; and with a departure of *one* beat per second, the response of the fork was yet stronger, but greatly inferior in intensity to that produced when the fork was in unison with its proper sound—the second harmonic of the reed; yet the resonator reinforces this flattened sound as forcibly as it does that which emanates from the unloaded fork. These facts concerning the want of sharpness in the detection of pitch by means of resonators are not in accordance with the statements made in recent popular works on sound, where the resonator is described as remaining dumb until the exact pitch to which it is tuned is reached, when it responds with a suddenness which has been compared to an explosion!

The fork, the stretched fiber and the intensity of the sound of the reed remaining in the same conditions as in the above experiments, I gradually unloaded the fork until it made only *one beat in eight seconds*, and yet even this slight departure from unison with the second harmonic of the reed was evident in the difference in the intensities of the fork's responses when thus loaded and when the wax was removed. This fact I have repeatedly confirmed by testing the intensities of the two sounds by different hearers, who were placed so that they could not see when the fork was loaded or unloaded. Now E. H. Weber has found that only the most accomplished musical ears can distinguish between the pitch of two notes whose vibrations are as 1000 to 1001, but by the above method we can readily detect a departure from unison in the two notes amounting to the interval of 2000 to 2001, or to the $\frac{1}{120}$th of a semitone.

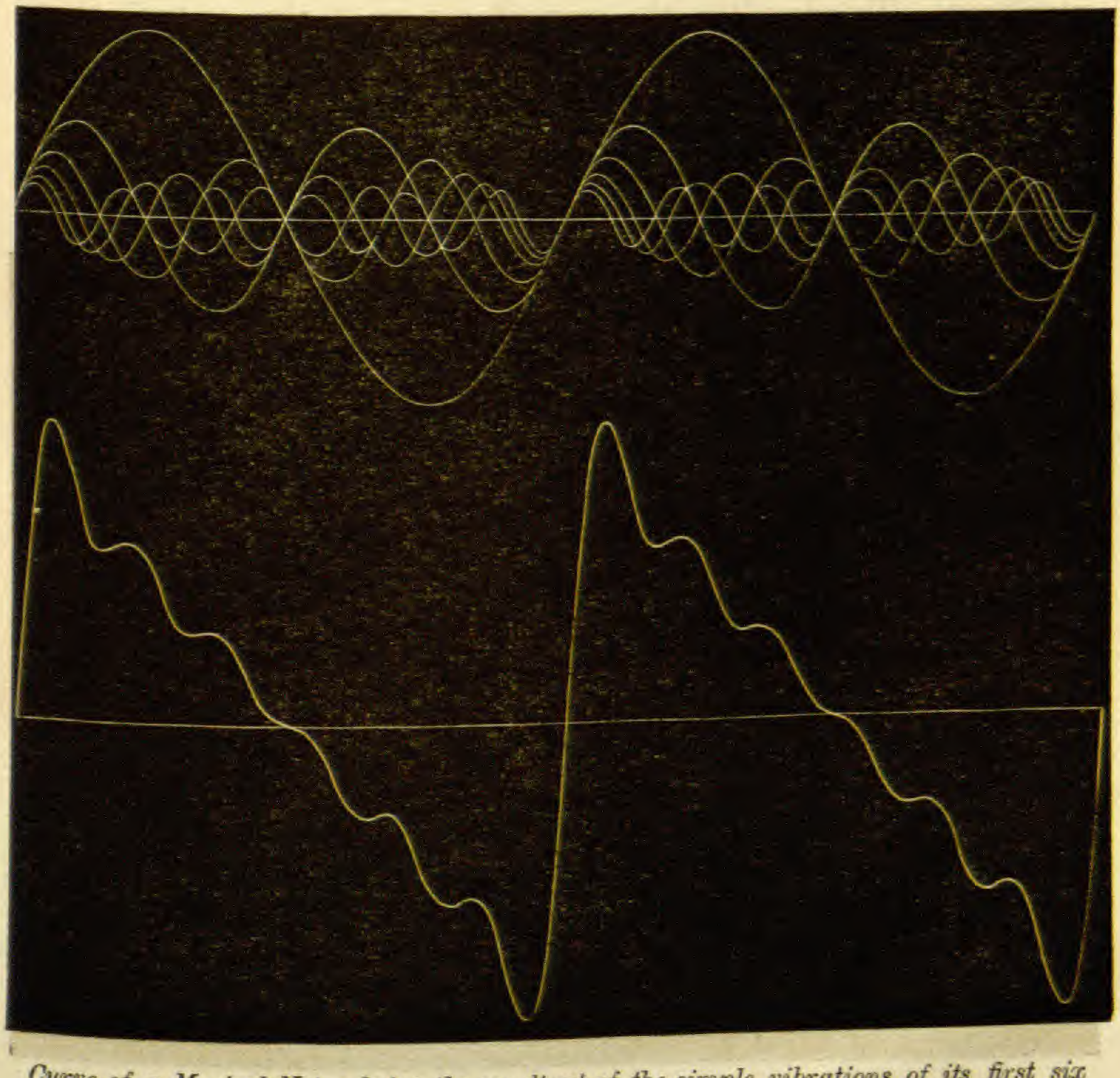
In connection with the preceding observations, the following experiments on resonators and sympathetic vibrations may be of interest. I substituted for the ear the manometric flames of König, viewed in a revolving mirror, and tested the response of the resonators to sounds not in accord with their proper notes. The results agreed with those previously obtained on placing the resonator to the ear. I now mounted an Ut_3 fork on its resonant case, and sounding it *strongly* before all the resonators of the harmonic series of Ut_2, I caused all of the manometric flames connected with these resonators to vibrate, each giving the same number of serrations as when the Ut_3 fork was brought near its own resonator. The same result was obtained when the fork was separated from its case, with this important difference, however, that when the face of fork Ut_3 was brought near the mouth of the Ut_4 resonator, the flame connected with this resonator gave at the same time serrations belonging to both Ut_3 and to Ut_4, and this result accords with the following experiment. If one sounds the Ut_3 fork on its box, and brings the mouth of the latter near the mouth of the box of the Ut_4 fork, the Ut_4 fork will co-vibrate, and after Ut_3 has ceased to vibrate, Ut_4 will sound out quite clearly. If, however, Ut_4 be lowered in tone, by weighting it with a piece of wax, so that it gives two beats per second with its proper tone, then the Ut_4 fork cannot be set into sympathetic vibration by impulses from Ut_3. Also, if forks Ut_3 and Ut_4 be detached from their boxes, and fork Ut_3 be strongly vibrated while the face of one of its prongs remains quite close and parallel to the face of a prong of fork Ut_4, the latter is set in vibration by the impulses of Ut_3 sent through the intervening air. Fork Ut_3 was now loaded so that it successively made 1, 2, 3, and 4 beats per second with the true Ut_3 pitch. When it made 3 beats per second, it caused Ut_4 to vibrate so feebly as to be barely audible, while 4 beats per second departure of Ut_3 from unison produced no effect on Ut_4.

The following experiment was now made to show the want of precision in the determination of the exact pitch of sonorous elements by means of resonators. We have just seen that the Ut_3 fork could not vibrate the Ut_4 fork when both forks were on their cases, and when Ut_3 was flattened by two beats per second; and also that a departure of four beats per second prevented Ut_3 from setting Ut_4 in sympathetic vibration, when both forks were off their resonant cases, and with their prongs close and parallel to each other. But, when the Ut_3 fork was loaded so that it made from 15 to 20 beats with its proper tone, it caused the serrations of the Ut_3 resonator to appear, accompanied by the serrations of its octave. The Mi_3 fork, although it developed the serrations belonging to its own pitch when brought opposite the Ut_3 resonator, yet did not,—as might have been expected,—develop its own tone and the octave when brought near the mouth of the Ut_4 resonator. It is probably not necessary to add that the above effects of simultaneously developing in the flame the serrations of the proper note of a resonator and those of its octave are only produced when the fundamental sound is intense.

(7.) *The Curve of a Musical Note, formed by combining the sinusoids of its first six harmonics; and the curves formed by combining the curves of musical notes corresponding to various consonant intervals.*

We have already seen that any composite vibration, which produces in us the sensation of a musical note, can always be reproduced by the simultaneous production of a certain number of the simple sounds of a harmonic series, provided these simple sounds have the proper relative intensities. Therefore to obtain the resultant curve corresponding to a musical note, we draw on one axis its harmonic components with their proper wave-lengths and amplitudes, and the algebraic sums of their corresponding ordinates are the ordinates of the required resultant curve.

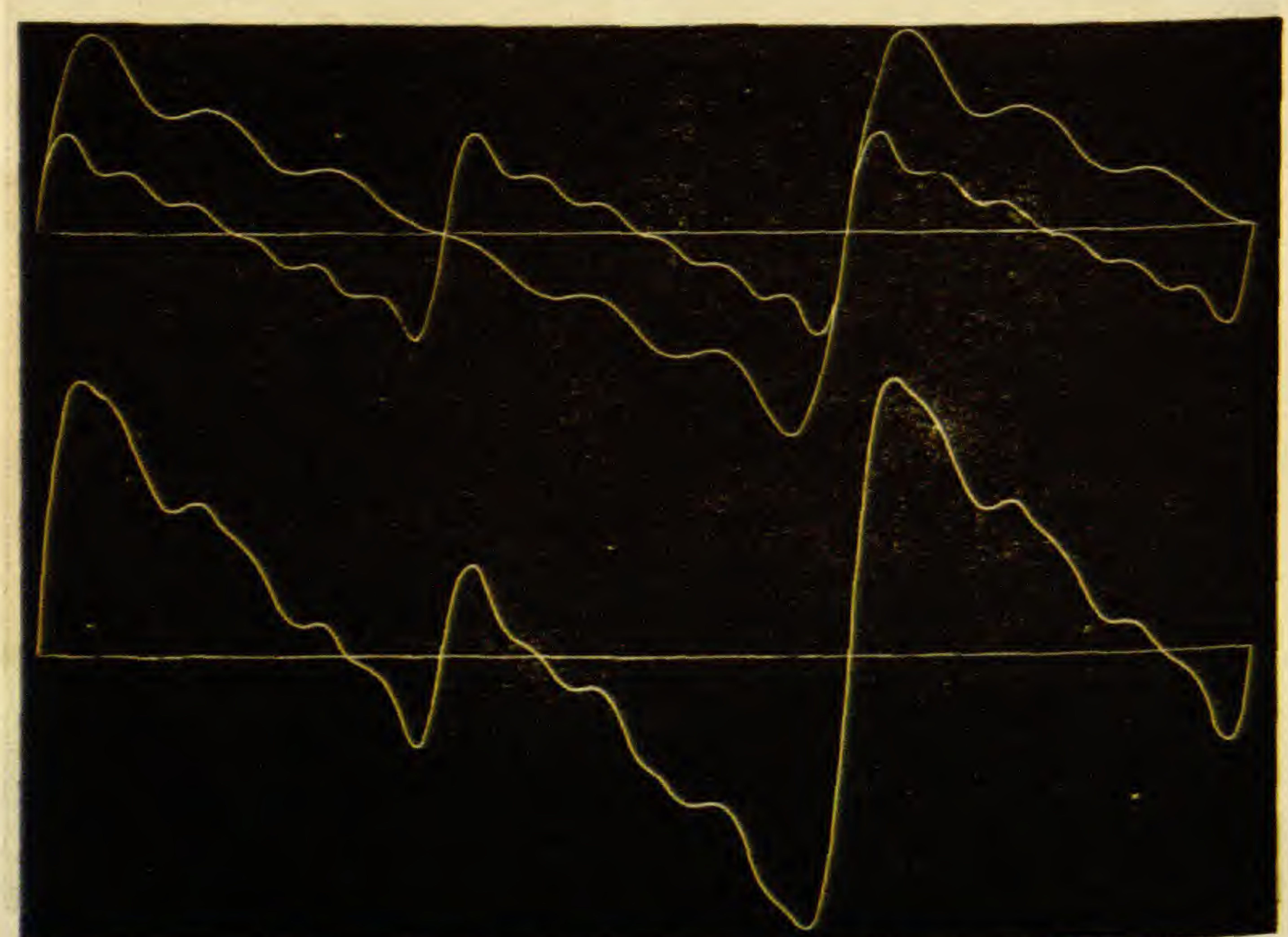
3.



Curve of a Musical Note; being the resultant of the simple vibrations of its first six harmonics.

Fig. 3 is the curve of a musical note, being the resultant of the simple vibrations of its first six harmonics. The first six harmonics having been drawn on a common axis, I erected 500 equidistant ordinates, and extended these ordinates some distance below the axis on which I desired to construct the resultant. The algebraic sum of the ordinates, passing through the harmonic curves, were transferred to the corresponding ordinates of the lower axis, and by drawing a continuous line through these points, I formed the resultant curve. The first six harmonics are alone used in the combinations which I have given, because the 7th, 9th, 11th harmonics, and the major number of those above the 12th, form dissonant combinations with the lower and more powerful harmonics. Indeed, the harmonics above the 6th are purposely eliminated from the notes of the piano by striking the string in the neighborhood of its 7th nodal point. The amplitudes of the harmonics of fig. 3 are made to vary as the wave-length; not that this variation represents the general relative intensities in such a composite sound, but they were so made to bring out strongly the characteristic flexures of the resultant. To simplify the consideration of the curves, they are all represented with the same phase of initial vibration. Of course the resultants have an infinite variety of form, depending on the difference in the initial phase, and on the amplitudes of the harmonic elements.

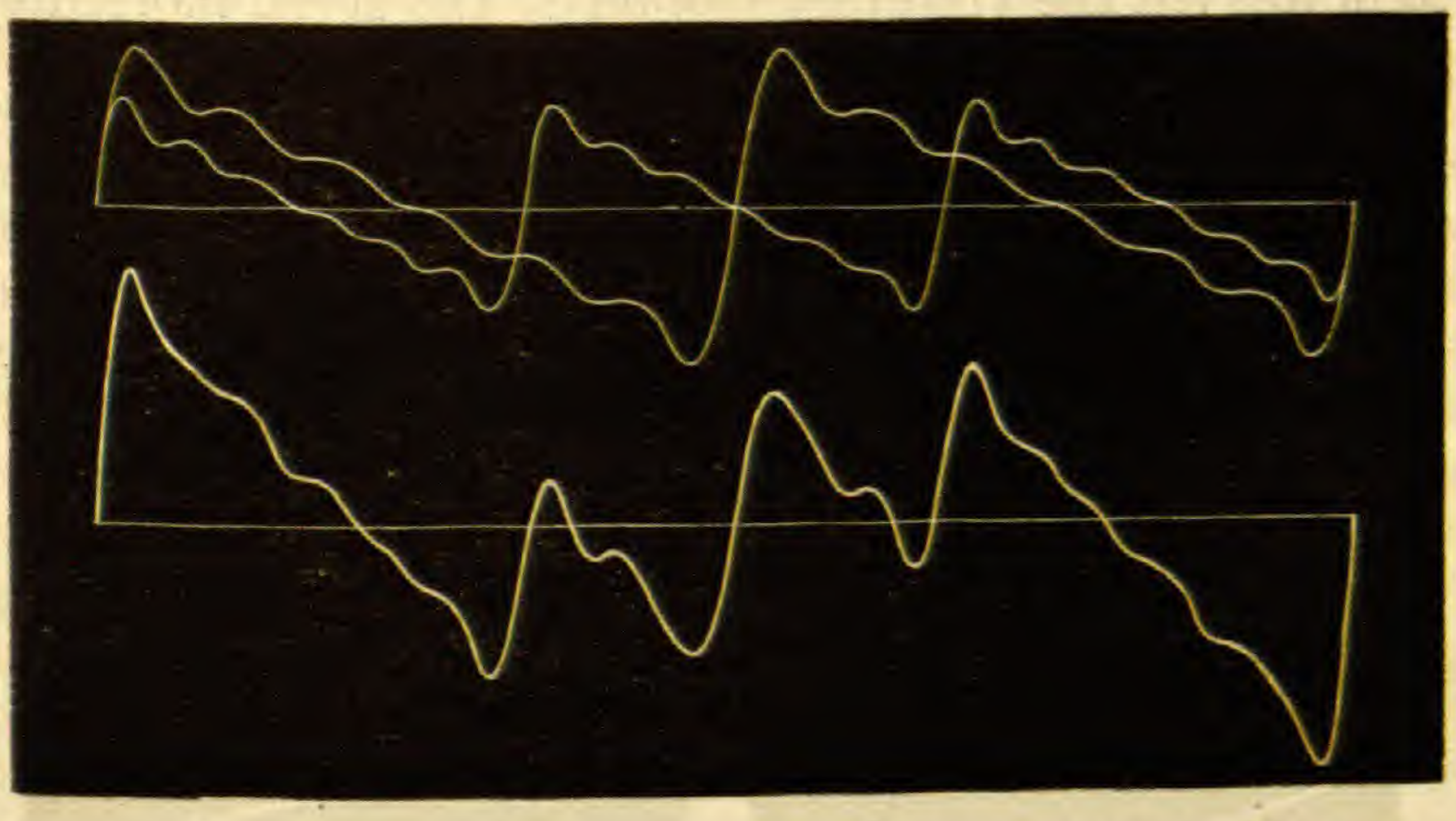
4.



Resultant curve of a musical note combined with its octave. $\lambda : \lambda' :: 1 : \frac{1}{2}$.

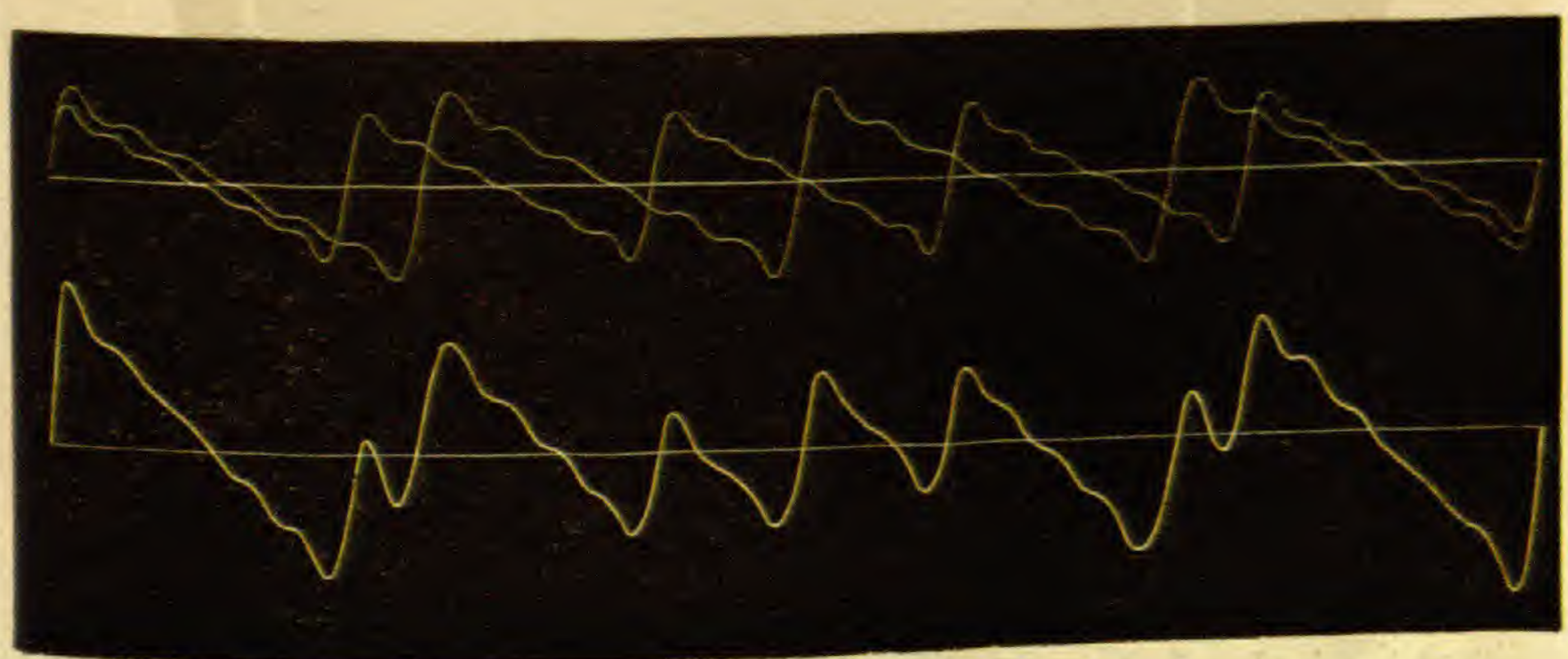
In figs. 4, 5 and 6, I have drawn the resultant curves formed by combining the curves of musical notes corresponding to the various consonant intervals indicated below the curves. As these curves are the resultants formed by the combination of the composite vibrations of musical notes, it follows that the components of these curves are not simple harmonics, as in the case of fig. 3, but are derived from the *resultant* of fig. 3, by reducing to one-fourth the amplitude of that curve and by taking wave-lengths corresponding to the intervals indicated below the figures.

5.



Resultant curve of a musical note combined with its fifth. $\lambda : \lambda' :: 1 : \frac{2}{3}$.

6.



Resultant curve of a musical note combined with its major third. $\lambda : \lambda' :: 1 : \frac{4}{5}$.

All of the curves which I have given in this paper were first drawn on a large scale and then reduced by photography to a size suitable to be transferred to the engraver's block.

(8.) *Experiments in which are produced from the above curves (sec. 6) the Motions of a Molecule of Air when it is animated with the resultant action of the six elementary vibrations forming a musical note; or is set in motion by the combined action of sonorous vibrations forming various musical intervals.*

We may imagine the curve corresponding to a musical note, represented in fig. 3, as formed by the trace of a vibrating molecule of air, or of a point of the tympanic membrane, on a surface which moves near these points. Therefore if we slide this curve along its axis, under a slit in a screen which allows only one point of the curve to appear at once, we shall reproduce in this slit the vibratory motion of the aerial molecule and

7.

of the point on the tympanic membrane. I have exhibited this motion in a continuous, or rather, recurring manner, as follows: On a piece of Bristol board I drew a circle, and in one quadrant of this circle I drew 500 equidistant radii. On these radii, as ordinates, I transferred the corresponding values of the same ordinates of the resultant of fig. 3, diminished to one-fourth of their lengths. I thus deflected the axis of curve fig. 3 into one-fourth of a circle curve; and this repeated four times on the Bristol board, rendered the curve continuous and four times recurring, as shown in fig. 7. I now cut this curved figure out of the board and used it as a template. I placed the latter centered on a glass disc of 20 inches in diameter. The disc was covered on one side with opaque, black varnish, and with the template and the separated points of a pair of spring-dividers, I removed from the glass disc a sinuous band, as shown in fig. 7. The glass disc was now mounted on a horizontal axis and placed in front of a lantern the diameter of whose condensing lens was somewhat greater than the amplitude of the curve. The image of that portion of the curve which was in front of the condenser was now projected on a screen, and then a piece of card board having a narrow slit cut in it was placed close to the disc, with the slit in the direction of one of its radii. On now revolving the disc I reproduced on the screen the vibratory motion of a molecule of air, or of a point on the tympanic membrane, when these are acted on by the joint impulses of the first six harmonic or pendulum vibrations, forming a musical note. On slowly rotating the disc one can readily follow the compound vibratory motion of the spot of light; but on a rapid revolution of the disc, persistence of visual impressions causes the spot to appear lengthened into a band; but this band is not equally illuminated—it has six distinct bright spots in it, beautifully showing the six inflections in the curve.

By sticking a pin in the center of fig. 7, as an axis about which revolves a piece of paper with a fine slit, the reader can gain some idea of the complex motion which I have described.

From the curves of figures 4, 5 and 6 can similarly be reproduced their generating motions. Of course it is understood that in all these cases the amplitude of these vibrations are enormously magnified when compared with the wave-lengths, and that it is really only when the amplitudes of the elementary pendulum vibrations are infinitely small that the resultant curves we have given can be rigorously taken as representing what they purport to; for the law of the "superposition of displacements" depends on the condition that the force with which a molecule returns to its position of equilibrium is directly proportional to the amount of displacement, and this condition only exists in the case of infinitely small displacements; yet the law holds good for the majority of the phenomena of sound.

As a periodic vibration can alone produce in the ear the sensation of sound, and as the duration of the period is always equal to the least common multiple of the periods of the pendulum vibrations of the components, it follows that in the case of a simple sound, or of a sound formed of a harmonic series, that the period equals the time of one vibration of the fundamental; but in the case of any other combinations the duration of the period of the recurring vibration increases with the complexity of the ratio of the times of vibration of the components; thus, the durations of the following combinations are placed after them in fractions of a second.

$$C_3+C_4=\frac{1}{256};\ C_3+G_3=\frac{1}{128};\ C_3+E_3=\frac{1}{64};\ C_3+E_3+G_3=\frac{1}{256};$$
$$C_3+E_3+G_3+C_4=\frac{1}{17}\text{th of a second.}$$

The above mentioned facts suggest a curious physiological inquiry, viz: Does it require a combination of sounds, simple or composite, to remain on the ear the duration of an entire period, in order that it shall give the same sensation as is produced when the same combination is sounded continuously? In other words, will a portion of the recurring composite vibration produce the same sensation as an entire period or several periods? The solution of this problem has been the object of a prolonged experimental research, but up to this time the results have been so difficult of interpretation that I have not arrived at any certain knowledge on the subject. I shall, however, return to this interesting but very difficult work.

ART. XVII.—*Description of a new Apparatus for Gas Analysis;* by C. W. HINMAN.

SOME time since, having occasion to make some analyses of illuminating gas, I read descriptions of several forms of apparatus for that purpose, but failed to find one which appeared quite satisfactory.

An apparatus was desired which should be as far as possible free from fragile or costly parts, and which, without being too complicated, should require no corrections to be made for variations in the pressure, temperature, or aqueous vapor; reliable results rather than minute accuracy being desired.

The apparatus finally adopted operates on the same principles as Williamson & Russell's,* except that in my apparatus the gas is not exploded in the measuring tube, but in a bulb for that special purpose. The trough is nearly the same as that of Doyère,† and pipettes are also used.

The apparatus as made consists of a measuring tube *a*, about 230 mm. long, about 20 mm. in diameter, divided into fortieths of an inch and calibrated with mercury as described by Bunsen. The tube is firmly held by a clamp on the end of the rod *b*, which rod slides up and down in *c*, and is clamped in any position by the screw *d*. A slow motion is given to *c*, and thus to the measuring tube, by means of the milled headed nut *e*, which works along a thread cut on the rod *f*, which is firmly secured

* Chem. Soc. J., [2] ii, 238.

† For figures and description of Doyère's Eudiometer, see Ad. Wurtz Dictionnaire de Chemie, I, p. 280, fig. 42.

to the body of the apparatus. The screw *g* can be turned in so that its end just fits into a longitudinal slot in the rod *f*. By this means *c* is prevented from turning around *f*, and the measuring tube can thus be kept exactly over its well: or *g* can be used to clamp *c* in any position. The pressure tube *h* is about 200 mm. long, and has the same diameter in the upper part as the measuring tube: the lower part, which is about 50 mm. long, has a diameter of only 6 mm. A mass of lead, covered with sealing wax, is attached to the lower part of *h*, so that when it is filled with air, it will keep upright when it is suspended from an eye at the top. The pressure tube is hung by a wire from the end of the arm *i*, which slides with friction along the rod *f*.* The mercurial trough is of cast iron, and consists of the plate *l*, which is about 130 mm. square, 15 mm. thick, and has a groove in its upper surface 10 mm. wide, 6 mm. deep, and 5 mm. from the edge; the well *k*, which is about 240 mm. deep and 30 mm. in inside diameter; and the side well *m*, for the pipette, which is of the same depth as *k*; but part of it extends 40 mm. above the top of *l*; *m* is 10 mm. wide and 90 mm. broad. The wells *m* and *k* have walls 6 mm. thick. Of course *k*, *l* and *m* are all cast in one piece. The groove in *l* has cemented into it, by means of a cement composed of beeswax and rosin, a rectangular trough of plate glass 5 mm. thick, cemented with the same cement into a sheet-iron frame *j*, which is made by bending a sheet of the right size into a box without ends, and then cutting out the sides so as to leave only a strip 12 mm. wide on each edge. The legs *n* are screwed on to the plate *l*.

The pipette consists of a bulb *o*, of a little larger capacity than the measuring tube, open at one end and at the other melted to a tube about 5 mm. in diameter, having a bore of 1 mm., bent so that when it is put into the side well *m*, the point of the pipette can be brought directly under the measuring tube. The other end of the bulb is joined to the end of a piece of good rubber tubing, about 300 mm. long, with rather thick walls and small bore, the other end of which is connected with a small syphon which dips to the bottom of a strong glass bottle *p*, of rather larger capacity than the bulb *o*. Immediately below the bulb is a screw clip *q*. The pipette is held in a stand devised by my assistant, Mr. Lewis. It consists of two discs of wood *rr* (cut away so as to fit the bulb), to which are screwed three rods of iron *ss*, so as to hold the bulb between the discs and at the same time form a tripod to support the pipette. The pipette tube is supported by being attached to a semi-cylindrical piece of metal *t*, which is fastened into the upper disc *r*. The feet of the tripod are screwed into a disc *r'*. This style of

* Compare Dr. Wolcott Gibbs, On a simple method of avoiding observations of temperature and pressure in Gas Analysis, this Journal, II, xlix, 376, 1869.—EDS.

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pipette is to hold reagents for absorption. I have invented another sort of pipette in which to explode gases. The bulb is of the same capacity as the former one, but the sides are 12 to 15 mm. thick, and instead of the pipette tube being melted on to the bulb, it is ground into the top with emery and then cemented in with shellac containing a little Venice turpentine. Two grooves are made in opposite sides of the end of the pipette tube where it is ground into the bulb, in which are platinum wires, the ends of which approach within 1 mm. of each other. Care is taken to have the form of the pipette such that all the gas will be expelled from the bulb when the mercury rises.

The apparatus is used in the following manner: Clean mercury is poured into the well until it is about 8 mm. deep above the top of *l*. A drop of water is put in the end of the pressure tube, which is suspended, so that the bottom nearly touches *l*, and water of the temperature of the room is poured in until the glass vessel is nearly full. Air is then taken out of or added to the pressure tube until the mercury in the lower part of the tube is just on a level with the top of a straight-edged piece of glass *u*, which is fixed just behind the pressure tube and measuring tube, so that its top edge is parallel to and about 15 mm. above the top of *l*. The measuring tube being thoroughly cleansed, and a drop of water spread over the sides, is fixed in the clamp; a thin slice of cork being placed between each side of the clamp and the tube. The tube is then placed upright in a small, long-handled cup of mercury, like that used by Doyère, and is lowered through the water into the mercury well. The piece *c* is then put over *b*, and fixed in position on the rod *f*. A clean pipette, full of mercury, is then lowered into the trough and the point brought directly under the measuring tube, which is then lowered so that the pipette touches the top of the measuring tube. The bottle *p* is placed on the table, the screw clip *q* loosened and the air is forced over into the bulb *o* of the pipette. When the pipette tube has become filled with mercury, the measuring tube is raised and the pipette withdrawn. The air in the pipette is then displaced by mercury, and if the gas to be analyzed can be taken from a rubber tube, the end of this is slipped over the pipette tube and the gas then drawn in. The pipette is put into the well, placed under the measuring tube, which is lowered a little, and the gas forced into it by raising the bottle *p* to the position *p′* and opening the clip *q*. If desirable, the gas can be bubbled directly into the measuring tube by means of a properly bent tube. When as much gas as is desired is in the measuring tube, the pipette or other tube is withdrawn, the measuring tube raised or lowered until the top of the mercury in it is on a level with the top of the glass *u*, and if the mercury in *h* is not at the same height it is adjusted by altering the height of the mercury in the trough, or by adding hot or cold water to that surrounding the tubes; care being taken to thoroughly mix the water by agitation with a stirrer. The gases in the measuring and pressure tubes are thus kept at the same pressure and temperature and are saturated with moisture. The reading of the measuring tube is effected by means of a telescope fixed at some distance from the apparatus. As the mercury is always at the same height, the telescope is always in the same position. When it is desired to subject the gas to the action of any reagent, a pipette containing a few drops of the reagent, the rest of the pipette being filled with mercury, is introduced into the trough and the gas drawn over as before described, and is then shaken up with the reagent. When the gas is to be transferred back to the measuring tube, mercury is drawn through the tube to remove any traces of the reagent, as it is essential that none of the reagent should be transferred with the gas. The point of the pipette being under the measuring tube, the bottle *p* is raised, the clip *q* opened, and the gas is forced into the measuring tube. When the gas is nearly over, the clip *q* is nearly closed, so that the gas passes quite slowly; the pipette is raised by a block of wood under its base, so that its point is 40 or 50 mm. above the surface of the mercury in the measuring tube. The progress of the liquid in the pipette tube must be watched carefully, and when it is about 20 mm. from the end, the clip *q* is closed, and by carefully pinching the rubber tube above the clip the liquid is forced within 1 or 2 mm. of the point of the pipette tube; the point of the pipette is then brought under the surface of the mercury, by raising the measuring tube, and the pipette withdrawn. The quantity of gas left in the pipette tube would be quite unmeasurable if it was five times larger.

It is convenient to have a pipette for every reagent. The pipette for explosions is manipulated in the same manner; the gases being exploded by an electrophorus. This explosion pipette, in many cases, enables the short measuring tube to take the place of a much longer one, and still be as accurate; since explosions can be made with just the requisite quantities of oxygen and the combustible gas, without any dilution. I have exploded illuminating gas and oxygen, and hydrogen and oxygen without any dilution, and I presume the bulb would resist much greater pressure, without fracture. I have found it easier to read the measuring tube, by having a piece of white paper pasted on the glass back of it and about 3 mm. above the top of the glass *u*. The pipettes used permit the complete transfer of the gases each way without the possibility of leakage, and they keep the reagents completely out of the measuring tube, where besides dirtying the surface of the mercury and thus rendering the reading uncertain, they might subsequently act injuriously on the gas. Thus if any caustic potash was left in the measuring tube and any carbon compound exploded with oxygen, there would be found too little carbon dioxide, as part of it would be absorbed immediately after it was formed and before it could be measured. The only part of the apparatus that can be called delicate is the pipette tube, as it is rather long and comparatively unsupported. But with ordinary caution there is little danger of its breaking. If the apparatus had been constructed one-half longer than it is, its accuracy would have been increased and the system would have been nearly as manageable. This kind of apparatus is especially suited to short measuring tubes, for with long ones the pipette becomes fragile and unmanageable, besides being liable to have air drawn in through the rubber tube. The metal work is all of iron. Most of the work was done by myself and assistant. The only work that is difficult is in joining the tube to the bulb of the explosion pipette. The apparatus needs about twenty kilogrammes of mercury to work to advantage with four absorption pipettes.

The following analyses were made with this apparatus. They show the observations required and the manner of making the calculations.

Illuminating gas manufactured by the Boston Gas Light Co., and of an illuminating power of about eighteen candles. The two analyses here given were made with the same sample.

1st Analysis.		2nd Analysis.		
Divisions.	Volumes in C.C.	Divisions.	Volumes in C.C.	
147·3	22·90	149·7	23·26	Gas taken.
146·7	22·82	149·2	23·19	Carb. dioxide, absorbed by KOH.
146·5	22·79	149·0	23·16	Oxygen, absorbed by pyrogallate.
138·2	21·58	140·3	21·88	Hydrocarbons, " SO_3 in H_2SO_4.
326·4	48·91	328·0	49·14	Oxygen added.
84·9	13·72	82·8	13·41	Exploded.
8·8	2·49	5·9	2·06	Carbon dioxide absorbed.
64·0	10·65	45·3	7·88	Hydrogen added.
23·0	4·59	14·2	3·28	Exploded.

1st.

·47	N_2	(A) CH_4+CO+H_2	$=21·11$
6·06	H_2O formed	(B) CH_4+CO	$=11.23$
2·02	O_2 left	(C) $2CH_4+\frac{1}{2}CO+\frac{1}{2}H_2$	$=25·31$
27·33	" added	$2C-A=3CH_4$	$=29·51$
25·31	" used	$A-B=H_2$	$=9·88$
11·23	CO_2 formed	$B-CH_4=CO$	$=1·35$
21·11	Combustible gas		

Volumes.	Per cent.	
22·90	100·00	Gas taken.
·08	·35	Carbon dioxide.
·03	·13	Oxygen.
1·21	5·29	Hydrocarbons.
9·84	42·97	Methane (CH_4).
1·39	6·06	Carb. monoxide.
9·88	43·15	Hydrogen.
·47	2·05	Nitrogen.

2nd.

·53	N_2	(A) CH_4+CO+H_2	$=21·35$
4·60	H_2O formed	(B) CH_4+CO	$=11·35$
1·53	O_2 left	(C) $2CH_4+\frac{1}{2}CO+\frac{1}{2}H_2$	$=25·73$
27·26	" added	$2C-A=3CH_3$	$=30·11$
25·73	" used	$A-B=H_2$	$=10·00$
11·35	CO_2 formed	$B-CH_4=CO$	$=1·31$
21·35	Combustible gas		

Volumes.	Per cent.	
23·26	100.00	Gas taken.
·07	·30	Carbon dioxide.
·03	·13	Oxygen.
1·28	5.50	Hydrocarbons.
10·04	43·16	Methane (CH_4).
1·31	5·63	Carb. monoxide.
10·00	42·99	Hydrogen.
·53	2·28	Nitrogen.

The hydrocarbons gave 17·70 per cent carbon dioxide by a subsequent determination. The calculations show that an error in the nitrogen determination affects the carbon monoxide, methane and hydrogen by the amount of the error. If in the place of the nitrogen found in the first analysis, we substitute that found in the second, we have CH_4 43·20 pr. ct.; for CO 5·85 per ct.; and for H_2 42·93 per ct.; and these numbers agree much better with the numbers of the second analysis. I have not been able to find any duplicate analyses of coal gas and not many of any kind, so I am not able to compare the accuracy of these analyses with that of analyses made with a different apparatus. Each of these analyses required from three to four hours for its execution.

Analyses of common Air.

Air collected April 25, 1874, freed from CO_2.

210·2	32·07	Air,	H_2O formed	20·15
317·2	47·57	H_2 added,	O_2	6·716
178·2	27·42	Exploded,	per cent of O_2	20·94

Same Sample.

197·7	30·25	Air,	H_2O formed	19·00
304·7	45·75	H_2 added,	O_2	6·333
173·6	26·75	Exploded,	per cent of O_2	20·93

Air collected April 26.

214·3	32·66	Air,	H_2O formed	20·52
311·9	46·81	H_2 added,	O_2	6·84
170·5	26·29	Exploded,	per cent of O_2	20·94

Same Sample.

207·7	31·70	Air,	H_2O formed	19·90
306·1	45·95	H_2 added,	O_2	6·633
168·8	26·05	Exploded,	per cent of O_2	20·92

Each of these air analyses required about half an hour for its execution. These analyses seem to have about the same degree of accuracy as the average of those made by Bunsen.

Gas generated by action of KOH on Fe and Zn.

	Div.	C.C.		
Air	193·5	29·64	18·47 H_2O formed	
Gas added	278·5	41·98	12·313 H_2 found	99·78 % H_2
Exploded	151·4	23.51	12·34 gas added	

Same Gas.

Air	166·3	25·68	14·07 H_2O formed	
Gas added	231·0	35·08	9·38 H_2 found	99·79 % H_2
Exploded	134·3	21·01	9·40 gas added	

These analyses show that the air was not quite expelled from the generating flask before the gas was collected.

This apparatus is especially useful where it is desired to obtain reliable results in a comparatively short time. As before mentioned, it was designed more especially for the analysis of illuminating gas. It will be noticed that the explosion pipette of this apparatus is nearly the same as the Cavendish eudiometer; the chief difference being in the manner of introducing and expelling the gas.

Office of Gas Inspection, Boston, May 14, 1874.

ART. XVIII.—*Researches on the Hexatomic compounds of Cobalt;* by WOLCOTT GIBBS, M.D.

[Continued from vol. vi, p. 116, August, 1873.]

5. THE salts described by Fremy* under the names of chloride, nitrate and sulphate of fuscocobalt contain also eight atoms of ammonia, and may be regarded as belonging to the octamin series. These salts have, according to Fremy, respectively the formulas:

$$Co_2(NH_3)_8.O.Cl_4+3OH_2$$
$$Co_2(NH_3)_8.O.(NO_3)_4+3OH_2$$
$$Co_2(NH_3)_8.O.(SO_4)_2+4OH_2$$

in modern notation. They are brown resinous masses, are difficult to obtain in a state of purity, and have as yet been but little studied. If we admit that the formulas are accurate, we may write them in accordance with the theoretic views which I have adopted, as follows:†

$$Co_2\left\{\begin{array}{l} NH_3-Cl \\ NH_3-Cl \\ \left.\begin{array}{l} NH_3-NH_3 \\ NH_3-NH_3 \end{array}\right>O \\ NH_3-Cl \\ NH_3-Cl \end{array}\right. \quad Co_2\left\{\begin{array}{l} NH_3-NO_3 \\ NH_3-NO_3 \\ \left.\begin{array}{l} NH_3-NH_3 \\ NH_3-NH_3 \end{array}\right>O \\ NH_3-NO_3 \\ NH_3-NO_3 \end{array}\right. \quad Co\left\{\begin{array}{l} \left.\begin{array}{l} NH_3 \\ NH_3 \end{array}\right>SO_4 \\ \left.\begin{array}{l} NH_3-NH_3 \\ NH_3-NH_3 \end{array}\right>O \\ \left.\begin{array}{l} NH_3 \\ NH_3 \end{array}\right>SO_4 \end{array}\right.$$

Jörgensen‡ suggests that these salts may contain hydroxyl in place of oxygen. There is at present no method of deciding the question, and I have adopted the view which seems to me the most probable. Künzel's hyposulphate above mentioned may be regarded as belonging to this series, and as having the structural formula:

$$Co_2\left\{\begin{array}{l} \left.\begin{array}{l} NH_3 \\ NH_3 \end{array}\right>S_2O_6 \\ \left.\begin{array}{l} NH_3-NH_3 \\ NH_3-NH_3 \end{array}\right>O \\ \left.\begin{array}{l} NH_3 \\ NH_3 \end{array}\right>S_2O_6 \end{array}\right.$$

* Ann. de Chimie et de Physique, [3,] tome xxxv, p. 257.

† Blomstrand has given the same formulas with trifling variations. Chemie der Jetztzeit, p. 355.

‡ Gmelin-Krauts' Handbuch, vol. iii, p. 468.

but, according to Geuther, the formula given by Künzel must be tripled, and the salt then belongs to the dodekamin or luteocobalt series. In the absence of direct proof of the existence of luteocobalt in this salt, Künzel's formula appears the more probable of the two. The compounds above mentioned, with those which I have myself described, form the only known members of the octamin group, a further study of which will doubtless yield an ample return.

6. *Action of ammonic nitrite on salts of cobalt.*—To obtain a clear view of the nature and mode of formation of the salts of xanthocobalt, I have carefully studied the relations of ammonic nitrite to salts of cobalt under different conditions. This subject has already been examined by Erdmann, and in my laboratory by Sadtler. Erdmann found that when a neutral solution of cobaltic chloride is mixed with a neutral solution of ammonic nitrite no turbidity ensues, but after spontaneous evaporation in the air a salt crystallizes, with the formula, as Erdmann writes it (old style):

$$Co_2\widehat{O_3 . 2}NH_3,\ 3NO_3 + NH_4O,\ NO_3.$$

This salt is isomorphous with the corresponding potassium salt, the crystals belonging to the rhombic system. Erdmann does not explain the reaction which takes place in the formation of this or the corresponding potassium salt, and regards the compounds in question as double salts. When slightly acid solutions were employed, Erdmann obtained, in addition to the above mentioned salt, an ammonic salt corresponding to Fischer's salt, $Co_2(NO_2)_{12}(NH_4)_6+3OH_2$, as we should now write it. The existence of this salt was first remarked by Genth and myself.* Sadtler studied the action of ammonic nitrite on acid solutions of cobaltic chloride, and obtained two salts having respectively the formulas:

$$Co_2(NO_2)_{10}(NH_4)_4+2OH_2$$
$$Co_2(NO_2)_{12}(NH_4)_6+2OH_2,$$

but did not observe the formation of Erdmann's ammonium salt. In repeating these experiments I always obtained Erdmann's ammonium salt, $Co_2(NH_3)_4(NO_2)_8(NH_4)_2$, in largest quantity. The crystals are uncommonly beautiful and well defined. Of these crystals

0·3390 gr. gave 0·1783 gr. SO_4Co=20·02 per cent.

The formula requires 20·00 per cent. In one experiment, in which a little free acetic acid was present, I obtained large dark sherry-wine colored prismatic crystals, which after solution and recrystallization gave only very thin lozenge-shaped tabular

* This Journal, 2d Series, vol. xxiv, p. 86.

crystals, the form and appearance of which are highly characteristic. These crystals gave no reactions with salts of luteocobalt, purpureocobalt and roseocobalt, and none with potassic chromate and dichromate, ammonic oxalate or argentic nitrate. The absence of the first mentioned reactions shows that they do not contain $Co_2(NH_3)_4(NO_2)_8$ or $Co_2(NO_2)_{12}$, while the fact that they give no reactions with alkaline chromates and oxalates shows that they do not contain any known cobaltamin. Of these crystals

0·1554 gr. gave 0·0974 gr. SO_4Co=23·86 per cent cobalt.
0·3081 gr. gave 0·0635 gr. NH_3 =20·61 per cent ammonia.

The formula $Co_2(NH_3)_6(NO_2)_6$ requires

Cobalt,	23·79	23·86
Ammonia,	20·56	20·61

These analyses are sufficient to identify the salt in question with one which Erdmann has described in the paper referred to, as formed by the action of ammonia and potassic nitrite upon cobaltic chloride, unfortunately with but very scanty details. I attribute to this salt the formula

$$Co_2 \begin{cases} NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \end{cases}$$

and consider it to be the nitrous representative of the hexamin $Co_2(NH_3)_6$. I have not succeeded in obtaining from it other members of the same series; but it is, to say the least, probable that the dichrocobalt-chloride of Fr. Rose,* $Co_2(NH_3)_6Cl_6)+2OH_2$, represents the corresponding chloride. Künzel † has described a sulphite to which he attributes the formula

$$Co_2(NH_3)_6(SO_3)_3+OH_2,$$

but according to Geuther‡ this formula must be doubled, the salt belonging to the dodekamin or luteocobalt series, with the formula

$$Co_2(NH_3)_{12}(SO_3)_6+Co_2(SO_3)_6+2OH_2.$$

Erdmann's hexamin salt is of special interest because, as I shall show, it forms the first term in a remarkable series of metameric bodies; its formation under the circumstances may with great probability be expressed by the equation:

$$2CoCl_2+10NH_4.NO_2+3O=Co_2(NH_3)_6(NO_2)_6+4NH_4Cl+3OH_2,$$

* Untersuchungen über ammoniakalische Kobalt-Verbindungen. Heidelberg, 1871.
† Journal für prakt. Chemie, 72, p. 209. ‡ Ann. de Pharmacie, 128, p. 127.

as the salt is not formed immediately, but only after absorption of oxygen from the air. The formation of Erdmann's ammonium salt may in like manner be represented by the equation:

$$2CoCl_2+8NH_4.NO_2+2O=Co_2(NH_3)_4(NO_2)_8+4NH_4Cl+2OH_2,$$

the presence of oxygen being necessary in this case also.

In another experiment I obtained no hexamin nitrite, but only Erdmann's ammonium salt and the two salts described by Sadtler, and to which he gave respectively the formulas:

$$Co_2(NO_2)_{10}(NH_4)_4+2OH_2$$
$$Co_2(NO_2)_{12}(NH_4)_6+2OH_2.$$

These last salts were found in considerable quantity mixed together as a yellow sparingly soluble crystalline powder, when a strong solution of ammonic nitrite was poured upon finely pulverized cobaltic chloride, and acetic acid was added in small excess. I consider the formation of these two salts to be represented by the equations:

$$2CoCl_2+10NH_4.NO_2+3O=Co_2(NO_2)_{10}(NH_4)_4+6NH_3+3OH_2$$
$$2CoCl_2+12NH_4.NO_2+3O=Co_2(NO_2)_{12}(NH_4)_4+8NH_3+3OH_2.$$

Professor Sadtler has shown that in these cases also an absorption of oxygen from the air takes place. When a solution of ammonic nitrite is added to a strong alcoholic solution of cobaltic chloride, Erdmann's ammonium salt, $Co_2(NH_3)_4(NO_2)_8(NH_4)_2$, is chiefly formed, and only a small quantity of the four and six-atom salts. The compound formed crystallizes from the alcoholic solution in very beautiful and well defined prismatic forms.

From the above it will be seen that at least four distinct compounds are formed by the action of ammonic nitrite upon solutions of cobaltic chloride in presence of a weak acid and of the oxygen of the air. It is at least probable that all four are formed at the same time, though in varying proportions. I have already shown that, in the presence of free ammonia and of ammonic nitrate, cobaltic chloride and ammonic nitrite yield the nitrate of the octamin series. Of the action of ammonic nitrite upon cobaltic salts in the presence of free ammonia, I shall speak in treating of the formation of the salts of xanthocobalt.

7. I have stated above that Erdmann obtained the hexamin nitrite, $Co_2(NH_3)_6(NO_2)_6$, by the joint action of potassic nitrite and ammonia upon cobaltic chloride. On repeating his experiments, I found that small quantities of this salt were formed, but that the chief products of the action were salts of xanthocobalt, the formation of which Erdmann does not appear to have noticed. Small quantities of salts of the octamin series are also formed. The filtered solution obtained in this reaction

was precipitated by potassic dichromate, and the orange-red needles obtained recrystallized for analysis; of these crystals

0·6145 gr. gave 0·7393 gr. CrO_4Ba=51·40 per cent Cr_2O_7.
0·7712 gr. gave 0·9277 gr. CrO_4Ba=51·40 per cent Cr_2O_7.
0·5615 gr. gave 96·5 c.c. nitrogen (moist) at 15° C. and 763^{mm}·1= 20·12 per cent nitrogen.
0·5028 gr. gave 86 c.c. nitrogen (moist) at 15° C. and 763^{mm}·1= 20·05 per cent nitrogen.

The formula $Co_2(NH_3)_{10}(NO_2)_2Cr_2O_7$ requires 53·22 per cent Cr_2O_7 and 20·67 per cent nitrogen, while the formula of the octamin salt, $Co_2(NH_3)_8(NO_2)_4Cr_2O_7$, requires 32·91 per cent Cr_2O_7 and 19·30 per cent nitrogen, so that the analyses leave no reasonable doubt that the salt was a mixture of a salt of xanthocobalt with a smaller proportion of the corresponding salt of the octamin series.

The above results clearly show that the action of ammonic nitrite upon salts of cobalt in presence of free acid is extremely complex, not less than six classes of salts being formed, of which two belong certainly to basic series, while three may be regarded as salts of ammonium. The sixth, $Co_2(NH_3)_6(NO_2)_6$, is probably also one term in a hexamin series.

8. The ammonia-nitrites discovered by Erdmann are of especial interest. They present the first and at present the only known instance in which cobalt, by uniting with ammonia and nitroxyl, NO_2, forms an electro-negative or chlorous radical. The compound $Co_2(NH_3)_4(NO_2)_8$ may be regarded as existing in combination with two atoms of a mon-atomic radical, exactly as the compound $Co_2(NH_3)_8(NO_2)_4$ combines with two atoms of chlorine. The structural formulas may be written respectively:

$$Co_2\left\{\begin{array}{l} NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NO_2 \\ NH_3-NO_2 \end{array}\right. \qquad Co_2\left\{\begin{array}{l} NH_3-NO_2 \\ NH_3-NO_2 \\ N{<}^{O}_{O}{>}N-O-OK \\ N{<}^{O}_{O}{>}N-O-OK \\ NH_3-NO_2 \\ NH_3-NO_2 \end{array}\right.$$

With these formulas we may advantageously compare those of chloride of luteocobalt, of Fischer's salt considered as anhydrous, and of chloride of xanthocobalt:

$$Co_2\left\{\begin{array}{l} NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \end{array}\right. \qquad Co_2\left\{\begin{array}{l} N{<}^{O}_{O}{>}N-O-OK \\ N{<}^{O}_{O}{>}N-O-OK \\ N{<}^{O}_{O}{>}N-O-OK \\ N{<}^{O}_{O}{>}N-O-OK \\ N{<}^{O}_{O}{>}N-O-OK \\ N{<}^{O}_{O}{>}N-O-OK \end{array}\right.$$

$$Co_2\left\{\begin{array}{l} NH_3-NO_2 \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NO_2 \end{array}\right.$$

The manner in which these compounds may be derived from each other by replacement is sufficiently obvious, and is best seen by assuming chloride of luteocobalt and Fischer's salt as the two extreme terms of the series in which the other three are intermediate.

Erdmann's analyses leave no reasonable doubt as to the constitution of the ammonia-nitrites. I have thought it worth while, however, to make a few additional analyses in support of his view. In the potassium salt:

0·4497 gr. gave 0·3397 gr. SO_4Co and SO_4K_2=75·54 per cent.
0·7338 gr. gave 0·5615 gr. " " =76·52 per cent.
0·5937 gr. gave 127 c.c. nitrogen at 6°·5 C. and 773mm·4=26·45 per cent nitrogen.

The formula $Co_2(NH_3)_4(NO_2)_8K_2$ requires 76·58 per cent $2SO_4Co+SO_4K_2$ and 26·58 per cent nitrogen. In the silver salt:

0·3580 gr. gave 0·2902 gr. SO_4Co and SO_4Ag_2.
0·5937 gr. gave 0·1675 gr. silver=28·21 per cent.

The cobalt by difference amounts to 15 33 per cent. The formula $Co_2(NH_3)_4(NO_2)_8Ag_2$ requires 28·05 per cent silver and 15·32 per cent cobalt.

Thallium salt.—When a solution of the potassium salt is added to one of thallous nitrate, a beautiful sherry-wine-colored crystalline precipitate is thrown down, which on recrystallization gives very well defined prismatic crystals, having apparently the same form as the corresponding potassium and ammonium salts.

Mercurous salt.—A solution of potassic ammonia-cobalt-nitrite gives immediately in solutions of mercurous nitrate a beautiful orange-colored crystalline precipitate, which may be dissolved in boiling water, but not without partial decomposition. The salt does not crystallize well from the solution. Of this salt:

0·7785 gr. gave 0·1775 gr. SO_4Co=8·68 per cent cobalt.

The formula $Co_2(NH_3)_4(No)_2(Hg_2)_2$ requires 8·71 per cent.

A solution of the potassic salt gives no precipitate with salts of cobalt, nickel, barium and copper, and none at first with plumbic acetate. After standing, however, a lead salt separates in fine acicular leafy crystals of a brown-orange color, soluble in hot water, but with partial decomposition. The same is true of the silver salt, but small quantities of this may usually be dissolved and recrystallized without change. The silver salt is extremely well characterized; its moderate degree of solubility and the facility with which it crystallizes in tabular lustrous crystals have made it of great service in my investigations, especially in distinguishing salts containing $Co_2(NH_3)_4(NO_2)_8$ from those which contain $Co_2(NO_2)_{12}$. Compounds of ammonia-cobalt-nitrite with barium, strontium, etc., are easily formed by double decomposition, the metallic chlorides being digested with a solution of the argentic salt. They are pale orange-yellow, soluble salts, which I have not further examined. A solution of the potassic salt gives beautiful crystalline precipitates with salts of various organic alkaloids, especially with those of brucin and strychnin. These are soluble in hot water without sensible decomposition, and may be recrystallized. Salts of anilin give a bright yellow precipitate with potassic ammonia-cobalt-nitrite, which is, however, immediately decomposed, phenol being set free. The potassic salt gives also splendid crystalline precipitates with salts of croceocobalt, xanthocobalt, luteocobalt, etc. I have already noticed the salt of croceocobalt, and will describe the salts of the other bases in due course.

Erdmann has not attempted to explain the formation of this class of salts. He remarks that a yellow insoluble compound is formed at the same time with the potassic salt $Co_2(NH_3)_4(NO_2)_8K_2$, which appears to be a mixture which cannot be obtained pure for analysis. I have also obtained this body, and also regard it as consisting mainly of Fischer's salt, $Co_2(NO_2)_{12}K_6$, though as Erdmann states, it contains a small percentage of ammonia. The formation of the salt $Co_2(NH_3)_4(NO_2)_8K_2$ may be expressed by the equation

$$2CoCl_2+4NH_4Cl+8KNO_2+O=Co_2(NH_3)_4(NO_2)_8K_2+6KCl+2HCl+OH_2,$$

if we suppose oxygen to be absorbed from the air. In consequence, however, of the formation of free chlorhydric acid, N_2O_3 is set free, and it is much more probable that this is reduced by the nascent-hydrogen; so that we have

$$N_2O_3+2H=2NO+OH_2.$$

The potassium salt is also formed, as I shall show, in various other cases; the similarity of some of its reactions to those of a solution of $Co_2(NO_2)_{12}Na_6$ in sodic nitrite for a long time misled me; but its relations to salts of silver, mercury and thallium enable us to recognize its presence with absolute certainty. The salt does not enter into combination with iodine.

XANTHOCOBALT.

9. Genth and I have shown in our memoir that the salts of xanthocobalt may be formed either directly by the action of nitrous acid vapors upon salts of cobalt, or by the action of the same acid upon salts of purpureocobalt and roseocobalt, in each case in the presence of free ammonia. I propose now to give the results of a more detailed study of the subject.

With respect to the constitution of this class of salts, I may remark, in the first place, that Genth and I left it undecided whether the salts in question contain NO or NO_2, pointing out the fact that the analyses do not decide in favor of either view, and adopting the former provisionally. Braun first proved conclusively that the salts of xanthocobalt contain NO_2, and this view has since been generally adopted. I have already shown (§ 1) that when cobaltic chloride, $CoCl_2$, is mixed with ammonia and ammonic nitrite and nitrate, the solution absorbs oxygen from the air, while the nitrate of the octamin series, $Co_2(NH_3)_8(NO_2)_4(NO_3)_2$, is formed. I have not observed in this reaction the formation of a salt of xanthocobalt. If present at all, such salts must be in very small relative quantity. Genth and I have shown, on the other hand, that when the red gases resulting from the action of nitric acid upon starch, sawdust or arsenous oxide are passed into solutions of cobaltic salts in presence of an excess of ammonia, salts of xanthocobalt are formed in a very short time, and in large quantity.

If we consider the red gas to consist of hyponitric oxide, N_2O_4, we may have

$$2Co(NO_3)_2+10NH_3+N_2O_4=Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4.$$

In preparing sulphate and nitrate of xanthocobalt by this process, I have on several occasions been able to detect only salts of this base among the products of the reaction. In one case, however, in which I employed cobaltic sulphate and added so large a quantity of ammonic sulphate that the solution gave no precipitate with ammonia, I obtained a very large relative quantity of Erdmann's salt $Co_2(NH_3)_6(NO_2)_6$. In other cases in which cobaltic chloride was present I detected crystals of the chloronitrate $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2$. The solutions after the action of the red gases also contain small quantities of the ammonia-cobalt-nitrite of ammonium, $Co_2(NH_3)_4(NO_2)_8(NH_4)_2$, as well as ammonic nitrite and nitrate.

On the other hand, however, I have already shown (§ 3) that salts of this radical are formed in large quantity, together with a smaller proportion of the octamin nitrate, by the action of a mixture of potassic nitrite and ammonia upon cobaltic nitrate in presence of air; but that xanthocobalt is exclusively formed by the action of the same mixture upon a solution of ammonic and cobaltic sulphates. I am unable to offer any plausible explanation for the difference of the products in the two cases.

When cobaltic nitrate, ammonic nitrite and ammonia are mixed and placed in a tightly-corked bottle, no action whatever appears to take place, even after the mixture has stood some days. But if plumbic hyperoxide, PbO_2, is added, the mixture soon becomes yellow, and after a few hours large crystals of nitrate of xanthocobalt are formed with distinct reduction of the plumbic hyperoxide. The reaction in this case may be represented by the equation:

$$2Co(NO_3)_2+10NH_3+2NO_2.NH_4+PbO_2=Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4+PbO+(NH_4)_2O.$$

Potassic hypermanganate may also be employed as an oxidizing agent, but is less convenient. The experiment just detailed appears to me to render it most probable that in the action of the red gases upon salts of cobalt in presence of ammonia, the resulting salts of xanthocobalt are not formed by the direct union of the cobaltic salt with ammonia and nitroxyl, but that ammonic nitrite is first formed, and that the oxygen necessary for the completion of the reaction is derived from the decomposition of some element of the complex mixture of NO, NO_2, N_2O_3 and NO_3H, which make up the red vapors.

The formation of salts of xanthocobalt by the action of the red gas upon salts of purpureocobalt and roseocobalt in the presence of free ammonia is easily explained. We have here simple cases of double decomposition, a particular instance of which, covering in substance the whole ground, may be expressed by the equation:

$$Co_2(NH_3)_{10}(NO_3)_6+2NH_4.NO_2=Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4+2NO_3.NH_4.$$

Salts of xanthocobalt are always formed when salts of purpureocobalt and roseocobalt are heated or even digested in the cold with alkaline nitrites. I have made a special study of the action of potassic and sodic nitrites upon chloride of purpureocobalt, the details of which are as follows:

10. *Action of sodic and potassic nitrites upon chloride of purpureocobalt.*—A quantity of chloride of purpureocobalt was dissolved in boiling water, with a little free acetic acid to prevent decomposition, and added to a hot solution of potassic nitrite in excess. The dark brown-red solution was evaporated at a

gentle heat to half its volume. On cooling, a small quantity of Fischer's salt $Co_2(NO_2)_{12}K_6+2OH_2$ separated; afterward sherry-wine colored prismatic crystals were formed in abundance. After recrystallization these were analyzed.

0·2824 gr. gave 0·1519 gr. $CoSO_4$=20·47 per cent cobalt.
0·5557 gr. gave 0·2092 gr. silver=12·37 per cent chlorine.

The same experiment was made with sodic nitrite, and with similar results. After two recrystallizations the salt formed was analyzed.

0·4163 gr. gave 0·2235 gr. $CoSO_4$=20·43 per cent cobalt.
0·2332 gr. gave 0·0876 gr. silver=12·38 per cent chlorine.
0·6625 gr. gave 192·12 c.c. nitrogen (moist) at 14° C. and $764^{mm}\cdot1$ =34·29 per cent.
1·2310 gr. gave 0·5825 gr. water=5·24 per cent hydrogen.
1·6542 gr. gave 0·7996 gr. water=5·37 per cent hydrogen.

The salt being found anhydrous, the analyses agree with the formula:

$$Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2,$$

which requires

		Found.	
Cobalt	20·52	20·47	20·43
Chlorine	12·34	12·37	12·38
Hydrogen	5·26	5·24	5·37
Nitrogen	34·09	34·29	

and which is fully sustained by other considerations, as I shall show. As the solutions of the alkaline nitrites employed also contained nitrates, the formation of the new salt may be represented by the equation:

$$Co_2(NH_3)_{10}Cl_6+2KNO_2+2KNO_3=4KCl+Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2.$$

The salt itself is then a nitroso-chloro-nitrate, and belongs probably to the α-dekamin or purpureocobalt series; but it may be more conveniently regarded as the chloro-nitrate of xanthocobalt. It has the wine color of the salts of the so-called xanthocobalt series, but crystallizes usually in prismatic forms, which are moderately soluble in hot water, and separate readily from the solution. With neutral potassic chromate the salt gives the beautiful yellow crystalline chromate of xanthocobalt:

$$Co_2(NH_3)_{10}(NO_2)_2(CrO_4)_2+2OH_2.$$

With potassic ferrocyanide it gives the characteristic red prismatic crystals of

$$Co_2(NH_3)_{10}(NO_2)_2FeCy_6+6OH_2,$$

and with ammonic oxalate, oxalate of xanthocobalt,

$$Co_2(NH_3)_{10}(NO_2)_2(C_2O_4)_2,$$

the reactions being too obvious to require explanation by equations.

As it is difficult to prevent the action of the alkaline nitrites upon chloride of purpureocobalt from going too far and decomposing the new salt first formed, I had recourse to a different mode of preparation, by which the salt can be prepared in any quantity and with the greatest facility. A hot solution containing one molecule of chloride of xanthocobalt was mixed with a solution containing one molecule of nitrate of xanthocobalt. On cooling, the nitroso-nitro-chloride crystallized in beautiful prismatic forms. In this case we have

$$Co_2(NH_3)_{10}(NO_2)_2Cl_4+Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4=2Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2.$$

Of the crystals so formed

0·6203 gr. gave 0·3310 gr. $CoSO_4$=20·31 per cent cobalt.
0·9268 gr. gave 0·3450 gr. silver=12·24 per cent chlorine.

The formula requires 20·51 per cent cobalt and 12·34 per cent chlorine. A portion of the crystallized salt was dissolved and precipitated by argentic nitrate. The filtrate from AgCl gave on evaporation crystals of nitrate of xanthocobalt, in which

0·2972 gr. gave 0·1469 gr. $CoSO_4$=18·81 per cent cobalt.

The formula of the nitrate requires 18·73 per cent. These results leave no doubt as to the constitution and true relations of the chloro-nitrate.

Gold salt.—When the chloro-nitrate is dissolved and a solution of aurochloride of sodium, $AuCl_4Na$, is added in excess, long prismatic wine-yellow crystals are formed. Of these crystals

0·8564 gr. decomposed by zinc and sulphuric acid gave 0·6300 gr. silver=24·16 per cent chlorine and 0·2858 gr. gold=33·36 per cent.

0·4084 gr. gave 0·1770 gr. Au+Co=43·34 per cent and by difference 9·98 per cent cobalt.

This formula, $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2+2AuCl_3$, requires

		Found.
Cobalt	9·98	9·98
Gold	33·33	33.36
Chlorine	24·03	24·16

The salt is readily decomposed by boiling with reduction of metallic gold.

Platinum salt.—Platinic chloride in solution precipitates the chloro-nitrate almost immediately in the form of wine-yellow needles. After recrystallization this salt was analyzed with the following results:

0·6405 gr. fused with potassio-sodic carbonate gave 0·5564 gr. silver=28·55 per cent chlorine, 0·1986 gr. platinum=31·00 per cent, and 0·0597 gr. cobalt=9·33 per cent.

The platinum and cobalt were weighed together as metals after reduction by hydrogen, and the cobalt was then dissolved by long boiling with nitric acid.

This formula, $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2+2PtCl_4$, requires

		Found.
Cobalt	9·40	9·33
Platinum	31·55	31·00
Chlorine	28·28	28·55

The salt had no water on heating to 140° C.

(To be continued.)

ART. XIX.—*On the Mechanism of Stromboli;* by ROBERT MALLET, M.A., F.R.S.

[Abstract of a paper received by the Royal Society of London, May 17, 1874, and read June 25, 1874.]

STROMBOLI stands unique (omitting the as yet imperfectly known Masaya in Central America), amongst the volcanoes of our globe as characterized by the rhythmical recurrence of its outbursts, which have continued with but little alteration for more than 2000 years. The phenomena of Stromboli have been more or less accurately described by several authors, from Spallanzani and Hoffman to Scrope and Daubenay. The last but one of these has proposed an explanation of the phenomena presented by the recurrent outbursts at short intervals of time, which within rather narrow limits are constant, and in the traditional convictions are supposed to have some connection with the state of the weather or of the barometer. This author points out that these explanations fail wholly to account for the long continued rhythmical action of this volcano, and for any presumable connection with the weather.

The writer, in company with Colonel Henry Yule, B.E. (well-known for his embassy in Siam, as editor of Marco Polo's Travels, and for other important labors), examined Stromboli and other islands of the Lipari group in 1864, and describes more systematically and minutely than has been done by previous writers, to his knowledge, the circumstances and phenomena of rhythmical eruption presented by the volcano. He then briefly refers to the history of observation as respects the geysers of Iceland; to some of the circumstances, as to those experimentally ascertained by Henderson, and to the able explanation of their rhythmical action given by Bunsen and Des Cloizeaux. He then points out that Stromboli presents the rhythmic recurrence of outburst of a geyser combined with the eruptive action of a volcano of small energy, and suggests, aided by a diagram, the very simple mechanism by which this combination is produced at Stromboli; and, comparing the action of such mechanism with the actual phenomena presented by the volcano, shows that they tally with each other minutely. The crater of Stromboli consists of a deep cavity, at bottom of which is a tundish-shaped funnel, formed chiefly of the loose material which after each outburst falls back into the crater. More or less liquid lava finds its way by a lateral duct into the bottom of this funnel of loose material, which it partially solders together in the period between successive outbursts. Below this mass of loose material, and penetrating to a depth considerably below the sea level, which is about 400 feet below the bottom of the crater, is the tube which in all ordinary volcanoes carries up together the mixed steam and liquid lava that are erupted through it and the crater. In this instance the tube is supplied with water percolated from the sea, which is heated by the high temperature of the ducts through which it passes by the heated walls of the tube, and by jets of superheated steam driven into its lower extremity by ducts, which, like those that conduct the lava to the bottom of the crater above, are derived from the great volcanic channels much more centrally situated in the island, and belong to the great craters by which the island itself was formed. The tube with its water and steam ducts thus performs the rhythmical functions of alternate outbursts and quiescence that belong to the geyser, and the ascending column of water in the tube, when the boiling point, due to statical depth, superincumbent tension, and obstructions at the bottom of the crater, has been reached for all parts of its height, is driven forth, throwing the loose material mingled with liquid lava out of the crater before it in a cloud of stones, dust, shreds of semi-liquid lava, and steam, and perhaps pulverized water. The main mass of the solid ejecta fall back into the crater, to be again expelled by the next outburst.

Several facts and details are adduced tending to corroborate this as being the true mechanism of this singular volcanic vent, and several inaccuracies of description of preceding writers, more especially the statement that the bottom of the crater is about 2000 feet above the level of the sea, are corrected—the author's levels being derived from approximate barometric measurements. The author then refers to the old traditions still current as to connections in the way of cause and effect between the phenomena of the volcano and those of weather, etc. He points out that the explanation previously given of the supposed mechanism of Stromboli fail as completely to account for any such connection as they do to explain the rhythmical action of the volcano itself. The only distinct relations that can be gathered from the inhabitants of the Lipari group, between the appearances presented by Stromboli with different states of weather, wind or atmospheric pressure, are shown to be not those of direct cause and effect, but to be referable to acknowledged meteorological principles.

ART. XX.—*Brief Contributions from the Physical Laboratory of Harvard College.* No. XI.—*Increase of Magnetism in a bar of soft iron upon the reversal of the magnetizing current;* by WILLIAM A. BURNHAM.

IN testing the intensity of the magnetism induced in a bar of soft iron which forms the armature common to two electro-magnets, according to the method described by Mr. Sears in the July number of this Journal for the present year, the galvanometer indicates an increase in intensity of the magnetic state of the bar on the first passage of the current through the magnetizing helices, after a reversal of the poles of the battery. This increase disappears upon subsequent magnetizations of the bar. Various observations were taken by placing the induction coil C (see paper by Mr. Sears in the July number of this Journal) at different distances on the bar which was magnetized. A bar of soft iron was experimented with in the first instance, the coil C being at a distance of five centimeters from the middle or zero point. A Thomson's reflecting galvanometer was used and the readings were taken when the circuit was made and also when it was broken. The following table shows the results with C at various distances upon the bar.

TABLE I.

Distances of C from the middle of the bar in centimeters.	5		6		7		8		9		10	
Constant deflections on making and breaking circuit.	+ 40	— 40	+ 50	— 50	+ 60	— 60	+ 70	— 70	+ 80	— 80	+ 90	— 90
Deflection on the first passage of the current after changing the poles.	— 90	+ 50	— 110	+ 60	— 130	+ 70	— 150	+ 80	— 170	+ 90	— 190	+ 100
Return to constant deflections on second and succeeding passages of the current.	— 40	+ 40	— 50	+ 50	— 60	+ 60	— 70	+ 70	— 80	+ 80	— 90	+ 90

It will be seen by this table, that when the coil C stands at a distance of five centimeters from the zero point on the armature upon making the circuit, the galvanometer needle swings to the left 40°, and upon breaking the circuit it is deflected 40° to the right. The signs in our table denote this change of direction. When the poles are reversed and the circuit made, it swings 90° to the right, and when broken 50° to the left, returning to the constant deflection of 40° upon the second and each succeeding passage of the magnetizing current while the poles remain the same. In the same manner, when the coil stands at 6, 7, 8, etc., the first swings of the galvanometer needle are respectively 50 to 50, 60 to 60, etc., increasing in regular proportion upon each reversal of the poles and returning upon each second and succeeding passages of the current to constant deflections. The following are the results when a bar of steel was substituted for the soft iron.

TABLE II.

Distances of C from the middle of the bar in centimeters.	5		6		7		8		9		10	
Constant deflections on making and breaking circuit.	+ 20	— 20	+ 30	— 30	+ 40	— 40	+ 50	— 50	+ 60	— 60	+ 70	— 70
Deflections on the first passage of the current after changing the poles.	— 40	+ 25	— 50	+ 35	— 60	+ 45	— 70	+ 55	— 80	+ 65	— 90	+ 75
Return to constant deflections on second and succeeding passages of the current.	— 20	+ 20	— 30	+ 30	— 40	+ 40	— 50	+ 50	— 60	+ 60	— 70	+ 70

In the case of steel, when the coil C stands at a distance of five centimeters from zero, the galvanometer needle swings from +20° to −20°. Upon reversing the poles it is deflected +40° and −25°, and returns to 20° upon the second passage of the magnetizing current. By comparing Tables I. and II. it will be seen that the increase of magnetization after a reversal of the poles is not so great in iron as in steel, owing doubtless to permanent magnetism in the steel.

No. XII.—*On the Effect of Longitudinal Vibrations upon Electro-magnets;* by E. L. CARNEY.

IF a bar of iron or steel which is rendered magnetic by a magnetizing spiral receives a sharp blow or shock, an induced current is generated in a coil of fine wire, which is slipped upon the bar and is connected with a galvanometer. This current is opposite in direction to that of the magnetizing current. (Wiedemann, *Galvanismus und Elektromagnetismus*, 1863, pp. 375, 397. Also Dr. Emil Villari, *Ueber den transversalen Magnetismus des Eisens und des Stahles*, Pogg. Ann., cxxxvii, 1869, pp. 569–591.) This investigation was undertaken to ascertain the effect of the excursion to and fro of the particles of the iron in the state of longitudinal vibration. A fine coil, which we shall term C, was slipped upon the bars which were experimented upon and connected with a reflecting galvanometer. The rods —130 cm. in length and 1 cm. in diameter—were firmly clamped at their middle points. In the experiments which are comprehended in Tables I. and II. the magnetizing coil was placed at right angles to the rod or bar. In the experiments given in the Tables III. and IV. the rod formed the core of the magnetizing helix. We shall term the currents which were excited in the fine wire coil C, which was connected with the galvanometer, on the making of the magnetizing circuit, secondary currents; and the currents which were excited in the same coil by longitudinal vibrations, tertiary currents.

Table I.—The magnetizing circuit was made and broken instantly. On exciting longitudinal vibrations, it was found that a tertiary current resulted which was opposite in direction to the secondary current due to making the magnetizing circuit, and corresponded in direction to that of the secondary current resulting from breaking the magnetizing or primary circuit. With steel the results obtained were constant, while with iron they were variable. It was found impossible to excite more than one tertiary current.

I have called the current made by depressing the key on the magnetizing circuit positive throughout these experiments.

On the iron rod the magnetizing helix was forty centimeters from the center; on the steel rod only fifteen.

TABLE I.

Iron rod.		*Steel rod.*	
Current made.	Current caused by vibrations.	Current made.	Current caused by vibrations.
+	–	+	–
55	35	30	10
50	45	30	10
70	45	30	10
55	44		
53	40		
60	38		

Table II.—In this case the key was depressed and kept so, while both secondary and tertiary currents were obtained. In some cases there was more than one tertiary current, which was apparently due to the feebleness of the first vibration, as the number of deflections so obtained was more noticeable when such was the case. However, this could not have been the sole cause, for more than one tertiary current appeared when the vibration was all that could have been wished for. In the case of the iron rod it will be noticed that the tertiary vibrations are greater than the secondary, while the reverse is true with the steel rod.

The current was then broken and the amount of deflection noticed; and afterward a tertiary current obtained in the usual way. The latter currents were opposite in direction to those previously described. As there is but one instance of more than one tertiary being obtained, it was probably due to the feebleness of the first vibration.

In all these experiments the tertiary currents were in the *same direction* as the secondary ones, differing in this respect from those of Table I.

The magnetizing helix in both cases was fifteen centimeters from the center of the rods.

Iron rod.

Current made.	Current caused by vibrations.	Current broken.	Current caused by vibrations.
+	+	–	–
40	60–10–10	45	40–5
47	50–5–10–5	50	45
42	40–15	40	30
15	20	10	30
15	30	17	30
17	32	16	32

Steel rod.

Current made.	Current caused by vibrations.	Current broken.	Current caused by vibrations.
+	+	–	–
20	10	25	10
22	7	27	15
23	5–3	29	15
20	10	20	10

Table III.—Here the magnetizing coil was placed on the rods instead of at right angles to them, as in the previous cases, otherwise the experiment was performed as described in Table II.

The only noticeable difference was the increased strength of the deflections, and the entire absence of more than one tertiary current. In the case of the iron rod, when the current *was broken* the tertiary current was greater than the secondary, while in Table II. the tertiary current was greater when the current *was made.*

On the iron rod the magnetizing helix was distant forty centimeters from the center; on the steel rod twenty-two.

Iron rod.

Current made.	Current caused by vibrations.	Current broken.	Current caused by vibrations.
+	+	—	—
80	60	55	70
70	62	45	65
70	47	55	65
68	60		

Steel rod.

Current made.	Current caused by vibrations.	Current made.	Current caused by vibrations.
+	+	—	—
32	25	32	32
20	20	28	24
27	29	30	18
28	5	30	24
30	15	26	17

Table IV.—In all the previous cases the circuit was made while the rod was not vibrating.

Now the rod was first set in vibration, and the magnetizing circuit was made while it was vibrating, and the deflection noticed; when the rod came to rest, a tertiary current was excited by setting it again in vibration.

Here again more than one tertiary current was obtained, which could not have been due to the feebleness of the first vibration.

It will be noticed that the deflections obtained by the tertiary vibrations are opposite in direction to those obtained by the secondary ones, as was also the case in Table I.

On the iron rod the magnetizing helix was distant forty centimeters from the center; on the steel one fifteen.

Iron rod.		*Steel rod.*	
Current made while vibrating.	Current caused by vibration only.	Current made while rod was vibrating.	Current caused by vibrations only.
+	—	+	—
95	80	60	10
70	80	48	18
80	95	50	28
102	65–20	60	28–10
100	55–20	68	22–2
100	105	50	7
90	125		
122	105–25		

All the previous experiments were repeated with the rods at right angles to the meridian, but no noticeable difference in results was obtained. The conclusions are as follows:

1. When the primary magnetizing circuit was made and instantly broken, a tertiary current was excited by the vibrations which was less in amount and opposite in direction to the secondary current arising from the magnetism of the bar.

2. When the primary magnetizing circuit was made permanently, or in other words, while the bar was a permanent electro-magnet, the tertiary currents were in the same direction as the secondary; and in the case of soft iron uniformly greater.

3. When the rod formed the core of the magnetizing helix the tertiary currents were in the same direction as the secondary currents; and when the magnetizing circuit was broken they were, in the case of soft iron, greater than the secondary currents.

4. When the magnetizing circuit was made while the rod was in a state of vibration the tertiary currents were opposite in direction to the primary, and in several instances more than one was obtained at each trial.

No. XIII.—*Experiments on the Dissipation of Electricity by Flames*; by J. W. Fewkes.

By means of an electrometer made on the principle of Sir William Thomson's quadrant, I have been able to perform a few experiments in relation to the dissipation of small quantities of electricity by different kinds of flames.

These experiments were conducted with such small quantities of electricity as could be obtained by rubbing a vulcanite plate six inches square with a catskin. The sensitiveness of the electrometer to the electricity thus formed was very great. The experiments are given below.

Experiment 1.—An alcohol lamp, carefully insulated, was connected with the electrometer. The sections of the quadrant to which it was attached were then charged by means of the vulcanite plate, the opposite sections being at the same time in connection with the earth. The lamp was then carefully lighted. The spot of light, which had been deflected to the edge of the scale by the charge, quickly returned to the zero point, indicating a quick dissipation of the electricity by the flame.

Exp. 2.—The same conditions as those in Exp. 1 were observed, with the exception that a Bunsen burner was substituted for the alcohol lamp. The dissipation of electricity was the same as before, and took place, as near as could be observed, at the same rate as before.

Exp. 3.—I then substituted for the Bunsen flame a very fine jet of light, obtained by passing the gas through a finely pointed glass tube. The results obtained from this experiment indicate that the rate of dissipation is in no respect related to the size of the flame.

Exp. 4.—The end of the wire connected with the quadrant was then placed so that when the gas was lighted the wire point would be in the flame. The quadrant was then charged and the gas turned on without being lighted. The spot of light had no movement, and gave no sign of any loss of electricity by the quadrant. An artificial current of air across the wire point likewise had no effect in dissipating the charge.

Exp. 5.—The end of the wire was then placed in the jet of an atomizer, the same conditions being observed as in Exp. 1. The fine globules of steam and water issuing from the atomizer had no effect in dissipating the electricity of the quadrant.

I also performed two very striking experiments, which seemed to have some bearing upon this subject. The instruments used were the same as in the former experiments and the manipulation was as follows:

Experiment 1. — Carefully insulate a wire communicating with the electrometer, and place its point within a few inches of the flame of an insulated Bunsen burner. Let the spot of light be at the zero point Electrify the vulcanite plate with the catskin and hold it at an equal distance from flame and wire point. It is very difficult under these conditions to sufficiently electrify the quadrant so as to produce any deflection of the spot of light.

Exp. 2.—Place the wire point in the flame and then hold the electrified vulcanite plate up to the flame as before. The spot of light immediately is violently deflected, indicating the presence of electricity in the quadrant. This change, however, is soon dissipated by the flame, and the spot quickly returns to the zero point.

These last experiments seem to indicate that the flame has a much greater attraction for the electricity of the vulcanite plate than the copper point of the wire. Hence the difficulty of charging the quadrant in the first experiment.

When, however, the wire is in direct communication with the flame, as in the second experiment, the flame and the quadrant are at the same potential, and the increase of electricity in the flame produces a corresponding deflection of the spot of light.

No. XIV.—*Polarization of the Plates of Condensers;* by A. S. Thayer.

It is well known that in polarization batteries, of which Planté's battery is a type, a combination of the ions, resulting from electrolysis, takes place when the plates of the battery are connected, and a current results which slowly diminishes in strength. In the case of condensers made with solid dielectrics the same diminishing current is observed, and the following experiments would seem to show that it might be due to an electrolysis or decomposition of the material separating the plates of tin-foil. The experiments consisted in placing condensers of various kinds in a circuit, through which a current was made to pass by two Bunsen's cells, and noting their changes. The plates of the condensers were of tin-foil and had an area of about fifteen square inches. The experiments were as follows:

(1.) The dielectric used was a sheet of dry glazed paper. The condenser could not be charged so as to give a perceptible discharge.

(2.) When a sheet of glazed paper, moistened with shellac, was substituted for the dry paper, the discharge was sufficient to send the light off the scale of the galvanometer, and continued for some minutes.

(3.) Dry goldbeaters' skin was used as a dielectric, and no deflection could be obtained.

(4.) The goldbeaters' skin, when moistened with shellac, gave a slowly diminishing deflection.

(5.) The dielectric was made by flowing the surfaces of the plates with a solution of wax and gasoline, and a slowly diminishing deflection was obtained.

(6.) The condenser used in (4) was tried again after the shellac had dried and again gave a diminished deflection less than the first deflection.

(7.) The condenser used in (5), when tried again after a day or two, did not again give a deflection.

(8.) Unglazed paper dry and oiled gave no deflection.

(9.) Glazed paper oiled gave a very slight deflection, and the galvanometer needle immediately returned to zero.

(10.) Glazed paper wet with water and covered with shellac gave the greatest deflection of all the dielectrics. The light was sent completely off the scale and was only brought back by shunting the galvanometer. The discharge also continued a long time.

(11.) The conducting power of some of the various dielectrics was tested. The goldbeaters' which had been covered with shellac transmitted no current after it had been allowed to stand for a week. Freshly oiled and dry oiled paper did not conduct at all. Glazed paper covered with shellac gave a deflection nearly off the scale. Glazed paper wet with water and covered with shellac transmitted a current sufficient to send the light entirely off the scale.

What these experiments directly go to show are, first, that condensers with moist dielectrics received a greater charge than those made with dry, and second, that the better the dielectric conducted, the greater the charge the condenser was capable of receiving. From these facts it would seem that the slow discharge of these condensers was very probably due to polarization. The best condensers, as shown by the experiments, possessed dielectrics which were moist and possessed considerable conductivity. The dielectrics when dry scarcely conduct at all. Their conduction when moist must therefore have been mainly due to electrolysis, since liquids conduct electricity only in very small quantities without being decomposed. The electrolyte was therefore decomposed, and the re-combination of the products of decomposition caused the return current. An exact analogy is thus determined between the case of the lead plates and these condensers. Whether it is an analogy that would hold in the case of all condensers which slowly discharge themselves, is an interesting question.

SCIENTIFIC INTELLIGENCE.

I. Physics.

1. *Dielectricity of Insulators.*—M. L. Boltzmann has presented to the Vienna Academy of Sciences a paper on an experimental determination of the constant of dielectricity of insulating bodies. Faraday first observed the property of insulating solid bodies of being dielectric, that is, of increasing the capacity of a condenser by their presence between its two plates. Siemens, and later Gibson and Barkeley, have studied the phenomenon experimentally, while Clausius, Maxwell and Helmholtz have sought for the theoretical conditions. Representing by $\frac{dv}{dNi}$ and $\frac{dv}{dNe}$ the differential ratios of the potential on the inner and outer faces of the insulator, along the normal, the quotient $\frac{dv}{dNe} : \frac{dv}{dNi}$ will be the constant of dielectricity D. Neglecting the free electricity accumulated at the borders of the plates of the condenser, and calling n the distance of the two plates, or the thickness of the dielectric layer, the capacity of the condenser will be inversely proportional to $m-n+\frac{n}{D}$.

The measurement of the quantity of electricity was made by a Thomson's electrometer. The condenser was charged by a battery of 18 Daniell cells. The condenser was made of the form proposed by Kohlrausch. The insulators were hard caoutchouc, paraffine, sulphur and resin; then as imperfect insulators, stearine, glass and gutta-percha. The theoretical conclusions of Helmholtz on the relation between the capacity of the condenser and the thickness of the insulating layer and plates were verified even when there were several insulating plates instead of one.

These experiments were made both with a momentary and with a continuous charge; both cases gave nearly equal capacities. From this we may conclude that the dielectric polarization is produced quite rapidly.

The author found as probable values of the constant of dielectricity the numbers for sulphur, 3·86; hardened caoutchouc, 3·15; resin, 2·55; paraffine, 2·32.

Maxwell arrived at the conclusion that the square root of the constant of dielectricity ought to equal the index of refraction. The following table shows that this law holds true quite within the limits of probable error:

	$\sqrt{D}$.	Index of refraction.
Sulphur,	1·960	2·060
Resin,	1·597	1·563
Paraffine,	1·523	1·536—1·516

Pogg. Annal., cli, 682; *Bib. Univ.*, cxcviii, 202. E. C. P.

2. *Flow of Saline Solutions through Capillary Tubes.*—M. T. HÜBENER finds that the velocity of flow of solutions in capillary tubes appears to depend solely on their weight and capillary adhesion. Thinking that besides the adhesion and weight, the intermolecular friction must be an important factor in determining the velocity, he compared a number of solutions brought to the same density, but having very different chemical compositions. The liquid was introduced into a vertical rectilinear glass tube 50 cms. in length and 1·75 cms. in diameter, having a capillary continuation about 60 cms. in length. The large tube presented two marks, and with a seconds-watch the time which was required for the level of the liquid to fall from one of these marks to the other, was accurately measured.

Operating in this way upon solutions of chloride, bromide and iodide of potassium, chloride of sodium and ammonium, with a density of 1·059 and a fixed temperature, the author ascertained that *the velocity* of flow of saline solutions is as much lower as the atomic weight of the salt dissolved is less. For the different binary bodies above indicated, it is the metal which has the greatest influence upon the velocity of flow, much more than the metalloid. The variations presented by the velocity from one body to another are as much more marked as the tube is more capillary and as the concentration of the solution is greater.

On comparing two solutions, of chloride of sodium and of ammonium, at 1·1058 density, the author arrived at the remarkable result that the times of flow of these two salts are found to be very sensibly proportional to their equivalents. From this experiment and from others analogous, extended also to the chlorides of the alkaline-earthy metals barium, strontium, magnesium, M. Hübener thinks it may be concluded generally, with a high degree of probability, that the velocities of flow of these bodies in solution in water, to a certain degree of concentration, are in the same ratio as their equivalents.

The explanation of these facts is, according to M. Hübener, to be found in the circumstance that the molecules of substances which have a higher equivalent are larger, but, on the other hand, in less number, and consequently must give rise to less friction with the solvent in which they are held, thus communicating greater mobility to the solution.—*Bib. Univ.*, 197, 75; *Phil. Mag.*, xlviii, 77. E. C. P.

3. *Change of Volume by Fusion.*—Mr. R. MALLET presented to the Royal Society, at a recent meeting, a paper on the alleged expansion in volume of various substances in passing from a state of fusion to that of solidification. Since the time of Reaumur it has been assumed that certain metals, including bismuth, cast-iron, antimony, silver, copper and gold expand at or near their points of solidification. Recently certain cast-iron slags have been added to this number, but the following experiments seem to show that except in the case of bismuth the evidence is insufficient, and that with iron and slag it is quite erroneous. A conical vessel of wrought-iron about two feet in depth, 1·5 feet in diameter at the base, and with an open neck six inches in diameter, was weighed accurately, empty, and also when filled with water, level to the brim; the weight of its contents in water was thus obtained. After being dried, it was now filled to the brim with molten gray cast-iron, additions of molten metal being made to maintain the vessel full until it had attained its maximum temperature (yellow heat in daylight) and maximum capacity. The vessel and its contents of cast-iron when cold were weighed again, and thus the weight of the cast-iron obtained. From these data the specific gravity of the molten metal is readily obtained after applying the proper corrections. The final result for the molten cast-iron was found to be 6·650. The specific gravity of the solid cast-iron was determined in the usual manner and found to be 7·170, or greater than when in the molten state. This result was verified by heating two similar 10-inch shells (1·5 inches thick) to the same high temperature and filling one with melted iron. Its dimensions were then measured every 30 minutes during cooling. The second shell was employed to measure the permanent change of volume due to heating and cooling. After applying the proper and somewhat complicated corrections, it was found that there was no difference in volume of the two shells and that the solid ball was in perfect contact with the enclosing sphere, showing that no change in volume of the contents of the shell had taken place, which was further corroborated by the fact that the central portion was much less dense than the exterior, whereas if there was an expansion, the reverse would be the case.

To account for a solid floating on a liquid of a less specific gravity than its own, the author assumes the existence of a force which he calls the repellant force. The amount of this force depends on the relation between the volume and the effective surface, or surface indented by the solid. It also depends on the difference of temperature of the solid and liquid.

In the case of lead, solid pieces float on the liquid metal, although the contraction on solidifying is here marked and well known. In fact the solid at 70° has a specific gravity of 11·361, while when melted its specific gravity is only 11·07. Floating or sinking takes place according to the relation between the volume and effective surface; thin pieces with large surface always floating, and vice versa.—*Nature*, 156. E. C. P.

II. Geology and Natural History.

1. *Reasons for some of the changes in the subdivisions of Geological time in the new edition of Dana's Manual of Geology;* by the Author.

(1.) *Archæan time.*—The first era in geological history is called, in the old edition of the Manual, *Azoic* time or age. The term Azoic was always objectionable, because it affirmed what was not proved. The discovery of the supposed animal fossil called *Eozoon*, shortly after the first edition was issued, led soon to the proposed substitution of *Eozoic* for Azoic. Those who received the suggestion with favor did not consider that if the so-called Azoic included an Eozoic era, it included a true Azoic also, an era of rocks and seas without life; for while the rocks and seas of the globe were above the temperature of boiling water, the Eozoic era could hardly have begun. The assumption that all those early rocks were Eozoic has nothing to favor it.

A general term for the whole era, free from hypothesis, was therefore needed. Murchison's term, *Bottom rocks*, was not satisfactory. *Archæan*, signifying simply *beginning-time*, was therefore used. Under *Archæan time*, there are the Azoic and the Eozoic ages, although their limits have not yet been marked out in the rocks of the world, and probably never will be, since the rocks are now crystalline, through metamorphism, and, with few exceptions, it cannot be learned whether life existed during their formation or not.

I pass now to the Lower Silurian era, which, in the new edition, is divided into (1) the Primordial or Cambrian, (2) the Canadian, and (3) the Trenton periods, instead of (1) the Potsdam or Primordial, (2) the Trenton and (3) the Hudson periods of the old edition.

(2.) *Primordial or Cambrian Period.*—This period in the new book has unchanged limits, except in the removal of the Calciferous sand-rock, the uppermost portion, whose fossils, as stated by Billings and the Geological Reports of Canada, are more nearly related to those of the following part of the Lower Silurian. The word *Potsdam* is dropped because the Potsdam sandstone is the least characteristic part of the formation. The term *Cambrian* is added, because the period is identical essentially with the Cambrian of the British geologists. The trilobites and other species found within a few years in the Cambrian rocks of Britain—till then supposed to be almost destitute of organic remains—are all Primordial species, and so they were declared in the announcement of the facts by the discoverer. Lyell, in his Student's Elements of Geology (1871), makes the Cambrian extend to the Tremadoc slates, above the Lingula flags (or the equivalent of the Potsdam sandstone); and he states that all, from the Lingula flags to the Menevian of the Lower Cambrian inclusive, are embraced in the Primordial of Barrande. Geikie, in his edition of Jukes's Geology, draws the upper line below the Lingula flags, but says these flags "pass down conformably into the top of the Cambrian series, and are indeed closely linked with that series, both stratigraphically and paleontologically." Moreover, he adds, quoting Ramsay, the fossils of the Lingula flags differ to a great extent generically and almost wholly specifically, from those of the overlying groups."

The fossils of the supposed Huronian of Newfoundland, described by Billings, are referred in the work (p. 176) to the Primordial, because of the position and the occurrence of fossils. The Huronian is not a formation of known age. The original Huronian contains no fossils to mark off the era, and may yet turn out to be Silurian; and nearly all other regions or rocks called Huronian have been so pronounced on lithological evidence; which is no evidence at all, since the kinds of the Huronian are not confined to it.

(3.) *Canadian Period.*—The fact of the existence of an important Lower Silurian formation in Canada, near Quebec, abounding in fossils, and of about the age of the Calciferous sand-rock and the Chazy limestone, is mentioned, in the first edition of this work, as one of the discoveries of the Canadian Geological Survey, under Sir William Logan. The Reports of the Survey point out the close relations in fossils of the Calciferous sand-rock, Quebec group, and Chazy, and their rather wide separation from the overlying Trenton limestone, showing that they represent, naturally, a distinct period in the Lower Silurian era. This is called the Canadian, because the rocks are well displayed in Canada, and there the most important part of the facts respecting it were first brought to light.

(4.) *Trenton Period.*—The Trenton limestone and the Cincinnati (or Hudson River) group are closely alike in fossils, and the relation which exists between them (one dependent on physical changes) is best indicated by making the two represent subdivisions of one period; the differences being not equal to those distinguishing periods. The *Hudson River group* of the old edition of the Geology, was so named from the slates along the Hudson River. But when Logan gave his strong reasons for regarding these slates as of the age of his Quebec group, most American geologists felt it important that the name Hudson River, as applied to the beds overlying the Trenton limestone, should be changed. The change has been made to "Cincinnati" group in the recent geological reports on the surveys of States west of New York, and this term was accordingly adopted in the Geology.

(5.) *Upper Silurian, Oriskany Period.*—The Upper Silurian, in the first edition, closes with the Lower Helderberg formation, in accordance with the early suggestion of De Verneuil. It has always been doubted whether this places the top of the American Upper Silurian high enough in the series to correspond with the foreign; because so many Upper Silurian corals and other species of Britain occur higher up, that is, in the American Lower Devonian; and, moreover, remains of terrestrial plants exist in the Upper Silurian of Britain and Europe, but none had been found in North America. Professor Hall, in the third volume of his Paleontology (1859), presents reasons for transferring the Oriskany period from the Devonian to the Upper Silurian. He says, of these Oriskany sandstones, that "although usually very distinct from the limestones below, there are nevertheless localities where a passage occurs between the two rocks; and in such instances, some of the fossils, usually restricted to the lower beds [Lower Helderberg], pass into those above [Oriskany beds]. Instances of this kind occur in Maryland; and from the collections of the Canada Survey, by Sir William Logan, we are prepared to find in some parts of the Continent an intimate blending of these formations." During the preparation of the new edition of the Manual, I received word from Professor Hall that he still adhered to these views; and I accordingly made the change. Dr. Dawson has, within a few years, found fossil land-plants in beds at Gaspé, on the Gulf of St. Lawrence, containing Oriskany fossils; and now the earliest known species of this kind are from the Upper Silurian in America, as well as in Great Britain.

(6.) *Devonian Age, Catskill Period.*—In the Appendix to the first edition of the Geology, it is stated that recent observations made by E. Jewett and J. M. Way, in Delaware County, New York, tend to prove that the rocks of the so-called Catskill group are probably all Chemung. A note on this subject, by E. Jewett, is published in the American Journal of Science for 1862. But since then, no further facts on this important question have been published. Moreover, while the new edition of the Geology was in preparation, I received letters from Professor James Hall, of Albany, and from J. Peter Lesley, of Philadelphia (who was with Professor Rogers in the geological survey of Pennsylvania, and is now at the head of the new survey of the State), protesting strongly against the conclusion Mr. Jewett had announced. For these reasons, additional information appeared to be needed before making the Catskill beds Chemung. The Catskill formation of Pennsylvania has a thickness, according to Rogers, of 6,000 feet. Reference to the facts observed by Jewett and Way were inadvertently omitted.

(7.) *Quaternary Age, or Age of Man.*—This age in the new edition is merged in the Quaternary. The reasons for this, and for other modifications of statements introduced, are so fully given in the work, that they need not be here repeated.

The term Quaternary, which has been substituted for Post-tertiary, is in general use among European geologists. Moreover, the geological work of the era was so widely different in its most prominent features from that of the Tertiary and preceding ages, that an independent term, implying no special relation to the Tertiary, was desirable; and such is Quaternary.

2. *The Winged Fruits of the Carboniferous genus Cardiocarpus.*—The genus Cardiocarpus was probably related to the modern Conifers of the *Welwitschia* type, as shown by the similarity of the fruit and also by the close relation of the leaves, if those called Cordaites belong, as both Geinitz and Newberry have independently remarked, to Cardiocarpus. The Welwitschia is an embryonic form of Conifer, it producing no leaves except the cotyledonous; but, while probably unlike *Cordaites* in its embryonic features, it shows what leaves and fruit are consistent with the type of Conifers.—*Dana's Manual of Geology*, pp. 328, 330.

3. *Coal of the Carboniferous era not made of bark.*—The suggestion has been made, in view of the many Sigillaria stumps hollowed out by decay and flattened stems of other trees, found in the Coal-measures, filled with shale or sandstone, that the vegetable debris from which the coal has proceeded was largely bark, or material of that general nature. But the occurrence of such stumps and stems outside of the coal-beds, while proof that the interior wood of the plants was loose in texture and very easily decayed, is no evidence that these trees contributed only cortical portions to the beds of vegetable debris. Moreover, the cortical part of Lepidodendrids (under which group the Sigillarids are included by the best authorities) and of Ferns also, is made of the bases of the fallen leaves, and is not like ordinary bark in constitution; and Equiseta have nothing that even looks like bark. This cortical part was the firmest part of the wood; and for this reason it could continue to stand, after the interior had decayed away,—an event hardly possible in the case of a bark-covered Conifer, however decomposable the wood might be. Further, trunks of Conifers are often found in the later geological formations, changed, *throughout* the interior, completely to brown coal or lignite.—*Ibid.*, p. 362.

4. *The ash of the better coals of the American Carboniferous age often derived wholly from the plants.*—The following are analyses of the ash of *Lycopods* (1, 2), *Ferns* (3, 6), *Equiseta* (7, 8), *Conifer* (9), a Moss of the genus *Sphagnum* (10), and a *Chara* (11).

	KO	NaO	CaO	MgO	Fe^2O^3	Mn^3O^4	Al^2O^3	PO^5	SO^3	SiO^2	Cl
1. Lyc. clavatum	31·90	2·68	4·13	5·89	6·00	--	22·20	7·30	3·55	13·01	--
2. Lyc. clavatum	25·69	1·74	7·96	6·51	2·30	2·53	26·65	5·36	4·90	13·94	3·13
3. Asplenium filix	45·5	5·2	7·9	7·4	1·5	--	--	20·0	6·8	2·2	4·6
4. Aspidium filix	39·80	5·31	18·74	8·28	0·97	--	--	2·56	5·40	4·38	14·72
5. Osm. spicant	23·65	3·33	4·09	6·47	1·17	--	--	1·76	1·29	53·00	5·82
6. Pteris aquilina	19·35	4·78	12·55	2·30	3·94	--	--	5·15	1·77	43·65	6·20
7. Eq. arvense	19·16	0·48	17·20	2·84	0·72	--	--	2·79	10·18	41·73	6·26
8. Eq. Telmateia	8·01	0·63	8·63	1·81	1·42	--	--	1·37	2·83	70·64	5·59
9. Pinus abies	12·84	5·64	58·27	2·81	1·60	*tr.*	*tr.*	2·60	1·60	12·55	2·06
10. Sphag. commune	8·02	12·40	3·17	4·92	6·35	*tr.*	5·89	1·06	4·33	41·69	12·09
11. Chara fœtida	0·85	0·44	95·35	0·99	0·67	--	--	0·54	0·42	1·22	0·16

Analysis 1, is by Ritthausen; 2, Aderholt; 3, A. Weinhold; 4, Struckmann; 5, 6, 9, Malaguti & Durocher; 7, 8, E. Wittig; 10, H. Vohl.; 11, Schultz-Fleet.

In the analyses that have been made of Lycopods, the amount of ash is 3·2 to 6 per cent in weight of the dried plant; of Ferns, 2·75 to 7·56 per cent; of *Equisetum arvense*, 18·71 per cent; of *Eq. Telmateia*, 26·75 per cent; of Conifers, mostly less than 2 per cent; of *Chara fœtida*, 31·33 per cent; of *Fungi*, 3·10 to 9·5 per cent; of Lichens, 1·14 to 17 per cent (the last in *Cladonia*), but mostly between 1·14 and 4·30 per cent. In *Lycopodium dendroideum*, Hawes, in his analyses, found 3·25 per cent of ash; in *L. complanatum*, 5·47 per cent, and in *Equisetum hyemale*, 11·82 per cent.

Lycopodium chamæcyparissus afforded Aderholt 51·85 per cent of alumina; or, when without spores, 57·36 per cent; while Ritthausen obtained 39·07 alumina for this species, and 37·87 for *L. complanatum*. In Lycopods, the silica constitutes 10 to 14 per cent of the ash. In the ash of Mosses have been found 8 to 23·58 per cent of potash, 4 to 16 of silica, 1·06 to 6·56 of phosphoric acid, 4·9 to 10·7 of magnesia. Among Ferns, the amount of ash, so far as determined, varies from 5 to 8 per cent.

The ash of *Fungi* affords 21 to 54 per cent of potash, 0·36 to 11·8 of soda, 1·27 to 8 of magnesia, 15 to 60 of phosphoric acid, and 0 to 15·4 of silica. Among *Lichens*, the ash of *Cladonia rangiferina* contains 70·34 per cent of silica; of other species, less, down to 0·9 per cent.

Trapa natans, of bogs, in Europe, affords 13 to 25 per cent of ash; and 25 per cent of this are oxide of iron (Fe^2O^3), with a little oxide of manganese. Of the ash of the fruit scales, over 60 per cent are oxide of iron.

Since, according to the average composition of Lycopods, the dried plant affords 5 pounds of ash to 100 of the plant, and 40 per cent of this are alumina and silica (27 alumina and 13 silica), these two ingredients make up 2 per cent of the plants. Ferns, with the same amount of ash, afford, as the average, 27 per cent of silica, with no alumina. Equiseta afford, on an average, 20 per cent of ash, and 50 per cent of this may be silica. Supposing, now, that Lycopods (*Lepidodendrids*, etc.) afforded one-half the material of the coal-beds, and the other plants the rest, and that the silica and alumina of the former averaged 40 per cent, and of the latter only 27 per cent, this being all silica, then the amount of these ingredients afforded by the vegetation would be 1·66 per cent of the whole weight when dried. This would make the amount of silica and alumina, in the bituminous coal made from such plants (supposing three fifths of the material of the wood lost in making the coal, as estimated on page 363), 4 per cent; and the whole amount of ash about 4·75 per cent. At the same time, the ratio of silica to alumina would be nearly 3 to 2.

Now many analyses of the bituminous coal of the Interior basin have obtained not over 3 per cent of ash, or impurity, although the general average, excluding obviously impure kinds, reaches 4·5 to 6 per cent; being, for the coals of the northern half of Ohio, 5·12, and for the southern half 4·72 per cent.

It hence follows that (1) the whole of the impurity in the best coals may have been derived from the plants; (2) the amount of ash in the plants was less than the average in modern species of the same tribes; (3) the winds and waters for long periods contributed almost no dust or detritus to the marshes; and (4) the ash, or else the detritus, was greatest in amount toward the borders of the Interior marsh-region. In that era of moist climate and universal forests, there was almost no chance for the winds to gather dust or sand for transportation.—*Ibid.*, pp. 365, 366.

5. *Fossil plants of the Carboniferous age in the Protogine of the Alps.*—The hypothesis has been urged of late years that the gneissic, hornblende, chloritic and other allied metamorphic rocks of New England and the rest of the world are exclusively *pre-Silurian;* and the fact that the protogine (chloritic gneiss) of the Alps had been made, by one geologist, pre-Silurian has been regarded by the supporter of the hypothesis as the best evidence in its favor: though if good truth, it really proves nothing with reference to the age of the protogine of the rest of the world. But, in Nature, of July 30th, occurs the following announcement.

"Prof. Schimper, of Strasburg, in a paper read before the Botanical Congress at Florence, claims to have discovered a fossil plant in "protogine," a rock hitherto considered of igneous origin, which occurs in the form of erratic blocks on the sides of Mont Blanc and in the plains of Piedmont. The specimen, which was collected by M. Sismonda, and is preserved in the Museum of the Turin University, has been identified by Prof. Schimper as *Annularia sphenophylloides*, a plant, perhaps aquatic, widely distributed in the coal strata of Mont Blanc."

6. *The proposed genus Anomalodonta of Miller identical with the earlier Megaptera of Meek.*—In the first issue of the Cincinnati Journal of Science I observe that the editor, Mr. S. A. Miller, proposes a new genus under the name of *Anomalodonta*, to include a group of shells allied to *Ambonychia* of Hall. These shells constitute a very interesting type, evidently belonging to the family Aviculidæ. Like *Ambonychia*, they are destitute of an anterior wing, but have, posteriorly, a very large one, which gives the shell a trigonal outline. Mr. Miller's type specimens show them to have a broad striated area, such as *Myalina* possesses, with, at the anterior end of the hinge, an oblique fold or ridge, not properly a tooth perhaps, together with a corresponding depression, in one or both valves. We see just such characters in the broad forms of *Myalina*, as for example in *M. ampla* of Meek and Hayden, so that, although generically distinct from Mr. Miller's shell, there seems to be little to distinguish the latter except outline and radiating costæ, unless he is right in stating that his shell has a large impression of the anterior adductor muscle, which, in view of the affinities of the shell, is very improbable, especially so large an impression and in so low a position as he has figured it. Compared with *Ambonychia*, the hinge of his shell differs materially in wanting the two anterior well-defined teeth of that genus, and also its three elongated posterio-lateral teeth.

This genus *Anomalodonta* was proposed for a group of shells identical with the one for which Meek & Worthen proposed the subgeneric name of *Megaptera* in the Proceedings of the Chicago Academy of Science in 1866. Mr. Miller himself states that his proposed genus will include *Ambonychia* (*Megaptera*) *alata* Meek (Ohio Paleontology, vol. i, p. 131), of which he says Mr. U. P. James has specimens showing the same hinge characters. He also states that his proposed genus will include, among others, *Megaptera Casei* of Meek & Worthen, which is the type species of *Megaptera*. In short, he proposes a new genus to include the type of a previously established one, together with another species of the same, described by one of the authors of the forenamed genus. *Megaptera* has unquestionable priority, and must stand before Mr. Miller's name and all others also, unless, however, the name *Megaptera* may be objected to on account of its previous use by Dr. Gray for a genus of whales. Even in that case Mr. Miller's name could not stand, because Mr. Meek, in the Proceedings of the Philadelphia Acad. Nat. Sci., 1872, proposed to use the name *Opisthoptera* as a substitute in case such objection should arise. Indeed, I am informed by a friend that Mr. Meek in his work, now in manuscript, has decided to use the name *Opisthoptera Casei* and *O. alata* for *Megaptera Casei* and *M. alata.*

The fact that Meek & Worthen did not describe the hinge of *Megaptera*, which was unknown to them, in their generic diagnosis, has no bearing upon the question of priority. If it were otherwise, few genera of fossils would be allowed to stand after the discovery of more perfect specimens than the original workers possessed. Nor does the fact that Meek & Worthen called theirs a sub-genus affect the question, for the rule covers such cases as well as others, and the use of such a designation under similar circumstances indicates on the part of an author a degree of caution that all true naturalists will approve.

Further, upon comparing Mr. Miller's description of his species *gigantea* with Meek's *alata*, I am unable to find that the former possesses characters sufficient to separate it specifically from the latter. Mr. Miller seems to have relied solely upon the large size and the preservation of concentric striæ of his shell to separate it from *M. alata.* As Mr. Meek distinctly mentions the existence of the striæ in his shell, Mr. Miller's species seems to be reduced to the very insufficient ground of size alone. C. A. W.

7. *On the Quaternary containing the New Brunswick fossil Cetacean; on Niagara Coral reefs; and on Niagara fossils in trap;* by Rev. D. HONEYMAN. (From a letter to J. D. Dana, dated Halifax, N. S., Provincial Museum, Aug. 6.)—I have just returned from the locality of the fossil Cetacean, in New Brunswick. I have examined the bed of the fossil and found it a clay covered with from twelve to twenty feet of sand and gravel. In the bed I found well-preserved shells, e. g., *Balanus Hameri, Buccinum undatum, Fusus tornatus, Natica Grœnlandica, N. helicoides, Leda caudata, L. truncata, Mya arenaria, M. truncata, Tellina Grœnlandica, T. proxima,* and *Saxicava rugosa.* This is one of a series of cuttings extending from Bathurst toward Canada. I have collected the above and others from cuttings, six in number, at intervals for a distance of thirty miles.

The Rev. Mr. Paisley described one of these cuttings in the Canadian Naturalist. This occurs about three miles north of Bathurst at Somersetvale. It is the first cutting north of the river Tittigouche. The fossils are here in the greatest variety and abundance. The following measurements give the approximate height of the bed above the present sea level: The height of the iron bridge over the Tittigouche is 60 feet; the foundation is about ten feet above sea level; the height of the top of the clay in the cutting is about seven feet above the height of the bridge; total, 77 feet. I found the vertebra of a mammal with the shells in this cutting. As Dr. Gilpin is at present in the country, it has not been examined.

I also examined two other noteworthy localities; one of these is Belledune. On the shore I found Niagara limestone strata, having an abundance of corals. I collected fine specimens of *Favosites Gothlandica*, *Halysites catenulata*, species of *Cyathophyllum*, or allied.

At Cape Bon Ami, I unexpectedly came upon a fine exposure of strata of the same age—great ancient coral reefs, replete with magnificent specimens of corals, which were found almost falling out of the rocks, and scattered on the shore. I collected magnificent specimens of *Favosites Gothlandica*, *F. basaltica*, with specimens of *Zaphrentis*, sp. ?, *Orthoceras*, sp. ?, *Strophomena depressa*, *Atrypa reticularis*, *Athyris nitida*, *Rhynchonella*, sp. ? *Orthis*, sp. ? and crinoidal joints.

Associated with this is a great dike of intrusive trap. It is both basaltiform and amygdaloidal; geodes and amygdules are abundant. The minerals are agates, zeolites, and calcite. In examining the amygdules, a moniliform specimen attracted my attention; on detaching it and examining it with a magnifying glass, I found it to be a specimen of *Favosites Gothlandica*, which had dropped out of the penetrated Niagara limestone strata, had become imbedded in this ancient *lava*, and formed an *amygdule* in common with the agates, etc. On examining the mass of amygdaloidal trap at a short distance from its line of contact with the fossiliferous strata, I was fortunate enough to find another specimen more firmly imbedded in the trap than the preceding, so much so as to be wholly incorporated with it, there being small pieces of agate imbedded in the hand specimen of enclosing trap secured. The exposed section of coral shows structure as distinct as any figured on page 239 of your Manual. I also found a detached specimen of *Cyathophyllum* with trap attached. Well-preserved fossils embedded in trap are new in my experience. These indicate conditions which some might be disposed to question. They show that the trap was soft fluid when the corals were imbedded. They show that the trap was post-Upper Silurian. They show that the existing heat in that part of the mass was not sufficient to metamorphose the organism,—or that corals have a greater power of resistance than other organic remains.

8. *Descriptions of new species of Goniatidæ, with a list of previously described species;* by James Hall. 4 pp. 8vo. Printed in advance of the 27th Report on the State Museum of Natural History. (Received from the author in May last.) The species described are *Goniatites complanatus* Hall, (referred, from a

poor specimen, in the 4th volume of the N. Y. Paleontology, to *Clymenia*); and a variety *perlatus*, of the same species, from the Chemung in Cortland Co., N. Y.; *G. unilobatus* Hall, from the Hamilton shales, Cayuga Lake; *G. simulator* Hall, from the Chemung, near Ithaca; *G.* (*Clymenia?*) *Nundaia* Hall, same locality; *G. Chemungensis*, var. *equicostatus*, from the Chemung of Athens, Pa.

9. *Preliminary Report on the First Season's Work of the Geological Survey of Yesso;* by B. S. LYMAN. 46 pp. 8vo. Tokei, 1874.—Besides other topics, the Report gives information respecting the position of the Yesso coal, speaking of it as brown coal, probably of Tertiary age. Springs of thick oil, or rather black tar, come from deposits, perhaps Tertiary, in Washinoki, Yamukushinai and Idzumisawa.

10. *Reply to Dr. T. Sterry Hunt;* by F. A. GENTH. (Read before the American Philosophical Society, July 17, 1874.)—Dr. T. Sterry Hunt has published, in the Proceedings of the Boston Society of Natural History, vol. xvi, March 4th, 1874, an article, entitled: "*On Dr. Genth's Researches on Corundum and its associated minerals*," in which he charges me—*in common with many others*—of having *fallen into errors* and of having been *led to conclusions wholly untenable*, for *lack of a clear understanding as to replacement, alteration and association in the mineral kingdom.*

He then gives an outline of the manner in which the various alterations in a mineral species may take place, by replacement, envelopment and epigenesis, with examples for each, and dwells at more length upon the fallacy of considering the alterations of many minerals and rock masses as the *result of an epigenic process; a doctrine which has been embodied in the dictum of Professor Dana: "regional metamorphism is pseudomorphism on a broad scale."**

*Mr. Hunt knows that this "dictum of Prof. Dana," as he calls it (which is a clause in a sentence of a book-notice written by me in this Journal, xxv, 445, 1858), does not represent at all the views on metamorphism that I have held for the past twelve years; for, besides having given him a copy of my Geology in 1862, I took especial pains, in 1872, in my review of his American Association Address, to refer him to the chapter on Metamorphism in the work, that he might read and appreciate the grossness of the misrepresentation. He knows, also, that the statement in the preceding part of the same sentence in his paper, that the "advocates of the doctrine of transmutation have not hesitated to assert, upon this supposed evidence [that of facts connected with replacement and envelopment], the conversion of almost every mineral species into some other, and to extend this view to rock-masses, declaring that the *greater part of all the so-called metamorphic or crystalline rocks are the result of an epigenic process*"—and another, in the next sentence following this, that "*the advocates of this doctrine maintain that a mass of granite or diorite may be converted into serpentine or limestone, and that a limestone may be changed into granite or gneiss, which may in its turn become serpentine*" (both of which he put forth in the same Address), have been shown by me to be untrue. For I assured him in the review and its sequel that I had never held the extravagant doctrine thus attributed to myself and others; that the idea of such transformations as are above enumerated had never occurred to me until found in his foolish charge against "Gustaf Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others," in his Address; and I demon-

He then refers briefly to the results of my investigation on corundum, in which I have shown that by "epigenic" pseudomorphism this mineral has been altered into numerous more complex species and rock masses—and winds up by stating that he *not only has carefully studied my paper*, but has also *examined the extensive collection of species upon which my conclusions were based*, and that—*all the phenomena in question are nothing more than examples of association and envelopment, and that the corundum-bearing veins had their parallels in the granitic veins with beryl and tourmaline in the White Mountain rocks, and the calcareous veinstones with apatite, pyroxene, phlogopite and graphite of the Laurentian rocks.*

I may be permitted to say a few words in reply to Dr. Hunt's assertion, than I had fallen into errors and had been led to wholly untenable conclusions.

When I had the good fortune to obtain a few years ago the *first real* pseudomorph after corundum—the spinel from India, and afterward brought together numerous specimens of analogous alterations, showing from the *same* locality crystals of corundum without any, and others representing all stages of alteration from a thin coating to the complete disappearance of every vestige of corundum, and when I proved that such changes have resulted in the conversion of corundum into about two dozen mineral species; I could not understand how any unprejudiced mind could arrive at any other conclusions, but that these extraordinary occurrences which I have described were the result of epigenic pseudomorphism.

This opinion has been adopted almost without exception by all who have had an opportunity to examine my specimens, or who have studied my paper. If Dr. Hunt differs from me, I certainly will not deny to him the right to believe what suits his own notions; but when he boldly charges me with having committed errors, I want better proofs than a repetition of his views, with which we were familiar long ago. He certainly has not a single fact which could show the fallacy of my conclusions, or he would have produced it.

The corundum alterations have nothing in common with the Fontainebleau crystals, or with stanniferous orthoclase, or the green

strated that all writers on pseudomorphism, with but one or two exceptions, would repudiate it as strongly as myself. Nevertheless, he finds it to suit his purpose to repeat the citation and the perverted statement without a qualifying remark.

My review of Mr. Hunt's Address, and the sequel, in which these points are fully discussed, are contained in volumes iii of this Journal, page 86, iv, page 97, 1872, and v, page 312, 1873. I state that the regional metamorphism in which serpentine—a hydrous magnesian rock—is formed, has been regarded by writers on pseudomorphism, myself included, as pseudomorphism on a broad scale; and that other hydrous magnesian rocks may come under the same category; and I still so believe with regard to most beds of serpentine. But this is no ground for the assertions of Mr. Hunt above cited.

Solitary in his views on pseudomorphism and defiant of facts, he shows, by his efforts to fix absurd opinions on others, a degree of desperation in argument that is, fortunately, very uncommon. His way of carrying a point is strongly in contrast with Dr. Genth's faithful work.—J. D. D.

and red tourmalines from Paris, Me., or the beryls filled with orthoclase, or the zircon and galenite filled with calcite, and cannot be explained *rationally* as examples of *association* and *envelopment.*

To give strength to his statements, however, Dr. Hunt says that he had "*examined*" with me "*the extensive collection of specimens upon which my conclusions were based.*" When Dr. Hunt favored me with a visit, I was in hope that he *would examine* my specimens, but his time was so short that he saw only about one-third of them, and the "*examination*" (*! ?*) of these was finished in about *five minutes.*

As to his last sentence, I must confess that I am unable to discover the least parallelism between the corundum-bearing veins and the granitic veins, with beryl and tourmaline, so common in the White Mountains, and the calcareous veinstones with apatite, pyroxene, phlogopite and graphite of the Laurentian rocks;—but can see in the former nothing but the product of a partial, and in many instances of a pretty thorough alteration of the original corundum into micaceous and chloritic schists or beds, or, as Professor Dana would express it, "*a pseudomorphism on a broad scale.*"

University of Pennsylvania, July 4th, 1874.

11. *Note on the Enemies of Difflugia;* by J. Leidy. (Proc. Acad. Sci. Philad., p. 75, 1874.)—Prof. Leidy remarked that in the relationship of *Difflugia* and *Amœba* we would suppose that the former had been evolved from the latter, and that its stone house would protect it from enemies to which the *Amœba* would be most exposed. The *Difflugia* has many enemies. I have repeatedly observed an *Amœba* with a swallowed *Arcella*, but never with a *Difflugia.* Worms destroy many of the latter, and I have frequently observed them within the intestine of *Nais, Pristina, Chætogaster*, and *Æsolosoma.* I was surprised to find that *Stentor polymorphus* was also fond of *Difflugia*, and I have frequently observed this animalcule containing them. On one occasion I accidentally fixed a *Stentor* by pressing down the cover of an animalcule cage on a *Difflugia*, which it had swallowed. The *Stentor* contracted, and suddenly elongated, and repeated these movements until it had split three-fourths the length of its body through, and had torn itself loose from the fastened *Difflugia.* Nor did the *Stentor* suffer from this laceration of its body, for in the course of several hours each half became separated as a distinct individual.

12. *Remarks on the Revivification of Rotifer vulgaris;* by J. Leidy. (Ibid, p. 88).—Prof. Leidy remarked that during the search for Rhizopods, having noticed among the dirt adhering to the mosses in the crevices of our pavements many individuals of the common wheel-animalcule, *Rotifer vulgaris*, he had made some observations relating to the assertion that they might be revivified on moistening them after they had been dried up.

Two glass slides, containing beneath cover glasses some dirt, exhibited each about a dozen active living Rotifers. The glass slides were placed on a window ledge of my study, the thermometer standing at 80°. In the course of half an hour the water on the slides was dried up and the dirt collected in ridges. The next morning, about twelve hours after drying the slides, they were placed beneath the microscope. Water was applied and the materials on the slides closely examined. On each slide a number of apparently dried Rotifers were observed. These imbibed water and expanded, and some of them in the course of half an hour revived and exhibited their usual movements, but others remained motionless to the last.

The same slides were again submitted to drying, and from one of them the cover glass was removed. They were examined the next day, but several hours after moistening them only two Rotifers were noticed moving on each slide.

I next prepared a slide on which there were upward of twenty actively moving Rotifers, and exposed it to the hot sun during the afternoon. On examination of the slide the following morning, after moistening the material, all the Rotifers continued motionless, and remained so to the last moment.

From these observations it would appear that the Rotifers and their associates became inactive in comparatively dry positions and may be revived by supplying them with more moisture, but when the animals are actually dried they are incapable of being revivified. Moisture adheres tenaciously to earth, and Rotifers may rest in the earth, like the *Lepidosiren*, until returning waters restore them to activity.

13. *Notice of some new Fresh-water Rhizopods;* by J. LEIDY. (Ibid, p. 77.)—Prof. Leidy remarked that besides the ordinary species of *Amœba*, which he had observed in the vicinity of Philadelphia, he had discovered what he suspected to be a new generic form. It has all the essential characters of Amœba, but in addition is provided with tufts of tail-like appendages or rays, from which he proposed to name the genus OURAMŒBA.

The rays project from what may be regarded as the back part of the body, as the animal always moves or progresses in advance of the position of those appendages. The rays are quite different from pseudopods, or the delicate rays of the Actinophryens. They are not used in securing food, nor is their function obvious. The *Ouramœba* moves like an ordinary *Amœba*, and obtains its food in the same manner. The tail-like rays are not retractile, and they are rigid and coarse compared with those of Actinophryens. They are simple or unbranched, except at their origin, and they are cylindrical, of uniform breadth, and less uniform length. When torn from the body they are observed to originate from a common stock attached to a rounded eminence.

Several forms of the *Ouramœba* were observed, but it is uncertain whether they pertain to one or to several species. One of the forms had an oblong ovoid body about $\frac{1}{8}$th of a line long and $\frac{1}{12}$th of a line broad. The tail-like rays formed half a dozen tufts, measuring in length about the width of the body. The latter was so gorged with large diatoms, such as *Navicula viridis*, together with desmids and confervæ, that the existence of a nucleus could not be ascertained. The species may be distinguished with the name of OURAMŒBA VORAX.

A second form, perhaps of a different species, moved actively and extended its broad pseudopods like *Amœba princeps.* When first viewed beneath the microscope it appeared irregularly globular and about $\frac{1}{14}$th of a line in diameter. It elongated to the $\frac{1}{6}$th of a line, and moved with its tail-like appendages in the rear. These appendages formed five tufts about $\frac{1}{25}$th of a line long. The interior of the body exhibited a large contractile vesicle and a discoid nucleus. This second form may be distinguished with the name of OURAMŒBA LAPSA.

Another *Ouramœba* had two comparatively short tufts of rays, and a fourth, of smaller size than the others, had a single tuft of three moniliform rays.

It is possible that *Ouramœba* is the same as the *Plagiophrys* of Claparede, though the description of this does not apply to that.

Plagiophrys is said to be an Actinophryen, furnished with a bundle of rays emanating from a single point of the body, but the rays are described as of the same kind and use as those of *Actinophrys. Plagiophrys* is further stated to be provided with a distinct tegument like *Corycia* of Dujardin, or *Pamphagus* of Bailey, but the body of *Ouramœba* is as free from any investment as an ordinary *Amœba*, and the rays are fixed tail-like appendages, with no power of elongation or contraction.

The species of *Ouramœba* were found among desmids and diatoms, on the surface of the mud at the bottom of a pond, near Darby Creek, on the Philadelphia and West Chester Railroad.

Two of the commonest species of *Difflugia* of our neighborhood I had until recently confounded together as *D. proteiformis*, and, perhaps, the two forms may be included under the latter name in Europe. In one the mouth is deeply trilobed, and the animal is usually green with chlorophyl globules. In the other the mouth is crenulate, usually with six shallow crenulations, and the animal is devoid of chlorophyl. The former is usually the smaller, and may be distinguished with the name of D. LOBOSTOMA; the latter may be named D. CRENULATA.

In an old brick pond, on the grounds of Swarthmore College, Delaware County, among *Difflugia pyriformis*, *D. spiralis*, *D. corona*, *D. acuminata*, and others not yet determined, there occurs an abundance of a large species, apparently undescribed. It is sometimes the fourth of a line in length, and is compressed pyriform, but is quite variable in its relation of length to breadth, and in the shape of the fundus of the shell. This is often trilobate, but from the non-production of one or more or all the lobes, differs in appearance in different individuals. The animal is filled with chlorophyl grains, from which it might be named D. ENTOCHLORIS.

Another large *Difflugia*, allied to *D. lageniformis*, is not unfrequent about Philadelphia. The shell is beautifully vase-like in shape. It has an oval or sub-spherical body with a constricted neck, and a recurved lip to the mouth. The body of the shell opposite the mouth is acute and often acuminate. The animal contains no chlorophyl. One shell measured $\frac{1}{6}$ of a line long by $\frac{1}{8}$ of a line broad; another measured $\frac{1}{4}$ of a line long by $\frac{1}{7}$ of a line broad. The species may be named D. AMPHORA.

A Difflugian, found in a spring on Darby Creek, is interesting, from its transparency, which allows the structure of the animal to be seen in all its details. The investment is membranous and apparently structureless. The soft granular contents occupy about one-half of the investment, and are connected with this by long threads. The pseudopods are protruded in finger-like processes. The form of the animal is compressed ovoid, with the narrow pole truncate and forming the transversely oval mouth. It is probably the species *Difflugia ligata*, described by Mr. Tatem, of England. Its length is about $\frac{1}{33}$d of a line. The character of the investment is so different from that of ordinary Difflugians that the species may be regarded as pertaining to another genus, for which the name of CATHARIA would be appropriate.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Change of level in the Great Salt Lake.*—On Tuesday, July 21st, under the direction of Dr. J. R. Park, of the Deseret University, a monument or pillar was placed near the shore of Great Salt Lake at Black Rock, to indicate the rise and fall of the water.

The necessity of such an indicator has been long apparent to many of the more intelligent observers. Several years since, Prof. Henry, of the Smithsonian Institution, Washington, D. C., wrote to Mr. B. Young on the subject, and requested that a fixed monument might be placed in some convenient part of the lake, furnished with divisions in feet and inches, by which the height of the water could be periodically measured. We are not aware that any notice was taken of the Professor's communication. This request Prof. Henry renewed last March, in a communication to Dr. Park, soliciting, at the same time, some definite information as to the past and present height of the water of the lake.

In common with all others who have given the subject any consideration, the Professor regarded this isolated body of water as an immense rain gauge, indicating from year to year the changes in the mean quantity of water that falls in the region constituting the basin of which it forms the bottom. The changes are indicated both by the depth of the water and the varying extent of ground over which it is spread.

It is apparent to all persons conversant with the meteorology of the Great Basin, that rainfall is a term of comparatively trivial import in this altitude. Except in the autumn, when the waters are at low stage, no rains occur here that materially affect the streams from which Salt Lake derives its volume. The evaporation is so great that even the extraordinary succession of heavy rains that have fallen during the present month (July) has had no perceptible effect upon the waters of the lake—not even to compensate for the amount of evaporation during the month. On the contrary, such is the intensity of heat and the absence of moisture from our atmosphere, that the waters of the lake have steadily subsided, showing, from the best data obtainable, a fall of about fourteen inches, and this mostly in the present month. Admitting a total surface area of 1,300,000 square feet, this would give a ratio of evaporation of one millionth part of an inch to every square foot of surface. The season of high waters, then (when the rivers and streams are swollen by the melting of the deep snows in the mountains), is the only period at which the falling of the lake is arrested; the rise continues until an equilibrium is formed between the surface exposed and the amount of evaporation which goes on from every square foot of the surface.

It is to be regretted that there are no means of definitely ascertaining the variations of the lake since the settlement of the valley in 1847. It is generally conceded by old residents, however, that the lake is now some twelve or fourteen feet higher than then.

Until the spring of 1852 there was no perceptible permanent rise; the increase from the high waters of the spring, and the subsidence during the autumn, being about equal, and the extreme variation some fifteen inches. From the autumn of 1852 to the autumn of 1856 the snows fell heavier in the mountains, and produced a corresponding increase in the volume and extent of the lake; exhibiting, in the latter year, a rise of some six feet above the lowest stage of 1852. From 1856 to 1861 a gradual subsidence of the waters took place, the fall in 1861 reaching a mark two feet below its lowest stage prior to 1852. The year 1861 will be remembered by the farmers of Utah as a season of unprecedented drought, the crops being seriously injured from lack of water for irrigation purposes. A contraction of the lake's surface was also observable to about three fourths of its dimensions in 1852; and more than half of this area was less than five feet in depth. Captain Stansbury, in 1850–51, reports its greatest depth as fifty-six feet.

In the spring of 1862 the lake began to extend its area and continued to rise until 1868, when it had reached a point twelve feet higher than the lowest stage of 1861, with an area estimated at one and a half times that of 1861.

Since 1868, up to the present time, the rise and fall have been about equal, the lake holding its own, with a slight increase and an extreme variation of about two feet.

The data we have presented above show conclusively an irrepressible determination of the waters to rise. The mountain streams are steadily enlarging. The humidity of the atmosphere annually increases as the area of cultivation in the valleys becomes greater, and, as a consequence, the evaporation less. Tens of thousands of acres of farming, meadow and pasture lands have been submerged along the eastern and northern shores of the lake. Many square miles of valuable lands as yet available and occupied by the farmer, skirting the lake, would be completely submerged should the waters rise but a few inches above the average level of the past five years.

There is one fixed mark corroborative of the immense increase in the lake during the past decade. Prior to 1861, Black Rock was connected with the mainland by a roadway of black limestone, crushed and regularly turnpiked, as if by design. Over this road, rising in the center about two feet above the waters, the salt manufacturers of earlier days—Mr. White and others—having salt vats and flues on the shores of Black Rock, transported their saline products. That self-same roadway, yet plainly discernible, is now twelve feet under the surface of the Great Salt Lake. Essentially it is a submarine turnpike!

It is no mere conjecture that the lowland farmers along the shores of the Great Salt Lake may some day find themselves in the predicament of the demure Hollander—compelled to resist, by earthworks, the encroachments of salt water, or submit to the retiring process of inundation.—*Utah Mining Gazette*, July 25.

2. *On the Physical Cause of Ocean-Currents;* by JAMES CROLL, of the Geological Survey of Scotland.—In a lecture at the Royal Institution, and also in the Athenæum, Nature, Philosophical Magazine, and other quarters, Dr. Carpenter has been advancing a somewhat plausible-looking objection to my views in reference to under-currents. As this objection bears upon a point which in my last communication I omitted to consider, perhaps you will permit me, through your columns, briefly to refer to it. The objection in question, as stated in Dr. Carpenter's own words, is as follows:

"According to Mr. Croll's doctrine, the whole of that vast mass of water in the North Atlantic, averaging, say, 1500 fathoms in thickness and 3600 miles in breadth, the temperature of which (from 40° downward), as ascertained by the 'Challenger' soundings, clearly shows it to be mainly derived from a polar source, is nothing else than *the reflux of the Gulf-stream*. Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf-stream to be about 1-900th that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf-stream returns into the Mid-Atlantic circulation, only a branch of it going on to the northeast, the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison a mere rivulet (the northeast motion of which, as a distinct current, has not been traced eastward of 30° W. long.) seems still more obvious."

The objection seems to me to be based upon a series of misapprehensions: (1) that the mass of cold water 1500 fathoms deep and 3600 miles in breadth is in a state of motion toward the equator; (2) that it cannot be the reflux of the Gulf-stream, because its sectional area is 900 times as great as that of the Gulf-stream; (3) that the immense mass of water is, according to my views, set in motion by the Gulf-stream.

I shall consider these in their order. (1) That this immense mass of cold water came originally from the polar regions I, of course, admit, but that the whole is in a state of motion I certainly do not admit. There is no warrant whatever for any such assumption. According to Dr. Carpenter himself, the heating power of the sun does not extend to any great depth below the surface; consequently there is nothing whatever to heat this mass but the heat coming through the earth's crust. But the amount of heat derived from this source is so trifling, that an under-current from the Arctic regions, far less in volume than that of the Gulf-stream, would be quite sufficient to keep the mass at an ice-cold temperature. Taking the area of the North Atlantic between the equator and the tropic of Cancer, including also the Caribbean Sea and the Gulf of Mexico, to be 7,700,000 square miles, and the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, we find that the total quantity of heat derived from the earth's crust by the above area is equal to about 88×10^{15} foot-pounds per day. But this amount is equal to only $\frac{1}{894}$ that conveyed by the Gulf-stream, on the supposition that each pound of water carries 19,300 foot-pounds of heat. Consequently an under-current from the polar regions of not more than $\frac{1}{35}$ the volume of the Gulf-stream would suffice to keep the entire mass of water of that area within 1° of what it would be were there no heat derived from the crust of the earth; that is to say, were the water conveyed by the under-current at 32°, internal heat would not maintain the mass of the ocean in the above area at more than 33°. The entire area of the North Atlantic from the equator to the arctic circle is somewhere about 16,000,000 square miles. An under-current of less than $\frac{1}{17}$ that of the Gulf-stream coming from the Arctic regions would therefore suffice to keep the entire North Atlantic basin filled with ice-cold water. In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions either in the form of under-currents, or in the form of a general under-flow as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water; for there is no source from which this influx of water can derive heat, save from the earth's crust. But the amount thus derived is so trifling as to produce no sensible effect. For example, a polar under-current one-half the size of the Gulf-stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient under such circumstances to maintain the mass 1° Fahr. above the temperature it possessed when it left the polar regions.

(2) But suppose that this immense mass of cold water occupying the great depths of the ocean were, as Dr. Carpenter assumes it to be, in a state of constant motion toward the equator, and that its sectional area were 900 times that of the Gulf-stream, it would not therefore follow that the quantity of water passing through this large sectional area must be greater than that flowing through a sectional area of the Gulf-stream; for the quantity of water flowing through this large sectional area depends entirely on the rate of motion.

(3) I am wholly unable to understand how it could be supposed that this under-flow, according to my view, is set in motion by the Gulf-stream, seeing that I have shown that the return under-current is as much due to the impulse of the wind as the Gulf-stream itself.

I am also wholly unable to comprehend how Dr. Carpenter should imagine, because the bottom-temperature of the South Atlantic happens to be lower, and the polar water to lie nearer to the surface in this ocean than in the North Atlantic, that therefore this proves the truth of his theory. This condition of matters is just as consistent with my theory as with his. When we consider the immense quantity of warm surface-water which, as has been proved, is being constantly transferred from the South into the North Atlantic (a quantity which to a large extent is compensated by a cold current from the Antarctic regions), we readily understand how the polar water comes nearer to the surface in the former ocean than in the latter. In fact, the whole of the phenomena are just as easily explained upon the principle of under-currents as upon Dr. Carpenter's theory.

Dr. Carpenter lays considerable stress on the important fact established by the 'Challenger' expedition, that the great depths of the sea in equatorial regions are occupied by ice-cold water, while the portion heated by the sun's rays is simply a thin stratum at the surface. It seems to me that it would be difficult to find a fact more hostile to his theory than this. Were it not for this upper stratum of heated water there would be no difference between the equatorial and polar columns, and consequently nothing to produce motion. But the thinner this stratum is the less is the difference, and the less there is to produce motion. I have been favored by the Hydrographer to the Admiralty with a series of temperature-soundings taken along the equator; and from these I find that to so small a depth does the super-heating extend, that the surface of the ocean at the equator requires to stand only four and a half feet above that at the poles in order to the ocean being in perfect equilibrium. In this case, if we suppose, in order to constant circulation, that the polar column is kept in ex ess of the equatorial by the weight of, say, two feet of water, there would then remain only a slope of two and a half feet between the equator and poles.—*Phil. Mag.*, June, 1874.

3. *On the Physical Geography of, and the Distribution of Terrestrial Mollusca in, the Bahama Islands;* by THOMAS BLAND. (Ann. Lyc. Nat. Hist., N. Y., x, Nos. 10, 11, March–June, 1873.) —Mr. Bland, whose explorations in West India conchology have made him thoroughly acquainted with the geographical distribution of the species, has brought forward in this paper (which, but for a pressure of original communications, would have been before this cited entire in this Journal) many facts of great interest on the relations of the Bahamas to the adjoining islands. The number of land shells known to exist in the Bahamas is 80, 60 of them inoperculates. He shows that the alliance of the Great Bank with Cuba is very close, as is apparent in the numerous representatives of *Polymita* and *Strophia*, the occurrence of *Polygyra*, *Thelidomus*, and *Melaniella* (three groups unknown in Haiti), and other facts; "while the development of *Plagioptycha* in the Turk Islands and Great Inagua, with the fact that *P. Albertsiana* and *P. disculus* are common to them and Haiti, appears to indicate their connection with the latter island."

After alluding to some observations in Dana's Corals and Coral Islands on the diminished size of the islands to the eastward, and the evidence of subsidence found in the fact, Mr. Bland continues his paper as follows:

"The facts regarding the diminution in size of the islands of the West Indies to the eastward, are of peculiar interest, not only as affording conclusive evidence of the greater subsidence in that direction, but in connection with geographical distribution.

"The banks and islands forming the long Bahama chain diminish in size to the southeast, where are situated at its termination the submerged Mouchoir Carré, Silver and Navidad Banks. In a similar manner, the submerged Virgin Island Bank (with Anegada on its northeastern extremity, geologically, in the opinion of Dr. Cleve, resembling the Bahamas), Sombrero and the Anguilla Bank, terminate the chain of the West Indies (parallel with the Bahamas) eastward from Cuba.

"In the caves of Anguilla, the remains of large extinct mammalia are found, which must have inhabited a far more extensive area, subsequently broken up by subsidence.

"Packard (Amer. Nat., 1872) remarks, 'There is every probability that the separation of these islands (of the eastern part of the West Indies) took place at a late period of time, and probably subsequent to the spread of the Post-pliocene fauna over North America.' *

"Dr. Cleve (*l. c.*) observes that 'the Bahama Islands, the Island of Anegada, and a part of Barbuda, belong to a very recent period.' †

"The same author (*l. c.* 18), referring to the 'Leeward Islands,' states as follows:

* See also Cope, Proc. Acad. Nat. Sci. Philad., 1868, and Bland, Proc. Amer. Phil. Soc., 1871.

† *Helicina convexa* is common to Bermuda and Barbuda.

'The islands north of Guadaloupe form two parallel chains, from northwest to southeast. The western chain commences with *Saba*, and consists of *St. Eustatius*, *St. Kitts*, *Nevis*, *Redonda*, and *Montserrat*. All of those islands are volcanos; and, if the line were extended farther to the north, it would reach the island of Anegada, of Post-pliocene date, and all the volcanos seem to be of the same or nearly the same geological time. The Bahama Islands, which are also most probably of Post-pliocene date, have the same direction, and seem to be the continuation of the same or of a parallel line of elevation. East of the volcanic range is another completely different range of islands. They are not volcanic, and commence with *Sombrero*, comprising *Anguilla*, *St. Martin*, *St. Bartholomew*, *Barbuda* and *Antigua*. All of these islands are of the Tertiary age, *Eocene*, *Miocene*, and *Pliocene*.'

"In his 'Summary of the Geology of the West Indies' (*l. c.* 47), Dr. Cleve says:

'From the facts exposed above, it may consequently be inferred that, of the two prevailing lines of elevation in the West Indies, the one running from west to east originated before the Miocene time, and that the other from northwest to southeast, commencing with the Bahamas and continuing in the same direction down to Trinidad, was formed after the Miocene time.'

"While considering the facts and geological grouping of the islands quoted above from Dr. Cleve's paper, it should be remembered that the land-shell fauna of Saba, of St. Eustatius, St. Kitts, and Nevis (all three on one bank), and of Redonda and Montserrat, and of Barbuda and Antigua (the last two on the same bank) is, in common with most of the islands to the south, to and inclusive of Trinidad, distinct from the fauna of the islands between and inclusive of the Bahamas and Cuba, and the Anguilla Bank, on which are Anguilla, St. Martin, and St. Bartholomew.*

"This difference of the faunas, and the well-defined line of their separation, must be considered in connection with the past and present geological history of the islands.

"The distribution of the species of the genera *Macroceramus* and *Strophia* illustrates in a marked manner the distinctness of the two faunas just mentioned. *Macroceramus* has two species in the Bahamas (one common to the Great Bank, Florida and Cuba, *M. Gossei*, being the only species found in Jamaica); 36 in Cuba, and 10 in Haiti, of which 1 (*M. Gundlachi*) occurs in both.

"There are two other species only in the islands between and inclusive of Porto Rico and those of the Anguilla Bank, *M. signatus*, which, besides Haiti, is found in Tortola, Necker Island, and Anegada, all on the Virgin Bank, and in Anguilla and St. Bartholomew on the Anguilla Bank; *M. microdon* occurs in Porto Rico, Vieque, St. Thomas, Tortola, and Anegada. The genus is not represented in St. Croix, and not in any of the islands south of the Anguilla Bank.

"*Strophia* has 16–18 species in the Bahamas, of which 1 is also in the Florida Keys, and at least 6 in Cuba; 17 in Cuba; none in Jamaica; 2 in Haiti, of which one, *S. striatella*, occurs in Cuba, Porto Rico, Necker Island, and Anegada, and the other, *S. microstoma*, is found also in Cuba, Haiti, and Porto Rico (*fide* Pfr.). Remains of a fossil species, undeterminable, are noticed in Som-

* See Bland, Proc. Amer. Phil. Soc., *l. c.*

brero, and a fossil species in St. Croix. There is no representative of the genus on the Anguilla Bank or to the south of it.

"The exceptions are curious. *Macroceramus Gossei* and *Strophia uva* are found in Curaçao!

"Dana, as already quoted, refers to parallel bands of greater and less subsidence in the Pacific Ocean, and to analogous conditions in the Atlantic;—the subsidence was probably, he says, 'much greater between Florida and Cuba than in the Peninsula of Florida itself; and greater along the Caribbean Sea parallel with Cuba, as well as along the Bahama reefs, than in Cuba.' Recent soundings show in these respects the following facts:

"The greatest depth in the Gulf of Florida, between Key West and Havana, is within five miles of the latter, 800 fathoms (4,800 feet), and I have already stated that there is a depth in the Nicholas Channel, between Salt Key Bank and Cuba, of 534 fathoms (3,204 feet).

"Between Cuba and the east end of Jamaica the depth is 1,244 fathoms (7,464 feet). Eastward of Jamaica, along the southern side of Haiti, in about the latitude of Beata Island, great depths have been ascertained,—one sounding west of that island gave 2,136 fathoms (12,816 feet), and one to the eastward of it 1,840 fathoms (11,040 feet). The greater subsidence still further to the east, between the Virgin Bank and St. Croix, may be inferred from the enormous depth there found, of no less than 2,580 fathoms (15,480 feet).

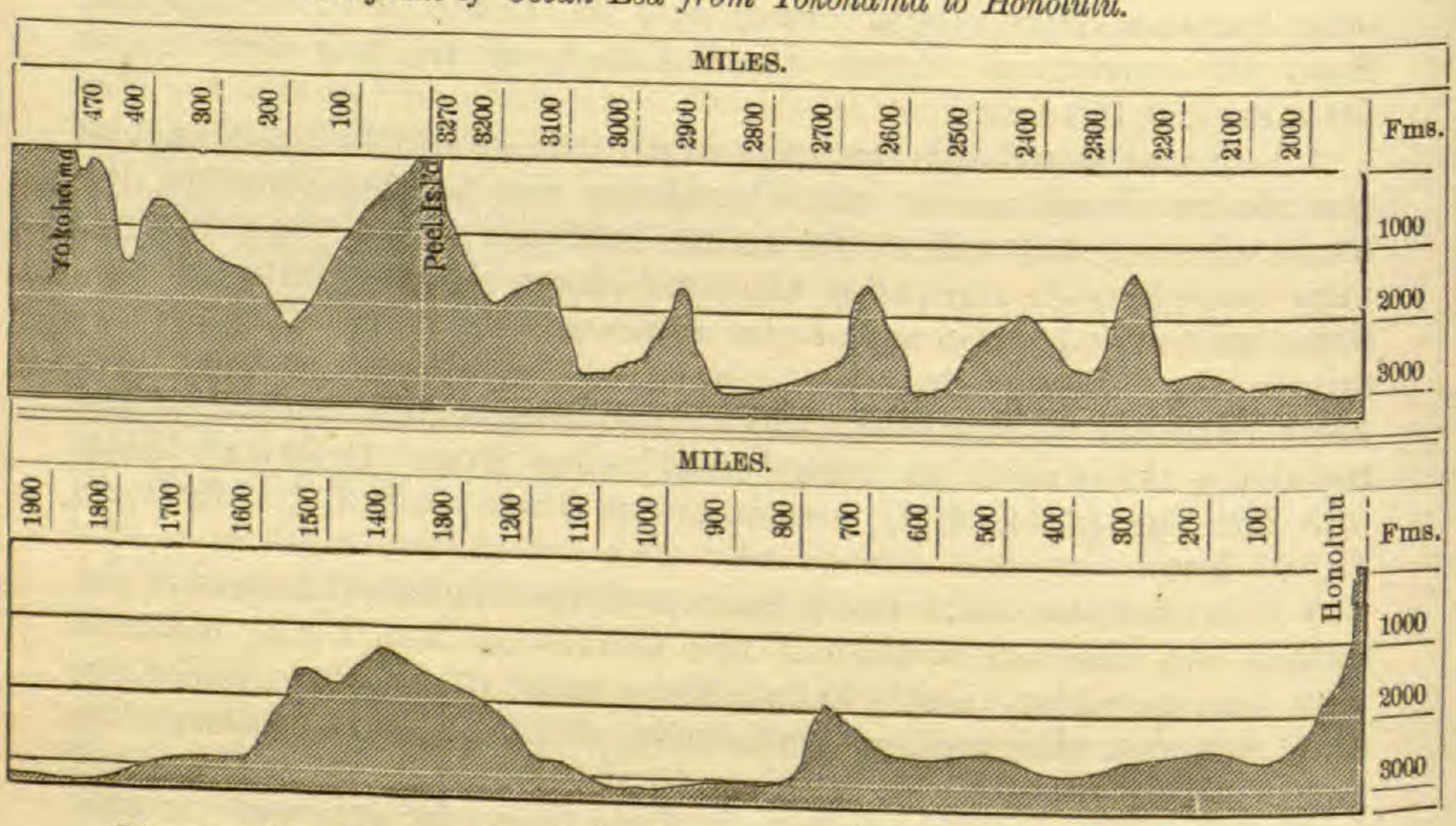
"A line of soundings from the south side of Jamaica and east of the Pedro Bank, across the Carribean Sea to Aspinwall (a distance of about 550 miles), shows the instructive fact that, with no very considerable exception, the sea bottom slopes gradually from Jamaica toward the coast of the Isthmus of Panama. About 60 miles from Manzanilla Point (N. E. of Aspinwall), the depth is 1,215 fathoms (7,290 feet). The bottom then rises comparatively rapidly,—the depth at about forty miles from Aspinwall being 677 fathoms (4,062 feet), and at about twenty miles, 227 fathoms (1,362 feet).

"In connection with the relations of the land-shell faunas of the islands on the north side of the Carribean Sea, I may mention that the greatest depth between the coast of Yucatan and Cape San Antonio, the western extremity of Cuba, about midway between the two, is 1,164 fathoms (6,984 feet): between the east end of Jamaica and the west end of Haiti (so far as is yet known), 600 fathoms (3,600 feet); and north of Mona Island, in the Mona Passage (between Haiti and Porto Rico), 250 fathoms (1,500 feet). I postpone comparison of the faunas of the islands and the adjacent parts of the North American continent; but, in regard to the depth between Haiti and Jamaica on the west side, and Porto Rico on the east, it is noticeable that, while the fauna of Haiti has very little relation with that of Jamaica, it has much alliance with that of Porto Rico."

4. *On the Ocean's bed between Honolulu and Yokohama, from soundings on board the United States ship Tuscarora.*—Sixty casts were taken at intervals averaging about 57 miles. The water fell rapidly and steadily from Honolulu until lat. 21° N., long. 159° 20′ W. was reached (a distance of about 95 miles), to 2,418 fathoms' depth, making a slope of nearly 162 feet to the mile. From that point to lat. 20° 16′ N., long. 168° W., in a distance of 530 miles, there was a slight gradual slope of only four feet to the mile. Between the last named point and lat. 20° 52′ N., long. 172° 39′ W., a distance of 185 miles, there is a submarine mountain, with its summit in about lat. 20° 41′ N., long. 171° 33′ W. Its height is 5,160 feet. It has a slope of 40 feet to the mile on its eastern side, and 128 feet on its western.

From the last station mentioned, where there was 3,045 fathoms of water, the bottom was regular for 240 miles, until lat. 21° 29′ N., long. 178° 15′ W. was reached. Between this station and lat. 22° 1′ N., long. 173° 43′ E., the second submarine mountain was passed, with its summit in lat. 21° 41′ N., long. 176° 54′ E.; its eastern slope averages 37 feet to the mile for about 127 miles from its base, and 51 feet the rest of the distance to the summit. Its western slope is 55 feet; its height is about 12,000 feet.

Diagram of Ocean Bed from Yokohama to Honolulu.



From this last station the plateau can be regarded as level, for over 470 miles, until lat. 23° 31′ N., long. 161° 57′ E. was reached. Between this point and lat. 24° 07′ N., long. 160° 09′ E., the third submarine ridge or mountain was discovered, with its summit in lat. 23° 45′ N., long. 160° 56′ E. Its height is 9,600 feet. Admitting the slope of this mountain to be regular from its base to its summit (which is of course the minimum slope possible), it will have an eastern slope of 192 feet to the mile, and a slope on its western side of 204 feet. Between the last position and lat. 23°

56′ N., long. 156° 10′ E., was the fourth elevation, having its summit in lat. 23° 55′ N., long. 158° 07′ E. From its summit, for a distance of about 45 miles to the westward, there was but a slight inappreciable fall, and from that point to the base a slope of 90 feet to the mile; its eastern slope was 60 feet. Its height is 6,000 feet. For the next 60 miles, the bottom was regular till lat. 24° 02′ N., long. 155° 08′ was reached, between which and lat. 20° 25′ N., long. 153° 01′ was a fifth mountain, extending to the surface in an island known as Marcus Island. A cast was taken in lat. 24° 20′ N., long. 154° 06′ E., a distance of about seven miles from this island, to the northward: 1,500 fathoms of water were found, which gives the land a northern slope to this point of 1,284 feet to the mile. From this point to the eastern base, the slope was about 200 feet to the mile, and to the western, 157 feet.

For 176 miles from the last position, the bottom was, comparatively speaking, regular; then the next 180 miles was occupied by the sixth mountain ridge, its eastern base in lat. 25° 11′ N., long. 149° 46′ E.; its western in lat. 26° 09′ N., long. 146° 10′ E., and its summit in lat. 25° 42′ N., long. 148° 39′ E. Its height was about 7,800 feet; its eastern slope, 163 feet to the mile, and its western 59 feet. From this point to Port Lloyd, a distance of 210 miles, the slope was about 86 feet to the mile. I have taken, in consideration of the height and slope of the submarine elevation, about 3,000 fathoms of water as the depth of the plateau proper; for it was only at that distance that it remained regular for any distance, and again the average depth between the bases of the mountains or mountain ridges, and for over half the entire distance of the line, was about that. All the slopes computed are the minimum.—*N. Y. Tribune*, June 13.

5. *Meeting of the American Association at Hartford.*—The twenty-third meeting of the American Association, at Hartford, commenced on Wednesday, the 12th of August, and closed on the following Wednesday. Dr. John L. LeConte, of Philadelphia, was the President of the meeting, and Professor C. S. Lyman, of New Haven, the Vice-President. The attendance was large, the papers numerous and for the most part excellent, and the interest in the sessions was sustained to the end, although the meeting was prolonged one day beyond the usual time. The citizens of Hartford contributed very largely to the pleasure and scientific profit of the occasion, through the many excursions for which they had made liberal arrangements. These included a trip by steamboat down the Connecticut to its mouth; and others—to the Rocky Hill quarry, where the trap and its junction with the sandstone is well exposed; the silk manufactory at Cheneyville; Tarriffville and its gorge through the trap and sandstone; the Portland quarries, opposite Middletown, where, in the bottom of one quarry, *twenty-one consecutive tracks of the huge Otozoum Moodii, along a line over fifty feet long*, were exposed, having been recently opened to view; and on Thursday, the day after the close of the session, by rail, to Salisbury, a region of extensive limonite exca-

vations, 62 miles west of Hartford. The address of the retiring President, Professor Lovering, of Cambridge, was delivered on Friday evening; it was an admirable review of the recent progress of physical science. The next meeting will be held at Detroit, Michigan, during the week commencing with Wednesday, the 11th of August. J. E. Hilgard, of the Coast Survey, was appointed President of the meeting.

The following is a list of the papers accepted for reading:

Section A.

Differential Measurements of Solar Temperature; S. P. LANGLEY.

Velocity of Primitive Undulations; P. E. CHASE.

The Distribution of the Poles of certain Nebulæ; C. ABBE.

Exhibition of a Combined Collimator and Personal Equation Machine, with a Notice of the Results obtained with it; W. A. ROGERS.

Sudden Fluctuations of Levels in quiet waters:—Records of Observations; C. WHITTLESEY.

Relation between the Barometric Gradient and the Velocity of the Wind; W. FERREL.

New way of illustrating the vibrations of organ-pipes; J LOVERING.

On a rotating, terrestrial Planisphere; S. D. TILLMAN.

The Phantascope or Zoötrope as a means of illustrating crystallography to a class; R. H. RICHARDS.

Notice of Prof. A. K. Eaton's new compound one-prism Spectroscope; R. E. ROGERS.

On the number and distribution of the brighter fixed stars; B. A. GOULD.

On the proper motion of Eta-Draconis in Right Ascension; W. A. ROGERS.

On the Harvard College Observatory System of Communicating Time for Civil Purposes; W. A. ROGERS.

Description of a new Mechanism for printing hourly the Directions and Velocity of the Winds; G. W. HOUGH.

On a method of transmitting Time Signals over Telegraph wires; G. W. HOUGH.

On the Influence that varying Atmospheric conditions have upon the perception of Sounds, especially those of the human ear, with experiments; L. TURNBULL.

A new demonstration of an old theorem in Geology; B. S. HEDRICK.

On a simple form of Sphygmograph; A. A. BRENEMAN.

On an improved Electric Bell; A. A. BRENEMAN.

Simple Formulas for some of the Higher Curves; J. D. WARNER.

On the Mineral Warwickite; J. L. SMITH.

On Metallic Iron in Basaltic Rock and the dissociation of Oxide of Iron; J. L. SMITH.

Curious Association of Datolite, Garnet, and Idocrase; J. L. SMITH.

A Convenient Method of Making Absolute Alcohol; J. L. SMITH.

Exhibition of a new Sulphuretted Hydrogen Apparatus; H. CARMICHAEL.

A new Method for the Illustration of Sound Waves; H. CARMICHAEL.

Exhibition of a Platinum Digestor; H. CARMICHAEL.

On the Phosphorescence of Glass, produced by electricity; A. W. WRIGHT.

On the use of Natural Twin Crystals of Quartz in the construction of Polariscopes; A. W. WRIGHT.

On the nature of the Zodiacal Light, and the distribution of the matter which occasions it; A. W. WRIGHT.

On the Tails of Comets; H. M. PARKHURST.

Loss of Light in its transmission through Space; H. M. PARKHURST.

On the Pressure produced by Impact; J. J. SKINNER.

Some Improvement on Descartes' Barometer; B. S. HEDRICK.

On Abnormal Phenomena of Sound; J. HENRY.

On the Estimation of Nitrogen by the absolute method; S. W. JOHNSON.

Best methods of making pure Carbonate of Soda and Carbonate of Potash for analysis; J. L. SMITH.

Report on a remarkable compound of Phosphorus and Carbon, forming a new Pyrophorus; P. H. VAN DER WEYDE.

On the Estimation of Nitric Acid by the methods of Thorpe and Bunsen; S. W. JOHNSON.

Investigation into the chemical nature of Petroleum; P. H. VAN DER WEYDE.

On the alleged formation of Ammonium Nitrite from Water-Vapor and Nitrogen, and on Price's Test; S. W. JOHNSON.

The change of Peat into Cannel or other Coal by Heat and Pressure, practically demonstrated; P. H. VAN DER WEYDE.

On the Thermo-electrical Properties of some Minerals and their Varieties; A. SCHRAUF and E. S. DANA.

The Nitrogen of the Soil; H. P. ARMSBY.

On the Periodicity of the Rainfall in the United States in relation to the Periodicity of the Solar Spots; J. BROCKLESBY.

An account of a Remarkable Phenomenon connected with the quarrying of marble in Rutland, Vermont; J. BROCKLESBY.

On the molecular volume of water of Crystallization; F. W. CLARKE.

On the molecular heat of similar compounds; F. W. CLARKE.

Action of Mechanical Vibration in retarding Chemical Combination; S. S. HALDEMAN.

Sewage Question chemically considered; T. S. HUNT.

On Wet Processes of Copper Extraction; T. S. HUNT.

On the Cement of Ransome's new Artificial Stone; T. S. HUNT.

On Lithium Glass; C. B. DUDLEY.

On the use of a glazed wrought iron tube for Nitrogen Determinations; A. P. S. STUART.

The Chemistry of Steel; B. S. HEDRICK.

Outlines of the Typical Photo-lithographic Processes; J. W. OSBORN.

On a Modification of Loewenthal's Method for the Estimation of Tannic Acid; W. MCMURTRIE.

The Inner Satellites of Uranus; E. S. HOLDEN.

On a new form of Steam Generator, applicable especially to very small powers; W. P. TROWBRIDGE.

Annual Mortality of Officers of the U. S. Army for fifty years, from 1824 to 1873, including deaths in war; B. ALVORD.

Mutual Action of Elements of Electric Currents; E. B. ELLIOTT.

Periodicity of Rates of Interest from observations for the 25 years 1849 to 1874; E. B. ELLIOTT.

Population of the United States, each year, from 1780 to 1880, with proof of estimate and interpolation; E. B. ELLIOTT.

Credit of the United States Government; E. B. ELLIOTT.

Expenditures of the United States Government per capita to population, by four year's periods from the organization of the Government, to the 30th of June, 1874; E. B. ELLIOTT.

Section B.

On the Cave Fauna of the Middle States; A. S. PACKARD, Jr.

Change by Gradual Modification not the Universal Law; T. MEEHAN.

On the Insects more particularly associated with *Sarracenia variolaris* (Spotted Trumpet leaf); C. V. RILEY.

On the Summer Dormancy of the Larvæ of *Phycoides nycteis* Doubleday, with Remarks on the Natural History of the Species; C. V. RILEY.

On the Habits and Transformations of *Canthon hudsonias* (Forst.)—the common "Tumble-dung;" C. V. RILEY.

On the Larval Habits of the Cantharid genera *Epicauta* and *Henous*; C. V. RILEY.

On the Origin of North American Unionidæ; E. S. MORSE.

On the Relations of Dentalium; E. S. MORSE.

On the ascending process of the Astragalus in Birds; E. S. MORSE.

The Lobster; W. W. WHEILDON.

Glacial Phenomena in the Sierra Nevada; J. MUIR.
Note on the gestation of the little Brown Bat; B. G. WILDER.
Botanical Observations; W. H. SEAMAN.
On an Organ of Special Sense in the Lamellibranchiate genus Yoldia; W. A. BROOKS.
The Wings of Pterodactyls; O. C. MARSH.
Small Size of the Brain in Tertiary Mammals; O. C. MARSH.
On the Male and Female Organs of the Sharks, with special reference to the use of the "Claspers;" F. W. PUTNAM & S. W. GARMON.
The Genera of Butterflies studied Historically; S. H. SCUDDER.
The Recency of certain Volcanoes of the Western U. S.; G. K. GILBERT.
Notes on Natural Erosion by Sand in the Western Territories; G. K. GILBERT.
The advantages of the Colorado Plateau Region as a field for geological studies; G. K. GILBERT.
On the Disintegration of Rocks and its Geological Significance; T. S. HUNT.
Growth of Crystallization in traps and slags; H. CARMICHAEL.
Some remarks on certain German trap rocks; H. CARMICHAEL.
On *Sarracenia variolaris* as a Fly Catcher; J. H. MELLICHAMP.
Darlingtonia Californica an insectivorous Plant; W. M. CANBY.
On Regeneration or Organic Molecular Conservation: a contribution to the doctrine of evolution; L. ELSBERG.
On the Cotton Worm (*Aletia argillacea* Hübn.); A. R. GROTE.
Physical History of New Hampshire; C. H. HITCHCOCK.
Geological Map of the United States and Territories, with Critical and Explanatory Descriptions; Prof. C. H. HITCHCOCK and W. P. BLAKE.
Discovery of twelve skeletons of *Dicotyles compressus* in the Valley Drift in Columbus, Ohio; J. H. KLIPPART.
On the organic Change produced in the Bee by the different conditions to which it is subjected in its larval state; Mrs. S. B. HERRICK.
A list of the Vertebrate Animals of Outagamie Co., Wisconsin, with notes, etc.; D. S. JORDAN.
Notes on some rare and interesting Carices of New York; G. VASEY.
Are there plants the seeds of which before germination lie in the ground a definite number of years; J. HYATT.
Notice of a pair of Trap-door Spiders from South Carolina; C. R. DODGE.
How do young Birds peck out of the shell; J. W. P. JENKS
An Inquiry concerning the Reversion of Thoroughbred Animals; W. H. BREWER.
On the present Distribution of Woodlands within the United States; W. H. BREWER.
Notes on Tree Growth; A. GRAY.
On the Classification of the Indian Languages of Mexico; P. C. BLISS.
Remarks on the Anderson School of Natural History on Penikese Island; F. W. PUTNAM.
An instance of Replacement of Injurious Insects by Human Agency; J. L. LE CONTE.
On the Equivalency of the Coal-measures of the United States and Europe; C. A. WHITE.
On some localities of contact of Trap and Sandstone in the Connecticut Valley; W. N. RICE.
Further observations on the Geology of Northwestern Massachusetts, with special reference to the Hoosac range; S. TENNEY.
On the composition of the Pottery of the Mound-builders; E. T. COX.
On the true character of the so-called Eozoon Canadense; L. S. BURBANK.
On the Mechanical condition of the Pebbles in the Newport Conglomerate and their supposed flattening by pressure; W. B. ROGERS.
On the thickness of the Virginia Tertiary, as indicated by the Artesian borings at Fortress Monroe; W. B. ROGERS.
Notes on the Paleozoic Formations of South America; O. A. DERBY.
On the Trap rocks of the Connecticut Valley; E. S. DANA.

The Physical and Geological Characteristics of the Great Dismal Swamp and the Eastern Counties of Virginia; N. B. WEBSTER.

Origin of the Cascades of the Columbia River, Oregon; W. P. BLAKE.

Observations in a visit to the Cave of Cacahuamilpa, in Mexico; P. C. BLISS.

An ascent of the Volcano of Popocatapetl in Mexico; P. C. BLISS.

Observations on the Mesozoic of North Carolina; W. C. KERR.

On the Significance of Classes among Vertebrates; T. GILL.

On the Relations of certain Genera of Cervidæ; T. GILL.

Progress of Science in Maryland; Mrs. A. L. PHELPS.

Cremation among North American Indians; J. L. LECONTE.

A Remarkable Ancient Stone Fortification in Clark County, Ind.; E. T. COX.

Remains of an Ancient Earth Work in Marblehead, Massachusetts; J. J. H. GREGORY.

6. *Chemical Centennial.*—The first day of August, 1774, illustrious as the date of Joseph Priestley's discovery of oxygen, was commemorated on the first of August, 1874, by the gathering of American chemists at Priestley's grave, on the banks of the Susquehanna, at Northumberland, Pennsylvania, and by a like gathering of British chemists at Birmingham, England, for the unveiling of a statue to Priestley. Both gatherings appear to have been prosperous, and the interchange of salutations by ocean cable placed the two assemblies in sympathy on an occasion well calculated to awaken kindred sentiments. The American gathering was presided over by Professor Charles F. Chandler of the Columbia College School of Mines, who responded to an address of welcome from the citizens of Northumberland, delivered by Col. David Taggart of that place.

The following addresses were also delivered on the occasion, by previous appointment:—

A Sketch of the Life and Labors of Dr. Joseph Priestley, by Professor Henry H. Croft, of Toronto, Canada.

A Review of the Century's Progress in Theoretical Chemistry, by Professor T. Sterry Hunt, of Boston, Massachusetts.

A Review of the Century's Progress in Industrial Chemistry, by Professor J. Lawrence Smith, of Louisville, Kentucky.

An Essay on American Contributions to Chemistry, by Professor Benjamin Silliman, of New Haven, Connecticut.

Professor Joseph Henry of Washington, who had been expected to preside at the meeting, and to deliver an address at Priestley's grave, was unfortunately detained by a sudden illness, and in his absence the assembly, gathered at sunset at the grave of Priestley, on Friday evening, July 31st, was addressed in a most appropriate manner, by President Henry Coppée, of Lehigh University.

Many descendents of Dr. Priestley reside at Northumberland. Dr. Joseph Priestley was the chairman of the local Committee of Arrangements; and another of the same degree of consanguinity, the third generation, was among those whose hospitalities added to the pleasures of the occasion. The old Priestley mansion, with its ample apartments, was thrown open to the guests, and in one of them was a collection of apparatus and other personal memorials of the eminent man whose name consecrated the day and the place.

The telegraphic dispatches above alluded to were as follows:

The American Chemists assembled at Northumberland, Penn.: Our marble statue representing Priestley discovering oxygen will be unvailed to-morrow, presented by the subscribers through Professor Huxley to the town, and accepted by the Mayor. We greet you as colleagues in honoring the memory of a great and good man.

THE PRIESTLEY MEMORIAL COMMITTEE,
Birmingham, Eng.

NORTHUMBERLAND, Penn., July 31, 1874.

The brother chemists at the grave to their brothers at the home of Priestley send greeting, on this centennial anniversary of the birth of chemistry.

We understand that the addresses will appear in a Memorial number of the American Chemist, soon to be published, which will contain also a full account of the proceedings. B. S.

7. *Elements of Metallurgy*, &c.; by J. ARTHUR PHILLIPS, M.Ins.C.E., F.G.S. 764 pp. 8vo, with 205 illustrations on wood. London, 1874. (Chas. Griffin & Company.)—This new edition of Mr. Phillips's well known "Manual of Metallurgy" (1852, 1854 and 1858) bears date June, 1874. It is in fact a new book. About one-half of the volume (354 pages) is given to the subjects of Fuel (106 pages), and Iron and Steel (248 pages). It is remarkable for its clear, concise and comprehensive style, by which qualities a vast amount of valuable and accurate information is brought within a moderate compass. The author's own practical skill and experience, both in metallurgical operations and in personal explorations of many of the metalliferous regions and workings described, add to the value of his work. It is especially interesting to American readers from its frequent citations of American examples. Some omissions we note: for example, under the "Wet Processes for Copper," no mention is made of the Hunt and Douglass process, depending on the solution of oxide of copper in a bath of sodic chloride and ferrous chloride forming soluble chloride and dichloride of copper, from which metallic iron throws down the metallic copper, regenerating the ferrous chloride which had been consumed in chloridizing the copper oxide and thus renewing the bath for a fresh attack. This very ingenious and original process is now in successful use here on a large scale and is certainly worthy of mention in so good a book as Mr. Phillips's Metallurgy. B. S.

8. *Volume of Collected Researches of J. Lawrence Smith*, 1874.—A letter received by us from Dr. Smith states that the apparent injustice to Professor Brush, referred to in the August number of this Journal, page 144 of this volume, was wholly unintentional on his part, and desires us to request that those having the work should change the wording of the note (p. 109) by substituting "associated with" for "assisted by."—EDS.

On the Marine Mammals of the North Pacific, by Capt. C. M. Scammon, U. S. Revenue Service. 4to, with many plates. San Francisco, 1874. (John H. Carmany & Co.)

My Visit to the Sun; or Critical Essays on Physics, Metaphysics, and Ethics. By Lawrence Benson. Vol. I. Physics. 158 pp. 8vo. New York, 1874. (James S. Burton.)

THE

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JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XXI.—*Researches in Acoustics;* by ALFRED M. MAYER. Paper No. 6,* containing:

1. The Determination of the Law connecting the Pitch of a Sound with the Duration of its Residual Sensation.
2. The Determination of the numbers of Beats, throughout the musical scale, which produce the greatest dissonances.
3. Application of these Laws (1) and (2) in a New Method of Sonorous Analysis, by means of a perforated rotating disc.
4. Deductions from these Laws leading to new facts in the Physiology of Audition.
5. Quantitative applications of these Laws to the fundamental facts of Musical Harmony.

„Consonanz ist eine continuirliche Dissonanz, eine intermittirende Tonempfindung."—*Helmholtz.*

1. *The Determination of the Law connecting the Pitch of a Sound with the Duration of its Residual Sensation.*

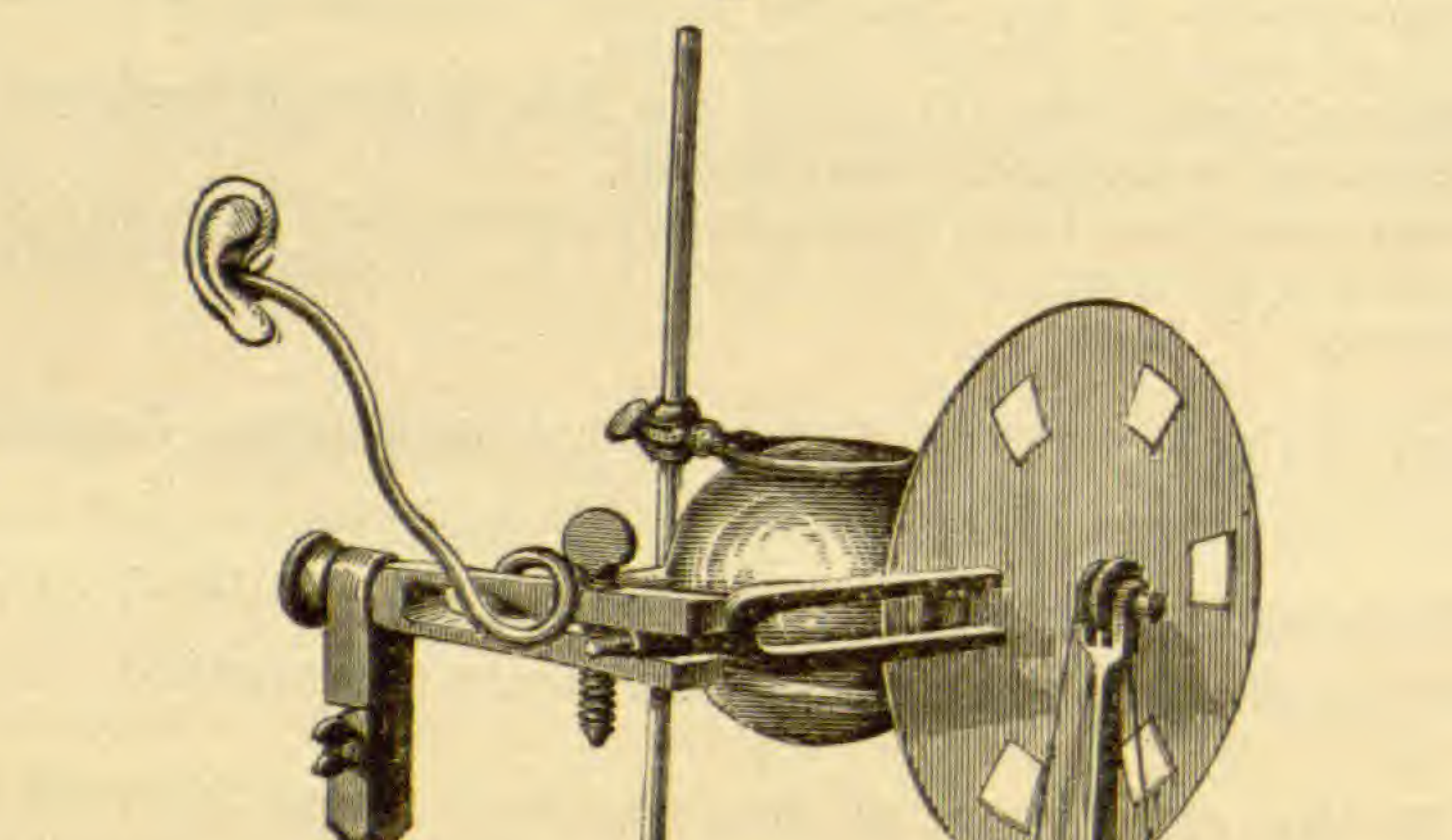
WHILE the durations of the residual sensations on the eye, corresponding to lights of various colors and intensities, have been the subjects of many masterly memoirs, I know of no attempts to determine the durations of the residual sonorous sensations. Helmholtz founds indeed his Physiological Theory of Music on the facts that a certain number of beats per second produce in the ear a maximum dissonant sensation, while a greater number may blend into a smooth continuous sound; and in discussing the position in his scale of the

* Read before the New York Opthalmological Society on March 9, 1874; and before the National Academy of Sciences, in Washington, on April 23, 1874.

"damping powers" of the co-vibrating parts of the organ of Corti, Helmholtz (Tonempf., p. 212 et seq.) infers, from the difficulty of trilling on the bass notes, that the co-vibrating parts of the ear, set in motion by sounds of low pitch, maintain their vibrations longer than those excited by sounds belonging to higher portions of the musical scale. He says: "Trills of this kind, of ten notes per second, are of a sharp and clear execution in the greatest portion of the musical scale; below the *la* of 110 vibrations, in the grand and contra octaves, however, they sound bad, harsh, and the sounds begin to blend." Yet it does not appear that Helmholtz ever attempted to determine that *quantitative relation* existing between the pitch of a sound and the duration of its residual sensation, which I will now endeavor to establish. This law in its further applications will render quantitative many of the qualitative statements contained in Helmholtz's renowned work.

The method of obtaining the facts—of which our law expresses their general relation—was similar to the method used in the study of the analogical phenomena of light. A simple sound was obtained by vibrating a fork before the mouth of its corresponding resonator, and this sound was broken up into flashes, or explosions, by alternately screening and unscreening

1.



the mouth of the resonator, by means of a perforated disc, which rotated between the resonator and the fork; as is shown in the accompanying figure (1). The mean diameter of the open sectors of the disc equalled the diameter of the mouth of the resonator, while the spaces of card-board between the open sectors was twice the width of these openings. Thus the resonator's mouth was exposed to the vibrations during an interval which equalled that during which it was screened from them. A rubber tube led from the nipple of the resonator to one ear, while the other ear was tightly closed with a lump of bees-wax.

In my first experiment, I firmly clamped an UT_2 resonator, and vibrated opposite its mouth an UT_2 fork. I now placed the tube in the ear, and on slowly rotating the disc I perceived a series of sharply separated explosions or beats; on gradually increasing the velocity of the disc these explosions gradually approached each other, and on reaching a certain frequency in their succession they blended into a continuous, smooth sensation, similar to that experienced when the disc was removed and the fork vibrated gently before the resonator. I now kept the disc at the velocity required just to blend the separate beats, and I found, on timing its rotations, that the resonator was sending into my ear 26 explosions or beats per second. Hence, sonorous waves of UT_2 cut into 26 parts per second, or, in other words, divided into lengths of five waves separated by the same lengths of quiescence, produce the same sensation as that caused by an uninterrupted flow of these sonorous waves into the ear. I now replaced the UT_2 resonator and fork by an UT_4 resonator and its corresponding fork and again rotated the disc with the same velocity that it had during the above described experiment. In these circumstances I no longer experienced a continuous sensation, but one which reminded me somewhat of the clatter of frogs in a marsh. This fact at once showed that a greater number of beats per second were required to blend the separated pulses of a sound of higher pitch; and this blending I actually obtained on sending into my ear about eighty beats per second of UT_4.

I now prepared a series of discs adapted to four octaves of resonators and forks, and with them I made the following determinations. I was not able to use an UT_1 fork and resonator, so I substituted for the former an UT_1 closed organ pipe, gently blown, and for the latter a small funnel of gutta-percha, whose mouth was placed close to the disc while a rubber tube connected the funnel with the ear. I will here remark that in some series of experiments all of the resonators were replaced by this funnel of gutta-percha and that the results were the same as those obtained by the use of the resonators. I now obtained the aid of a friend who has a trained musician's ear, and he and I arrived at the following results,—the mean of our separate determinations. He always required a greater number of beats per second to obtain the continuous sensation.

After the experiments were finished I found that his determinations were about 5 per cent higher than mine.

Column S of the following table contains the simple sounds experimented on; they are designated by the notation stamped by König on his forks. Column N gives the number of vibrations* per second corresponding to the sounds of column S. In column D are the corresponding durations of the residual sensations, expressed in vulgar and in decimal fractions. The reciprocal of the number of beats per second required to produce a continuous sensation by a given sound is taken as the duration of the residual sensation of this sound.† In column L are given the number of wave-lengths contained in the separate impulses into which the sound had been divided in order to produce the continuous sensation.

S	N	D	L
UT_1	64	$\frac{1}{16}$=·0625 *sec.*	4·0
UT_2	128	$\frac{1}{26}$=·0384 "	4·9
UT_3	256	$\frac{1}{47}$=·0212 "	5·4
SOL_3	384	$\frac{1}{60}$=·0166 "	6·4
UT_4	512	$\frac{1}{78}$=·0128 "	6·5
MI_4	640	$\frac{1}{90}$=·0111 "	7·1
SOL_4	768	$\frac{1}{109}$=·0091 "	7·0
UT_5	1024	$\frac{1}{135}$=·0074 "	7·6

Although, at first sight, the apparatus which I have used in this research may appear coarse, yet experience showed that the accuracy of a determination depended more on the ear than on the mechanical appliances of our experiments; for the average difference in the measures of the duration of any one residual sensation did not exceed the $\frac{1}{2200}$ of a second. The perforated disc made $3\frac{3}{4}$ revolutions to one of the driving crank, and if the disc has twelve perforations, then the above difference of $\frac{1}{2200}$ of a second is given by the difference between two observations, in one of which the driving crank made 30 revolutions in ten seconds, and in the other made 32 revolutions in the same time. It is evident that the apparatus readily detects this difference, especially as I often ran it during thirty seconds to obtain the number of beats striking the ear during one second.

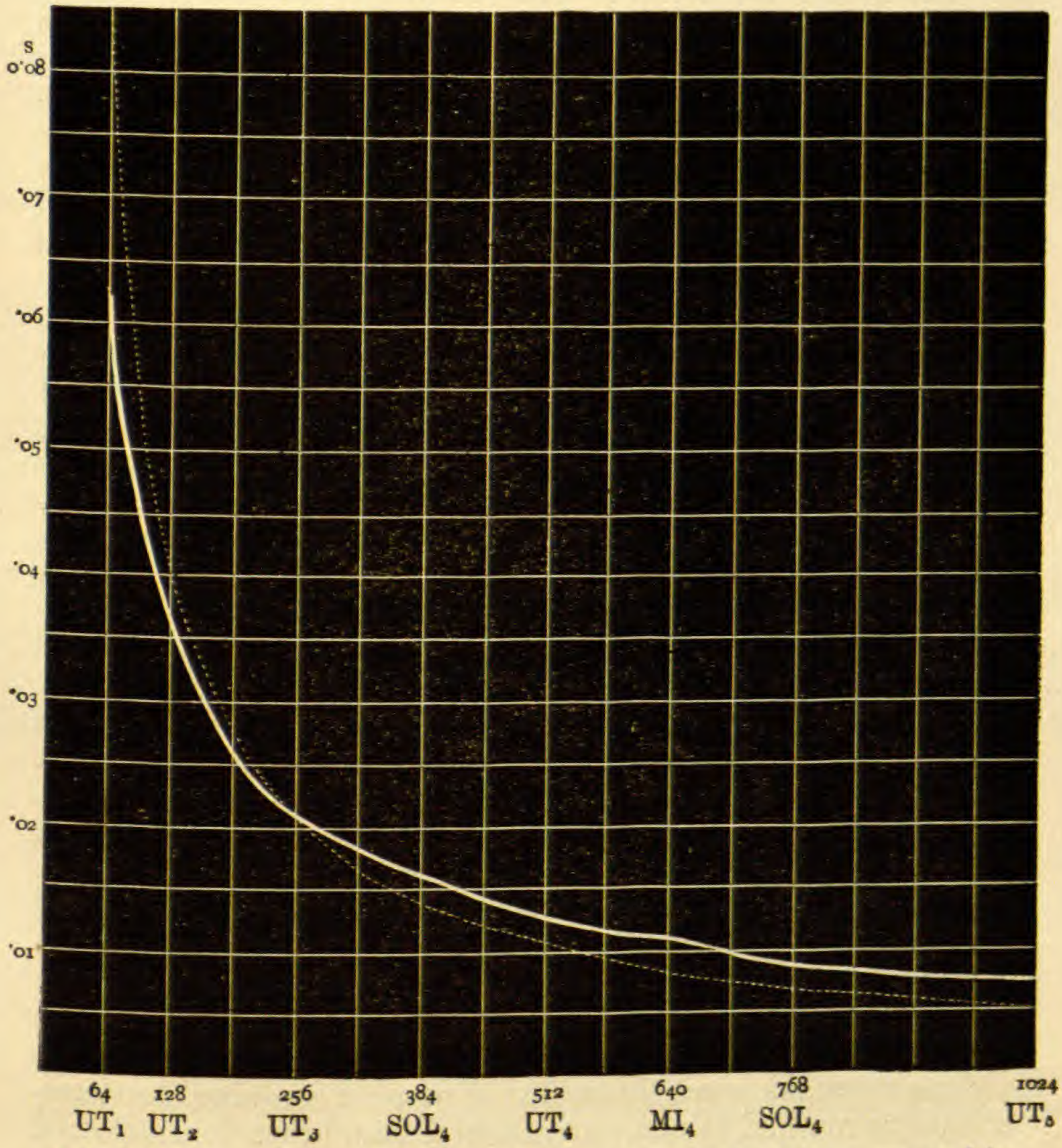
Before accepting as final the above determinations, I ascertained that great differences in the intensities of the pulses had

* I here, as always, refer to complete vibrations, i. e., to a motion to *and* fro, or to what the late Prof. De Morgan proposed to call *a swing-swang.*

† This is to say that we take as the duration of the residual sensation the interval during which the impress of a beat has not diminished sufficiently in intensity to cause discontinuity in the sensation. To obtain the duration of the entire sensation we should have to know the intensity of the sensation at the end of the above interval, and law giving the rate of diminution of the sensation.

little effect on the number of beats required to produce a continuous sensation. When a great increase in intensity was given to the pulses, their number had to be slightly increased, to produce the same continuous sensation as that experienced

2.



with feebler pulses; but the difference was barely measurable. It is also important to remark, that, after the blending of the pulses has been once attained, a further increase in the velocity of the disc does not change the character of the sensation. Extreme velocities, of course, produce such violent agitations at the mouth of the resonator as to render experimenting impossible.

I now projected the above determinations into a curve; placing on the axis of abscissas the numbers of vibrations of the various sounds and on the ordinates their corresponding durations of residual sensations. Thus was produced the full-lined curve of the accompanying figure (2). The dotted curve is an equilateral hyperbola and expresses the assumed law that the durations of the residual sensations are inversely as the numbers of the vibrations producing them. In drawing the latter curve I took the point corresponding to the ordinate of UT_3 as the basis of the assumption. The curve given by the observations does not coincide with the hyperbola. The formula* of the curve of the observations is

$$y=\frac{41600}{x+18}+24.$$

The law connecting the pitch of a sound with the duration of its residual sensation is

$$D=\left(\frac{53248}{N+23}+24\right).0001,$$

In which D=the duration of the residual sensation, and N= the number of vibrations corresponding to D. The ordinate of MI_4 is not quite embraced in the law; I have left it outside, for no matter how careful and many were the observations on this sound, I could not alter its place in the curve of the experiments. According to observation, the duration of MI_4 is ·01111 of a second; according to the law, it equals ·00827; giving a difference of ·00284 second.

2. *The Determination of the numbers of Beats, throughout the musical scale, which produce the greatest Dissonances.*

The determination of the law, which shows the connection existing between the pitch of a sound and the number of its beats which causes the most dissonant sensation, was made with the same apparatus which served for the discovery of the law just discussed. The determination of the number of beats producing the greatest dissonant effect with a given sound is difficult; for the point of maximum dissonance is not sharply marked, and individual judgment and peculiarities come in, so that the range of the determination for any given sound by different persons is considerable. But on discussing the determinations reached by any one person, I found that they followed a well marked law, which, as might have been inferred, is closely connected with the law of the duration of the residual sensation. Indeed, we find that any one observer always makes the numbers producing the maximum dissonances a constant fraction of the numbers of beats which give continuous sensations. Thus, I find that $\frac{3}{10}$ of the latter numbers give me the most disagreeable sensations; another observer has placed the fraction as high as $\frac{6}{10}$. I imagine that we do not

* To Mr. P. P. Poinier, one of the students of my laboratory, I gave the task of discussing this curve, and to him I am indebted for the above formula.

greatly depart from an average judgment in stating that about $\frac{4}{10}$ of the numbers of beats, throughout the musical scale, which produce continuous sensations, correspond to the numbers of beats giving the greatest dissonant effects. Thus we can go from the law connecting the pitch of a sound with the duration of its residual sonorous sensation to the law giving the numbers of beats throughout the musical scale which produce the most dissonant sensations.

3. *Application of the above laws in a new method of Sonorous Analysis, by means of a perforated rotating disc.*

It is an interesting deduction from the laws we have established that a composite sound can be analyzed by means of a rotating disc with sectors cut out of it. Thus, on rotating a large perforated disc with great velocity before a reed pipe, and placing the ear close to the disc,—or in connection with the gutta-percha funnel, by means of the rubber tube,—we shall have the composite sound reaching the ear in a series of impacts which succeed each other so rapidly that even those of the highest harmonic of the reed blend into a continuous sensation; but, on gradually lowering the velocity of rotation the impacts of this highest harmonic can no longer blend and we perceive the harmonic beating on the ear alone. This can be readily confirmed by the aid of a resonator. A further slight lowering of the velocity brings out the beats of the next lower harmonic, and so on, until the velocity has been so diminished that even the beats of the lowest, or fundamental, harmonic are perceived; and then all of the component sounds of the reed are beating in unison; but yet the effects they produce on the ear are very different, for the higher harmonics, notwithstanding their feebler intensities, must be heard more distinctly, because their intermittences are furthest removed from the numbers that cause their sensations to blend. In other words, the highest harmonics, in the phase of the experiment above described, approach nearer than the lower to the numbers of beats required to cause them to give their greatest dissonant effects. This method of sonorous analysis was arrived at as a deduction from our laws, and subsequent experiments confirmed the assumption that a sonorous analysis could be thus effected. This curious discovery has its analogue, in the case of light; for, when a disc with alternate white and black sectors is rotated so slowly that distinct flashes of white light are perceived, the retina is thrown into states of successive increasing and decreasing excitation; now the moment of the maximum of excitations is not the same for all colors, but the excitation takes place sooner for the red and the violet than for the green. "Si l'on fait tourner un semblable disque, lentement d'abord, puis, graduellement, de plus en plus vite, et qu'on le regarde fixement, en évitant de de suivre du regard l'image en mouvement, on remarque que le blanc se colore en rougeâtre sur le bord qui se présente le premier, et en bleuâtre sur le bord postérieur. Pour un faible éclairage, le ton rougeâtre tire plus sur le jaune-rouge, le bleuâtre sur le violet; pour un éclairage intense, le premier tire sur le rose, le second sur le bleu-vert. Si la rotation est lente, le ton bleuâtre s'étend d'abord sur une plus grande partie du blanc que le ton rougeâtre. Si, au contraire, la rotation est rapide, le rouge s'étend en rose sur tout le blanc, tandis que le bleu-vert s'avance sur les secteurs noirs; en somme, le violet paraît alors prédominer sur tout le disque. Pour une rotation encore plus rapide, on ne distingue plus l'un de l'autre les différents secteurs; on voit alors le champ finement jaspé de taches qui papillotent entre le rose-violet et le gris-vert. Enfin, si la rapidité de la rotation augmente encore, le papillotage diminue, la couleur grise résultant du blanc et du noir ressort de mieux en mieux et n'est plus recouverte que par de grandes taches variables, d'un rose violet, qui présentent l'aspect des taches et des bandes qu' on voit sur un tissu de soie mouillé." (Helmholtz, Optique Physiologique, Paris, 1867, p. 500.)

4. *Deductions from these laws leading to new facts in the Physiology of Audition.*

The immediate consideration of the laws we have established gives the most convincing confirmation of Helmholtz's ideas of the high differentiation in the dynamic constitution, or mechanism of the ear. The very fact of the ear's power to effect a sonorous analysis was shown by Helmholtz to be a proof of this; but our physiological law, susceptible of a mathematical expression, affords the most direct proof that one could desire of the existence in the ear of a highly differentiated mechanism, so differently affected in its different parts by sounds of different pitch. Indeed, Helmholtz also divined this even from his restricted premises, which I have had the privilege of enlarging, for, he says (Tonempf., p. 215): "As the difficulty of making a trill in the bass is the same on all musical instruments, and as it is evidently altogether independent of the mode of production of sound on each instrument, we have to conclude that we have here to do with a difficulty which resides in the ear itself. Here is a phenomenon which neatly proves that the vibrations of the mobile parts of the ear for bass sounds are not 'damped' sufficiently, or quickly enough, to prevent two sounds to succeed each other so rapidly without blending.

"This fact proves, besides, *that there should be in the ear different parts which are set in vibration by sounds of different height,* *and which give the sensations of these sounds.* Some may imagine that the mass of the vibratile elements of the ear, comprising the tympanic membrane, the ossicles and the liquid of the internal ear, can vibrate, and that it is on this property of this mass that depends the impossibility of sonorous vibrations ceasing with the same rapidity in the ear. But this hypothesis does not suffice to explain the known facts.

"When, in fact, an elastic body enters into vibration under the influence of an exterior sound, it takes the number of vibrations of the latter; but as soon as the exciting sound ceases, it vibrates with the number of vibrations which belongs to it when vibrating freely. This fact, which is a consequence of theory, can be very neatly proved for tuning forks by means of the vibration-microscope.

"Therefore, if the ear vibrate as an entire system, and is capable of prolonging notably its vibrations, this prolongation should depend on the number of its own free vibrations, which is altogether independent of the number of vibrations of the exterior sound, which excited the vibratory motion. It at once follows that it will be as difficult to trill among the high notes as among those of the bass, and, also, that the two sounds of the trill will blend, not with each other, but with a third sound belonging to the ear itself. We have already made known one of the sounds in the preceding chapter: it is the fa_6.* In these circumstances, consequently, the result should be altogether different from that given us by the observation of the facts."

If we extend our law downward and upward, throughout the range of audible sounds, we have for forty vibrations per second a residual sensation lasting $\frac{1}{11}$ of a second; while for 40,000 vibrations per second we have a residual sensation enduring only $\frac{1}{500}$ of a second. If we apply the law to vibrations below forty per second, when they do not produce a continuous sound, but explosive sensations in the ear, we reach a remarkable result. Thus, the residual sensation corresponding

* I here adopt, as I always do, the French notation, which is used by König. Those who use the French translation of Helmholtz's work should be on their guard to observe that the translator has lowered all of his notation one unit below that used in France, and he thus gives all of Helmholtz's notes too low by an octave. Thus, the translator's Ut_2 should read Ut_3.

The fact to which Helmholtz refers above is that the human ear is tuned by resonance, to the fa_6, of 2730 complete vibrations; so that the vibrations of this note, and of those near it, cause piercing sensations in our ears. If a short tube be adapted to the external auditory canal these disagreeable sensations disappear, as the canal can no longer resound to the above note; but the same piercing sensation will now reappear on sounding a lower note. Mad. E. Seiler, now of Philadelphia, has shown that dogs are peculiarly sensitive to the acute *mi* of the violin.

to thirty vibrations per second should remain in the ear $\frac{1}{10}$ of a second, after the vibrations outside the ear have ceased; then we at once ask why is it, if the residual sensation lasts $\frac{1}{10}$ of a second, that thirty beats or pulses per second do not blend? Do not *three* distinct impulses fall on the ear in each $\frac{1}{10}$ of a second? For they follow one another at thirtieths of a second. This abrupt breaking down of the law can only be explained by the highly probable supposition that co-vibrating bodies in the ear, tuned to vibrations below forty per second, do not exist, and, therefore, as there are no bodies in the inner ear to co-vibrate and keep up these oscillations, after the cause which would have set them in motion has ceased to exist, it follows that when the ear receives less than forty vibrations per second it can only vibrate *en masse*, and the durations of these oscillations of the ear, as a whole, are far too short to remain the $\frac{1}{30}$ of a second. The last supposition as to the vibration of the ear as a mass may serve to explain why the higher notes—far beyond those used in the musical scale—produce continuous sensations. For, to these very high sounds we can hardly imagine corresponding tuned bodies; yet they produce continuous sensations. But may it not be imagined that the ear with them does also only vibrate as one mass, and that the duration of this vibration is sufficient to give continuous sensations from pulses following at the rate of several thousand per second? But for notes thus perceived, without the intervention of corresponding co-vibrating parts in the inner ear, differences of pitch should be difficult, even impossible, to distinguish, and this we find to be the case.

The fact that the durations of the residual sensations diminish, as the numbers of vibrations producing the sounds increase, leads to the knowledge of a new and curious phenomenon in the physiology of audition, viz: that the timbre of a composite sound begins to change at the instant the vibrations, outside the ear, have ceased; for from that instant the residual sensation becomes more and more simple in its character, until at last only the simple sound of the fundamental harmonic remains in the ear, and soon after, this sensation also vanishes. Thus, after the vibrations of an UT_2 reed pipe containing twenty harmonics have ceased, the residual sensation of the twentieth harmonic, or that highest in pitch, disappears in the $\frac{1}{227}$ of a second; but the sensation of the fundamental, or lowest harmonic, remains in the ear $\frac{1}{29}$ of a second after the sensation of the highest has vanished; and the fundamental remains $\frac{1}{58}$ of a second after the cessation of the sensation of the harmonic next above it.

The successive rates of increase of the ordinates of the curve (which expresses our law), as we go from that ordinate belonging to the highest note to that belonging to the lowest, represent the rate of successive extinctions of these harmonics in the composite residual sensation. These successive changes in timbre are well illustrated by sounding all the twenty forks of the harmonic series of UT_2 and then stopping the vibrations successively, in going from the highest to the lowest.

The remarkable phenomenon we have just described also has its counterpart in the analogous series of changes in visual sensations which happen when the eye has received the sudden impress of a bright white light and is then immediately closed in darkness. Thus, the average duration of the residual sensation in the eye is the $\frac{1}{24}$ of a second for lights of moderate intensity; but if the image of a bright cloud be received on the eye for $\frac{1}{3}$ of a second, the "positive sensation" remains for twelve seconds. The duration of this residual sensation depends on the color; lasting longer for red than for violet and longer for violet than for green. Here an analogy with our sonorous sensations is presented, for those ætherial vibrations producing red are fewer in number than either green or violet, and the sensation of red lasts longer than either green or violet, and, therefore, it follows that we should have the residual image of the sun go through these changes—white, greenish-blue, blue, violet, purple, red; and this is what really happens when the sun's image is momentarily formed on the retina and the eye then kept in darkness.

The above analogy is, however, imperfect if it really is established that the residual sensation of violet lasts longer than that of green, when the vibrations, giving these two colors, have equality of energy. The analogy also is one of *sensations*, not one of the *mechanisms* existing between the agents and the sensations they produce; for, in the case of the ear, anatomical facts give us bases for the explanation of the ear's power of effecting a sonorous analysis, and for the understanding of the reason of our law of the duration of the residual sensation. In other words, in the ear we have laid before us the mechanism of (1) the receiving apparatus, of (2) the transmitting apparatus, and of (3) the sensory apparatus: but in the eye we comprehend only (1) and (2), but we know as yet nothing that gives us an understanding of the dynamics of the sensory apparatus. For, has modern histology given us any facts concerning the structure of the human retina which point to the establishment of Young's hypothesis of three distinct sets of retinal nerve terminations? The more we study the minute structure of the retinal rods and cones, the farther appears to remove an understanding of the mode of operation of the sensory apparatus of the eye. May not research in this direction be guided by the hypothesis that the *molecular* constitution of the retinal rods and cones is such that their molecules are severally tuned to the vibrations corresponding to the colors red, green and violet? This would lead us to look for effects of actinism on the retina as showing the link existing between the transmitting and sensory functions of the eye. Do not the facts of the known persistence of chemical action, after it has been once initiated, and the time which would be required for the retinal molecules to recombine, or rearrange themselves, after the ætherial vibrations had ceased, comport with the known durations of the residual visual sensations, and with the main facts of physiological optics, better than the hypothesis that masses of the retinal elements are set in vibration, rather than their molecules?

5. *Quantitative applications of the Laws to the fundamental facts of Musical Harmony.*

To show the full value of these laws in introducing quantitative precision in the explanations of consonance and dissonance would require an extended space; we here present only such application as will serve to show their importance in giving clear and simple guides in reasonings in the physiological theory of musical harmony.

We have seen that in the case of the simple sound C_1, of 64 vibrations per second, that 16 beats gave a continuous sensation; therefore, to determine the nearest consonant interval of this sound, we have $64 : 64+16 :: C_1 : E_1$. Hence the nearest consonant interval of C_1 is its major third on the natural scale. To determine by our law the nearest consonant interval of C_2, of 128 vibrations per second, we have $128 : 128+26 :: C_2 : E^{b}_2$; here the minor third on the natural scale is the nearest consonance. In the following table we give the determinations of the nearest consonant intervals of the simple sounds of the Cs through five octaves.*

C_1	of	64	vibrations,	interval	= major third.
C_2	"	128	"	"	= minor third.
C_3	"	256	"	"	= " minus $\frac{1}{4}$ + of a semitone.
C_4	"	512	"	"	= " minus $\frac{1}{2}$ — of a semitone.
C_5	"	1024	"	"	= one tone.
C_6	"	2048	"	"	= one tone minus $\frac{1}{2}$ + of a semitone.

We thus see that while in the neighborhood of C_1 the nearest consonant interval is the major third, that in the octave of C_5 the nearest consonant interval has contracted to a tone. This result shows why it is that the middle portion of the

* We have, for simplicity of illustration, determined the above intervals on the basis of the pitch of the lower note; but, as the beats are produced by the conjoined action of the two sounds, it would have been more accurate to have taken, as a second approximation, the mean pitch of the two sounds. Thereby, the above determinations would be somewhat changed for lower but not perceptibly for higher notes.

musical scale is best adapted for expression, and is most used in musical composition; for, while in the lowest octaves the available consonant intervals are few on account of the spaces separating them, in the higher octaves the consonances are so contracted that these higher consonant intervals lose their sharpness of definition.

It is here to be remarked that in our experiments we have obtained continuous and discontinuous sensations from beats produced by one sound of a constant pitch; but with musical intervals we obtain beats from two sounds differing in pitch. In the latter case DeMorgan, Guéroult and Mr. Sedley Taylor have shown that there exists a variation, or oscillation, in pitch whenever the two sounds are not of the same intensity. Mr. Taylor,* from this fact, advances the idea that these oscillations in pitch cause a *noise* in place of a *sound*, and to this result is due, in great part, the dissonance produced by beats of two different sounds. He brings forward in support of his opinion experiments with a tuning-fork, rapidly spun in a lathe; where he states that no alteration in pitch, but only alteration in intensity, can take place. I have repeated this experiment, but have not been able to confirm his observation, on account of the change in pitch which takes place almost as soon as the fork is rotated. Indeed, I have thus succeeded in elevating the pitch of a fork to nearly a minor third. But assuming that this explanation of dissonance is correct, yet our views hold good when we consider intervals formed of sounds, equal in intensity.

Helmholtz (Tonempf., ch. viii, p. 258 et seq.) states generally that, *in the higher regions of the musical scale*, thirty to forty beats per second give rise to the most disagreeable dissonance, but that if these beats follow so rapidly that about 132 fall upon the ear in a second, then the sensation they cause is smooth and continuous, and the two notes producing these beats form a consonant interval. Helmholtz, at the same time, dwells with emphasis and with some minuteness on the additional important fact, that the dissonant and consonant effects of beats do not altogether depend on their frequency per second, but also on the position in the musical scale of the sounds producing these beats. Tyndall, in his otherwise admirable little book "On Sound," has overlooked the latter important fact, and while assuming broadly that 33 beats per second—no matter what the pitch of the notes producing them —give the greatest dissonance, while 132 beats per second give consonance, he has made logical deductions from these premises which do not bear the test of experiment or conform to the experiences of musicians. On page 299 of Tyndall's work we

* On the Variations of Pitch in Beats. By Sedley Taylor, Esq., Phil. Mag., July, 1872.

read: "Does this theory accord with the facts that have been adduced? Let us, in the first place, examine our four tuning-forks, to ascertain whether their deportment harmonizes with this theory. And here let me remark that we have only to do with the fundamental tones of the forks. Care has been taken that their overtones should not come into play, and they have been sounded so feebly that no resultant tones mingled in any sensible degree with the fundamental tones. Bearing in mind, then, that the beats and the dissonance vanish when the difference of the two rates of vibration is 0; that the dissonance is at its maximum when the beats number 33 per second; that it lessens gradually afterward, and entirely disappears when the beats amount to 132 per second,—we will analyze the sounds of our forks, beginning with the *octave.*

"Here our rates of vibration are

$$512-256\text{; difference}=256.$$

"It is plain that in this case we can have no beats, the difference being too high to admit of them."

But if Prof. Tyndall had taken, in place of the above forks, two forks giving 40 and 80 vibrations per second, he would, according to his premises, have made this octave a most dissonant interval; for would he not have had $(80-40=40)$ forty beats per second entering his ear? Similarly, if we assume that 33 beats per second always produce the maximum dissonance, then even the interval $C_1 : C_2$, which gives a difference of 64, is far removed from consonance.

Prof. Tyndall then proceeds: "Let us now take the *fifth.* Here the rates of vibration are

$$384-256\text{; difference}=128.$$

"This difference is barely under the number 132, at which the beats vanish: consequently the roughness must be very slight.

"Taking the *fourth*, the numbers are

$$384-312\text{; difference}=72.$$

"Here we are clearly within the limit, when the beats vanish, the consequent roughness being quite sensible.

"Taking the *major third*, the numbers are

$$320-256\text{; difference}=64.$$

"Here we are still further within the limits, and, accordingly, the roughness is more perceptible.

"Thus we see that the deportment of our four tuning-forks is entirely in accordance with the explanation which assigns dissonance to beats."

Now all of the above intervals were formed of simple sounds, without overtones or resultants, and in not one of the experiments adduced does the number of beats fall "within the limit where the beats vanish," and "the consequent roughness" referred to does not exist. Thus, in the case of *the fifth* (384−256; difference=128), the blending would have been reached, according to our law, by fifty-six beats per second. In the case of *the fourth*, (384−312; difference=72), the blending is reached by sixty beats; while with *the major third* (320—256; difference=64), fifty-one beats give the limit of dissonance. All of these intervals are really consonant, and any musical ear will attest, on repeating the experiments, that the intervals cited cause no perceptible roughness. Indeed, the combinations cited by Prof. Tyndall, when formed of simple sounds, are all eminently consonant intervals, throughout the whole range of audible sounds. The same criticism applies, with nearly equal force, to the experiments on pages 302 to 304, cited as illustrating Helmholtz's hypothesis. Any one may convince himself of this by means of free-reed organ pipes brought *rigorously* into these intervals on the natural scale.

These criticisms on the book of my friend Professor Tyndall are not given merely for the sake of criticism, but because his eminence as an original investigator, and his great power as a popular teacher of scientific truths, have given an extensive distribution to his writings, and I am sure he will be obliged to any one who, in the proper friendly spirit, will show how his work, written to diffuse scientific knowledge, may be rendered more efficient in accomplishing that object.

ART. XXII.—*On the Thermo-electrical Properties of some Minerals and their varieties;** by A. SCHRAUF and EDWARD S. DANA.

1. All previously published investigations in thermo-electricity have led to this result: that certain minerals are in part *positive*, and in part *negative*, in contact with copper, and that on this account, they hold different positions in the thermo-electrical series.

If, for example, in the series given by Seebeck,† we number bismuth 1 and tellurium 34, we have as

No. 5	Platinum (pure).	No. 7	Copper (pure, from CuO).
No. 14	" (2).	No. 12	" (commercial).
No. 28	" (1).	No. 21	" "

These variations recall to mind the property which belongs to all metals, of suffering very great changes in cohesion, elasticity, etc., in consequence of a minute admixture of some foreign substances.

* From the Transactions of the Royal Academy of Sciences at Vienna, vol. lxix, March 1874. Translated by E. S. Dana, and read before the American Association at the Hartford meeting.

† Seebeck, Gilb. Ann., vol. lxxiii, 430; Pogg. Ann., vol. vi.

This important fact has been well established in the case of gold and iron.

The variation from + to −, however, has been shown to characterize crystallized minerals as well as the amorphous metals. Hankel,* Marbach,† Friedel ‡ and G. Rose ‖ have investigated this subject in relation to the hemihedral crystals of pyrite and cobaltite. Friedel first suggested the connection between positive and negative thermo-electricity and right and left hemihedrism, but did not pursue the matter further: evidently because, in the case of pyrite, the morphological and electrical relations do not allow of definite determination.§ Rose attempted to obtain a solution of the same problem by identifying ± with right and left hemihedrism. For the correctness of this supposition, however, there has been as yet no conclusive proof, while, on the other hand, more intricate hypotheses seem to be required for its support.¶ For minerals which do not crystallize hemihedrally, this hypothesis of Rose is evidently inapplicable; and, what is more, if accepted, no variation in electrical character should, in such cases, be expected. It is here important to mention that, some years before the investigations of Rose, exceptions of this nature were observed by Prof. Stefan;** he found granular galena negative, crystallized galena positive.

The thermo-electrical series, recently published by Flight,†† also affords several such examples. His list embraces a large number of minerals. Making hematite 1 (at the negative extremity of the series) and fused chalcocite (No. 3) 56 (at the positive end), we find, as below:

Chalcopyrite	(1)	is No.	2
Fused chalcopyrite	(1)		21
Galenite			4
Fused galenite			31

What changes, if any, these natural sulphids underwent in consequence of being fused, do not appear to have been accurately investigated, although this is a point of the highest interest. It is clear from the last observations that an explanation is often to be found in an altered condition of aggregation.

* Hankel, Pogg. Ann., vol. lxii, 197. † Marbach, Compt. Rend., vol. xlv, 707.
‡ Friedel, Instit., 1860, 420; Ann. d. Chim., 1869, vol. xvi, 14.
‖ G. Rose, Pogg. Ann., 1871, vol. cxlii, 13.

§ In a recent paper (which reached us after the completion of our manuscript) Friedel (Comptes Rendus, xvi, Feb., 1874, p. 508) claims priority for his views over G. Rose, but at the same time he explains that while his hypothesis is to be assumed as true, it does not *crystallographically* admit of proof.

¶ Compare on this subject, Brezina, Tschermak's Min. Mittheil, 1872, p. 23.
** Stefan, Sitzungsb. d. k. Akad. Vienna, 1865, vol. li, 260.
†† Flight, Ann. d. Chem. u. Pharm., vol. cxxxvi.

The view of Franz,* that the direction of the stream (±) in bismuth depends upon the cleavage lamellæ, offers a satisfactory explanation of some of the phenomena.† On the other hand, the right or left hemihedrism entirely fails to account for the change of ± in holohedral or amorphous fused materials; and in such cases other causes must exist and must be sought for.

It was with great pleasure, consequently, that we availed ourselves of the kindness of Prof. Stefan in putting at our disposal a very sensitive galvanometer belonging to the Physical Institute (Vienna); and we take this opportunity to acknowledge our obligations to him.

2. The thermo-electrical investigation of minerals can have as its object either the determination of their absolute position in the series, or their relative position upon contact with copper. Of these, we selected the latter, since our attention was especially directed to the exceptions, as they may be called. Our method of investigation, consequently, was identical with that which has been made use of and fully described by Prof. Stefan and G. Rose. We mention here merely that the homogeneity of our copper wire was carefully ascertained. The wire when warmed and brought in contact with the cold wire left the needle in complete rest,‡ so that no considerable error in consequence of subordinate currents is possible.

A series of preliminary experiments was devoted to establishing the relation between form and thermo-electricity, especial attention being directed to determining the influence of the surface, and of hemihedrism. The following results were obtained:

(*a.*) In galenite, cobaltite, and pyrite, the interior of the crystal has the same electrical character as the surface under normal circumstances; consequently, the particular planes by which the crystals are bounded have no influence upon the ± sign.

b. Surfaces made by grinding (on the minerals just named), whatever position they have with reference to the cleavage directions, have the same electrical character with the mass as a whole.

(*c.*) In tetrahedrite and chalcopyrite, which crystallize hemihedrally, and with the most complete opposition of right and left, no change of ± could be discovered.‖

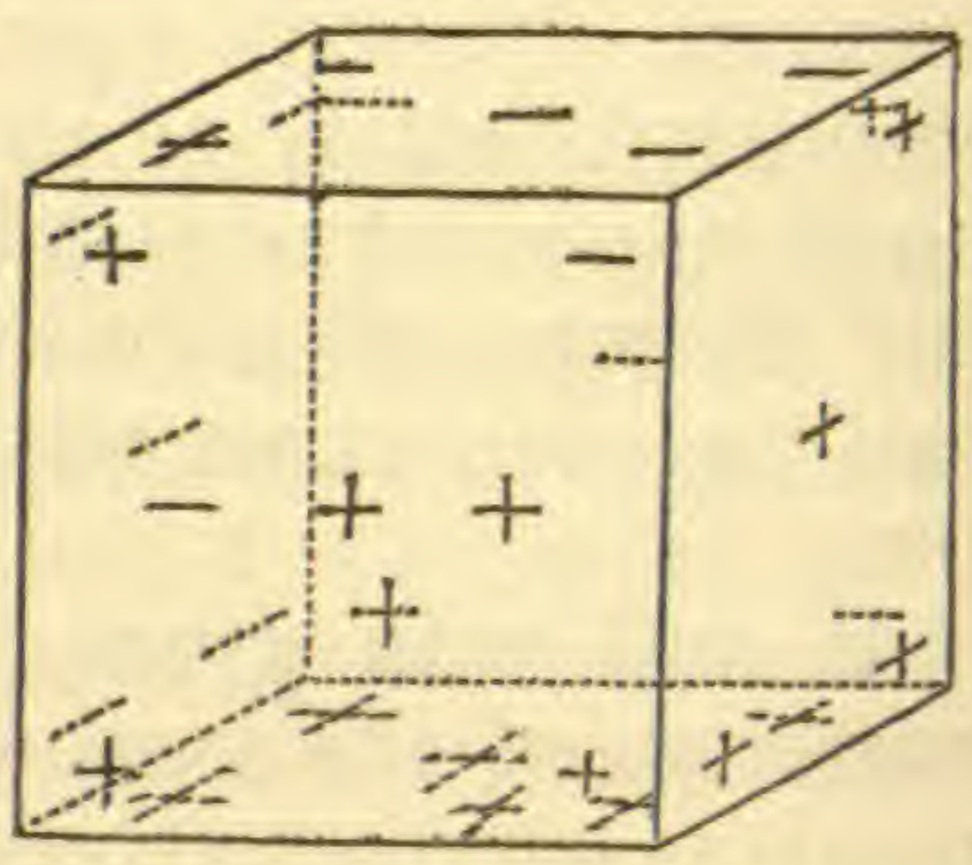
* Franz, Pogg. Ann., vol. lxxxiii, 375; vol. lxxxiv, 388; vol. xcvii, 34.

† Schrauf (Lehrbuch d. Phys. Min., II Bd., p. 386) has adopted this explanation. In accordance with the results of the present investigation, this view of the electrical phenomena must be altered.

‡ We may mention in addition that a brass wire in contact with itself (cut from the same piece) caused a considerable deflection of the needle, in the direction of the + stream.

‖ Rose (l. c.) found the same constancy in the sign in chalcopyrite.

1.



(*d.*) On crystals of pyrite the ± portions are sometimes distributed very irregularly (fig. 1), and any explanation of this by the supposition of a twin structure is impossible. We should have to assume, in this case, a parallel-wise interpenetration of the different individuals or lamellæ.

(*e.*) The majority of the pyrite crystals are negative in relation to copper; the positive portions appear often to be only thin layers, of a different nature from the mass of the crystal. Homogeneous + pyrite crystals are exceedingly rare.

These results show clearly that an investigation of the thermo-electrical properties of minerals is of value only when their chemical composition is known. It is well known how much the composition of individual minerals varies; how questionable it is, for example, whether different pyrites have the same composition.* As before observed, a minute admixture of foreign materials may exert a very decided influence upon the applicability of the metals in the arts, and consequently upon their physical characters. On this account our attention, in the following investigations, was directed especially to the chemical side of the subject; and, without meaning to overlook the connection between the form and the variation of ±, we endeavored, first of all, to determine the influence of the material itself. To this end the varieties of certain minerals are especially well adapted; and, in fact, out of a long series of sulphids, arsenids and tellurids of cobalt, iron, nickel and bismuth, we were successful in finding some examples capable of showing the relation between the thermo-electricity and the chemical nature of the minerals. It was necessary first, however, to investigate a large number of species.

3. Our results in regard to the thermo-electrical character of the minerals tested related throughout to their behaviour in contact with copper. The position of the wire employed was unfortunately not exactly in the middle of the thermo-electrical series as given by Seebeck (l. c.), but on the other hand near pure gold and silver. It was to be expected, in consequence of this, that the larger number of substances would be negative in relation to copper wire; and the observations were in accordance with this. A number of minerals which gave no perceptible stream, and are consequently marked 0 in the following list, might perhaps be + in contact with a different

* The use made of pyrite in England (more than eight million cwt. yearly) shows more than any single analysis the amount of Cu, Ag, and Au, contained in this mineral.

piece of copper. But these facts have only a minor importance in this investigation. Cobalt, nickel and bismuth are strongly negative, and iron strongly positive; their sulphids, on the other hand, act still more vigorously on the needle of the galvanometer than the native metals. The variation from the true result, occasioned by the relative position of the copper wire used, must consequently be quite unimportant.

A. *The following minerals gave no electrical stream in contact with copper.* Taking into consideration the position of the copper employed by us, as just explained, these minerals must stand nearer the positive end of the series (except so far as their non-conducting power was the occasion of the negative result).

Argentite	Ag_2S	isometric.
Acanthite	Ag_2S	orthorhombic.
Sphalerite	ZnS	isomet. hemihedral.
Alabandite	MnS	isometric.
Hauerite	MnS_2	"
Rutile	TiO_2	orthorhombic.
Brookite	TiO_2	monoclinic.
Antimonite	Sb_2S_3	
Boulangerite	$Pb_3Sb_2S_6$	
Kobellite	$Pb_6Bi_2Sb_2S_{12}$	
Sartorite	$PbAs_2S_4$	

This list of minerals investigated embraces unfortunately the majority of those substances from which we expected to throw great light upon our subject.

In the dimorphous group of the silver sulphids the change of form does not correspond to any variation in the thermo-electrical properties. Argentite and acanthite are in this respect similar, so also rutile and brookite. In sphalerite, also, the form seemed to have no influence, although right and left hemihedrism is distinguishable. Alabandite and hauerite were selected in order to show the influence of an increasing percentage in sulphur. The group boulangerite, kobellite, sartorite and antimonite were investigated in order to ascertain their relation to galenite; but no essential differences were observed. It is to be mentioned that (in consequence of their containing antimony) they stand nearer the positive end of the series, while galenite, on the other hand, is strongly negative.

B. *The following minerals* are positive* (+), *or negative* (−), *in contact with copper:*

1. *Bismuth compounds.* [*Bi*−]

Bismuthinite	Bi_2S_3	− Sweden.
Tetradymite	$Bi_2(TeS)_3$	− Schubkau, Orawitza.

* It is to be observed, in examining this table, that arsenic, antimony, and tellurium, as native metals, are + in reference to copper, while in composition they are generally negative.

Tetradymite	$Bi_2(TeS)_3$	+	Georgia, England.
Wehrlite	$Bi_2(TeS)_4$	+	Hungary.
2. *Nickel compounds.*		[*Ni* –]	
Millerite	$Ni S$	0.. –	
Gersdorffite	$Ni (AsS)_2$	–	Schladming
Ullmannite	$Ni (Sb,AsS)_2$	–	Karinthia.
Niccolite	Ni_2As_2	–	Pribram.
Rammelsbergite	$NiAs_2$	–	Hesse.
Grünauite	(NiBiFeCu)S	–	Siegen.
Pyrite (containing Ni)	FeS_2+4%Ni	–	Dramen, Norway.
3. *Cobalt compounds.*		[*Co* –]	
Linnæite	Co_3S_4	–	Müsen.
Smaltite	$(CoFeNi)As_2$	–	Saxony, Hesse.
Cobaltite	$Co(SAs)_2$	+–	Sweden.
Glaucodot	$(CoFe)(SAs)_2$	+–	Hakansbö.
Alloclasite	$(CoBi_2)_3(SAs_3)_4$	–	Orawitza.
Skutterudite	$CoAs_3$	+	Skutterud, Kongsberg.
"	"	–	Modum, Tunaberg.
4. *Lead compounds.*		[*Pb* –]	
Galenite	PbS	–	Usual varieties.
"	"	+	Monte Poni.
Clausthalite	PbSe	–	Harz.
Naumannite	(PbAg)Se	–	Harz.
Lehrbachite	(PbHg)Se	–	Harz.
Zorgite	(CuPb)Se	+–	Variable, mixture?
5. *Silver and Gold compound.*		[*Ag Au O*]	
Sylvanite	$(Ag,Au)Te_2$	–	
6. *Copper compounds.*		[*Cu O*]	
Chalcopyrite	$Cu_2Fe_2S_4$	–	
Bornite	$Cu_6Fe_2S_6$	+	
Tetrahedrite	$Cu_4Sb_2S_7$	0..+	
Chalcocite	Cu_2S	+	
Berzelianite	Cu_2Se	+	Sweden.
Zorgite	(PbCu)Se	+–	Variable, mixture. Harz.
7. *Iron compounds.*		[*Fe* +]	
Marcasite	FeS_2	+	
Pyrite	"	+	Piedmont, Devonshire, Turinsk.
"	"	–	Most localities.
Pyrite (containing Ni)	FeS_2+4%Ni	–	Drammen, Norway.
Pyrrhotite	Fe_7S_8	+	Sweden.
Hematite	Fe_2O_3	–	Brazil.
Magnetite	Fe_3O_4	–	Monroe.
Leucopyrite	$FeAs_2$	–	Andreasberg, Schladming.
Löllingite	Fe_2As_3	–	Reichenstein.

Mispickel	$Fe(SAs)_2$	+	England.
" ?	"	–	Freiberg.
" (Weisserz)	$(FeAg)(SAs)_2$	–	Freiberg.
Danaite	$(CoFe)(SAs)_2$	+	Franconia.
"	"	–	Norway.

Although tables *A* and *B* embrace a considerable number of minerals, they afford only a few general conclusions.

(*a.*) In the compounds of the negative metals Bi, Co, Ni, Pb, the character of the metal outweighs that of the S.

(*b.*) The addition of antimony has the result of weakening this negative character; that of tellurium strengthens it.

(*c.*) In combination with iron the arsenides are negative, but the majority of the sulphides positive.

4. A more accurate understanding of the relations between chemical nature and thermo-electricity can be obtained only from those minerals which are sometimes +, sometimes –. Thus, while in the case of iron, copper, and nickel, the addition of an equivalent of S or As takes place without electrical change, other minerals show a change in sign when the mineralogical formula seems to remain unaltered. With the exception of the observations of Stefan on galenite, those on pyrite and cobaltite are the only other published examples. These latter have been the basis of a very one-sided theory, as it seems to us, although this was natural, since the two species in question are similar in their hemihedral forms.

As a result of our observations, we have succeeded in adding four new cases of minerals with this ± variation. We are inclined to lay especial stress upon these, because these minerals—tetradymite, glaucodote, mispickel, skutterudite—are not hemihedral, so that the change in electrical character cannot be conditioned on right and left hemihedrism.

These cases we regard as proving the dependence of the thermo-electricity upon the chemical composition.*

In order to give the proof of this, it is necessary to introduce into the following tables, besides our observations on the thermo-electricity, also the determination of the chemical composition and the density.† We did not fail to recognize the desirability of an exact analysis for each mineral whose electrical character was investigated; but in the absence of them we

* Tait has shown recently that iron changes its thermo-electrical sign at a red heat; and this same is true of nickel at a somewhat lower temperature. Tait is inclined to ascribe this phenomenon to a change in the arrangement of the molecules. Our investigations, however, are based upon the character of a stream called out by the minimum amount of heating, thus excluding the possibility of the substance passing into an allotropic condition in consequence of the heat. The molecular arrangement is, therefore, to be assumed as unchanged in these experiments.

† Analyses and specific gravity determinations, taken from other authors, are printed in italics.

endeavored to supply the want approximately by determining the specific gravity wherever this was possible.*

A. *Tetradymite.*

	Schubkau	Orawitza	Georgia	England	Wehrlite Hungary
	+	+	—	—	+
Te	34·6	35·9	48·7	?	29·7
S	4·8	4·2	0·		2·3
Bi	60·0	59·3	51·5		61·1
Fe					
Ag			0·5		2·0
	Wehrle†	Frenzel ‡	Balch ‖		Wehrle §
G=	7·30 Wehrle†		7·868 Balch ¶		

The tetradymite discovered in 1873 at Orawitza had the same electrical character as that long known from Schubkau and Deutsch-Pilsen. The composition of these three Te-Bi compounds is in fact similar, and they contain a small amount of sulphur. The variety from Dahlonega, Georgia, is remarkable for its well-established want of sulphur. In these compounds, as also in sylvanite, the tellurium has an increasing action in a negative direction, while sulphur, notwithstanding the small amount present, acts in a positive direction.

B. *Danaite.*

	Franconia.	Hakansbö.	Skutterud.	Modum.	Löllingite. Fe_2As_3 Reichenstein.
	+	—	—	—	—
G=	6·335	6·096		6·159	
As	41·7		46·7		65·8
S	17·8		17·4		1·8
Fe	32·9		26·2		32·3
Co	6·4		9·1		
	Hayes.**		Scheerer.††		Karsten.‡‡
G=	6·207 Variety Vermontite from Franconia.	6·08 6·059 Variety Akontite from Hakansbö.‖‖			

* Each of our specific gravity determinations is the mean of several trials. The average mean error may be stated as ± 0·002.

† Wehrle, Schweigg. J., 1830, vol. lix, 482. ‡ Frenzel, Leonh. Jahrb., 1873, 800.

‖ Balch, Amer. Jour. Sci., II, vol. xxxv, p. 99.

§ Wehrle, Baumgart. Zeitsch. Wien, vol. xix, 144.

¶ Balch, Dana Min., 1870, p. 31. ** Hayes, Am. Jour. Sci., II, vol. xxiv, p. 386.

†† Scheerer, Pogg. Ann., vol. xlii, p. 546.

‡‡ Karsten, Dana Min., 1870, p. 78. ‖‖ Dana, Min., 1870, p. 79.

Besides the Danaite, we have added, for the sake of comparison, the chemical composition of löllingite. Taking into consideration the fact that, in the compounds, arsenic and cobalt play the part of negative elements, but iron and also most of the sulphids that of positive, a partial explanation of the change of ± becomes possible. In the crystals from Franconia the percentage of FeS is larger (=50·7) than that of AsCo (=48·1). The reverse is true in the danaite from Norway, where FeS =43·6 and CoAs=55·8. These varieties show a very marked difference in specific gravity.

C. *Skutterudite.*

	Modum.	Tunaberg.	Skutterud.	Kongsberg.
	—	—	+	+
G=	6·934		6·664	
As			79·0	
Co			19·5	
Fe			1·4	
			Wöhler*	

The specific gravity of the varieties from Tunaberg and Kongsberg could not be determined, as in the specimens at hand the mineral was in small imbedded portions. The variation in the case of the varieties from Modum and Skutterud is especially remarkable. Complete crystals from the Skutterud locality were investigated, as well as massive specimens: they were all +. Skutterudite is isometric, but no hemihedrism has been observed.

D. *Glaucodot.*

	Hakansbö.			Chili.
	Shell.	Total.	Kernel.	
	—		+	?
G=	6·011		5·905	
As	(Fused B.B. to a pearl).	44·0	(Fused B.B. to a pearl).	43·2
S		19·8		20·2
Co		16·1		24·7
Fe		19·3		11·0
G=		5·973 Ludwig.†		5·975—6·003 Plattner.‡

The crystals of glaucodot have often a length of 1½ inches. They belong to the orthorhombic system; the cleavage is both basal and prismatic, and may be observed not only in the outer layer of the crystal, but also in the central, although less perfectly. About twenty large crystals from Hakansbö were

* Wöhler, Pogg. Ann., vol. xliii, 591.
† Ludwig, Sitzungsberichte der k. Akad, Vienna, I, vol. lv, 445, 1867.
‡ Plattner, Pogg. Ann., vol. lxvii, 127, 1849.

investigated, all of which showed the same abnormal behavior. The outer shell, 2mm. in thickness, on all the planes, was negative, while the kernel was always positive. If 2mm. of the exterior be filed away, the − shell passes gradually into the + kernel; all fracture surfaces are in the same way +.

In one and the same crystal, then, we find this change of sign. Such a case especially in the orthorhombic system does not allow of hemihedrism being assumed as an explanation. Taking into consideration the great variation in the specific gravity between the exterior and interior portions, as shown by our determinations, it is fair to conclude that the change of ± depends upon some change in the composition. What this change was we were not in a position to determine. The blowpipe beads from the outer and inner portions $(CoFe)As_2$, obtained in the usual manner, were both negative. Their percentage of cobalt was almost the same; one experiment, for example, gave approximately 19·5 per cent Co. The variations are consequently, in all probability, caused by the elements Fe, S, as in danaite.

It was a matter of great regret to us that we were unable to obtain for investigation any of the glaucodot from Chili. The analysis of this species is placed in the above table, as the comparison of two analyses shows the possibility of the variation in Fe just mentioned. In the glaucodot from Chili the relation of Co to Fe is 2:1; in that from Hakansbö 5:4. It is evident from the specific gravity that the analysis * of the glaucodot from Hakansbö was made from a fragment containing both shell and kernel.

An observation of Tschermak † adds plausibility to this hypothesis of a variation in the amount of Fe. He describes crystals of cobaltite imbedded in the outer portion of a crystal of glaucodot. In the transition from glaucodot to cobaltite, the elements Fe and Co are alone involved, and then only in their relative proportions, thus:

	As	S	Co	Fe	
Glaucodot	44·0	19·8	16·0	19·3	Ludwig l. c.
Cobaltite	43·4	20·8	33·1	3·2	Stromeyer ‡

E. *Galenite.*

	Sardinia granular. +	Harz, England. −	Pribram crystals. −	Kobellite $Pb_6Bi_2Sb_2S_{12}$ 0
G=	7·428		7·575	

* An analysis by v. Kobell agrees completely with the former.
† Tschermak, Sitzungsb, d. k. Akad. Vienna, vol. lv, 449, 1867.
‡ Stromeyer, Schweigg. J., vol. xix, 336.

The $\pm$ varieties are distinguished here, as in all other cases mentioned, by their specific gravity.* For the sake of comparison, kobellite has been added; in it, the negative character of the lead and bismuth is neutralized by antimony. What part the admixture of Sb, As and Ag play in galenite is uncertain. We note here that we found bismuthinite to be $-$ but boulangerite 0.

F. *Cobaltite.*

Over four hundred and eighty crystals of cobaltite were investigated: of these, the majority were negative, corresponding to the excess of the elements Co and As: only a quarter of them were positive. The crystals themselves are homogeneous and, unlike those of glaucodot, show no difference between shell and kernel. The following are the observations, arranged according to crystalline form.

	No. crystals investigated.	
Cube prominent	49	+
Octahedrons "	242	−
Pyritohedrons "	32	+
" "	20	−
Cube, octahedron and pyritohedron combined	115	−
	24	+

Rose regarded the $\pm$ character in this species as dependent upon the right or left hemihedrism. A proof to the contrary, from the consideration of the crystalline form, is not possible, as the proof itself lies in the supposition. It is of more importance, therefore, to ask whether all crystals are of the same composition. All cobalt compounds contain iron to a varying extent. If the other constituents are constant, the specific gravity must increase with the percentage of cobalt; the density of cobalt is greater than that of iron. With this in mind, it is intelligible that for glaucodot we have G=6·0, for cobaltite G=6·3 (mean value, compare analyses above).

This consideration led us to determine the specific gravity of a very large number of the crystals of cobaltite whose electri-

* In consideration of the high values of the specific gravity, it is perhaps desirable for us to mention our method of determination. We had at our disposal two balances: a balance made by Kusche in Vienna (maximum load two grams), in the Mineral Cabinet; and another made by Oertling (maximum load 50 grams), private property of Schrauf. We avoided the use of a pygnometer, and adopted in preference the method of two direct weighings in air and water. All our determinations correspond to a mean temperature of 17°–20° C. The agreement of the single observations leaves little to be wished for; we mention some direct results (*not* the mean of several trials) with the weight.

Variety A.	Total 0·64 Gr.,	G=7·430
	6·95 "	" 7·425
Variety B.	0·45 "	" 7·570
	10·14 "	" 7·577

cal characters had been tested. We give the result in a number of individual cases.

−	+		Exceptions. +
G=6·375 Octahedron	6·072	Cube alone from Tunaberg	6·411 Cube
6·370 Oct.	5·934		6·415 Cube, Pyr.
6·356 Oct. Tunaberg	6·046		
6·341 Pyr., Oct.	6·010		
6·442 Cube Pyr.	5·927		
6·387 Oct. Pyr.	5·905		
	6·151		
	6·160		
	6·215	Pyritohedrons from Hakansbö	
	6·263		
	6·208	Pyr. from Tunaberg	
	6·265		
	5·984	Pyr. from Skutterud.	

These figures agree essentially—to 80 or 90 per cent—with our preceding conclusions, that the crystals rich in cobalt are negative and have a higher specific gravity. We may, however, with more certainty conclude that the octahedrons are negative and have G>6·30, while the cubes are positive and have G<6·1; the pyritohedrons vary in sign ±, and have G>6·1. We found also two exceptions in the density, which we place beside the others without attempting an explanation by the suggestion of a possible admixture of nickel.

G. *Sulphides of Iron.*

The important work of G. Rose has directed especial attention to this species. On page 258 we have already given some results, which we obtained under the supposition of some essential connection between form and electrical character. A few observations are here added which relate especially to the chemical composition.

Sulphid of Iron, artificial.	Marcasite.	Pyrite.	
+	+	−	+
*Flight**	G=4·83	G=5·019 Elba	G=4·8[illegible]6 } Cubes Devon.
		=5·020 Piedmont	=4·941
		=5·195 *polished crystals. Zephar.*†	=4·992 Cube
			=4·998 Turinsk.

Among the very large number of crystals we were able to investigate, we found only a few which over the whole surface, as well as in the interior, were homogenous +.‡ This will explain the small number of determinations of specific gravity.

* Flight, Ann. Chem. Pharm., vol. cxxxvi.

† Zepharovich, Dana Min., 1870, p. 63.

‡ The majority of the crystals of pyrite are negative.

The abnormally low density of the cubes from Devonshire suggests that they may be altered material, pseudomorphs of marcasite after pyrite. The mineralogical habit is still that of pyrite, but the polish of the planes is gone. Some + spots upon other – crystals would perhaps have a similar explanation.

The higher specific gravity of pyrite in comparison with marcasite, and its – character (marcasite +), and its crystalline form resembling that of cobaltite, may possibly all have the same cause, viz: the admixture of Cu, Ni, Ag, Au, which gives pyrite its metallurgical value. The ± varieties of FeS_2 are also distinguished by their density. We do not venture to draw any further conclusions from these scanty observations.

5. By the preceding investigations, the fact has been established that all ± varieties of minerals are distinguished also by their specific gravity. It is remarkable that for tetradymite, glaucodot, skutterudite, galenite, cobaltite and pyrite the density of the positive variety is less than that of the negative variety. We simply mention this fact without wishing to establish any dependence of the thermo-electricity upon the density in general, the specific gravity having been taken by us as merely an indirect method of arriving at the chemical composition. The influence of the form upon the density we have attempted to exclude by making our comparisons only with varieties having the same structure, comparing crystals with crystals, massive specimens with massive. The effect of the temperature* was also eliminated so far as possible.

With reference to the influence of the form upon the thermo-electricity we may remark in conclusion: The investigations of alloys of SSe, of BiS, of the oligoclase feldspars, of $(KaNa)_2SO_4$ has shown that the crystalline form does not change with the composition by regular gradations, but rather by abrupt leaps, and consequently remains identical within certain limits of chemical variation. The density, optical properties, and, as we regard it, thermo-electricity give a sharper indication as to identity or difference of material, than the crystalline form. Before establishing relations between the form and thermo-electricity, it must first be shown that the material in hand is identical.

Our observations have demonstrated that in some cases the change in thermo-electrical character corresponds to a change in the chemical composition, and always to an alteration in the density.

* On the influence of temperature, compare note 1, p. 261. It may be mentioned in addition, that a change in density must also accompany the change of ± in the case of iron at an elevated temperature.

ART. XXIII.—*On the Possible Periodic Changes of the Sun's Apparent Diameter;* by SIMON NEWCOMB and EDWARD S. HOLDEN.

THE question whether the sun's apparent diameter is subject to any changes which can be detected by observation, is one which has frequently engaged the attention of investigators.

In 1809, von Lindenau examined the Greenwich observations of the sun, from 1750 to 1755 and from 1765 to 1786, and he was led to the conclusion that the sun was an ellipsoid with a compression of $\frac{1}{279}$ to $\frac{1}{140}$ (Zach, *Monat. Corr.*, 1809, June).

Bessel, in the following number of the same journal, showed that a progressive shifting of the frame which held the reticle of the Greenwich transit instrument would account for the periodicity in the observed values of the solar diameter, and since that time, the generally accepted conclusion has been, that the figure of the apparent solar disc was circular and its diameter constant.

In Gould's *Astron. Journal*, iii, p. 97, Winlock has given a discussion of the Greenwich observations of Bradley, and in the course of the investigation the varied personal errors of various observers are obtained for the first time.

It is to be noted, moreover, as a point in the history of this question, that Bianchi (*Astr. Nach.*, No. 213, bd. ix, col. 366) rediscussed this subject (1831), apparently without a knowledge of Lindenau's research, and that he found the solar compression to be $\frac{1}{220}$. By different combinations of his data, he, however, obtained values for this quantity varying from $\frac{1}{1925}$ to $\frac{1}{110}$.

Le Verrier (*Annales de l'Obs. de Paris*, tome iv^{ieme}, p. 69) also examined this question, and by a process, which he merely indicates, arrived at the conclusion that no real variation in the sun's diameter so great as $0^s{\cdot}02$ was likely to exist.

Since that time the question has not been directly discussed until it was raised by Secchi, whose observations and conclusions have lately received thorough and searching examination by Auwers (*Monatsberichte der k. Akademie der Wissenschaften zu Berlin*, May, 1873). As the date of this is so recent we shall merely refer to it in passing, calling attention also to certain papers by Respighi and Secchi in the *Comptes Rendus*, 1873.

In the *Vierteljahrschrift* of the German Astronomical Society, January, 1873, Wagner has given a discussion of some of his own transit observations, which show that the state of the sun's image as to sharpness or goodness of definition has a direct influence upon the observed value of the diameter.

That is, if with Wagner an observer assigns a weight to each observed transit of the sun, this weight expressing the goodness of the image as to steadiness and definition, it will be found that each class of observations so defined will give a diameter peculiar to itself, and differing in a constant way from the diameters deduced from the other classes. Dr. Gyldén has found the same thing to be true of observations of the sun's vertical diameter made with the Pulkowa vertical circle, and Dr. Becker of Neuchâtel corroborates Wagner's results for horizontal diameters. We shall have occasion to revert to Wagner's statement, and to show that the observations of both diameters made at Washington in the years 1866 to 1870 entirely confirm it.

The great importance of the conclusions drawn by Secchi from his observations has induced us to test them by a method different from those of Auwers and Wagner. The difficulty which besets this entire subject is to distinguish between actual variations of the sun's diameter and errors of observations. When, like Auwers, we take a number of series of observations extending over a considerable period, we find with great probability that there is no considerable variation with a period varying between several weeks and a year. But the number of observations is so small that entire certainty with respect to small variations, and especially with respect to variations having a period of only a few days, cannot be attained by this method. Whether we take the mean by days or by months, we shall find the mean results for different days and different months to be different, and it will always be impossible to say that these differences are wholly due to errors of observation.

There is another way of considering the subject by which we may hope to attain greater certainty. Suppose we have two series of observations of the sun's diameter made simultaneously at two different observatories, so that each observation of the one series is accompanied by a simultaneous one of the other series. Then, if the outstanding difference between each measure, and the mean of the whole series to which it belongs, is due entirely to the accidental errors of observations, there will be no relation between the differences of the two series. But, if a portion of the difference is due to an actual change in the sun itself, the differences which are positive in the one series will be accompanied by a preponderance of positive differences in the other series. For on the days when the sun is larger than the average, the probability of finding a positive correction will be more than $\frac{1}{2}$ at each observatory, and hence the probability of an agreement of sign will be greater than $\frac{1}{2}$. If the probability in each case be $\frac{1}{2}+\alpha$, it is easy to see that the probability of an agreement will be $\frac{1}{2}+2\alpha^2$.

Our results should, however, depend, not on a simple enumeration and comparison of the signs of the residuals, but also on the magnitude of the latter, and we may secure this dependence by taking the algebraic product of each residual of the one series by the corresponding one of the other. If the residuals are purely accidental, the mean value of these products should approximate to zero as the number of observations is increased, while in the case of actual variability it will approximate to some positive limit. Let us investigate exactly what this limit will be.

If we have two determinations of any quantity, each affected by a common but unknown error s, and also by independent accidental errors r and r', whose law of probability is that usually assumed in the method of least squares, so that the total errors of the two determinations are $s + r$ and $s + r'$ respectively; and if an infinite number of pairs of determinations are made; it is required to find the mean value of the product $(s + r)(s + r')$.

If the measure of precision of the determinations is put equal to unity, the probability that any error of one observation of a pair will fall between the limits $s+r$ and $s+r+dr$ is

$$\frac{1}{\sqrt{\pi}} . e . ^{-rr} dr;$$

the probability that the error of the other observation of the pair will fall between the limits $s+r'$ and $s+r'+dr'$ is

$$\frac{1}{\sqrt{\pi}} . e . ^{-r'r'} dr'.$$

The probability of the combination is therefore

$$\frac{1}{\pi} . e^{-rr} . e^{-r'r'} . dr . dr'.$$

This probability multiplied by the product of the errors is

$$\frac{1}{\pi} . (s+r) e^{-rr} (s+r') e^{-r'r'} dr . dr'.$$

The mean value of the product required is the sum of all these products, as r and r' each varies independently from $+\infty$ to $-\infty$, or the double integral

$$\frac{1}{\pi} . \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} (s+r) e^{-rr} (s+r') e^{-r'r'} . dr . dr'.$$

Integrating first with respect to r' we find

$$\int_{-\infty}^{+\infty} (s+r') e^{-r'r'} dr' = s . \int_{-\infty}^{+\infty} e^{-r'r'} dr' = s\sqrt{\pi} .$$

The double integral therefore becomes

$$\frac{s}{\sqrt{\pi}} \cdot \int_{-\infty}^{+\infty} (s+r) \cdot e^{-r \cdot r} dr = \frac{s}{\sqrt{\pi}} \cdot s\sqrt{\pi} = s^2.$$

The mean value in question is therefore equal to the square of s.

To apply this method to the case in question we should have two series of observations, each sufficiently long and numerous to eliminate every source of personal and systematic error. Being unable to find at hand two series sufficiently extended, made nearly on the same meridian, we have taken for comparison the observations made at Greenwich and Washington during the years 1862–1870 inclusive. The difference of meridian, five hours, will prevent the detection of any inequality of which the period is less than a day, while one with a period of six months or a year will be confounded with errors of observation having that same period, which probably arises from atmospheric condition. But, an inequality, either regular or irregular, of which the period ranges between a day and a half year, will admit of complete detection by the proposed comparison.

All the Greenwich observations which we have used were made with the transit circle: from January, 1862, to January, 1866, the Washington observations were made with the Ertel transit and the Troughton mural circle; after this date the Pistor and Martin's meridian circle was alone used for this purpose.

The method of observation at each place is well known, and it only remains to be said that all transits were registered by chronograph. The observations are distributed as follows:

		No. of Observations.	
		H. D.	V. D.
Greenwich	1862–1870:	832	905
Washington	1862–1865:	491	430
"	1866–1870:	490	491
		1813	1826

Many observers were employed in this work, and it was first necessary to make the observations homogeneous by subtracting from each separate "apparent error of Ephemeris diameter" the "personal error of the observer." These last errors were assumed to be constant throughout a year, and were determined by isolating the work of each observer for each year and by fixing the mean "apparent error of Ephemeris diameter" given by his observations, which was called the "adopted personal error" of that observer for that year. In some cases some slight changes from this rule have been allowed, but the following tables are believed to represent each observer's habit as well as possible from the data.

TABLE OF ADOPTED PERSONAL ERRORS OF GREENWICH OBSERVERS.

Observer.	1862.		1863.		1864.		1865.		1866.		1867.		1868.		1869.		1870.	
	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.
	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″
J. C.	+0.18	+2·5	+0·23	+3·2	+0·17	+3·2	+0·19	+2·7	+0·14	+2·5	+0·17	+2·6	+0·21	+2·9	+0·19	+4·6	+0·20	+3·9
E.	+0.13	−0·2	+0·13	+0·2	+0·05	−0·5	+0·09	−0·9	+0·11	−0·2	+0·12	−0·2	+0·08	−1·0	+0·10	−0·2	+0·09	−1·0
H. C.	—	—	—	—	—	—	—	—	+0·20	−0·2	+0·20	+1·0	+0·20	+1·5	+0·20	+2·1	+0·20	+2·8
S.	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3	+0·10	−0·3
T. C.	+0·15	+0·4	+0·15	+0·4	—	—	+0·15	+0·4	—	—	—	—	—	—	—	—	—	—
K.	—	—	—	—	—	—	−0·02	+0·5	−0·02	+0·5	−0·02	+0·5	−0·02	+0·5	—	—	—	—
W. C.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	+0·18	+1·3
R.	+0·13	+0·2	+0·08	−0·3	+0·03	−0·8	—	—	—	—	—	—	—	—	—	—	—	—
G. K.	—	—	—	—	—	—	—	—	—	—	—	—	+0·05	+1·2	−0·03	0·0	−0·07	−1·0
D.	+0·06	+1·6	+0·06	+2·2	+0·04	+2·4	+0·06	+2·1	+0·04	+2·4	+0·07	+2·2	+0·06	+2·3	+0·09	+2·3	+0·09	+2·3
N.	+0·13	+1.1	+0·13	+1·1	—	—	—	—	—	—	—	—	—	—	—	—	—	—
L.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	+0·10	−1·0
P.	—	—	—	—	+0·20	+1·2	—	—	+0·19	+2·0	+0·19	+2·0	+0·19	+2·0	—	—	+0·21	+0·2
C.	+0·05	+1·2	−0·01	+1·2	−0·01	+1·2	−0·01	+1·2	−0·01	+1·2	+0·04	+1·8	+0·04	+1·8	+0·04	+1·8	+0·04	+1·8

The corresponding table for Washington observers is given below:

TABLE OF ADOPTED PERSONAL ERRORS OF WASHINGTON OBSERVERS.

Observers.	1862.		1863.		1864.		1865.		1866.		1867.		1868.		1869.		1870.	
	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.
	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″
N.	—	—	—	+0·8	—	+1·2	—	+1.3	+0·14	+2·7	+0·14	+3·2	+0·16	+3·8	+0.18	+3·0	—	—
H.	—	—	—	—	—	—	—	—	+0·12	+2·5	+0·08	+1·4	—	—	—	—	—	—
Ha.	—	+0·8	—	+0·4	—	+0·5	—	+0·6	—	—	—	—	+0·16	+0.6	+0.16	+1·4	+0·14	+2·8
E.	—	—	—	—	—	—	—	—	—	—	—	—	+0·03	0·0	—	—	+0·08	+1·6
R.	—	—	—	—	—	—	—	—	−0·01	+0·1	—	—	—	—	—	—	—	—
T.	—	—	—	—	—	—	—	—	+0·09	+1·4	+0·03	+0·6	+0·09	−0·2	+0·10	+0·2	—	—
A.	—	—	—	—	—	—	—	—	—	—	+0·14	+0·6	+0·13	+1·2	—	—	—	—
F.	—	—	—	—	—	—	—	—	—	—	—	—	−0·02	+1·4	+0·04	+3·0	+0·08	+1·2
S.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	+0·14	+2·2
All observers	+0·06	—	+0·07	—	+0·03	—	+0·01	—	—	—	—	—	—	—	—	—	—	—
Hu.	—	0·0	—	+0·9	—	—	—	—	—	—	—	—	—	—	—	—	—	—
He.	—	+0·7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
D.	—	—	—	—	—	+1·4	—	+0·8	—	—	—	—	—	—	—	—	—	—

The next step in the process is to subtract from the apparent error of ephemeris diameter given by each observation the adopted personal error of the observer of that day, thus forming a series of residuals for observations both of horizontal and vertical diameter at both Washington and Greenwich.

The mean monthly residuals for each month of each year were then formed and these are given in the following table:—

HORIZONTAL DIAMETER: GREENWICH.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1862	+0·03	+0·05	−0·06	−0·05	+0·10	−0·01	+0·01	−0·04	+0·03	−0·06	+0·03	+0·03
1863	−0·02	+0·04	+0·01	−0·03	−0·10	−0·01	0·00	−0·02	+0·03	−0·01	+0·06	+0·04
1864	+0·04	+0·09	−0·04	0·00	−0·04	+0·05	−0·02	0·00	−0·07	−0·06	+0·05	+0·09
1865	+0·06	+0·04	0·00	−0·06	+0·04	+0·03	−0·02	+0·01	−0·15	−0·07	+0·01	+0·08
1866	+0·10	+0·03	−0·07	−0·03	−0·04	−0·02	+0·01	−0·08	−0·03	−0·01	+0·04	+0·10
1867	0·00	0·00	+0·01	−0·05	0·00	−0·03	1 obs.	+0·01	+0·02	0·00	+0·04	−0·07
1868	+0·04	−0·01	+0·05	−0·01	−0·08	+0·02	−0·04	+0·01	−0·01	0·00	+0·01	+0·03
1869	+0·04	+0·01	+0·03	0·00	−0·01	−0·02	−0·01	−0·09	+0·02	+0·02	+0·01	0·00
1870	+0·04	+0·02	−0·01	−0·02	−0·05	+0·02	+0·02	+0·02	+0·01	+0·03	+0·01	+0·01
Mean	+0·036	+0·030	−0·009	−0·028	−0·020	+0·003	−0·006	−0·020	−0·016	−0·018	+0·029	+0·034

VERTICAL DIAMETER: GREENWICH.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
1862	+0″·9	+2″·3	+1″·5	−1″·0	+0″·2	0″·0	+0″·1	0″·0	−0″·2	0″·0	−0″·1	+1″·4
1863	−0·3	+0·1	+0·2	+0·6	−0·4	−0·5	−0·4	+0·1	−0·2	−0·1	+0·3	−0·3
1864	+0·7	+1·4	−0·1	0·0	−0·3	−0·2	−0·7	−0·2	−0·4	−0·9	+0·2	+0·2
1865	+0·4	+1·5	+0·9	+0·2	−0·2	0·0	−1·0	+0·3	−0·7	−0·1	+0·3	+0·7
1866	+0·8	−0·1	−1·4	0·0	−1·5	−1·4	−0·5	0·0	0·0	+0·7	+0·6	+2·5
1867	+0·3	+1·8	+0·2	0·0	−0·1	−0·1	1 obs.	−1·4	+0·5	−0·6	+0·7	0·0
1868	+0·6	−0·2	−0·8	−0·1	0·0	+0·2	−0·2	+0·3	−0·1	+0·1	+0·7	−0·3
1869	+0·5	−0·2	−1·3	−0·4	−0·3	0·0	0·0	+0·1	+0·9	−0·9	0·0	+2·7
1870	+0·3	0·0	+0·5	−0·2	−0·8	−0·2	−0·6	+0·3	+0·6	+0·2	+1·1	+1·0
Mean	+0·44	+0·73	−0·03	−0·10	−0·38	−0·24	−0·42	−0·06	+0·04	−0·18	+0·42	+0·88

The corresponding data for Washington are given below.

These monthly means show a decided period which is probably caused, as Wagner has shown, by atmospheric influences.

HORIZONTAL DIAMETER: WASHINGTON.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1862	+0·165	+0·080	+0·060	+0·013	+0·026	+0·037	+0·051	−0·007	−0·026	−0·011	+0·215	−0·046
1863	+0·052	−0·041	−0·003	+0·093	+0·059	+0·081	+0·153	+0·066	−0·015	+0·052	−0·061	−0·014
1864	−0·078	+0·020	−0·068	+0·028	−0·057	−0·063	−0·030	−0·035	------	------	------	------
1865	−0·072	−0·135	+0·013	−0·121	−0·061	−0·045	−0·087	−0·078	−0·079	−0·056	−0·162	−0·110
1866	+0·052	−0·005	−0·091	−0·023	−0·035	+0·008	−0·023	+0·024	+0·040	+0·005	+0·024	−0·020
1867	+0·013	0·000	−0·076	−0·024	+0·010	−0·022	+0·004	+0·028	+0·049	−0·002	+0·035	+0·087
1868	−0·090	0·000	−0·096	+0·003	+0·077	+0·002	−0·005	+0·051	−0·017	−0·042	+0·023	−0·042
1869	+0·004	−0·007	−0·027	+0·040	+0·056	------	------	------	------	------	------	------
1870	------	−0·007	−0·038	−0·017	−0·008	−0·024	−0·004	+0·060	------	------	------	------

VERTICAL DIAMETER: WASHINGTON.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
1862	+1″·7	+0″·4	+1″·4	−0″·9	−0″·7	−0″·5	−0″·7	+0″·9	+1″·2	+1″·2	−0″·3	−0″·9
1863	------	------	------	------	−0·7	0·0	+1·1	0·0	−1·0	−0·8	+0·2	+0·4
1864	−1·9	+0·7	−0·7	+0·3	−0·4	+0·8	+0·5	+0·3	+1·6	+0·6	0·0	+1·0
1865	−0·8	−1·9	−0·1	+0·7	+0·6	+0·5	−0·3	−0·9	+0·9	+0·5	−1·4	0·0
1866	+0·02	+0·32	+0·52	−0·16	−0·14	0·00	+0·59	+0·01	+0·48	−0·04	+0·34	+0·95
1867	−0·20	+0·29	−1·29	−0·43	+0·22	+0·63	+0·08	0·00	−0·72	−0·16	−1·06	+0·97
1868	−1·30	−0·08	−0·20	−0·91	+0·48	+0·05	+0·77	−0·75	+0·23	+1·46	+0·38	+0·64
1869	+0·97	−0·15	+0·80	−0·23	−0·47	------	------	------	------	------	------	------
1870	------	+0·03	0·00	−0·63	−0·76	−0·66	+0·30	−0·07	------	------	------	------

In order to test this all the Washington observations with the transit circle made from January, 1866, to January, 1870 (during which time it was the practice to assign weights to observations), were divided into classes according to their weights. An observation with weight 5 denoted that the image of the sun was steady and well-defined; a weight 1 denoted unsteadiness in the extreme, and bad definition. Observer "R," by a misapprehension, assigned weights by a different rule and his work is accordingly omitted from the present investigation. The following table shows the mean "apparent error of ephemeris" (corrected of course for personal error). The figures in the various columns are, as stated, mean apparent errors of ephemeris, and the small figures below them show the number of observations upon which each number depends.

Weights=	1.		2.		3.		4.		1–2.		2–3.		3–4.	
Year	H.D.	V.D.	H.D.	V.D.	H.D.	V.D.	H.D.	V.D.	H.D.	V.D.	H.D.	V.D.	H.D.	V.D.
	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″	s.	″
1866	−0·130	−1·06	−0·024	−0·06	+0·008	+0·39	+0·045	+0·68	------	------	−0·120	−1·80	+0·180	−0·20
	2	3	32	30	34	36	4	5			1	1	1	1
1867	+0·015	−0·33	−0·010	−0·44	+0·021	+0·01	+0·013	−0·11	+0·100	+0·40	−0·105	+0·93	------	------
	8	6	33	39	40	40	9	7	1	1	4	3		
1868	−0·032	−0·32	−0·026	−0·76	+0·033	+0·60	------	------	−0·040	+1·80	−0·016	−0·32	------	------
	5	5	17	17	18	18			2	3	6	5		
1869	−0·100	+0·10	−0·028	+0·24	+0 047	+0·30	------	------	------	------	−0·013	+0 66	------	------
	2	2	10	9	12	10					6	7		

In Wagner's paper, above referred to, sufficient data are given to allow his work to be treated in a like way, and his observations give when so treated the following results, which agree, in general, with the figures in the above table.

Pulkowa Class	IV.	s. −0·055	Wash. Weight	2–3	s. −0·063
	IV–III.	−0·020	"		
	III–IV.	−0·002	"	3	+0·027
	III.	+0·029	"	3–4	+0·180

Although the weights of Washington observations were assigned by as many as six or seven different observers, each by a different standard, the agreement between the resulting errors of ephemeris diameter at Washington and Pulkowa is evident, in regard to sign at least.

To show how rough a division of sun observations according to state of image will exhibit the effect of good definition upon the deduced values of diameter, we further divided the Washington observations into two classes: class 1 comprises those observations made when the cloudiness of the sky was 0; class 2 comprises those made when the cloudiness ranged from 5 to 10. (0=clear sky, 10=all overcast.) These numbers 0–10 were but the rough estimates of the three watchmen of the observatory, made at noon: and yet the following table shows strongly the effect of the cloudiness upon the diameter deduced from observation.

	Horizontal Diameter.		Vertical Diameter.	
Year.	Cloud=0:	Cloud 5–10.	Cloud=0:	Cloud 5–10.
	s	s	″	″
1867	−0·033	+0·012	−0·45	+0·35
1868	−0·047	+0.030	−1·45	+0·45
1869	0·000	+0·039	+0·75	+0·11
1870	−0·093	+0·014	−0·70	−0·44

The numbers in the various columns are again the mean apparent errors of ephemeris diameter (corrected). The cause of the periodicity in the monthly means being now understood, it remained to free the separate residuals from periodic error, which was done by the application of corrections derived from the following formulæ :—

For Horizontal Diameter.

Greenwich 1862–70 : $-0^s{\cdot}023 \cos\theta - 0^s{\cdot}006 \sin\theta - 0^s{\cdot}016 \cos 2\theta - 0^s{\cdot}011 \sin 2\theta$.

Washington 1862–65 : $+0^s{\cdot}008 \cos\theta - 0^s{\cdot}010 \sin\theta$.

" 1866–70 : $+0^s{\cdot}001 \cos\theta - 0^s{\cdot}019 \sin\theta$.

For Vertical Diameter.

Greenwich 1862–70 : $-0''{\cdot}47 \cos\theta - 0''{\cdot}13 \sin\theta - 0''{\cdot}07 \cos 2\theta - 0''{\cdot}12 \sin 2\theta$.

Washington 1862–65 : $+0''{\cdot}09 \cos\theta + 0''{\cdot}21 \sin\theta$.

" 1865–70 : $-0''{\cdot}09 \cos\theta + 0''{\cdot}02 \sin\theta$.

Each residual error of ephemeris diameter (diminished by the corresponding personal error) was now further corrected by the application of a correction derived from the above formulæ, and a series of residuals formed. Whenever a Washington and a Greenwich observation were made on the same day, the corresponding corrected residuals were multiplied together, and the sums of these products were tabulated as below.

Year.	Sums of Products.		Number of Corresponding Observations.	
	Hor. Diam.	Vert. Diam.	Hor. Diam.	Vert. Diam.
	s.	"		
1862	−0·0492	−19·69	33	24
63	+0·0007	− 2·10	40	25
64	−0·0185	−12·55	44	24
65	+0·0094	+13·87	49	41
66	+0·0151	+ 7·20	48	54
67	−0·0364	− 8·41	31	28
68	+0·0122	+ 2·67	37	39
69	+0·0433	+ 6·73	6	11
1870	−0·0843	−10·86	25	25
Σ	$-0^s{\cdot}1077$	$-23''{\cdot}14$	313	271

We see by this table that there is a decided preponderance of negative products. This result seems conclusive against the assumption of any sensible variability of which the period, regular or irregular, lies between one day and six months. In fact, if we regard the preponderance as due not to chance, but to some systematic tendency, it would show that the greater diameters at Greenwich corresponded to the smaller ones at Washington, and *vice versa:* a result which could arise only from a tendency to vibrations of short period, probably not differing much from 10 hours. We are, however, inclined to attribute this result to chance. The mean value of the product of two residuals is about ·^{s}007 in horizontal diameter, and 1″·5 in vertical diameter. In the number of products added up, the accidental accumulations of products of one sign might very well amount to 15 times this mean, while the sums for the individual years are not, on the whole, materially greater than would arise from chance accumulation. Were it not so, the most remarkable feature of the table would be the correspondence of sign between the sums of residuals of vertical and horizontal diameter for each year, which we could not expect more than 10 times in $512=2^9$ trials. This, if not accidental, would indicate that during some years, 1864 and 1870, for instance, there was a tendency to a ten hour vibration of the solar diameter. From what has been said, we are not authorized to attribute this correspondence to anything but chance.

ART. XXIV.—*A New Calculating Machine;* by GEORGE B. GRANT.

"SINCE the dawn of mathematical science in Europe, the attempt to construct a machine, capable of satisfactorily performing arithmetical operations, has occupied the attention of a great number of ingenious men, several of whom have been among the most celebrated of their time for originality of genius and for the large contribution which they have made to the progress of science."*

Leaving out of mention the Greek abacus and the Chinese schwan-pan of ancient times, the first recorded attack on this difficult problem was made in the tenth century by Gerbert, Chancellor of France and afterward Pope under the title of Sylvester II. He is credited with the introduction into Europe of the arabic numerals, and endeavored to construct a mechanism to facilitate their use. But of his results we have no published account.

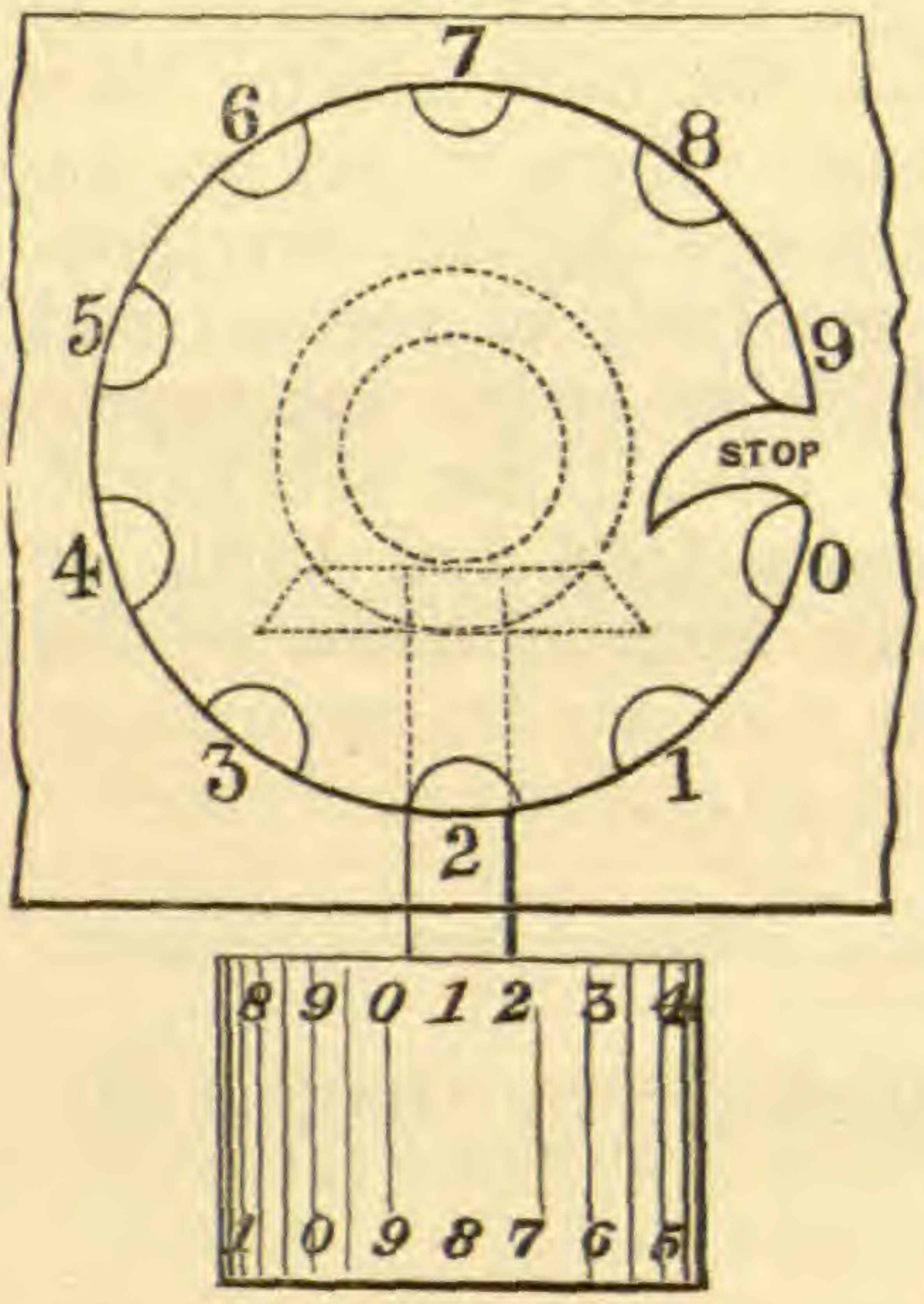
The first successful device was the invention of John Napier, celebrated for his invention of logarithms. His "rhabdology," or as they are better known, "Napier's bones," can hardly be classed as mechanism, and they are too well known to need description here.

The first actual machine of which we have detailed information was the invention of no less a man than Blaise Pascal, the distinguished philosopher of France, who in 1645 published an account of his "arithmetical machine," on the invention of which he had spent several years.

* President Barnard of Columbia College, in the U. S. Reports of the Exposition of 1867. "The Industrial Arts and Exact Sciences." Harper & Brothers. New York. 1867.

The diagram, fig. 1, will illustrate Pascal's design sufficiently for the purposes of this article. A horizontal wheel having ten teeth turns in an opening in the fixed upper plate of the machine, and about the opening are the ten numerals. A pencil placed between the teeth at any number and brought round to the stop will turn the wheel through that number of teeth and record it on the large figured cylinder. The cylinder is provided with two rows of figures complementary to each other, so that the result is addition or subtraction, according as we read by one row or the other. Carriage was accomplished by mechanism on the shaft of the cylinder, which forced the next cylinder forward one figure whenever its own passed from 9 to 0

1.



PASCAL'S MACHINE.

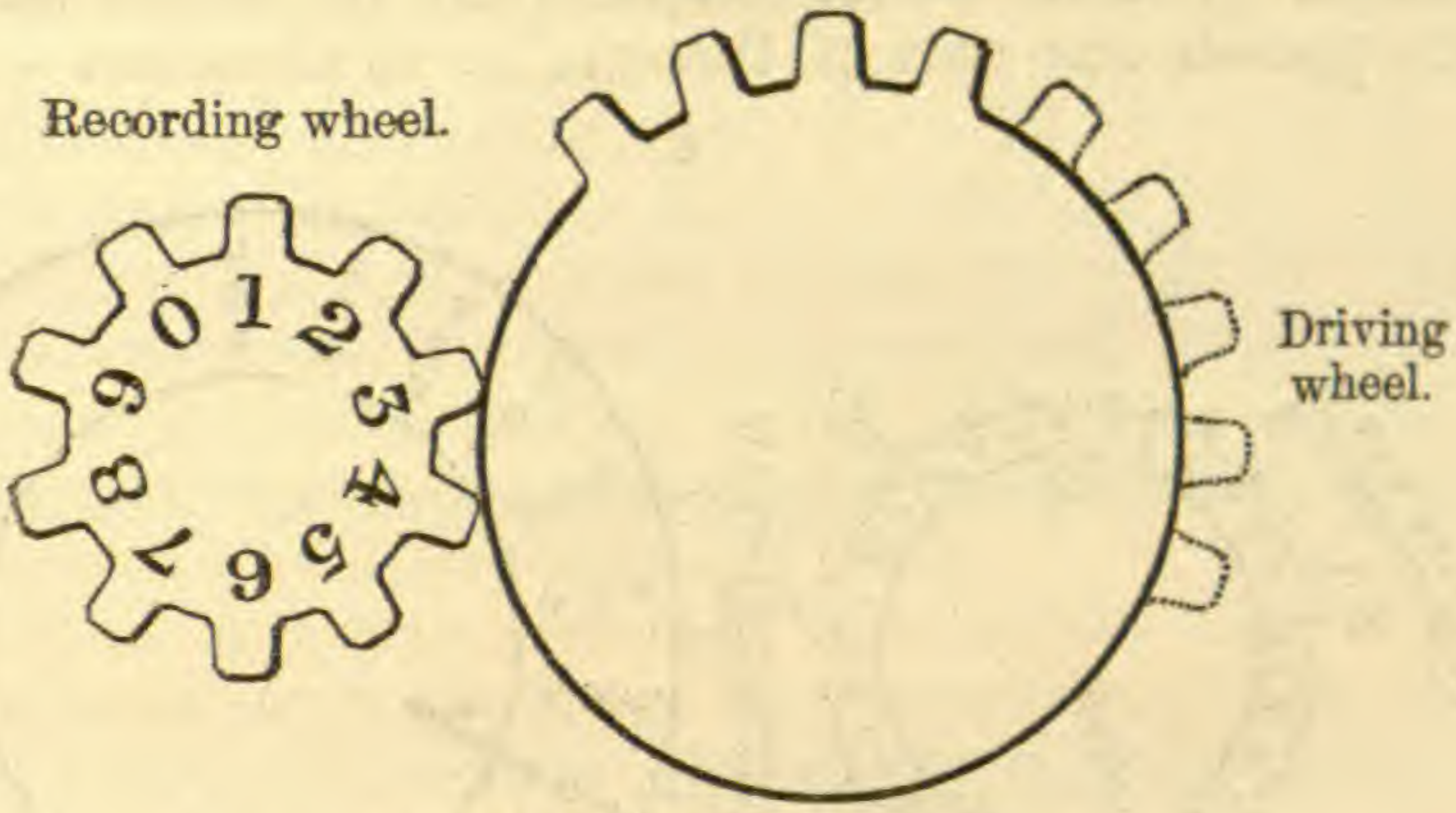
Pascal's machine was not a success, for though correct in theory it was so complicated, delicate, uncertain, and limited in its operations, that it was practically useless. But the principles of its action, particularly the toothed wheel, fixed figured arc and stop, and the complementary rows of figures, have appeared in most subsequent machines, and have been patented many times as new both in this country and in Europe.

Without giving a complete history of the subject, I can mention the names of Roger Bacon, Leibnitz—the inventor of the Infinitesimal Calculus, Diderot, Gersten, and Sir Samuel Morland, as among those who have paid particular attention to this subject.

The next attempt resulted in a substantial success, and by a man otherwise entirely unknown. Charles Xavier Thomas de Colmar patented in 1822 his "arithmometer," a machine that not only solved the long tried problem, but solved it practically, and which is now in use in large numbers for actual work. The able description, in the United States Reports above referred to, of this machine and of the subject in general, is so accessible to the public, that a detailed account is unnecessary here

Colmar's invention has been directly or indirectly adopted by every inventor who has successfully appeared in public since his time. The principle of his invention is shown by the diagram fig. 2, in which a recording wheel is in gear with a driving wheel having a variable number of teeth, and the number added to the recording wheel at each turn of the driver will depend on the number of teeth exposed. Colmar varied his number by placing nine rows of teeth side by side,

2.



COLMAR'S PRINCIPLE.

having from one to nine teeth each, and made the recording wheel movable, to be placed to gear with either row at pleasure. Other inventors, as Staffel of Russia, have made the teeth separately removable, so that those not wanted could be put out of gear. Another has hung the driver on a movable axis and provided means for meshing the two wheels at the proper place for the desired number of teeth to act. Still another spreads Colmar's nine rows of teeth out on a plane, and moves the recording wheel over them.

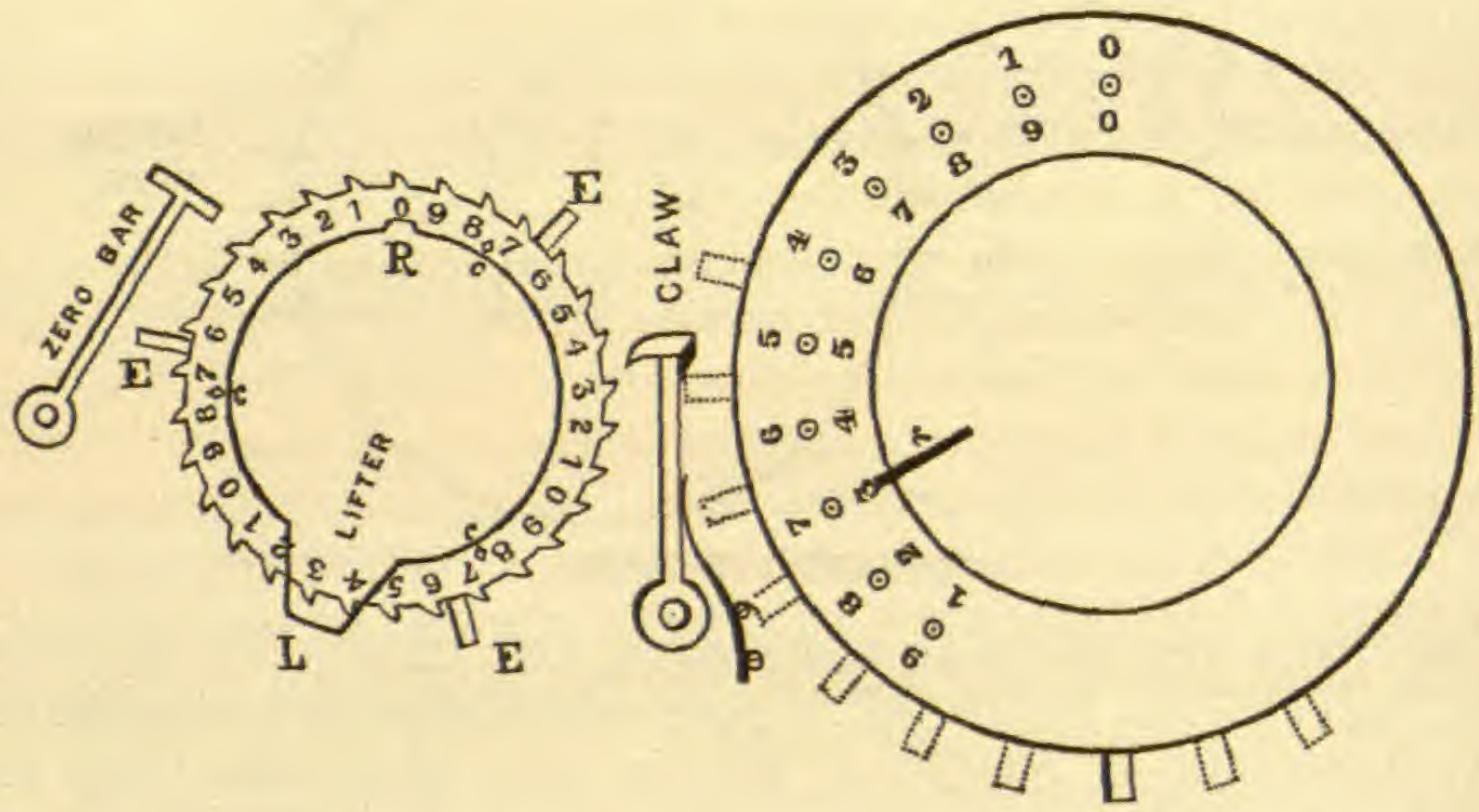
Calculating machines by the score may be found in the patent records of the United States, England and France. Colmar's idea has been twisted into every conceivable position, but no improvement has been made on the disposition originally adopted by him. And his machine is the only one now in use, to any mentionable extent.

Colmar's machine, as the first solution of an old and well studied problem, and as a specimen of the mechanism of his day, certainly deserves the highest praise. But it is undenied that it is in a high degree complicated, a mass of small cog wheels and delicate mechanism, which require close adjustments, and which easily get out of order, requiring the greatest care in handling and the best skill in repairing. The needs of the present time require, and the state of the art at this day admits of, a better design, one that is simpler and of more substantial construction, easier to put in order and easier to keep in order, and better suited for the use of those but little accustomed to machinery. And as such we will introduce the subject of this paper.

The New Machine.—In the diagram fig. 3 all framing and unessential parts are omitted, and the positions so arranged as to show the principle of action most clearly.

Two parallel cylinders are geared to turn together. One cylinder is larger than the other, but the gears are equal, so that one turn of the handle on the larger revolves both once. The larger cylinder slides laterally on its arbor, and can be placed opposite any part of the smaller at pleasure.

3.



On the small cylinder are a number of recording wheels, more or less according to the capacity desired. Each is provided with thirty teeth, and a numeral is stamped at each tooth. A fixed point, R, is chosen as the reading point, and the number shown at any time at that point is the reading of the wheel.

On the large cylinder are a number of driving wheels, each having an adding pin, P, which can be fixed in ten different positions by the pin at *r*.

On a bar between the cylinders is a row of fixed spring claws, one for each recording wheel. If the claw be pushed slightly to one side, it will drop off its catching pin on to the wheel and hold it.

As the handle is turned, the recording wheel revolves with its cylinder, but when the adding pin strikes and lets down the claw, it will be held still till the lifter L is reached, by which the claw is returned to its pin and the wheel allowed to pass on. It has, by being held, been carried over a number of teeth from its original reading, more or less according to the position of the adding pin on its cylinder. If the adding pin is placed at its zero position, it will come to the claw simultaneously with the lifter, and the wheel will not be affected. But if it be placed at 7 for example, it will reach the claw seven teeth in advance of the lifter, and the number seven will be added to the wheel.

The action between each wheel claw and adding pin is the same, and it is plain that the number represented by the setting of the adding pins will be transferred to the recording wheels at each turn of the handle. If now the larger cylinder be set up one space, each pin will act on the next wheel above and ten times the number set up will be added.

The carriers for effecting the carriage of the tens are placed, one for each wheel, between it and the next one higher, each one being a tooth in advance of the preceding one. It is a simple lever fixed on the cylinder behind the lifter, L. As the wheel passes from 9 to 0, a stud *c* upon it will strike the carrier and throw it over slightly. When over, it is in the path of the next claw above, and will throw it off so as to add one to its wheel before reaching a second lifter.

The machine as above described is complete and ready for work, all that is essential being the recording wheels, adding pins, claws, lifters, and carriers. But for the sake of convenience and efficiency, various attachments might be added. As for instance, a ratchet and click to prevent backward motion of the handle, a counter to register the turns of the handle, and for some purposes a printing apparatus, to record the results of the work.

Eraser.—A valuable attachment always found on an efficient calculating machine is an apparatus by which a result may be erased and all the wheels brought to zero at once. This erasure is required before each operation, and to do it by hand, one wheel at a time would be tedious and inaccurate.

Projecting from the wheel is the erasing pin, E, and fixed on the side of the frame is the zero bar. This bar is common to all the wheels, and is ordinarily up out of the way of the pins, but when pressed down will be in their path. If then the cylinder be turned backward, each pin will stop when it reaches the bar, and all the wheels will be brought to zero simultaneously.

The process by this machine is always an addition, never a subtraction. But subtraction of any number is accomplished by setting it up by the inner row of figures on the adding wheel, they being so arranged that the complement of the number set up will be used.

The size of the machine varies with its capacity. The recording wheels are $1\frac{1}{4}$ inches and the adding wheels $2\frac{1}{2}$ inches in diameter, and the distance from wheel to wheel is three-eighths of an inch. A ten-wheel machine would occupy a box $6 \times 6 \times 4$ inches in dimensions.

The compound action of the claw being thrown from its pin, catching on the wheel, carrying it along a definite distance, rising and catching again, may be considered complicated and delicate. But the fact is it is very reliable: a poorly made apparatus has been worked at the rate of 10,000 operations per minute with perfect accuracy. The object of having one cylinder nearly three times the size of the other is to secure this accuracy. The angular motions are equal, but the actual speed of the pin is nearly twice that of the wheel, ensuring that the claw shall strike the right tooth every time, even if the parts are not precisely in their proper places.

Operation.—Multiplication is accomplished by the principle of successive addition of the multiplicand to itself, first used mechanically by Colmar, and in fact the only practical and reliable principle yet proposed. Since the invention of Napier's bones, many have tried to give them a more mechanical shape, and to use them in automatic machinery. But though it has been done in several ways, the result has always been too complicated, clumsy and costly, to compete with the theoretically more roundabout, but practically simpler and shorter method borrowed by Colmar from the mental process.

For an example let us multiply 657 by 325. The adding pins are first set to the number 657 and the recording wheels brought to zero by the bar, after which five turns of the handle will transfer it five times on to the wheels and 5×657, or 3,285, will appear. Then set the cylinder up one space, and two turns will multiply 657 by 2 and add ten times the product, or 13,140, to the 3,285 previously shown. Another shift of the pins and three turns will complete the operation and show the final product 213,525 in plain figures. This result can be obtained in ten seconds, allowing one-half a second for each turn and each figure set up, and one second for erasing and for each shift of the cylinder.

The larger the numbers used the greater the proportional gain over mental labor; for the machine will add ten figures to ten as quick as one to one, while mentally it would take ten times as long. But the greatest advantage of the mechanical method is not in the time saved, but in the superior ease and accuracy with which the work is done.

Reduction of Star Places.—If the result ab of any multiplication is not erased, the result of the next cd will be $ab+cd$, and if either c or d are set up negatively the result is $ab-cd$. By an attachment, this machine is peculiarly adapted to work the quantity $Aa+Bb+Cc+Dd+$ &c. for star reductions, where the same values of ABCD or of *abcd* are used for a large number of operations. Four or more fixed quantities, ABCD, etc., can be set up once for all, and either one be quickly brought to act at pleasure to the exclusion of the others, it not being necessary to set it up figure by figure. With this attachment, reductions to apparent place may be made at the rate of two per minute, whereas it usually takes from three to five minutes for each operation.

Addition and subtraction are of course worked directly, but in common with all machines yet contrived, it offers small advantages over the common method, except in regard to ease of execution. For large work of four or more places it would be found useful, but to ordinary accounts no machine has yet been profitably or extensively applied.

For division two different processes may be employed. One, the old tentative process used by Colmar, is nothing but the usual mental method put into mechanical shape. The other is an improvement on the first, by which it is rendered automatic and entirely independent of mental labor. It is peculiar to this machine, and presents the first solution of the problem of mechanical division.

By the tentative method we first set up our dividend 213525 on the wheels by hand, or better, by transferring it from the pins. We then set the pins to our divisor 0657, by means of the inner or negative rows of figures, taking care to leave one zero in advance of it. Then place it up opposite the 135 of the dividend, and turn the handle, stopping at every turn to observe the dividend. It will continually decrease, and when you perceive that it is less than the divisor, you must stop and set the pins down one place before proceeding to the next figure. In the above case, the dividend after three turns will read 16425, and that being less than 65700 the first quotient figure is 3. The wheels will then read 00003016425, the quotient figure being recorded automatically by the machine on the upper wheels left vacant by the retreating dividend.

To explain the automatic method we need to follow the process of continual subtraction a little more closely.

213525	
9999343	−1
147825	
9999343	−2
82125	
9999343	−3
16425	
9999343	−4
999950725	
657	+1
16425	

As 9999343, the complement of our divisor 657, is added, the divisor decreases, and after three divisions is reduced to 16425, a number less than 65700, requiring us by the old method to stop and set down for the next figure. And it is necessary to watch for this point, for mechanism to determine that 16425 is less than 65700 has never yet been contrived, and it is doubtful if a simple apparatus could be devised for that purpose.

But it may be observed that if 65700 is subtracted from 16425 once more, that a negative number would result. And where, as on this machine, a negative number is expressed by its complement, its mechanical perception is an easy matter, since for such a case the upper wheels all read nine. A snap, which will indicate when the last wheel stops on nine, will answer our purpose, and warn us that a mistake of one turn has been made. Having made a mistake, we must correct it by adding our divisor once, and bring the dividend back to a positive number, ready to set down for the remaining figures.

The problem of the calculating machine is an exceedingly difficult one, as anyone acquainted with the immense labors of Pascal, Leibnitz, Babbage and Scheutz will acknowledge. Pascal speaks of his invention as "a work of some years." Leibnitz, at the height of his fame, devoted four years to this object, and failed; Babbage worked from 1822 to 1842 on his Difference Engine to no purpose; and Scheutz was from 1834 to 1854 bringing his machine to the partially successful condition it is now in. The machine described above for the first time is the result of nearly four years of study and labor.

Cambridge, Mass., July 15, 1874.

ART. XXV.—*Researches on the Hexatomic compounds of Cobalt*; by WOLCOTT GIBBS, M.D.

[Continued from page 200.]

Bromo-nitrate of xanthocobalt.—One molecule of bromide of xanthocobalt was mixed with one of the nitrate of the same base, both salts being in solution in hot water. A dark, sherry-wine-colored salt separated, after some hours, in well defined crystals. In this salt

0·8925 gr. gave 0·4190 gr. SO_4Co=17·86 per cent cobalt.
0·7116 gr. gave 0·1244 gr. silver =12·94 per cent bromine.

The formula $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Br_2$ requires 17·77 per cent cobalt, and 24·09 per cent bromine. The salt was redissolved and allowed to crystallize a second time. In the salt thus obtained

0·8538 gr. gave 0·3984 gr. SO_4Co=17·76 per cent cobalt.
0·8474 gr. gave 0·2672 gr. silver =23·62 per cent bromine.

These results leave no doubt that a definite bromo-nitrate, analogous to the chloro-nitrate, is formed by direct union of the nitrate and bromide. The salt appears to be, however, much less stable than the corresponding chlorine salt. A portion of it was crystallized a third time, and then gave 23·04 per cent of bromine, indicating the commencement of a separation into bromide and nitrate. The facility with which the chloro-nitrate is formed by the direct union of its constituents, led me to attempt the formation of other new salts by a similar process. I mixed one molecule of chloride of purpureocobalt, and one of nitrate of xanthocobalt, in the hope of obtaining a salt with the formula $Co_2(NH_3)_{10}(NO_2)(NO_3)_2Cl_3$, since

$$Co_2(NH_3)_{10}Cl_6+Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4=$$
$$2Co_2(NH_3)_{10}(NO_2)(NO_3)_2Cl_3.$$

After boiling the mixture with a little free acetic acid, the solution deposited on cooling deep orange-red, apparently homogeneous, crystals. Of these

0·3145 gr. gave 0·1746 gr. SO_4Co=21·13 per cent cobalt.
0·9203 gr. gave 0·5080 gr. silver =17·99 per cent chlorine.

The formula $Co_2(NH_3)_{10}(NO_2)(NO_3)_2Cl_3$ requires 20·90 cobalt, and 18·86 per cent chlorine. The analyses seem to show that a salt having the composition given may exist. On recrystallization, the salt was more or less completely decomposed, as the following analyses show:

0·2125 gr. gave 0·1161 gr. SO_4Co=20·80 per cent cobalt.
0·5933 gr. gave 0·2470 gr. silver =13·70 per cent chlorine.
0·7888 gr. gave 0·3308 gr. silver =13·78 per cent chlorine.

These numbers approximate to those required by the formula, $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2$. I attempted in like manner to form salts synthetically by mixing other salts in the proportions indicated by the equations:

$$Co_2(NH_3)_{10}(NO_2)_6+Co_2(NH_3)_{10}Cl_6=Co_2(NH_3)_{10}(NO_2)_3Cl_3.$$
$$Co_2(NH_3)_{10}(NO_3)_6+Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4=$$
$$Co_2(NH_3)_{10}(NO_2)(NO_3)_5.$$

The experiments led, however, to no definite results.

The chloro-nitrate above described is the salt to which I, at one time, attributed the formula

$$Co_2(NH_3)_{10}(NO_2)_4Cl_2+2OH_2,$$

and which I regarded as the chloride of a special radical, "flavo-cobalt," $Co_2(NH_3)_{10}(NO_2)_4$. The mere analyses can hardly distinguish with certainty between the two formulas, and I was for some time misled by an erroneous interpretation of my results. The compounds of cobalt containing ammonia and nitroxyl, NO_2, have in general the same color, and differ but little in solubility, so that it is extremely difficult to separate them, and in my analyses of what I believed to be the sulphate and nitrate of the same base, I had undoubtedly to deal with impure salts of xanthocobalt. Krok* has described a salt with the formula $Co_2(NH_3)_{10}Cl(NO_2)_5+3OH_2$. There is no theoretical reason why such a compound should not exist, but Krok's analyses do not appear to me sufficient, as the cobalt, chlorine and ammonia only were determined, and not the whole quantity of nitrogen in the salts. Moreover, it is not proved that the salt can be recrystallized without decomposition, or that it forms definite compounds with metallic chlorides.

* Acta Univers., Lund, 1870.

As the chloride and nitrate of xanthocobalt are capable of uniting directly to form the chloro-nitrate above described, it might be supposed that the two salts are isomorphous, and, therefore, crystallize together in all proportions. According to Prof. Dana's measurements, cited in the first part of this memoir, nitrate of xanthocobalt crystallizes in forms belonging to the dimetric or square prismatic system. Prof. Cooke has kindly determined the form of the corresponding chloride, and finds that the crystals are either trimetric or monoclinic. The chloro-nitrate cannot, therefore, be regarded as a mixture of two isomorphous salts.

11. Finally, salts of xanthocobalt are formed by the action of Fischer's salt, $Co_2(NO_2)_{12}K_6$, upon salts of purpureocobalt and roseocobalt. When, for instance, chloride of purpureocobalt is dissolved in boiling water, with a little free acetic or chlorhydric acid, and $Co_2(NO_2)_{12}K_6$ is added, in small portions at a time, the violet color of the salt gradually disappears as the boiling continues, and the solution finally assumes a fine orange-brown tint. The filtered solution gives on cooling fine crystals of chloride of xanthocobalt, the reaction being probably expressed by the equation

$$Co_2(NO_2)_{12}K_6+3Co_2(NH_3)_{10}Cl_6=3Co_2(NH_3)_{10}(NO_2)_2Cl_4+$$
$$6KCl+2Co(NO_2)_2+2NO_2.$$

During the boiling red vapors are given off. In one experiment the chloride of xanthocobalt formed was analyzed, with the following results:

0·5027 gr. gave 0·2987 gr. SO_4Co=22·62 per cent cobalt.
0·7616 gr. gave 0·6351 gr. silver =27·35 per cent chlorine.

The formula $Co_2(NH_3)_{10}(NO_2)_2Cl_4$ requires 22·52 per cent cobalt and 27·09 per cent chlorine. The salt gave all the reactions of the chloride.

On the other hand, Fischer's salt is an almost constant product of the action of the alkaline nitrites upon salts of the decamin series. I have already mentioned its occurrence among the products of the action of potassic and sodic nitrite upon chloride of purpureocobalt. When nitrate of xanthocobalt is boiled with potassic nitrite and a little acetic acid, Fischer's salt is formed in abundance, and the nitrate is gradually decomposed, without formation of any other product which I could detect.

Chromate.—When neutral potassic chromate is added to a solution of nitrate of xanthocobalt, a beautiful yellow crystalline precipitate is thrown down, which may be washed with cold water, in which it is but slightly soluble. Hot water also dissolves this salt in very small quantity. The chromate has the formula

$$Co_2(NH_3)_{10}(NO_2)_2(CrO_4)_2+2OH_2$$

as the following analyses show:

0·4340 gr. gave 0·3652 gr. CrO_4Ba=35·96 per cent CrO_4.
0·3472 gr. gave 0·2900 gr. CrO_4Ba=35·70 per cent CrO_4.
0·6954 gr. gave 0·3370 gr. water = 5·38 per cent hydrogen.

The salt lost only 0·68 per cent water on drying up to 145° C.

The formula requires 35·84 per cent CrO_4, and 5·24 per cent hydrogen. It is remarkable that the salt should retain its water of crystallization at so high a temperature. The neutral chromate of xanthocobalt furnishes the most convenient method of obtaining the chloride and sulphate of xanthocobalt in a state of purity. For this purpose the chromate is to be boiled with water and a little acetic acid, and a solution of baric chloride added until baric chromate is no longer formed. From the filtrate the chloride of xanthocobalt crystallizes readily, and a second crystallization gives the salt perfectly pure. The sulphate may then be prepared from the chloride by double decomposition with argentic sulphate. In the preparation of the chloride by the above process, it is not necessary to operate with pure nitrate, but the crude salt and solutions obtained directly by the action of the red gases upon cobaltic nitrate and ammonia may be employed. I am even disposed to consider double decomposition of the chromate with baric nitrate the easiest method of obtaining a perfectly pure nitrate of xanthocobalt.

Dichromate.—Potassic dichromate produces in strong solutions of nitrate of xanthocobalt a beautiful orange-yellow precipitate of crystalline needles, easily purified by recrystallization, a few drops of acetic acid being added to prevent decomposition. The salt is easily soluble in hot water, and crystallizes readily, though not in well defined crystals, from the solution. Like the neutral chromate, it is available as a means of recognizing salts of xanthocobalt, and of obtaining them in a state of purity. Of this salt

0·6570 gr. gave 0·8200 gr. CrO_4Ba=53·33 per cent Cr_2O_7.
0·3974 gr. gave 0·4950 gr. CrO_4Ba=53·23 per cent Cr_2O_7.
0·4868 gr. gave 0·1830 gr. Cr_2O_3 =53·40 per cent Cr_2O_7.

The formula $Co_2(NH_3)_{10}(NO_2)_2(Cr_2O_7)_2$ requires 53·22 per cent.

Iodosulphates.—A solution of potassic iodide gives no precipitate at first with one of nitrate of xanthocobalt, but after standing some time, pale brown-yellow acicular crystals of the iodide $Co_2(NH_3)_{10}(NO_2)_2I_4$ are formed. When a solution of iodine in potassic iodide is added to one of nitrate of xanthocobalt, iodine is precipitated in crystals, but no hyperiodide is formed, as in the case of the iodide of the hexamin series already described. Potassic iodide gives, with a solution of sulphate of xanthocobalt, brown yellow needles, which, after re-solution, gave larger prismatic crystals. Of these

0·5396 gr. gave 0·2207 gr. SO_4Co=15·57 per cent cobalt.
0·8856 gr. gave 0·2689 gr. SO_4Ba=12·51 per cent SO_4.
0·4541 gr. gave 0·1288 gr. silver =33·37 per cent iodine.

The formula $Co_2(NH_3)_{10}(NO_2)_2SO_4I_2+2OH_2$ requires

		Calculated.	Found.
Cobalt,	2	15·40	15·57
Iodine,	2	33·16	33·37
SO_4	1	12·53	12·51

When a solution of iodine in potassic iodide is added to one of sulphate of xanthocobalt, very beautiful, deep ruby-red, well defined crystals are formed, which are readily decomposed by hot water, with evolution of iodine vapor, and cannot be recrystallized for analysis. Of these crystals

0·6094 gr. gave 86·5 c.c. nitrogen at 13° C. and 758·6 mm. (h= 2·08 mm.)=16·63 per cent nitrogen.
0·2142 gr. gave 0·0687 gr. SO_4Co=12·21 per cent cobalt.
0·6104 gr. gave 0·1870 gr. SO_4Co=11·64 per cent cobalt.
0·3940 gr. gave 0·1672 gr. silver =49·90 per cent iodine.
0·5437 gr. gave 0·2310 gr. silver =49·96 per cent iodine.
0·3020 gr. gave 0·0724 gr. SO_4Ba= 9·87 per cent SO_4.
1·0627 gr. gave 0·2787 gr. SO_4Ba=10·80 per cent SO_4.

The formula $Co_2(NH_3)_{10}(NO_2)_2SO_4I_4$ requires

		Calculated.	Found. 1	Found. 2
Cobalt,	2	11·99	12·21	11·64
Iodine,	4	51·60	49·90	49·96
SO_4	1	9·75	9·77	10·80
Nitrogen,	12	17·07		16·63

Salts 1 and 2 were from different preparations.

The analyses do not correspond as closely to the formula as might be wished, but it must be remembered that the salt cannot be recrystallized without decomposition, and is probably not quite free from the first described, or normal iodo-sulphate. The salt gives off iodine on heating. The structural formulas of the two salts may be written as follows:

$$Co_2\left\{\begin{array}{l} NH_3-NO_2 \\ NH_3-NH_3-I \\ NH_3-NH_3 \\ NH_3-NH_3 \\ NH_3-NH_3-I \\ NH_3-NO_2 \end{array}\right. \!\!\begin{array}{l} \\ \\ \big>SO_4 \\ \\ \\ \end{array} \qquad Co_2\left\{\begin{array}{l} NH_3-NO_2 \\ NH_3-NH_3-I \\ NH_3-NH_3-O \\ NH_3-NH_3-O \\ NH_3-NH_3-I \\ NH_3-NO_2 \end{array}\right. \!\!\begin{array}{l} \\ \\ \big>S\big<\begin{array}{l} O-I \\ O-I \end{array} \\ \\ \\ \end{array}$$

This mode of writing the formulas, however, involves certain theoretical conclusions, which I shall examine in detail hereafter I added $PtCl_6Na_2$ to a solution of sulphate of xanthocobalt, hoping to obtain a salt with the formula $Co_2(NH_3)_{10}(NO_2)_2(SO_4)Cl_2(PtCl_6)$, analogous to a platinum salt of roseocobalt, which I shall hereafter describe, and which has the formula $Co_2(NH_3)_{10}(SO_4)_2PtCl_6$. The beautiful crystalline precipitate formed proved to be only the salt $Co_2(NH_3)_{10}(NO_2)_2Cl_2PtCl_6+OH_2$, described in the first part of this memoir. 0·3882 gr. gave 0·1612 gr. Co+Pt=41·52. The formula requires 41·39 per cent.

Nitrite of xanthocobalt.—When argentic nitrite is boiled with a solution of chloride of purpureocobalt, the liquid soon loses its fine violet color, and assumes the wine-yellow tint of the salts of xanthocobalt. The filtrate from the argentic chloride gave, on careful evaporation, two distinct salts—a salt in beautiful scaly crystals, and one in octahedral crystals. The two salts were separated by crystallization. Of the scaly salt

0·2854 gr. gave 0·2286 gr. $SO_4Co+SO_4Ag_2$=79·97 per cent.

The formula of the ammonia-cobalt-nitrite, $Co_2(NH_3)_4(NO_2)_8Ag_2$ requires 80·75 per cent, and the salt was easily identified, by its appearance and properties, with the silver salt of Erdmann's series. As the octahedral salt was rather difficult to obtain perfectly pure by this method, I had recourse to the decomposition of sulphate of roseocobalt by baric nitrite. A solution of the last named salt is to be added to one of the sulphate as long as a precipitate is formed. The sherry-wine-colored filtrate is then to be cautiously evaporated, when fine dark wine-colored octahedral crystals form. Of these crystals

0·4750 gr. gave 0·2303 gr. SO_4Co=18·46 per cent cobalt.
0·1220 gr. gave 0·0594 gr. SO_4Co=18·54 per cent cobalt.
0·3129 gr. gave 0·0403 gr. water, when heated to 140° C. =12·87 per cent.
0·4289 gr. gave 0·1141 gr. ammonia=26·60 per cent.

The formula $Co_2(NH_3)_{10}(NO_2)_6+4OH_2$ requires

		Calculated.	Found.	
Cobalt,	2	18·55	18·46	18·54
Ammonia,	10	26·72	26·60	
Water,	4	11·32	12·87	

The percentage of water in the analysis is too high, and would seem to show that a slight decomposition of the salt had taken place. I attempted to determine the percentage of NO_2 in this salt by titrition with potassic hypermanganate, but though the analyses were made with the greatest care, I obtained as a mean of three determinations, agreeing well together, only 11·24 per cent, which would correspond to less than two atoms. In other cases also I found that the method could not be employed.

So far as the empirical formula is concerned, the salt may be regarded as a nitrite of purpureocobalt or roseocobalt, $Co_2(NH_3)_{10}(NO_2)_6+4OH_2$. Its solution gives, however, the reactions of salts of xanthocobalt with the greatest distinctness, and I regard it, therefore, as the normal nitrite of this series, with the formula $Co_2(NH_3)_{10}(NO_2)_2(NO_2)_4+4OH_2$. Its formation from sulphate of roseocobalt and baric nitrite is expressed by the equation:

$$Co_2(NH_3)_{10}(SO_4)_3+3BaNO_2=Co_2(NH_3)_{10}(NO_2)_6+3SO_4Ba,$$

and from chloride of purpureocobalt and argentic nitrite by the equation,

$$Co_2(NH_3)_{10}Cl_6+6AgNO_2=Co_2(NH_3)_{10}(NO_2)_6+6AgCl.$$

The formation of the silver salt of Erdmann's series, $Co_2(NH_3)_4(NO_2)_8Ag_2$, is probably due to a secondary action, and may, perhaps, be expressed by the equation

$$Co_2(NH_3)_{10}(NO_2)_6+2AgNO_2=Co_2(NH_3)_4(NO_2)_8Ag_2+6NH_3.$$

Ammonia-cobalt-nitrate of xanthocobalt.—When a solution of potassic ammonia-cobalt-nitrite is added to one of nitrate of xanthocobalt, a beautiful crystalline precipitate is formed, of a deep orange-red color, which requires a rather large quantity of boiling water for solution, and which may be recrystallized without decomposition. The solution gives the reactions of salts of xanthocobalt, and gives also, with argentic nitrate, the characteristic silver salt $Co_2(NH_3)_4(NO_2)_8Ag_2$. Of this salt

0·5074 gr. gave 0·3172 gr. SO_4Co=23·77 per cent cobalt.
0·4731 gr. gave 135 c.c. nitrogen (moist) at 12° C. and 757·8 mm.= 33·69 per cent. nitrogen.

The formula $\{Co_2(NH_3)_{10}(NO_2)_2\}\{Co_2(NH_3)_4(NO_2)_8\}_2$ requires 23·79 per cent cobalt, and 33·87 per cent nitrogen. This salt is metameric with the corresponding salt of the hexamin series already described, and with Erdmann's salt, $Co_2(NH_3)_6(NO_2)_6$, since we have

$$\{Co_2(NH_3)_{10}(NO_2)_2\}\{Co_2(NH_3)_4(NO_2)_8\}_2= 3Co_2(NH_3)_6(NO_2)_6.$$

In endeavoring to obtain measurable crystals by allowing a solution of this salt to stand for some time and evaporate at ordinary temperatures, I found that the salt was partially decomposed, a considerable quantity of cobaltic nitrate being formed.

Oxalate of xanthocobalt.—In the first part of this memoir, in consequence of an oversight, the formula given for the oxalate of xanthocobalt contains (old style) five atoms of water of crystallization. The salt is really anhydrous, and the analyses given agree with the formula $Co_2(NH_3)_{10}(NO_2)_2(C_2O_4)_2$. The salt is obtained from hot solutions in granular crystals. Its solution in hot dilute nitric acid deposits abundant crystals of the nitrate, the oxalate being almost completely decomposed. Sulphate and nitrate of xanthocobalt may be readily prepared from the oxalate by boiling with a small excess of mercurous sulphate or nitrate, adding, in the first case, a little dilute sulphuric, in the last, a little nitric, acid. As the oxalate can be precipitated by ammonic oxalate from the crude nitrate, this furnishes a cheap and expeditious method of obtaining the pure sulphate.

The formulas of the salts of xanthocobalt at present known become in the new notation:

Chloride,	$Co_2(NH_3)_{10}(NO_2)_2Cl_4$
Bromide,	$Co_2(NH_3)_{10}(NO_2)_2Br_4$
Iodide,	$Co_2(NH_3)_{10}(NO_2)_2I_4$
Nitrate,	$Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4$
Nitrite,	$Co_2(NH_3)_{10}(NO_2)_2(NO_2)_4+4OH_2$
Sulphate,	$Co_2(NH_3)_{10}(NO_2)_2(SO_4)_2$
Iodo-sulphate,	$Co_2(NH_3)_{10}(NO_2)_2(SO_4)I_2+2OH_2$
Hyper-iodo-sulphate,	$Co_2(NH_3)_{10}(NO_2)_2(SO_4)I_4$
Auro-chloride	$Co_2(NH_3)_{10}(NO_2)_2Cl_4+2AuCl_3+OH_2$
Platino-chloride,	$Co_2(NH_3)_{10}(NO_2)_2Cl_4+PtCl_4+OH_2$
Hydrargo-chloride,	$Co_2(NH_3)_{10}(NO_2)_2Cl_4+4HgCl_2+OH_2$
Oxalate,	$Co_2(NH_3)_{10}(NO_2)_2(C_2O_4)_2$
Chromate,	$Co_2(NH_3)_{10}(NO_2)_2(CrO_4)_2+2OH_2$
Dichromate,	$Co_2(NH_3)_{10}(NO_2)_2(Cr_2O_7)_2$
Ammonia-cobalt-nitrite,	$\{Co_2(NH_3)_{10}(NO_2)_2\}\{Co_2(NH_3)_4(NO_2)_8\}_2$
Ferrocyanide,	$Co_2(NH_3)_{10}(NO_2)_2(FeCy_6)+6OH_2$.

I have collected them for the purpose of convenience of reference and comparison.

PURPUREOCOBALT.

12. In the first part of this memoir Genth and I have endeavored to show that purpureocobalt and roseocobalt form two distinct series of salts; that chloride of roseocobalt, for instance, $Co_2(NH_3)_{10}Cl_6+2OH_2$, cannot be regarded as differing from chloride of purpureocobalt only by water of crystallization. This view has been adopted by some chemists, rejected, and even ridiculed, by others. I shall endeavor to show, by a more extended study and comparison of the two series of salts, that they are essentially different, and, furthermore, that, as the theory of these compounds proposed by Blomstrand suggests, there are more than two series containing the group $Co_2(NH_3)_{10}$. Reserving the discussion for the present, I proceed to the description of the salts which serve to throw light upon the question.

Auro-chloride of purpureocobalt.—When a solution of chloro-aurate of sodium is added to a hot solution of chloride of purpureocobalt, containing a little free chlorhydric acid, no precipitate is formed at first, but after standing a few hours crystals of a new salt are deposited. The crystals in question present flat prismatic forms. They have a dark ruby-red color, with a dull violet luster, and after standing, exhibit a distinct superficial reduction of gold. Of these crystals

0·9028 gr. gave 0·3206 gr. gold, and 1·0560 gr. silver=35·50 per cent gold, and 38·45 per cent chlorine.

0·6840 gr. gave 0·1896 gr. SO_4Co and 0·2425 gr. gold=10·55 per cent cobalt, and 35·45 per cent gold.

		Calculated.	Found.	
Cobalt,	2	10·64	10·55	
Gold,	2	35·55	35·50	35·45
Chlorine,	12	38·44	38·45	

In the first analysis the salt was reduced by zinc and dilute sulphuric acid, the gold weighed directly, and the chlorine determined in the filtrate. In the second, the salt was heated with sulphuric acid, and the reduced gold separated from the cobaltic sulphate by dissolving the latter in boiling water. The formula of the salt is

$$Co_2(NH_3)_{10}Cl_6+2AuCl_3,$$

or rationally

$$\overset{vi}{Co_2}\left\{\begin{array}{l} NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl=\overset{v}{Au}Cl_3 \\ NH_3-NH_3-Cl=\overset{v}{Au}Cl_3 \\ NH_3-NH_3-Cl \\ NH_3-Cl \end{array}\right.$$

From the formula it appears that the salt is unsaturated, similar salts containing 4 or 6 molecules of auric chloride being also possible.

Chloro-hydrargyrates of purpureocobalt.—When mercuric chloride is added in excess to a solution of chloride of purpureocobalt, a rather dull red salt separates in small needles, slightly soluble in cold water, but much more easily soluble in hot water, especially in the presence of free chlorhydric acid, and readily crystallizing from the hot solution. This salt has the formula

$$Co_2(NH_3)_{10}Cl_6+6HgCl_2$$

as the following analyses show:

0·5884 gr. gave 0·3922 gr. Hg_2Cl_2=56·60 per cent mercury.

0·4409 gr. gave 0·4025 gr. silver =30·00 per cent chlorine.

		Calculated.	Found.
Mercury,	6	56·47	56·60
Chlorine,	18	30·04	30·00

When the chloride of purpureocobalt is in excess, or when the two chlorides are mixed in the proper atomic proportions, another double salt separates in very beautiful violet-colored prismatic crystals, which, like the last mentioned salt, are but slightly soluble in cold water, but are much more soluble in boiling water, and crystallize from the solution on cooling. This salt has the formula

$$Co_2(NH_3)_{10}Cl_6+4HgCl_2$$

as the following analyses show:

0·7938 gr. gave 0·4735 gr. Hg_2Cl_2=50·65 per cent mercury.
0·3970 gr. gave 0·3771 gr. silver =31·23 per cent chlorine.
0·9752 gr. gave 0·9356 gr. silver =31·42 per cent chlorine.
1·3600 gr. gave 0·1024 gr. cobalt = 7·52 per cent cobalt.

		Calculated.	Found.	
Mercury,	4	50·47	50·65	
Chlorine,	14	31·35	31·23	31·42
Cobalt,	2	7·44	7·52	

On Blomstrand's view the formulas of the two mercury salts may be written

$$\overset{vi}{Co_2}\begin{cases} NH_3-Cl-HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-Cl-HgCl_2 \end{cases} \qquad \overset{vi}{Co_2}\begin{cases} NH_3-Cl \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-NH_3-Cl=HgCl_2 \\ NH_3-Cl \end{cases}$$

It is possible that the salts containing six atoms of mercury belong to the roseocobalt series, as I find that it is formed when a solution of $HgCl_3Na$ is added to one of the soluble sulphate of roseocobalt, $Co_2(NH_3)_{10}(SO_4)_3+5aq$, which I shall describe farther on. I may also remark that if the progress of science should make us acquainted with a method of determining cobalt in these salts with precision, they would enable us to determine the atomic weight of that metal with great accuracy, the first salt containing 5·54, and the second 7·44, per cent of cobalt, so that a relatively large error in the weight of $(NH_3)_{10}Hg_6Cl_{18}$, or of $(NH_3)_{10}Hg_4Cl_{14}$, would exert an inappreciable influence on the result. If we regard the salt $Co_2(NH_3)_{10}Cl_6+4HgCl_2$ as unsaturated, it ought to combine with other electro-negative chlorides to form salts with three metallic elements. I have not, however, found this to be the case, so far, at least, as the chlorides of gold and platinum are concerned.

In analyzing these salts, I found it most advantageous to determine the mercury in the form of calomel, by dissolving the salt in water, adding a little chlorhydric acid, and then reducing the mercuric to mercurous chloride by a solution of sodic hypophosphite, the solution of mercuric salt having the temperature of 40° C. The mercurous chloride was then weighed on a porous earthenware cone at 100° C. In determining chlorine it is best to dissolve the salt in hot water, with a little free sulphuric acid. The mercury may then be separated as HgS, and the chlorine determined in the filtrate after removing the excess of SH_2 by a solution of ferric alum.

Antimonio-chloride of purpureocobalt.—A solution of antimonious chloride added to one of chloride of purpureocobalt gives a precipitate of small, granular, dull violet red crystals. These may be washed with strong chlorhydric acid and dried by pressure between folds of porous paper, and afterward at 100° C. Water decomposes it readily, with precipitation of SbOCl. The formula of this salt is

$$Co_2(NH_3)_{10}Cl_6+SbCl_3,$$

as appears from the following analyses:

0·8100 gr. gave 0·3402 gr. SO_4Co=15·99 per cent cobalt.
0·6500 gr. gave 0·1370 gr. SbO_2 =16·64 per cent antimony.

The formula requires 16·22 per cent cobalt, and 16 49 per cent antimony.

Bismuthous chloride gives a lilac-red precipitate in solutions of chloride of purpureocobalt, insoluble in strong chlorhydric acid, and readily decomposed by water, with precipitation of BiOCl.

Neutral chromate.—When a solution of nitrate of purpureocobalt is added to one of neutral potassic chromate, a red crystalline precipitate is formed, which, after washing with cold water, may be dissolved in boiling water, with addition of a few drops of acetic acid. After some hours the neutral chromate separates in crystals, which have a peculiar red color with bronze-yellow reflections. The crystals are thin, acicular leaves. The salt dissolves rather easily in hot water, but the solution is soon more or less completely decomposed, unless free acid is present. The dilute solution is orange-yellow; concentrated solutions are red. The dried salt somewhat resembles litharge. Different preparations of this salt gave, on analysis, results which differed somewhat from each other, but only in the amount of water of crystallization. In one preparation

0·2637 gr. gave 0·2480 gr. CrO_4Ba=43·17 per cent CrO_4
0·3651 gr. gave 0·0811 gr. cobalt =22·21 per cent.
0·3598 gr. gave 0·0058 gr. water up to 170° C.=1·61 per cent.

I consider the loss of weight on heating to arise partly from hygroscopic moisture, and partly from slight decomposition, and believe that the salt is really anhydrous. The formula

$$Co_2(NH_3)_{10}.O.(CrO_4)_2$$

requires cobalt, 21·99 per cent, and CrO_4, 43·32 per cent. The formation of the neutral chromate is expressed by the equation:

$$Co_2(NH_3)_{10}(NO_3)_6+2CrO_4K_2+OH_2=Co_2(NH_3)_{10}.O.(CrO_4)_2 +4KNO_3+2NO_3H.$$

The nitric acid set free dissolves a portion of the chromate forming the dichromate, which remains in solution. When a solution of neutral potassic tungstate, WO_4K_2, is digested with dry neutral nitrate of purpureocobalt, a pink tungstate of purpureocobalt is formed, and the liquid then gives a strong acid reaction with litmus. The reaction is probably the same as that given above for the chromate.

Potassic iodide gives a dull red crystalline precipitate with neutral chromate of purpureocobalt in solution. The analyses of this salt led to no definite formula, and the precipitate appeared to be a mixture of the chromate described, $Co_2(NH_3)_{10}.O.(CrO_4)_2$, and the iodo-chromate, $Co_2(NH_3)_{10}I_2(CrO_4)_2$. By digesting powdered chloride of purpureocobalt with neutral potassic chromate, Braun obtained a dark brown-red powder, to which he gives the formula $Co_2(NH_3)_{10}(CrO_4)_3$. According to the same writer, when powdered chloride of purpureocobalt is added, in small portions at a time, to a concentrated solution of potassic dichromate, a beautiful crystalline powder is formed, which has also the formula $Co_2(NH_3)_{10}(CrO_4)_3$. In this case chromic acid, CrO_4H_2, must be set free. When a solution of potassic chromate is added to one of chloride of purpureocobalt, the crystalline precipitate formed, according to my observations, always contains chlorine. My analyses led, however, in this case also, to no definite formula, but pointed to a mixture of the chromate, $Co_2(NH_3)_{10}.O.(CrO_4)_2$, and the chlorochromate, $Co_2(NH_3)_{10}Cl_2(CrO_4)_2$. Braun has also described a salt, to which he gives the formula $2NH_3.Co_2O_3.3CrO_3+2NH_4Cl$, which I should write $Co_2(NH_3)_2(CrO_4)_3+2NH_4Cl$, but the analyses are incomplete without a determination either of ammonia or of nitrogen.

Dichromate.—A solution of potassic dichromate gives, with one of nitrate of purpureocobalt, a granular red precipitate, which may be recrystallized by solution in boiling water, to which a little acetic acid has been added. The salt then separates in small, indistinct crystals of a dark brick red color, with bronze reflections. It is somewhat soluble in cold, and dissolves readily in boiling, water. Of this salt

0·6031 gr. gave 0·0747 gr. cobalt=12·38 per cent.
0·7101 gr. gave 1·1252 gr. CrO_4Ba=67·71 per cent. (Chromium=52·2.)
0·6295 gr. lost, at 105° C., 0·0077 gr. water=1·22 per cent; at 120° C., 0·0118 gr.=1·87 per cent; and at 133° C., 0·0166 gr.=2·64 per cent.

At 133° C. the salt was slightly decomposed. Between 133° and 145° C. it lost 4·46 per cent with partial decomposition. These analyses correspond to the formula $Co_2(NH_3)_{10}(Cr_2O_7)_3 + OH_2$.

		Calculated.	Found.
Cobalt,	2	12·35	12·38
Cr_2O_7,	3	67·97	67·71
Water,	1	1·88	1·87

The salt was dried for two weeks in pleno over sulphuric acid. In preparing nitrate of purpureocobalt by Mr. Mills' process, in which an ammoniacal solution of cobaltic nitrate is oxidized by potassic dichromate, I obtained, besides the nitrate, a large quantity of beautiful orange-red crystalline scales, with gold reflections. The crystals were easily purified by recrystallization. They are readily soluble in hot water, and crystallize from the solution almost completely on cooling. The formula of this salt is $Co_2(NH_3)_{10}(Cr_2O_7)_3 + 5OH_2$, as the following analyses show:

0·6366 gr. gave 0·0735 gr. cobalt=11·54 per cent.
0·6447 gr. gave 0·2888 gr. CrO_3 =63·31 per cent Cr_2O_7.
0·1740 gr. gave, up to 139° C., 0·0125 gr. water=7·19 per cent.
0·0800 gr. gave, up to 145° C., 0·0082 gr. water=10·25 per cent.
Mean, 8·72 per cent.

In the last water determination the salt was slightly decomposed. The formula requires

		Calculated.	Found.
Cobalt,	2	11·48	11·54
Cr_2O_7,	3	63·20	63·31
Water,	5	8·76	8·72 (mean.)

The difference in appearance and in the number of atoms of water of the dichromate of purpureocobalt may possibly arise from the fact that, in one case, a solution of the nitrate of purpureocobalt was poured into one of potassic dichromate in excess; in the other, the nitrate was presented to the dichromate as fast as formed—in some sense in the nascent state. But it is singular that the two hydrates are not the same after recrystallization. A solution of potassic dichromate gives, with one of chloride of purpureocobalt, a dark red crystalline precipitate, the analyses of which pointed to a mixture of $Co_2(NH_3)_{10}(Cr_2O_7)_3$ and $Co_2(NH_3)_{10}.Cl_2.(Cr_2O_7)_2$. I did not succeed in obtaining the basic dichromate $Co_2(NH_3)_{10}.O.(Cr_2O_7)_2$. In all the chromates of the cobaltamines which I have studied, the direct determination of the water of crystallization has been effected with peculiar difficulty, in consequence of the tenacity with which these salts retain water up to temperatures very near to those at which incipient decomposition occurs.

(To be continued.)

ART. XXVI.—*The Mathematical and Philosophical State of the Physical Sciences;* by Prof. JOSEPH LOVERING. From the Presidential Address of Prof. Lovering before the American Association at Hartford, August, 1874.

THE luminiferous ether and the undulatory theory of light have always troubled what is supposed to be the imperturbable character of the mathematics. The proof of a theory is indisputable when it can predict consequences, and call successfully upon the observer to fulfill its prophecies. It is the boast of astronomers that the law of gravitation thus vindicates itself. The undulatory theory of light has shown a wonderful facility of adaptation to each new exigency in optics, and has opened the eye of observation to see what might never have been discovered without the promptings of theory. But this doctrine, and that of gravitation also, have more than once been arrested in their swift march and obliged to show their credentials. After Fresnel and Young had secured a firm foothold for Huyghens' theory of light in mechanics and experiment, questions arose which have perplexed, if not baffled, the best mathematical skill. How is the ether affected by the gross matter which it invests and permeates? Does it move when they move? If not, does the relative motion between the ether and other matter change the length of the undulation or the time of oscillation? These queries cannot be satisfactorily answered by analogy, for analogy is in some respects wanting between the ether and any other substance. Astronomy says that aberration cannot be explained unless the ether is at rest. Optics replies that refraction cannot be explained unless the ether moves. Fresnel produced a reconciliation by a compromise. The ether moves with a *fractional* velocity large enough to satisfy refraction, but too small to disturb sensibly the astronomer's aberration. In 1814, Arago reported to Fresnel that he found no sensible difference in the prismatic refraction of light, whether the earth was moving with full speed toward a star or in the opposite direction, and asked for an explanation. Fresnel submitted the question to mathematical analysis, and demonstrated, that whatever change was produced by the motion of the prism in the relative velocity of light, the wave-length through the prism, and the refraction, was compensated by the physiological aberration when the rays emerged. Very recently, Ketteler of Bonn has gone over the whole ground again with great care, studying not only Arago's case but the general one, in which the direction of the light made any angle with the motion of the earth: and he proves that the light will always enter the eye in the same apparent direction as it would have done if the earth were at rest. The mathematical and physical view taken of this subject by Fresnel has been under discussion for sixty years, and forty eminent physicists and mathematicians might be enumerated who have taken part in it. Fresnel's explanation has encountered difficulties and objections. Still, it is consistent not only with Arago's negative result, but with the experiments on diffraction by Fizeau and Babinet, and the preponderance of mathematical evidence is on that side. Mr. Huggins runs counter to the general drift of physical and algebraical testimony (although he appears to be sustained by the high authority of Maxwell), when he attributes some displacement of the spectrum lines to the motion of the earth, and qualifies the observed displacement on that account The number of stars which Huggins has observed is insufficient for any sweeping generalization. And yet he seems inclined to explain the revelations of his spectroscope, not by the motion of the stars, but by that of the solar system: because those stars which are in the neighborhood of the place in which astronomers have put the solar apex are moving, apparently, toward the earth, while those in the opposite part of the sky recede. If it be true that the earth's annual motion produces no displacement in the spectrum, then the motion of the solar system produces none. Or, waiving this objection, if the correct explanation has been given by Huggins, astronomers have failed, by their geometrical method, of rising to the full magnitude of the sun's motion. The discrepancy appears to awaken no distrust in Mr. Huggins' mind as to the delicacy of the spectrum analysis or the mathematical basis of his reasoning. On the contrary, he would remove the discrepancy by throwing discredit on the estimate of star-distances made independently by Struve and Argelander from different lines of thought.

Next we ask, if it is certain that even the motion of the luminary will change the true wave-length, the period of oscillation, and the refrangibility of the light which issues from it? The commonly received opinion on this subject has not been allowed to pass unchallenged. It is fortified by more than one analogy: but it is said that comparison is not always a reason. It is not denied that, when the sonorous body is approaching, the sound waves are shortened, the number of impulses on the ear by the condensed air is increased, and the pitch of the sound is raised. Possibly, the color of light would follow the same law; but there is no experiment to prove it, and very little analogy exists between the eye and the ear. There is no analogy, whatever, between the subjective sensation by either organ and the physical action of the prism. The questions at issue are these: Does refraction depend upon the absolute or the relative velocity of light; are the time of oscillation of the particles of ether and the normal wave-length, corresponding to it, changed by any motion of translation in the origin; or is the conservation of these elements an essential attribute of the luminiferous medium. It has been said that Doppler reasoned as if the corpuscular theory of light were true, and then expressed himself in the language of undulations. Evidently, there is an obscurity in the minds of many physicists, and an uncertainty in all, when they reason upon the mechanical constitution of the ether, and the fundamental laws of light. The mathematical theory is not so clear as to be able to dispense with the illumination of experiment. Within the present year, Van der Willigen has published a long and well considered memoir on the theoretical fallacies which vitiate the whole of Huggins' argument for the motion of the stars and nebulæ. His analysis proves that the motion of the luminary will not interfere with the time of oscillation and the wave-length provided that the origin of the disturbance is not a mathematical point but a vibrating molecule, and that the sphere of action of this molecule upon surrounding molecules is large enough to keep them under its influence during ten or a hundred vibrations, before it is withdrawn by the motion of translation. If this theoretical exposition of the subject should be generally adopted by mathematicians, the spectroscopic observations on the supposed motion of the stars must receive another interpretation. On the other hand, if a luminary is selected which is known to move, independently of spectroscopic observations, and the displacement of the spectrum lines accords with this motion, it will be time to reconsider the mathematical theory, and make our conceptions of the ether conform to the experiment. The spectroscopic observation of Angström on an oblique electric spark does not favor Huggins' views. Secchi testifies to opposite displacements when he examined, with a direct vision spectroscope, the two edges of the sun's equator, one of which was rotating toward him and the other from him, and Vogel has repeated the observation with a reversion-spectroscope. This would have the force of a crucial experiment were it not that an equal displacement was seen on other parallels of latitude, and that the bright bands of the chromosphere were moved, but not the dark lines of the solar atmosphere.

When Voltaire visited England in 1727 he saw at the universities the effect of Newton's revolutionary ideas in astronomy. The mechanism of gravitation had exiled the fanciful vortices of Descartes, which were still circulating on the continent. So he wrote: "A Frenchman who comes to London finds many changes in philosophy as in other things: he left the world full, he finds it empty." The same comparison might be made now, not so much between nationalities as between successive stages of scientific development. At the beginning of this century the universe was as empty as an exhausted receiver: now it has filled up again. Nature's abhorrence of a vacuum has been resuscitated, though for other reasons than these which satisfied the Aristotelians. It is the mathematicians and not the metaphysicians who are now discussing the relative merits of the *plenum* and the *vacuum*. Newton, in his third letter to Bentley, wrote in this wise: "That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it." Roger Cotes, who was Newton's successor in the chair of mathematics and natural philosophy at Cambridge, was only four years old when the first edition of the "Principia" was issued, and Newton outlived him by ten years. The venerable teacher pronounced upon the young mathematician, his pupil, these few but comprehensive words of eulogy: "If Cotes had lived, we should have known something." The view taken of gravitation by Cotes was not the same as that held by his master. He advocated the proposition that action at a distance must be accepted as one of the primary qualities of matter, admitting of no farther analysis. It was objected by Hobbes and other metaphysicians, that it was inconceivable that a body should act where it was not. All our knowledge of mechanical forces is derived from the conscious effort we ourselves make in producing motion. As this motion employs the machinery of contact, the force of gravitation is wholly outside of all our experience. The advocates of action at a distance reply, that there is no real contact in any case, that the difficulty is the same with the distance of molecules as that of planets, that the mathematics are neither long-sighted nor short-sighted, and that an explanation which suits other forces is good enough for gravitation.

Comte extricated himself from this embarrassment by excluding causes altogether from his positive philosophy. He rejects the word attraction as implying a false analogy, inconsistent with Newton's law of distance. He substitutes the word gravitation, but only as a blind expression by which the facts are generalized. According to Comte's philosophy, the laws of Newton are on an equality with the laws of Kepler, only they are more comprehensive, and the glory of Kepler has the same stamp as that of Newton. Hegel, the eminent German metaphysician, must have looked at the subject in the same light when he wrote these words: "Kepler discovered the laws of free motion; a discovery of immortal glory. It has since been the fashion to say that Newton first found out the truth of these rules. It has seldom happened that the honor of the first discoverer has been more unjustly transferred to another." Shelling goes farther in the same direction: he degrades the Newtonian law of attraction into an empirical fact, and exalts the laws of Kepler into necessary results of our ideas.

Meanwhile, the Newtonian theory of attraction, under the skillful generalship of the geometers, went forth on its triumphal march through space, conquering great and small, far and near, until its empire became as universal as its name. The whirlpools of Descartes offered but a feeble resistance, and were finally dashed to pieces by the artillery of the parabolic comets; and the rubbish of this fanciful mechanism was cleaned out as completely as the cumbrous epicycles of Ptolemy had been dismantled by Copernicus and Kepler. The mathematicians certified that the solar system was protected against the inroads of comets, and the border warfare of one planet upon another, and that its stability was secure in the hands of gravitation, if only space should be kept open, and the dust and cobwebs which Newton had swept from the skies should not reappear. Prophetic eyes contemplated the possibility of an untimely end to the revolution of planets, if their ever-expanding atmospheres should rush in to fill the room vacated by the maelstroms of Descartes. When it was stated that the absence of infinite divisibility in matter, or the coldness of space, would place a limit upon expansion, and, at the worst, that the medium would be too attenuated to produce a sensible check in the headway of planets, and when, in more recent times, even Encke's comet showed but the slightest symptoms of mechanical decay, it was believed that the motion was, in a practical, if not in a mathematical sense, perpetual. Thus it was that the splendors of analysis dimmed the eyes of science to the intrinsic difficulties of Newton's theory, and familiarity with the language of attraction concealed the mystery that was lurking beneath it. A long experience in the treatment of gravitation has supplied mathematicians with a fund of methods and formulas suited to similar cases. As soon as electricity, magnetism and electromagnetism took form, they also were fitted out with a garment of attractive and repulsive forces acting at a distance: and the theories of Cavendish, Poisson, Aepinus and Ampère, endorsed as they were by such names as Laplace, Plana, Liouville and Green, met with general acceptance.

The seeds, which were destined to take root in a later generation, and disturb, if not dislodge, the prevalent interpretation of the force of gravitation, were sown by a contemporary of Newton. They found no congenial soil in which they could germinate and fructify until the early part of this century. At the present moment, we find the luminiferous ether in quiet and undivided possession of the field from which the grosser material of ancient systems had been banished. The *plenum* reigns everywhere; the vacuum is nowhere. Even the corpuscular theory of light, as it came from the hands of its founder, required the reinforcement of an ether. Electricity and magnetism, on a smaller scale, applied similar machinery. If there was a fundamental objection to the conception of forces acting at a distance, certainly the bridge was already built by which the difficulty could be surmounted. The turning-point between the old physics and the new physics was reached in 1837, when Faraday published his experiments on the specific inductive capacity of substances. This discovery was revolutionary in its character, but it made no great stir in science at the time. The world did not awake to its full significance until the perplexing problem of ocean telegraphs converted it from a theoretical proposition into a practical reality, and forced it on the attention of electricians. The eminent scientific advisers of the cable companies were the first to do justice to Faraday. This is one of the many returns made to theoretical electricity for the support it gave to the most magnificent commercial enterprise.

The discovery of diamagnetism furnished another argument in favor of the new interpretation of physical action. What that new interpretation was, is well described by Maxwell. "Faraday, in his mind's eye, saw lines of force traversing all space, where the mathematicians saw centers of force attracting at a distance; Faraday saw a medium where they saw nothing but distance; Faraday sought the seat of the phenomena in real actions going on in the medium; they were satisfied that they had found it in a *power* of action at a distance impressed on the electric fluids." The physical statement waited only for the coming of the mathematicians who could translate it into the language of analysis, and prove that it had as precise a numerical consistency as the old view with all the facts of observation. A paper published by Sir William Thomson, when he was an undergraduate at the university of Cambridge, pointed the way. Prof. Maxwell, in his masterly work on electricity and magnetism, which appeared in 1873, has built a monument to Faraday, and unconsciously to himself also, out of the strongest mathematics. For forty years mathematicians and physicists had labored to associate the laws of electrostatics and electrodynamics under some more general expression. An early attempt was made by Gauss in 1835, but his process was published, for the first time, in the recent complete edition of his works. Maxwell objects to the formula of Gauss because it violates the law of the conservation of energy. Weber's method was made known in 1846; but it has not escaped the criticism of Helmholtz. It represents faithfully the laws of Ampère and the facts of induction, and led Weber to an absolute measurement of the electrostatic and electromagnetic units. The ratio of these units, according to the formulas, is a velocity; and experiment shows that this velocity is equal to the velocity of light. As Weber's theory starts with the conception of action at a distance, without any mediation, the effect would be instantaneous, and we are at a loss to discover the physical meaning which he attaches to his velocity. Gauss abandoned his researches in electromagnetism because he could not satisfy his mind in regard to the propagation of its influence in time. Other mathematicians have worked for a solution, but have lost themselves in a cloud of mathematical abstraction. The two theories of light have exhausted all imaginable ways in which force can be gradually transmitted without increase or loss of energy. Maxwell cut the Gordian knot when he selected the luminiferous ether itself as the arena on which to marshal the electromagnetic forces under the symbols of his mathematics, and made light a variety of electromagnetic action. His analysis gave a velocity essentially the same as that of Weber, with the advantage of being a physical reality and not a mere ratio Of the two volumes of Mr. Maxwell, freighted with the richest and heaviest cargo, the reviewer says: "Their author has, as it were, flown at everything: and, with immense spread of wing and power of beak, he has hunted down his victims in all quarters, and from each has extracted something new and interesting for the intellectual nourishment of his readers." Clear physical views must precede the application of mathematics to any subject. Maxwell and Thomson are liberal in their acknowledgments to Faraday. Mr. Thomson says: "Faraday, without mathematics, divined the result of the mathematical investigation; and, what has proved of infinite value to the mathematicians themselves, he has given them an articulate language in which to express their results. Indeed, the whole language of the *magnetic field* and *lines of force* is Faraday's. It must be said for the mathematicians that they greedily accepted it, and have ever since been most zealous in using it to the best advantage."

It is not expected that the new views of physics will be generally accepted without vigorous opposition. A large amount of intellectual capital has been honestly invested in the fortunes of the other side. The change is recommended by powerful physical arguments, and it disenthralls the theories of science from many metaphysical difficulties which weigh heavily on some minds. On the other hand, the style of mathematics which the innovation introduces is novel and complex; and good mathematicians may find it necessary to go to school again before they can read and understand the strange analysis. It is feared, that with many, who are not easily deflected from the old ruts, the intricacies of the new mathematics will outweigh the superiority of the new physics.

The old question, in regard to the nature of gravitation, was never settled: it was simply dropped. Now it is revived with as much earnestness as ever, and with more intelligence. Astronomy cast in its own mould the original theories of electrical and magnetic action. The revolution in electricity and magnetism must necessarily react upon astronomy. It was proved by Laplace, from data which would now, probably, require a numerical correction, that the velocity of the force of gravitation could not be less than eight million times the velocity of light; in fact, that it was infinite. Those who believe in action at a distance cannot properly speak of the transmission of gravitation. Force can be transmitted only by matter; either with it or through it. According to their view, action at a distance *is* the force, and it admits of no other illustration, explanation or analysis. It is not surprising that Faraday and others, who had lost their faith in action at short distances, should have been completely staggered by the ordinary interpretation of the law of gravitation, and that they declared the clause which asserted that the force diminished with the square of the distance to be a violation of the principle of the conservation of energy.

Must we then content ourselves with the naked facts of gravitation, as Comte did, or is it possible to resolve them into a mode of action, in harmony with our general experience, and which does not shock our conceptions of matter and force? In 1798, Count Rumford wrote thus: "Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation."

Probably Rumford had never seen the paper of Le Sage, published by the Berlin Academy in 1782, in which he expounded his mechanical theory of gravitation, to which he had devoted sixty-three years of his life. In a posthumous work, printed in 1818, Le Sage has developed his views more fully. He supposed that bodies were pressed toward one another by the everlasting pelting of ultramundane atoms, inward bound from the immensity of space beyond, the faces of the bodies which looked toward each other being mutually screened from this bombardment. It was objected to this hypothesis, which introduced Lucretius into the society of Newton and his followers, that the collision of atoms with atoms, and with planets, would cause a secular diminution in the force of gravity. Le Sage admitted the fact. But as no one knew that the solar system was eternal, the objection was not fatal. As the necessity for giving a mechanical account of gravitation was not generally felt at the time, the theory of Le Sage fell into oblivion. In 1873, Sir William Thomson resuscitated and republished it. He has fitted it out in a fashionable dress, made out of elastic molecules instead of hard atoms, and has satisfied himself that it is consistent with modern thermo-dynamics and a perennial gravitation.

Let us now look in a wholly different quarter for the mechanical origin of gravitation. In 1870, Prof. Guthrie gave an account of a novel experiment, viz: the attraction of a light body by a tuning-fork when it was set in vibration. Thomson repeated the experiment upon a suspended eggshell and attracted it by a simple wave of the hand. Thomson remarks "that what gave the great charm to these investigations, for Mr. Guthrie himself, and no doubt also for many of those who heard his expositions and saw his experiments, was, that the results belong to a class of phenomena to which we may hopefully look for discovering the mechanism of magnetic force, and possibly also the mechanism by which the forces of electricity and gravity are transmitted." By a delicate mathematical analysis, Thomson arrives at the theorem that the "average pressure at any point of an incompressible, frictionless fluid, originally at rest, but set in motion and kept in motion by solids, moving to and fro, or whirling round in any manner, through a finite space of it," would explain the attractions just described. Moreover, he is persuaded by other effects besides those of light, that, in the interplanetary spaces and in the best artificial vacuum, the medium which remains has "perfectly decided mechanical qualities, and, among others, that of being able to transmit mechanical energy, in enormous quantities:" and he cherishes the hope that his mathematical theorems on abstract hydrokinetics are of some interest in physics as illustrating the great question of the eighteenth and nineteenth centuries—Is action at a distance a reality, or is gravitation to be explained, as we now believe magnetic and electric forces must be, by action of intervening matter?

In 1869 and 1873, Prof. Challis of Cambridge, England, published two works on the Principles of Mathematical Physics. They embody the mature reflections of a mathematical physicist at the advanced age of three score years and ten. Challis believes that there is sufficient evidence for the existence of ether and atoms as physical realities. He then proceeds to say: "The fundamental and only admissible idea of *force* is that of *pressure*, exerted either actively by the ether against the surface of the atoms, or as reaction of the atoms on the ether by resistance to that pressure. The principle of deriving fundamental physical conceptions from the indications of the senses does not admit of regarding *gravity*, or any other force varying with distance, as an essential quality of matter, because, according to that principle, we must, in seeking for the simplest idea of physical force, have regard to the sense of *touch*. Now, by this sense, we obtain a perception of force as pressure, distinct and unique, and not involving the variable element of distance, which enters into the perception of force as derived from the sense of sight alone. Thus, on the ground of simplicity as well as of distinct perceptibility, the fundamental idea of force is pressure." As all other matter is passive except when acted upon by the ether, the ether itself, in its quiescent state, must have uniform density. It must be coextensive with the vast regions in which material force is displayed. Challis had prepared himself for the elucidation and defence of his dynamical theory by a profound study of the laws of motion in elastic fluids. From the mathematical forms in which he has expressed these laws he has attempted to derive the principal experimental results in light, heat, gravitation, electricity and magnetism. Some may think that Mr. Challis has done nothing but clothe his theory in the cast-off garments of an obsolete philosophy. If its dress is old, it walks upon new legs. The interplay between ether and atoms is now brought on to the stage, not as a speculation sustained by metaphysical and theological arguments, but as a physical reality with mathematical supports. I should do great injustice to this author if I left the impression that he himself claimed to have covered the whole ground of his system by proof. Mathematical difficulties prevented him from reaching a numerical value for the resultant action of a wave of ether upon the atom. What he has written is the guide-post, pointing the direction in which science is next to travel: but the end of the journey is yet a great way off. The repeated protests of Mr. Challis against the popular physics of the day, and his bold proclamation of the native, independent motion of the ether, have aroused criticism. What prevents the free ether, asks the late Sir John Herschel, from expanding into infinite space? Mr. Challis replies that we know nothing about infinite space or what happens there; but the existence of the ether, where our experience can follow it, is a physical reality. The source of the motion which the ether acquires is not the sun: for the most efficient cause of solar radiation is gravitation and condensation. Our author avoids the vicious circle of making gravitation, first the reason, and afterward the consequence of the motion of the ether. He says: "It follows that the sun's heat, and the heat of masses in general, are stable quantities, oscillating, it may be, like the planetary motions, about *mean* values, but never permanently changing, so long as the Upholder of the universe conserves the force of the ether and the qualities of the atoms. There is no law of destructibility: but the same Will that conserves can in a moment destroy." The following remarks upon this theory deserve our attention: "The explanation of any action between distant bodies by means of a clearly conceivable process, going on in the intervening medium, is an achievement of the highest scientific value. Of all such actions that of gravitation is the most universal and the most mysterious. Whatever theory of the constitution of bodies holds out a prospect of the ultimate explanation of the process by which gravitation is effected, men of science will be found ready to devote the whole remainder of their lives to the development of that theory."

The hypotheses of Challis and Le Sage have one thing in common; the motion of the ether and the driving storm of atoms must come from outside the world of stars. "On either theory, the universe is not even temporarily automatic, but must be fed from moment to moment by an agency external to itself." Our science is not a finality. The material order which we are said to know makes heavy drafts upon an older or remoter one, and that again upon a third. The world, as science looks at it, is not self-sustaining. We may abandon the hope of explaining gravitation, and make attraction itself the primordial cause. Our refuge then is in the sun. When we qualify the conservation of energy by the dissipation of energy, the last of which is as much an induction of science as the first, the material fabric which we have constructed still demands outward support. Thomson calculates that, within the historical period, the sun has emitted hundreds of times as much mechanical energy as is contained in the united motions of all the planets. This energy, he says, is dissipated more and more widely through endless space, and never has been, probably never can be, restored to the sun, without acts as much beyond the scope of human intelligence as a creation or annihilation of energy, or of matter itself, would be.

From the earliest dawn of intellectual life, a general theory of the constitution of matter has been a fruitful subject of debate, and human science and philosophy have ever been dashing their heads against the intractable atoms. The eagerness of the discussion was the greater, the more hopeless the solution. For every man who set up an hypothesis upon the subject there were half a dozen others to knock it down; until at last speculation, which bore no fruit, was suspended. A lingering interest still hung around the question, whether matter was not infinitely divisible, and the atomic philosophers were not chasing a chimera. From every new decision on this single point there was an appeal, and the foothold which the atoms had secured in chemistry was gradually subsiding. Of a sudden, the atomic theory has gained a new lease of life. But the hero of the new drama is not the atom but the molecule. In all the physical sciences, including astronomy, the war has been carried home to the molecules: and the intellectual victories of this and the next generation will be on this narrow field. From the outlying provinces of physics; from the sun, the stars and the nebulæ; from the comets and meteors; from the zodiacal light and the aurora; from the exquisitely tempered and mysterious ether; the forces of nature have been moving in converging lines to this common battle-ground, and some shouts of victory have already been heard. In the long and memorable controversy between Newton and Leibnitz, and their adherents, as to the true measure of force, it was charged against the Newtonian rule that force was irrecoverably lost whenever a collision occurred between hard, inelastic bodies. The answer was, that nature had anticipated the objection and had avoided this kind of matter. Inelastic bodies were yielding bodies, and the force which had disappeared from the motion had done its work in changing the shape. But unless the body could recover its original figure by elasticity, there was no potential energy and force was annihilated. It is now believed, and to a large extent demonstrated, that the force, apparently lost, has been transformed into heat, electricity, or some other kind of molecular motion, of which the change of shape is only the outward sign. The establishment on a firm foundation of theory and experiment of the so-called conservation of energy, the child of the correlation of physical forces, is one of the first fruits of molecular mechanics.

It is no disparagement of this discovery, on which was concentrated the power of several minds, to call it an extension, though a vast one, of Newton's law of inertia, of Leibnitz's *vis viva*, and of Huyghens' and Bernouilli's conservation of living forces; these older axioms of mechanics having free range only in astronomy, where friction, resistance and collision do not interfere. The conservation of energy, in its extended signification, promises to be, like its forerunners, a valuable guide to discovery, especially in the dark places into which physical science has now penetrated. The caution which Lagrange has given in reference to similar mechanical principles, such as the conservation of the motion of the center of gravity, the conservation of moments of rotation, the preservation of areas, and the principle of least action, is not without its applicability to the new generalization. Lagrange accepts them all as results of the known laws of mechanics and not as the essence of the laws of nature. The most that physical science can assert is that it possesses no evidence of the destructibility of matter or force.

It is not pretended that the existence of atoms has been or can be proved or disproved. Some chemists think that the atomic theory is the life of chemistry: others have abandoned it. Its importance is lost in that of the molecular theory. And what has this accomplished to justify its existence? If we define the molecule of any substance as the smallest mass of that substance which retains all its chemical properties, we can start with the extensive generalization of Avogadro and Ampère, that the same volume of every kind of matter in the state of vapor, and under the same pressure and temperature, contains an equal number of such molecules. The conception of matter as consisting of parts, which are perpetually flying over their microscopic orbits and producing by their fortuitous concourse all the observed qualities of bodies, is as old as Lucretius. He saw the magnified symbol of his hypothesis in the motes which chase one another in the sunbeam. One of the Bernouillis thought that the pressure of gases might be caused by the incessant impact of these little masses on the vessel which held them. The discovery that heat was a motion and not a substance, foreshadowed by Bacon, made probable by Rumford and Davy, and rigidly proved by Mayer and Joule when they obtained its exact mechanical equivalent, opened the way to the dynamical theory of gases. Joule calculated the velocity of this promiscuous artillery, rendered harmless by the minuteness of the missiles, and found that the boasted guns of modern warfare could not compete with it. Clausius consummated the kinetic theory of gases by his powerful mathematics, and derived from it the experimental laws of Mariotte, Gay-Lussac and Charles. By the assumption of data, more or less plausible, several mathematicians have succeeded in computing the sizes and the masses of the molecules and some of the elements of their motion. It should not be forgotten that mathematical analysis is only a rigid system of logic by which wrong premises conduct the more surely to an incorrect conclusion. To claim for all the conclusions which have been published in relation to the molecules the certainty which fairly belongs to some of them would prejudice the whole cause.

One of the most interesting investigations in molecular mechanics was published by Helmholtz in 1858. It is a mathematical discussion of what he calls ring-vortices in a perfect, frictionless fluid. Helmholtz has demonstrated that such vortices possess a perpetuity and an inviolability once thought to be realized only by the eternal atoms. The ring-vortices may hustle one another, and pass through endless transformations, but they cannot be broken or stopped. Thomson seized upon them as the impersonation of the indestructible but plastic molecule which he was looking for, to satisfy the present condition of physical science. The element of the new physics is not an atom or a congeries of atoms, but a whirling vapor. The molecules of the same substance have one invariable and unchangeable mass: they are all tuned to one standard pitch and, when incandescent, emit the same kind of light. The music of the spheres has left the heavens and condescended to the rhythmic molecules. There is here no birth or death or variation of species. If other masses than the precise one which represents the elements have been eliminated, where, asks Maxwell, have they gone? The spectroscope does not show them in the stars or nebulæ. The hydrogen and sodium of remotest space are in unison with the hydrogen and sodium of earth.

In the phraseology of our mechanics we define matter and force as if they had an independent existence. But we have no conception of inert matter or of disembodied force. All we know of matter is its pressure and its motion. The old atom had only potential energy; the energy of its substitute, the molecule, is partly potential and partly kinetic. If it could be shown that all the phenomena displayed in the physical world were simply transmutations of the original energy existing in the molecules, physical science would be satisfied. Where physical science ends, natural philosophy, which is not wholly exploded from our vocabulary, begins. Natural philosophy can give no account of energy when disconnected with an ever present Intelligence and Will. In Herschel's beautiful dialogue on atoms, after one of the speakers had explained all the wonderful exhibition of nature as the work of natural forces, Hermione replies: "Wonderful, indeed! Anyhow, they must have not only good memories but astonishing presence of mind, to be always ready to act, and always to act, without mistake, according to the primary laws of their being, in every complication that occurs." And elsewhere, "Action, without will or effort, is to us, constituted as we are, unrealizable, unknowable, inconceivable." The monads of Leibnitz and the demons of Maxwell express in words the personality implied in every manifestation of force.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Fluoxyboric Acid.*—Basarow has recently investigated, in the laboratory of Professor Wurtz, the acid discovered by Gay Lussac and Thenard in 1809, and by them called fluoxyboric acid, with the formula $HBO_2(HF)_3$. The acid was prepared by saturating water with boron fluoride, obtained by heating in a platinum retort a mixture of calcium fluoride and boric oxide with sulphuric acid. The water readily absorbed the gas and yielded an acid having properties exactly described by its discoverers. On submitting it to fractional distillation, however, in a platinum retort, nothing but boron fluoride was evolved at 140° and below, and between 160° and 170°, there came over a fuming liquid, thick as honey and with a specific gravity of 1·777. From this point the density of the distillate steadily decreased, that coming over at 200° being 1·577. Redistillation from boric oxide increased this density. All the fractions save the last gave an abundant precipitate of boric acid on being mixed with water. From these data, Basarow concludes that fluoxyboric acid has no independent existence, since it cannot be fractionated. He believes it to be only a solution of boric acid in hydrofluoboric acid, the analogy being complete between the reaction of BF_3 and that of SiF_4 with water; only in the latter case the silicate separates as a gelati-

nous mass, and in the former the acid remains in solution. From the equation

$$(HBO_2.(HE)_3)_4 = HBO_2 + (HF.BF_3)_3 + (H_2O)_6$$

the relation of the old formula to Basarow's view is shown. By repeated distillation from boric oxide, an acid of density 1·712 was obtained from the above mentioned distillates, and analyzed. While the formula $HBO_2.(HF)_3$ requires boron 10·6, fluorine 54·8, and water 34·6 per cent, the author actually obtained boron 14·8, fluorine 47·0, and water 28·8 per cent as a mean of two analyses. These results confirm the above suspicion and prove that the fluoxyboric acid of Gay Lussac and Thenard has no existence. In the course of his research Basarow observed that one c.c. water absorbed at 0° and 762 mm. 1057 c.c. boron fluoride.—*Bull. Soc. Ch.*, II, xxii, 8, July, 1874. G. F. B.

2. *Ozone not produced by the Oxidation of the Essential Oils.*—Ever since the discovery of ozone by Schönbein, our chemical text-books have repeated his statement that ozone is produced by the action of light upon oil of turpentine in presence of air. In order to obtain more definite knowledge upon the subject, KINGZETT has studied the question very thoroughly, and has been entirely unable to prove the production of ozone, as asserted. His first experiments were directed to ascertain the rate of absorption of oxygen by turpentine and similar bodies. He found, for example, that turpentine itself, in sunshine, absorbed 36·6 c.c. of oxygen daily, but in the shade only 0·6 c.c.; from air, 0·75 c.c. Oil of caraway absorbed 3 c.c. daily, oil of bergamot 3 c.c., oil of juniper 2·5 c.c., oil of cubebs 2 c.c., oil of lemon 1·2 c.c., naphtha 0·7 c.c., and ether 0·19 c.c.; benzol showed no absorption during forty days. On agitating these oils, after the absorption, with potassium iodide and starch solution, the well-known blue coloration was more or less quickly developed. The color, however, did not appear at once, but, in the case of the bergamot especially, required several minutes. In confirmation of this test, an oxygenized oil was found to color yellow a solution of potassium iodide, resting upon it, and on heating to 70°, to turn blue a piece of potassium iodide and starch paper, placed in the mouth of the test-tube containing the oil. The paper also became rapidly blue when moistened and placed over the oil contained in a clock-glass. On testing the substance formed for ozone, it was observed that neither lead or manganese paper was affected, nor were their solutions changed by actual contact with the oil; a mixture of sulphuric and chromic acids, however, became violet. Confining now the experiments to turpentine, a quantity of this substance was placed in a bottle with an equal volume of water, air was slowly drawn through it for many hours, and it was allowed to stand several days in a shallow dish exposed to the air. The water, on examination, gave the blue coloration with starch paper, precipitated manganese dioxide from potassium permanganate, and produced a violet coloration with chromic and sulphuric acids; but caused no change either in lead acetate or manganese sulphate.

The oil itself, even after washing, gave both the starch and the chromic acid tests. On repeating these tests in vacuo, no difference was observed. Hence it appears that oxidized oil of turpentine contains, even after washing, a body with the reactions of hydrogen dioxide. To test this hypothesis, the oil was treated with lead peroxide, manganese peroxide and sodium hyposulphite. All these substances appeared to destroy the active agent, but in a decreasing order. If attached oxygen be the active agent, it must be more stable than ordinary ozone, which is at once destroyed by the hyposulphite. Noticing that heat, so far from destroying the active agent, actually increased it, equal volumes of turpentine oil and water were distilled incompletely. The residual oil in the retort, which could now contain neither hydrogen dioxide nor ozone, still gave strong reactions with the starch and chromic acid tests, as did also the residual water. Neither the oil nor the water of the distillate gave any coloration with either reagent. The active principle present can therefore be neither ozone nor hydrogen dioxide, but a body derived from the turpentine by the action of air and water upon it. Assuming it to be the hydrate of terpene oxide, $C_{10}H_{16}O.H_2O$, zinc chloride should destroy it. Accordingly, active turpentine was distilled with zinc chloride, and the active properties were entirely destroyed. That the effect was not due to the heat employed appears from the fact that, heated alone to 75°, the temperature at which, when treated with zinc chloride, it is destroyed, it remains still active. At the boiling point of the turpentine, 160°, it disappears. The author mentions in addition, two peculiar reactions of this active turpentine. In the one, the oil converted yellow mercuric oxide into a black powder, losing at the same time its activity. In the other, a mixture of the oil and acidulated water being electrolyzed, the oil was found to be inactive. In consequence of these results, the conclusion is that the active agent in turpentine oil exposed to light is hydrate of terpene oxide. Experiments are in progress to prove or disprove this conclusion.—*J. Chem. Soc.*, II, xii, 511, June, 1874. G. F. B.

3. *Action of the Copper-zinc Couple on the Chlorides of Ethylene and Ethylidene.*—GLADSTONE and TRIBE have continued their researches on the action of the copper-zinc couple discovered by them, upon organic bodies. In this paper, they describe its action on ethylene and ethylidene chlorides, which are isomers having the formula $C_2H_4Cl_2$. The former had a specific gravity of 1·272 at 14°, and its refractive index for A was 1·4448. The latter—prepared from ethyl chloride by the action of chlorine—boiled at 61°, had a specific gravity of 1·201 at 13°, and its refractive index for A was 1·4183. The refraction equivalent of the ethylidene chloride was consequently 34·6, that of the ethylene chloride 34·5; theory gives 35. The action of the zinc couple alone upon either of these chlorides was trifling even at the boiling temperature. In presence of water a small action took place; and in presence of alcohol the action was more

energetic, especially on the ethylidene compound. Five c.c. of this substance mixed with twice its volume of absolute alcohol was added to twice the usual quantity of the couple, and the whole heated to 60°–62° for ten hours and then to 80° for three hours more. The volume of gas evolved was 932 c.c., the theoretical yield being 989 c.c. There remained in the flask a viscid liquid, readily soluble in absolute alcohol. From this solution, water precipitated zinc oxide. The atomic ratio between the zinc and chlorine was 1 : 1·08. This fact, together with the deportment of the substance to heat, leave little room to doubt that the body is zinc chloroethylate $\left.\begin{matrix}C_2H_5O\\Cl\end{matrix}\right\}Zn$. The evolved gas, except five per cent absorbable by fuming sulphuric acid, was ethyl hydride. The reaction appears, therefore, to be as follows:

$$C_2H_4Cl_2+\left(\left.\begin{matrix}C_2H_5O\\H\end{matrix}\right\}\right)_2+Zn_2=C_2H_6+\left(\left.\begin{matrix}C_2H_5O\\Cl\end{matrix}\right\}Zn\right)_2.$$

Since the action of the couple upon ethyl chloride in presence of alcohol should produce, by analogy, ethyl hydride and zinc chlorethylate, the result now obtained proves the somewhat unexpected fact that the replacement of an atom of hydrogen in the chloride by one of chlorine causes no other effect in the action of the couple than the production of a second molecule of the chlorethylate. Probably, therefore, ethyl chloride is intermediately formed. Ethylene chloride, on the contrary, when heated in presence of the couple, in presence of alcohol, acts quite differently. Only 80 c.c. of gas was evolved even after five hours heating, and the atomic ratio of zinc and chlorine in the resulting liquid was found to be 1 : 1·9. The action of the couple has enabled these two isomers to be clearly distinguished from each other and the constitution of the former to be fixed. It has also completed the list of the zinc haloid derivatives.—*J. Chem. Soc.*, II, xii, 615, July, 1874.
G. F. B.

II. Geology and Natural History.

1. *Notes on the new edition of Mr. Darwin's work on the Structure and Distribution of Coral Reefs* (1874); by James D. Dana.*—Mr. Darwin, in the new and much improved edition of his work on Coral Reefs, mentions some points in the subject, on which he still finds reason to differ from the writer. I think that with regard to one or two of these points he has not fully understood my views; and, as to the others, that the arguments and facts which I have brought out have not received all the consideration they deserve. A review of some statements in his work may, therefore, be profitable. I follow the order of his criticisms as briefly stated in the first half of his preface.

* *The Structure and Distribution of Coral Reefs*, by Charles Darwin, M.A., F.R.S., F.G.S. Second edition, revised. 263 pp. 12mo, with three plates. London, 1874. (Smith, Elder & Co.)

(I.) The second sentence of the Preface is as follows:

"In this work [Dana's Corals and Coral Islands] he [the author] justly says that I have not laid sufficient weight on the mean temperature of the sea in determining the distribution of coral reefs; but neither a low temperature nor the presence of mud banks accounts, as it appears to me, for the absence of coral reefs throughout certain areas; and we must look to some more recondite cause."

The first two clauses of this sentence are true—the *but* between them being removed, as it may lead some readers to suppose the alternative mine. Yet Mr. Darwin's work does not show that even now he appreciates the influence of oceanic temperature on the distribution of coral reefs. In his discussions on the distribution of reefs and the causes limiting the same, this agency, the chiefest with marine life, both for depth and surface, according to all zoologists, is scarcely mentioned. There is one allusion to the subject on page 81. Mr. Darwin says: "I at first attributed this absence of reefs on the coasts of Peru and of the Galapagos Islands to the coldness of the currents from the south, but the Gulf of Panama is one of the hottest pelagic districts in the world;" and a note is added, giving some sea temperatures of the region referred to. Thus the cause is set aside even for the seas along the Peruvian coast, although the mean winter temperature of the water there is lower than exists in any reef region in the world, and is therefore sufficient of itself to exclude reefs. The fact that there are only small patches at Panama, where the temperature is tropical, does not annul the fact that the seas of Peru and the Galapagos are too cold for corals. Where temperature excludes, there is no use in discussing other unfavorable conditions.

The causes limiting growth and the distribution of reef-making corals and coral reefs, which I have discussed and applied in my work, are *seven* in number.

(1.) Marine temperature.

(2.) Fresh and impure waters from the entrance of large rivers; and muddy bottoms.

(3.) Deposition of sediment borne by rapid tidal currents.

(4.) The depth of water along coasts exceeding 100 feet, that is, exceeding the depth to which reef-corals may grow—a common condition along bold coasts, and often explaining, as I have found, the contrasts between the reef-bordered and open coasts of the same island.

(5.) Exposure to the heat of submarine volcanic eruptions (pp. 299–317).

(6.) The progressing coral-island subsidence too rapid for the polyps to keep the reef well at the surface, if at all (p. 270): which cause may lead, in atoll seas, to very narrow fringing reefs; to small sizes in coral atolls and a more or less complete obliteration of the lagoon; and to a submerging of the coral island beneath the surface; or, finally, to a complete disappearance of the island (pp. 332, 369).

(7.) The direction and temperature of oceanic currents (p. 112): this cause accounting for the non-distribution of central-Pacific species of corals to the Panama coast, and the paucity of species there, with the absence of the large Astræa group and the Madrepores.

On this last point I say in explanation, on page 112: "Owing to the cold oceanic currents of the eastern border of the Pacific—one of which, that up the South American coast, is so strong and chilling as to push the southern isocryme [the line passing through points of equal mean oceanic temperature for the coldest month of the year] of 68°, the coral-sea boundary, even beyond the Galapagos, and north of the equator—the coral-reef sea, just east of Panama, is narrowed to 20°, which is 36° less of width than it has in mid-ocean; and this suggests that these currents, by their temperature, as well as by *their usual westward direction*, have proved an obstacle to the transfer of mid-ocean species to the Panama coast." For the same reason, the transfer of corals—warm-water species—from the West Indies or Bermudas, eastward, to *western Africa*, is impossible. The width of the coral reef region on the African side of the Atlantic is only 15°, while it is 48° toward the American coast, and the tropical current is *eastward*.

A proper understanding of the action of the various causes influencing the growth and distribution of polyps and reefs, which have been mentioned in the preceding paragraphs, may leave much less than has been imagined for that "more recondite cause."

I did not think to include among the causes a too rapid *upward* change of level—on which Mr. Darwin lays much stress. But I recognized the fact that when a rise, like that which has occurred at the island of Oahu [putting an extended range of reef thirty feet out of water], takes place, and so divides the area of reef into an elevated and non-elevated portion, the latter will be, on this account, narrower than it would have been had the land been stationary. But the cause does not appear to me to have very many examples.

(II.) The third sentence of the Preface reads thus:

"Professor Dana also insists that volcanic action prevents the growth of coral reefs much more effectually than I had supposed; but how the heat or poisonous exhalations from a volcano can affect the whole circumference of a large island is not clear." And this is followed by the remark: "Nor does this fact, if fully established, falsify my generalization that volcanos in a state of action are not found within the area of subsidence, whilst they are often present within those of elevation."

In my discussion of this subject I have attributed the destruction here referred to about islands of active, or recently active, volcanos, not to aerial eruptions, as might be suspected from Mr. Darwin's words, but to *submarine;* and I happen to have said nothing about "exhalations." I have drawn my conclusions especially from four examples (pp. 302, 305, 306), the island of Hawaii

(Sandwich Islands), about which recent eruptions, and partly submarine, have taken place on the east, southeast, south and west slopes of the island, or through more than half of its circumference; Savaii, the largest of the Samoan or Navigator Islands, and the last of the group to become extinct, as its lava streams show; the eastern half of Maui, whose great crater must have been recently in action, while the western half bears the fullest evidence of long extinction; and the northern extremity of the Ladrones. I state that reefs often occur on favored parts of even such volcanic islands, as they well might if submarine eruptions were the cause, and I mention examples; thus agreeing with Mr. Darwin's criticism that "the existence of reefs, though scantily developed, and, according to Dana, confined to one part of Hawaii, shows that recent volcanic action does not prevent their growth." My statement about that Hawaiian reef is worded thus: "the only spot of reef *seen* by us was a submerged patch off the southern cape of Hilo Bay." Mr. Darwin cites an observation with regard to the occurrence also of reefs on the northern coast of Hawaii, which accords precisely with the principle I have laid down, since the northern part of the island is, as I state in my Geological Report of the island, that which was earliest extinct, and is oldest in all its features, and therefore that which would not have been reached by the submarine eruptions. The western peninsula of Maui, or the old part, has its coral reefs, while the eastern, or part recently active, has almost none. Savaii, in like manner, has coral reefs on its western and northern shores, while elsewhere without them.

I failed to find evidence in the case of either of these volcanic regions that they are situated within areas of elevation rather than subsidence. *Only ten miles* west of Savaii lies the large island of Upolu, having very extensive reefs—on some parts of the north side three-fourths of a mile wide; and it has not seemed safe to conclude that, while Upolu thus bears evidence of no movement or of but little subsidence, Savaii was one of elevation; or that the north and west sides of Savaii have differed in change of level from the rest of the island. In the island of Maui, having reefs on its old western half, it can hardly be that the eastern peninsula has changed its level quite independently of the western. In the linear group of the Ladrones the active volcanos are at the north end; the islands of the group are very small at that end, without coral reefs, while large at the other and with broad reefs. One of them, Assumption Island, near which our Expedition passed, is only a small, steep, cinder cone, the vent of a submerged volcanic mountain. Such facts afford, therefore, some reason for my statement that "the Ladrones appear to have undergone their greatest subsidence at the northern extremity of the range;" and no observations yet made suggest the contrary view.

The general proposition, that active volcanos are absent from areas of subsidence appears to me to need better proof than it has received. As regards the Pacific Ocean, I have found nothing to sustain it. The subsidence of the coral island area of the ocean was one of so vast extent—the breadth 4000 miles, according to

Mr. Darwin—that the sinking could have been no obstacle to the existence and cotemporaneous working of volcanos.

(III.) The next point in the Preface is a right correction of a misunderstanding on my part of one of Mr. Darwin's statements. It says: "Professor Dana apparently supposes (p. 320) that I look at fringing reefs as a proof of the recent elevation of the land, but I have expressly stated that such reefs, as a general rule, indicate that the land has either long remained at the same level, or has been recently elevated. Nevertheless, from upraised recent remains having been found in a large number of cases on coasts which are fringed by coral reefs, it appears to me that, of these two alternatives, recent elevation has been much more frequent than a stationary condition.."

When my work passes to a second edition, I shall make the needed correction.

But I still hold that, while barrier reefs, as Mr. Darwin urges, are proofs of subsidence, small or fringing reefs are in themselves no certain evidence of a stationary level, and are often evidence of subsidence, even a greater subsidence than is implied by barrier reefs. I have already stated that one cause limiting distribution of reefs is bold shores, a wall of rock of even a hundred and fifty feet producing a complete exclusion. If Tahiti were to subside two thousand feet, it would be an island of precipitous shores all around, like the Marquesas, instead of one with broad shore planes. Such bold shores are evidence of subsidence; and as only very small reefs, if any, could find footing about such an island, the narrow reef would be another consequence of the subsidence, and no evidence of a stationary condition. Again, the gradual sinking of an atoll, like the Gambier group, or of a Tahiti with its barrier reefs, at a rate a little fast for the growing corals, would necessarily contract the reef region, reduce the barrier reefs of a Tahiti to narrow fringing reefs; and make an atoll, however large, a small atoll with the reef-border narrow and the lagoon perhaps obliterated. An atoll thus reduced to a sand bank is an example of the effects of subsidence, and affords no evidence of elevation or of a long stationary condition of the region; and the same may be true of a region of narrow fringing reefs. I landed on two of the small coral islands of the equatorial Pacific which are in just the condition here described; and my book contains descriptions of others from a good observer, J. D. Hague, who resided on them several months "for the purpose of studying the character and formation of the deposits" of guano. I found the depression of the old lagoon, in one case partly, in the other wholly, dry; and I found also that the living reefs around were narrow. Mr. Darwin inclines to regard islands of this kind as either evidence of no movement, or, of elevation. On the contrary, since the coral-islands of the south Pacific diminish in size toward the region of these small islands, and since the region just beyond, to the north and northeast, is free from islands, and since all the features are such as would come to them from a continuation of the coral island subsidence to its nearly fatal end, I believe still that I was right in considering the ocean bottom in this part to have undergone a general subsidence greater than that to the south, southwest and west, where the atolls and barrier reefs are large.

Again, if submarine eruptions are destructive, narrow reefs may exist about volcanic islands that are undergoing a subsidence. Making a reef is slow work; and, judging from the eruptions of the present century about Hawaii, reefs would have had a poor chance in the past to form, except along the coasts that were out of reach of the submarine action.

With so many causes for the existence of narrow or fringing reefs, or of small patches of corals, it is assuredly unsafe to make them, without other corroborating testimony, evidence of a stationary condition of a region, or of an elevating movement rather than a subsiding.

(IV.) The next point in the Preface is stated as follows:

"Profesor Dana further believes that many of the lagoon islands in the Paumotu or Low Archipelago and elsewhere have recently been elevated to a height of a few feet [elsewhere stated, two or three feet] although formed during a period of subsidence; but I shall endeavor to show, in the sixth chapter of the present edition, that lagoon-islands which have long remained at a stationary level often present the false appearance of having been slightly elevated." And, in the body of the work, where the subject is taken up (p. 168), Mr. Darwin remarks that my belief in these small local elevations is grounded chiefly on the shells of Tridacnas embedded, in their living positions, in the coral rock at heights where they could not now survive.

The catalogue of such elevations which I give—after a dozen pages devoted to a discussion of the evidence respecting each—is as follows:

Paumotu Archipelago,	Honden,	2 or 3
" "	Clermont Tonnerre,	2 or 3
" "	Nairsa or Dean's,	6
" "	Elizabeth,	80
" "	Metia or Aurora,	250
" "	Ducie's,	1 or 2?
Tahitian Group,	Tahiti,	0?
" "	Bolabola,	?
Hervey and Rurutu Groups,	Atiu,	12?
" " " "	Mauke,	somewhat elevated
" " " "	Mitiaro,	" "
" " " "	Mangaia,	300
" " " "	Rurutu,	150
" " " "	Remaining Islands,	0?
Tongan Group,	Eua,	300?
" "	Tongatabu,	50 to 60
" "	Namuka and the Hapaii,	25
" "	Vavau,	100
Savage Island,		100
Samoan or Navigator Islands,		0
North of Samoa,	Swain's,	2 or 3
" " "	Fakaafo, or Bowditch,	3
" " "	Oatafu, or Duke of York's,	2 or 3

Scattered Equatorial Islands,	Washington,	2 or 3?
" " "	Christmas,	?
" " "	Jarvis's,	8 or 10
" " "	Malden's,	25 or 30
" " "	Starbuck's,	?
" " "	Penrhyn's,	35
" " "	Flint's and Staver's,	?
" " "	Baker's,	5 or 6
" " "	Howland's.	?
" " "	Phœnix and McKean's,	0
" " "	Enderbury's,	2 or 3?
" " "	Newmarket,	6 or 8?
" " "	Gardner's, Hull's, Sydney, Birnie's,	0?
Feejee Islands,	Viti Levu and Vanua Levu, Ovalau,	5 or 6?
	Eastern Islands,	0?
North of Feejees,	Horne, Wallis, Ellice, Depeyster,	0?
Sandwich Islands,	Kauai,	1 or 2
" "	Oahu,	25 or 30
" "	Molokai,	300
" "	Maui,	12
Gilbert Islands,	Taputeuea,	2 or 3
" "	Nonouti, Kuria, Maiana and Tarawa,	3 or more.
" "	Apamama,	5
" "	Apaiang or Charlotte,	6 or 7
" "	Marakei,	3 or more.
" "	Makin,	?
Carolines,	McAskill's,	60
Ladrones,	Guam,	600
"	Rota,	600
Feis,		90
Pelews,		0?
New Hebrides, New Caledonia, Salomon Islands,		none ascertained.

Of the cases of elevation here included, in *only two* are shells of Tridacnas alluded to; these are Honden Island and Clermont Tonnerre, in the Paumotus. It is not necessary to go over the evidence for the several cases, as it is stated at length in my work.

Mr. Darwin, while speaking on the subject of local elevations, on p. 176, and discussing the facts as regards the Samoan (Navigator) Islands, adds that "in another place he [Mr. Dana] says (p. 326) that some of the [Samoan] islands have probably subsided." From the remark the reader would infer that this Samoan subsidence was a local subsidence, like the elevations under consideration. But in fact my statement is in a chapter on the general coral-island subsidence, and, on the page there referred to (p. 326), I cite Mr. Darwin's conclusions as to the Gambier Island subsidence, and put with it my own from the width of the reefs of Upolu and other reef-bordered islands. At the same place I allude to the greater subsidence of Tutuila—the island next to the west, as proved by its bold shores and small reefs.

In conclusion, if I differ widely, for the reasons above stated, from Mr. Darwin, as to the limits of the areas of subsidence and elevation in the Pacific, and believe that the new edition of his work shows little appreciation of some of the most important causes that have limited the distribution of coral reefs, I have, as

I say in my work, the fullest satisfaction in his theory for the origin of atoll and barrier forms of reefs, and in the array of facts of his own observation which illustrate the growth of coral formations.

2. *The Mineralogical and Geological Collections* of the late Dr. Gerard Troost, formerly of Nashville, Tenn., have been purchased for the sum of $20,000 by the Trustees of the Public Library in the city of Louisville, Ky. Dr. Troost's well known cabinets, the fruits of more than forty years of industrious collecting, contains 13,582 specimens in mineralogy, 2,815 organic remains, between 2,000 and 3,000 rock specimens, besides a considerable collection of modern shells and some archæological specimens. The mineralogical collection is catalogued and minutely described in two large manuscript volumes.

3. *Mineralogical Contributions of G. vom Rath, of Bonn.* No. xiii. (Pogg. Ann., clii, 1874.)—This new number of Vom Rath's mineralogical papers contains notes on the crystallization of Tridymite; on a crystal of calcite from the amygdaloid of the Lake Superior region; a peculiar twinning of rutile and hematite; remarkable crystals of artificial native copper; hypersthene of Mount Dore in Auvergne; on Foresite, a new zeolite from the granite of Elba; and the notes at the close contain observations on some triclinic feldspars, and on crystals of cordierite, from Lake Laach. The article is illustrated by a large plate containing numerous excellent figures of the crystals described. A large part of them represent remarkable twins of the species tridymite. The new zeolite, *Foresite*, is orthorhombic, and near stilbite in form, with a very distinct, pearly cleavage parallel to *i-ĭ*. Octahedral faces on the summit make an angle of about 121° with *i-ĭ* and 132° with *O*. G=7·403 – ·7407 (at 170° C.). A mean of the analyses gives silica 49·96, alumina 27·40, lime 5·47, magnesia 0·40, potash 0·77, soda 1·38, water 15·07; from which the oxygen ratio for the protoxide, alumina, silica and water is 1 : 6 : 12 : 6.

4. *Fifth Annual Report of the Geological Survey of Indiana*, made during the year 1873; by E. T. Cox, State Geologist; assisted by Professor John Collett, Professor W. W. Borden and Dr. G. M. Levette. 494 pp. 8vo, with four maps.—This volume opens with a Report on the Vienna Exposition of 1873, to which Mr. Cox was Commissioner from Indiana; and a chapter on Spiegeleisen Manufacturing by H. Hartmann. After these follows the Geological Report, in which the geology of several of the counties of the State is described, and information is given respecting various mineral products of economical importance, with analyses of iron ores, limestones and hydraulic cement rocks.

5. *Palæozoic Fossils.* Vol. II, Part i. By E. Billings, F.G.S., Paleontologist of the Geological Survey of Canada under Alfred C. Selwyn, Director. 144 pp. 8vo, with 9 lithographic plates of figures of fossils.—Mr. Billings continues in this volume the publication of his very valuable paleontological labors connected with the Geological Survey of Canada. It treats of fossils of the Gaspé series of rocks (which are Upper Silurian and Devonian in age);

on new fossils from the Primordial of Newfoundland; on the genus *Stricklandinia*, and the Canadian species; on the structure of the Crinoidea, Cystidea and Blastoidea; on some fossils of the Arisaig series of rocks (Nova Scotia Upper Silurian).

6. *Revision of the Genera and Species of the Tulipeæ;* by J. G. Baker, F.L.S. Jour. Linn. Society, 14, no. 76, July, 1874.—This important monograph fills almost a hundred pages of the Linnean Society's Journal. It is prefaced by an instructive account of the structure of the flowers, fruit, and especially of the bulbs of this group. Mr. Baker remarks that: "The group now under consideration has never an introrse dehiscence of the anther, and in this respect recedes from the typical *Liliaceæ*, as I intend to explain more fully further on." Yet we find it nowhere explained how there can be any more typical *Liliaceæ* than *Lilium* itself and its nearest allies. As might be expected, he is "now quite satisfied" that *Liliaceæ* and *Colchicaceæ* are not to be ordinally separated, by considerations drawn from the Liliaceous side,—a conclusion which study of the Melanthaceous and other genera will be likely to confirm.

The Tulipaceous genera are here reduced to six: *Fritillaria*, with 55 admitted species, *Tulipa* with 48, *Lilium* with 46, *Calochortus* with 21, *Erythronium* with 5, and *Lloydia* with 4 species. Nearly all inhabit the northern hemisphere; *Tulipa* belongs wholly to the Old World, and *Calochortus* to the western part of the New. The Lilies appear to be well disposed under five subgenera, and the characters of the bulbs play a conspicuous part in specific definition. Dr. Kellogg's *L. Washingtonianum* of California is our sole representative of the *Eulirion* subgenus; *L. Philadelphicum* and *L. Catesbæi*, of *Isolirion;* while of the *Martagon* section we rejoice in *L. Canadense* of the east, with three Californian or Pacific varieties appended; *L. pardalinum* Kellogg, with three appended varieties, one of which, *Bourgæi*, from Lake Winnipeg, needs further looking to, being far out of range; *L. superbum*, with *L. Carolinianum* Michx. as a variety (the flowers of this, as we know it, are often smaller than in the larger forms of *L. Canadense*, and the bulbs of the two by no means so distinct as the character would imply); *L. Roezlii* of Utah, and the coast range of California, said to be known by its narrow acute perianth-segments; *L. Columbianum* of Hanson, from Oregon, said to be distinguished from the smaller *L. Canadense* mainly by the want of rhizoma to the bulb; and *L. Humboldtii* of Roezl, to which Kellogg's *Bloomerianum* is referred.

Of the ten subgenera of *Fritillaria*, six have tunicated bulbs and belong to the Old World—with one exception, if we follow Mr. Baker. For he refers to his third subgenus our American *F. pudica* (*Lilium? pudicum* Pursh, and *Amblirion pudicum* Raf.), and accordingly gives to that coated-bulbed subgenus Rafinesque's name, *Amblirion*. If he has really verified this character in our pretty little species, all is well; but the bulb certainly appears to have the same structure as the other American species, viz: a solid body producing numerous granules or granular scales. The eight remaining American *Fritillariæ* are distributed among three subgenera, which as to our species will hardly hold good. We do not reckon *F. alba* of Nuttall's Genera, which is doubtless a *Calochortus*, and probably *C. Nuttallii*. The *Crown-Imperial* (*Petilium*), Asiatic and with scaly bulb, forms the tenth subgenus.

Erythronium has a majority of American species. The key confines *E. grandiflorum* to yellow flowers, which does not accord with most of the Californian, and seemingly also of the Oregon plants referred to this species. *E. propullans* is remarkable for its small flowers as well as for the remoteness of the new bulb. After what has been done for *Calochortus* by Mr. Watson in this country and Mr. Baker in England, we may soon hope to understand this attractive but still difficult genus. The section *Cyclobothra*, being restricted to Mexican species, with fibrose-tunicated bulbs and narrow pods, the northern ones before referred to it constitute Baker's section *Macrodenus;* and he distributes *Calochortus proper*, with membranous bulb-coatings, into two subgenera, *Platycarpus*, with an oblong capsule, and *Mariposa* (an apt use of the Californian popular name, meaning butterfly), with narrow pods,—a difference which is seldom available at flowering-time, and apparently of barely specific importance. Of the species there is somewhat yet to be said and done; but Mr. Baker is heartily to be thanked for this as well as for other parts of his valuable elaboration of *Liliaceæ*. A. G.

7. *Asexual Growth from the Prothallus of Pteris Cretica.*—Dr. Farlow's paper, published last spring in the 10th volume of the Proceedings of the American Academy of Arts and Sciences, has been reprinted in the current (14th) volume of the London Journal of Microscopical Science, with a few additions by the author. The figures are neatly reproduced in two plates. The correction as to the species of *Pteris* concerned is here made, in the title and elsewhere. When first published the plantlets were supposed to be *P. serrulata*, but the correction was appended in a note in the Proceedings of the American Academy. We are sorry to find that, through some editorial oversight, or the want of it, the fact that this paper is reprinted from the American Academy's Proceedings, vol. 10, is not stated. The author of the paper has now returned to this country, and we understand will take charge of a laboratory for Cryptogamic Botany in Harvard University. A. G.

8. *Zizania aquatica*, the Indian Rice, as appears from Gardeners' Chronicle for August 1, is a new material for paper, of much promise. Other grasses have been more or less used; but we were not aware of the employment of *Zizania*. The British market is to be supplied from Upper Canada (Ontario), by a company organized for the purpose.

9. *Botany of S. Pacific Exploring Expedition under Admiral Wilkes.*—More than a dozen years ago, the Library Committee of Congress began the printing of the 17th volume of the results of the

U. S. Exploring Expedition under Commodore Wilkes. It was devoted to the lower Cryptogamic Botany and the general botanical collection made on our Pacific coast, from Puget Sound to San Francisco. The earlier part of the volume was printed in or before the year 1862. It contained, 1, the *Mosses*, by the lamented Sullivant, who secured a small separate edition with letter-press in folio, to match the plates: a few copies of this are still to be had. 2. The *Lichenes* by Professor Tuckerman; the *Algæ* by the late Professors Bailey and Harvey; the *Fungi* (only 31 species) by the late Dr. Curtis and Mr. Berkeley. The present writer secured some extra copies of this portion, and also of the plates of the portion next to be mentioned, and distributed them so as to make them generally known among botanists. A year and a half ago, shortly before the lamented death of Dr. Torrey, the MSS. of his report on the Phænogamous plants of our Pacific coast was called for: it has been carried through the press since the author's death. The present writer secured a small edition, and has had these parts, with the large folio plates (folded), made into a bound volume, entitled *U. S. Exploring Expedition Botany.* I. *Lower Cryptogamia.* II. *Phanerogamia of the Pacific coast of North America.* It is an imperial 4to, of 420 pages of letter-press, and 29 plates. Only 20 copies are on sale, with Westermann and Co. in New York, and at the Herbarium of Harvard University, Cambridge. From the same may be obtained a very few copies of the *Musci* of the expedition, by Sullivant, which properly belongs to the same volume, but of which the letter-press was made up in double columns into imperial folio pages and bound with the plates into a substantial volume. The government copies of the volume, only one hundred in number, are not to be had, having been all presented to foreign courts and State authorities. A. G.

III. Miscellaneous Scientific Intelligence.

1. *Territorial School of Mines, Colorado.*—A school of mines has been established by the Territorial Government at Golden, Colorado, one of the best places in the country for practical instruction. E. J. Mallett, Jr., is Dean of the Faculty and instructs in Theoretical Engineering; E. L. Berthoud, in Civil Engineering and Geology; R. Pearce, in Theoretical and Practical Metallurgy; J. Poole, in the Mechanical Preparation of Ores; F. Schmidt, in Mathematics, German and Spanish, and A. Lakes, in Free-hand and Mechanical Drawing.

2. *Alexander Wilson.*—A statue of Alexander Wilson, the ornithologist, will, in a few weeks, be erected within the burying-ground of the abbey of Paisley, and within a few minutes' walk of the spot where he was born.—*Athenæum*, June 27.

3. *Sixth Annual Report on the Noxious, Beneficial and other Insects of the State of Missouri.* Made to the Board of Agriculture by C. V. Riley, Entomologist. 170 pp. 8vo. Jefferson City, Missouri.—A valuable report economically and scientifically. The numerous woodcuts are unusually good, and, with few exceptions, are from drawings by the author.

4. *Half Hours with Insects.*—Prof. A. S. Packard, Jr., is continuing his chapters on Insects, in the Half-Hour Recreations in Natural History of Estes & Lauriat. The third part (out of the twelve to be issued) treats of the Relations of Insects to Man.

5. *British Association.*—Detailed reports of the papers read at the meeting will be found in Nature, now issuing, commencing with No. 251, August 20th, which contains Prof. Tyndall's address.

6. *Dana's Manual of Geology.*—An engraver's mistake which was corrected in the stereotype plates of the first edition of this work, has passed without correction in the first issue of the second. In the map of England on page 344, 8 should be 9, and 9 should be 8. It will be made right before printing again.

OBITUARY.

Death of Prof. JEFFRIES WYMAN.—This most estimable man and most excellent naturalist—facile princeps of American comparative anatomists—died suddenly at Bethlehem, N. H., on the 4th inst., and his mortal remains were committed to the earth in Mount Auburn on the 8th. He was born at Chelmsford, Mass., August 11, 1814, and had, therefore, just completed sixty years of age. He was graduated at Harvard University in 1833, took his medical degree in 1837, pursued his medical, and especially anatomical and zoological, studies at Paris for two years, when, returning to Boston, he was for a few years curator of the Lowell Institute. Here he delivered two courses of lectures on comparative anatomy and physiology (one of which was published); and here he first developed those admirable gifts as a scientific instructor which made him such a favorite with all serious students,—the gfit especially of lucid and well-ordered exposition, from which all adventitious matters were rigorously excluded. In 1844, he accepted the chair of anatomy and physiology in the medical school at Richmond, Virginia, a branch of Hampden-Sidney College. In 1847, after the death of Dr. Warren, he was reclaimed by his *alma mater*, and made Hersey Professor of Anatomy at Cambridge, Dr. Holmes having been appointed to the chair in the medical school in Boston. Prof. Wyman's position secured to him a good opportunity for prosecuting his researches, and for building up the museum of comparative anatomy—the objects which he had most at heart—but only a slender salary. After a time, however, this was increased by the judicious gift from a late citizen of Boston, who recognized Prof. Wyman's great value to the university and to science, and who stipulated that the income of his endowment should be paid to Prof. Wyman throughout his life, whether he held the professorial chair or not. He fulfilled its duties most acceptably, down to a recent period, when his state of health, for a long time delicate, and of late requiring a milder winter climate, demanded the abridgement, and at length the cessation, of his lectures and class work. But he merely changed the kind of work. When the Peabody Museum of American Archeology and Ethnology was established, the founder made Prof. Wyman one of his trustees, and the board committed the incipient museum to his charge and direction. The seven annual reports which he has drawn up and published—the last only a few weeks before his lamented decease—and, still more, the museum itself, which already takes high rank among such establishments, testify to his devotion and rare ability. Whatever this museum may become in after years—and much may be hoped of its future—it must, for a long while, be Prof. Wyman's memorial, equally with that of its founder. Means have been wisely and liberally provided, and his associates have done their part; but the museum has been created by Prof. Wyman. Arranged by his own unaided hands, it bears throughout the impress of his untiring and conscientious labor, scrupulous accuracy, and orderly and sagacious mind.

We cannot here undertake to recount his written contributions to science. They are mainly contained in the Journal and Proceedings of the Boston Natural History Society, of which he was for more than a dozen years the faithful president; in the Smithsonian Contributions to Knowledge, and in the pages of our own Journal. That they were not more numerous and important than they are must be attributed, partly to the insidious disease which for years has sapped his strength, partly to over-caution and the desire of completely mastering whatever he undertook, partly to a very dispassionate and unambitious temperament. He was one of those rare men who, in more than one sense, are "not easily provoked." Although his judgment—as keen as cool—was always wakeful, and his love of exact truth pervading, it is not remembered that he ever had a controversy. It was said by an experienced judge of character, who knew him long and intimately, that the Beatitudes ought to be read at his funeral, for few, within his knowledge, had so nearly exemplified them.

Professor Wyman had suffered from pulmonary disease for many years, and his life has of late been very precarious. But the present year seemed, on the whole, assuring to his friends of a longer respite, when a hemorrhage of the lungs suddenly terminated his most useful and honorable career. He leaves two daughters, children of his first wife, and a youthful son by his second wife, who did not long survive the birth. A. G.

Third Report of the Meteorological Office of the Dominion of Canada, for the fiscal year ending June 30th, 1873; by G. T. Kingston, M. A., Superintendent.

Report for the determination of the Astronomical Coordinates of the Primary Stations at Cheyenne, Wyoming Territory, and Colorado Springs, Colorado Territory, made during the years 1872 *and* 1873: Geographical and Geological Explorations and Surveys west of the Hundredth Meridian; First Lieut. G. M. WHEELER, Corps of Engineers, in charge. Dr. J. KAMPF and J. H. CLARK, civilian, astronomical assistants. 82 pp. 4to. Washington, 1874.

Psyche. Organ of the Cambridge Entomological Club. Vol. i, No. 1. 4 pages 8vo. Cambridge, Mass., May, 1874. B. Pickmann Mann, Editor.—This first number of Psyche contains a prospectus, a list of the English names of butterflies, the titles of a few recent works, a brief note on the discovery of a specimen of *Nymphalis Milberti*, and another announcing that Hentz's papers on spiders are to be reprinted in a single volume by the Boston Society of Natural History, and edited by Mr. E. Burgess.

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[THIRD SERIES.]

ART. XXVII.—*On the Number and Distribution of the Bright Fixed Stars*; by B. A. GOULD.

(Read before the American Association for the Adv. of Science, Aug. 17, 1874.)

THE magnificent work of Argelander entitled "Durchmusterung des nordlichen Himmels," is well known to lovers of astronomy. His problem was no less than the formation of a complete list of all the stars of the northern hemisphere to the ninth magnitude inclusive, together with as many as possible of the 9–10 magnitude. The undertaking was successfully carried out, affording not only an exhaustive series of charts, but likewise a "working-list," which an association of northern observatories is now employing for the determination of the accurate positions of the 315,000 stars which it contains. It furthermore records the aspect of the visible heavens, at the time, with an accuracy amply sufficient for all purposes which do not require minute precision.

In this work, the magnitude of each star was estimated to the nearest half unit as it passed through the field of view; and since all the stars were observed more than once, and most of them several times, the mean of the several estimates was taken and is given in the published catalogue to the nearest tenth of a unit. In 1869 Professor Littrow of Vienna made a careful enumeration of the number thus given for each degree of magnitude, in order to ascertain how far the results would indicate an approximate uniformity of distribution for the stars lying within the portion of space under consideration.

Could we assume, 1st, that, in general, the apparent brightness of a star depends upon its distance from us, all being of the same order of intrinsic brilliancy, and 2dly, that stars are distributed through space with approximate uniformity, the total number visible, down to any given magnitude, would be proportional to the contents of the sphere within which stars of this magnitude are contained. Thus the degree of approximation to the truth, which is fairly attributable to the hypothesis mentioned, may be inferred from the degree of accordance which can be obtained between two series of numbers, the one representing the number of stars of each successive order of magnitude actually existing in the heavens, and the other being proportional to the contents of those spherical shells whose limiting radii correspond to these respective magnitudes. If any accordance at all satisfactory should be found to exist, it would render the assumptions probable to a corresponding extent, and would afford at the same time an independent, even though indirect and crude, determination of the constant ratio between the amounts of light which belong to two successive magnitudes, and thereby an indication of the relative distances of the stars.

If the data on which the inquiry is based be sufficiently extensive, we may reasonably expect that the effects of considerable deviations from perfect exactness in the general hypotheses will be masked in the totals, provided the assumptions be not essentially erroneous. But there are serious difficulties in the way of an accurate comparison, arising from the inadequacy of our data, in consequence of inevitable errors in the estimates of magnitude, the moral impossibility of maintaining exactly the same ratio for different degrees of brightness, and other almost equally important obstacles. Notwithstanding all these, Professor Littrow considers his results as justifying the belief that there is a considerable degree of general uniformity, both in the distribution and the intrinsic brilliancy of the fixed stars which belong within the limits of this investigation; sufficient, indeed, to warrant an application of the formulas, resulting from this inquiry, to regions outside of the distance of 9th magnitude stars. From a discussion of the numbers given by the Durchmusterung as far as the 8th magnitude, assorted according to whole units, he obtains the value 0·423 as the ratio of brilliancy between typical stars of two successive magnitudes; and similarly from an assortment by half magnitudes, comprising all stars to the 8th magnitude inclusive, he obtains the value 0·431. Each of these computations gives, for the average distance of a star of the 9th magnitude, about 26 times that of the most distant first magnitude star.

The discordances between the numbers obtained by enumeration from the Durchmusterung and the empirical values deduced from Professor Littrow's formula, vary satisfactorily in sign; and the sums of the positive and of the negative residuals are nearly equal for the groups from $3\frac{1}{2}$ to $8\frac{1}{2}$ inclusive, when the residuals are reduced to fractions of the computed values. Still some of the discordances are large, the difference for stars of the 4th magnitude amounting to more than one-third part of the empirical number, while for those of the 9th magnitude it would exceed one-half of the whole. The appended table exhibits the number of stars for each half-unit of magnitude as obtained from the counting, and from Littrow's formula, according to which the number comprised within the half-magnitude m is given by the expression

$$Z_m = 0{\cdot}6098\ (3{\cdot}5295)^m.$$

To these I add the values corresponding to another formula, which I have deduced for the purpose of giving a somewhat better representation to the stars brighter than the $3\frac{1}{2}$ magnitude. In this the light-ratio for each magnitude is 0·4523 and the number of stars comprised in the half-magnitude m is

$$Z_m = 1{\cdot}0691\ (3{\cdot}2878)^m.$$

The residuals appended to each of the empirical series represent the excess of the computed numbers over the observed ones, expressed in hundredths of the former.

STARS OF THE NORTHERN HEMISPHERE.
According to the Durchmusterung.

Mag.	Counted.	Littrow's formula.	Difference.	New formula.	Difference.
1	6	3		$5\frac{1}{2}$	−11
$1\frac{1}{2}$	4	4		$6\frac{1}{2}$	+38
2	22	8		12	−90
$2\frac{1}{2}$	12	14	+14	21	+43
3	51	27	−89	38	−34
$3\frac{1}{2}$	60	50	−20	69	+13
4	128	95	−35	125	+ 2
$4\frac{1}{2}$	140	178	+21	232	+40
5	379	334	−13	411	+ 8
$5\frac{1}{2}$	463	628	+26	745	+37
6	1242	1179	− 5	1350	− 8
$6\frac{1}{2}$	2231	2215	− 1	2449	+ 9
7	4608	4161	−11	4441	− 3
$7\frac{1}{2}$	6878	7817	+12	8052	+14
8	14525	14686	+ 1	14600	+ 1
$8\frac{1}{2}$	28486	27590	− 2	26474	− 8
9	78185	51830		48003	

The marked defect of the computed numbers for stars of the 9th magnitude has led me to include the several values for this magnitude in the table, although they were intentionally omitted in deducing the formulas. I do not think that any expressions of the form ab^m can be found which will represent all the values to the $8\frac{1}{2}$ magnitude inclusive, with an essentially better accordance. The numerical values of the residuals might be very slightly improved by using the method of least squares, but not to any important extent. The discordance of the formulas given would be still greater for stars of the $9\frac{1}{2}$ magnitude, since Littrow's gives 97,370 stars and my own only 87,040, while in the Durchmusterung there are 177,505 stars of the 9·3, 9·4 and 9·5 magnitudes; those of the 9·6 and 9·7, which are needed in addition to make up the half-magnitude group, not having been observed.

It would thus appear that, unless the fainter stars of the Durchmusterung have been estimated much too bright, the hypotheses in question cannot be extended beyond the $8\frac{1}{2}$ magnitude, even if they are to be regarded as approximately representing the tenth for the stars up to this limit, the distance corresponding to which would be, according to Littrow's formula, about 22 times, and according to the other formula about 18 times, that of a star of the first magnitude.

The Uranometria Nova of Argelander, which gives the carefully observed magnitudes of all the stars which he could distinguish with the naked eye, afforded the standard for the magnitudes of the Durchmusterung; yet the first glance makes manifest large discrepancies between the rapidly made estimates in the latter work and the sharp determinations of the former. The recently published revision and extension of the Uranometria by Professor Heis of Aachen, assigns the magnitude to the nearest third of a unit for every star which he could discern, the lowest being $6\frac{2}{3}$, or one-third of a magnitude fainter than Argelander's inferior limit. The far greater precision of these determinations would give a more trustworthy basis for our inquiries than the Durchmusterung affords, were the numbers large enough to eliminate such irregularities as may justly be treated as accidental; but this seems not to be the case.

The completion of our Argentine Uranometry now augments the number of accurately determined stars, and renders it possible to assign the actual magnitudes for all stars throughout the heavens which are easily visible to the naked eye. I have taken much pains to secure an accordance between the adopted scale of magnitudes and that employed by Argelander in his Uranometry, and regard it as unlikely that the probable error of our individual magnitudes exceeds one-tenth of a unit. So too does Heis seem to have omitted no efforts for securing an accordance of his work with the same standard, and it is improbable that any essential error can exist in these estimates, made as they are by an astronomer of exceptional keenness of sight.

It is thus easy to ascertain the total number of stars in the firmament for each grade of magnitude within Heis's limits. The results are especially trustworthy, since every individual magnitude has been determined by careful and repeated comparisons with established standards and the same scale; and there now arises the interesting question, how far the accurate numbers given by the two Uranometries, for stars as bright as the 6th magnitude throughout the entire heavens, would agree with the rougher estimates of a number of stars eleven times greater, but situated in the northern hemisphere only and including the 8½ magnitude?

I have therefore devoted some labor to the independent determination of a formula which should represent as well as possible the results derived from the Uranometries alone, upon the hypothesis already stated. The best value which I have been able to deduce for the constant ratio of the light of stars of successive magnitudes is 0·4988, the degree of accordance of which with observation upon our fundamental assumptions may be inferred from the appended table, which gives the number of stars for each half unit of magnitude, as deduced from the Uranometries, and from the formula

$$Z_m = 3{\cdot}2384\ (3{\cdot}0184)^m,$$

together with the residuals, determined as in the preceding table. Those stars of Professor Heis's Uranometry which are given for the fractional thirds of a magnitude, are combined to form the numbers for the fractional halves in our table; a crude procedure, but the only practical one under the circumstances.

NUMBER OF STARS IN THE HEAVENS.

Mag.	Uranometries.			Formula.	Difference.
	N.	S.	Total.		
1	8	6	14	15½	+10
1½	7	4	11	17	+35
2	25	20	45	395	−53
2½	35	33	68	51	−33
3	55	41	96	89	− 8
3½	103	87	190	155	−23
4	132	108	240	269	+18
4½	254	154	408	467	+17
5	392	240	632	811	+22
5½	696	563	1259	1409	+16
6	1374	1075	2449	2449	0
6½	----	2022	----	4255	----
7	----	3317	----	7392	----

Various inferences are suggested by this table. The formula corresponding to our hypotheses differs greatly from the former ones, both in the coefficient and the ratio. The degree of accordance is not such as to warrant any great faith in the correctness of our assumptions, yet a certain approximation in the numbers cannot be denied, extending apparently as far as the 8th magnitude, if we assume the numbers of the Durchmusterung, augmented proportionally for application to the entire firmament. For the higher magnitudes the formula gives numbers even more glaringly in defect than those derived from the Durchmusterung. And finally a comparison of the numbers in the northern hemisphere, taken from Heis, and those of the southern hemisphere, derived from the Uranometria Argentina, shows a decided excess of stars in the northern sky, a fact which is itself at variance with our assumption of an approximately equable distribution.

Yet although the accordance of the observed magnitudes with any series of numbers in geometrical progress is far from being close enough to warrant any important inferences regarding the total number of stars of any magnitudes outside of the limits of observation, it affords the only means at our disposal for any of those rough estimates which are needed in cosmological inquiries regarding these numbers. As soon as we have extended our researches to that distance at which the agglomeration of stars in the Milky Way begins to be appeciable, all further inquiries of this character are futile, and it becomes necessary to consider the Galaxy by itself. From a comparison of the geometric series with the numbers observed for stars not beyond the $8\frac{1}{2}$ magnitude, Littrow has inferred that the same formula may be applied to regions considerably beyond this limit, and from the consideration of W. Herschel's star-gauges in the poorest regions of the sky, he fixes upon a magnitude not far from $11\frac{1}{2}$ as indicating the outer limit of this equable distribution, and thus assigns $4\frac{1}{2}$ millions as the probable number of stars within this limit. To me neither premise appears very tenable. If the influence of the Milky Way is not appreciable even for stars of the 9th magnitude, then the number of stars at a less distance than the limit of galactic agglomeration is not even approximately conformable to geometric progression; so that these inferences from the star-gauges must be illusory. The rapid increase in the number of stars after passing the 9th magnitude may be partially accounted for by the difficulty of estimating magnitudes correctly, near the inferior limit of brightness; yet very large allowances for over-estimation of the fainter magnitudes fail to permit any satisfactory representation by a geometric progression. It is impossible to represent even approximately the more than 150,000 stars of the 9th magnitude, together with the numbers for magnitudes below $4\frac{1}{2}$, by any such formula, without over-representing the intermediate numbers by more than one-half. And even then, we fail to obtain an adequate value for the stars beyond 9 and 9½. We may reproduce the numbers which the Uranometries give for the 6th magnitude and the Durchmusterung for the 9th, by means of a series which doubles the number of stars for each successive half-magnitude; yet even this, when extended to stars of the 9½ magnitude, would assign to the entire heavens a less number than the Durchmusterung gives for the northern hemisphere alone. I attribute, however, to this last consideration comparatively little weight, for the reason already indicated.

In the second column of the annexed table, the number of northern stars corresponding to each half-magnitude is reproduced, being taken from Heis's Uranometry for magnitudes up to the 6th inclusive, and from the Durchmusterung for higher ones. The last line, however, is not for the full half-magnitude corresponding to 9½, but merely for9 ·3, 9·4 and 9·5; the remaining two, 9·6 and 9·7, not being contained in Argelander's work. The third column contains the numbers from the Uranometria Argentina, and the fourth the corresponding ones for the entire heavens, being the sum of the two preceding as far as the 7th magnitude, and thereafter the double of the numbers observed in the northern hemisphere. The final column shows, for the sake of comparison, the values which result from the simple hypothesis that the number of stars doubles for each successive half-magnitude applied to the observed number of the 6th magnitude. Simple as it is, this series presents less violent discordances than any of the others which I have deduced, for magnitudes above the 4th. For stars

TOTAL NUMBER OF STARS.

Mag.	N.	S.	Total.	Formula.
1	8	6	14	4
1½	7	4	11	5
2	35	20	45	10
2½	25	33	68	19
3	55	41	96	38
3½	103	87	190	76
4	132	108	240	153
4½	254	154	408	306
5	392	240	632	612
5½	696	563	1259	1224
6	1374	1075	2449	2449
6½	2231	2022	4253	4898
7	4608	3317	7925	9796
7½	6878	----	13756	19592
8	14525	----	29050	39184
8½	28486	----	56972	78368
9	78185	----	156370	156736
	177505	----	355010	161660

brighter than these, the discordances, although relatively enormous, are intrinsically small. The total number of all stars to the 4th magnitude inclusive is 664 according to direct observation, but would be only 305 by the empirical series. And the theory of an approximately equable distribution for stars as bright as the 9th inclusive thus appears less improbable, if we may suppose some 360 additional stars to be situated in our immediate vicinity.

In this connection I desire to mention a fact which early attracted and repeatedly compelled my attention during my residence in South America. It has generally been assumed that the number of visible stars of any given magnitude,—whether brighter or fainter,—diminishes as their distance from the Milky Way increases. In the elevated position and pure atmosphere of Cordoba, this nebulous circle is seen with a vividness far surpassing that to which we are accustomed here, and moreover most of that portion which lies in the southern hemisphere is intrinsically brighter than the northern half; so that its position is far more clearly defined than I have ever seen it elsewhere. And few celestial phenomena are more palpable there than the existence of a stream or belt of bright stars, including *Canopus*, *Sirius* and *Aldebaran*, together with the most brilliant ones in *Carina*, *Puppis*, *Columba*, *Canis Major*, *Orion*, &c., and skirting the Milky Way on its preceding side. When the opposite half of the Galaxy came into view, it was almost equally manifest that the same is true there also, the bright stars likewise fringing it on the preceding side, and forming a stream which. diverging from the Milky Way at the stars *Alpha* and *Beta Centauri*, comprises the constellation *Lupus* and a great part of *Scorpio*, and extends onward through *Ophiuchus* toward *Lyra*. Thus a great circle or zone of bright stars seems to gird the sky, intersecting with the Milky Way at the Southern Cross, and manifest at all seasons, although far more conspicuous upon the Orion side than on the other. Upon my return to the North, I sought immediately for the northern place of intersection; and although the phenomenon is by far less clearly perceptible in this hemisphere, I found no difficulty in recognizing the node in the constellation *Cassiopea*, which is diametrically opposite to *Crux*. Indeed it is easy to fix the right ascension of the northern node at about 0h. 50m., and that of the southern one at 12h. 50m.; the declination being in each case about 60°, so that these nodes are very close to the points at which the Milky Way approaches most nearly to the poles. The inclination of this stream to the Milky Way is about 25°, the Pleiades occupying a position midway between the nodes.

A considerable portion of the bright stars of our firmament is situated within this zone or stream, or in its immediate vicinity.

It has been a source of surprise to me that it had not previously attracted the notice of astronomers, and since writing the foregoing paragraphs I had begun the preparation of some data in statistical form to demonstrate its existence, when I discovered that it had been alluded to by Sir J. Herschel in his Results of Observations at the Cape of Good Hope. His words are as follows (p. 385):

"It is . . . . in the interval between η *Argus* and α *Crucis* that the Galactic circle, or medial line of the Milky Way, may be considered as crossed by that of the zone of large stars which is marked out by the brilliant constellation of *Orion*, the bright stars of *Canis Major* and almost all the more conspicuous stars of *Argo*, the *Cross*, the *Centaur*, *Lupus* and *Scorpio*. A great circle passing through ϵ *Orionis* and α *Crucis* will mark out the axis of the zone in question, whose inclination to the galactic circle is therefore about 20°, and whose appearance would lead us to suspect that our nearest neighbors in the sidereal system (if really such) form part of a subordinate sheet or stratum, deviating to that extent from the general mass which seen projected on the heavens forms the Milky Way."

Yet he does not appear to have recognized the fact that this zone of bright stars may be traced with tolerable distinctness through the entire circuit of the heavens, forming a great circle as well defined as that of the galaxy itself.

These stars, or, more strictly speaking, this excess of stars, in the regions in question, must be deducted from our total before the remainder of the fixed stars can legitimately be subjected to statistical discussion with a view to determining the law of their distribution, and the distance at which the agglomeration which we recognize in the Milky Way begins to make itself manifest. Then we shall perceive that the inferences from the accurate estimates in the Uranometries and the rough ones of the Durchmusterung are by no means discordant, and that the distribution of the fixed stars, up to the 9th magnitude inclusive, is not merely tolerably uniform but approximately such that the number of stars doubles for each successive half-magnitude. The distance corresponding to the 9th magnitude is from thirty-two to forty times that of the faintest first-magnitude star; and the light-ratio between stars differing by a single magnitude becomes 0·3968. This is very close to the ratio 0·4, which photometric researches have seemed to indicate as best expressing the actually existing scale, and which is the value usually accepted. Did we adopt precisely this ratio, we should find 315 as the total number of stars as bright as the 4th magnitude, being only ten more than was given by the value just considered, while the computed numbers for all other magnitudes below the ninth would be brought somewhat nearer to the observed ones, and for the 9th magnitude itself would be 151,260.

The phenomena and numerical relations to which I have rerred in this paper seem of considerable importance in their bearing upon the position of our sun in its cluster, the form of that cluster, and the scale of distances between its constituent stars.

ART. XXVIII.—*The Deportment of Titanium with reagents in Iron Ores containing Phosphoric Acid;* by E. H. BOGARDUS.

(Read before the Natural History Society of Rutger's College, New Brunswick, N. J., April 9, 1874.)

THE injurious action of titanium upon iron when present in the metal in considerable quantity, and its frequent occurrence in nature, render it important that a reliable method for its detection be known, and that its presence or absence be proved in all analyses of ores.

During the summer of 1872, while engaged in the examination of some samples obtained in the mines of New Jersey, I encountered a difficulty, which I shall explain somewhat in detail. Subsequent experiments proved the presence of titanium, and that this substance had caused me my trouble. The deportment of the element was so different from what is taught in books of analyses, and from what I had previously observed, that it seemed to me that the usual directions given for its detection were faulty and liable to mislead the analyst. A more extended investigation served to strengthen this impression, and, in view of the importance of the subject, led to the preparation of the following paper.

Although it was not intended at that time to make complete analyses of the ores, it was thought important to test for titanium. To this end a sample was fused with bisulphate of potash and digested in cold water. The dissolved substance was then filtered and the clear liquid treated with sulphuretted hydrogen to effect the reduction of the iron. This should have caused no precipitate of titanium under ordinary circumstances. Sulphur, of course, separated in proportion to the iron reduced, and was removed by filtration. The liquid was now boiled, and as it remained clear, even after long heating, the absence of titanium might have been regarded as demonstrated. The examination, however, did not stop here: it was decided to test the sulphur remaining on the filter. On burning this precipitate a residue was obtained amounting to nearly six centigrams, although but a gram of ore had been taken for the examination. As sulphur would have been vaporized by the heat, the burning, of course, proved the presence of some other body. As the absence of zinc and the members of the fifth and sixth groups had been proved, the only elements supposed to be precipitable by sulphuretted hydrogen under the existing conditions, the supposition was that a mistake had occurred. The experiment was, therefore, repeated and with identical results. As it now seemed certain that sulphuretted hydrogen had caused a precipitate, the next step made was to determine its character. The substance was white both before and after heating, and resembled closely sulphide of zinc. After burning, it held no sulphur. The absence of color and sulphur would alone have seemed sufficient proof of the absence of the fifth and sixth groups, without the concurring evidence as afforded by the previous regular qualitative tests. But since it could not be denied that a substance had formed and its formation seemed an anomaly, it was determined to examine again and this time for all bases without exception. It would require too much time, and would serve no useful purpose, to enter into a minute description of the course taken in this examination, so I will confine myself to a statement of a few of the properties as developed during the experiments. After burning the precipitate and fusing it in bisulphate of potash, it was insoluble in water. It was sparingly soluble in hydrochloric acid immediately after precipitation with sulphuretted hydrogen, but it could be dissolved after repeated boiling in fresh quantities of acid. Fusion in carbonates of the alkalies rendered it soluble in hydrochloric acid It was insoluble or nearly so in boiling solution of potash; precipitated with difficulty or not at all in slightly acid solutions. It colored microcosmic salt brown, gave a transient green color to the blowpipe flame, fused to a black non-magnetic globule on heating on charcoal, and when dissolved in hydrochloric acid imparted a brown tint to turmeric paper like that produced by boracic acid. The effect on turmeric paper led to the supposition that the substance was partly zirconia. The thought at length suggested itself that as there was no sulphur in the precipitate, its formation might be due to a reducing action. Sulphurous acid being a powerful reducing agent, it was determined to substitute this for sulphuretted hydrogen. A sample of ore was fused as at first in bisulphate of potash; then dissolved, filtered, and the reducing gas passed through the liquid. A red coloration at first appeared, followed by a white precipitate like that obtained in the first instance. A quantity of ore was now digested in hydrochloric acid and filtered to separate the insoluble matter. After nearly neutralizing the free acid with ammonia, acetate of ammonia was added in excess, and then acetic acid in large quantity. On passing sulphuretted hydrogen a white precipitate was again obtained. This experiment was repeated several times with identical results, proving that the substance would form in a solution holding free acetic acid. The precipitate was again subjected to a qualitative examination and this time with success. Previously no search had been made for acids: it was now decided to test for them. Phosphoric acid was soon found and the true character of the substance was easily determined. The precipitate was fused in carbonates of soda and potash and digested in water. The phosphoric acid was all dissolved and was detected with molybdate of ammonia. The nearly white residue was proved to be a mixture of titanium and iron. One-half gram of the ore was mixed with an equal weight of an ore holding a large quantity of titanium. The mixture was fluxed in bisulphate of potash, and finally subjected to the action of sulphuretted hydrogen. A precipitate weighing six centigrams was obtained. The sample first examined could give only two and a half centigrams of precipitate. This experiment was followed by the regular tests for titanium, and its presence was soon demonstrated. Rutile was now powdered and mixed with phosphate of soda and an ore free from titanium. After fusion with bisulphate of potash and digestion in water, sulphuretted hydrogen gave a precipitate as in the previous trials. As it now seemed proved that sulphuretted hydrogen could precipitate titanium, it became interesting to know whether the precipitate formed was definite in its composition. It was intended to pursue this question to a satisfactory solution if possible, but owing to the pressure of other matters but little was done; too little to justify the assertion that the precipitate was a definite compound. Several experiments were subsequently made, on mixed samples of rutile and an ore and phosphate of soda, and a precipitate was obtained in every instance with sulphuretted hydrogen. In one case the phosphate of soda was dissolved in water and added to the fused rutile and ore. Rutile was now fused in bisulphate of potash and digested in water; after filtering, sulphuretted hydrogen gave no precipitate. This trial was made to determine whether iron was necessary to precipitation; the result seemed to demonstrate the necessity of its presence and that the formation of a precipitate is dependent upon the reduction of iron. The deportment of titanium with phosphoric acid led to the thought that possibly it might be employed as an agent in the quantitative separation of the phosphorus in iron ores. No experiments were made with a view to the settlement of this question. In all the co-precipitations of titanium and phosphoric acid, the latter was present in excess. It is possible that titanium in excess would have prevented precipitation with sulphuretted hydrogen. The subject seems worthy of an investigation. Let us now pass in brief review over the ground we have traversed, and endeavor to see what light has been thrown upon the properties of titanium and whether the method of its detection, by fusion with bisulphate of potash, as given in books, is all that could be desired, and whether it could not occasionally mislead the analyst. First, the properties of titanium. It was found that it would not precipitate readily in an acid solution on boiling. Phosphorus seemed to hinder, if not to entirely prevent, precipitation; so this method, which is much employed, is faulty. Indeed, this was known before, for some works of analyses tell us that zirconia at least will prevent the separation of titanium from boiling liquids. 2d. We find that titanium colors turmeric paper a brown, or nearly orange color, a fact before known but not generally mentioned in analytical works. 3d. Sulphuretted hydrogen throws down titanium, although no books seem to teach it. Let us now imagine the case of a student testing ores for titanium. He fuses his sample in bisulphate of potash and digests it in water; he filters, and passes sulphuretted hydrogen through the liquid. Sulphur of course separates and gives the solution a milky appearance; he regards this sulphur free from titanium. As the milkiness would render it impossible to see whether a precipitate formed on boiling, he decides to filter and thus render the liquid clear. On boiling no precipitate forms. The titanium has been thrown away with the sulphur, and the analyst has formed a false idea of the composition of his sample. It is plain, then, in case of filtration, after passing sulphuretted hydrogen, that the sulphur should be examined, provided titanium be not found on boiling the liquid.

ART. XXIX.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXI.—*Experiments on the Decay of Nitrogenous Organic Substances;* by H. P. ARMSBY, Ph.B.

IT has been shown by the investigations of Mulder (Chemistry of Animal and Vegetable Physiology, p. 673), Boussingault (Agronomie, &c., t. i, p. 318), and Dehérain (Annales des Sciences Naturelles, Botanique, t. xviii, p. 147), that the soil and other organic or partly organic substances are capable, under some circumstances, of causing free nitrogen to enter into combination in them, and thus of increasing their content of that element. This increase has generally been attributed to the oxidation of free nitrogen to nitrous or nitric acids.

On the other hand, it has been shown by Reiset (Recherches, &c., sur l'Agronomie, 1865, p. 48), Lawes, Gilbert, and Pugh (Phil. Trans., 1861, ii, p. 501), and Schloesing (Compt. Rend., t. lxxvii, p. 353), that during the decay of nitrogenous organic substances in presence of free oxygen, nitrogen may be evolved in large quantities in the free state. König & Kiesow (Centralblatt f. Agriculturchem, II. Jahrg., 7, 9) have shown that the presence of gypsum prevents this loss of nitrogen.

The following experiments were undertaken in the hope of obtaining some information in regard to the conditions favoring these reactions. The method adopted was to allow organic matter containing a known amount of nitrogen to decay under such conditions that all the ammonia given off could be collected and estimated, and at the end of the experiment to determine again the nitrogen. The experiments were conducted in glass bottles, each of which was closed by a rubber stopper and connected with a three-bulbed U-tube containing standard sulphuric acid to prevent any escape of nitrogen as ammonia, and was also provided with a glass tube for the admission of the gas used in the experiment. Two sets of experiments (marked I. and II.) of four each were made. In I. the atmosphere in the bottles consisted of air purified from ammonia and nitric acid, in II. of nitrogen prepared from air by means of phosphorus and carefully purified. It still contained traces of oxygen.

With the exception of the atmosphere, the conditions of corresponding experiments in the two sets were the same, as far as possible.

The organic matter used consisted of dried and sifted barnyard manure intimately mixed with about one-fourth its weight of dried and pulverized flesh. The mixture contained 2·11 per cent of water. The following were the materials and quantities used:

	Org. matter.	Gypsum.	Potash (KOH)	Water.	Total Nitrogen.
I. 1.	15 grms.	----		6 c.c.	0·486 grms.
I. 2.	15 "	----	0·798 grms.		0·486 "
I. 3.	15 "	15 grms.		6 c.c.	0·486 "
I. 4.	15 "	15 "	0·798 grms.		0·486 "
II. 1.	15 "	----		6 c.c.	0·453 "
II. 2.	15 "	----	0·798 grms.		0·453 "
II. 3.	15 "	15 grms.		6 c.c.	0·453 "
II. 4.	15 "	15 "	0·798 grms.		0·453 "

The potash was from a standard solution, of which 7 c.c. were used in each experiment, equivalent to about 6 c.c. of water. The mixtures were made moist but not coherent. All the inorganic materials used were free from combined nitrogen.

The first set (I.) was begun March 9th, the second (II.) March 31st, the per cent of nitrogen in the organic matter having decreased slightly in the interval. Purified air, amounting in all to about 5,800 c.c. for each bottle, was aspirated daily through I. The temperature during the experiments ranged from 50°—80° F., averaging about 70°. The apparatus stood in diffused daylight but not in direct sunlight. The experiments terminated May 15–21st, when the mixtures were dried with suitable precautions, and analyzed for nitrogen and ammonia. The nitrogen was determined by combustion with soda-lime and titration, the ammonia by boiling with magnesia and titration of the distillate. No ammonia was found in the standard acid in the U-tubes. The following are the results calculated on the whole quantity used:

	Materials.	Gain (+) or loss (−) of nitrogen *		G'in of ammonia expressed as nitrogen.
		Weight.	Per cent.	
I. 1.	Organic matter alone,	−0·054	−11·11	0·0118
I. 2.	Organic matter and potash,	+0·074	+15·22	0·0301
I. 3.	Organic matter and gypsum,	−0·0302	− 6·21	0·1651
I. 4.	Org. matter, potash and gypsum,	−0·063	−13·09	0·0907
II. 1.	Organic matter,	+0·0067	+ 1·48	0·0614
II. 2.	Organic matter and potash,	+0·0876	+19·34	0·0255
II. 3.	Organic matter and gypsum,	−0·0052	− 1·14	0·0784
II. 4.	Org. matter, potash, and gypsum,	−0·0088	− 1·94	0·0479

By an inspection of this table we see:

1st. That with the exception of I. 2 and II. 2, there is a loss of nitrogen in every case (the slight gain in II. 1 being within the errors of experiment.)

2d. That this loss is very much less in the second set of experiments, where only traces of oxygen were present.

This result agrees with those of Lawes, Gilbert, and Pugh (Phil. Trans., 1865, ii, p. 509), and goes to show that the loss of nitrogen is caused by a process of oxidation. The effect of the gypsum seems to be to hinder the action. There seems to be no obvious relation between the circumstances of the experiments and the amount of ammonia formed.

3d. That in the two experiments in which the organic matter was mixed with potash a considerable gain of nitrogen took place, which was greater in the case in which only traces of oxygen were present. These two mixtures were carefully tested for nitric acid, but no trace of it was found. This fact, taken in connection with the greater gain of nitrogen in the absence of free oxygen, appears to show that nitrification is not the only means by which the nitrogen content of organic matter may be increased, and this conclusion is supported by the results of Dehérain already referred to.

* Including that of the ammonia.

This author obtained a considerable fixation of nitrogen by glucose and other organic substances mixed with alkali, in the absence of free oxygen or at least of a sufficient quantity to have formed nitric acid with even a small part of the nitrogen absorbed; and though he makes no mention of any direct test for nitric acid, it seems impossible that the gain of nitrogen observed by him could have been due to nitrification.

It has been proved by Will (Ann. Chem. Pharm., xlv, 106) that ammonia is not formed by the union of free nitrogen with nascent hydrogen; hence the gain of nitrogen in the experiments described above is not due to that reaction with the nascent hydrogen produced in decomposition.

We must then conclude that *decaying organic substances, in the presence of caustic alkali, are able to fix free nitrogen without the gain being manifest as nitric acid or ammonia, and probably without the formation of these bodies.*

In further support of this view may be mentioned the results of Boussingault's investigations on this subject (Agr., &c., t. i, p. 318). He several times obtained a gain of total nitrogen by a sample of soil, greater than that gained both as ammonia and nitric acid, thus showing either a gain of nitrogen in other forms than these, or a reversion of these to inert forms in the soil. The latter explanation can hardly be accepted, however, for the results of the above described experiments, nor for Dehérain's, since the greater gain took place in the almost entire absence of oxygen; a fact which favors the supposition that no nitric acid was formed.

In conclusion, I wish to express my obligations to Prof. S. W. Johnson, at whose suggestion these experiments were undertaken, for the use of materials and apparatus, and for many valuable suggestions in regard to the conduct of the experiments.

ART. XXX.—*On the Molecular Heat of Similar Compounds;* by FRANK WIGGLESWORTH CLARKE, S.B., Professor of Chemistry and Physics in the University of Cincinnati.

IT is commonly stated in the text-books of physics that similar compounds have equal molecular heats. Thus, for the chlorides of the general formula MCl, the product of the specific heat into the atomic weight gives approximately a single value. But the equality, under ordinary circumstances, is only approximate, as a few examples will show.

Taking the chlorides of the alkaline metals, we have the following good determinations of specific heat: LiCl, ·28213;

NaCl, ·21401; KCl, ·17295—all by Regnault; RbCl, ·112, Kopp. From these, multiplying by the atomic weights, we get the following molecular heats:

LiCl,	11·99.
NaCl,	12·52.
KCl,	12·88.
RbCl,	13·54.

Here we have a gradual increase, accompanying an increase in the atomic weights. A similar increase is found in Kopp's, Avogadro's and Neumann's determinations for NaCl and KCl, and is also manifested if we work with the average of all the reliable published determinations for the same series of bodies.

In a similar manner we can compare chlorides, bromides, and iodides. Thus we have KCl, ·17295; KBr, ·11322; KI, ·08191; all by Regnault. The molecular heat, then, is

KCl,	12·88.
KBr,	13·47.
KI,	13·60.

Again there is a slight increase accompanying the rise in atomic weight. The chloride, bromide, and iodide of silver, and the same series for lead, illustrate this still farther, although it is not worth while to cite the figures here. For sodium, starting with the fluoride, we have about the same thing, only the bromide proving, perhaps, an exception. But since only one determination of the specific heat of the bromide has ever been published, it is likely that even this compound may be brought into line.

Two series of oxides are very perfect, as follows: As_2O_3, ·12786; Sb_2O_3, ·09009; Bi_2O_3, ·06053; all by Regnault. SiO_2 (quartz), ·186; TiO_2 (rutile), ·157; SnO_2 (tinstone), ·0894; all by Kopp. These determinations give us the following molecular heats:

As_2O_3,	25·31.	SiO_2	11·16.
Sb_2O_3,	26·31.	TiO_2	12·87.
Bi_2O_3,	28·33.	SnO_2	13·40.

If, instead of single determinations, we take the mean of all the published values for the specific heat of these substances, we get a similar result, perfectly in accordance with the idea of a steady increase. The same increase is illustrated by comparing the sulphates of sodium and potassium, their nitrates, their carbonates, or their pyrophosphates. We may also compare $NaNO_3$ with $NaPO_3$, and show that the latter substance has a molecular heat somewhat higher than the former. In short, by means of very complete tables of specific heat, I have been able to demonstrate the fact of this increase in over twenty series of compounds. Exceptions (even seeming exceptions) are rare, and occur for the most part when there are but single determinations of specific heat for the apparently irregular substances. Still it is possible that there may be series of compounds in which equality exists, or in which there may be even a decrease corresponding to an increase in the atomic weights. One more series will suffice to illustrate the usual rule, and I cite this because it is the one most frequently quoted in the text-books to show the supposed equality. Instead of citing single determinations of specific heat, I will here give the results obtained from an average of all the published values:

$CaCO_3$,	21·09.
$SrCO_3$,	21·34.
$BaCO_3$,	21·49.
$PbCO_3$,	21·91.

The same regularity holds if, instead of taking an average of all, we take simply Regnault's or Kopp's determinations alone.

The fact that the molecular heat increases with the atomic weight holds good for liquids as well as for solids, and is here even more striking. Three series will serve my purpose in this connection. For the specific heats themselves I will refer to my tables of specific heat soon to be published by the Smithsonian Institution, merely stating that I have used Regnault's determinations for all the substances except CCl_4, for which Hirn's value has been employed.

CCl_4,	31·91.
$SiCl_4$,	32·42.
$TiCl_4$,	35·25.
$SnCl_4$,	37·15.

PCl_3,	27·62.	C_2H_5Br,	23·44.
$AsCl_3$,	31·95.	C_2H_5I,	24·58.

It may then be stated as a general rule, to which the present evidence offers only a few exceptions, *that in any definite series of similar solid or liquid compounds the molecular heat increases with the atomic weight, although in a very different ratio.* I have tried to establish a similar relation among the elements, but thus far without success. The results here are extremely discordant.

Now what is the meaning of this regularity? Is Dulong and Petit's law set aside by it? Speaking for solid substances alone, I should answer, certainly not. The divergencies from equality are easily explained. The specific heat of a substance varies with the temperature, generally increasing as the temperature rises. But the rate of increase is very different for different bodies. The specific heat of carbon increases very rapidly, that of platinum with extreme slowness. So, then, in order to demonstrate Dulong and Petit's law, we must compare specific heats taken not at the *same* temperature, but at *corresponding* temperatures. This gradual increase to which I have called attention seems, then, to indicate a regularity in these corresponding temperatures, rather than an irregularity in Dulong and Petit's law. Taking our series of alkaline chlorides as an example, we may fairly suppose that their melting points (which have not been measured) are related to each other as the melting points of the metals contained in them. Of these, lithium has the highest melting point, potassium next, then sodium, and rubidium the lowest. If the melting points are the corresponding temperatures for solids, and we determine specific heats at one temperature, say at 20°, we shall have a value for lithium taken at a great distance from its proper temperature, one for potassium at a less distance, one for sodium still closer, and that for rubidium nearest of all. Then we should find that the substance having the lowest melting point possessed the highest molecular heat, a result very naturally to be expected. It seems probable, therefore, that a careful investigation side by side of specific heats and melting points would lead to the discovery of some direct relation between these two sets of constants. For specific heat much new material is needed. For melting points there are but few valuable determinations extant.

ART. XXXI.—*Relation between the Barometric Gradient and the Velocity of the Wind;* by WM. FERREL.

(Read before the Philosophical Society of Washington in June, and also the American Association for the Advancement of Science in August, 1874.)

1. Let G = the barometric gradient in the direction in which it is the steepest, estimated by the amount of change in the mercurial column in the distance of 100 miles;
   - v = the velocity of the wind per hour;
   - r = the radius of curvature of the isobar or line of equal barometric pressure;
   - i = the inclination of the direction of the wind to the isobar on the side of lowest pressure;
   - n = the earth's hourly angular velocity of rotation in terms of the radius;
   - l = the latitude of the place;
   - P = the barometric pressure of the atmosphere;
   - P′ = the value of P at the earth's surface.

The following equation then expresses very nearly the relation in all cases between the barometric gradient and the velocity of the wind:

$$(1) \qquad G = \frac{(2n \sin l + u)v \sec i}{8300000} \cdot \frac{P}{P'} = \frac{(0{\cdot}524 \sin l + u)v \sec i}{8300000} \cdot \frac{P}{P'},$$

in which

$$(2) \qquad u = \frac{v \cos i}{r}.$$

This relation expresses a general law which is of as much importance in meteorology, so far as the barometric pressures and the velocities of the winds are concerned, as Kepler's laws were in astronomy, and must hold for all latitudes from the equator to the poles both at sea and on land, and for all altitudes from the earth's surface even beyond the region of the cirrus clouds. It may be applied to the mean constant motions and pressures of the atmosphere depending upon the mean difference of temperature between the equator and the poles, unaffected by the seasons, which gives rise to two grand hemispherical cyclones, of which the poles are the centers, and of which the cyclonic motions consist of the approximately eastward motions in the middle latitudes and the trade winds in the torrid zones, and also to the ordinary cyclones of comparatively limited extent, including the violent tornados and waterspouts, all of which are cyclones contained within the larger cyclones and controled by their motions. It is likewise applicable to the resultant of any number of cyclones contained within, or interfering with, one another.

2. From (2) it is seen that u is the angular velocity of gyration around the center of curvature of the isobar, which in a perfect cyclone becomes the gyratory velocity around the center of the cyclone. But in connection with this gyratory motion there is generally a motion either toward or from the center of the cyclone, toward the center below and from the center above in an ordinary cyclone, giving rise to a spiral motion, and hence at a certain height the motion toward or from the center must vanish and the gyrations be circular. The value of i, therefore, has different signs below and above.

The value of i depends mostly upon friction, but in some measure also upon the inertia of the atmosphere in cases where the motions are either increasing or decreasing, as in the beginning or dying away of the cyclone. Its value, therefore, is greatest on land and near the earth's surface, and comparatively small at sea or anywhere in the upper regions of the air. Its value also depends upon the sine of the latitude, and is equal to 90° at the equator, where $\sin l = 0$, since there can be no gyrations there, and the motions must be either toward or from a center of rarefaction or condensation.

In a perfect cyclone the isobars are circular and r becomes the radius of the circle, and in this case the gradients are estimated in the direction of the radius from the center. In the two polar hemispherical cyclones the gradients are estimated in the direction of the meridians, and the isobars, supposing the cyclones to be perfect and unaffected by local disturbing causes, correspond with the parallels of latitude. In this case r in (2) is the distance from the earth's axis, and $v \cos i$ expresses the component of motion relative to the earth's surface in the direction of the parallels of latitude. In order to have the expression of G (1) strictly applicable to the polar cyclones we should have $u \sin l$ instead of u, but the value of u in comparison with $2n \sin l$, in this case, is so small that u may be neglected entirely without sensible error, so that the expression may be regarded as applicable either to the polar cyclones, or to any ordinary cyclone, or to the resultant of any number of these cyclones interfering with one another.

3. In ordinary cyclones or tornados, in which circumstances may be such as to give a very great gyratory motion near the center, the term in (1) depending upon u cannot in general be neglected, and it may even become much greater than the term depending upon n, that is, upon the earth's rotation, and in violent tornados and waterspouts, in which there are rapid gyrations very near the center, the value of the term depending upon u may be so much greater than that depending upon n, that the latter may be entirely neglected in comparison with the former. For instance, if the distance r from the center of the cyclone should be 400 miles and the velocity v equal to 40 miles per hour, if we regard the value of i as being not very large, (2) would give $u = 0{\cdot}1$, which in middle latitudes would be about one fourth of $0{\cdot}524 \sin l$, and its omission would give rise to considerable error. And if the distance from the center were only 100 miles and the velocity of the wind the same, the two terms in the expression of G would be about equal, and for very small distances from the center with a large value of v, it is readily seen that the term containing n and depending upon the earth's rotation may be very small in comparison with the other. In large cyclones, however, and at a considerable distance from the center, the value of G depends mostly upon the effect of the earth's rotation, and only in some measure upon the mere centrifugal force arising from the gyrations relative to the earth's surface around the center, and depending upon the term containing u merely and not n.

4. The preceding general law expressed by (1) is deduced from carrying out more in detail principles which the writer has already had published at different times and places, but a complete demonstration of the law would be too complex and

require too much mathematical analysis to be given here. It may, however, be important to give the following explanation, rather than a complete demonstration, of this law so far as it applies to ordinary cyclones of not very great extent, especially as the method of presenting the matter here is different from any which has heretofore been used.

If water in a basin at rest has a motion of gyration around the center, the mere centrifugal force arising from the gyrations causes the water to recede from the center and produce a gradient in the surface of the water, and consequently in the pressure of the fluid upon the bottom of the vessel. If we let r represent the distance from the center and u the angular velocity of gyration, the centrifugal force is expressed by ru^2 simply. But if in addition to the gyratory motion of the water in the basin, the basin itself also has a gyration like a dish around its center, of which the angular velocity is represented by n', we then have for the whole centrifugal force

$$r(n'+u)^2=r(n'^2+2n'u+u^2),$$

and the gradient referred to the level of the water at rest in the basin, also at rest, depends upon this force. But if we refer the gradient to the level of the water at rest relatively to the basin having the gyratory velocity n', we must neglect the first term in the second member above depending upon n' merely, and the gradient then depends merely upon

$$r(2n'u+u^2)=(2n'+u)v,$$

putting $v=ru$ for the lineal velocity of motion or gyration in this case.

5. In the case of a cyclone upon the earth's surface of such extent only that the curvature of the surface can be neglected, we have exactly a similar case. In addition to the gyratory motion of the cyclone relative to the earth's surface, it is well known that the part of the earth's surface occupied by the cyclone has a motion, one component of which is that of the gyration of a disk around its center. At the poles of the earth the angular velocity of this gyration is that of the earth's rotation around its axis represented by n, but for any place between the poles and the equator it is equal to the velocity of the earth's rotation multiplied into the sine of the latitude, that is, equal to $n \sin l$. In this case we have $n \sin l$ corresponding to n' in the case of the basin of water, and hence we have $(2n \sin l + u)v$ for the centrifugal force upon which the gradient of the cyclone depends, for the term in this case depending upon $n^2 \sin^2 l$, corresponding to n^2 in the case of the basin of water, must be neglected, as in that case, since the atmospheric gradient is referred to the elliptic surface of the earth, and not to the surface in the case of no rotation around its axis. This

corresponds with the centrifugal force in (1), upon which the gradient depends in the case of no friction, in which the gyrations are circular, as supposed in the basin of water, and in which consequently sec $i=1$.

6. If we put

h=the height of any stratum of the atmosphere of equal pressure;

ρ= the density of this stratum;

ρ'= the value of ρ at the earth's surface;

we shall have

$$\frac{D_r\mathrm{P}}{\rho}=gD_rh, \tag{4}$$

and, putting a for 100 miles, we get, according to the definition of G,

$$\mathrm{G}=\frac{aD_rh}{10500}\cdot\frac{\rho}{\rho'}, \tag{5}$$

in which 10500 is the ratio very nearly between the density of the atmosphere and of mercury at the surface of the earth, where the atmospheric pressure is supposed to be 30 inches and the temperature equal to 32° of Fahrenheit. When the mean temperature of the atmosphere is greater, the value of this constant should be increased $\frac{1}{490}$ for each degree of temperature.

With the value of D_rh obtained from (5) we get from (4)

$$\frac{D_r\mathrm{P}}{\rho}=10500\frac{g}{a}\mathrm{G}\,.\,\frac{\rho'}{\rho}.$$

But the first member of this equation is the expression of the horizontal accelerating force arising from a difference of pressure, and this in the case of no motion either toward or from the center of the cyclone, as in the case of the basin of water, must be equal to the part of the centrifugal force causing the gradient, which, when the gradient is referred to the elliptic surface of the earth, we have seen, is $(2n\sin l+u)v$. Hence we get in this case

$$\frac{D_r\mathrm{P}}{\rho}=10500\frac{g}{a}\mathrm{G}\,.\,\frac{\rho'}{\rho}=(2n\sin l+u)v.$$

Where there is motion toward or from the center of the cyclone we must add a term, F, to the last member of this equation, to represent the frictional resistance to the motion, and likewise one to represent the inertia of the air where its motion is either accelerated or retarded. On account of the extreme mobility of the air this last term may be generally neglected without any sensible error, in any of the usual motions of the atmosphere, for it can be shown that only a very small part of the observed barometric gradients are necessary to overcome the inertia in accelerating and retarding the observed velocities. Neglecting, therefore, this effect, and adding F to the last member of the preceding equation, we get

(6) $$G=\frac{g}{a}\cdot\frac{(2n\sin l+u)v\cos i+F}{10500}\cdot\frac{\rho}{\rho'}.$$

Since the motion is now spiral and not circular, and the centrifugal force depends simply upon the component belonging to circular motion, we must use here $v\cos i$ instead of v simply in the preceding expression.

7. By the principle of the preservation of areas in the case of central forces only and no friction, we would have in all parts of the cyclone

$$r^2(n\sin l+u)=\text{constant}.$$

Hence we get by differentiation

$$-rD_tu=2(n\sin l+u)D_tr.$$

The second member of this equation expresses the force which tends to produce a gyratory motion around the center of the cyclone. In the case of no friction this force is all spent in accelerating or retarding the gyratory velocity as the particles of air approach or recede from the center, but where there is friction, it is mostly spent in overcoming the frictional resistance. We shall, therefore, have very nearly

(7) $$F'=2(n\sin l+u)D_tr,$$

putting F′ for the resistance to motion at right angles to the radius, or in the direction of the gyratory motion.

The resistance to the horizontal motion of any stratum of atmosphere which has to be overcome by the existing forces consists in the difference of the actions through friction of the stratum immediately above and below that stratum, so that when the relative velocity of either two contiguous strata is the same there is no resistance to be overcome by the forces. In the motion of the winds the velocity of the lower stratum relative to the earth's surface is greatest, the velocity of the second relative to the first a little less, and so on, until at a moderate height above the earth's surface the relative velocities, and consequently the resistance of friction, are very small. The frictional resistance, therefore, which has to be considered, belonging to each stratum or particle, is principally near the earth's surface, and at a small distance above it becomes comparatively small. Near the earth's surface, where the velocities of the strata increase with the height, this resistance is in the contrary direction of the motion of the atmosphere, and the component of this resistance, contrary to the direction of the gyratory motion, of which the velocity is $ru\cos i$, is represented by F′.

The other component of the resistance, therefore, contrary to the direction of the radius, in which the velocity of motion is $D_t r$, is represented by

$$F = F' \cdot \frac{D_t r}{ru \cos i} = \frac{2(n \sin l + u)(D_t r)^2}{ru \cos i}.$$

This expression is always positive, but it applies only to the part near the earth's surface, where the one component of motion is toward the center of the cyclone.

With this value of F (6) gives, neglecting $\frac{1}{2}u$ in comparison with $n \sin l$ in the value of F′,

$$\text{(8)} \qquad G = \frac{a}{g} \cdot \frac{(2n \sin l + u) v \cos i}{10500} \left(1 + \frac{(D_t r)^2}{(v \cos i)^2}\right) \cdot \frac{\rho}{\rho'},$$
$$= \frac{a}{g} \cdot \frac{(2n \sin l + u) v \sec i}{10500} \cdot \frac{\rho}{\rho'},$$

since by the definition of i we have

$$\text{(9)} \qquad \tan i = -\frac{D_t r}{v \cos i}.$$

Since the unit of time is one hour in the expression of G, we must put $g = 3600^2 \times 32{\cdot}2 \text{ ft.} = 79040 \text{ miles.}$

With this value of g, putting $a = 100$ miles, and with the value of n, the angular rotatory velocity per hour of the earth's rotation in terms of the radius, which is 0·262, we get from (8) by putting $\frac{\rho}{\rho'} = \frac{P}{P'}$, the expression of G in (1). At the earth's surface the factor $\frac{P}{P'}$ becomes unity.

8. If the internal and external parts of the cyclone had the same temperature, the strata of equal pressures would be parallel, or equidistant, and $D_r h$ would be a constant for the same place at all altitudes, and G would be proportional to S. But in order to keep up the motive power of the cyclone there must be a difference of temperature between the internal and external parts, and this causes an increase or decrease in the value of $D_r h$ with the altitude, and consequently by (5) an increase or decrease of G in a greater ratio than that of ρ, other things remaining the same. In an ordinary cyclone, in which the temperature is greatest, and consequently the density least, in the interior of the cyclone, the value of G increases in a less ratio than ρ, and hence in order to satisfy (8) $v \cos i$ must have a less value above than below; but the reverse of this is true where the density is greatest in the interior at the same pressure, as in the polar hemispherical cyclones, and hence the mean constant motions of the upper strata of the atmosphere relative to the lower ones is eastward in all latitudes.

9. The value of i in (8) depends upon friction, and its value can only be determined from observation. If in (7) F′ vanishes, $D_t r$ must vanish, and consequently by (9) i vanishes and the gyrations are circular. The greater the value of F′ for the same velocity or value of v, the greater must be the value of $D_t r$, and consequently of i. If for different velocities friction increases as the velocity, $D_t r$ must increase as the velocity increases, in order to satisfy (7), and hence (9) in this case gives i a constant for all velocities, so long as u can be neglected in comparison with $n \sin l$ in (7), but near the center of the cyclone, where u becomes very large in comparison with $n \sin l$, $D_t r$ must become very small, other things remaining the same, and hence i must also become small. For all ordinary winds it is probable that the value of i is nearly constant for all velocities, and if so, being once determined from observation for a given kind of surface, as that of the sea, or of any kind of land surface, as that of a prairie, this value is probably applicable to cyclones of all degrees of violence upon such surface, except near the center, and may be regarded as a known and fixed constant for that kind of surface.

10. Some very important work has been done by Rev. Clement Ley to determine the value of this constant, the results of which have been given in the Journal of the Scottish Meteorological Society for the first quarter of 1873, p. 66. The following mean values of i, given in connection with the several stations in the following table, were deduced from a considerable number of observations taken indiscriminately by comparing the directions of the wind with that of the isobars, as given by the signal service of the several countries to which the stations belong:

Scarborough,	4° 58′	Thurso,	15° 4′	Nottingham,	27° 44′
Brest,	7 25	Holyhead,	18 4	Oxford,	29 12
Scilly,	10 1	Aberdeen,	21 3	Brussels,	29 57
Yarmouth,	13 49	London,	21 7	Paris,	36 23
Pembroke,	14 47	Greencastle,	22 1	Skudnesnaes,	41 17

From these results Mr. Ley arrived at the following conclusions:

I. The winds commonly incline from the districts of higher toward those of lower pressure. The collective mean for the 15 stations is 20° 51′.

II. This inclination is much greater at inland than at well exposed coast stations. The collective mean for Brest, Scilly, Yarmouth, Pembroke and Holyhead is 12° 49′, while for the internal stations, London, Nottingham, Oxford, Brussels and Paris, it is 28° 53′.

These results exactly confirm the theory of ordinary cyclones, which requires, where there is friction, that there should be a motion of the air below toward the center of the cyclone, as well as a motion of gyration, and hence i must have a positive value. This, it is seen, is obtained from observation for each one of the 15 stations taken separately. Moreover, the inland stations, where the resistances are greater, give a greater value of i than the stations on the sea coast, where the resistances are smaller. This likewise accords with theory.

From the small value of i obtained by Mr. Ley for the coast stations, we may infer that at sea, and likewise in the upper regions of the atmosphere, it is still considerably less. The value of i, therefore, in (8), except at internal stations where the resistances are great, may be regarded so small that its secant can be taken as unity, and hence either the gradient or the velocity of the wind is known the one from the other. For inland stations near the surface, where the resistances are great, the value of i must be obtained from observation.

11. With regard to the inclinations of the winds to the isobars belonging to the different quarters of the compass, Mr. Ley obtained for the inclination of S.E. winds 35° 11′, of N.E. winds 17° 43′, of N.W. winds 9° 4′, of S.W. winds 20° 13′. In these results S. winds were taken as S.W. winds, E. winds as S.E. winds, &c. Hence E.S.E winds have the greatest, and W.N.W. winds the least inclinations to the isobars, which correspond very nearly to the N.E. and S.W. sides of the cyclones. Mr. Ley states that the average direction of the cylones was about N.E. Hence it appears that the inclination of the front part of the cyclone is greatest, and that of the rear the least, and this may perhaps be found to be a general law. It was also found that the differences in the inclinations of the winds from the different quarters is greatest at coast stations.

Mr. Ley likewise obtained the very important result that the difference between the inclination of strong and light winds at stations on the coast is trivial, but at inland stations the mean inclination is less with strong than with light winds, and that at all stations the inclination is more regular with a gale than with a light wind. This is all exactly in accordance with the preceding theory, for we have just seen in a preceding paragraph that where the value of u in (7) becomes large, as it does near the center of a cyclone, the value of i must become less; but the strongest winds are also near the center of the cyclone, and hence, in general, the strongest winds are found to have the least inclinations to the isobars. And as the tendency of the very rapid gyrations near the center is to approximate to a circular gyration, it is evident that the inclinations must be more regular with such winds, which are generally gales, than with light winds; as found to be the case by Mr. Ley from observation.

12. If the water in a basin of water has an interchanging

motion between the internal and external parts, and this basin itself has a horizontal gyratory motion around its center, it is well known that the water toward the center, by the principle of the preservation of areas, tends to run into rapid relative gyrations, but if the basin has no such gyratory motion, the water does not run into such gyrations, but simply keeps the gyrations belonging to the basin. So if, by means of the rarefaction of some area of the atmosphere, so as to cause a difference of density between the internal and external parts, an interchanging motion is kept up between these parts, the atmosphere must run into gyrations, that is, give rise to a cyclone, if the area occupied by this part of the atmosphere has a gyration around its center, as we know it has, unless this center is on the equator. The value of u, therefore, in (7) depends upon the amount of gyratory motion which the part of the surface of the earth occupied by the cyclone has around its center, and vanishes at the equator where this gyratory motion vanishes. The factor $(n \sin l + u)$, therefore, in the second member of (7), depends upon the latitude of the place, and vanishes at the equator. Hence the greater the latitude the less must be the value of $D_r r$, when $v \cos i$ and consequently F', the resistance to this component of the motion, remain the same; and therefore by (9) the less must be the value of i. At the equator also, where $v \cos i$ vanishes on account of the vanishing of the gyrations, $\tan i$ becomes infinite, and consequently $i = 90°$.

13. Mr. Ley's results obtained from the averages of all the stations correspond to about the parallel of 50°. We now see that, according to theory, the less the latitude the greater must be the value of i obtained from observation, and that at the equator the motions would be found at right angles to the isobars. It would, therefore, be interesting to have the value of i obtained from observations for other latitudes. This has been done recently by Professor Loomis (this Journal, July, 1874), from two years' observations of the Signal Service of the United States, as given in one of the tri-daily weather maps for each day. The method adopted is different from that used by Mr. Ley, but the results obtained should be the same by each method. The angle of inclination obtained from these weather maps was over 45°, which is more than one-third of this value greater than that obtained by Mr. Ley for the five inland stations. The average latitude of the stations on the United States weather maps is considerably less than that of the five inland stations used by Mr. Ley, and a part of the preceding difference is, no doubt, due to that cause, as theory requires, but perhaps the greater part is due to the greater amount of frictional resistance to the gyrations in a new and wooded country.

In the case of the trade winds, a part of the polar cyclones, the inclination of the direction of the wind to the isobars at sea is about 45°, and this being at about the latitude of 20°, the value of this angle, by theory, should be very much greater than its value at sea at the parallel of 50°, which from Mr. Ley's small value obtained for the coast stations is perhaps less than 10°, so that this also confirms the preceding theory.

14. Having now learned something from both observation and theory with regard to the value of the theoretically unknown angle i contained in the relation expressed by (1), we shall now proceed to make some comparisons of this law or relation with observation. We shall first consider the mean gradients and velocities belonging to the two polar hemispherical cyclones. Observation shows that the barometric pressure is a maximum, and that, consequently, G vanishes about the parallel of 35° in the northern hemisphere, and a little nearer the equator in the southern hemisphere, and that there are calm belts, called the tropical calm belts, at these latitudes, except so far as they are occasionally disturbed by local cyclones. By the relation expressed by (1), if we put $G=0$, we must also have $v=0$, unless $\cos i$ vanishes, which, we have seen, does not, except at the equator. There must, therefore, be a calm where $G=0$, and hence we have the tropical calm belts. At the equator we also have $G=0$, and with this value (1) is satisfied with $v=0$, that is, with a calm belt, as observed at the equator; but it is likewise satisfied by $(2n \sin l+u)$ vanishing at the equator, and hence v is arbitrary.

Again, observation shows that the mean constant barometric pressure increases in the middle latitudes in going from the poles to the equator, and decreases within the tropics, and hence G is positive in the former case and negative in the latter. The relation of (1), or of (8), requires that in the former case we should have the principal component of velocity, $v \cos i$, positive, and in the latter case negative, that is, that in the middle latitudes there should be an eastward motion, and between the tropics and the equator a motion toward the west. This, it is well known, is in accordance with observation.

15. The barometric pressures given by Professor Loomis (Meteorology, p. 18) indicates that at about the parallel of 64° in the northern hemisphere, and about the parallel of 74° in the southern hemisphere, there is a minimum of barometric pressure, and hence G vanishes in these latitudes, and therefore, for reasons which have already been given, we must have calm belts there. The observations from which these pressures have been deduced perhaps did not sufficiently embrace all longitudes and all seasons to give the mean constant pressures, unaffected by local circumstances and the sea-

sons. This we know is the case in the southern hemisphere, where the most southern observations, obtained mostly by the British Board of Trade, were necessarily made during the summer season, when the barometric pressure is the greatest in those latitudes. If, however, it can be clearly shown by observation that the mean annual pressure is a minimum at these latitudes all around the globe, then the gradient or value of G is negative between these parallels and the poles, and there must consequently be a wind between these latitudes and the poles having the component $v \cos i$ from the east, and as the other component of motion at the surface must in this case be from the pole toward the minimum pressure, the wind must be from some point between the north or south, according to the hemisphere, and the east.

16. The real velocities and directions of the mean constant winds have been determined only very roughly from observation on any part of the globe, and hence no very accurate comparisons of our law with observation can be made. Such comparisons, however, all seem to establish the truth of the law within the limits of the errors of observation. The mean constant isobars in the British Islands, as determined by Mr. Glaisher, all effects of the seasons and of local disturbing causes being eliminated, gives very nearly $G=0{\cdot}02$ of an inch. With this value in (1), supposing the direction of the wind to be toward the east, or nearly so, or that the value of i is small, we get $v=6$ miles nearly. This is a very little less than the mean eastward velocity of the wind here, as determined by the late Prof. Coffin, and given in his "Winds of the Northern Hemisphere." Again, from the table of barometric pressures given by Buchan (Handy Book of Meteorology, p. 27) we deduce the approximate value of $G=-0{\cdot}02$ about the parallel of 20° in the northern hemisphere. The stations in this table belong to widely different longitudes, and the gradients, no doubt, differ considerably on the same parallel in different longitudes, so that this is merely a rough average value for that latitude. On the ocean in this latitude, where the trade winds prevail, without any disturbance from local disturbing causes, the direction of the wind being about N.E., is inclined to the isobars about 45°, and the direction being westward the value of i in this case is about $180°+45°=225°$. With this value of i, and the value of G above, we get from (1) $v=15$ miles nearly, the principal component $v \cos i$ being negative and consequently westward in this case. This velocity cannot differ very much from the usual velocity of the trade winds, so that the result seems to confirm our law with regard to velocities.

From the table of barometric pressures given by Buchan, which has been already referred to, we obtain for the parallel of 52° in the southern hemisphere the value of G=0·07 of an inch, and the tabular results from which this is obtained are the averages of a very large number of observations obtained by the British Board of Trade, so that this gradient may be regarded as being pretty accurate, and it is perhaps very nearly the same on that parallel all around the globe. Supposing the value of i here to be small, that is, that the wind blows very nearly from the west, we get from (1), with this value of G, $v=21$ miles for the velocity of the winds on this parallel. All accounts represent the west winds in this latitude as being very strong all around the globe. Mr. Laughton says:* "Between the parallels of 40° and 60° the westerly wind blows with a constancy little inferior to that of the trade, but with much more violence. About the parallel of 50°, indeed, it is found, as a rule, to be blowing 'half a gale of wind,' and this not only south of the Atlantic, but all round the world." A velocity of 21 miles per hour makes a pretty strong constant wind, and this theoretical velocity probably corresponds very nearly with the average wind in this latitude.

17. We come now to make some applications of the law to ordinary cyclones. Since in the center of a cyclone the barometric pressure must be a minimum and consequently G=0, in order to satisfy (1) in this case we must have $v=0$, and hence there must be a calm at the center. In the large cyclones the gradient or value of G may not become sensible for a considerable distance from the center, and in such cases there is no sensible velocity of the wind within that distance. The area of almost a perfect calm in some cyclones is said to be as much as 30 miles in diameter.

The gyrations of the external part of a cyclone are necessarily in the contrary direction, and hence the component of gyratory motion $v \cos i$ must be negative, and consequently sec v in (1), and the sign of G becomes reversed. At some distance, therefore, from the center of a perfect cyclone between the center and the outer limit, the barometric pressure must be a maximum and G vanish. At this distance, therefore, by our law we have $v=0$, that is, a calm. Hence areas of high barometer must generally be areas of calms.

18. If the isobars of a cyclone drawn to every tenth of an inch of the barometer reduced to sea level are 100 miles apart, we have G=0·1 of an inch. With this value of G, supposing the value of i to be so small that we can put $\sec i=1$, we get from (1) at the distance of 400 miles from the center of the cyclone, or center of curvature of the isobars, and on the parallel of 45°, $v=29$ miles for the velocity of the wind; and this would be very nearly the actual velocity at sea, where the

* Physical Geography in its Relations to the prevailing Winds and Currents, p. 41.

value of i is small. But if the value of i is 45°, which is nearly the value obtained by Professor Loomis from the average of all the stations of the United States Signal Service, then we get for the velocity of the wind, under the same circumstances, $v=21$ miles. With a still much greater inclination than this average inclination, which must frequently happen, this velocity becomes much less.

At a distance of only 100 miles from the center, all the other circumstances being the same as above, we get $v=22$ miles in the case in which i is small, as at sea; and in the case in which the value of i is 45°, we get $v=18$ miles nearly. The comparison of these values of v with those of the preceding paragraph shows that the velocity of the wind belonging to the same gradient diminishes considerably toward the center of the cyclone.

19. The preceding law or relation contained in (1) cannot be tested by comparing the observed velocities of the wind in individual cases with those deduced from the corresponding gradients obtained from the isobars as laid down on the weather maps of the Signal Service; for these being laid down from observations made at stations which are in many cases several hundred miles apart, the effects of the minor more local disturbances cannot be taken fully into account, since within an ordinary cyclone there may exist several smaller cyclones with distinct centers of their own, of so small extent that their effects upon the isobars cannot be determined from stations merely at wide distances apart. The law can only be tested by comparing the average velocity of a great many individual cases with the average of the corresponding observed gradients, as deduced from the isobars, taking account of the distances of the stations from the centers of curvature of the isobars.

20. According to the empirical law of Dr. Buys Ballot, the wind blows at right angles to the line joining the highest and lowest pressures, or, in other words, in the direction of the isobars, and with a force proportional to the steepness of the gradient. This, so far as it represents the true law of nature, is wholly contained within the preceding theoretical and much more general law. Theoretically the direction of the wind can never exactly coincide with that of the isobars, but according to the results obtained by Mr. Ley the inclination to this direction may be small in the higher latitudes at sea, and on level prairie or mostly cultivated countries, where the winds are not obstructed by wood-lands; but according to our theoretical law this angle, even at sea, must become large toward the equator, and accordingly we find that the trade winds at the parallel of about 20° are inclined to the direction of the isobars about 45°. This law of direction then, as a general law applicable to all latitudes, is not even ap-

proximately true near the equator. With regard to the force or velocity of the wind, we see from a mere inspection of the expression of G in (1), that the velocity in all parts of the cyclone for the same latitude is not proportional to G, but is less near the center of a cyclone; and this has likewise been shown (§17) by obtaining numerical results in special assumed cases. It is, moreover, seen that in different latitudes the value of v corresponding to the same gradient, or value of G, must be nearly inversely as the sine of the latitude, especially at a considerable distance from the center of a cyclone, where u in (1) is small in comparison with $2n \sin l$. For this reason the violence of the wind in a cyclone corresponding to the same gradient is much greater within the tropics than in the higher latitudes.

21. If we put

G′ = G divided by 100 miles;

D = the barometric depression in inches at the earth's surface in the middle of a cyclone;

we shall have

$$(10) \qquad D = \int_r G' = \int_r \frac{(0{\cdot}524 \sin l + u) v \sec i}{830000000},$$

in which v must be expressed in miles, and the integration be made from $r=0$ to r equal its value when the barometric pressure is equal to some assumed value, as the mean, below which the depression is estimated.

It is well known that there is a depression of the barometer below the mean in the middle of all cyclones. In the polar regions, near the centers of the two polar hemispherical cyclones, the barometric pressure is considerably below the mean, near the north pole more than a half inch, and near the south pole about an inch, the depression at the latter being greater on account of there being but little land and few mountains in the southern hemisphere to obstruct the gyrations around the pole, upon which the depression depends. In the middle, also, of ordinary cyclones a depression below the mean of two or more inches is sometimes observed. Up to this time no meteorologist has accounted for these depressions. It has been attempted to account for them by means of the centrifugal force arising from the gyrations relative to the earth's surface merely, neglecting the gyrations arising from the earth's rotation; but this in all cases gives an effect very much too small. For upon this principle we neglect the principal term in the expression of D (10) depending upon $0{\cdot}524 \sin l$, and retain only that depending upon u, which, we have seen, is generally very small in comparison with the former. In fact, in the two polar hemispherical cyclones u is so small in comparison with $0{\cdot}524 \sin l$, that it can be entirely omitted without sensible error, so that by retaining merely this very small term we get from (10) no sensible depression of the barometer toward the poles at all. In ordinary cyclones, also, the effect depending upon u merely is very small in comparison with that depending upon the other term, except in violent tornados of small extent, in which the principal part of the gyrations is near the center. By using the complete expression of D we obtain a complete and satisfactory explanation of all these depressions. When a cyclone is of large extent no great velocity and corresponding steepness of gradient is necessary in any part to give a great depression, for the greatness of the depression arises from the extent of the integration. For instance, in the great polar cyclone of the southern hemisphere, we have seen that the greatest eastward velocity, corresponding to the steepest part of the gradient about the parallel of 52°, which is necessary to give the great depression near the south pole, is only twenty-one miles per hour.

22. All barometric oscillations depend almost entirely upon cyclonic action, and are caused generally by the passage of ordinary cyclones over the place of observation. Hence at the equator, where cyclones cannot be formed, there are scarcely any sensible barometric oscillations except the regular small diurnal oscillations. In this case the expression of D in (10) becomes indeterminate, since at the equator $(2n \sin l+u)$ vanishes, but sec i becomes infinite. But since the value of i depends upon friction and upon the neglected effect of inertia, the value of D must likewise, in this case, depend upon these. Observation shows that at and near the equator the barometric oscillations are extremely small. Col. Sykes observes with regard to the small diurnal oscillations on the plateau of the Deccan: "In many thousand observations made by myself there was not a solitary instance in which the barometer was not *higher* at 9–10 A. M. than at sunrise, *lower* at 4–5 P.M. than at 9–10 A. M., *whatever the indication of the thermometer or hygrometer might be:* nor was there a solitary instance in the year 1830 in which the maximum *night tide* was not higher than the 4–5 o'clock day tide."* Humboldt likewise observes with regard to the regularity of these oscillations in the torrid zone: "This regularity is such that, in day time especially, we may infer the hour from the height of the column of mercury without being in error, on an average, more than 15 or 17 minutes. In the torrid zone of the new continent I have found the regularity of this ebb and flow of the aerial ocean undisturbed either by storm, tempest, rain, or earthquake, both on the coasts, and at elevations of nearly 13,000 English feet above the level of the

* Phil. Trans., 1835, p. 167.

sea." These small diurnal oscillations have nothing to do with the question which we are considering, but as the range of these oscillations is only 0·1 of an inch, the preceding extracts show that the irregular oscillations due to other causes, which in the higher latitudes frequently amount to an inch or more, in the torrid zone are so small as scarcely to interfere with the regularity of these small diurnal oscillations, and hence must be themselves very small. At the equator there cannot be any gyration, and hence in the storms and tempests of which Humboldt speaks, the motion of the air at the surface of the earth is directly toward the center of the area of diminished density, and both the inertia of the air and the friction belonging to this motion are overcome by the force arising from almost insensible barometric gradients. The inertia, therefore, of the air merely, which we omitted at the outset in obtaining the relation of (1), is overcome by a very small part of the usual gradients, and its omission, consequently, gives rise to only a very small error in any case. In a cyclone the force arising from the gradients is almost exactly balanced by that arising from the centrifugal force of gyration, including that of the earth's surface, and so great is the mobility of the air, and so small is the amount of friction, that it is only a very small part of this force which is spent in keeping up the motion of the air between the internal and external parts of the cyclone.

23. If friction is as the first power of the velocity, it is evident from (7), other circumstances remaining the same, that the gyratory motion of a cyclone, and consequently the value of v, is as the sine of the latitude, or $\sin l$, and hence the value of D in (10) is as the square of this sine. The mean monthly ranges of the oscillations of the barometer must, therefore, increase with the latitude, and should be somewhat in proportion to $\sin^2 l$. This is shown to be the case from observation. In the following table we have the mean monthly ranges given by observation * corresponding to the given latitudes in the first column, and in the last column we have the ranges which increase as $\sin^2 l$, putting the range at the pole equal to 1·6 inches.

l.	Mean monthly ranges.	1·6 in. $\sin^2 l$.
0°	0·1 in.	0·0 in.
30	0·4	0·40
45	1·0	0·80
65	1·33	1·30
78	1·2	1·36

Of course we cannot expect a very nice agreement in the last two columns, since there are several circumstances besides lati-

* Loomis' Meteorology.

tude which affect these oscillations. The rapidity of the gyrations in a cyclone, and consequently the amount of barometric depression in the center, depends very much upon the amount of aqueous vapor with which it is fed, and this diminishes from the equator to the pole. The great cyclones also, which move from lower to higher latitudes, continually increase in diameter, and hence the amount of depression in the center, and the amount of barometric oscillation at any place caused by the passage of these cyclones over it, must increase toward the poles. These two effects, however, tend in some measure to counteract each other. The monthly range of 0·1 of an inch at the equator is simply that of the diurnal oscillations due to another cause, so that the irregular oscillations sensibly vanish there, as required by theory.

No adequate cause has heretofore been given for these oscillations, nor one which does not equally apply at the equator and in the higher latitudes. We have here given a complete explanation of them, and of their vanishing, or nearly so, at the equator, and of their gradually increasing with the latitude. We now also see the reason why the diurnal oscillations are so regular, not only exactly at the equator, but throughout a belt of considerable extent on each side of it. For the irregular oscillations, which interfere with the regularity of the diurnal, being as the square of the sine of the latitude, must be very small for a considerable distance from the equator. The preceding results also still further verify the law expressed in (1), since the expression of (10), upon which these results are based, is obtained directly from the former.

24. Wherever the atmosphere over any large area of the earth's surface receives a gyratory motion from any cause, this motion gives a value to u and v in (1), and, hence, likewise to G. The term, however, depending upon u, in any such case, is so small in comparison with that depending upon $2n \sin l$, that it can be omitted without sensible error. We can also put $\sec i=1$ in this case, and then we have $v=ru$, and v may be either positive or negative. If v is positive, that is, if the gyration is from right to left in the northern hemisphere, or the contrary in the southern, the value of G is positive, and the pressure increases from the center outward, and there is low barometer at the center; but if the gyrations are the contrary way in either hemisphere, the value of G is negative, and consequently *high barometer* at the center of gyration. The westward motion of the air at the parallels of the trade winds in the Atlantic Ocean, and the eastward motion in the middle latitudes, are in some measure obstructed by the continents and deflected from their course. The westward motion north of the equator is turned northward over and along the coast of

the United States, and the eastward motion in the North Atlantic is deflected south and north by the continent of Europe, while on the corresponding part of the coast of America the air is drawn away faster than it is supplied by the flow over the continent on account of the greater resistances on land than on sea, and the same occurs on the African coast in the torrid zone. Hence the deflected currents supply these deficiencies and give rise to two gyrations in the North Atlantic, the one being a gyration from left to right over a large area having its center about the parallel of 30° and half-way between the continents; the other, in the northern part of the Atlantic, being a gyration from right to left, and having its center about the parallel of 65°. Hence there is an area of high barometer in the middle of the former gyration, and of low barometer in the middle of the latter. The effect of these gyrations is indicated by the isobars drawn on the British Admiralty charts, on which several of these isobars, drawn to tenths of an inch of the barometer, always return into themselves. If we suppose these isobars to be 500 miles apart at any place, on the parallel of 30°, the value of G would be 0·02 of an inch, and with this value of G, putting $l=30°$, we get from (1) $v=8$ miles nearly for the velocity of gyration per hour at that place. In the northern gyration the velocity corresponding to the same gradient would be less on account of the greater value of $\sin l$. From the isobars of the Admiralty chart we see that there are two similar gyrations in the North Pacific, causing an area of high barometer about the latitude of 30°, and of unusually low barometer toward the pole. There are also indications of these gyrations immediately south of the equator in both the Atlantic and Pacific Oceans, and also in the Indian Ocean, as shown by the isobars, but not toward the poles, since the continents in the southern hemisphere do not extend far enough south to produce the necessary deflections there.

25. If we multiply the second member of (1) by 10,500, the ratio between the density of the air at the earth's surface and mercury, by which we get 800 nearly for the denominator instead of 8,300,000, G expresses the gradient of sea-level due to the deflecting forces of the earth's rotation. The southern part of the North Atlantic is supposed to make a gyration in about three years, on account of the more complete deflections of the continents in this case similar to those of the atmosphere. This, at the distance of 1,500 miles from the center of gyration, would give $v=-0·35$ of a mile, v being negative, since the gyrations are from left to right. With this value of v, putting $\sec i=1$, we get from (1) with the new denominator, $G=-0·58$ of a foot for the change of sea level in 100 miles on the parallel of 30°. The value of G being negative shows that the sea-

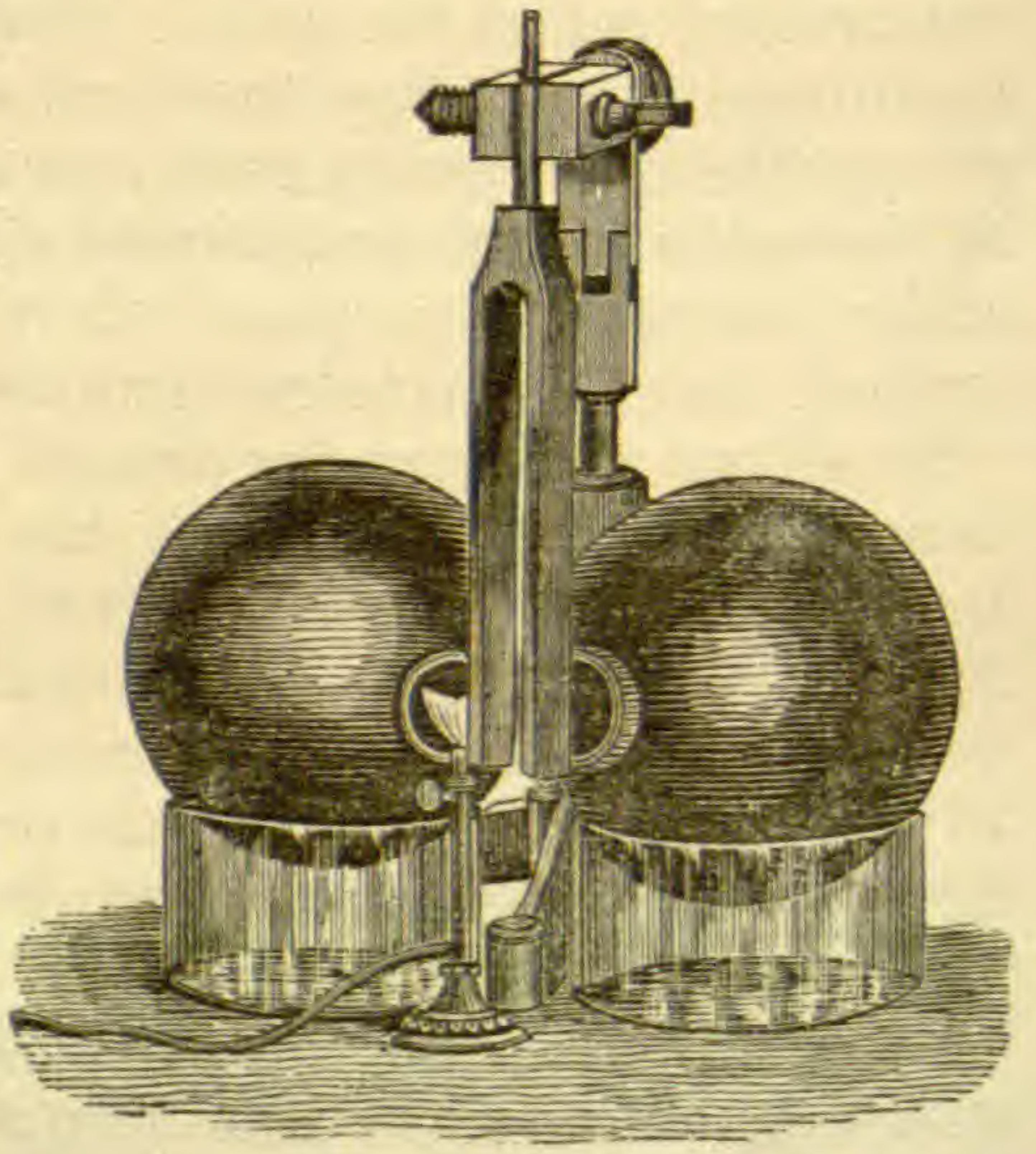
level is highest in the middle of the gyration, and the difference of sea-level between the center and the external part of the gyration would amount to several feet. Similar gradients in the sea-level are produced wherever there are ocean currents, the gradient being proportional to the velocity of the current and the sine of the latitude.

Art. XXXII.—*Researches in Acoustics;* by Alfred M. Mayer. Paper No. 7, containing:

Experiments on the Reflection of Sound from Flames and Heated Gases.

The reading of the recent interesting research of Prof. Tyndall on "Experimental Demonstrations of the Stoppage of Sound by partial Reflections in a non-homogeneous Atmosphere" (Proc. R. S., Jan., 1874; Nature, Jan. 29, Feb. 5), and of the subsequent paper by Mr. Cottrell "On the Division of a Sound-wave by a Layer of Flame or Heated Gas into a reflected and a transmitted Wave" (Proc. R. S., Feb. 12, 1874), caused me to turn my attention to the experimental illustration of the reflection of sonorous vibrations from flames, heated gases, and from sheets of cold gases and vapors.

The following experiments are of easy execution, and show in a marked manner the reflecting powers of sheets of flame and heated gas, and even serve to give approximate measures of these reflecting powers.



Take two similar resonators and place the planes of their mouths at a right angle; then in this angle firmly fix the fork corresponding to the resonators, so that the broad face of one of its prongs faces the mouth of one resonator, while the space between the prongs faces the mouth of the other resonator. (See the figure.)* By trial the two planes of the fork are placed at such distances from the resonators that complete interference of the vibrations

* The wood-cut illustrating this paper has been kindly given to me by Dr. Henry Wurtz, the editor of the American Gas-Light Journal, in which publication an account of these experiments first appeared—May 2, 1874.

issuing from their mouths is obtained, and the only sound that reaches the ear is the faint sound given by the fork's action on the air outside the angle included by the mouths of the resonators. If, in these circumstances, we close the mouth of either resonator with a piece of cardboard, the open resonator will strongly reinforce the sound of the fork. If we now also cover the mouth of the latter resonator with a piece of cardboard we shall again have silence. Also, if we substitute, for one of the pieces of cardboard, a slip of stout glazed note-paper, the same result is obtained. But, if we replace the piece of note-paper by a similar piece of French tracing-paper, a faint sound issues from the resonator so covered, because the tracing-paper is sufficiently permeable to sonorous vibrations to permit the resonator to slightly reinforce the sound of the fork. This reinforcement becomes greater if we substitute for the tracing-paper a piece of tissue paper—such as is used in printed books to cover steel engravings—and a yet greater reinforcement is produced when we put in the place of the tissue-paper a piece of the soft, loosely woven paper which is used by French instrument makers for the inner wrapping of their packed wares. I thus obtained a graded series of substances, more and more permeable to sonorous vibrations.

I again obtained neutralization, by interference, with the mouths of the resonators open, and then screened the mouth of one of them with a bat's-wing coal-gas flame. The vibrations issuing from the resonators were now no longer neutralized, but the vibrations from the uncovered resonator had a great ascendancy over the other, so that a strong sound issued from it. I now tried to destroy this superiority by screening its mouth successively with the graded series of paper screens. The loose, soft paper was not equal to it; nor was the tissue-paper; but the tracing-paper just equalled the effect of the gas-flame in guarding the mouth of the resonator from the entrance of sonorous vibrations. On lowering the gas-flame, so that its top luminous border was just below the mouth of the resonator—and therefore only a sheet of heated air ascended across the latter,—the balance of the tissue-paper against the hot gases and vapor remained unimpaired. Thus it appears that the reflecting power of a sheet of coal-gas flame or of a sheet of the heated carbonic acid and vapor of water just above it, exactly equals, in the above described circumstances, the reflecting power of tracing-paper.

I have also found that the passage of a sheet of cold coal-gas across the mouth of the resonator, was sufficient to destroy the balance of the interference, and caused a faint sound to issue from the other resonator; a similar effect, and nearly equal in intensity, was obtained with a sheet of cold carbonic acid gas;

while cold, dry hydrogen closed the mouth of the resonator more effectively than either of the above gases, but was far inferior in this shielding action to the sheet of heated gases above the bats-wing gas flame. I attempted to get a rough measure of the "acoustic opacity" of hydrogen, but could find no substance sufficiently permeable to balance it, except a sheet of rather fine wire gauze. We should not place too much confidence in measures of the reflecting power of surfaces made by the method just described, and which I have used merely to give approximations of the reflecting powers of the above named gaseous sheets; for the substance which closes the mouth of the resonator may allow a considerable portion of the sonorous vibrations to enter the latter, and yet the resonator may not be able to reinforce the sound by reason of its being thus thrown out of tune with the fork, by an unyielding surface closing its aperture. Thus, a sheet of thick note-paper prevents resonance as effectively as a thick piece of Bristol-board, or a plate of metal; yet we know well that these substances have very different powers to reflect sonorous vibrations. As a flat coal-gas flame equals a piece of tracing-paper in reflecting sonorous vibrations, it follows that we can substitute the former for the latter in all experiments where the presence of the paper produces, by its reflecting power, an alteration in intensity or in pitch. Thus, if we vibrate a fork before the mouth of a resonator while the nipple of the latter is open, we obtain a far inferior reinforcement to what takes place when the nipple is closed Now, the nipple can be partly closed with a gas-flame or a sheet of heated air. Thus, alternately closing and opening the nipple of an Ut_4 resonator with the flame of a Bunsen burner, gives excellent results.* The reflecting power of a bats-wing flame is also well shown by successively closing and opening the mouth of any resonant box of forks in the octave Ut_4 to Ut_5. Also, if the plug be taken out of the ends of closed organ pipes and these pipes be placed horizontally, the reflecting effect of the flame is heard when the latter is passed forward and backward across the open ends of the pipes, while the ear is placed in the axes of the pipes. The simplest method, however, is to sound the fork (either continuously by electro-magnetism, or by a bow) in front of its resonator, and successively to close and open the mouth of the latter with a flame or sheets of heated gas, or of cold vapors or gases. The contemplation of these experiments naturally calls up the question, Is the action of the flame due entirely to reflection? may it not also absorb part of the sonorous vibrations, as in the analogous phenomena of the reflection of light? If the inten-

* In all of the experiments described in this paper care was taken that no heated air or gases entered the resonators, and thereby put them out of tune.

sity of the sonorous vibrations, which have traversed the flame, equal the intensity of the vibrations which impinged on the flame minus the intensity of those which were reflected from the flame, then there is no absorption of these vibrations by the flame; but if this equality does not exist, then there is absorption in the flame; and this means that *the flame is heated by the sonorous vibrations;* which enter the flame as *sonorous vibrations*, but issue from the flame as *heat vibrations.* It thus, at first, appears that the absorption of the sonorous vibrations might be detected by their production of an increase in the temperature of the flame; just as sonorous vibrations are absorbed by caoutchouc and reappear as heat in this substance.

In the following manner I have recently made experiments in the direction of determining the equivalent of a given sonorous aerial vibration in fraction of a Joule's unit of 772 foot-pounds. I stretched between the prongs of an Ut_3 tuning-fork a piece of sheet caoutchouc, $\frac{1}{100}$ inch in thickness, and about $\frac{1}{2}$ inch broad. The effect of this rubber on the vibrating fork is rapidly to extinguish its vibrations, while the rubber itself is heated; and if a fork be vibrated continuously by one and the same force when the rubber is stretched on it, and then when it is taken off, the aerial vibrations produced by the fork are far more intense in the latter circumstances than in the former. By a method described by me in this Journal, Feb., 1871, I measured the relative intensities of the aerial vibrations in these two conditions of vibration. The sheet of caoutchouc was enclosed in a compound thermo-battery and the fork vibrated during a known interval; the rubber was heated by the vibrations which would have appeared as sonorous vibrations if the rubber had been removed from the fork. The amount of heat given to the caoutchouc was accurately determined by the deflections of a Thomson reflecting-galvanometer connected with the thermo-battery; and by knowing the interval during which the fork vibrated, the amount of heat given to the caoutchouc during this interval, and the equivalent of the heated rubber in water, I calculated the intensity of the sonorous vibrations in terms of a thermal unit, from which I at once obtained the value of the sonorous aerial vibrations when the fork was not heating the rubber; in other words, when it vibrated freely. I thus found that the sonorous aerial vibrations, produced during ten seconds, by an Ut_3 fork placed in front of its resonator, equalled about $\frac{1}{100000}$ of a Joule's unit; that is, they can be expressed in the work done in lifting 54 grains one foot high. This quantity of heat, which is equal to the heating of one pound of water $\frac{1}{100000}$ of a degree Fahr., expressed the amount by which the gas flame would be heated if it absorbed all of the sonorous vibrations issuing from the Ut_3

resonator. But this is such a small fraction of the entire heat in the flame, that it is far within the actual fluctuations in its temperature, and even if the flame were constant in temperature, this small increase could not be detected by any known thermometric method. We cannot therefore determine the amount of absorptive power of a flame or of a sheet of heated air for sonorous vibrations, by experiments on their increased temperatures when sonorous vibrations impinge on these bodies.

Hoboken, April 10th, 1874.

ART. XXXIII.—*Velocity of Primitive Undulation;* by PLINY EARLE CHASE, Professor of Physics in Haverford College.*

NEWTON'S hypothesis of a primitive æthereal energy, as the source of cosmical attraction, underlies the fifty theses† which I have submitted as a contribution to the discussion of Anderssohn's "Lösung des Problems über den Sitz und das Wesen der Anziehung," at the coming Breslau meeting of the German Association of Naturalists and Physicians. In those theses I have embodied some of the most important conclusions to be drawn from the physical investigations in which I have been engaged during the past twelve years, and I desire to invite special attention to the curious analogy in the closing thesis, which may perhaps furnish an important clue to the unity of forces that so many are seeking, and of which Secchi treats so largely.

Henry's experiments have shown that there may be ponderable resistance occasioned by imponderable agency; Plateau's, that motion may shape cosmical masses; Anderssohn's, that so-called attractions may be explained by thrusts as well as by pulls; my own, that purely mechanical vibrations may produce polarity. The tendency of modern theorizing to resolve all physical agencies into forms of motion, suggests the inquiry whether the primitive motion, of which gravity and other forces may be special developments, has a constant, or a variable velocity. The hypothesis of constancy seems to me more probable *a priori*, and the subordinate variability, directly as the mass and inversely as the square of radius, can be readily explained in accordance with that hypothesis; inasmuch as the number of radial impulses, intercepted by a given surface, varies as the inverse square of the distance, and the inertia varies as the mass. Galileo's law of rectilinear motion, Newton's laws of motion with reference to centers of force, and the

* Read before the American Association at Hartford, Aug. 13, 1874.
† Proc. Amer. Phil. Soc., xiv, 141–7.

resulting laws of curvilinear motion under combined tangential and focal impulses, are all simple corollaries from the hypothesis of a universally oscillating, perfectly elastic, imponderable, material æther, which would necessarily establish special systems of centripetal and centrifugal undulations about every center of gross inertia. We may, therefore, reasonably seek for the value of the hypothetical constant velocity, and in order that our terms may all be strictly comparable, we will look merely at radial action, either toward the sun's center, or toward the central sun. Let

$2F = M$ = modulus of Newtonian æther = height of a homogeneous æthereal atmosphere, at sun's surface, which would propagate undulations, or motion toward a vacuum, with the primitive constant velocity $= 2 \times$ virtual fall, which would produce the same velocity.

$2r$ = diameter of sun at any given time, either actual, or when homogeneously expanded or contracted to any point in space; $r_{,} = nr_0$ being the present radius, and r_0 the radial unit.

$2r_0 = v_0$ = diameter of sun at which the æthereal and gravitating motions would all be equal, and 2F would $= 2r = v_0 = v_{,} = v_{,,} = v_{,,,} = 2f$; v_0 being the limit of possible æthereal velocity = primitive constant velocity, expressed in the unit of time of traversing $2r_0$.

$v_{,} = \sqrt{2gr} = 2\sqrt{\frac{f}{r}}$ = velocity communicated by infinite fall to sun's surface, (present, past or future; actual, expanded or contracted).

$v_{,,} = \frac{2}{\pi} v_\rho = \frac{4r}{t_\rho}$ = mean velocity of grand-central undulation accordant with v_ρ, (v_ρ being superficial equatorial velocity, and t_ρ being time of solar rotation).

$v_{,,,} = 2f = 2f_0 t^2 = g = \sqrt{2gf} = 2r_0$ = falling velocity communicated, at sun's equatorial surface, by virtual fall through the radius of a circumference equivalent to a primitive wave-length = focal distance of the vertex of a corresponding parabolic wave. Then

$g = \frac{v_0^2}{2F} = \frac{v_{,}^2}{2r} = \frac{v_{,,}^2}{2nr} = \frac{v_{,,,}^2}{2f}$, and we have the following geometrical series:

$M \propto r^2$	$M_{,} = 2r_0 n^2$	$= 473800\, r_{,}$
$— \propto r^{\frac{8}{2}}$	$—— = 2r_0 n^{\frac{3}{2}}$	$= 973{\cdot}3\, r_{,}$
$2r \propto r$	$2r_{,} = 2r_0 n$	$= 2\, r_{,}$
$— \propto r^{\frac{1}{2}}$	$—— = 2r_0 n^{\frac{1}{2}}$	$= r_{,} \div 243{\cdot}3$
$v_0 \propto r^0$	$v_0 = 2r_0 n^0$	$= r_{,} \div 118450$

$v_{,} \propto r^{-\frac{1}{2}}$ $v_{,} = 2r_{0}n^{-\frac{1}{2}} = r_{,} \div 57640000$
$v_{,,} \propto r^{-1}$ $v_{,,} = 2r_{0}n^{-1} = r_{,} \div 28055000000$
$— \propto r^{-\frac{3}{2}}$ $— = 2r_{0}n^{-\frac{3}{2}} = r_{,} \div 13654000000000$
$v_{,,,} \propto r^{-2}$ $v_{,,,} = 2r_{0}n^{-2} = r_{,} \div 6645600000000000$

The values in the right-hand column are determined by the constant ratio, $n^{\frac{1}{2}} = 486{\cdot}7$, which is approximately found by dividing the known value of $v_{,}$ by the approximately known value of $v_{,,}$, as follows:

The approximate values of t_{ρ} vary between the estimate of Spörer (2127554 sec.), and that of Herschel, Bianchi and Laugier (2188080 sec.). The respective corresponding values of $v_{,,}$ are $\frac{r_{,}}{531888}$ and $\frac{r_{,}}{547020}$ in one second of time. Earth's mean distance from sun being $214{\cdot}86r_{,}$, we find $v_{,} = \frac{2\pi r_{,}\sqrt{2}}{1 \text{ yr.} \div 214{\cdot}86^{\frac{3}{2}}} = {\cdot}000887r_{,}$ per second; $g = \frac{4\pi r_{,}^{2}}{(1 \text{ yr.} \div 214{\cdot}86^{\frac{3}{2}})^{2}} = {\cdot}000000393r_{,}$ per second; $n^{\frac{1}{2}} = v_{,} \div v_{,,} = 471{\cdot}7$ according to Spörer, $478{\cdot}3$ according to Carrington, $480{\cdot}3$ according to Faye, $485{\cdot}1$ according to Herschel, or, as will be shown below, $486{\cdot}7$ if the velocity of primitive undulation is identical with the velocity of light. Then $v_{0} = v_{,}n^{\frac{1}{2}} = {\cdot}4183r_{,}$ per second (S.), $\cdot4242r_{,}$ (C.), $\cdot4295r_{,}$ (F.), $\cdot4302r_{,}$ (H.), or $\cdot4316r_{,}$ (v l.).

The value $v_{,,,} = 2r_{0}$ represents the following comparative considerations of rectilinear or curvilinear motion. Let

v_{0} = linear velocity communicated by virtual fall through $\frac{M}{2}$, = circular velocity acquired by virtual fall through $\left(\frac{r_{0}}{2}\right.$ = circum. $\left.\div 4\pi\right)$, in the time of traversing r_{0} by revolution in a circular orbit, = parabolic perihelion velocity, or velocity of infinite fall, at a distance of $2r_{0}$ from the attracting center.

w = a circular wave of same length as the primitive wave, = wave generated by a single centripetal impulse.

$\frac{g}{2} = \frac{w}{4\pi}$ = primary virtual fall corresponding to a single wave-length.

Then $w = 4\pi r_{,} \div n^{3} = r_{,} \div 1057700000000000$, if $n^{\frac{1}{2}} = 486{\cdot}7$. If we adopt other estimated values of $n^{\frac{1}{2}}$, the number of wave-lengths per second may be provisionally estimated as between $\cdot4183r_{,} \div w$ (S.) and $\cdot4302r_{,} \div w$ (H.), or between $367(10)^{12}$ and $446(10)^{12}$. These extreme estimates both correspond, in rapidity, to undulations near the hottest portion of the solar spectrum,* and indicate *a possible complete identification of the sun's attractive and thermo-dynamic energies.*

According to Struve's constant of aberration, v_λ (the velocity of light) $= r_{,} \div \frac{497 \cdot 83}{214 \cdot 86} = \cdot 4316 r_{,}$ per second. The corresponding value of $n^{\frac{1}{2}} = 486 \cdot 7$, and the number of vibrations per second $= 457(10)^{12}$, as before stated. Therefore the constant velocity of primitive æthereal impulsion, v_0, which would account for all the gravitating motions of the solar system, *is almost, if not exactly, identical with the velocity of light*, the several approximate values being, in units of light-velocity,

According to Spörer,	·969
" " Carrington,	·983
" " Faye,	·987
" " Herschel,	·997
Light,	1·000

These values exhibit a discrepancy, varying between $\frac{1}{3}$ of one per cent (H.) and 3·1 per cent (S.). If this close accordance is significant of actual physical influence, there are various uncertainties of observation, in the elements of the calculation, which might account for a still greater discrepancy. Perhaps the most important is the uncertainty as to the proper allowance for the differences in the angular velocity of sun-spots in different latitudes. If there were no resistance to the revolution of the spots, their velocity should be planetary, and therefore far greater than it actually is. On the other hand, if they were rigidly imbedded in a rotating mass, their angular velocity should be uniform. The differences are such as to suggest the possibility that the velocity may be partly planetary and partly rotary. In that case it is not unlikely that even Herschel's estimate of v_p may be $\frac{1}{3}$ of one per cent too great. The well known thermo-dynamic principles which point to a gaseous structure of the sun, as well as Professor Young's observations on the force of solar explosions, seem to indicate so nice an adjustment of centripetal and centrifugal forces within the sun's mass, as to strengthen the hypothesis that its mean mass, its mass-fluctuations, the varying motions of its spots, and the varying orbital eccentricities of its attendant planets and satellites, may all be continually determined by the constant primitive motion.

The three following additional relationships may, perhaps, prove to be something more than merely curious.

* If the identification is exact, the actual number of oscillations per second is $457\,(10)^{12}$; the number in the extreme red ray being $440\ (10)^{12}$ if we estimate sun's mean distance at 92,000,000 miles.

1. The common ratio of the geometrical series, $n^{\frac{1}{2}}$, is very nearly, if not exactly, $\sqrt{\cdot 4 \frac{\text{mass of sun}}{\text{mass of planets}}}$; $r\sqrt{\cdot 4}$ being the radius of spherical gyration.

2. The number of vibrations of the mean caloriferous rays, in the unit of time which satisfies all the equations of the geometrical series, is nearly, if not exactly, the cube of $\frac{2 \times \text{sun's mass}}{\text{Jupiter's mass}}$.

3. If the sun's velocity of equatorial rotation be regarded as the velocity at the vertex of a parabolic wave, and consequently as measured by the focal distance of the vertex,* the motion of sun's center in space will be measured by the parameter of the same parabola. This is in weighty accordance with the stellar and planetary correlations which I communicated to the American Philosophical Society† on the 20th Sept., 1872.

If we were to suppose a slight æthereal expansion by the intense heat at the sun's surface, the inrushing colder æther might produce gyrations analogous to our atmospheric cyclones. But independent of any such hypothesis, it is by no means certain that there may not be some slight progressive æthereal motion, combined with the motion of wave-form, or some radial oscillation accompanying the transverse light-undulations. Any such progressive motion, capable of communicating, at each wave-impulse, a velocity$=\frac{v\lambda}{685\,(10)^{18}}$, would suffice to produce the greatest manifestation of gravitating force in the solar system. In pursuing any investigations for the further elucidation of the subject, due regard should be paid to the double system of centripetal and centrifugal waves originated by inert matter-molecules, and to the æthereal properties which admit of Laplace's supposed instantaneous transmission of gravitating action.‡ It is well also to consider that all the permanent gravitating motions, represented by $v_{,}$, $v_{,,}$, tend to perpetual oscillation, and may therefore be subjected to all the laws of oscillation.

Whatever difficulties we may find in trying fully to account for the foregoing accordances, these accordances are well established *facts* in nature. They are facts in perfect keeping with Newton's original hypothesis, as well as with the hydro-dynamic and mechanical investigations of Challis, Norton, and Leray, with the experiments upon cosmo-plastic forces, and

* $g=2f \therefore \sqrt{2gh} = 2\sqrt{fh}$. † Proc. A. P. S., xii, 518–22.

‡ But not necessarily of gravitating *motion*.

with the various harmonies of cosmical and molecular mass and motion which I have published elsewhere; facts which seem to me to lend new probability, if not moral certainty, to the belief in a discoverable and measurable uniform origin of all physical forces.

Philadelphia, August, 1874.

ART. XXXIV.—*On Serpentine Pseudomorphs, and other kinds, from the Tilly Foster Iron Mine, Putnam Co., New York;* by JAMES D. DANA. With plates VI and VII.

THE "Tilly Foster" iron mine is situated about two miles and a half to the northwest of Brewster, on the Harlem Railroad. The rocks of the region are Archæan, being part of the Highland Range, which reaches from New Jersey, across Eastern New York, nearly to the borders of Connecticut. The ore of the mine—magnetite—is distributed, according to a published report, through a band 132 feet wide; and, like that of Northern New York and other Archæan regions, it constitutes a bed conformable to the stratification.

The special mineralogical interest of the locality was first ascertained by Professor O. D. Allen, of the Sheffield Scientific School of Yale College, and the collections made by him have been the source of many of the facts which are here detailed. Prof. Allen has given me further aid in the research by his chemical examinations. Prof. Brush too has kindly placed the specimens in his cabinet before me for study. I have also visited the region, and thus added to the number and variety of the specimens under examination. The analyses and descriptions of some of the minerals of the mine by Mr. E. S. Breidenbaugh, in a paper published, in 1873, in the sixth volume of this Journal (p. 207), have given me additional assistance.

I. GEOLOGICAL STRUCTURE OF THE REGION.

1. *Archæan rocks.*—The Archæan rocks of the region are mostly different varieties of syenyte and syenytic gneiss, from black to white in color, and, as usual in Archæan formations, they are often in abrupt alternations, so as to make broad bands, riband-like, of black and white, with black blotchings; and the lines of bedding are much contorted. The syenytic gneiss varies to a granular hornblende rock, containing little feldspar; also to a whitish granulyte-like rock, but little schistose, consisting of quartz and orthoclase, with a very sparse sprinkling of hornblende; also to a hornblendic gneiss, in which both hornblende and biotite (or a black mica) are present: also to a

true gneiss, but only sparingly. The feldspar of the rocks is generally whitish, though sometimes flesh-red; and, judging from the absence of striæ on either cleavage surface, it is orthoclase. Whitish mica (muscovite) is of rare occurrence. Minute zircons may often be found by searching in the syenytic rock with a lens.

The rocks are generally very durable; but at the railroad cut in the village of Brewster, both the syenytic gneiss and the included hornblende rock are crumbling to a depth, in some places, of three or four feet, and this disaggregation appears to be in rapid progress.

2. *Ore-bed.*—The magnetite of the ore bed is more or less mixed with chondrodite. In a portion of it, the magnetite greatly predominates, and the ore passes for massive magnetite. But through the larger part the chondrodite constitutes half or more of the mass, while much of the outer portion of the bed is correctly described as chondrodite containing, along with some other minerals, disseminated grains of magnetite. Massive chondrodite is the chief constituent of the refuse from the mine, and may be had there by the ton.

The chondroditic rock and ore often contain disseminated chlorite; less generally, dark green or greenish-black hornblende and grayish or brownish-gray enstatite; occasionally, disseminated white dolomite and brownish-black biotite; while orthoclase is not found, except in the enclosing syenyte. Molybdenite is occasionally met with, and rarely apatite. A little pyrrhotite and chalcopyrite occur with some of the ore, and still less frequently pyrite.

A small part of the rock is dolomite, with disseminated grains or crystals of chondrodite and occasional grains of magnetite.

3. *Veins in the ore-bed.*—In small veins or nests in the ore-bed, the various minerals occur well crystallized. The chondrodite, chlorite, and magnetite are often in excellent crystals, and with these occasionally apatite; and in some cases cavities were bristled with slender prisms of enstatite. The dolomite is present in simple rhombohedrons, some of them two or three inches across. This mineral usually overlies the other crystals mentioned, but crystals of chondrodite are sometimes isolated in the chlorite and also in crystals of dolomite; and magnetite occurs in these minerals and also in crystals of chondrodite.

Some veins, half an inch to three inches in width, consist mainly of coarsely crystallized chlorite, and others are filled with enstatite in long fibrous masses. Still others consist mainly of dolomite; but this dolomite is a filling, covering beautiful crystallizations of chondrodite, chlorite and magnetite implanted on the walls of the little veins: yet the same dolomite often contains isolated crystals of chondrodite, magnetite or chlorite.

The filling is occasionally dolomite and brucite together; and rarely splendent crystals of chondrodite occur isolated in the latter.

The crystallization of the chondrodite and of other minerals of the bed is now under investigation by Mr. E. S. Dana, and nothing therefore need be added here.

In the wall-rock of the bed there are occasional small veins containing crystals of hornblende and magnetite, and sometimes plates of biotite. The magnetite of these veins is octahedral, with even, polished faces, while that of the ore-bed is dodecahedral, with usually convex and striated faces.

The above species appear to have been crystallized in the fissures they occupied at the time of the crystallization or the metamorphism of the rock. The crystallized chlorite, according to an analysis by Mr. Breidenbaugh (loc. cit.), is the species ripidolite; but it is not certain that all the granular chlorite of the ore-bed is of the same species. The color of the ripidolite is generally deep green, rarely reddish.

4. *Minerals of later origin, resulting from alterations of the older minerals, or in other ways.*—Besides these, there is another series of minerals that are manifestly of later origin.

The ore-bed is jointed in various directions. The part containing little chondrodite is mostly solid, with few fractures; but the larger part which contains much chondrodite, along with that which consists mainly of this brittle mineral, is broken throughout to fragments, and so extremely so that the pieces are often smaller than the hand. A great mass of the purer iron ore is in some places in the midst of the more chondroditic; and then the former looks like rock enveloped in a fragmentary deposit arranged more or less concentrically about it. The joints, like those of the Archæan rocks of that vicinity, show that the region has been subjected to disturbing forces; but the extraordinary amount of fracturing is a consequence of the exceeding brittleness of the chondrodite.

The fracturing opened the rocks to movements of water, or moisture in some form, and was the occasion also of such movements and of much chemical action therefrom.

The fragments, large and small, down to those an inch or less in size, are generally coated with a white or greenish *serpentine*, which often looks like a varnish over the surface, and again is an inch or more thick.* They all feel soapy to the fingers on

* Mr. Breidenbaugh found for the composition of the *white serpentine* (this Journ., vi, 209) SiO_2 42·28, Al_2O_3 0·86, FeO 2·57, MgO 40·29, CaO 1·35, K_2O *trace*, Na_2O 0·48, H_2O 12·52=100·35, giving the oxygen ratio for the protoxides silica and water 3·1 : 4·03 : 2, that of serpentine being 3 : 4 : 2.

He obtained for the *biotite*, SiO_2 40·08, Al_2O_3 14·21, Fe_2O_3 11·51, MgO 22·03, Na_2O 0·22, K_2O 9·73, H_2O 1·69, Fl *trace* = 99·47; for the constituents of the *ripidolite*, SiO_2 32·33, Al_2O_3 14·56, FeO 5·29, MgO 33·74, CaO 1·04, K_2O 0·87, account of it. The great piles of refuse rock that are heaped up near the railroad leading from the mine are, in the main, piles of chondroditic masses thus coated or varnished with serpentine. Over the most of them the serpentine is white, a kind that looks much like meerschaum.

The serpentine also penetrates the masses, and from many of them a fragment of chondrodite as large as a filbert cannot be obtained that has not films of serpentine in or about it. It also fills the cavities in the old veins that were partly filled with crystallizations of chondrodite, chlorite, magnetite and dolomite, so that the crystals of these minerals are buried under it; or it penetrates the veins where there were no distinct cavities.

Besides serpentine, there is sometimes also a coating of *brucite* (hydrate of magnesia), and occasionally this mineral is in large crystallizations. *Fluorite* is another of the secondary incrusting minerals, although not common; it is sometimes in pink massive forms, and occasionally in small amethystine cubes.

In addition, the ore bed abounds in *pseudomorphous minerals*. Crystallizations of chlorite, enstatite, chondrodite, dolomite, apatite and other kinds occur converted into serpentine of various colors. The universal serpentinization of masses and crystals conveys the impression that the rock along all the multitudinous fissures, and, to a large extent, through the interior of solid portions, had been subjected to long digestion in heated magnesian waters.

There are also other kinds of pseudomorphs, indicating great corroding and recomposing power in the waters, as described beyond.

Further, there are species of still later origin. Implanted in the serpentine sometimes occur polished cubes and cubo-pyritohedrons of pyrite; and in seams in the serpentine, the mineral pyrrhotite, another sulphide of iron. There are also, on some of the surfaces of blocks, occasional small groups of crystals of aragonite, or thin crusts of hydromagnesite.

II. The Pseudomorphs and their teachings.

The pseudomorphs which have been thus far observed are of five groups: *first* (A), those consisting of *serpentine*, or of *serpentine and dolomite* combined: *second* (B), those consisting of *bru-*

Na_2O 0·54, H_2O 12·02 = 100·39. Whether all of the chlorite is of the species ripidolite is not ascertained.

The *enstatite* afforded him SiO_2 54·17, Al_2O_3 3·30, FeO 9·94, MnO 0·24, MgO 31·99, CaO 0·99, K_2O 0·16, Na_2O 0·32, ignition 0·13=101·24. He analyzed a massive, faintly fibrous kind. It occurs also long fibrous, and radiated fibrous, the fibers easily separating; G.=3·29.

A brown *chondrodite* gave him SiO_2 35·42, FeO 5·72, MgO 54·22, Fl 9·00 = 104·36; equivalent of oxygen replaced by fluorine, 3·79.

The *dolomite* gave Mr. C. A. Burt (loc. cit., p. 213) CO_2 47·01, FeO 0·70, MnO 0·39, MgO 20·79, CaO 30·14=99·03, making the ratio of carbonate of lime to carbonate of magnesia nearly 1: 1.

cite; *third* (C), those consisting of *magnetite*; *fourth* (D), those consisting of *pyrrhotite*; *fifth* (E), those consisting of *dolomite.*

A. *Consisting of Serpentine.*—Of these there are eleven kinds, 1. Cubic, *after an unknown mineral.* 2. Hexagonal prisms, probably *after calcite.* 3. Hexagonal prisms, probably *after apatite.* 4. Plates, clusters of divergent folia, and masses, *after chlorite* (part, or all, ripidolite). 5. Masses and crystals, *after chondrodite.* 6. Prismatic and massive forms, *after enstatite.* 7. Crystalline massive forms, *after hornblende.* 8. Foliaceous to massive forms, *after biotite.* 9. Rhombohedral, *after dolomite.* 10. Massive, *after brucite.* 11. Rectangular tables or plates, *after an unknown mineral.*

B. *Consisting of Brucite* (hydrate of magnesia).—12. Foliated forms, *after dolomite.*

C. *Consisting of Magnetite.*—13. Rhombohedrons, *after dolomite.* 14. After *chondrodite* and other minerals.

D. *Consisting of Pyrrhotite.*—15. Plates *after serpentine* (that of No. 11).

E. *Consisting of Dolomite.*—16. Pseudomorphs, *after crystals of chondrodite.*

A. Serpentine, or Serpentine-and-Dolomite, Pseudomorphs.

1. *Cubic Pseudomorphs.*

The cubic pseudomorphs—the rarest of those at the mine—often form part of the same specimen with the hexagonal of the second kind above enumerated; and they are shown together in figure 1, plate VI; and fig. 2 is a view of part of the opposite side of the same specimen.

The present form of the specimen is owing to the removal of a portion of the original mass. The bottom layer has usually the cubes with the cubic axis at right angles to the surface beneath; but, above this, the mass conforms mostly to one position, some one of the original crystals having dominated over the rest. This latter fact is well shown in fig. 2.

1. *Composition.*—The pseudomorphs consist either of serpentine, or of serpentine and dolomite combined; and in the latter kind there are all proportions, from those purely serpentine to those that are largely dolomitic.

The serpentine of these pseudomorphs has usually a pale green color, though in some specimens olive-green. According to an analysis by Professor Allen, its composition, after expelling 2·09 per cent of hygroscopic moisture, is

Silica	Magnesia	Water	Fe_2O_3, Al_2O_3
41·87	42·43	13·40	2·30 = 100

and, accordingly, it is the common kind.

The dolomite of these compound pseudomorphs is white and translucent, like ordinary crystalline dolomite. Owing to the difficulty of separating it entirely from the serpentine, only a qualitative analysis has been made of it by Professor Allen; and this indicated the presence of carbonic acid, magnesia, and lime, and left no reason to doubt its identity with the ordinary dolomite of the mine and of other parts of the specimens.

2. *Structure.*—The structure is the same for the pseudomorphs consisting of both serpentine and dolomite, as for those of serpentine alone. A description of the former, which is of greater interest, will, therefore, suffice for both.

These compound pseudomorphs usually constitute easily-cleavable masses, two or three inches through. The two minerals are united in one crystallized mass, not by intimate mixture, but by side-by-side juxtapositions of independent rectangular blocks or layers of each, all fitted together like parts of a simple crystal.

The *cleavage is cubic* and exceedingly perfect, without the slightest distinction for the two combined minerals, the rectangular blocks (which are always bounded by the cleavage surfaces) separating easily, even more so than those of a crystallized mass of galenite.

Figures 3, 4, plate VI, illustrate this singular tesselated combination, the serpentine being the green portion and the dolomite the white. To appreciate the remarkable delicacy of the tesselation it must be noted that the specimen here figured (taken from the large pseudomorphous mass of fig. 2) is but a third of an inch broad, the view being enlarged five times lineally. In order that the blocking of the serpentine in the dolomite may be better apprehended, the top of the specimen represented in fig. 3 is given separately in fig. 4. Some of the rectangular serpentine spots in these figures are wholly isolated in the dolomite areas; but this isolation is superficial only, for there is internal connection with the other serpentine portions.

Figure 5 affords another illustration of this tesselated structure. It represents a side of a thin plate (a fragment from a large mass) about two-thirds of an inch square and an eighth of an inch thick. The upper, and under, and middle portions are serpentine, and between lie enclosed blocks or layers of dolomite. The lines over the surfaces in the figures represent lines of cleavage, but only those that were externally very distinct.

3. *Origin of the Pseudomorphs.*—This combination of two so distinct minerals, one a hydrous silicate and the other a carbonate, in a common crystalline mass, having one system of cleavage, is proof that, for one or both, the form is pseudomorphous. That both are so, is made manifest by the structure of the minerals.

In the first place, the *serpentine* shows itself to be pseudomorphous by the fact that the cleavage is not *true* cleavage, but merely a jointed structure; for the smaller blocks afforded by it have no cleavage within, but instead, a wax-like, massive structure; and the uncleavable plates are sometimes quite thick. In true crystals every smallest grain has the cleavage structure as truly as the entire crystal, cleavage belonging to all possible planes in its direction; and, therefore, absence of the cleavage in any part indicates absence of crystalline structure. In some parts of the specimens the cubic cleavage-lines are distinct, *without any cleavage whatever*, the blocks that seemed to be marked out being all solidly united into an uncleavable mass.

It is evident, from the above, that this is not serpentine crystallizing in cubic forms, and, therefore, a new mineral species. It is simply *common serpentine*, which has somehow become possessed of a form foreign to it.

Secondly, the cubic cleavages of the *dolomite* are only joints or divisional planes in rectangular directions. The demonstration of this is found not only in the failure of the cubic cleavage in the interior of the blocks or slices of it, but also *in the existence in the same of the rhombohedral cleavage of ordinary dolomite in all its perfection.* The oblique lines *d*, *d*, *d*, on figure 4 are due to the existence of this cleavage; and in the blocks the cleavage may be obtained indefinitely, precisely as in common crystallized dolomite. Again, portions of one of the masses have sometimes no cleavage but the rhombohedral.

The dolomite is, therefore, *ordinary rhombohedral dolomite.*

Here, then, dolomite and serpentine have together received cubic divisional planes in some way independent of their own powers of crystallization. They have derived it, moreover, from the alteration of one and the same crystallized mineral.

What the original mineral was is not taught us by any thing now occurring at the iron mine. It must have had, not only easy rectangular cleavages, but an open cleavage structure, that is, cleavage-joints, such as exist in galenite; for these cleavage-joints are the divisional planes or joints that are retained in the pseudomorph.

As crystallized galenite (sulphide of lead) has cubic cleavages and also cleavage-joints, it is a question whether this was not the original mineral in the case of this pseudomorph. But the change of a sulphide of lead to dolomite or serpentine, or its removal and the concurrent substitution of these minerals, is hardly supposable.

Anhydrite (anhydrous sulphate of lime), although orthorhombic, has easy rectangular cleavages in three directions and its masses have generally rectangular fractures throughout, following planes of cleavage; so that it possibly might sometimes give cubic jointing to its pseudomorphs. Moreover, the mineral is occasionally found in veins of ore, as in the Hartz and at Fahlun in Sweden. But the crystalline masses of this orthorhombic mineral are made up usually of tabular cleavable masses, and never have the perfect cubic regularity of divisional planes found in these pseudomorphs.

Common salt has at times the cubically-jointed structure required for producing such pseudomorphs. But its presence in an iron ore bed is improbable; and, if present, it could hardly undergo a change to dolomite or serpentine and retain its cleavage structure, on account of its easy solubility.

Fluorite (fluor spar) occurs in masses made up of cubes; but strictly and only cubes; and its cleavage is octahedral.

I can make no other suggestions as to the original mineral. This much is certain, that the species was isometric in crystallization, and had easy cubic cleavage; and its crystallized masses had numerous cleavage-joints.

Whatever the mineral, it underwent two kinds of pseudomorphic changes. We note, *first*, that the two changes could not have gone on together; for two so different minerals could not have been simultaneously formed in the same crystalline mass from the same chemical solution. *Secondly*, that the dolomite must have been formed before the serpentine portion; for the serpentine blocks or plates have the faces perfectly smooth and polished, while those of dolomite, while very even, always appear faintly eroded, as seen under a lens.

Was *part* of the *original* crystalline mass first changed to dolomite, and the rest subsequently to serpentine? Or, was the whole mass first changed to dolomite, and afterward a part of it changed to serpentine?

The last supposition seems to be the most reasonable. The change from the original cubical mineral to dolomite must have gone forward through infiltration along the open cleavage-joints, and thus these cubic cleavage planes were imparted to it. The change of part of the dolomite (or of the whole in some cases) to serpentine took place subsequently, by the same general method, and so the dolomite gave the latter its borrowed cubic cleavage-joints.

Further, this change of the dolomite took place by rectangular blocks, one such rectangular plate or block being changed alike throughout, when another adjoining, separated only by planes of the cubic cleavage, remained unaffected. This pseudomorphism by blocks seems at first improbable; yet the specimens prove it to be a fact, and other examples of it are given beyond, showing that it is a common method.

The question comes up: Could the chemical change from dolomite to serpentine have gone forward through an alkaline solution of silica, the magnesia having been derived from the dolomite; or through a solution of a magnesia-silicate, that is, of serpentine itself? The former supposition can not be true. For if a block of dolomite had been changed to serpentine in that way, it would have been changed also in density, and therefore in size; and the difference in size would have been apparent by displacements throughout the pseudomorphous mass. The fact is, all parts are fitted together as exactly as if the whole were of one mineral. It follows then, if the serpentine has displaced dolomite (instead of the original cubic mineral), that the material introduced in solution to effect the change was not silica, but a magnesia-silicate. It follows also that the same magnesia waters had the power of dissolving, and so removing the dolomite; and that the infiltering magnesia-silicate took the place of the dolomite as the removal went on; it thus being a case of substitution and not of alteration.

If the *first* of the above suppositions is true, there was no change of dolomite to serpentine, but only of the original cubic mineral to each dolomite and serpentine—part to one, and then the rest to the other; and both must have been cases of substitution or removal, in order that the blocks should fit together with the exactness characterizing the pseudomorphs.

2. *Hexagonal prisms, probably after Calcite.*

The hexagonal prisms here referred to are terminated in three rough rhombohedral planes (fig. 1, plate VI); and, as nearly as I can ascertain, the angle between the latter planes is that of calcite. The prisms, several of which occur together in groups, are half an inch to two-thirds in diameter, and one of those examined is two inches in length. They consist mainly of serpentine, but contain a little dolomite. These large prisms have a thick coating of serpentine, which externally is smoothly rounded and shining. In figure 1, two of the prisms of a group are shown partly denuded of the coating, while a third has still the coat on. This coat is a tenth to a sixth of an inch through, and is transversely semi-columnar in structure.

Beneath this coating there is usually some incrusting white dolomite, granular-crystalline in structure; and the base of one crystal is mainly dolomite, with some small pieces of magnetite.

The original mineral of these prisms was probably calcite (carbonate of calcium), as the form is a common one of that species. It is possible that dolomite may occur under such a form; but so large and long prisms of dolomite trihedrally terminated have never been observed. Since the dolomite that is now in the pseudomorphs is an aggregation of crystalline grains instead of having the cleavage of a simple crystal, it is not part of the original mineral.

3. *Hexagonal prisms, probably after Apatite.*

These prisms occur imbedded in the cubic serpentine. They are slender, being less than a line in diameter. By measurement, they are regular hexagonal prisms; and as apatite is one of the occurring minerals of the mine, they are probably pseudomorphs after that species.

Relations of the Cubic to the Hexagonal Pseudomorphs, and to the associated Minerals.

(1.) In the specimen, a portion of which is represented in fig. 1, the cubical portion *rests upon* the coating of the rhombohedral portion; and over a large part of the specimen the former is easily separated, and leaves exposed the shining surface of the latter. Only in a small part are the two soldered together. The prismatic pseudomorph in view has been exposed by such a removal.

It is thus demonstrated that the group of large hexagonal prisms preceded the existence of the cubical portion. We hence discover that the following was the order of events.

1. The crystallization of a group of hexagonal prisms, probably of calcite.

2. The change of these crystals to serpentine and partly to dolomite.

3. The incrusting of these prisms, after this change, by serpentine, making a coating over the whole, as shown in figure 1.

4. The deposition, over this coating, of a cubic mineral whose masses had many cubic cleavage-joints.

5. The change of the cubic mineral to dolomite, by infiltration along the open cleavage joints, and hence with a retention of many cubic cleavage surfaces.

6. The change of part of the dolomite, thus cubic in cleavage surfaces, to serpentine, through the infiltration along the cleavage-joints of a solution of magnesia-silicate, the alteration affecting entire rectangular blocks or plates of the dolomite, but leaving throughout adjoining blocks or plates unaffected.

While proving here that there were an earlier and a later era of pseudomorphism, it is not proved that the successive eras may not have been comprised within a single epoch of pseudomorphism. They may have been successive events in a single month or year.

(2.) With regard to the relations of the pseudomorphs to the associated minerals, we note:

In an olive-green specimen of the cubic serpentine there are imbedded tabular crystals of chlorite (ripidolite), and also large dodecahedral crystals of magnetite. Along with these minerals occur the pseudomorphs after apatite.

This association proves either that the crystals of chlorite and magnetite were formed at the time the original cubic mineral was deposited: or that they were made during the change of the cubic mineral to serpentine. The former may have been the fact; and yet there is no trace of alteration in the chlorite crystals to show that they have passed through a time of serpentine deposition.

4. *Pseudomorphs after Chlorite.*

The change of the crystallizations of ripidolite to serpentine, in specimens from this locality, was early observed, in all its stages, by Professor Allen. Mr. Breidenbaugh, in his account of the white serpentine, remarks that there is a gradual shading in the color of the ripidolite from bright green to pure white, and "in its texture from the foliation and transparency of the unchanged mineral to the compactness and opacity of the serpentine." Specimens are common. Some are crystals, white and pearly, retaining the form of the ripidolite; others, aggregations of whitish folia, from fissures half an inch to three inches in width; others, white, grayish or greenish divergent fibrous masses, either large divergent groups, or stellated aggregations; and others are structureless white or greenish serpentine.

Many of the specimens illustrate the progress of the change from chlorite to serpentine. The surface of a half-changed crystal is often marked with green and white, as represented in figures 6 and 7, plate VI; showing that, in the change, the cleavage-joints were a barrier to its progress; portions of the chlorite bounded by cleavage lines remaining still green, while other portions outside and beneath are wholly changed, on the principle illustrated in the cubic pseudomorphs. The green plates in the figure have the angles 60° and 120°, and perfectly even sides.

When the change to serpentine is complete, there is often one or more of the outer folia on one side or another of the mass that still has some of the color of the chlorite and removes all doubt as to the origin of the foliated mass. But from these there are gradations to other varieties, in which the foliaceous or radiated structure is wholly lost.

The massive crystalline-granular chlorite of the ore-bed also occurs changed to serpentine; some of it retaining the granular structure, and other portions destitute of it. The color is often dark olive-green, while that of the crystals and foliated masses is white or pale green. This fact suggests that this massive chlorite may be another species containing more iron. Breidenbaugh found in a massive chlorite of the mine 9·62 per cent of protoxide of iron, while the ripidolite crystal afforded only 5·29 per cent; but his analysis leaves some doubt as to the nature of the former species.

(To be continued.)

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the First Products of the Distillation of Benzol.*—It is well known that in the rectification of benzol, a considerable portion of distillate comes over at temperatures below that at which benzol itself boils. Helbing has examined, in the laboratory of Professor Erlenmeyer, a sample of this first distillate obtained from J. Weiler & Co., Cologne. It was a yellow, disagreeably-smelling liquid of specific gravity 0·899 at 17·5°, becoming turbid on strong cooling. Elementary analysis gave carbon 80·07, hydrogen 6·71, sulphur 13·61 per cent, thus excluding metacetone and all other oxygenated products. By careful fractionating in Linneman's apparatus, for an entire month, no liquids of absolutely constant boiling point could be obtained. The fractions obtained between 35° and 60° gave readily, when treated with alcoholic potash, potassium ethyldisulphocarbonate; thus proving the presence of carbon disulphide. The fractions between 60° and 75° also afforded CS_2. Bromine acted energetically on all the distillates. Finally, the largest portion of the liquid collected into products boiling between 43° and 55°, and above 75°. On cooling the distillate obtained above 79°, it solidified completely; that from 75° to 79° only partially. By repeating the cooling process, 460 grams benzol were obtained from 900 of the crude product; or about 51 per cent. Calculated from the sulphur in the elementary analysis, the amount of carbon disulphide must be about 16·28 per cent. The fractions which had come over between 18° and 40°, were then subjected to fractional distillation and fractional condensation; but in neither case could a product be obtained free from CS_2. Treatment with alcoholic potash being no more successful, the author gave up the attempt to isolate the hydrocarbons as such, and treated them with bromine, in order to convert them into bromides. Owing to the violence of the action this required great care; and the product, after washing with dilute potassium hydrate solution and with distilled water, was a heavy yellowish-red liquid of disagreeable odor. On distilling it in a current of steam a colorless strongly-refracting liquid first came over, which was nearly pure CS_2; then followed a heavy, slightly yellow liquid of an agreeable sweetish odor; and finally a few oily drops collected in the receiver, which solidified on cooling to a yellowish-white fatty mass. On analysis the fluid bromide just mentioned afforded numbers agreeing with the formula $C_5H_{10}Br_2$, or amylene bromide. The solid bromide gave on analysis the formula $C_4H_6Br_4$, or that of crotonylene tetrabromide. The fraction of the crude product boiling between 50° and 70° was likewise converted into bromide, fractionated and analyzed. It gave the formula of hexylene bromide. Helbing concludes therefore that the first runnings of the benzol still contain considerable amylene, less crotonylene and still less hexylene.—*Ann. Chem. Pharm.*, clxxii, 181, July, 1874. G. F. B.

2. *On the Preparation of Active Amyl alcohol.*—In studying the cause of the anomaly presented by amyl chloride of rotating to the left, as distinguished from the iodide and bromide, which rotate to the right, LE BEL found that the amyl chloride obtained by saturating the crude product of the distillation of amyl alcohol with aqueous hydrochloric acid, by hydrochloric acid gas and again rectifying, was optically inactive. Assuming that this result was due to the presence of yet undecomposed alcohol in the chloride, it was treated, one portion with sulphuric acid to unite with and so to retain this alcohol, and another portion with phosphoric chloride to convert this remaining alcohol into chloride. Upon subsequent distillation, the two products obtained were both right-handed, but the latter was much the more active. Hence the last products of the conversion of amyl alcohol into chloride are much the more optically active; as also must be the alcohol from which they are derived. To prove this, a portion of amyl alcohol rotating 1° 58 to the left was treated fractionally with hydrochloric acid gas, until nine-tenths of it had been removed as chloride. The remaining tenth rotated 4° 32′, being 32′ more than the alcohol obtained by Pasteur. The boiling point of this alcohol was 127°, and that of its chloride 98°. The author attributes the very different rotatory powers ascribed to the amyl compound ethers, to the presence of these two alcohols in the original substance used, with one of which certain acids unite more readily than with the other.—*Bull. Soc. Chim.*, II, xxi, 542, June, 1874. G. F. B.

3 *On a new method of preparation of Salicylic Acid.*—Wishing to investigate more thoroughly the character of salylic acid, asserted by him several years ago to be an isomer of benzoic acid and therefore of great importance in chemical theory, KOLBE was led to examine more fully the synthetic modes of producing salicylic acid, from which it is derived. His attention was at first directed to the method described long ago by himself and Lautemann; to act upon phenol with carbon dioxide in presence of sodium. Observing that when sodium was dissolved in hot phenol, a stream of dry carbon dioxide gas being passed through the apparatus, there is formed beside sodium salicylate, also sodium carbonate and sodium phenylate, and that the more of the former, the less of the latter product was formed; and further that the sodium phenylate on more strongly heating it, absorbed more carbonic gas and yielded sodium salicylate, Kolbe was led at once to propose a new and simpler method of preparing salicylic acid; a method too, applicable on the large scale.* In strong commercial soda lye of known strength, so much melted phenol as is necessary to saturate it is dissolved. The whole is then evaporated in a shallow iron vessel, with constant stirring and crushing, until a dry powder is obtained. This powder, which is sodium phenylate, is then heated in an oil or air-bath, best in a metallic retort, to 100° and dry carbonic gas

* The manufacture of salicylic acid on a commercial scale by this process has been undertaken by Dr. von Heyden in Dresden. Kolbe refers chemists desiring to work with this acid to this fact, and asks the exclusive privilege for a year from this fall of working upon salicylic and paraoxybenzoic acids in this connection.

is passed over it. The temperature rises to 180°, and then phenol begins to distil over. The process is finished when the temperature has reached 220° to 250° and no more phenol comes off. The contents of the retort when cool are dissolved in water, acidified with hydrochloric acid, heated to expel phenol, filtered and allowed to cool. Salicylic acid, in nearly the theoretical quantity, crystallizes out. The same result is afforded by calcium and barium phenylates; but the very curious result was observed by Kolbe that potassium phenylate gave paraoxybenzoic acid when thus treated; due apparently to the temperature.—*J. pr. Ch.*, II, x, 89, July, 1874. G. F. B.

4. *Vanilline.*—At a meeting of the Paris Academy of Sciences held Sept. 14, Dr. W. A. Hofmann announced that his two students, MM Tiemann and Haarmann, who had obtained vanilline (the aromatic principle of the vanilla bean) from pine sap, propose to manufacture this substance on a large scale. The sap of a tree of medium height gives vanilline to the value of 100 fr., and the wood is not injured by the extraction of the sap. This will be the second vegetable product manufactured by purely chemical methods.—*Nature*, Sept. 24.

5. *Expansion of Hard Rubber.*—M. Kohlrausch having several times noticed that glass flasks, closed by stoppers of hard rubber, burst, concluded that this substance must be very dilatable. This hypothesis was fully verified by experiment, for the expansion of this body was found to be about three times that of zinc. From his measures, the coefficient of dilatation for 1° between 16°·7 and 25°·3 = ·0000770, and between 25°·3 and 35°·4 = ·0000842. Thus, not only has hard rubber a very great coefficient of dilatation, but the latter increases very rapidly with the temperature.

This remarkable property can be applied to the construction of very delicate thermometers. Thus, with a small instrument, consisting of two strips of rubber and ivory, 20 cms. long, glued together and fastened at one end, we obtain, at the other extremity, a movement of several millimeters for a change of temperature of one degree. The coefficient of hard rubber is equal, at zero, to that of mercury; above, it is greater. We can, then, as a curiosity, construct a mercury thermometer with a reservoir of this substance, whose changes will be the opposite of those of a common thermometer, and which will fall with an increase of temperature.—*Bib. Univ.*, cxcix, 311; *Pogg. Ann.*, cxlix, 577. E. C. P.

6. *Air Pressure required to sound various Wind Instruments.*—Dr. W. H. Stone, in a paper before the Physical Society of London, describes some experiments on the wind pressure in the human lungs during performance on wind instruments. The first experiment aimed simply at measuring, by a water gauge, the extreme pressure which could be supported by the muscles of the lips, both of trained musicians and others. About six feet of water was the ordinary maximum when a small tube was inserted between the lips. When the lips were supported by a capped mouthpiece, as in brass instruments, a much greater pressure could

be sustained, and the lip muscles invariably gave way long before the expiratory power of the thoracic muscles was exhausted.

A second experiment consisted in introducing a small bent tube into the angle of the mouth, connected with a flexible tube passing over the shoulder. It was found that most instruments could be played as well with this addition as without it. It obviously established a communication between the cavity of the performer's mouth, and therefore of his thorax, and the pressure gauge. The following table was compiled from many observations on some of our principal English musicians. The small tube was inserted in his mouth, and he was directed to sound in succession the chief notes of his instrument. As soon as the tone became full and steady, the position of the water gauge was noted. A fair "mezzo-forte" note was employed. Of course, by forcing the wind and overblowing the instrument, a much greater pressure could be obtained, but those given here were sufficient to produce an average orchestral tone:

Oboe,	lower notes,	9	inches,	highest,	17	inches.
Clarinet,	"	15	"	"	8	"
Bassoon,	"	12	"	"	24	"
Horn,	"	5	"	"	27	"
Cornet,	"	10	"	"	34	"
Trumpet,	"	12	"	"	33	"
Euphonium,	"	3	"	"	40	"
Bombardon,	"	3	"	"	36	"

It will be noticed that the clarinet in this, as in some other respects, differs from its kindred instruments, and also that most of the pressures are small, not exceeding, or, indeed, attaining, the pressure of a fit of sneezing or of coughing. They are, therefore, very unlikely to injure the lungs, or to produce the emphysema erroneously attributed to them.—*Phil. Mag.*, xlviii, 114. E. C. P.

7. *Forces caused by Evaporation from a Surface.*—Prof. O. REYNOLDS, in a paper before the Royal Society, gives a new explanation of the observations of Prof. Crookes and others, that, under certain circumstances, hot bodies appear to repel, and cold ones to attract, other bodies. Prof. Reynolds claims that these effects are the results of evaporation and condensation, and that they are valuable evidence of the truth of the kinetic theory of gases, viz., that gas consists of separate molecules, moving at great velocities.

A light stem of glass with pith-balls on its ends was suspended by a silk thread in a glass flask, so that the balls were nearly on a level. Some water was then put in the flask, and boiled until all the air was driven out of the flask, which was then corked and allowed to cool. Approaching the flame of a lamp to one of the pith-balls, it was repelled, while a piece of ice attracted the ball. If more than a very minute amount of air was present in the flask, these effects were masked by the formation of convection currents. When a piece of ice was held near one of the pith-balls, it was attracted, but after a short time the moisture condensed on it, rendering it heavier than the other ball. The wet ball was then more than ordinarily sensitive, while the other, which had become dry, was nearly insensible to heat. On removing the air, and so much of the vapor that the pressure was less than that due to the temperature, the balls became dry, and were no longer sensitive to the lamp, although still affected by the ice. When no vapor was present, the convection currents reigned supreme, even with very small pressures.

These experiments seem to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward. According to the kinetic theory of gases, during evaporation the particles are shot off from the surface, and, since action and reaction are equal, they will produce a corresponding pressure back on the surface, corresponding to the force producing the recoil of a gun. In the same way, during condensation, the particles are stopped as by a target, and produce the effect of a pressure. Each of these impulses will, however, be less than that due the ordinarily impact of a particle, since the particle is, during condensation, simply stopped, while ordinarily it is first stopped and then thrown with equal velocity in the opposite direction. The pressure is, therefore, increased during evaporation, and diminished by condensation. In the case of water at 60° the evaporation of 1 lb. would be sufficient to maintain a force of 65 lbs. for one second. In the case of mercury, this force will be only 6 lbs., but the latent heat of mercury being only one-thirtieth that of water, the same expenditure of heat would maintain nearly three times as great a force as in the case of water. In Prof. Crooke's experiments the use of the Sprengel pump to produce a vacuum seems to account for the presence of a condensable vapor.—*Phil. Mag.*, xlviii, 146. E. C. P.

8. *Index of Refraction of Liquids.*—MM. TERQUEM and TRANNIN propose a new method of determining the index of refraction of liquids, based on total reflection. A small tank with parallel sides of glass is used to contain the liquid. In this are placed two plates of glass cemented along the edges and free to turn through a measurable angle. The whole is placed between the collimator and observing telescope of an optical circle. The image of the slit being distinctly seen, the plates are turned until total reflection takes place, when the slit disappears. Then turning the plates in the opposite direction, a second measurement is obtained, and the difference equals twice the limiting angle of the liquid with regard to air. With common light the image turns red before disappearing, but with monochromatic light the disappearance is almost instantaneous and can be determined within a quarter of a minute. With a Geissler tube containing hydrogen as a source of light, the image undergoes two marked changes of color, due to the total reflection of the two rays $H\gamma$ and $H\beta$. The error then is only about half a minute, and for $H\alpha$ about a quarter of a minute. The measurements of several liquids are given, agreeing very closely

with previous determinations, the difference being readily accounted for by the difficulty of obtaining the liquid perfectly pure. —*Comptes Rendus*, lxviii, p. 1843. E. C. P.

9. *Electrical Phenomena.*—P. DOM. MARIANINI describes two curious phenomena illustrating the difference in the spark from a body electrified positively or negatively. Two metallic forks have two prongs, one terminating in a point, the other in a ball, and placed opposite each other, so that each point is opposite a ball. An electric spark will pass through one or the other, according as it is positive or negative.

The shorter end of the siphon dips in a vessel of water, and the larger end is drawn out to a point, so that, owing to capillarity, no water will flow. The water level may then be raised or lowered by trial until it reaches such a height that the siphon throws out diverging drops when connected with a positive source of electricity, and there is no flow when the source is negative. The opposite result takes place when the end of the siphon is placed opposite the machine, the water being connected with the earth.

M. A. ROITI attempts to determine whether the electric current is an ether current, by the fact that the velocity of light in a liquid traversed by a current ought to be influenced by the direction of the current, if their velocities are comparable. He employs an arrangement like the interferential refractometer of Arago, and finds that there is no sensible action.—*Il nuovo Cimento*, ix, 97, 148; *Journ. des Phys.*, iii, 227. E. C. P.

10. *Note on the view of Mallet as to the Fusion of Metals;* by ADOLF SCHMIDT, of the Missouri Geological Survey. (Communicated.)—This Journal for September mentions a paper read by Mr. R. Mallet, before the Royal Society, on the fusion of metals. In that paper the author is said to have explained the fact that some metals, when solid, float on a melted bath of the same metal, by the assumption of a "repellent force." Before definitively adopting this rather mysterious explanation, I recommend to all who are yet in doubt in regard to this subject to make the following experiment:

Have a solid ball of cast-iron of 1½ to 2 inches diameter cast and filed off pretty smoothly. Have a ladle or vessel of at least ¾ cubic foot capacity filled with molten cast-iron. If then you lay the cold cast-iron ball on the surface of the molten iron, you will find that the ball, in spite of the "repellent force," assumed by Mr. Mallet, will sink to the bottom of the ladle at once. With an iron rod you can feel the ball at the bottom of the ladle and roll it about. But, after twenty or thirty seconds, the ball will slowly rise to the surface of the bath and remain there. It is thus evident that cast-iron at ordinary temperatures is both *heavier* and *denser* than molten iron, but that, as its temperature rises, the solid iron expands, and becomes lighter and finally floats on the molten iron. The latter fact shows simply that solid iron, when at a high temperature, approaching its melting point, is less dense and lighter than molten iron, which fact again implies that molten iron must undergo a rapid expansion in the moment of its solidification. The extent of this expansion is, however, less than that of the subsequent contraction in cooling, so that the cold iron is again denser than the molten iron.

The error of Mr. Mallet and of many preceding observers consists in this : Their observation, that the solid metal floats on the molten metal refers to the former *when heated*, while their determinations of specific gravity of the solid metal are made with the metal *when cold.* But my experiment, as above described, shows that this cold metal, which has the highest specific gravity, does *not* float, and the heated metal which *does* float has undoubtedly a smaller specific gravity. There is certainly nothing either incongruous or wonderful in all this, and nothing that would require or justify the assumption of a "repellent force." None of Mr. Mallet's experiments, as far as they are mentioned in the "Journal," prove anything against the temporary expansion of certain metals in the moment of solidification, and all the observations I made on this point in foundries verify it.

Washington University, St. Louis, Mo., September 21st, 1874.

II. Geology and Natural History.

1. *Notes on the Geology of Costa Rica ;* by W. M. Gabb.—[The following letter was recently received from Mr. Gabb, who has been at work exploring in Costa Rica during the year past. His labors are bringing much that is new to light over a region of which little is geologically known. We trust that they may be continued until the resources, features, and structure of the country are thoroughly investigated. No one is better fitted than Mr. Gabb for the work.—J. D. D.]

San José, Costa Rica, Aug. 10, 1874.

I have just returned from my last trip into the wilderness of Talamanca, and am now winding up the work in the shape of reports, maps, etc. The close of the survey, in fact the last half, found me the only representative of the original corps. Not only all of my first assistants, but in some cases two relays of substitutes, gave out and retired, with health seriously injured. One man, now a year in a healthy climate and in the doctor's hands, is not well yet. Fortunately, I lost no lives, and, so far as I myself am concerned, neither my life nor my spleen is injured, and except being a little lighter in weight I am as good as new, and ready to go into the field again as soon as my report is finished and *my money paid!*

We were about four months away on my last journey, and while our hardships were neither few nor trivial, our scientific results were satisfactory.

We reached the summit of Pico Blanco June 13, and spent three hours on the summit. Without having at hand the tables for going into the calculations for corrections, etc., the barometric results are approximately 10,200 feet. This is 1,500 feet lower than the formerly received opinion. Where this originated I cannot learn. From the flank of Pico Blanco I made a rude leveling across to the summit of the "U-jum," mentioned in my last letter, and got its approximate height—about 9,600 feet. The other one, mentioned on the strength of reports of the Indians, and which I had not seen, is 200 to 300 feet lower.

I find that "U-jum" means in the native language a bold peak, and is a generic term. It is applied by the people equally to the summit of "Kamuk" (Pico Blanco). For distinction, therefore, I propose to retain for the last named its Spanish name; for that at the head of the Coer we might accept the word U-jum, more especially since I have already used it in a specific sense; while for the intermediate peak, at the head of the ridge between the Lari and its tributary, the Dipari, I suggest the name of Mt. Lyon, in honor of my friend Mr. J. H. Lyon, the only white man in Talamanca, and without whose active sympathy and unremitting exertions our expedition could not but have been a failure.

To estimate Pico Blanco, I give you the data,

Old Harbor, sea level,	barometer, 39·042,	att. therm. 80,	det. therm. 81
Summit of Peak,	" 19·814,	" " 63·5,	" " 62

Its geography has been misstated by some writers, who have placed it on a spur of the Cordillera. It is distinctly in the center line of the main chain, the waters falling rapidly from it toward the two oceans. In this connection we have robbed Iraza, the "show mountain" of Costa Rica, of one of its chief glories. Everybody who comes to the country rides up to the crater on mule back and then writes a book about his achievement, not omitting to state that this is the only point in the world where one can see both oceans at once.

From even as low as 600 feet below the summit of Blanco we saw at a glance thirty miles of the Atlantic, and all of forty of the Pacific. This is a little better than a glimpse of near Greytown on one side, and a suspicion of the Gulf of Nicoyo on the other, which the Iraza (or volcano of Cartago) people get.

Geologically, Pico Blanco must henceforward be erased from the list of volcanos. It is the culminating point of a granite intrusion from below Miocene rocks. I say intrusion, after due weighing of probabilities as to Azoic core, which I know will suggest itself to you. I have not space here to enter into details. Only one fact can be dwelt on now. Not a pebble of granite or syenite has yet been found by me in our conglomerates!

While I say it is *not* a volcano, yet there is a large mass of true volcanic rock forming the apex. It is, however, only a dike, laid bare by denudation, and does not extend 300 feet below the summit. Hundreds of similar dikes are found all through the hills, but have to be looked for, surrounding circumstances not having been so favorable for bringing them into view. Besides, there is not the slightest trace of a crater form, either entire or in part. The top is a straight, ragged ridge, with an isolated point at the west end, a trifle higher than the other part.

I do not know what to say of U-jum and Mt. Lyon. Like Blanco, the last few hundred feet of their summits are bare of forest. On the mountain visited this is replaced by a growth of sage, furze, heather, moss, fern, whortleberries of a light reddish brown color, etc. The fires, smoke, etc., reported at times may result from the burning of this vegetation, but they certainly do *look* volcanic at a few miles distance.

One day, when the sky overhead was clear, I heard thunder from a distant storm; at least, so I believe it to have been. But two or three of my Indians exclaimed at once "*U-jum oruna*" (U-jum is angry). There is no active volcano in their country or within their ken, if U-jum is not one, and how could they know of volcanic rumblings except by some such experience? I must hold my opinion in abeyance until good fortune enables me to visit one or both of these interesting peaks.

But my sheet is nearly full, and I have left much unsaid. We have now full notes for a thorough topographical map of the region. I have all of the geology and large collections in zoology, and more than anybody else will probably ever get in ethnology.

2. *Note on the occurrence of Metamorphic Silurian Rocks in North Carolina;* by Prof. FRANK H. BRADLEY. (From a letter to J. D. Dana, dated Knoxville, Tenn., September 1-th, 1874.)—In my recent trip into North Carolina, from which I have just returned, I determined the metamorphic rocks of the southwestern corner of that State and of the adjoining part of Georgia to be unquestionably of *Silurian* age. I did not go east of Franklin; but, so far, they are all *Lower* Silurian. The crystalline marbles of Murphy and vicinity—white, black and flesh-colored—which are partly siliceous and inclose a rich bed (?) of gold-bearing quartz, are the precise equivalent of our Knox limestones of Quebec Group age, though much thinner than the equivalent beds in East Tennessee. Like their unaltered equivalents in the Great Valley, they are accompanied by heavy beds of brown hematite of the best quality. They are accompanied by heavy beds of agalmatolite and itacolumite, and as these rocks are said to also accompany the limestones of Gainesville, Ga., on the south side of the Blue Ridge, I suspect that these latter will prove to be of the same age.

3. *Abstract of a paper on the Trap Rocks of the Connecticut Valley;* by E. S. DANA. (Read before the American Association at the Hartford Meeting, Aug. 1874.)—This paper was a report of some preliminary results obtained in a series of investigations now being carried on by E. S. Dana and G. W. Hawes.

The trap belongs to an extensive series of fissure eruptions connected with the Mesozoic sandstone, not only of the Connecticut Valley but also of Nova Scotia, New Jersey, Pennsylvania, and North Carolina. These dikes of trap, however, are nowhere displayed to such extent and in such numbers as in our vicinity—in Connecticut—where they have been studied topographically and mapped with a marvelous degree of accuracy and minuteness by Percival.

The trap rock, microscopically investigated, shows throughout a crystalline texture. It is made up of pyroxene, labradorite and magnetite, with also occasionally some chrysolite and apatite. Chlorite is often present as the result of local change. The pyroxene sometimes, in coarse varieties, runs off into long-bladed prisms, somewhat resembling hornblende, which name has in consequence often been given to it. The pyroxene is the first constituent to suffer from surface alteration. The magnetite is commonly in irregular masses, but sometimes shows curious and beautiful arborescent crystallized forms, frequently observed elsewhere in similar rocks. It is interesting to observe that these peculiar dendritic forms are confined, as far as now observed, to the more hydrous of the trap rocks, although future study may not confirm this. The feldspar shows before the microscope its triclinic character; and an analysis of the rock by Mr. Hawes proves that it has the composition of labradorite. This analysis gives for the rock exactly the composition of a dolerite, and it must receive this name, as it has generally done hitherto, though in this we go counter to foreign usage, according to which dolerite is a younger rock, not older than the Tertiary. It is important to observe that the rock, as it contains no hornblende, is not diorite, though that name has also been given to it.

Turning, however, from what may be called the normal rock, for example, that analyzed by Mr. Hawes just referred to, containing almost no water, we find other varieties containing a considerable amount; and here the microscope comes to our assistance, making it possible to extend our observations over a wide range in a short time.

We find that the trap which has come up *through the older crystalline rocks* is most of all free from hydrous minerals, or any evidence of alteration, its grains having a fresh, vitreous look on the fracture. This is also true, but not quite so completely, of trap from the West Rock range, which adjoins the region of crystalline rocks, and that from East Rock near New Haven, a little more to the eastward, and of trap from other points south and west. As we go from West Rock toward the eastern side of the Mesozoic sandstone region, the character of the rock changes; it loses its lustre and hardness, and is often amygdaloidal. The change is due to the hydrous character. The rock from the Saltonstall ridge, for example, contains 4 to 5 per cent of water. This trap is throughout more or less green under the microscope, containing a chlorite, sometimes in plates and seemingly made at the expense of the pyroxene, sometimes in cavities, these last often entirely invisible to the unaided eye. This general character belongs to the trap more or less decidedly from East Haven north, following the eastern range through the Durham or Middletown Mountain; it is also true of a large portion of the Meriden range and its continuation north to Mt. Tom.

In addition to this massive, though generally chloritic trap, which makes up most of the great ridges laid down on Percival's map, we find also another variety quite different; this is light green in color, soft, very hydrous, and has its feldspar as well as pyroxene very much altered. It is most characterized by its amygdaloidal structure, the cavities being very numerous, filled with calcite or chlorite or quartz, and sometimes with datolite or anaclite. The cavities are also sometimes in part filled, curiously enough, with bitumen; this is true of some dikes west of Hartford. This amygdaloid, moreover, is found in a series of low, subordinate ridges parallel to or concentric with some of the most prominent ranges, for example at the Meriden Hills, as first pointed out by Percival—one of the most curious phenomena connected with this subject, and not easy of explanation.

The fact, just alluded to, that the amygdaloidal and massive trap occur in the same dike, seems to shut out the idea that these subordinate ridges are different in age from those adjoining. If we accept the idea suggested by Prof. Dana in a recent article, that the moisture in igneous rocks found access to them while they were in process of eruption, we may regard all these as results of changes wrought in what originally was essentially the same material by local causes in introducing moisture. In accordance with this view the trap intersecting the crystalline rocks is anhydrous, while that of the interior of the sandstone region, where alone subterranean streams of waters were possible, are more or less hydrous. But I leave the subject here, since until many more facts have been collected and the work of the microscope has been supplemented by that of the laboratory under the hands of Mr. Hawes, any definite conclusion will be unsafe.

In a few individual cases the Triassic trap from Nova Scotia, New Jersey, Pennsylvania, and North Carolina has been examined, and as far as microscopic structure goes the rock from these distant points is hardly to be distinguished from the trap of the Connecticut Valley.

4. *Wood Tin in Georgia;* by WILLIAM P. BLAKE. (From a communication to one of the editors.)—In 1860, while examining a series of specimens of the residual black sand from the sluices used in collecting gold in North Carolina and Georgia, I found several minute grains of wood tin in the sand from the Nacoochee Valley, White County, Georgia. Although it occurs sparingly, the fact that it exists is worthy of record, as it may possibly be traced to larger deposits. I have examined sand from a great number of other localities, southwestward from Rutherfordton in North Carolina, without finding any traces of tin. The usual minerals of the "black sand" about Dahlonega, Georgia, are specular iron, magnetite, ilmenite, rutile, cyanite and garnet. At the Walton Branch, in North Carolina, corundum, zircon and monazite are abundant, with the ordinary mixtures of iron minerals, and xenotime occurs in minute crystals, but no tin ore was found.

5. *Das Erdbeben von Herzogenrath am* 22 *October*, 1873, *Ein Beitrag zur exakten Geologie;* A. VON LASAULX. Bonn, 1874. pp. 157, with several plates.—During the autumn of 1873 the

neighborhood of Aachen (Aix la Chapelle) was the center of a series of earthquake shocks, which continued from September 28th to December 2d; the most violent and extended of them, however, took place on the 22d of October. This earthquake has been the subject of minute and careful study by v. Lasaulx, and all directly or indirectly interested in such matters will find his pamphlet of great interest. After giving all the observations made at different places, he discusses the general character of the earthquake, that is, its intensity and extent, duration, and direction, with the accompany phenomena of sound, etc. He determines also, by careful calculations, the exact superficial center of the action, and also the center in the earth's interior from whence it went out, and its velocity of propagation (360 meters per second). His conclusion upon the second point is important and interesting, he says: the center of propagation (Ausgangspunkt der Erschütterung) did not lie at a depth so great that the direct cause of the first impulse could have been on the limiting zone between the fluid interior of the earth and the solid crust, nor could it have been in any immediate connection with this; on the contrary, this center must have been in the region of the older sedimentary rocks. He adds that it is not improbable that it was connected with the making of cracks and fissures in the earth's crust.

The seismometer, or seismochronograph, is a little instrument devised by the author for registering the time of earthquake shocks. It is intended to be attached to the reliable clocks to be found in telegraph offices and places of that kind, and acts by means of a little lever which falls, stopping the pendulum, at the moment of the shock. This is effected by means of a metal ball, which is dislodged from its delicate resting-place and sets free a spring, which in turn acts upon the lever. The direction of the shock is also approximately recorded. The author justly urges that, with some such apparatus in general use, our observations of the time of earthquake phenomena would be much more numerous and trustworthy. E. S. D.

6. ***Mineralogische Mittheilungen**, gesammelt von G. Tschermak.* Heft II, 1874. Vienna. 77 pp.—Prof. Tschermak is performing a great service to all mineralogists in collecting and making available the memoirs which fill the pages of the successive numbers of the Mittheilungen. His position as Director of the Royal Mineralogical Museum of Vienna—a city well known for its scientific activity—gives him especial advantages for obtaining valuable contributions. This journal appears quarterly in connection with the Jahrbuch der k. k. geol. Reichsanstalt, though it is also published independently. It is now in its fourth year, and as the only journal devoted exclusively to mineralogy, already holds a high place among scientific serials. The last number received is the second issued for 1874, and contains the following papers:

Simple crystals of Albite from the Schneeberg, by J. Rumpf; Morphological study on Atacamite, by Edward S. Dana (New

Haven); On the occurrence of native Iron in a dike of Basalt at Ovifak in Greenland, by G. Nauckhoff; Monograph on Roselite, by A. Schrauf; On Clinochlore, by A. Schrauf; On the occurrence of Meteorites at Ovifak, Greenland, by G. Tschermak; Analyses of Feldspar, Chinochlore (from Chester county, Penn.), Magnesia-mica, Mispickel crystals, etc., from the laboretory of Prof. Ludwig; also short notices of Glauberite from Sicily, of Stalagmites from the Adelsberg Grotto, and of a new and interesting twin (*drilling*) of Calcite.

7. *Description de la Formation Carbonifère de la Scanie*; par E. Erdmann.—Stockholm, 1873. 84 pp. 4to.—This is an abridged edition, in French, of Erdmann's Swedish memoir. The carboniferous formation described is supposed to be of the age of the Lias. Besides this and the Pre-Silurian formation, there are in Scania the Upper and Lower Silurian overlaid conformably by the sandstone of Hör, which Erdmann inclines to make the base of the Scanian Carboniferous. The Carboniferous has afforded plants and mollusks which render it probable that it is not older than the Lias. Above, there are Cretaceous beds, probably over 150 meters in thickness, and then the Quaternary.

8. *Petrographische Studien an den Phonolithgesteinen Bohemens*; Dr. E. Boricky. Prague, 1874. pp. 93, with two colored plates.—The contributions of Dr. Boricky upon the basaltic rocks of Bohemia have already been noticed in this Journal. This pamphlet upon the phonolytes is of the same character, and is marked by the same care and minuteness of description. The beautiful colored plates will be of great help to those studying the memoir.

9. *Descriptions of the Mollusks of the Tertiary of Piedmont and of Liguria*; by L. Bellardi.—Volume xxvii of the Memoirs of the Turin Academy, published in 1873, contains Part I. of this memoir. It occupies pages 33 to 294, and is illustrated by 15 beautiful and well crowded lithographic plates. The species include the Cephalopods, Pteropods, and Heteropods, and the Gasteropods of the families Muricidæ and Tritonidæ.

10. *Beskrifning öfver Besier-ecksteins kromolitografi och litotypografi använda vid tryckningen af geologisk Öfversigtskarta öfver Skåne, meddelad af* Algernon Börtzell.—No geological charts are more admirably colored than those of Sweden. This paper, giving an account of the method, and representing by a colored plate the mixtures of colors for producing 200 different shades of color, is of the highest interest to the geologists of this country, and all workers in chromolithography.

11. *Das Elbthalgebirge in Sachsen* von Dr. H. B. Geinitz; I Theil, 7 Lieferung.—This new number contains eight new lithographic plates, 53 to 60 inclusive, of Gasteropods from the Lower Quader or Middle Cretaceous, representing more than a hundred species.

12. *Geological Survey of Georgia.*—A survey of the State of Georgia is in progress under the direction of Professr Little, with Mersrs. McCutchen and Schley as assistants.

13. *Geological Survey of Sweden.*—The geological survey of Sweden is at present under O. M. Torell as chief geologist; the other geologists are A. E. Törnebohm, E. Erdmann, D. Hummel, O. J. Gumælius, M. J. Stolpe; assistant geologists, V. Karlsson, J. G. O. Linnarsson, L. J. Palmgren; actuary, J. E. Börtzell; chemist, G. H. Santesson.

14. *Memoirs of the Geological Survey of Italy.*—The second part of volume ii of these memoirs contains a paper by B. Gastaldi on the ophites or Pietri verdi of the Alps.

15. *American Entomology.*—A record of American Entomology for 1873, by A. S. PACKARD, Jr., is contained in the Sixth Annual Report of the Trustees of the Peabody Academy of Science at Salem, Mass.—The same report contains also descriptions of N. A. Noctiudæ by A. R. Grote, of N. A. Phalænidæ, by A. S. Packard, Jr., besides others of N. A. Phyllopoda by A. S. Packard, Jr., and notes on some dredgings near Salem by A. E. Verrill.

16. *The Carnivorous Habits of some of our Brother-Organisms—Plants*, is the theme chosen for his address by the president of the Department of Zoology and Botany (Dr. Hooker), at the Belfast meeting of the British Association. Although the subject owes the great interest it now inspires chiefly to some sagacious investigations by Mr. Darwin, still mainly unpublished, it has been so much more attended to in a popular way in the United States that it has no longer here the complete novelty which it appears to possess on the other side of the Atlantic. Here the principal facts, as known up to a recent date, along with those relating to plant-climbing and insect-fertilization, are matters of school instruction; and a narrative in The Nation recapitulated what is known, and what has been observed in this country especially, from the time of Macbride, and later, of Dr. Curtis, down to contributions of Mr. Canby and Mrs. Treat. Also the curious new discoveries made last summer by Dr. Mellichamp upon the most interesting of the Sarracenias, given in full in the New York Tribune, and in abstract in this Journal, were presented anew, and with further particulars, to the American Association, at the Hartford meeting early in August; and Mr. Canby supplemented these with an account of the behavior of the California analogue of *Sarracenia*, viz., *Darlingtonia*.

The report which has reached us of Dr. Hooker's interesting dissertation is unofficial and evidently somewhat imperfect, having many of the literary faults incident to a reporter's transcript. But its new points—of which alone we can now take notice—are clearly presented and are full of interest. Passing by the resuscitation of Linnæus' forgotten suggestion that the *Sarracenia* leaf may be a metamorphosis of that of *Nymphæa*, hollowed out into a cup to hold the water in which it can no longer float (on the strength of which he claims Linnæus as a Darwinian evolutionist!), and the classification of the pitchers of the several species according to their endowments and adaptations, which has been before alluded to; and interposing only the remark that in wild and even in cultivated plants of *Sarracenia flava* we have found the watery secretion bedewing the inside of closed pitchers nearly ready to open, gathering into drops, and trickling toward the bottom of the cavity; we proceed to *Nepenthes*, the pitcher-plants of the oriental tropics and of hot houses, of which Dr. Hooker has made a special study. Time has not yet been secured for the completion of the investigations; but Dr. Hooker has made out the following capital points: 1. The rim of the Nepenthes pitchers in all the species examined "secretes honey," a lure to insects, etc.; so also does the under side of the lid when this remains overhanging, but not in those species with lid everted. In the latter, honey on the lid would tend to allure insects away from the pitcher instead of into it. 2. Below the rim is a very smooth, opaque surface, affording no foothold to insects, and producing no secretion. 3. "Below this zone the interior of the pitcher is entirely occupied by the secretive surface, consisting of a cellular floor crowded with spherical glands in inconceivable numbers. Each gland precisely resembles a honey-gland of the lid, and is contained in a pocket of the same nature, but semicircular, with the mouth downward, so that the secreted fluid all falls to the bottom of the pitcher. In *Nepenthes Rafflesiana* 3,000 of the glands occur on a square inch of surface, and upward of a million in an ordinary-sized pitcher. I have ascertained that, as was indeed to be expected, they secrete the fluid which is contained in the bottom of the pitcher before this opens, and that the fluid is always acid." 4. A further secretion of this fluid is excited by supplying the pitcher with animal matter. "When the fluid is emptied out of a fully formed pitcher that has not received any animal matter, it forms again, but in comparatively very small quantity. . . . . I do not find that placing inorganic substances in the fluid causes an increased secretion; but I have twice observed a considerable increase in pitchers after putting animal matter in the fluid." 5. This fluid evidently digests animal matters. Treated with "white of egg, raw meat, fibrine, and cartilage," in all cases the action is most evident, in some surprising. After twenty-four hours' immersion, the edges of cubes of white of egg are eaten away and the surfaces gelatinized. Fragments of meat are rapidly reduced, and pieces of fibrine weighing several grains dissolve and totally disappear in two or three days. With cartilage the action is most remarkable of all; lumps of this weighing eight and ten grains are half gelatinized in twenty-four hours, and in three days the mass is greatly diminished and reduced to a clear, transparent jelly." "Little action takes place in any of the substances placed in the fluid drawn from pitchers and placed in glass tubes, nor has any followed after six days' immersion of cartilage or fibrine in pitchers of *N. ampullaria* placed in a cold room, whilst on transferring the cartilage from the pitcher of *N. ampullaria* in the cold room to one of *N. Rafflesiana* in the stove, it was immediately acted upon." From this it is conjectured that something analogous to pepsine is secreted by the pitcher after the reception of the animal matter.

In speculating, at the close, upon this apparently wholly anomalous inversion of the functions of plants and animals, Dr. Hooker compares the phenomena he has been considering with the nourishment which the embryo of albuminous seeds, when germinating, draws from the deposit which surrounds or is in contact with it, referring also to Van Tieghem's experiments, in which an artificial emulsion was successfully substituted for the real endosperm. The analogy is hardly good; all plants and all organs use organized matter in their formation and are wholly made of it; the growth of an embryo, a bud or shoot, and of a cell, are alike in this. The difference is that they feed upon vegetable, not upon animal matter; upon matter assimilated by the plant itself, not upon matter further assimilated by an animal. Nor is the real analogy quite reached in the comparison with parasitic plants, even "flowering plants that pass through their lives without ever doing a stroke of the work that green plants do." It is found, however, in parasitic plants of a lower grade which feed directly upon animals or animal matter, living or dead;—from which point of view these higher carnivorous plants under consideration may not only "find their place as one more link in the continuity of nature," and be thought to have done so through gradations such as Dr. Hooker suggests, but they may also be conceived as cases of far-reaching *atavism.*

As these subjects become matters of popular interest, the grossest misapprehensions and mis-statements must be expected. In England, the Graphic leads off with a well-executed page of woodcuts, swarming with flies and hornets, some of which are busy about the mouth of a nondescript *Sarracenia*, having a curiously lobed or scolloped lid. The letter-press describes *Nepenthes* and *Cephalotus* as having "lids which shut down upon their victims," while *Darlingtonia* "curls its leaf around them," and so on.

A. G.

17. *Linnean Society of London.*—Upon the retirement of Mr. Bentham, after twelve or thirteen years of most efficient service, Prof. Allmann, of Edinburgh, was elected president at the last anniversary meeting, the zoologists taking their regular turn. The choice is a happy one. It is to be hoped that the new president will continue the annual addresses which his predecessor initiated and made so interesting The society is established in the commodious quarters provided for it by Government in the new portion of Burlington House, the apartments of late temporarily occupied being given up to the Royal Society. One of the Fellows is now appointed to take charge of the publication of the Journal of the Linnean Society, the editorial care of which had mainly devolved upon the late president. Some changes in the by-laws which seem to have been required in order to secure responsible editorship, and which were adopted upon the recommendation of the council, were yet contested in a manner which

caused the society to take legal advice upon the matter. The award of Lord Hatherley, now published, affirms the validity of the new enactment, and so settles, it is to be hoped finally and satisfactorily, a controversy which, being wholly of a domestic nature, had better not have been referred to in the scientific journals. A. G.

18. *Restored Professorship of Botany at the Jardin des Plantes, Paris.*—One of the three chairs of Botany at the *Jardin des Plantes*, namely, the one long occupied by the Jussieus, was suppressed after the death of *Adrien de Jussieu*, in 1853, and a chair of paleontology established instead. Thanks to the exertions of Count Jaubert, this botanical chair has been reconstituted, and M. Bureau has been named to fill it. M. Maxime Cornu succeeds M. Bureau as *aide-naturaliste.* The reorganized laboratory of instruction at the Garden, under the charge of these two active botanists, has been in most successful operation during the past season. A. G.

III. Astronomy.

1. *On the Spectrum of Coggia's Comet;* by Dr. Huggins.—The new point noticed in this communication was that the bands of the comet were so far shifted as to indicate—supposing there really was carbon in the comet—that the relative motion of the approach of the comet to the earth was forty-six miles per second. The comet really, however, approached the earth at the rate of twenty-four miles per second; and it was therefore uncertain whether the whole or part of the difference in this velocity was due to the motion of matter within the comet. The brighter portion of the head of the comet was due evidently to a larger proportion of the matter giving a continuous spectrum. It seemed probable, therefore' to the autnor that the nucleus was solid, heated by the sun and throwing ont matter which formed the coma and tail; and part of this was in a gaseous form, giving the spectra of bright ilnes. The other portion existed probably in small incandescent particles; the polariscope showing that certainly not more than one-fifth of the whole light was reflected solar light.—*Proc. Brit. Assoc., Nature*, Sept. 10.

2. *Meteoric Iron of Iquique, in Peru;* by Gustaf Rose.—This paper was left completed but unpublished by its eminent author, and has now been included in the volume issued by the "Gesellschaft Naturforschender Freunde der Berlin" as the "Festschrift" of its recent Centennial meeting. This mass of meteoric iron originally weighed twenty-five pounds. The part in the museum of the University weighs twenty pounds. It was found ten leagues to the east of the village of Iquique near the western boundary of the Pampa of Tamarugul. Raimondi, the original describer of it, obtained for the specific gravity 7·86. In an analysis by Rammelsberg, 2·66 per cent was removed by a solution of chloride ef mercury. The composition of this part gave—

Iron	Nickel	Phosphorus	Insol. in H^2Cl	
2·17	0·37	0·05	0·07	= 2·66

The nickel and cobalt of the insoluble part was found to be 15·49 nickel and 0·19 cobalt. This left 81·66 for the iron. Raimondi found three pieces of it to contain 81·42, 85·61, 87·59 of iron, and 18·52, 14·37, 12·38 of nickel.

3. *Meteorites.*—A meteorite which fell near Virba (?) in Turkey on the 20th of May last has been described before the Academy of Sciences of Paris by Daubrée. It has a grayish silicated exterior and contains numerous grains of nickeliferous iron and sulphide of iron. It contains also some grains of chromic iron.

Daubrée states also that he had obtained four new fragments of the meteorite which fell at Saint Amand (Loir-et-Cher) in 1872. —*L'Institut*, Aug. 5.

IV. Miscellaneous Scientific Intelligence.

1. *American Meteorology.*—Mr. L. Blodget, in a paper published in the Proceedings of the American Philosophical Society, vol. xiv, at p. 150, after alluding to the fact that an easterly movement in the general circulation of the wind has been made certain by observations on Pike's Peak and Mt. Washington, proceeds to mention facts that establish another point in meteorology: —that the extremes of cold observed at times, especially over the northwestern interior of the United States, were not propagated along the surface, but were produced where they occur, "as if brought down from the upper atmosphere, or as if the result of the action of causes extraneous to the earth's atmosphere." He concludes that the greater number of winds in cold weather particularly are winds that descend, and that to this descent most of their continued force is due. In the course of his paper he says:

"The descent of masses of heavy, cold air, must often be induced simply to fill the void caused by contraction of the volume of air from which rain and snow fall. All along the belt of westerly winds this contraction is going on, and this very rapidly during all the colder months. Moving with a constant motion toward the earth, as well as along the surface, it is only a natural vicissitude of this condition, that the descending mass should, at intervals, be poured, like a mass of cold water, over the border of the humid belt, producing the extremes that so often appear to *strike down from above.*" * * *

"I venture to assume, therefore, a large measure of influence in causing extremes of cold in these latitudes to the descending volume of air incident to the shrinking and wasting of heat and moisture from the atmospheric current eastward in the course of traversing the continent. Its northern border is perpetually invaded by fitful alternations of displacement; sometimes getting calm and intensely cold, reducing the temperature in winter to 10°, 20° or 30° F. below zero; and in spring, when the general accession of heat gives a more free play of the forces, a frequent recurrence of heavy northwest dry winds *poured from above*, and from the north, displacing and condensing the local or surface atmosphere; and this overflow is almost constantly repeated until the whole system of circulation has been swept beyond our limits at the north, by the advance of summer. During most of the summer months the rarifying and expanding forces prevail so completely, as to remove all these phenomena far to the north, or possibly to another hemisphere."

2. *Preliminary Map of Central Colorado*, showing the region surveyed in 1873; primary triangulation by J. T. GARDNER; topography by G. R. BECHLER, H. GANNETT and A. D. WILSON. U. S. Geol. and Geogr. Survey of the Territories, F. V. HAYDEN, U. S. Geologist in charge. Department of the Interior.—This map, with the accompanying sketch showing the triangulation, contains, besides the positions of rivers and settlements, the locations of a very large number of heights, the elevation above the sea level of many of them, the lines used in the primary and secondary triangulation and the positions that were occupied for the survey.

The mountain ranges include the following: (1.) The *Eastern* or *Front* Range, in which stands Long's Peak, 14,271 feet high, and Pike's Peak, 14,147 feet, about ninety miles apart. West of this, (2.) the *Park* Range, east of the head waters of the Arkansas, containing Mt. Powell, 13,398 feet high, Mt. Lincoln, 14,296 feet, and several other summits over 13,000 feet in height. (3.) Next, west of the Arkansas, the *Sawatch* Range, in which, beginning at the north, there are the Mountain of the Holy Cross, north of the head waters of the Arkansas, near latitude 39½° and the meridian of 106°, 14,176 feet high, Massive Mt. 14,368 feet, Mt. Elbert 14,326 feet, La Plata Mt. 14,302 feet, Grizzly Peak 13,315 feet, Mt. Harvard, 14,383 feet, Mt. Yale, 14,151 feet, Mt. Princeton, 14,199 feet, Mt. Antoro, 14,245 feet, Mt. Shavano 14,093 feet, Mt. Ouray, 14,043 feet, the last near the parallel of 38½°; then the continuation south southwestward in the Sangre de Christo Range, in which are Hunt's Peak, 12,446 feet high, Mt. Rito Alto, 12,989 feet, Mt. Crestoner, 14,233 feet, the last near the parallel of 38° on the meridian of 105½°. (4.) Still farther west, crossing the meridian of 107° near the parallel of 39°, the *Elk* Mountains, containing, beginning at the north in latitude 39° 15′, and longitude 107° 10′, Sopris Peak, 12,972 feet high, Capital Mountain, 13,992 feet, Snow Mass Mt. 13,961 feet, Maroon Mt. 14,000, Gothic Mt., on the meridian of 107°, 12,491 feet, Castle Peak, more to the east on the parallel of 39°, 14,106 feet, White Rock, 13,847, Crested Butte, 12,014 feet, Italia Peak, farther east, 13,491 feet. These Elk Mountains lie at the head of the San Juan and Grand Rivers, tributaries of the Colorado. The map registers the results of a great amount of topographical work for a single season.

3. *Tableau des Terrains Sedimentaires;* par E. RENEVIER, Professor of Geology in the Academy of Lausanne; 36 pp., with a series of nine tables in as many folded sheets. 1874 (Bull. Soc.

Vaud. Sci. Nat., Nos. 70, 71, 72).—The tables, which are the chief part of this important memoir, and must have cost the author much labor, are arranged in columns, and present the synonymy and equivalency of the various subdivisions of the periods in geology, with also the prominent localities in different countries, and the characteristic fossils. They commence with the Quaternary, to which one sheet, of a pale yellow color, is devoted. The next sheet, of an ochre-yellow color, includes the Pliocene and Miocene, under the head of the Neogen or Molassic period, for each of the seven subdivisions of which the names of a score or more of fossils are given, and in other columns, the localities and synonymy of different countries. The third, of a bright yellow, comprises the "Eocene or Nummulitic Period." The fourth, of a green color, the Cretaceous, under which fourteen subdivisions or stages are given; and so on through the series to the first of the Paleozoic. The tables will be found very convenient as a help toward understanding the names of subdivisions used in Europe, and also in many other ways.

4. *Franz-Joseph Land.*—The land discovered by the Austrian Polar expedition under Lieut. Weyprecht lies to the north of Nova Zembla over the meridians 58° and 59° and between the parallels of 80° 15′ and 83°, the latter being the most northerly point observed. It is stated to be about as large as Spitzbergen, with many fiords and numerous islands off the coast. It is mountainous; the elevation of the ridges averages 2,000 or 3,000 feet, and the highest summit to the south, named Mt. Humboldt, is 5,000 feet above the sea. Glaciers were of great extent. The ridges are dolomitic. Elk, hares, and traces of foxes and bears were found, and myriads of birds.

5. *Observaciones Magneticas y Meteorologicas del Colegio de Belen de la Compania de Jesus, en la Habana. Año Meteorologico de* 1872. Small folio. Habana, 1874.—The Meteorological Observatory of the college of Belen is under the charge of Señor Benito Viñes, S. J. The observations recorded have evidently been made with great care and fullness. They are presented in tables for each month, with diagrams showing the changes for the month under the heads: velocity of the wind, direction of the wind, horizontal force, declinometer, hygrometric state, tension of the vapor of water, thermometer and barometer.

6. *Table for Dilution of Alcohol.*—The following table, constructed by Berguier, gives the quantity of distilled water necessary to reduce alcohol of a certain known percentage to any desired lower degree of strength. Opposite the number representing the percentage of alcohol in the given sample are placed the quantities of it, and of water, respectively, necessary to produce alcohol of the percentage indicated at the top of any column. Thus, to obtain alcohol for 80° from that of 94°, the number 94 is sought for in the first column, and opposite to it, under the column "Required Strength," "80°," the numbers 808 and 192 indicate respectively the quantities, by weight, of alcohol of 94°, and

of distilled water necessary to produce 1,000 parts of alcohol at 80°. For the sake of convenience, the specific gravities of each grade are given in a separate column.

Table of the relative quantities, by weight, of alcohol of different degrees of concentration, and of distilled water necessary to produce 1,000 *parts of alcohol of a desired lower degree of concentration.*

Strength, in per cent, of alcohol employed.	Specific Gravity.	Required Strength.									
		90° 0·8228 sp.gr.		85° 0·8357 sp.gr		80° 0·8483 sp.gr		60° 0·8956 sp.gr		56° 0·9047 sp.gr	
		Alcoh.	Water	Alcoh.	Water	Alcoh.	Water	Alcoh.	Water	Alcoh.	Water
100°	0·7938	857	143	795	205	735	265	522	478	482	518
99	0·7969	871	129	807	193	747	253	530	470	490	510
98	0·8001	885	115	820	180	759	241	539	461	498	502
97	0·8031	899	101	833	167	771	229	547	453	506	494
96	0·8061	913	87	846	154	783	217	555	445	514	486
95	0·8089	927	73	859	141	796	204	564	436	522	478
94	0·8118	942	58	873	127	808	192	573	427	530	470
93	0·8145	956	44	886	114	820	180	582	418	538	462
92	0·8172	970	30	899	101	832	168	590	410	546	454
91	0·8199	985	15	913	87	845	155	599	401	554	446
90	0·8228	---	---	927	73	858	142	609	391	563	437
89	0·8254	---	---	941	59	871	129	618	382	571	429
88	0·8279	---	---	955	45	884	116	627	373	580	420
87	0·8305	---	---	970	30	898	102	637	363	589	411
86	0·8331	---	---	985	15	912	88	646	3[illegible]4	598	402
85	0·8357	---	---	---	---	926	74	656	344	607	393
84	0·8382	---	---	---	---	940	60	667	333	616	384
83	0·8408	---	---	---	---	955	45	677	323	626	374
82	0·8434	---	---	---	---	969	31	687	313	636	364
81	0·8459	---	---	---	---	994	16	698	302	646	354
80	0·8483	---	---	---	---	---	---	709	291	656	344
79	0·8508	---	---	---	---	---	---	720	280	666	334
78	0·8533	---	---	---	---	---	---	732	268	677	323
77	0·8557	---	---	---	---	---	---	744	256	688	312
76	0·8581	---	---	---	---	---	---	756	244	699	301
75	0·8603	---	---	---	---	---	---	768	232	710	290
74	0·8625	---	---	---	---	---	---	781	219	722	278
73	0·8649	---	---	---	---	---	---	794	206	734	266
72	0·8672	---	---	---	---	---	---	807	193	747	253
71	0·8696	---	---	---	---	---	---	821	179	759	241
70	0·8721	---	---	---	---	---	---	835	165	772	228
69	0·8745	---	---	---	---	---	---	849	151	785	215
68	0·8769	---	---	---	---	---	---	864	136	799	201
67	0·8793	---	---	---	---	---	---	880	120	813	187
66	0·8816	---	---	---	---	---	---	896	104	828	172
65	0·8840	---	---	---	---	---	---	911	89	843	157
64	0·8863	---	---	---	---	---	---	928	72	858	142
63	0·8886	---	---	---	---	---	---	946	54	874	126
62	0·8908	---	---	---	---	---	---	963	37	891	109
61	0·8932	---	---	---	---	---	---	981	19	907	93
60	0·8956	---	---	---	---	---	---	---	---	925	75
59	0 8979	---	---	---	---	---	---	---	---	943	57
58	0·9001	---	---	---	---	---	---	---	---	961	39
57	0·9025	---	---	---	---	---	---	---	---	980	20

—14 C. ccxii, 1874, 251.

7. *Tin-bearing country, New England, in New South Wales, Australia.*—A report by the Licensed Surveyor, C. S. Wilkinson, to the Surveyor General, dated July 14, 1873, contains the following among its statements. The region described lies within a radius of about twenty-five miles from Inversell to the south and east. The principal tin mines are on Cope's Creek, Middle Creek and Macintyre River. The rocks are granites, greenstone trap, Carboniferous beds, Miocene, Pliocene and Quaternary.

The Quaternary includes drift deposits. On the Macintyre valley the stratified drift is in terraces of various heights above the river. Rev. W. B. Clarke states that he has traced some of this drift at great heights above the valley for more than eighty miles. They are evidently the work of the waters from the melting glacier and those of the continued floods during the time of high latitude depression of (in the northern hemisphere, at least) the Champlain period.

The Pliocene of the region includes extensive basaltic outflows. The Tertiary abounds in stems and leaves of plants of the genera *Laurus*, *Cinnamomum*, *Daphnogene*, and others, which are referred to the Lower Miocene, M'Coy finding some species closely like those of Oeningen and of the vicinity of Bonn.

The Carboniferous beds are of the same age with those of the Hunter.

Stream tin is found in the Drift, and also in the Miocene; and valuable veins of tin ore occur in the granite. The granite is stated to be closely like that of Cornwall, and is pronounced of Upper Carboniferous age.

The amount of tin ore raised during 1872 was about 800 tons. The tin-mining region of Inversell is but a small portion of the stanniferous country of New South Wales and Queensland.

8. *Tortoises of Mauritius closely related to those of the Galapagos, places that are nearly antipodes to one another.*—Dr. A. Günther, in a memoir on "the Living and Extinct Races of Gigantic Land-Tortoises," Parts I and II of which have been published, states that there are remains of gigantic Tortoises on Mauritius and the neighboring island of Rodriguez associated with those of the Dodo and Solitaire, which indicate that the races have only recently become extinct. They differ from other Tortoises of the region in having a flat cranium and truncated beak, and in this respect they have the greatest affinity with the tortoises still inhabiting the Galapagos Archipelago. Dr. Günther observes that the presence of these allied tortoises at points so remote from one another can be accounted for only on the view that *they are in each case indigenous.*—*Ann. Mag. Nat. Hist.*, xiv, 311, Oct., 1874.

9. *The Journal of the Franklin Institute*, Philadelphia.—The August number of this excellent journal contains a paper by Prof. S. P. Langley, on the external aspects of the Sun,—its photosphere and spots, its chromosphere and corona, which is a valuable review of this whole subject, as viewed by an astronomer who

had himself made original observations on the subject. It is illustrated by a plate of one of the sun-spots, which is as wonderfully well executed as the spot is marvelous in itself.

This scientific monthly, devoted to physics and practical chemtry, as well as mechanical science, is now edited by Prof. George F. Barker (Professor of Physics in the University of Pennsylvania), and could not be in better hands.

10. *Transactions of the Wisconsin Academy of Sciences, Arts, and Letters, Madison, Wisconsin;* vol. ii, 1873. 254 pp., 8vo.—Among the papers in this volume are the following: R. Irving, on some points in the geology of northern Wisconsin; P. R. Hoy, on some peculiarities in the fauna near Racine; W. W. Daniels, on the rapidity of the absorption of arsenic by the human liver; T. C. Chamberlin, on some evidences bearing upon the method of the upheaval of the quartzites of Sauk and Columbia Counties, and on fluctuations in the level of the same quartzites; R. Irving, on the junction of the Primordial sandstones and Huronian schists in Wisconsin, and on the occurrence of gold and silver in quartz from Clark County.

Dr. J. W. Hoyt, of Madison, is president of the Academy.

11. *Cincinnati Quarterly Journal of Science.*—The number for October, the last of the first volume, contains descriptions of new species of mollusks from the Cincinnati group by the Editor J. A. Miller; a paper on other species from the same group by U. P. James; also descriptions of new species of *Lichenocrinus, Glyptocrinus* and *Beyrichia*, besides other papers of interest.

OBITUARY.

M. Elie de Beaumont.—On the 24th of September died Elie de Beaumont, the eminent geologist, and long the "Secrétaire Perpetuel" of the Academy of Sciences of Paris. Born on the 25th of September, 1795, he entered the Polytechnic School in 1819, and leaving it with the highest honors, entered the Ecole des Mines in 1821. He became Professor in the Ecole des Mines in 1829, Professor in the Collége de France in 1832, Engineer in Chief of Mines in 1833, Inspector-General of Mines and Member of the Academy of Sciences in 1835, its Perpetual Secretary in 1853 in place of Arago, Senator in 1854, and Grand Officer of the Legion of Honor in 1860.

Dr. Friedrich Hessenberg died in his native city of Frankfort, on the 8th of last July. A jeweler by trade, he yet found time to devote to his favorite science of mineralogy, and though entirely self-educated, he ranked among the foremost mineralogists and crystallographers in Germany. His "Mineralogische Notizen," published in 11 numbers, contain some of the most important contributions ever made to the science.

Half-hour Recreations in Popular Science, Dana Estes, Editor, No. 12. The Circulation of the Waters on the Surface of the Earth, by H. W. Dove. Boston, 1874.

Fig. 1.

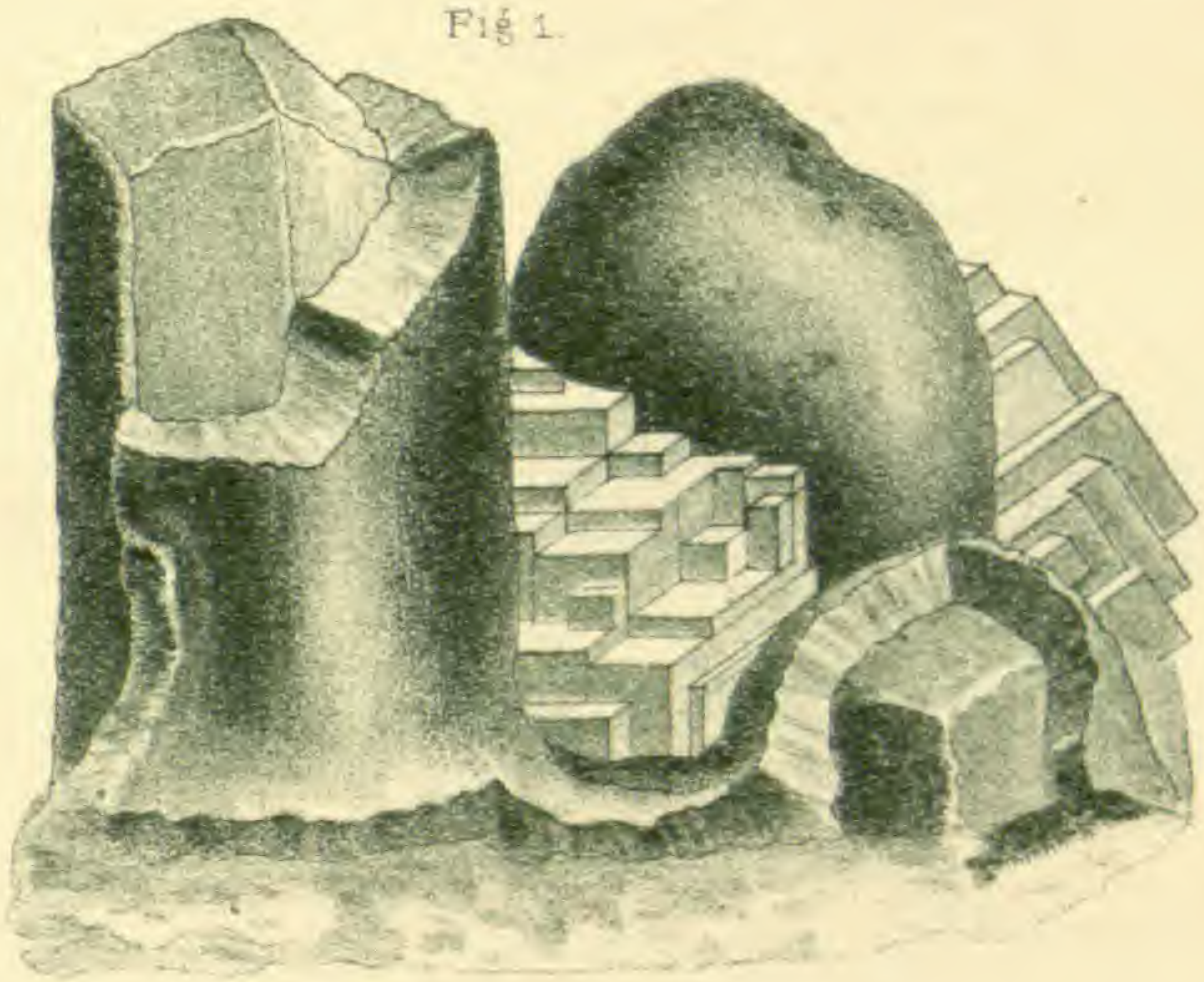


Fig. 2.

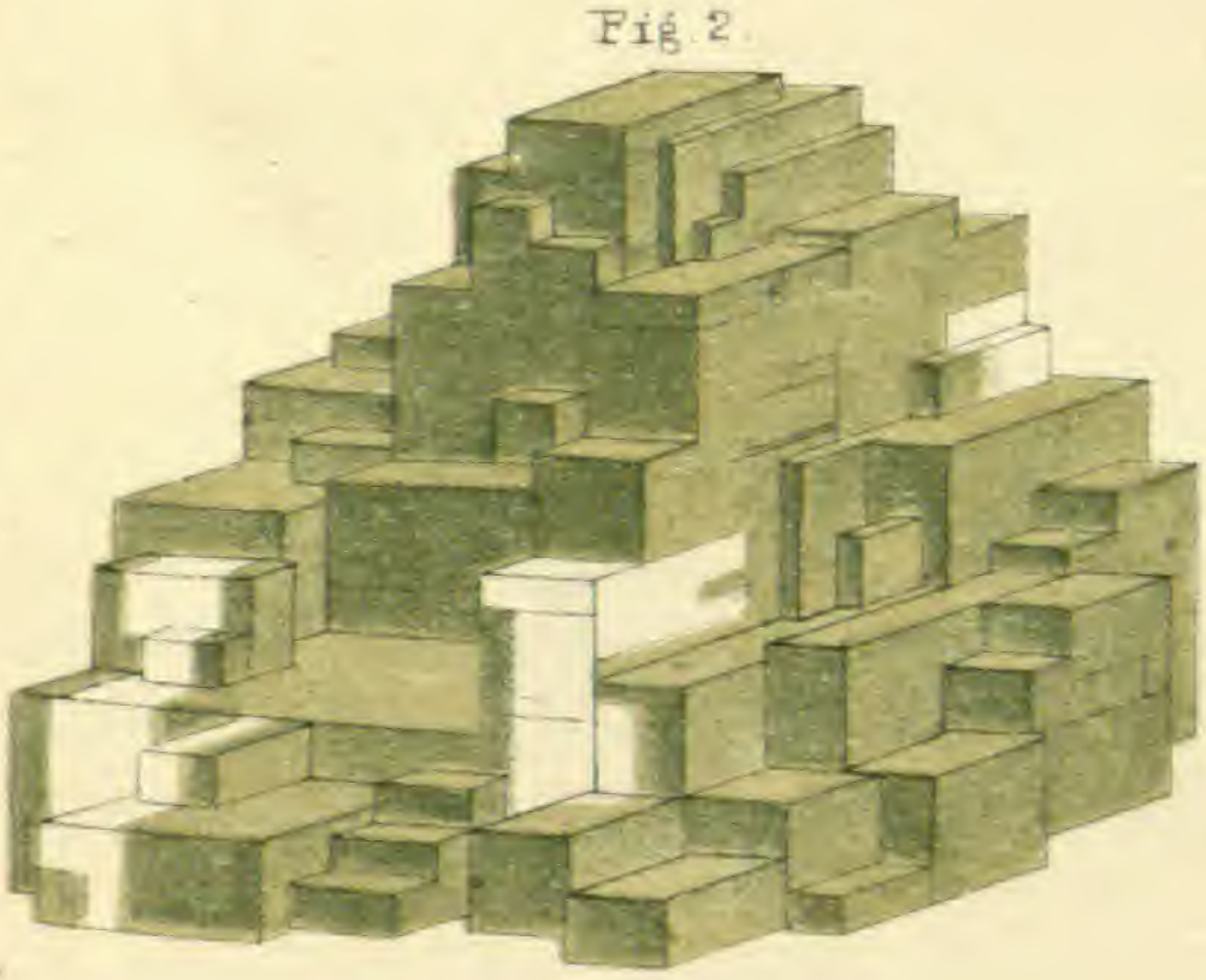


Fig. 4.

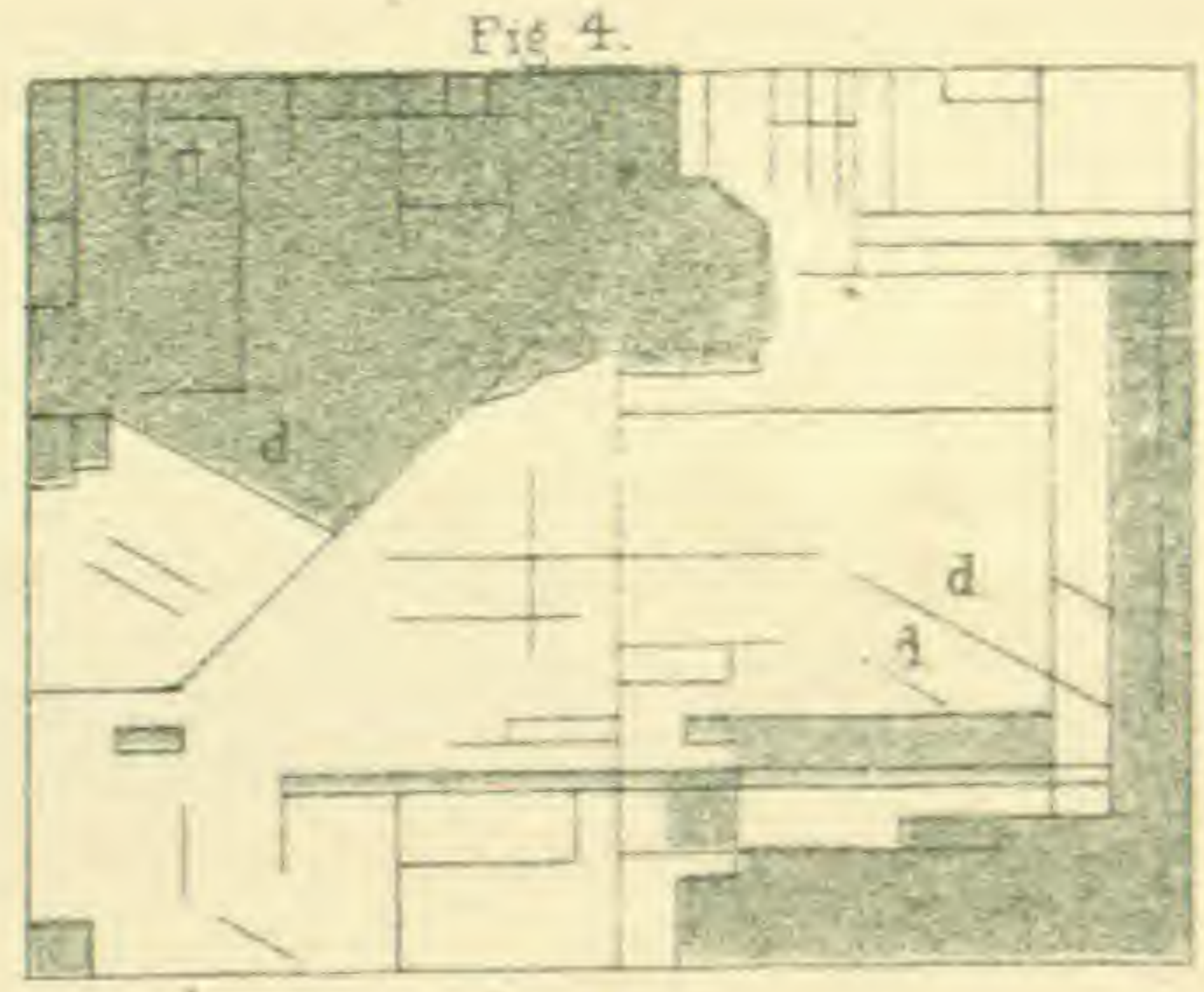


Fig. 3.

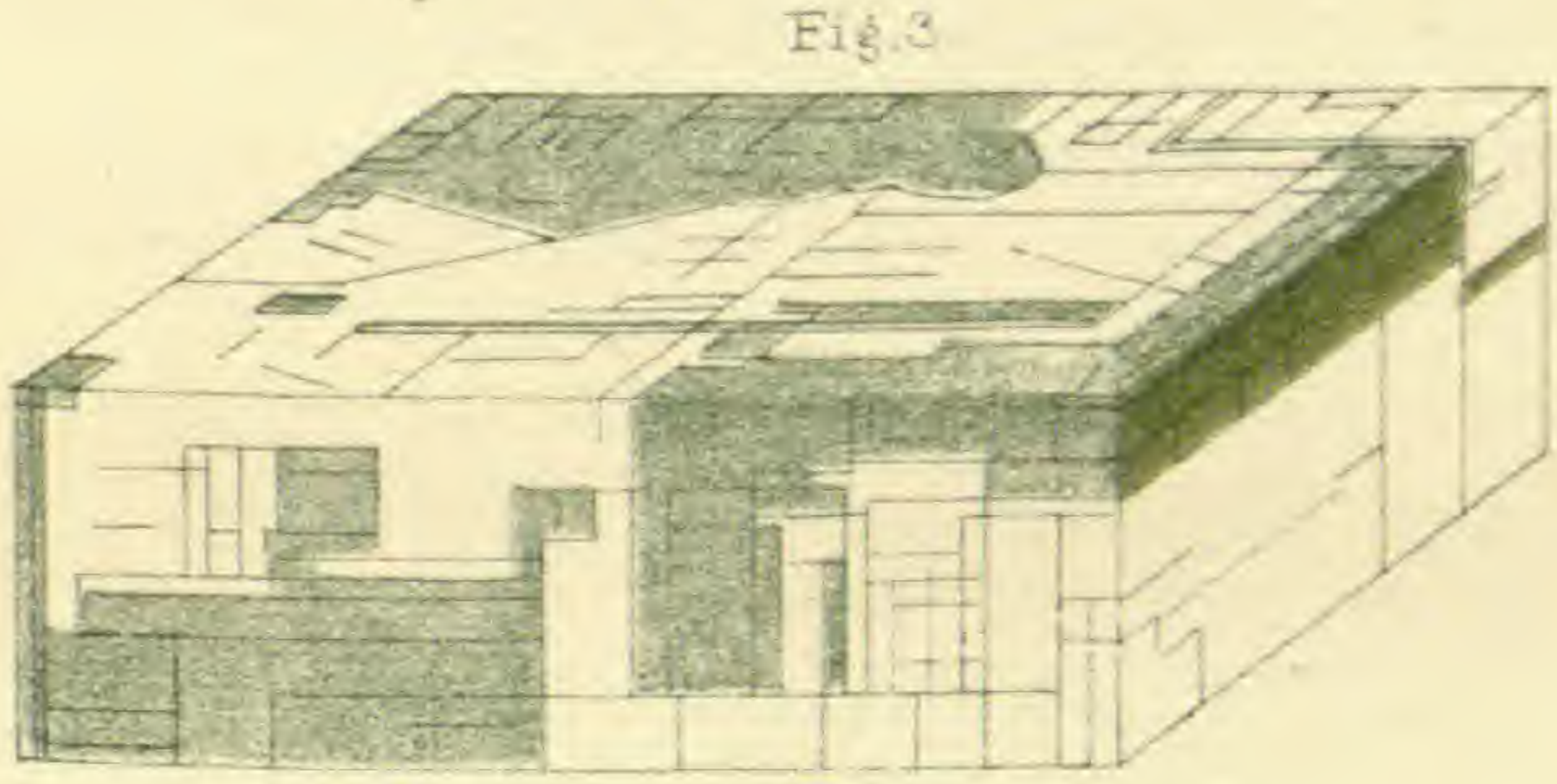


Fig. 5.

Fig. 6.

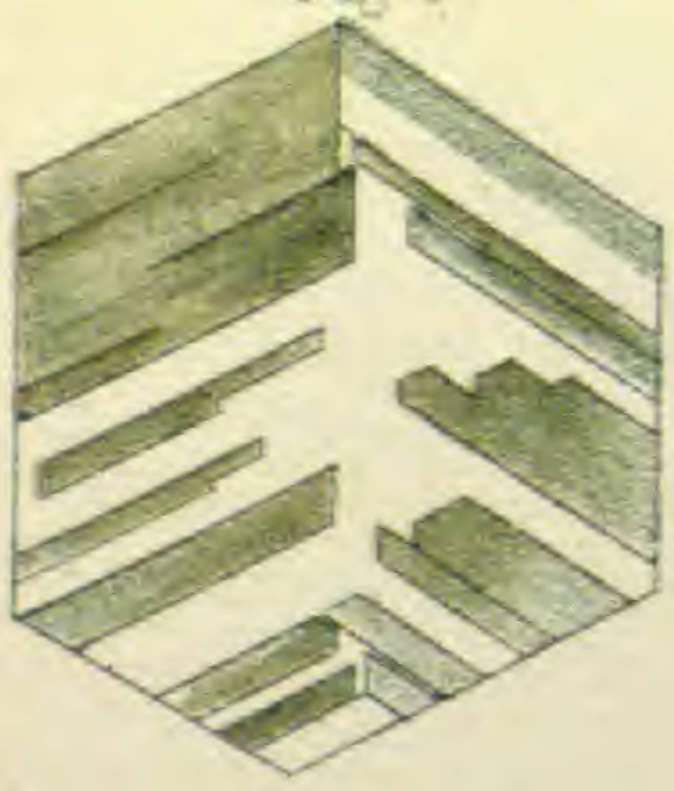
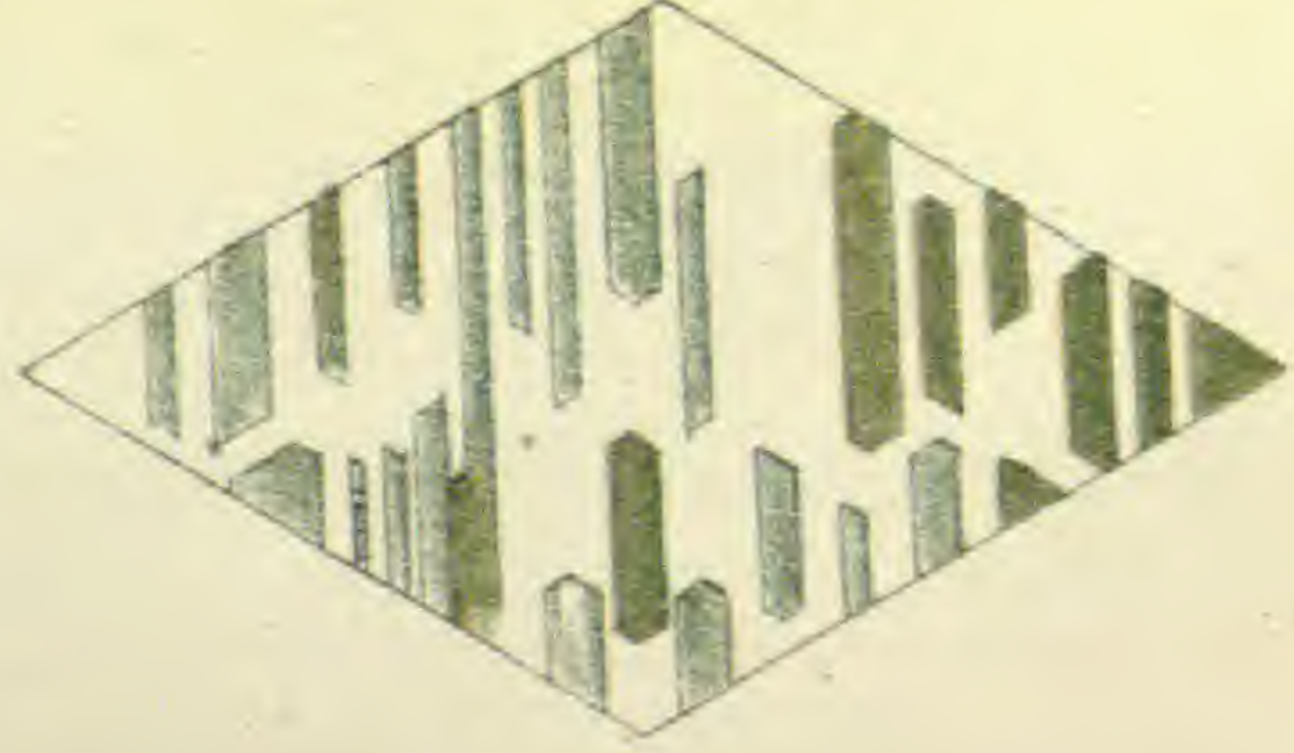


Fig. 7.



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SERPENTINE PSEUDOMORPHS.

THE

AMERICAN

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[THIRD SERIES.]

ART. XXXV.—*Review of von Seebach's Earthquake of March 6th*, 1872, *in Central Germany*;* by BEN K. EMERSON, Prof. of Geology in Amherst College.

THE Neapolitan Earthquake Report of Robert Mallet marks an epoch in the history of earthquake investigation. Although the view first casually advanced by Dr. Young† and definitely stated by Gay-Lussac,‡ that an earthquake is "a very strong sonorous wave," had been elaborated twenty-five years before by Mallet himself in a Memoir on the Dynamics of Earthquakes,§ and by William Hopkins in his celebrated Report on the Theories of Elevation and Earthquakes,‖ and although methods for finding analytically the depth of the center of disturbance from seismometric observations had been given in both these papers, and later in Mallet's Earthquake Reports, yet the remark of Hopkins, that "the roughest approximation to the position of the focus from which such vibrations proceed would constitute a very important geological element," remained as true in 1872 as in 1847, when it was written. In the quarter century which intervened between these two dates, immense labor was bestowed upon earthquake-catalogues by Mallet, Perrey and others, and upon the discussion of these to determine geographical lines or areas of disturbance, periodicity in relation to the seasons, the phases of the moon, &c.; and it

* Das mitteldeutsche Erdbeben vom 6. März, 1872. Ein Beitrag zu der Lehre von den Erdbeben von Karl von Seebach.

† Lectures on Natural Philosophy, vol. i, p. 717, 1807.

‡ Annal. de Chim., vol. xxii, p. 428.

§ Proc. R. I. Acad., vol. xvi, 1846. ‖ Brit. Assoc. Rep., 1847.

would seem that this source of information had been very nearly exhausted, and that resort must be had to a purely physical treatment of the subject. The Earthquake Reports of Robert Mallet* had done much to render this possible, especially by perfecting the seismometer, by the beautiful mathematical determination of the zone of greatest intensity, the theorem for finding the focus from the time of shock at three points, and by elaborate series of experiments on the transit velocity of elastic waves, produced by the explosion of gunpowder, in loose sand and in both compact and jointed granite at Killiny Bay,† and in quartzite and schist at the government quarries at Holyhead, North Wales.‡ It was eminently fitting that he should write the first treatise on the principles of observational seismology, and give the first illustration of their use in determining the elements of an earthquake: its true transit velocity, wave velocity, depth of the "centrum," or region where the impulse originated, the position of the "epicentrum" and isoseismic curves.

I know of no earlier attempts to determine any of these elements, except the early, very imperfect ones, by Milne, of the Lisbon earthquakes of 1755 and 1761;§ by Dr. B. Trask, of that of Jan., 1857, in California;‖ and two by Julius Schmidt, of the earthquake at St. Goar on the Rhine,¶ and that of Veterna hola in Hungary,** where by application of the calculus the velocity of transit on the earth's surface was determined.

The method employed by Mallet for the Neapolitan earthquake of 1857†† was based on the fact that the principal cracks found in buildings, walls, &c., would be normal to the direction of the wave at the point where they were formed. Lines drawn perpendicular to these planes would, therefore, intersect in the focus or "centrum," assuming it to be a point, or sphere of small diameter, and the azimuth of these lines would intersect on the earth's surface at the "epicentrum," or point directly over the focus, the inclination of these two sets of lines to each other giving the angle of emersion for each place of observation. Further, the angle of emersion thus known, the velocity of vibration of the wave was obtained by calculation from the distance to which bodies of known weight

* Brit. Assoc. Rep., 1850, '51 '52, '55, '58.

† Second Rep. of Earthquake Phenomena, Rep. Brit. Assoc., 1851.

‡ Account of experiments made at Holyhead to ascertain the transit velocity of waves analogous to earthquake waves through local rock formations, Phil. Trans., 1861, p. 655.

§ Ed. Phil. Jour., vol. xxxi.

‖ This Jour., 1858, xxv, p. 146.

¶ Das Erdbeben von 29 Juli, 1846, im Rheingebiet von Dr. Jakob Nöggerath.

** Untersuchungen über dem Erdbeben am 15 Jan., 1858, J. F. Schmidt, Astronom. Athen.; Mit. d. K-k. Geograph. Gesell., zu Wien, 1858.

†† The Great Neapolitan Earthquake, or the Principles of Observational Seismology, 2 vols.

were projected, or by the degree of resistance overcome in forming cracks, as determined by separate experiments. The results obtained by Schmidt and Mallet, together with the results of the latter's experiments on transit velocity in various rocks, are brought together below in comparison with the numbers obtained by the method we are reviewing, the whole including all the reliable numerical results for earthquake elements thus far published.

The middle-German earthquake of the 6th of March had its center (epicentrum) about halfway between Erfurt and Coburg, and was felt in a region bounded approximately by a line passing through the cities of Berlin, Breslau, Regensburg, Tübingen, Frankfort and Brunswick. The zone of greatest intensity was south of a line passing between Gera and Altenburg, but large objects were nowhere overthrown, and few fissures were formed in walls and buildings. The method of Mallet was, therefore, not applicable, but the author employs instead a method, already used by Schmidt to determine the surface velocity, and here for the first time carried out in detail. This method is based upon accurate determinations of the time of shock for the greatest possible number of localities within the region affected, and for this purpose the author proceeded immediately to collect from periodicals, from railroad and telegraph officials, from astronomical and physical observatories, and from other sources, all notices of the time of shock. These notices, with all the facts reported concerning the earthquake, are given in full—arranged geographically with their authorities—in the first part of the work (pp. 5–103). The times were then reduced to Berlin time, tabulated, and discussed under the assumption that the difference in transit velocity resulting from the different conducting power of different strata, and the irregular distribution of fissures, joints, &c., would, for large distances, counteract each other and result in a velocity approximately uniform. Under this assumption, which seems fully justified by the result, that portion of the vibrations which reach the surface of the earth would form concentric widening circles, and all points on each circle would be shaken at the same instant. These are the isoseistic curves of the author (coseismic curves, Mallet).

If, then, a perpendicular be drawn from the middle of a chord connecting two such points (points shaken at the same absolute time, and thus lying in the same isoseistic circle), it would pass through the epicentrum, and the intersection of several such perpendiculars would determine the position of this epicentrum. This method was applied as follows. From the 149 observations of time, those from the astronomical observatories of Göttingen and Leipzig, being coincident in time and most trustworthy, were taken, and upon a large and accurate map these towns were united by a straight line, and a perpendicular erected from the middle of the same. Then, Eger and Halle coinciding also in time, and deserving, from the character of the observer in one case, and from the agreement of different observers in the other, the highest confidence, were chosen, and in the same way a perpendicular was erected from the center of the line uniting them. These perpendiculars cut each other near the village of Amt-Gehren (lat. 50° 38·6′ N.; long. 8° 41·25′ E. of Paris) and fix the place of the epicentrum. In order to establish this more firmly, the author proceeds to discuss the remaining observations, and to construct isoseistic curves for each minute. The earliest reported time is 3h. 55m. P. M.—reported from twelve localities. Of these, several are proved to be false by their relation to the accurate determinations of Göttingen-Leipzig and Eger-Halle. Nevertheless, five lie in a circle whose center is at Amt-Gehren, and give thus the isoseistic curve for 3h. 55m. In this way the curves for each minute up to the limit, Breslau, 4h. 5m. 25s., are determined (plate II), each curve when fully established uniting three or more places which reported the same time, and all having a common center at Amt-Gehren. As a result of the observations which deserve any confidence, above 40 per cent point directly to the same spot, a degree of accuracy far greater than could have been expected.

For the determination of the true transit velocity, the depth of focus, and the time of initial impulse at the focus, a very beautiful graphic method is introduced. If t = the time of shock, and α = the distance from the epicentrum, of a given locality, m, a short algebraic discussion obtains the equation for a hyperbola, wherein these values α and t = the axes of abscissas and ordinates. Laying off, then, miles (α, α', α'') on the axis of abscissas, and minutes (t, t', t'') on the axis of ordinates for each given locality (m, m', m'', &c.), these latter must describe a hyperbola. The degree of accuracy with which they do this gives, in the first place, an indication of the degree of accuracy of the determinations of time and the proper correction for each, and furnishes a means of controling the determination of the epicentrum given above. If this hyperbola be thus drawn on profile paper in large scale, we can read off directly the true transit velocity = the number of units on the axis of abscissas (miles) measured off by the curve in passing over one unit on the axis of ordinates (minutes). For the earthquake under discussion this equalled 24 nautical miles per minute, or 742 meters per second. The center of the hyperbola, or the point at which the asymptote prolonged cuts the axis of ordinates, gives t^{0}, i. e., the time of the initial shock at the centrum, and the distance of this point from the focus of the hyperbola gives the time occupied by the wave in passing vertically from the center or origin of the shock to the surface of the earth. The latter value multiplied by the mean transit velocity already obtained, gives the depth of the "centrum" in miles.

The first of these constants (t^0)—the time of the initial shock—is, in this case, easily determined by drawing the hyperbola; the second, however—the distance of the center of the hyperbola from its focus—is not attainable from the lack of observations near the epicentrum. The method fails here only from lack of observations, and not from anything inherent in itself. In this case the author employs the method of Mallet, and calculates the depth of center of shock from the direction of cracks produced in buildings in the most violently shaken portion of the earthquake area, and reaches the conclusion that "the depth of the true focus ("centrum") of the earthquake of March 6, 1872, may have equalled in minimum 7·76 nautical miles =14,360 meters, in maximum 11·64 n. m. =21,470 meters; the mean depth being 9·68 n. m =19,850 m. This completes the second part of the work (pp. 104–170); and in a third part (pp. 171–188), the author collects his results and compares them with those obtained for other earthquakes by Mallet and Schmidt, calculating, by the method employed above, the constants, not obtained by these writers, from the data given by them.

Depth of the Center.—The author compares the depth of center of the Neapolitan earthquake, as given by Mallet, with the result obtained above, draws in plate II the hyperbola for the Rhenish earthquake of 29th July, 1864, from numbers given by Julius Schmidt, and, in plate III, that for the earthquake of Veterna hola, using data supplied by the same author. The four results are tabulated below, and give all the data at present known for the depth of center of earthquakes. The depths are in nautical miles.

Locality.	Date.	Calculator.	Mean.	Maximum.	Minimum.
Middle Germany,	6 Mar., '72,	v. Seebach,	9·68	11·68	7·76
Naples,	'58,	Mallet,	11·765	8·125	2·75
Rhineland,	29 July, '64,	v. Seebach,		24·	20·93
Veterna hola,	15 Jan., '58,	v. Seebach,		17· (?)	14·16 (?)

Intensity.—The intensity at the earthquake center of the shock or blow which caused the vibrations, is here sought. In passing out from this center the intensity of vibration must grow less as the square of the distance from that center. And at the outer limit of the shaken region the intensity of the vibrations is approximately the same for every earthquake, because this outer limit is determined by the observations of a great number of individuals, and indicates only the point where the vibrations cease to be perceptible to the senses, which would be when they had been weakened to a certain low constant intensity. The initial intensity would then be proportional to the square of the distance from the center to this outer limit. Calculated thus, the relative intensity is:

Middle Germany,	3305·2
Naples,	1252·6
Rhineland,	1577·3
Veterna hola (Hungary),	1612·5

It follows that the intensity of an earthquake is, as a rule, proportional to its area, and that earthquakes of great intensity and limited area originate at a comparatively small distance below the surface.

Transit Velocity.—The true average transit velocity of the four earthquakes under discussion, as found by the author, are given in the table below, together with the results obtained experimentally by Mallet, at Killiny Bay and Holyhead, for the real transit velocity in different rocks by the explosion of mines of powder.

	Meters per second.	Intensity.
Middle Germany,	742·0	3505·2
Naples,	259·7	1252·6
Rhineland,	567·6	1577·3
Hungary,	206· (?)	1612·5

In wet sand,	251·4
" granite much jointed,	398·16
" compact granite,	507·36
" quartzite and schist,	379·16

One of the results of Mallet's experiments at Holyhead—that the transit velocity increases with an increase in the intensity of the initial shock—is brought out clearly by the comparison in the table above of the initial intensity with the transit velocities of the first three earthquakes discussed.

The form of the Centrum.—One of the most interesting and, at first sight, most startling deductions of the author, relates to the form and position of the area in which the shock originated. If this area had been, as has been heretofore assumed, spherical and of small dimensions, the region of most violent action (in overturning buildings, &c.) would have been in a circle around the epicentrum whose radius would have been 8·8 nautical miles. This is deduced from an interesting theorem given by Mallet.* The vibrations reaching the earth at the epicentrum will have of course the greatest intensity, but the horizontal component will then be zero, and as one passes out from the epicentrum, although the intensity of the vibrations diminishes,

* Brit. Assoc. Rep., 1858, p. 101.

the horizontal component increases more rapidly, and thus the zone of maximum disturbance will be in a circle at some distance from the epicentrum, its position being expressed, for a homogeneous perfectly elastic medium, by the equation:

$$a : h : r = 1 : r2 : r3.$$

a=radius of circle of maximum disturbance,
h=depth of centrum,
r=distance from centrum to circle of maximum disturbance.

The zone of greatest disturbance in the earthquake under discussion was found by observation not to fulfill these conditions, but to be a short curved band at a distance of forty nautical miles to the N.W., and lying not concentric but radial to the epicentrum. In explanation of this anomaly the author makes the same assumption concerning the shape of the centrum which was made by Mallet concerning that of the Neapolitan earthquake, and which seems *a priori* the most probable, namely, that it would be a fissure in the earth's crust, and would be a plane and not spherical. If now this plane be placed at an oblique angle to the horizon, the vibrations will have a greater intensity in a direction normal to the length of the surface, and on the other side of the plane the direction of greatest intensity would be toward the interior of the earth, and the zones of equal intensity would be elliptical, as they are seen to be partially in plate I. If, then, a line be drawn from this area of greatest disturbance to the centrum, the plane of the latter must be normal to the line. This fixes the dip and strike of the fissure whence, and possibly by the formation of which, the shock originated. We give the result in the author's own words. "The centrum—the focus of the earthquake of March 6, 1872, lies near Amt-Gehren, 9·6 nautical miles beneath the earth's surface, and is most probably a fissure, which strikes N.N.W. and S.S.E., is, however, of only slight horizontal extent. This fissure is not vertical, but dips E.N.E. into the interior of the earth."

We have sought to give a clear *résumé* of the contents of this valuable work, both of the methods employed and the results obtained. We should, however, wish, with the author, that these results, though in themselves valuable and instructive, should be taken rather as an illustration of a new and valuable method, which is especially applicable where Mallet's could not be used—in regions visited by earthquakes of slight intensity and wide extent, as, possibly, for the regions whose centers lie near Montreal and Portsmouth, N. H., and certainly for California. For the latter region especially, the ease and rapidity with which, by this means, all the elements of an earthquake may be obtained, should enforce the important recommendation made by Professor Whitney* to the naturalists of that State, that an important field for scientific research is here opened to them—the erection, namely, at suitable localities, of simple, inexpensive and permanent seismometers, like those described by Mallet in the Admiralty Manual, and redescribed in the work under review with important simplifications, which would enable one to give for each station—with but moderate expense for erection, and none for maintenance of instruments—the exact time and intensity of each shock.

It seems not too much to hope, with the author, "that hereafter no earthquake will visit a civilized region without the attempt being made to discover, by the method here proposed, which would require for its application but a few hours' time, its geologically important elements."

NOTE.—The work of Professor v. Seebach is already bearing fruit in Germany, and the earthquake of 22d Oct., 1873, in the northwest portion of the Rhine Provinces and the adjacent parts of Belgium and Holland, the most violent of a long series extending through the months Sept.–Dec., is to be the subject of a similar investigation at the hands of A. v. Lasaulx.—*Jahr. f. Min.*, 1874, p. 167.

[Dr. von Lasaulx's admirable paper has already appeared, at Bonn, and is noticed in this volume, at page 392.—EDS.

ART. XXXVI.—*A Jet Aspirator for Chemical and Physical Laboratories*; by Prof. ROBERT H. RICHARDS.

DURING the winter of 1868–9 I was called upon by Professor Storer to put up, in the chemical laboratory of the Massachusetts Institute of Technology, the aspirator which has, quite commonly, gone by the name of the Bunsen filter pump. Some experiments were made to ascertain the best form of filter pump, the results of which may be found in the London Chemical News for Aug. 12, 1870.

This aspirator (as all who have used it are aware) relies upon the column of water which has passed through it for its power, and there are two main conditions of its success. 1. The slower the water is fed to it, the greater will be its power. 2. The column of water which performs the work dates from the aspirator downward to the point of outlet. The longer this column, the greater will be the power of the aspirator.

While working at Bunsen's filter pump it occurred to me that the great force of water which is fed to the building from the hydrant (which was entirely lost in Bunsen's and Sprengel's apparatus) ought to be made available. Accordingly, I looked up the subject in the winter of 1870–71, to see what had been done in the way of jet pumps, etc. Giffard's injector was the most perfect and accurately described of anything that was found.

* N. A. Review, vol. cviii, p. 518.

In Ewbank's Hydraulics I found quite a number of jet arrangements, none, however, which in any way touched the problem which was before me, i. e., to draw air by means of a pressure of water drawn from a hydrant. A jet pump was then made somewhat of the form presently to be described, but which received its water supply from the side tube *k*, fig. 1, and drew the air in through the central tube *l*.

With this form of aspirator the air was rarefied to about one-half its normal density, 380mm., but the instrument used an enormous quantity of water (and Boston water rates are high), and it was found to be a matter of the greatest nicety to adjust the tube *l* in order to obtain any aspiration at all. For these reasons, after a number of trials it was abandoned.

Quite a number of instruments were then made to ascertain whether water fed through the central tube *l* would draw air, but in every case the water came in in the form of a fine jet, and entirely failed to bring with it the air.

The idea then occurred to me that if some means could be devised to break up this jet into foam it could not fail to carry the air along with it and produce the exhaustion desired. Accordingly, in the autumn of 1871, an aspirator of the same form as Giffard's injector was made and placed in an inverted position, fig. 2. The jet at first was broken into spray at *i*, and returning downward to *j*, it converted the whole water jet into foam, and the instrument was a complete success. However, this form was difficult to make, and as its principle had been attained, it remained to find a simpler shape which should produce the same result. The zig-zag bends now used were hit upon and proved entirely successful, and the form was so simple that no further changes were sought. This form was perfected during the winter of 1872–3 and was shown before the Society of Arts at Boston, in April, 1873.

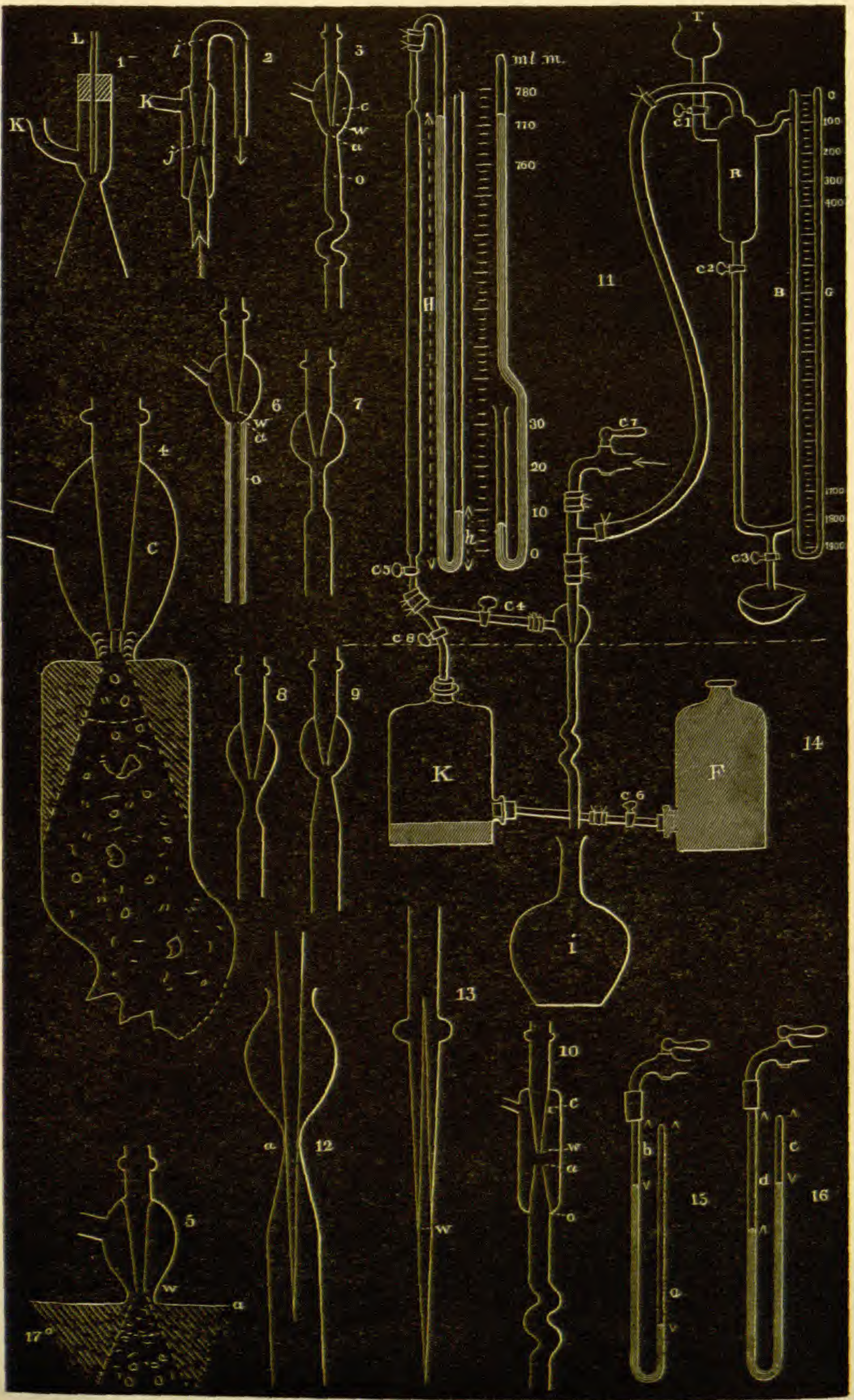
Since the middle of July, 1874, I have tried a number of experiments with a view to ascertain the best form of the aspirator and also under what conditions it can be most successfully employed.

There are several points which must be studied separately to produce a perfect instrument. If w=diameter of jet, fig. 3; a=diameter of the apex of the cone a o; o=diameter of base of the cone and outlet tube; then the points for our consideration will be as follows:

(1) The relative size of w and o; (2) the relative proximity of a and w; (3) the form and angle of the cone joining a and o; (4) the relative sizes of a and w depending upon:

(*a*) The amount of water column pressure; (*b*) the amount of rarefaction desired; (*c*) the quantity of air desired.

(1) The relative sizes of w and o.—As has been previously



stated, a successful aspirator of this form must have true foam existing in its outlet tube *o* when it is at work, and it is evident that the larger this tube becomes relatively to *w*, beyond certain limits, the less will be the probability of our attaining a true foam. On the other hand, the smaller the tube *o* relatively to *w* the greater and more injurious will be the retarding force of friction. While keeping these two limits in sight the following experiments were made, the air being freely admitted. Aspirators of these sizes were tried.

$w^2 : o^2 = 1 : 15$ gave foam.
$w^2 : o^2 = 1 : 20$ " "
$w^2 : o^2 = 1 : 24$ " tube of water.
$w^2 : o^2 = 1 : 28$ " " "

which determines approximately that 1 : 20 is the most economical ratio.

(2) The relative proximity of *a* and *w*.—I have made no special determination to get at this distance, nor do I believe that there is an exact value to it which is better than all others. I have found, however, that if (in the aspirators made on the scale that I shall presently describe) this distance much exceeds one-quarter of an inch, it will invariably be fatal to the success of the aspirator. This failure seems to be accounted for by the fact that nearly all of these aspirators have a more or less jerky movement and the water for an instant comes back into the chamber *c*, fig. 3. When this happens, unless *w* is close to *a*, the jet of water issuing from *w* will lose its identity in the mass of water below it and the aspirator will become powerless.

(3) The form and angle of the cone joining *a* and *o*.—The state of things in this aspirator is not directly, in so many words, water pressure stemming atmospheric pressure. It is the loss of momentum incurred by this rapid jet, which carries along and drives out the atmosphere. Other things being equal, the quicker we can stop this jet the greater will be the loss of momentum per minute, and hence the greater the power. Therefore, if we could stop instantly the stream which moves at ten miles or more per minute (1202 c.c., through a jet ·040 of an inch in diameter), we could make it perform, by this means, its greatest work. That is to say, if we join *a* and *o* by a sharp shoulder, this result will be attained. But by doing this an unlooked-for state of things modifies our conclusions. An eddy is formed in the angle of the shoulder where the water lies idle (see fig. 4) and apparently out of the lines of force from the jet. This eddy easily makes its way up into the chamber *c* and paralyzes the aspirator.

The following rough experiment was made to ascertain, if possible, the exact position of these lines of force. An aspirator was cut off at *a* (see fig. 5), and its lower extremity *a* was dipped into a large beaker full of water and the water pressure turned on to determine the form in which the foam would naturally arrange itself. It assumed the form of a perfect cone, with no observable curve.

The angle of this cone was determined under quite a number of different pressures, to see if it would vary with the pressure. The method of measuring the angle was rough (sighting by two rulers and transferring to paper), but the angle does not seem to vary with the pressure.

TABLE I.

Water pressure, in mm. of mercury which would balance it.	Angle of foam outline.
2054	17°
2054	$17\frac{3}{4}$°
2054	14°
1521	16°
1275	$16\frac{1}{4}$°
1103	$14\frac{3}{4}$°
892	17°
425	13°

The exact condition of things in this aspirator seems to be that the water jet, on arriving at *a*, fig. 3, is obliged to fill the whole cross section at *a*, thus forming a wad or diaphragm which, if we trace in its forward movement to *o*, we shall find that this elementary portion of the jet must do one of two things in order that it shall fill the cross section at *o*: it must either lose momentum or it must take air with it to help it to fill up the space. Practically both these things take place. Sufficient reason has already been given for having the angle of this cone as large as possible (i. e., the elimination of friction and the most rapid reduction of momentum).

The following experiment, however, was tried with a view to prove these points. An aspirator (fig. 6) was made, having $a = o$ and $w : a = \cdot056'' : \cdot080''$. The distance from *a* to *o* was varied by cutting lengths successively off: the length *ao* is noted under each experiment in the table.

a o.	Air tension.
5·2 inches.	0 mm.
4· "	232 "
3· "	521 "
2· "	628 "

This indicates that the shorter the constricted tube *a o* became, the better was the result in exhaustion.

The experiment just described also distinctly shows that the aspirator must have a definite point assigned for the field of contest between the air and water. Figures 7 and 8 are very poor forms, while fig. 9 is an excellent one. Experience in making a large number has verified this, and when the angle is made 17° the water is always able to stem and hold at bay as much of the atmosphere as its pressure is able to push out. An experiment was made to prove this. An aspirator with an angle of 35° was attached to the water supply and to the manometer, and while using 2287 mm. water pressure, gave 538 mm. air tension, when the water ran back into *c* and the aspirator was paralyzed. The angle of this same instrument was then changed to 17° by re-melting the glass, and at the next trial it produced a certain proportion of the vacuum, 538 mm. same as before, but this time it stemmed the atmospheric pressure and the water was unable to flow back again, proving the efficacy of the 17° angle for the cone. It may perhaps in the future be proved that some curved outline, for instance a trumpet-shaped enlargement, would be more efficacious than a true cone, but I have not as yet been able to experiment on it, and the true cone which I have assumed has brought very good results.

(4.) The relative size of *a* and *o* depending upon—

(*a*) The amount of water column pressure.

(*b*) The amount of rarefaction desired.

(*c*) The quantity of air desired.

As *a* is the scene of contest between the pressure of the water exerted downward and the atmospheric pressure exerted upward, the chamber *c* being vacuous or partially so, it seemed as an *a priori* certainty that, in order to have the water and air balance each other, the ratio of areas of $a : w$ should bear some definite proportion to the ratio of pressures of water and air; if, for instance, P = the pressure of water and Q= the atmospheric pressure, then it would seem probable that this equation would satisfy all the conditions (*a* and *w* being diameters):

$$P \times w^2 = Q \times a^2.$$

If this question was merely a statical one, this would undoubtedly be true, but momentum enters very prominently into the discussion during the process of exhaustion, and it would seem as though it must change the state of things, although when the absolute vacuum is reached the instrument is in true statical equilibrium. The experiments presently to be noticed were made with a view to answer these questions. A gauge was made for measuring the water pressure shown in fig. 11, which gives the whole arrangement of barometer, air manometer and water gauge. I is a funnel tube for replenishing the mercury; R is a reservoir to contain the mercury; G is the gauge tube, sealed above, and having an arbitrary scale calibrated to read equal volumes within the gauge tube; B is the balance tube. The operation of measuring water pressure by means of this gauge is as follows:

(1.) Open C_1, fill R, close it again.
(2.) " C_2, " B, " " "
(3.) Let on the water pressure which causes the mercury in B to fall and that in G to rise.
(4.) Bring the mercury in B opposite to and on a level with that in G. This is done by raising the mercury with cock C_2 or by lowering it with cock C_3, as the case may be. Now take the reading: call it r.
(5.) Shut the water off and bring B and G again on a level, while C_1 is open for free admission of the atmosphere. Take the reading and call it R.
(6.) A barometer by the side of the air manometer will tell the number of mm. supported by the atmosphere: call it n and let $m =$ the number of mm. of exhaustion shown by the air manometer; then by the law of gas tension we have—when the aspirator has produced a perfect vacuum—

$\frac{R}{r} \times n =$ mm. of mercury, able to balance the pressure of water.

When the aspirator is open to the free admission of air—

$\frac{R}{r} \times n - n =$ mm. of mercury, as above.

When the aspirator has produced a partial vacuum—

$\frac{R}{r} \times n - (n - m) =$ mm. of mercury, as above.

These formulæ give the pressure of water which is actually called into use by the aspirator.

In order to test the relative sizes of a and w as best suited for various columns of water, seven aspirators were made in glass, of varying dimensions, which are here shown:

TABLE II.

	w. Inches.	a. Inches.	o. Inches.	Ratio $w^2 : a^2$.	Cone angle.
No. 1,	·068	·068	·250	1 : 1	17°
" 2,	·0612	·075	·240	1 : 1½	"
" 3,	·055	·078	·230	1 : 2	"
" 4,	·052	·090	·230	1 : 3	"
" 5,	·0425	·085	·250	1 : 4	"
" 6,	·040	·090	·270	1 : 5	"
" 7,	·0635	·246	·254	1 : 15	"

The dimensions are all given in fractions of an inch, and I feel confident that the error does not exceed ·002″. These fine measurements were made with a Brown and Sharp's micrometer gauge, and the method employed was this: A very acute cone of glass was dropped into the partly finished pump (fig. 12) to measure a and its exact point of contact noted by the scratch of a file. The diameter of the cone at the point of contact was measured by the gauge. To obtain the jet w, a having already been measured, let $\frac{1}{r}=\frac{w^2}{a^2}=$ the ratio which we desire to fulfil, then $\sqrt{\frac{a^2}{r}}=$the diameter of w which we require. Another acute glass cone is cut off at the right diameter by means of the gauge and a file, and the apex half of this cone is dropped, base downward, into the already formed jet (fig. 13), which is then cut off exactly at the base of the cone. The greatest source of error lies in the fact that the steadiest hand cannot always rely upon making the cross section of his jets true mathematical circles.

Explanation of letters, terms, etc., used in the following experiments.

G_1 is the column recording the gauge readings under the atmospheric pressure alone.

G_2 is ditto when the water has been turned on and the aspirator has attained its greatest vacuum.

G_3 is the column recording gauge readings taken just after G_2 and with only one change in the apparatus, i. e., the air is freely admitted to the aspirator, the water cock remaining untouched.

H, h and H-h are the air manometer readings, the latter giving the number of mm. sustained by the difference in tension between the normal and the exhausted air.

In each case below will be found a calculated statement of the actual *water pressure* which was used to sustain the (H-h) of the atmosphere, which is placed next to it on the same line. One pair of columns is marked "with vacuum," the other "without vacuum;" in the first of these the water pressure is calculated from G_2 and G_1.

$$\frac{R}{r}\times n-(n-m)=\frac{G_1}{G_2}\times n-\{ n-(H-h) \}.$$

In the second it is calculated from G_3 and G_1.

$$\frac{R}{r}\times n-n=\frac{G_1}{G_3}\times n-n.$$

The exact cause of the difference of readings G_2 and G_3 is worthy of note. The reading G_3 is taken when the water cock is in exactly the same position as when G_2 was read, but the atmosphere has been admitted to the chamber c and the additional impelling force to the water which lowered G_2 is now wanting and hence the gauge rises when G_3 is taken. The question may be asked, "what right have we to charge to one experiment a different amount of pressure from what we do to another when the gauge in both cases reads the same, or, as sometimes happens, the difference between the readings would indicate an opposite difference (i. e., the smaller one greater than the larger), from the resulting calculation." My answer is that when the atmosphere is admitted it balances the atmospheric pressure on top of the feed reservoir, and when it is *not* admitted, the whole atmospheric pressure is added to that of the column of water which exerts the force, increasing its power by 15 lbs. on the square inch, and if there is need of further proof, it may be obtained from a set of experiments noted by the asterisks below each set of trials. It will there be seen that more water came through the aspirator when it was producing a vacuum than when it was not, even though the pressure gauge readings were the same, which plainly indicates that with the vacuum, there is greater impelling force to the water than without it.

ASPIRATOR No. 1. $a^2 : w^2 = 1 : 1$.

$a =$ ·068″		Barometer,	764
$w =$ ·068		Air,	74° F.
$o =$ ·250		Water,	72° F.

Record of experiments.

G_1.	G_2.	H.	*h*.	H−*h*.	G_3.	H.	*h*.	H−*h*.
1732	900	743	1½	741½	750	415	415	0
	1000	730	18	712	811	415	415	0

Calculated results.

With vacuum.		With no vacuum.	
Water P.	Air tension.	Water P.	Air tension.
1448^{mm}	741½^{mm}	1000^{mm}	0
1271	712	867	0

ASPIRATOR No. 2. $w^2 : a^2 = 1 : 1\frac{1}{2}$.

$a =$ ·075″		Barometer,	766^{mm}
$w =$ ·0612		Air,	75° F.
$o =$ ·240		Water,	73° F.

Record of experiments.

G_1.	G_2.	H.	*h*.	H−*h*.	G_3.	H.	*h*.	H−*h*.
1740	900	744	1	743	727	415	415	0
1740	1000*	742	3	739	775†	415	415	0
1740	1050	689	71	618	825	415	415	0
					1000‡			

Calculated results.

With vacuum.		With no vacuum.	
Water P.	Air tension.	Water P.	Air tension.
1457^{mm}	743^{mm}	1057^{mm}	0
1305	739	954	0
1121	618	850	0

* 1990 c.c. of water passed per minute. † 1708 c.c. of water passed per minute.
‡ 1335 " " " " "

ASPIRATOR No. 3. $a^2 : w^2 = 2 : 1.$

$a = \cdot078''$ Barometer, 762^{mm}
$w = \cdot055$ Air, 74° F.
$o = \cdot230$ Water, 72° F.

Record of experiments.

G_1.	G_2.	H.	*h.*	H−*h.*	G_3.	H.	*h.*	H−*h.*
1732	500	744	1	743	493	415	415	0
1732	810	743	2	741	685	"	"	0
1732	910	742	3	739	730	"	"	0
1732	914*	740	6	734	740†	"	"	0
1732	940	735	11½	723½	750	"	"	0

Calculated results.

With vacuum.		With no vacuum.	
Water P.	Air tension.	Water P.	Air tension.
2620^{mm}	743^{mm}	1915^{mm}	0
1608	741	1164	0
1427	739	1045	0
1416	734	1021	0
1365	723½	997	0

ASPIRATOR No. 4. $a^2 : w^2 = 3 : 1.$

$a = \cdot090''$ Barometer, 764^{mm}
$w = \cdot052$ Air, 75° F.
$o = \cdot230$ Water, 73° F.

Record of experiments.

G_1.	G_2.	H.	*h.*	H−*h.*	G_3.	H.	*h.*	H−*h.*
1732	570	740	5½	734½	565	415	415	0
1732	600§	736	11	725	585‖	415	415	0
					600¶			

Calculated results.

With vacuum.		With no vacuum.	
Water P.	Air tension.	Water P.	Air tension.
2292^{mm}	$734\frac{1}{2}^{mm}$	1578^{mm}	0
2166	725	1498	0

ASPIRATOR No. 5. $a^2 : w^2 = 4 : 1.$

$a = \cdot085''$ Barometer, 764^{mm}
$w = \cdot0425$ Air, 75° F.
$o = \cdot250$ Water, 73° F.

Record of experiments.

G_1.	G_2.	H.	*h.*	H−*h.*	G_3.	H.	*h.*	H−*h.*
1731	550	742	3½	738½	550	415	415	0
1731	572**	741	4	737	566††	"	"	0
1731	600	737	11	726	582	"	"	0
					572‡‡			

* 1577 c.c. water passed per minute.
† 1350 " " " "
1101 " " " when $G_3 = 914$.
** 1148 c.c. water passed per minute.
‡‡ 953 " " " " "
§ 1800 c.c water passed per minute.
‖ 1540 " " " " "
¶ 1516 " " " " "
†† 956 " " " " "

Calculated results.

With vacuum.		With no vacuum.	
Water P.	Air tension.	Water P.	Air tension.
2378	738½mm	1639mm	0
2285	737	1572	0
2176	726	1508	0

ASPIRATOR NO. 6. $a^2 : w^2 = 5 : 1.$

$a =$	·090″	Barometer,	762mm
$w =$	·040	Air,	75° F.
$o =$	·270	Water,	72° F.

Record of experiments.

G_1.	G_2.	H.	*h.*	H−*h.*	G_3.	H.	*h.*	H−*h.*
1732mm	471	740	6	734	471	415	415	0
1732	500*	739	7½	731½	490† 500‡	"	"	0

Calculated results.

With vacuum.		With no vacuum.	
Water P.	Air tension.	Water P.	Air tension.
2774mm	734mm	2039mm	0
2609	731½	1931	0

ASPIRATOR NO. 7. $a^2 : w^2 = 15 : 1.$

$a =$	·246″	Barometer,	764mm
$w =$	·0635	Air,	77°
$o =$	·254	Water,	72°

Record of experiments.

G_1.	G_2.	H.	*h.*	H−*h.*	
1748	462	495	320	175	Air and water exactly balanced each other.

Calculated results.

With partial vacuum.	
Water P.	Air tension.
2301mm	175mm

Comparison of the amount of water column pressure required by aspirators of different ratio, in order that they may produce an approximate vacuum:

TABLE III.

Water pressure.	Air tension.	Ratio of Aspirator.
1448mm	741½mm	1 : 1
1457	743	1 : 1½
1608	741	1 : 2
2292	734½	1 : 3
2378	738½	1 : 4
2774	734	1 : 5
2301	175	1 : 15

* 1202 c.c. water passed per minute. † 1037 c.c. water passed per minute.
‡ 1027 " " " " "

From which we may obtain the following table:

TABLE IV.

Actual ratio. Air tension: Water pressure.	Ratio of $w^2 : a^2$	Difference between pressure and area ratios.
1·954	1·	+ ·954
1·961	1·5	+ ·461
2·170	2·	+ ·170
3·120	3·	+ ·120
3·220	4·	− ·780
3·793	5·	−1·207
13·148	15·	−1·852

This table shows a distinct variation from the theory enunciated above, $Q \times a^2 = P : w^2$, and so long as the actual pressure ratio is larger than the theoretical area ratio, it would seem to be accounted for by friction; but when, as in the last three cases, it becomes less, I confess I am at a loss to discover the law. The differences form a very nice series, growing less and less until they become negative quantities, but up to the time of writing I have failed to ascertain the cause. At the first glance it looks as if the water was doing more than its theoretical amount of work; momentum surely cannot account for it.

Experiments to determine the relative amounts of air and water used by the different aspirators under a constant air tension, and in each case with that water pressure which is the minimum required to produce its best vacuum. Barometer 767, air 74° F., water 73° F.

Record of experiments.

Aspirator.	G_1.	G_2.	H.	*h*.	H−*h*.	Time.	Amt. air.	Amt. water.
No. 1	1734	900	450	374	76	1 min.	512 c.c.	1837c.c.
No. 2	"	1000	450	374	76	"	335	1390
No. 3	"	910	450	374	76	"	578	1227
No. 4	"	570	470	350	120	"	900	1644
No. 5	"	550	460	362	98	"	634	1016
No. 6	"	471	450	374	76	"	757	1110
No. 1	"	900	600	186	414	"	332	2266
No. 2	"	1600	"	"	"	"	182	1840
No. 3	"	910	"	"	"	"	225	1469
No. 4	"	570	"	"	"	"	383	1754
No. 5	"	550	"	"	"	"	470	1108
No. 6*	1755	476	"	"	"	"	520	1160

Calculated statement for comparison of relative air and water volumes of the different aspirators.

* Tried at another time; bar. 764, air 77°, water 72°.

TABLE V.

Aspirator.	Water pres.	Air ten.	Ratio air to water volume.	Time.
No. 1	787mm	76mm	3·588	1 min.
No. 2	639	76	4·015	"
No. 3	771	76	2·123	"
No. 4	1686	120	1·826	"
No. 5	1749	98	1·602	"
No. 6	2133	76	1·466	"
No. 1	1125	414	6·825	"
No. 2	977	"	10·110	"
No. 3	1109	"	6·524	"
No. 4	1980	"	4·551	"
No. 5	2065	"	2·351	"
No. 6	2467	"	2·357	"

When the experiment is made, C_7 is arranged to give the desired pressure on the water gauge, and the air in K is partially exhausted until the air manometer reads the desired figure. C_4 is then closed, and when the watch comes to a convenient second, *i* is instantly put under to catch the water and C_4 is at the same instant opened. The manometer is prevented from rising and indeed held exactly on its desired reading by means of cock C_6. At the instant the minute is up, *i* is removed and C_6 closed; then *i* contains the whole of the water used, and the number of cubic centimeters of water required to fill F again represents the amount of air drawn. To be sure, there is a slight error from having the levels of water in K and F different, but this is entirely insignificant when compared with the forces at issue.

Experiment to ascertain the length of time required to produce a vacuum in a vessel of given size. Aspirator $a^2 : w^2 = 5 : 1$.

Water pres.	H−*h*.	Time.	Water pres.	H−*h*.	Time.
2774	714	1 min.*	2774	637	5 min.
"	731	1½ " *	"	662	6 "
"	731	2 " *	"	676	7 "
			"	693	8 "
"	212	1 " †	"	702	9 "
"	394	1½ "	"	709	10 "
"	474	2 "	"	714	12 "
"	509	2½ "	"	721	14 "
"	548	3 "	"	723	16 "
"	577	3½ "	"	727	20 " ‡
"	601	4 "	"	729	24 " ‡

* Aspirator attached to manometer only. The cock C_8, fig. 14, was closed.

† The other figures record a series taken with C_8 open and the aspirator attached to a vessel which was partly full of common water and which had 1145 c.c. of air space above the water.

‡ At this time the water was effervescing quite freely. I did not try to get H−*h* down to 731 mm. as above.

Another experiment to determine the amount of time needed to exhaust a vessel of known capacity. Aspirator $a^2 : w^2 = 5 : 1$.

Water pres. approximately.	H−h.	Time.	Water pres. approximately.	H−h.	Time.
2774	725	½ min.*	2774	676	4 min.
"	737	1 " *	"	703	5 "
"	739	1½ " *	"	712	5½ "
"	739	2 " *	"	725	6½ "
			"	727	7 "
"	0	0 " †	"	729	8 "
"	255	½ "	"	734	10 "
"	460	1 "	"	737	12 "
"	549	2 "	"	739	15 "
"	633	3 "	"	739	20 "

In order to ascertain how long any vessel will require for exhaustion the law is directly as the volume. A flask of double capacity takes twice as long.

The experiments below were made to ascertain the maximum exhaustion attainable by these four aspirators, the readings being taken much more closely than those given previously. Barometer 767·2mm, air 76½° F.

Aspirator.	Water temp.	G_1.	G_2.	H.	h.	H−h.
No. 1	74°	1740	520	744	0	744
No. 2	73½	1740	520	744	0	744
No. 3	74	1740	500	744	0	744
No. 4	73	1740	500	743	1½	741½

Table showing the amount of rarefaction attained by each aspirator when doing its best work by means of the pressure at hand.

TABLE VI.

Aspirator.	H−h.	Aq. vapor tension. V.	Water temperature.	Water pressure.	Bar. B.	B−(H−h+V).
No. 1	744	21·70	74°	2544	767·2	1·50
No. 2	744	21·39	73½	2544	767·2	1·81
No. 3	744	21·70	74	2647	767·2	1·50
No. 4	741½	21·09	73	2644	767·2	4·61
No. 5	738½	21·09	73	2378	764·	4·41
No. 6	734	20·47	72	2774	762·	8·53
No. 7	175	20·47	72	2301	764·	568·53

The column $B-(H-h+V)$ in this table shows us the error of the aspirator, i.e. the number of millimeters that its air exhaustion varies from the theoretical perfection to be aimed at. The pump will never be able to eliminate the tension of aqueous

* Aspirator attached to manometer only; cock C_8, fig. 14, was closed.

† The other figures record a series with C_8 open and the aspirator attached to a flask without any water in it, of 1340 c.c. capacity.

vapor so long as water is used in it. I have made one attempt to produce a mercury pump to work upon the same principle, but the instrument was not a perfect success. It will need study to secure its best form, and to ascertain whether it can be made more available than Sprengel's fall pump, which is now commonly in use.

An experiment was made to ascertain whether the form of the converging cone which connects the reservoir *c*, and the constricted point *a*, fig. 3, influences in any way the success of the aspirator. The form of aspirator used was that represented in fig. 10., which removes the converging cone entirely. The data of the experiment were:—$w=\cdot056''$, $a=\cdot080''$, $a^2:w^2=2:1$. The barometer stood at 773½.

Air tension.	Water pressure.
745·½mm	2406·9mm
742·½	1621·2
737·	1505·5

In the second experiment the aspirator was about at its limit, i. e., an increase of water pressure would not materially increase its power, while a slight decrease would seriously impair its power, which gives us an opportunity to compare this form of aspirator by means of Table IV with an aspirator of the ordinary form:

	Ratio Air ten. to water pres.	Ratio w^2. a^2.
In aspirator fig. 10,	2·09	1 : 2
" " " 3,	2·17	1 : 2

The difference between these two numbers 2·09 and 2·17, is slight when we consider the extreme difficulty of obtaining the aspirator's exact limit. It may be concluded, therefore, that the converging angle does not influence the power of the instrument. But I found that the form fig. 10, although of the same vacuum-sustaining power, required a great deal longer to produce the vacuum, as many bubbles of air were seen bounding off from the sides of the cone *a o* which ought to have been carried down in it, and very probably would have been carried down if *a* had been joined to *c*, by a converging cone. I therefore conclude that the converging cone joining *c* and *a* does not influence the quality of the instrument, but that it does influence the quantity of air carried by the instrument. I find that an angle of 17° for the converging cone *c a* as well as for the diverging cone *a o* yields very satisfactory results.

Prof. Mixter informs me that Fechner of Berlin uses a jet aspirator differing in form and principle from my Giffard's injector experiment, only in the fact that he fills the cone *a o* with foam by placing *o* in a beaker of water.

From Prof. J. Lawrence Smith, I learned a few days since, that a metallic jet aspirator is used in France. The maker and the particulars of its form he did not recall at the moment.

In the store of Messrs. Codman & Shurtleff of Boston I find an instrument called a saliva pump, made to be screwed to the hydrant at any point. This instrument, while it is essentially the same as mine, differs in obtaining its foam in the cone *a o*, fig. 3, by an obstruction which is introduced at *o*. The cone *a o* is much too acute, according to my experience, but I should judge that it would do fair work, and for its present application is just as good as if it had the most economical form. This instrument was invented and is now made by Dr. J. E. Fisk of Salem, Mass.

Mass. Institute of Technology, Boston.

Note.—Since writing this account, I have given dimensions and rules for the manufacture of these aspirators, to suit approximately any head of water, to Mr. E. B. Benjamin, 10 Barclay street, New York city, who has in his employment an excellent glass blower, and I have sent samples of my own manufacture to aid him in reproducing my idea. It will be necessary for persons desiring to obtain the aspirators to make several determinations during the same day of the column of water which they have at hand. Figs. 15 and 16 show the apparatus which is all sufficient to obtain an approximate determination of sufficient accuracy for the purpose. The U-tube may be of $\frac{1}{8}$th inch bore and of same diameter at both ends, one foot long, should have a slight rib at the top, should be attached to the cock with a rubber connector containing cloth fiber to give it strength, and should be wired on with copper wire. About one half the quantity of mercury that would be required to fill the U-tube should be used. The four measurements, *a*, *b*, *c*, *d*, should be taken in mm. for each experiment, and after the experiment the U-tube will have to be very cautiously removed to allow a gradual expansion of the air in C. Fig. 15 shows the beginning of the experiment, fig. 16 the end.

The following formula will give the approximate number of atmospheres of water pressure at hand, which must be stated in ordering from Mr. Benjamin.

$c\left(1-\frac{d-c}{760}\right) : a\left(1+\frac{a-b}{760}\right) = 1$ atmosphere : atmospheres of water pressure.

or

$$\frac{a+a\,.\,\frac{a-b}{760}}{c-c\,.\,\frac{d-c}{760}} = \left\{\begin{array}{l}\text{atmospheres of}\\ \text{water pressure.}\end{array}\right.$$

I am aware that this formula gives one atmosphere more pressure than the water column yields; but if the reader will refer back to the experiments, he will find that the aspirator actually utilizes one atmospheric pressure more than is due to the water column, for high water pressures one or more atmosphere, and for less powerful ones the aspirator utilizes a portion of an atmosphere more than is due to the column of water.

ART. XXXVII.—*On the Molecular Volume of Water of Crystallization;* by FRANK WIGGLESWORTH CLARKE, S. B., Professor of Chemistry and Physics in the University of Cincinnati.

IT is an important problem in theoretical chemistry to determine the nature of the difference between water of constitution and water of crystallization. Some time since it occurred to me that perhaps a clue to the solution of this problem might be obtained from a careful comparison of the molecular volumes of hydrates and crystallized salts. Having abundant data concerning specific gravities at my command, this work became comparatively easy; and, although I was not led to absolutely satisfactory results, I still came upon some points of considerable interest. Of course my chief difficulties arose from the discrepancies between different determinations of specific gravity, as made by different experimenters. This difficulty was especially great in dealing with the hydrates; where in some instances two determinations of specific gravity for the same substance will differ by nearly fifty per cent. But the untrustworthy character of *some* of my material need not vitiate essentially the conclusions drawn from the whole mass.

My first step was to determine whether the molecular volume of water of crystallization is a constant quantity, or whether it is different in different salts. To settle this question, I had the published specific gravities of thirty-one anhydrous salts, and corresponding determinations for the same substances, plus water of crystallization. The needful calculations were of the most extremely simple character. Subtract the molecular volume of the anhydrous salt from that of the hydrated compound, and divide the remainder by the number of molecules of water present: the quotient will be the molecular volume of the water. It will at once be seen that this method of calculation involves the assumption that the volume of the salt itself is constant; an assumption which, *a priori*, is wholly unwarrantable. In my work I started with it as a mere supposition, not as an assumption, and found it at the end to be wholly justified by my results. Let me now quote the data used in my work, and subsequently present my conclusions. The following specific gravity determinations were employed. The deduced molecular volume is placed after each one, in brackets:

$CaCl_2$, 2·240, Filhol (molec. vol. 49·6). $CaCl_2$, 6 aq., 1·635, Filhol (vol. 133·9). $SrCl_2$, 2·8033, Karsten (56·5). $SrCl_2$, 6 aq., 1·921, Buignet (138·8). $BaCl_2$, 3·886, Schröder (53·5). $BaCl_2$, 2 aq., 3·052, Schiff (79·9). $FeCl_2$, 2·528, Filhol (50·2). $FeCl_2$, 4 aq., 1·926, Filhol (103·3). $CoCl_2$, 2·937, Playfair and Joule (44·2). $CoCl_2$, 6 aq., 1·84, Bödeker (129). $CuCl_2$, 3·054, Playfair and Joule (44). $CuCl_2$, 2 aq., 2·47, Bödeker (69). $MgCl_2$, 2·177, Playfair and Joule (43·7). $MgCl_2$, 6 aq., 1·562, Playfair and Joule (129·9).

Li_2SO_4, 2·210, Kremers (46·6). Li_2SO_4, aq., 2·02, Troost (59·9). Na_2SO_4, 2·597, Playfair and Joule (54·7). Na_2SO_4, 10 aq., 1·520, Filhol (196·6). $CaSO_4$, 2·960, Naumann (45·9). $CaSO_4$, 2 aq., 2·331, Filhol (73·8). $MnSO_4$, 3·1, Bödeker (48·7). $MnSO_4$, 5 aq., 2·087–2·095, Kopp (115·2). $FeSO_4$, 3·138, Playfair and Joule (48·4). $FeSO_4$, 7 aq., 1·8889, Playfair and Joule (147·2). $NiSO_4$, 3·643, Pape (42·4). $NiSO_4$, 7 aq., 1·931, Schiff (145·4). $CoSO_4$, 3·531, Playfair and Joule (43·8). $CoSO_4$, 7 aq., 1·924, Schiff (145·8). $CuSO_4$, 3·631, Playfair and Joule (43·9). $CuSO_4$, 5 aq., 2·254, Playfair and Joule (110·6). $MgSO_4$, 2·628, Filhol (45·7). $MgSO_4$, 7 aq., 1·685, Schiff (145·9). $ZnSO_4$, 3·400, Karsten, Filhol (47·4). $ZnSO_4$, 7 aq., 1·957. Buignet (146·7). $Al_2(SO_4)_3$, 2·171, Playfair and Joule (157·5), $Al_2(SO_4)_3$, 18 aq., 1·671, Playfair and Joule (39·).

$CuK_2(SO_4)_2$, 2·797, Playfair and Joule (119·5). $CuK_2(SO_4)_2$, 6 aq., 2·16376, Playfair and Joule (204·1). $ZnK_2(SO_4)_2$, 2·816. Playfair and Joule (119·1). $ZnK_2(SO_4)_2$, 6 aq., 2·153, Schiff, Kopp (206). $CuAm_2(SO_4)_2$, 2·197, Playfair and Joule (132·7). $CuAm_2(SO_4)_2$, 6 aq., 1·891, Playfair and Joule (211·2), $ZnAm_2(SO_4)_2$, 2·222, Playfair and Joule (131·9). $ZnAm_2(SO_4)_2$, 6 aq., 1·897, Playfair and Joule (211·0). $AlK(SO_4)_2$, 2·228, Playfair and Joule (116). $AlK(SO_4)_2$, 12 aq. 1·726, Playfair and Joule (276). $AlAm(SO_4)_2$, 2·039, Playfair and Joule (116·4). $AlAm(SO_4)_2$, 12 aq., 1·625, Playfair and Joule (279).

$Na_2B_4O_7$, 2·367, Filhol (85·3). $Na_2B_4O_7$, 10 aq., 1·692, Filhol (225·8).

SrN_2O_6, 2·857, Filhol (78·2). SrN_2O_6, 5 aq., 2·113, Filhol (148·3). CaN_2O_6, 2·240, Filhol (78·5). CaN_2O_6, 4 aq., 1·90, Ordway (130·5).

Na_2CO_3, 2·430, Playfair and Joule (43·6). Na_2CO_3, 10 aq., 1·475, Schiff (193·9). $CaCO_3$, 2·7000, Karsten (37·0). $CaCO_3$, 5 aq., 1·783, Pelouze (106·6). $KNaCO_3$, 2·5289–2·5633, Stolba (48). $KNaCO_3$, 12 aq., 1·6088–1·6334, Stolba (208·6).

$NaC_2H_3O_2$, 1·421, Bödeker (59·8). $NaC_2H_3O_2$, 6 aq., 1·40, Bödeker (137·8).

In this list it will at once be noticed that there is a considerable variety of salts, varying in hydration from one to eighteen molecules of water. Chlorides, sulphates, both simple and double, nitrates, carbonates, borates, and acetates, are represented. Now, if we calculate from these data, the molecular volume of the water of crystallization in the manner already described, we shall get the following series of values:

In $CaCl_2$, 6 aq.	14·0	In $CuK_2(SO_4)_2$, 6 aq.	14·1
" $SrCl_2$, 6 aq.	13·7	" $ZnK_2(SO_4)_2$, 6 aq.	14·5
" $BaCl_2$, 2 aq.	13.2	" $CuAm_2(SO_4)_2$, 6 aq.	13·1
" $FeCl_2$, 4 aq.	13·3	" $ZnAm_2(SO_4)_2$, 6 aq.	13·2
" $CoCl_2$, 6 aq.	14·1	" $AlK(SO_4)_2$, 12 aq.	13·3
" $CuCl_2$, 2 aq.	12·5	" $AlAm(SO_4)_2$, 12 aq.	13·5
" $MgCl_2$, 6 aq.	14·4		
		" $Na_4B_2O_7$, 10 aq.	14·0
" Li_2SO_4, aq.	13·3		
" Na_2SO_4, 10 aq.	14·2		
" $CaSO_4$, 2 aq.	13·9	" CaN_2O_6, 4 aq.	13·0
" $MnSO_4$, 5 aq.	13·3	" SrN_2O_6, 5 aq.	14·0
" $FeSO_4$, 7 aq.	14·1		
" $CoSO_4$, 7 aq.	14·6	" Na_2CO_3, 10 aq.	15·0
" $NiSO_4$, 7 aq.	14·7	" $CaCO_3$, 5 aq.	13·9
" $CuSO_4$, 5 aq.	13·3	" $KNaCO_3$, 12 aq.	13·4
" $MgSO_4$, 7 aq.	14·3		
" $ZnSO_4$, 7 aq.	14·2	" $NaC_2H_3O_2$, 6 aq.	13·0
" $Al_2(SO_4)_3$, 18 aq.	13·4		

The mean of all is 13·76.

Here, now, we have practically one value for the molecular volume of water of crystallization, no clearly marked exception having yet appeared to me. When, instead of selected *single* determinations of specific gravity, the average of all the reliable published values for each salt is used in the work of calculation, the uniformity becomes, if anything, more striking. The difference between the extremes in the above series of values is less than is frequently found between two determinations for one substance.

When we consider the variety of salts with which we are dealing, and the great differences in the extent of their hydration, it seems certain that so remarkably uniform a series of values can be interpreted in only one way.

When water unites with an anhydrous salt to become water of crystallization, that water undergoes the entire condensation which ensues, the volume of the salt itself remaining unaltered. That is, the volume of any salt containing water of crystallization is the volume of the anhydrous salt, plus that of the water condensed to a volume of 13·76. An idea of the amount of this condensation may be derived from the fact that the molecular volume of ice is about 19·6. If, in uniting with water of crystallization, the salts themselves were to undergo a change of volume, it is evident that, in such a variety of compounds, we could not possibly obtain a constant value for the water. Every variation in the salt would produce a corresponding variation in the remainder. We might, indeed, get similar remainders for a series of salts of equal hydration, but we could certainly hope for nothing of the kind in comparing compounds with one, two, four, five, six, seven, ten, twelve, and eighteen molecules of water of crystallization.

Now, turning to the hydrates, and studying them just as we have studied ordinary crystallized salts, we find a totally different state of affairs. Instead of getting a constant value for our remainder, we shall get a number of values having no apparent relation to each other. A few instances here and there will illustrate this fact.

I_2O_5, 4·487, Ditte (74·4). I_2O_5,H_2O (HIO_3), 4·269, Ditte (82·4), K_2O, 2·656, Karsten (35·4). K_2O, H_2O(KHO), 2·044, Filhol (54·8). CaO, 3·180, Filhol (17·6). CaO, H_2O, 2·078, Filhol (35·1). SrO, 4·611, Filhol (22·4) SrO, H_2O, 3·625, Filhol (33·5). BaO, 5·456, Filhol (28·0). BaO, H_2O, 4·495, Filhol (38). Mn_2O_3 (braunite), 4·752, Rammelsberg (33·2). Mn_2O_3. H_2O (manganite), 4·335, Rammelsberg (40·6). Fe_2O_3, 5·037, H. Rose (31·7). Fe_2O_3, H_2O (göthite), 4·37, Yorke (40·7). Al_2O_3 (sapphire), 4·0001, Schaffgotsch (25·7). Al_2O_3, H_2O (diaspore), 3·45, J. L. Smith (35·0). B_2O_3, 1·803, Davy (38·8). B_2O_3, $3H_2O$, 1·4347, Stolba (86·4).

Arranging these serially and subtracting, we get the following remainders to represent the water:

In I_2O_5, H_2O,	8·0	In Mn_2O_3, H_2O,	7·4
" K_2O, H_2O,	19·4	" Fe_2O_3, H_2O,	9·0
" CaO, H_2O,	17·5	" Al_2O_3, H_2O,	9·3
" SrO, H_2O,	11·1	" B_2O_3, $3H_2O$,	15·9
" BaO, H_2O,	10·0		

It would be easy to carry this out still farther, but already the want of uniformity is striking enough. One compound, however, is worth noting. The molecular volume of K_2O, H_2O, halved as it ought to be for KHO, is almost the exact mean between the values for ice, 19·6, and K_2O, 35·4. This, of course, is what we should expect.

In the above series two things are noteworthy. First, the lack of uniformity, which indicates (but does not prove) what we would naturally expect, viz., that when an oxide unites with water to form a hydrate, *both* undergo condensation. Secondly, what supports the same idea, that the volumes thus arbitrarily calculated for water average much lower than the values obtained from crystallized salt.

To sum up, the evidence presented in this paper renders the following statement highly probable.

When water unites with an anhydrous substance to become water of crystallization, the water undergoes the entire condensation. When it unites as water of constitution, the condensation is distributed throughout the molecule. The law of this distribution remains to be ascertained.

Art. XXXVIII.—*Warwickite;* by J. Lawrence Smith, Louisville, Ky.

It is several years since Professor Brush and myself, while engaged in the re-examination of American minerals, pointed out the mineral warwickite as possessing a peculiar composition, altogether different from what it had been supposed to have.

The mineral was first described as a new species by Professor Shepard in 1838 (Am. Journ. Sci., vol. xxxiv), and again more fully in 1839 (ibid, vol. xxxvi, p. 313). In both of these descriptions, however, he confounded two very distinct substances, viz: the mineral proper and an impure variety of it, which, while possessing the general crystallographic form, contained but a small portion of the true warwickite; in fact, this impure variety bears about the same relation to the true mineral as the siliceous lime crystals, known as Fontainbleau limestone, does to calcite. The pure variety occurs in small slender crystals detached with difficulty from the gangue; this form was not analyzed by Professor Shepard, for he says that one of the crystals that furnished material for his examination was five centimeters long by one centimeter across, and had no metallic luster, which luster really marks the true warwickite, especially on the cleavage surfaces.

The result of Professor Shepard's analyses were so different from what I have found either in the pure or impure varieties that it is needless to give them here.

Subsequently this mineral was taken up by Professor T. S. Hunt, and from his results he supposed that he had discovered a new mineral, which he called enceladite. But Professor Hunt fell into the same error as Professor Shepard from examining an impure mineral, finding as much as 18 per cent of silica, 14 of alumina and 7 of water. Subsequently Professor Hunt examined a purer specimen and gave as its composition:

Titanic acid	31·5
Magnesia	43·5
Peroxide of iron	8·1
Loss by ignition	2·0

His analysis showed a loss of nearly 20 per cent, which he thought had occurred by an accident to the sand bath during the analysis; and not having any more of the material, he was unable to verify his results.

It was just here that the re-examination of this mineral was taken up, which soon made it evident that the pure mineral had never yet been analyzed. The utmost care was taken in selecting the best specimens of the rock containing the mineral, and then, by mechanical and chemical means, in separating the mineral from the rock; then again in crushing up the small crystals and selecting out the pure little fragments under a strong glass, the pieces usually not being more than a millimeter in size; and thus nearly as much time was consumed in selecting and purifying the mineral as in analyzing it, although the latter was slow work.

When pure warwickite was examined, it was found that one of its most important constituents had been overlooked. But so little of the pure mineral was then at our disposal, and so difficult was it to separate it from the associated minerals, that all that could be arrived at, at that time, was the discovery of over 20 per cent of boracic acid, and the establishment of the fact that warwickite was essentially a borotitanate of magnesia and iron (Am. Journ. Sci., II, xvi, 293).

Since obtaining the above results, I have procured a number of pieces of the rock containing the small crystals, and separated sufficient for analyses in a nearly pure state; although, from the fact that almost microscopic crystals penetrate the crystals of warwickite, it is impossible to separate the last traces of spinel. When the mineral is powdered in the mortar the small particles of spinel will be felt, and with a magnifying glass can be discovered. Notwithstanding these difficulties, I am satisfied that I have made out its composition. Its physical characters have been pretty well described in works on Mineralogy. Its specific gravity as made out by me is 3·362; by Brush, 3·351 small crystals, 3·423 large crystals; and by Damour, 3·355. The luster of the cleavage surface is very bright and characteristic, being of a dark hair-brown or chocolate color. It is very readily cleaved in the direction of the prism.

The results of my analysis are as follows:

		Oxygen.	Ratio.
Boracic acid	27·80	19·06	9
Titanic acid	23·82	10·37	5
Magnesia	36·80	14·46	6
Oxide of iron	7·02	2·10	1
Silica	1·00		
Alumina	2·21=98·65		

The silica and alumina were impurities, the alumina arising from spinel that it had been impossible to separate, and which was combined with a little of the magnesia; and these have been deducted in making out the oxygen ratio. Moreover, the titanic acid obtained in the analysis retained a minute quantity of oxide of iron. After a most careful study of the composition as made out by the above analysis, confirmed by several other partial analyses, I feel warranted in giving the following as the true composition of warwickite:

			Per cent.
$3\dddot{B}$	=	105·	30·57
$2\ddot{Ti}$	=	81·	23·58
$6\dot{Mg}$	=	121·44	35·36
$1\dot{Fe}$	=	36·	10·49
		343·44	100·00

The exact formula by which to express this mineral is not easily given, as we know nothing of compounds containing boracic and titanic acid associated together; the expression I am disposed to adopt is $\dot{Mg}^5\dddot{B}^3 + (\dot{Mg},\dot{Fe})\ddot{Ti}^2$.

I would remark that at the same locality from which the warwickite comes, there occurs a titaniferous spinel containing about 15 per cent of magnesia, as analyzed by Rammelsberg, and would have for its formula

$$\dot{Mg}\ddot{Ti} + \dot{Fe}\ddot{Ti}.$$

Warwickite is the only borotitanate thus far made known.

I am indebted to M. Des Cloiseaux for the following crystallographic account of warwickite. The prisms never are well terminated: they are either orthorhombic or clinorhombic. The forms that Des Cloiseaux has observed are $h^1 m\ g^2 g^1$ and $h^1 h^2 m\ g^2 g^1$. The angles measured by him are: $h^1 h^2 = 162° 5'$; $h^1 m = 135°\ 40'$; $h^1 g^2$ (over m) $= 109°$, $h^1 g^1 = 90°\ 20'–90° 30'$, $m g^1 = 134° 20'–134° 35'$, $g^2 g^1$ (adjacent)$= 161° 20'–161° 25'$, $g^1 h^2$ (over m)$= 108°\ 40'$. From these, M. Des Cloiseaux calculates $mm = 91°\ 20'$ and $88°\ 40'$, $h^1 h^2 = 161°\ 58'$, $h^1 m = 135°\ 40'$, $h^1 g^2 = 108°\ 30'$, $h^1 g^1 = 90°$, $m g^1 = 134°\ 80'$, $g^2 g^1 = 161°\ 10'$, $g^1 h^2 = 108°\ 2'$.

Art. XXXIX.—*Curious association of Garnet, Idocrase and Datolite;* by J. Lawrence Smith, Louisville, Ky.

Specimens of a rock were sent to me some time since from Santa Clara in California, which were found to be composed of four minerals associated together, viz: calcite, which represented the rock of the country from whence it came, and datolite, garnet and idocrase. The *datolite* is colorless and crystalline, without, however, presenting any crystals; sp. grav. 2·988. It is perfectly pure, as shown by the following analysis of a portion from which the calcite was carefully separated:

Silica	38·02
Boracic acid	21·62
Lime	33·87
Water	5·61
	99·12

The association of this mineral with garnet and idocrase is, I believe, now mentioned for the first time.

The *garnet* is the variety cinnamon stone; the crystals are very large and perfect dodecahedrons, some of them being three or four centimeters across. They have a greenish color over the exterior, and are cinnamon-colored within or through the mass of the crystals; sp. grav. 3·59. An analysis of the mineral furnished the following results:

Silica	42·01
Alumina	17·76
Sesquioxide of iron	5·06
Oxide of manganese	·20
Lime	35·01
Magnesia	·13
	100·17

The *idocrase* occurs in compact fibrous crystals of a green color, the crystals themselves not being sufficiently distinct to exhibit the true crystalline form. What is most singular, the idocrase penetrates and permeates the crystals of garnet, losing itself as it were in the garnet; passing off by such insensible shades, that it is impossible to say where the idocrase terminates and the garnet begins. A large crystal of garnet, when cut in two and polished, shows the idocrase penetrating it, like so many green streamlets through the interior. Its specific gravity is 3·445. A portion carefully separated from the garnet gave the following results:

Silica	36·56
Alumina	17·04
Sesquioxide of iron	5·93
Oxide of manganese	·18
Lime	35·94
Magnesia	1·07
Potash	·51
Loss by heat	2·00
	99·23

I know of no locality where the above minerals are associated in the manner described. The fact respecting the garnet and idocrase is especially interesting; for while we find these minerals frequently associated, we have nowhere else found the crystals of the two penetrating and interlacing each other, so as to form between them a uniform mass, yet each mineral retaining its identity.

It can be readily understood how two such minerals as lime, garnet and idocrase may occur in the manner just mentioned, when we consider the formulæ of the two:

Garnet,	$(\frac{1}{2}\dot{R}^3+\frac{1}{2}\overset{\cdots}{R})^2\ddot{Si}^3$
Idocrase,	$(\frac{3}{5}\dot{R}^3+\frac{2}{5}\overset{\cdots}{R})^2\ddot{Si}^3$.

ART. XL.—*On a new method of investigating the Composite Nature of the Electric Discharge;* by ALFRED M MAYER.

IN 1842, Professor Joseph Henry* observed that when a needle was placed in a helix and magnetized by the discharge of a Leyden jar, the direction of the polarity of the needle varied with the "striking distance" of the jar; and these observations led Henry to the discovery that the discharge was multiple, and oscillatory in its nature. In 1862 Feddersen† confirmed Henry's discovery, on examining the nature of the discharge by means of a revolving mirror. Subsequently Rood (in a series of classical researches, published in this Journal in 1869–71–72) studied the multiple character of the discharge of the inductorium by means of rotating discs perforated with narrow radial slits. In 1873 Cazin‡ also investigated the discharge with the rotating disc. The method I have devised leads us directly, by the simplest means, to phenomena which cannot be revealed by either revolving mirror or rotating disc. The first method that occurred to me was to attach a delicate metallic point to a vibrating tuning-fork, and to send the discharge from this point, through lamp-blackened paper, to a revolving metallic cylinder, on which the paper was stretched. We can to some extent analyze the electric discharge, in these conditions, from the series of perforations left in the paper in the trail of the vibrating fork. This method, though beautiful as an illustration, is useless as a means of investigation; for the metal cylinder, the paper and the fork form a species of Leyden jar, which is always in the circuit of the particular discharge whose nature you would investigate. The above method, though original with me, cannot be claimed as my

* Proc. Amer. Phil. Soc.

† Ueber die electrische Flaschenentladung, Pogg. Ann., vol. cxvi, p. 132.

‡ Journal de Physique, vol. ii, p. 252.

own, having recently found that it was devised by Donders,* and has been used in an investigation by Nyland.† To get rid of inductive action in the registering apparatus, I devised the following method: A cylinder is covered with thin printing-paper, and the latter is well blackened by rotating the cylinder over burning camphor. The paper is then removed from the cylinder, and cut into discs of about 15 cm. in diameter. When one of these discs is revolved about 20 times per second, it is rendered very flat by centrifugal action. It can then be brought between points or balls, even when the latter are separated by no more than $\frac{3}{4}$ mm. When in this position, the discharge between the points or balls perforates the disc and leaves a permanent record of its character, of the duration of the whole discharge, and of the intervals separating its constituent flashes and sparks. To obtain the time of rotation of the disc I use the method invented by Young in 1807 (see his Natural Philosophy, vol. i, p. 191). That is, I present momentarily to the rotating disc a delicate point which is attached to a vibrating tuning-fork. The number of vibrations per second of this fork has been determined to the last degree of precision by means of a break-circuit clock, which sends at each second a spark from an inductorium through the fork's sinuous trace on blackened paper, covering a revolving cylinder. The axis of the sinuous line on the disc is traced with a needle point, and then, on drawing radii through symmetrical intersections of this axis on the sinuous line, we divide the disc off into known fractions of time. The disc is now removed from the rotating apparatus and the carbon is fixed by floating the disc for a moment on thin spirit-varnish. When the disc is dry and flat, it is centered on a divided circle, provided with a low power reading-microscope, and the deviation of the whole discharge and the intervals separating its components can be determined to the $\frac{1}{50000}$ of a second.

Many results have been obtained with this apparatus. I defer their publication until I have carefully examined them, and have extended this research with the study, not only of the discharge of the inductorium, but also of the frictional machine, of the Leyden jar and of the Holtz machine, under every condition of charged surface and of striking distance, and when the current is flowing freely over a conductor and when it is doing work. I here present, merely as examples of the value of the method, the results I have obtained in three conditions of experiment.

* Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool, 1868–69.

† Archives Néerlandaises des Sciences exactes et naturelles, t. v, p. 292.

1. *Discharge of large inductorium* between platinum points one mm. apart. No jar in the circuit.*

The platinum electrodes were neatly rounded and formed on wire $\frac{6}{10}$ mm. in diameter. After the discharge through the rotating disc, nothing was visible on it, except a short curve formed of minute, thickly-set white dots; but, on holding the disc between the eye and the light, it was found to be perforated with 33 clean round holes, with the carbon undisturbed around their edges. The portion of the discharge which makes these holes lasts $\frac{1}{23}$ second, and the holes are separated by intervals which gradually decrease in size toward the end of the discharge, so that the last spark-holes are separated about one-half of the distance which separates the holes made at the beginning of the discharge. The *average* interval between the spark-holes is $\frac{1}{750}$ second. After this portion of the discharge has passed there is a period of quiescence lasting about $\frac{1}{1500}$ second; then follows a shower of minute sparks, which forms the short dotted line above spoken of. This spark-shower lasts $\frac{1}{330}$ of a second, and is formed of 30 sparks; hence the average interval separating these sparks is $\frac{1}{9900}$ second. The intervals separating these sparks are, however, not uniform, but are smaller in the middle of the spark-shower than at the beginning and at the end of this phenomenon. The spark-shower, indeed, is a miniature of the phenomenon obtained when a Leyden jar is placed in the circuit of the coil, and which is described below. The above numbers were determined as the average measures on six discs. It is here to be remarked that all of the discharges studied in this paper were made by suddenly depressing the platinum faced "break" of the primary circuit, and the break was held in this position until the disc had been removed from between the points or balls.

2. *Discharge of large inductorium between platinum points one mm. apart, with a Leyden jar of 242 sq. cm. connected with the terminals of the secondary coil.*

After this discharge through the disc a very remarkable appearance is presented, the full description of which I reserve for a more extended paper. The discharge in its path around the disc dissipates little circles of carbon. There are 91 of these circles, each perforated by 4, 3, 2 or 1 holes. I shall here have to adopt a new nomenclature for the description of this complex phenomenon. I call the whole act of discharge of the coil, *the discharge.* Those separate actions which form the little circles by the dissipation of the carbon I denominate *flashes*, and the perforations in these circles I call *sparks*. The discharge in the above experiment lasts $\frac{1}{24}$ of a second. The flashes at the beginning of the discharge are separated by intervals aver-

* The striking distance of this coil between brass points was 45 cm.

aging $\frac{1}{555}$ second up to about the 10th flash; after this the intervals of the flashes rapidly close up, so that during the fourth fifth of the discharge they follow at each $\frac{1}{5882}$ of a second. During the last fifth of the discharge the intervals between the flashes gradually increase, and the last flash is separated from its predecessor by $\frac{1}{1000}$ of a second.

3. *Discharge of large inductorium between brass balls, one cm. in diameter, separated one mm., with a Leyden jar of* 242 *sq. cm. inner coating, connected with the terminals of the secondary coil.*

This discharge also lasts $\frac{1}{24}$ second, and is similar to the preceding, except that larger circles are made on the disc by the dissipation of the carbon, and that there are fewer flashes, viz., 71. The total number of spark-holes in these flashes is 123. Thus, there are fewer flashes than in the experiments with the platinum points, but the total number of spark-holes is the same in each case. Hence there is, on an average, 1·34 spark to each flash with the points, and 1·73 spark-holes to each flash with the balls.

Experiments have also been made with rotating discs formed of "sensitized" paper, and interesting results have been obtained.

October 15, 1874.

ART. XLI.—*On the Periodicity of the Rainfall in the United States in relation to the Periodicity of the Solar Spots;* by Professor JOHN BROCKLESBY, of Trinity College, Hartford, Ct.

THE researches of scientists, especially of late, lead to the conclusion that there is an intimate connection, more or less marked, between the solar disturbances and various terrestrial phenomena. Thus, upon comparing the mean daily range of the magnetic declination, and also the number of auroras observed each year, with the extent of the spots on the solar disk, a striking correspondence is observed in the curves which respectively represent these phenomena. A periodicity in the cyclones of the Indian Ocean and West Indies corresponding with that of the sun-spot area is asserted by Mr. J. N. Lockyer to have been established by Mr. C. Meldrum, Director of the Meteorological Observatory at Mauritius, and it is claimed by Mr. Meldrum and others that the variation in the annual rainfall conforms also to the sun-spot cycle of about eleven years. Various investigations likewise point to the inference that changes in the annual temperature of the atmosphere occur in cycles of ten or eleven years, coincident with those of the solar commotions.

It is proposed in this paper to discuss the alleged connection between the variations in the annual rainfall and the variations in the extent of the solar spots, as regards this continent, so far as we have data for the same.

At the meeting of the British Association last year, Mr. Meldrum, in a paper on the "Periodicity of Cyclones and Rainfall in connection with the Sun-spot Periodicity," remarked as follows: "With the help of the researches of Mr. Lockyer, Mr. Symons and Dr. Jelinek of Vienna, I have now examined ninety-three tables of rainfall for various parts of the world, and I find that, scarcely without exception, more rain falls in maxima than in minima sun-spot years. So far as observations go, Europe, Africa, America, and Australia give very favorable results. Asia is represented, however, only by three stations, and France, which affords as yet only five stations, is the only European country which presents results opposed to the theory."

From the comparatively small number of the tables of rainfall which Mr. Meldrum gives, and from his silence upon the subject, we may, I think, safely conclude that he did not consult Mr. Charles A. Schott's elaborate article on the "Rainfall in the United States," published by the Smithsonian Institution, a work which embraces abstracts of records of aqueous precipitation from about twenty-two hundred stations. The investigations, therefore, of Mr. Meldrum, so far as this country is concerned, may be regarded as incomplete.

As the following discussion is based upon Mr. Schott's tables, it may not be amiss to state briefly in what manner they have been so constructed and arranged that the variations in the annual rainfall throughout the United States admit of ready comparison with the changes in the extent of the solar spots.

From the whole number of stations whose mean annual rainfalls are respectively recorded, Mr. Schott selects one hundred and seven, which, on account of their extent of record, involve the smallest probable errors in their average rainfalls; and he then ascertains the ratio of each annual rainfall to the average yearly rainfall for every one of these stations. The period of record of these stations extends, with greater or less intervals, from 1799 to 1867.

To free these ratios from accidental irregularities and to exhibit the nature of the fluctuations from year to year more distinctly, the author unites them in groups, formed of stations where the annual rainfall appears subject to the same laws. Group I is composed of stations on the Atlantic sea board, extending from Maine to Virginia. Group II comprises the State of New York, and adjacent parts of Canada, New Hampshire, Massachusetts and Vermont; group III includes parts of Iowa, Minnesota, Illinois and Wisconsin; group IV embraces the Ohio Valley, Ohio, Illinois, Indiana, Kentucky and part of Missouri; group V is formed of the Indian Territory and Arkansas; group VI comprises Louisiana, Alabama and West Florida; group VII comprehends the sea-coast from Virginia to Florida, and group VIII includes the sea-coast of California.

In these groups the percentage of the mean amount of rainfall is tabulated for each year of observation, the longest period of record extending from 1804 to 1867.

From the data thus afforded curves are constructed which present to the eye the annual fluctuations of the rainfall over the vast region embraced by these groups. Mr. Schott speaks briefly of the connection between the solar disturbances and the rainfall, stating that the rain curve for 1837–8 shows a decided minimum in precipitation, when there was a marked maximum of solar activity; but that the two phenomena lead to an opposite conclusion about the epoch of 1855–6; a minimum of rainfall then occurring with a minimum extent of sun-spot area. He does not enter into any detailed investigation of this supposed connection.

Under these circumstances, it appears, therefore, desirable, in order to detect what connection, if any, exists between the fluctuations of the annual rainfall and the variations in the extent of the solar spots, that these phenomena should be compared either year by year, or by groups of years; and this it is now proposed to do.

Taking Dr. Wolf's table of the relative extent of sun-spots for each year, within the period from 1804 to 1867 inclusive, in which period the yearly percentage of the average rainfall is also given in Mr. Schott's table of territorial groups, two methods of comparison can be employed if we wish to ascertain whether an annual excess of sun-spot area is attended by an excess of annual rainfall, and *vice versa.* The first mode is to take the average of the numbers representing the relative extent of the spots for the period mentioned, viz: from 1804 to 1867, then to note the years in the period that are above or below the average; and next to place the percentage of the mean annual rainfall opposite each year. We can thus detect whether an excess of sun-spot area in any year is marked by an excess of rain and deficiency of the former by a deficiency of the latter, or whether no such law exists. The second method is that adopted by Mr. Meldrum, who makes the comparison by forming quinquennial sets, taking two years on each side of the maximum year for a maximum set, and two on each side of the minimum year for a minimum set; or where this is not possible, a triennial group is formed.

Proceeding by the first method, it is found that the average sun-spot area for the period extending from 1804 to 1867 inclusive is denoted by the number 38. Taking now each year of this period, the year is printed in heavy or light type, according as the number representing its sun-spot area is greater or less than 38; heavy type indicating an excess and light type a deficiency. Each year is then also marked plus or minus from Mr. Schott's table of territorial groups, according as its rainfall is above or below the mean.

This being done, we have the following results. In group I, comprising the seaboard from Maine to Virginia, there are twenty-nine years in which the sun-spot area is above the average. In seventeen of these the rainfall is in excess, in ten below the average, and in two equal to it. In this group there are also thirty-five years when the sun-spot area is below the average. Out of these there are sixteen years when the rainfall is in excess and nineteen years when it is below the mean.

In group II, comprising the State of New York and adjacent parts of Canada, New Hampshire, Massachusetts and Vermont, there are twenty-five maxima sun-spot years. In fifteen of these the annual rainfall is in excess, in nine below the mean, and in one equal to it. There are also in this group seventeen minima sun-spot years, and in these the rainfall is in excess seven times, below the mean eight times, and equal to it twice.

In group IV, a region embracing the Ohio Valley, Ohio, Indiana, Illinois, Kentucky, and part of Missouri, there are twenty-five years of excess of sun-spot area, and in thirteen of these the rainfall is in excess of the mean, in twelve below it, and in one equal to it. There are also in this group twenty-five years when the extent of sun-spots is below the average, and in nine of these the rainfall is above the mean, in fourteen below the mean, and in two equal to it.

The tables of precipitation belonging to these three groups are regarded by Mr. Schott as tolerably trustworthy, and the results of the comparison show a tendency on the whole toward an excess of rain when there is an excess of sun-spot area, and *vice versa*. Yet we meet here with marked anomalies; for in the period from 1818 to 1826 inclusive, which are years of deficiency in solar activity, six annual rainfalls out of the nine are above the mean; and from 1835 to 1840 inclusive, which are years of excess in the sun-spot area, all the annual rainfalls in the three groups are below the average, except in one instance. Moreover, as we see by the table, years of excess and deficiency of rainfall are found as well in the periods when the sun-spot area is above the mean as in those where it is below it.

Passing now to the other territorial groups, the results from which are to be regarded as only rough approximations toward the truth on account of the insufficiency of the stations, we obtain the following results from all these taken together. In the years when the extent of the spots is in excess, the annual rainfall is above the mean thirty-five times, below it thirty-seven times, and equal to it five times. In the years when the sun-spot area is below the mean the rainfall is below the average thirty-two times, above it twenty times, and equal to it six times.

Taking now all the groups which embrace so large a portion of the United States, we obtain the following results in reference to the subject before us:

	Sun-spot area.	Ratio of the excess of rainfall (above the mean) in deficiency.
From 1804 to 1805 (inclusive),*	above the mean,	: 1
" 1806 to 1815	below " "	6 : 4
" 1816 to 1817	above " "	0 : 2
" 1818 to 1826	below " "	6 : 12
" 1827 to 1831	above " "	2 : 4
" 1832 to 1834	below " "	5 : 7
" 1835 to 1840	above " "	6 : 29
" 1841 to 1845	below " "	12 : 19
" 1846 to 1852	above " "	31 : 18
" 1853 to 1857	below " "	8 : 28
" 1858 to 1864	above " "	31 : 15
" 1865 to 1867	below " "	15 : 3

For the reasons already stated, namely, that groups I, II and IV. are only to be regarded as trustworthy, and that the rest give but rough approximations, the above results must be taken with great allowance. As they stand they are full of anomalies, about half the results favoring and the rest contradicting the law which is supposed to exist between the rainfall and the extent of the sun-spot area.

The results obtained by this first method of comparison are exhibited in the annexed table. (Table I.)

The second mode of investigating any relation which may exist between the annual fluctuations in the extent of the solar spots and the variations in the yearly rainfall, consists in forming groups of years, by taking one or two years on each side of the maximum and minimum year of any sun-spot period. As the mean secular period of the spots is about eleven years, quinquennial groups can generally be formed of maximum and minimum years of sun-spot area. When the period is short, triennial groups can be taken. This is the method adopted by Mr. Meldrum and other distinguished meteorologists.

* In 1804 the rainfall equals the mean also.

TABLE I.

Heavy type marks the years when the sun-spot area is above the average, viz. 38. Light type when below. + marks when the rain fall is above the mean. − when below. = when equal.

	Sea coast, Maine to Virginia.	State of N. York, and adjacent parts of Can., N. H., Mass., and Vt.	Parts of Iowa, Minn., Ill. and Wis.	Ohio Valley, Ohio, Ind., Ky., Ill. and part of Mo.	Indian Territory and Arkansas	Louisiana, Alabama and W. Florida.	Sea coast of Virginia to Florida.	Sea coast of California.				
GROUP.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	+	−		
1804 Max.	=	--	--	--	--	--	--	--	--	0	Total +	Total −
1805	−	--	--	--	--	--	--	--	--	1	--	−1
1806	−	--	--	--	--	--	--	--	--	1	--	---
1807	+	--	--	--	--	--	--	--	1	--	--	---
1808	+	--	--	--	--	--	--	--	1	--	--	---
1809	+	--	--	--	--	--	--	--	1	--	--	---
1810 Min.	−	--	--	--	--	--	--	--	--	1	--	---
1811	−	--	--	--	--	--	--	--	--	1	--	---
1812	+	--	--	--	--	--	--	--	1	--	--	---
1813	+	--	--	--	--	--	--	--	1	--	--	---
1814	+	--	--	--	--	--	--	--	1	--	--	---
1815	−	--	--	--	--	--	--	--	--	1	+6	−4
1816 Max.	−	--	--	--	--	--	--	--	--	1	--	---
1817	−	--	--	--	--	--	--	--	--	1	--	−2
1818	−	--	--	+	--	--	--	--	1	1	--	---
1819	−	--	--	−	--	--	--	--	--	2	--	---
1820	−	--	--	−	--	--	--	--	--	2	--	---
1821	−	--	--	=	--	--	--	--	--	1	--	---
1822	−	--	--	+	--	--	--	--	1	1	--	---
1823 Min.	−	--	--	+	--	--	--	--	1	1	--	---
1824	−	--	--	+	--	--	--	--	1	1	--	---
1825	−	--	--	+	--	--	--	--	1	1	--	---
1826	−	−	--	+	--	--	--	--	1	2	+6	−12
1827	−	+	--	+	--	--	--	--	2	1	--	---
1828	+	+	--	+	--	--	--	--	3	--	--	---
1829	+	+	--	−	--	--	--	--	2	1	--	---
1830 Max.	+	+	--	−	--	--	--	--	2	1	--	---
1831	+	+	--	+	--	−	--	--	3	1	+12	−4
1832	+	+	--	+	--	−	--	--	3	1	--	---
1833 Min.	−	−	--	−	--	+	--	--	1	3	--	---
1834	−	−	--	−	--	+	--	--	1	3	+5	−7
1835	−	−	--	−	--	+	--	--	1	3	--	---
1836	−	−	--	−	--	−	--	--	--	4	--	---
1837 Max.	−	−	−	−	−	−	−	--	--	7	--	---
1838	−	−	+	−	−	−	−	--	1	6	--	---
1839	−	−	−	−	=	−	+	--	1	5	--	---
1840	+	−	−	−	+	−	+	--	3	4	+6	−29
1841	+	=	−	−	+	−	+	--	3	3	--	---
1842	+	+	=	−	+	−	+	--	4	2	--	---
1843 Min.	+	+	=	−	−	−	+	--	3	3	--	---
1844	−	−	−	=	−	−	−	--	--	6	--	---
1845	−	−	−	+	−	+	−	--	2	5	+12	−19
1846	=	−	−	+	−	+	+	--	3	3	--	---
1847	+	−	−	+	+	+	+	--	5	2	--	---
1848 Max.	−	−	+	+	+	+	−	--	4	3	--	---
1849	+	=	+	+	+	−	−	--	4	2	--	---
1850	+	+	+	+	+	−	−	−	5	3	--	---

	Sea coast, Maine to Virginia.	State of N. York, and adjacent parts of Can., N. H., Mass., and Vt.	Parts of Iowa, Minn., Ill. and Wis.	Ohio Valley, Ohio, Ind., Ky., Ill. and part of Mo.	Indian Territory and Arkansas.	Louisiana, Alabama and W. Florida.	Sea coast of Virginia to Florida.	Sea coast of California.				
GROUP.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	+	—	Total +	Total —
1851	+	+	+	+	+	—	—	—	5	3	--	---
1852	+	+	+	—	+	—	=	+	5	2	+31	—18
1853	+	—	=	—	=	—	=	+	2	3	--	---
1854	+	—	—	—	—	—	—	+	2	6	--	---
1855	—	—	—	—	—	—	—	+	1	7	--	---
1856 Min.	—	=	—	—	—	—	—	—	0	7	--	---
1857	+	+	+	—	—	—	—	—	3	5	+8	—28
1858	+	+	+	+	—	—	=	—	4	3	--	---
1859	+	+	=	+	—	+	+	—	5	2	--	---
1860 Max.	+	+	—	+	—	+	—	+	5	3	--	---
1861	+	+	=	+	--	—	+	+	5	1	--	---
1862	+	+	+	—	--	—	--	+	4	2	--	---
1863	+	+	+	—	--	+	--	—	4	2	--	---
1864	+	+	+	—	--	+	--	—	4	2	+31	—15
1865	+	+	+	—	--	+	--	=	4	1	--	---
1866	+	+	+	+	--	+	--	+	6	0	--	---
1867 Min.	+	+	+	—	--	—	+	+	5	2	+15	—3

Proceeding in this manner, maxima and minima sun-spot sets of years are formed from 1808 to 1862 inclusive. Then the sum of the excess or deficiency of the rainfall in respect to its mean, is found for each set of years for the regions comprised in the I, II and IV territorial groups of Mr. Schott's tables. The results obtained are shown in the following table. (Table II.)

In this table we have five maxima sets of years and five minima, as respects the extent of the sun-spots. In the first territorial group, comprising the sea coast from Maine to Virginia, the results in the maxima years conform to the supposed law three times and twice are unconformable. In the minima years they are four times conformable and once unconformable.

In the second territorial group, embracing New York State and the adjacent parts of Canada, Massachusetts and New Hampshire, the results in the maxima years agree with the law twice and disagree twice, and in the minima years they accord with the law twice and are opposed to it once. The observations in this group extend over only four maxima and three minima sets of years.

In the fourth territorial group, comprising the Ohio Valley, Ohio, Indiana, Illinois, Kentucky, and part of Missouri, the observations extend over four maxima and four minima sets of years. The results in the maxima sets conform to the law twice and twice are unconformable, while in the minima sets of years they are in accordance with it three times and are unfavorable once.

TABLE II.

Sum of the excess or deficiency of the annual rainfalls of the groups as compared with the mean annual rainfall and expressed in the percentage of the latter.

Quinquennial and Triennial groups of minimum and maximum years of sun-spot area.	I. Sea coast of Maine to Virginia.	II. State of New York and adjacent parts of Canada, N. H., Mass. and Vt.	IV. Ohio Valley, Ohio, Ind., Ill., Ky., and part of Missouri.	TOTAL.
1808, 1809, 1810 minimum, 1811, 1812	—·02	----	----	—·02
1814, 1815, 1816 maximum **1817, 1818**	—·31	----	----	—·31
1821, 1822, 1823 minimum, 1824, 1825	—·30	----	+·22	—·08
1829, 1830 maximum **1831**	+·27	+·05	—·07	+·25
1832, 1833 minimum, 1834	—·03	—·11	—·08	—·22
1835, 1836, 1837 maximum **1838, 1839**	—·03	—·60	—·55	—1·18
1841, 1842, 1843 minimum, 1844, 1845	+·08	—·08	—·04	—·04
1846, 1847, 1848 maximum **1849, 1850**	+·06	—·05	+·53	+·54
1855, 1856 minimum, 1857	—·02	+·02	—·14	—·14
1858, 1859, 1860 maximum **1861, 1862**	+·41	+·27	+·21	+·89

If we now take the sum of the results, embracing all the three territorial groups, it is seen that in the five maxima sets of years there is an excess of rainfall three times and a deficiency twice, and that in the five minima sets of years there is a deficiency five times, the result being in entire conformity to the supposed law.

Yet amid these results striking anomalies are found, for in the maximum set of years extending from 1814 to 1818, there is a deficiency of 31 per cent of the mean; and from 1835 to 1839, which are maxima years, the three territorial groups all present results below the mean, the first giving −·03, the second, −·60, and the last −·55.

In view of the results obtained from these two modes of investigation, I think we may venture to infer, that so far as trustworthy observations have been made throughout the United States, they point to a connection existing between the variations in the sun-spot area and those of the annual rainfall, the rainfall tending to rise above the mean when the sun-spot area is in excess, and to fall below when there is a deficiency of solar activity.

ART. XLII.—*On Serpentine Pseudomorphs, and other kinds, from the Tilly Foster Iron Mine, Putnam Co., New York;* by JAMES D. DANA. With plates VI and VII.

[Continued from page 381.]

5. *Pseudomorphs after Chondrodite.*

THERE is little of the massive chondrodite of the ore-bed that is not penetrated throughout with serpentine; and much of it is so thoroughly so that it would be difficult to separate pure grains for an analysis, while some of it is wholly serpentine. The chondroditic rock, even that in the mine before its removal, is often so fragile, from the alteration which it has undergone, that it falls to pieces easily when struck with a hammer.

In the change to serpentine the honey-yellow chondrodite becomes pale yellow, then grayish yellow and grayish green, and finally pale bluish green to whitish; while the garnet-red variety, which contains more iron, becomes first reddish brown, some unaltered chondrodite grains being still present, and finally smoky blue to dark green. Much of the imbedded bluish serpentine derived from the alteration of chondrodite has some of the unaltered chondrodite still remaining about the exterior of the mass, while other portions or imbedded masses in the same rock are wholly serpentine.

Among the specimens, one kind is a dark olive-green serpentine marked with pale bluish green spots a sixth to a fourth of an inch across. The spots have a darker green center, and hence look a little like concretions ; but some of them toward one side of the specimen are partly unaltered chondrodite, and thus prove that all are derived from the alteration of this mineral. The chondrodite was disseminated through another mineral which is now the dark green serpentine; and this other mineral may have been granular chlorite. The specimens consist in part of dolomite, and this dolomite is also spotted with the pale serpentine, just as dolomite often is with unaltered chondrodite.

Other specimens are a massive granular dolomite sprinkled with large grains of serpentine, but with some of the grains on one side still chondrodite, either wholly or in part, and, also, with some of the serpentine grains having the crystalline form of the chondrodite, leaving no doubt as to their pseudomorphous character. The specimens are very similar to others from Amity, Orange Co., New York, in the cabinet of Professor Brush.

No mineral in the region is so generally altered as the chondrodite. Its crystals have withstood the change better than the massive kinds because of their hard nature and polished surfaces ; but these, wherever reached by the serpentine, are often penetrated by it, besides being enfeebled in lustre. A covering of chlorite has been no protection, as this was almost as easy of change as the chondrodite: and only the crystals under crystallized dolomite or brucite retain their brilliancy.

6. *Pseudomorphs after Enstatite.*

The large fibrous masses of enstatite often have the exterior, or else one end, changed to a dark green serpentine; and from the serpentine end there is a gradual transition to the unaltered enstatite. Besides these cases of incomplete alteration, other specimens are serpentine throughout. In one of the specimens, the ends of the crystals, consisting of dark green serpentine, project from a lamellar pale green serpentine ; and the latter is also penetrated by the dark green variety in columnar forms. The cleavable massive enstatite has undergone the same change as the fibrous. The green color of the serpentine from both is dark, owing to the amount of iron present; and, when the change is most complete, no trace of the original cleavable or fibrous structure is left.

7. *Pseudomorphs after Hornblende.*

The coarsely crystalline massive hornblende, of greenish-black color, occurs altered to serpentine; part still showing its original crystalline composition, and having the external characters of schiller spar, and other portions, where the change is more complete, being devoid of all structure, and true serpentine.

The serpentine of these pseudomorphs has the same dark green color as that from the enstatite, and thus differs widely from that derived from the alteration of ripidolite.

8. *Pseudomorphs after Biotite.*

The large grayish-black or brownish-black masses of biotite, the plates of which are 3 or 4 inches across, are sometimes changed to a dark green serpentine. The transition from the unaltered part of a plate to the altered sometimes takes place through the intermediate stage of a chlorite—the folia becoming green and inelastic; then, beyond this, they are purely serpentine, and lose finally all traces of the micaceous structure. As the mineral resembles the chlorite in having cleavage joints, though less open, the change has often been limited by divisional plates; and the altered end of a group of plates looks sometimes like a mere juxtaposed mass of serpentine. But the structure and surface lining of the biotite may in some parts be traced into the serpentine.

9. *Pseudomorphs after Dolomite.*

The dolomite of the ore bed is in some parts changed to serpentine of an apple-green color. The specimens of dolomite sprinkled with dark-colored spots of serpentine derived from chondrodite contain some *apple-green* serpentine derived from dolomite—the passage of the dolomite grains into the serpentine being distinct. In other cases imbedded masses of dolomite have the exterior for a fourth or a half of an inch changed to apple-green serpentine, while the interior is unchanged; the cleavage planes of the latter may sometimes be traced a little ways into the former, but for the most part the serpentine has the ordinary massive or structureless character. Such serpentine effervesces for a while in dilute hydrochloric acid, owing to a portion of dolomite still present.

10. *Pseudomorphs after Brucite.*

One of the imbedded masses of dolomite having an exterior of apple-green serpentine—about two by three inches in its dimensions—contains fibrous brucite distributed through a portion of it, and wholly replacing some of it—as more particularly described beyond (p. 453)—and this brucite has participated in the change to serpentine undergone by the dolomite. The fibrous structure of the brucite may here and there be traced into the serpentine.

11. *Rectangular tables or plates after an unknown mineral.*

(1.) *General structure.*—Figure 8, on plate VII, represents, enlarged ten diameters, a small portion (four-tenths of an inch broad) of a pseudomorphous mass of serpentine which is two to three inches thick. Thin green plates are distributed through a grayish white or whitish base. Transverse sections of the thin plates are shown in this figure; their thickness is mostly between a hundredth and a sixth of an inch. The outlines of the sections are generally very exactly rectangular; but some of them have various oblique angles, as shown in figs. 11*b*, *c*, *d*, *e*, and occasionally they are six-sided (fig. 11*a*)—forms which *oblique* sections of rectangular tables would have.

The thin plates are really portions of crystals dissected out or developed in consequence of the existence of cleavage-joints in the original mineral, in the same manner as has been exemplified in ripidolite (p. 381), and in the cubic pseudomorphs (p. 375). For a group of such plates is sometimes united at one end, or elsewhere, into a single thick plate, as illustrated in different parts of figure 8. They are those plates or blocks of a crystal in which the alteration has not gone so far as to obliterate all the color of the original mineral, while the white layers between, consisting of whitish serpentine, are the blocks or portions in which the change was carried to the removal of all coloring matter. Even these green plates show through their interior that they are partly changed to the white serpentine; for, as figure 8 shows, the sections are spotted irregularly with white or greenish white, and sometimes have an even line of white serpentine along the middle, indicating a partial subdivision of the plate.

In the specimens there are a few examples of a projecting solid angle, or solid edge, of a tabular crystal; and one of these is represented enlarged ten times (like fig. 8) in fig. 9. The edges are rectangular. The upper surface, or that parallel to the planes of perfect cleavage, is a little paler in color than the others. Moreover, the surface while smooth and even, is faintly striated in one direction; and the lateral plane toward which these striæ point is horizontally striated. In the lateral surfaces of the same tabular crystal there are white lines or rectangular patches, which are the edges of some of the white serpentine plates in the interior. At the bottom, and also at the top to the right, there are alternations of green and white layers, which correspond to the green and white plates of fig. 8.

The planes in the pseudomorphous mass are often parallel or nearly so through a thickness of a third of an inch, and rarely a half; but generally they are placed at all angles toward one another, and some are bent or contorted. Small portions are massive serpentine.

The condition of another portion of one of the specimens is shown in fig. 10; it represents a polished surface, four-tenths of an inch wide, enlarged about ten times. (To avoid erroneous deductions from the figures, the amount of enlargement should be kept in mind.) The rectangular forms are here shown and also various grades in the pseudomorphous change, from that of dark green serpentine to grayish green, bluish white, and white.

(2.) *Clouded band encircling the plates.*—About the green plates, and nearly parallel to their surfaces, the whitish serpentine is often marked with a clouded band, which deepens in color away from the plate, as illustrated toward the left and right sides of fig. 8 and also in fig. 10. The color of these bands is sometimes as dark as that of the green plates; but it is generally paler; and sometimes of a faint bluish shade, as shown in the whitish areas of fig. 10. A portion of one of the dissected plates with its encircling clouded band of green is shown, enlarged 10 times, in fig. 12; the color in this case was dark green, as represented.

(3.) *Air-cells.*—Another remarkable feature of these pseudomorphs is the existence of air-cells in great numbers in the whitish serpentine, alongside of many of the plates. They are sometimes directly in contact with the green plates, and then may be seen through them when a flat surface is exposed, as in one of the upper plates of fig. 9. Fig. 13 represents such a surface uncovered, magnified 10 times. They are often in considerable numbers along the outer edge of the clouded band just described. In fig. 12, the outside dots are the positions of these microscopic air-cells. The margin of the clouded band in the right half of fig 8 has numerous air-cells in the specimen. Owing to the great numbers of these minute air-cells, the pseudomorphous mass is light, *about a twelfth of the mass being air.* According to trials made for me by Mr. George W. Hawes, assistant in the Mineralogical Department of the Sheffield Scientific School of Yale College, the specific gravity before the included air is expelled is 2·30, and, after its expulsion by simple boiling in water, 2·48.

(4.) *Composition.*—The pseudomorphous mass is serpentine, both the green portions and the white, as shown through an analysis by Mr. Hawes, who has given me the following statement of his results.

"The analysis, made on an equal mixture of the green and white, afforded—

SiO_2	41·80
Al_2O_3	0·95
FeO	4·65
MgO	38·55
HO	13·95
	99·90

The oxygen ratio for $\dot{R}$, $\ddot{Si}$, $\dot{H}$, is 3 : 4 : 2·3. The materials of both the green and white portions is, therefore, serpentine. The difference in color is occasioned by the presence of iron in greater quantity in the green, as is shown by igniting fragments, when the green turns black and the white is unaltered; the green gives a deep iron color to the borax bead and the white only a faint iron reaction. The solution of the different minerals points to the same difference."

(5.) *Conclusions.*—The following conclusions flow from the facts that have been stated.

1. The original mineral was crystallized in tabular rectangular forms, and, as the striæ and angles show, was orthorhombic.

2. The tabular crystals were half an inch and less in thickness, and probably were imbedded in an uncrystallized mass of the same composition.

3. Cleavage was very easy parallel to one of the planes, the basal plane of fig. 9, and much less so parallel to one or both of the others; and as the subdivision into blocks or plates by pseudomorphism shows, there were many cleavage-joints.

4. The color was dark green or greenish black; but perhaps paler on the basal plane.

5. The crystals had considerable luster, some still remaining on the striated surface of the green plates.

6. The specific gravity was low.

7. The mineral contained some protoxide of iron; the clouded bands that border or encircle some of the plates have been produced through the escape of coloring matter from the green plates while the alteration was in progress; and this coloring matter contained iron. Moreover, the various serpentine pseudomorphs of the Tilly Foster Iron Mine are never dark green, except when the original mineral contained considerable iron.

8. It contained also some material that escaped as a gas—as the numerous air-cells alongside of the plates and the clouded bands show. The air-cells are in the white serpentine and not often in the green. There are no facts in connection with pseudomorphs after chondrodite (a fluorine-bearing mineral), or after dolomite (a carbonate), to indicate that either fluorine or carbonic acid was the gas that escaped.

9. The mass of the original mineral was, to a large extent, pressed out of shape, while softened during the pseudomorphic process, thereby displacing the plates, and in some parts contorting them and producing much confusion.

10. The white serpentine often occupies more space than the original block that was changed into it; for the green plates have sometimes been diverged by it, or pressed apart. This may have been due to the escaping gas alluded to, swelling up the white serpentine.

11. As to the original mineral, the only additional remark I now venture to make is that probably it was not any known mineral species. Chrysolite is orthorhombic; and has cleavage in one direction rather distinct, becoming more so and opening as alteration advances; and also it is green and contains protoxide of iron—many characteristics in favor of its being the original mineral. But it has no ingredient that could have produced the air-cells by its escape; and no observed facts authorize the opinion that its crystals and crystalline masses could have been resolved into tables like those in the pseudomorph here described.

Anhydrite is orthorhombic and has a perfect cleavage in one direction, but it has good cleavage in other directions also; and it contains no iron and no ingredient to escape as a gas.

B. Pseudomorphs consisting of Brucite or Hydrate of Magnesia.

12. *Brucite Pseudomorphs* (?) *after Dolomite.*

Brucite is sometimes found making a thin covering over the exterior of the masses of chondroditic rock, like the serpentine, and also constituting plates in fissures an inch or so thick, in the interior of which there are occasionally good crystallizations of the mineral. Such facts show that it is there a secondary product, resulting from the alteration of some magnesian mineral in the ore-bed, for it is not among the constituents of the rock-masses; and also that it was formed cotemporaneously with the serpentine.

It is also found, as has been stated, constituting with dolomite the filling of veins in the ore-bed, the filling being sometimes part dolomite and part foliated brucite, one interpenetrating the other. In such cases both minerals cover splendent crystallizations of chondrodite, and each contains isolated crystals of the same mineral equally splendent. The brucite effervesces more or less, owing to the presence in it of some dolomite.

One of the specimens, alluded to on page 449, is an imbedded mass made up of dolomite and a fibrous mineral which is probably brucite; much of the dolomite is penetrated with the brucite; moreover, as stated on the page referred to, both the dolomite and brucite are altered, over the exterior of the mass, to serpentine. The fibers of the fibrous surfaces cross at angles of 60° and 120°, and minute stellar forms occur in some parts of the dolomite. The fact that the fibrous mineral is essentially brucite, as suggested by its crystallization, is sustained by a partial chemical analysis made by Mr. G. W. Hawes. He found it to contain a trace of silica, arising probably, he suggests, from mixture with a little serpentine. The rest was soluble in boiling acid, though with more difficulty than ordinary brucite.

The above observations lead to the following conclusions. (1) That the brucite in the veins of the ore-bed was made out of dolomite, one of the constituents of the ore-bed; but (2) that its production took place at the time of the crystallization of the dolomite of the same veins, and while chondrodite was in process of crystallization, and probably during the period of metamorphism. Hence the case is not one of *true* pseudomorphism, that is, of alteration of crystals of dolomite to brucite, yet still the brucite was formed out of the dolomite of the enclosing rock.

(3) For the brucite associated with serpentine over the blocks of the chondroditic rock of the ore-bed, the origin was of later date, the epoch of the serpentine changes, but probably the same in kind.

The change of dolomite to brucite, under the action of heat and moisture, or heated mineral solutions, loses part of its extraordinary aspect, when it is remembered that, by Pattinson's process for obtaining magnesia, the subjection of dolomite to heat *separates the carbonic acid from the magnesia portion*, leaving the carbonate of lime intact, and then the magnesia is removed by carbonated waters.

C. Magnetite Pseudomorphs.

13. *Magnetite Pseudomorphs after Dolomite.*

Dolomite often occurs in the cavities or veins of the ore-bed in groups of rhombohedral crystals, which are often of large size; these crystals are more or less completely changed to magnetite. Among the crystals occurring together on a single specimen, some are frequently all magnetite; others so only at surface, or along an edge, or an angle, or one side; while others are wholly unaltered. Crystals having a magnetite exterior are occasionally two to three inches in their dimensions.

The change to magnetite took place by removal and substitution. The unaltered dolomite crystals of a specimen have always an eroded surface, and generally look as if they had been much reduced in size by a removal of the exterior. The change evidently began in each case at the surface, but not in all cases over all the surface planes at once; and sometimes it extended down into a crystal along rifts. The magnetite of a rhombohedral face shows its luster, as the crystal is turned to the light, successively in one large patch after another; thus indicating that the deposition began at different points and spread laterally; the group formed from each such center having the crystalline grains in like position, and therefore being simultaneous in their reflection of light to the eye.

These magnetite pseudomorphs are often buried under serpentine, while at the same time the serpentine has sometimes a crust of dolomite. Hence their formation was succeeded, sooner or later, by other chemical depositions, that of serpentine, and afterward that of dolomite.

14. *Magnetite Pseudomorphs after Chondrodite and other Minerals.*

On specimens similar in character to those affording magnetite pseudomorphs after dolomite, there are implanted crystals of chondrodite, some of which have a thin coating of magnetite, and others in which this coating is relatively thick; while the surfaces of chondrodite exposed to view are very rough, as if eroded. There appears to be little doubt that, as in the preceding case, there was a removal of the exterior of the crystals while the deposition of the magnetite was going forward. All improbability of such a removal is taken away by the occurrence of pseudomorphs of *dolomite after chondrodite* (p. 456)

There is abundant evidence that magnetite was freely deposited during the progress of the changes that have been above described.

The whitish and greenish serpentine derived from the alteration of chlorite and other minerals has sometimes a black or blue-black color, owing to the thick dissemination of magnetite in minute grains; and the serpentine pseudomorphs after chlorite might, in some cases, be rightly called serpentine-and-magnetite pseudomorphs; and so for some other serpentine pseudomorphs. By pulverizing this black serpentine and applying a magnet, Professor Brush proved that the color was due to magnetite. A thin section of the black serpentine showed under the microscope that it was simply a distribution of black grains in serpentine, some white translucent spots of serpentine appearing in the black area, and the grains were extremely minute. The cubic pseudomorph described on page 375 has over it spots of blue-black which are of the same origin. One large crystal of chondrodite (1·8 inches long), coated with magnetite, was covered with some of this black serpentine, and probably its own magnetite and that of the serpentine were deposited at the same time. In other cases of such secondary deposits, besides the magnetite in fine grains there are distinct magnetite crystals, and even those of large size.

Ebelmen has shown * that when carbonate of lime is brought into contact with a silicate of iron heated to fusion, magnetite is deposited. At the Tilly Foster iron mine, carbonate of lime-

* Comptes Rendus, xxxiii, 555.

and-magnesia (dolomite), and solutions containing iron silicates, were together, and in all probability at a very high temperature. Hence we may reasonably conclude that under such circumstances the magnetite was deposited.

D. Pseudomorphs consisting of Pyrrhotite, or the Sulphide of Iron FeS.

15. *Pyrrhotite Pseudomorphs after Serpentine.*

The thin plates in the pseudomorphs, No. 11 (p. 450), sometimes consist, in part of the specimen, of pyrrhotite instead of serpentine, and the pyrrhotite was substituted, not for the original mineral of the plates, but for the serpentine of which those plates now consist; for the plates were first made with their intervening layers of white serpentine before pseudomorphism took place. Rifts in the white serpentine of the region are occasionally occupied by pyrrhotite—a fact that proves it to have been one of the later products. The alteration of the green plates, and not of the intervening white serpentine, may have been owing to the presence in the plates still of some iron.

E. Pseudomorphs consisting of Dolomite.

16. *Dolomite Pseudomorphs after Chondrodite.*

Implanted crystals of chondrodite occur sometimes with an exterior or crust of dolomite; and on the same specimen other crystals, of the form of the chondrodite, consist wholly of dolomite. The chondrodite is rough and looks as if eroded; and the complete substitution of dolomite for it proves that the deposition of dolomite took place concurrently with the removal of the chondrodite. The crystals that have a coat of dolomite, and those that are all dolomite, have the same size, which proves that the deposition of the crust was attended with a removal of chondrodite, and probably an equal bulk of it. The dolomite was chemically examined and proved to be this species by Professor Allen. It is a concretionary kind, such as is found as a crust over the serpentine of some specimens.

Crystals of chondrodite coated with magnetite (No. 14) occur on the same specimens with these dolomite pseudomorphs after chondrodite; and the corroding and removal of the chondrodite was probably effected in the case of both by the same chemical agent.

4.—Conclusions and Recapitulation.

The facts are here recapitulated, partly that they may be convenient for consideration by the chemist, and with the hope that they may bring out an explanation of the chemical conditions by which the changes were brought about.

1. *Changes during the epoch of Serpentine formation.*

1. All the common minerals of the ore-bed, excepting the magnetite, that is, the *chondrodite*, *enstatite*, *hornblende*, *ripidolite*, *massive chlorite*, *dolomite*, *biotite*, and also the uncommon kinds, *apatite* and *calcite*, and two other species yet undetermined, occur changed to serpentine. The diversity of the minerals thus acted on gives augmented force to the remark in the opening part of this memoir, that the ore-bed had apparently been steeped in heated solutions or vapors; and that the moisture or vapors had great dissolving as well as decomposing and recomposing power.

2. The vast amount of fracturing undergone by the rocks of the ore-bed indicates a source for all the heat required for the various chemical changes—even if it were over 1,000° F.—in the transformation of motion into heat; and if this was the source, the epoch of serpentine production and pseudomorphism set in immediately upon the fracturing.

3. The reasons for so extensive *magnesian* changes in the ore-bed are the following: 1. The fact that the mineral constituents—the magnetite and the traces of sulphides and apatite excluded—are all *magnesian minerals*. 2. The fact that chondrodite is the predominant species—it being (1) a mineral that is easily decomposed by acids, and (2) one that would have yielded up fluorine to increase the decomposing power of the working moisture; and (3) one that is extremely brittle, in consequence of which it became broken into bits, and thus was opened throughout to the vapors set in action. The chondrodite is the constituent of the ore-bed most extensively altered; that next, the ripidolite and the massive chlorite; the next, the enstatite and hornblende; last, the dolomite.

4. The serpentine derived from the ripidolite is usually white to pale gray-green in color, partly because the amount of iron present is not large, and also because the loose aggregation of the folia gave a chance for percolating waters to drain off the iron; that from the dolomite is apple-green or gray-green; that from the pale chondrodite grayish; but that from most chondrodite bluish-green to smoky blue; that from the granular chlorite and from the enstatite, hornblende and biotite—all eminently iron-bearing species—dark olive-green.

5. The production of the serpentine was accompanied by the production of magnetite in grains and crystals (p. 454); of dolomite in crusts and crystals; of brucite in coatings and crystallized plates filling fissures (p. 453). It was accompanied also in some places by the removal, through solution, of chondrodite and dolomite, and the concurrent substitution of magnetite (p. 455), or by the solution of chondrodite and the concurrent substitution of dolomite (p. 456).

6. The era occupied by the serpentine-making and the accompanying changes may have been long; and yet there are no facts that definitely prove this. The great extent of the change suggests that it was long. But the successive depositions of serpentine and other such changes may have been effected without any great lapse of time. An example is afforded by the specimen containing the cubic pseudomorphs (fig. 1), it having serpentine of two separate times of formations, and affording evidence of the deposition of the cubic mineral between the two. In other specimens dolomite crystals are covered with (or converted into) magnetite; then are overlaid by a layer of serpentine; then by crystals or a crust of dolomite; and this by a second deposit of serpentine: yet these and other like cases prove nothing as to whether the time elapsed was long or not in a geological sense.

7. In the serpentine pseudomorphs after minerals having *cleavage-joints*, the pseudomorphism usually went forward by blocks bounded by the cleavage-joints. The change, begun in any point, spread as far as the first cleavage-joint, and there the thin stratum of air encountered proved to some extent a barrier to farther progress so that the block was finished out complete before proceeding to the next: as in the cubic pseudomorphs (p. 375), chlorite (p. 381), biotite, and the mineral described on p. 450. Hence side by side positions of altered and wholly unaltered portions are common in such minerals. This law must be a general one for pseudomorphic changes.

This fact of alteration by blocks or layers may have some bearing on the origin of the masses of interlaminated serpentine and carbonate of lime which go under the name of *Eozoon*, that is, if they are not of organic origin.

8. The facts at the Tilly Foster iron-mine afford an example of the production of serpentine, not by the help of the magnesian waters of the ocean or springs, but through the alteration of magnesian minerals. It is true that the serpentine of the fissures is not all of it properly pseudomorphous, since part is a deposit at a distance from the crystal or crystalline mass that afforded it. But it is all a result of the alteration of magnesian minerals.

2. *Changes during the epoch of Metamorphism.*

The crystallization of the chondrodite, and of most of the ripidolite, magnetite and dolomite, belongs (1) to the period (pre-Silurian) when the Archæan rocks, the ore-bed included, were crystallized or rendered metamorphic; and (2) to the time of formation of the small intersecting veins, which occurred probably during the epoch of metamorphism.

The chondrodite crystals implanted on the walls of the veins are usually accompanied by implanted crystals also of ripidolite and magnetite, and sometimes by those of dolomite. The chondrodite, ripidolite and magnetite of the veins are often overlaid by a crystallization of dolomite afterward introduced (perhaps immediately afterward), and in places by that of brucite. But crystals of ripidolite sometimes overlie chondrodite, and rarely are imbedded in a crystal of chondrodite; brilliant crystals of chondrodite occur *isolated* in the dolomite and in the brucite; and grains and crystals of magnetite are often imbedded in the dolomite: showing that all these minerals were made at one epoch, and together. The crystallization of most, but not all, of the chondrodite of a vein preceded the deposition of the accompanying dolomite.

3. *Pseudomorphism not "Envelopment."*

One single writer on pseudomorphism, Prof. T. Sterry Hunt, speaks of most pseudomorphous silicates among minerals, the serpentine pseudomorphs especially, as cases of "envelopment" that is, mixtures made by contemporaneous crystallization—a term borrowed from Delesse, but employed with a changed use The Tilly Foster iron-mine, like other localities, affords serpentine-pseudomorphs after chondrodite, chlorite, and other minerals that are *pure* serpentine; which, therefore, envelop nothing and are enveloped of nothing: and to which, therefore, the idea of "envelopment" has no application. They are manifestly the final results in a change to serpentine, the steps of which are illustrated, and admirably so, by various intermediate specimens.

ART. XLIII.—*On the age of the Lignitic formation of the Rocky Mountain region;* by Mr. F. B. MEEK.

[On this important question in American geology, Mr. Meek has presented a learned discussion in Hayden's Report for 1872, from which we cite the following facts.—EDS.]

According to the observations of Mr. Meek, in each of the territories, Utah, Wyoming and Colorado, specimens of *Inoceramus problematicus* occur at different levels in the Coal formation; near Bear River, Wyoming, a bed above the principal coal bed is full of good specimens; at Coalville the shells occur both below and above the main coal bed, through a range of beds having in all a thickness of 4,680 feet; all but 400 feet of this series lie above the great coal bed. Moreover, none of the specimens of Inoceramus—which are mostly casts—bear any evidence of having been washed out and transferred from an older formation. Besides *Inoceramus* at different levels, there is a species of *Gyrodes*, *G. depressa*, over 400 feet above the same coal bed near Coalville; and also, in other localities, a species of *Anchura* and one of *Cyprimera*, Cretaceous genera.

With regard to the beds of Bitter Creek (a small tributary of Green River in Wyoming), from Black Butte northwestward to Salt Wells Station, on the Union Pacific Railroad, Mr. Meek is less positive. His conclusions are given in the following paragraphs here cited from his paper.

"The entire absence among the fossils yet known from this formation of *Baculites*, *Scaphites*, *Ancyloceras*, *Ptychoceras*, *Ammonites*, *Gyrodes*, *Anchura*, *Inoceramus*, and all of the other long list of genera characteristic of the Cretaceous, or in part also extending into older rocks, certainly leaves its molluscan fauna with a strong Tertiary facies. Nor can we quite satisfactorily explain this away on the ground that the water in which this series of rocks was deposited partook too much of the character of that of an estuary, to have permitted the existence of any of these marine genera, because we do find in it the genus *Ostrea*, *Anomia*, and *Modiola*, which probably required water salt enough to have permitted the existence of *Inoceramus*, *Anchura*, and *Gyrodes*, if not of some or all of the genera mentioned above. Indeed, at Coalville, we find *Inoceramus* associated with some brackish-water types, and the additional Cretaceous genera, *Cyprimera*, *Anchura*, *Gyrodes*, &c., in closely-associated beds.

When we come to consider the invertebrate fossils yet known from this formation, in their specific relations, we find all, with possibly two or three exceptions, new to science and different from those yet found either at Bear River, Coalville, or indeed elsewhere in any established horizon; so that we can scarcely more than conjecture, from their specific affinities to known forms, as to the probable age of the rocks in which we find them. Considered in this respect, their evidence, however, is conflicting. Two of the species of *Corbula*, for instance (*C. tropidophora* and *C. undifera*), are most similar to species found in the brackish-water beds, at the mouth of Judith River on the Upper Missouri, that we have always considered Lower Tertiary; though there are some reasons for suspecting that they may be Upper Cretaceous. A *Corbicula*, both from the Black Butte and Point of Rocks localities, is even so *very* nearly like *C. cytheriformis* from the Judith River beds, that I have referred it doubtfully to that species.

Again, the species *Anomia gryphorhynchus*, found so abundantly at Point of Rocks in the same bed with the above-mentioned *Corbicula* and *Corbula trophidophora*, so closely resembles a Texas Cretaceous shell described by Roemer under the name *Ostrea anomiæformis*, that I am strongly inclined to suspect they may be the same; though whether identical or not, at least our shell is certainly not an oyster, as it has its muscular and cartilage scars precisely as in *Anomia*, while its beak is never marginal, and it has no ligament area. * * * *

On the other hand, the Corbiculas are decidedly Tertiary in their specific affinities, as well as in their subgeneric; *C. fracta*, for instance, and *C. crassatelliformis*, from the Hallville mines, being very closely allied to Paris Basin Tertiary forms, the first-mentioned species being the type of a sub-genus, so far as known, peculiar to the Tertiary elsewhere. The same may be said of *C. cytheriformis*, which also seems to belong to a group (*Veloritina*) peculiar to the Tertiary in Europe.

But the most surprising fact to me, supposing this to be a Cretaceous formation, is, that we found directly associated with the reptilian remains at Black Butte a shell I cannot distinguish from *Viviparus trochiformis*, originally described from the Lignitic formation at Fort Clark, on the Upper Missouri, a formation that has always been regarded as Tertiary by all who have studied its fossils, both animal and vegetable. The specimen mentioned does not show the aperture, nor all of the body volution; but, as far as can be seen, it agrees so exactly with that very peculiar species in size, the form and proportions of its volutions, the slope of its spire, its surface markings, the nature of its suture, and, in fact, in every respect so far as can be seen, that I have scarcely any doubt of its identity with the same.

The occurrence of this last-mentioned species here, along with a Cretaceous type of reptilian, and a *Corbicula* apparently identical with *C. cytheriformis* of the Judith River brackish-water beds, together with the presence of Corbulas very closely allied to Judith River species, at lower horizons in this series, and the occurrence of some vertebrates of Cretaceous affinities at the Judith River localities, would certainly strongly favor the conclusion. not only that this Judith formation, the age of which has so long been in doubt, is also Cretaceous; but that even the higher fresh-water Lignite formation at Fort Clark and other Upper Missouri localities may also be Upper Cretaceous instead of Lower Tertiary.

That the Judith River beds may be Cretaceous, I am, in the light of all now known of the geology of this great internal region of the continent, rather inclined to believe. But it would take very strong evidence to convince me that the higher fresh-water Lignite series of the Upper Missouri is more ancient than the Lower Eocene. That they are not is certainly strongly indicated, not only by the modern affinities of their molluscan remains, but also by the state of the preservation of the latter. Indeed, these shells (*Planorbis*, *Viviparus*, *Goniobasis*, *Physa*, &c.) are found loose, as they fall from the incoherent sand in great numbers, so entirely free from adhering matrix, either internal or external, and so little changed, that any one not familiar with the existing species of the country would naturally think them merely dead shells of the same, picked up along the shores of the streams. The entire flora of this Upper Missouri Lignite group has also always been considered, by the highest authorities on that department of paleontology, unquestionably Tertiary.

From the foregoing remarks it will be seen that our present information in regard to the age of the Bitter Creek series may be summarily stated as follows:

1. That it is conformable to an extensive fresh-water Tertiary formation above, from which it does not differ materially in lithological characters, excepting in containing numerous beds and seams of coal.

2. That it seems also to be conformable to a somewhat differently composed group of strata (1,000 feet, or possibly much more in thickness) below, apparently containing little if any coal, and believed to be of Cretaceous age.

3. That it shows no essential difference of lithological characters from the Cretaceous coal-bearing rocks at Bear River and Coalville.

4. That its entire group of vegetable remains (as determined by Professor Lesquereux) presents exclusively and decidedly Tertiary affinities, excepting one peculiar marine plant (*Halymenites*), which also occurs thousands of feet beneath undoubted Cretaceous fossils, at Coalville, in Utah.

5. That all of its animal remains yet known are specifically different from any of those hitherto found in any of the other formations of this region, or, with perhaps two, or possibly three exceptions, elsewhere.

6. That all of its known invertebrate remains are mollusks, consisting of about thirteen species and varieties of marine, brackish, and fresh-water types, none of which belong to genera *peculiar* to the Cretaceous or any older rocks, but all to such genera as are alike common to the Cretaceous, Tertiary, and present epochs, with possibly the exception of *Goniobasis* (which is not yet certainly known from the Cretaceous).

7. That, on the one hand, two or three of its species belong to sections or subgenera (*Leptesthes* and *Veloritina*) apparently characteristic of the Eocene Tertiary of Europe, and are even very closely allied to species of that age found in the Paris Basin; while, on the other hand, one species seems to be conspecific with, and two congeneric with (and closely related specifically to), forms found in brackish-water beds on the Upper Missouri, containing vertebrate remains most nearly allied to types hitherto deemed characteristic of the Cretaceous.

8. That one species of *Anomia* found in it is very similar to a Texas Cretaceous shell, and perhaps specifically identical with it; while a *Viviparus*, found in one of the upper beds, is almost certainly identical with the *V. trochiformis* of the fresh-water Lignite formation of the Upper Missouri; a formation that has always and by all authorities, been considered Tertiary.

9. That the only vertebrate remains yet found in it are those of a large reptilian (occurring in direct association with the *Viviparus* mentioned above), which, according to Professor Cope, is a decidedly Cretaceous type, being, as he states, a huge Dinosaurian.

It thus becomes manifest that the paleontological evidence bearing on the question of the age of this formation, so far as yet known, is of a very conflicting nature; though aside from the Dinosaurian, the organic remains favor the conclusion that it is Tertiary. The testimony of the plants, however, on this point, although they doubtless represent what would be in Europe considered clearly a Tertiary flora, is weakened by the fact that we already know that there is in Nebraska, in clearly Cretaceous rocks, a flora that was referred by the highest European authority to the Miocene. I do not know, however, how far Professor Lesquereux's opinion, that the Bitter Creek plants are Tertiary, may rest upon specific identifications among them of forms known to occur in well determined Tertiary rocks elsewhere."

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Magnetic Equivalent of Heat.*—M. Cazin has been investigating the heat produced in the core of an electro-magnet when the current is alternately made and broken. Call m the induced magnetism, l the distance between the poles, Q the number of units of heat created by the disppearance of the magnetism, and A a coefficient which is constant for the same coil and current. Then $AQ=m^2l$, in which, if all the magnetism is converted into heat, A will measure the number of units of magnetic energy equivalent to a unit of heat. To see if A is constant, the core of the magnet is surrounded by two coils and the current made and broken in one; A then varies according to the method of closing the secondary circuit. If wholly closed, A has its greatest value. If closed while the primary is open, it is less; and if closed while the primary is closed, A has the same value as if the secondary is wholly open. Hence the induction in the secondary circuit occasions a division of the heat.

A alters with the change in duration of the spark of rupture, by forming it in air, water, alcohol, ether, &c. This is explained by the current induced in the magnetizing coil in which part of the heat is found. A diminishes by connecting the point and mercury with the surfaces of a condenser, and also by increasing the number of convolutions.

From these facts it follows that to measure the magnetic equivalent of heat, we must use a bobbin of stout and short wire to reduce the induction to a minimum, employ a weak current, and break it in a very resisting medium, that the circuit may be broken in as short a time as possible.

The differential thermo-magnetic apparatus described, Comptes Rendus, lxxviii, 845, serves to measure the increased pressure of a mass of air enclosed in the magnetized core after the circuit has been broken N times. The temperature of the air is the same as that of the inner surface of the core. This is thin and surrounded with a thick layer of cotton, and hence the amount of heat generated is found by multiplying the increase of temperature, by the weight of iron, by its specific heat. In an example the coil had 480 turns, the core weighed 832·5 grams, the current =·0232, the unit being that required to set free 1 mgr. of hydrogen a second, and the current was interrupted 2778 times in ten minutes. The pressure of the air increased 17·8 mm. of water, corresponding to a change of temperature of 0·508. The amount of heat Q generated was ·0000174 units, and the value of m^2l was 1855· the unit being the amount requisite to repel an equal amount at distance of a decimeter with a force of a decigram. This gives the magnetic equivalent of heat=106,000,000 units of magnetic energy, approximately. Its true value is probably somewhat less since we have not here taken account of the heat transported by induction into the magnetizing coil, but have only taken care that it should be small.—*Comptes Rendus*, lxxix, 290. E. C. P.

2. *Unilateral Conductivity.*—Dr. A. SCHUSTER finds that certain conductors oppose different resistance when the current passes through them in opposite directions. It would be difficult to detect such differences in the ordinary way, as it would be masked by changes of temperature and of the current. If we use, however, the current induced by a moving magnet, we may be sure that it is always constant, as long as the strength of the magnet does not vary, and its motion is the same when approached as when receding. A magnet is made to rotate rapidly in a coil of wire and thus induces currents alternately in opposite directions. The electromotive force producing these currents must be the same in both directions; hence any difference in the two sets of currents must be due to a difference in the resistance. The induced currents move a galvanometer in opposite directions, but if they follow each other with great rapidity the needle will come to rest at the zero, provided the currents have equal strengths. If the two currents are strong, a very slight difference will have a marked effect on the needle.

The magnet was attached to the plate of a sirene and revolved about 40 times a second; the wire of the coil had a resistance of about 30 mercury units, and the electro-motive force in each direction was about ·12 of a Grove cell. The total resistance, including the galvanometer, was about 2500 units. To measure the deflection of the needle, a mirror, telescope and scale were used, and the delicacy of the galvanometer was such that an electro-motive force of ·0005 of a Daniel cell produced a deflection of 200 divisions of the scale. If now the wire to be tested was connected with the galvanometer and coil, and the magnet set in motion, generally the needle would begin to deviate to one side and finally come to rest at some distance from the zero. Changing the connections generally changed the effect. Repeating the experiment several times generally diminishes the deviation or causes it to disappear, but allowing the apparatus to rest for some hours or days brought it back. Inserting a wire which had never before been subjected to a current, drove the needle wildly to one side, although its resistance was but ·1 of a unit. Suppose the circuit produces no deflection, and a wire is introduced which deviates the needle; make this deviation disappear by repeating the experiment, and then remove the wire, when the deflection will reappear. If again made to disappear and the wire once more inserted, it will now be quite neutral, producing no deflection.

The only explanation offered to account for these phenomena is the following. Suppose a spark passed from a sphere to a point; it is known that the electro-motive force required is different according to the direction of the current. A circuit composed of a wire terminating in a sphere and separated from another wire by a thin layer of air, would therefore show unilateral conductivity, the current passing in one direction more easily than in the other. Metals condense gases in great quantity at their surfaces, and it is quite conceivable that if two wires are screwed together, that particles of air will separate the two surfaces and that a small voltaic arc will be the result.—*Phil. Mag.*, xlviii, 251. E. C. P.

3. *Specific Heat of Gases.*—WIEDEMANN has succeeded in measuring the specific heat of gases, by a method which gives results as accurate as those of Regnault, while the apparatus employed is much smaller and less complicated. Two glass vessels are used, one of which may be filled with water from a reservoir ten feet above and the air driven from it into the other vessel which contains a rubber bag with a capacity of 25 litres. This contains the gas to be studied, any portion of which may be expelled from the bag by admitting water from the reservoir. Its volume is obtained with great accuracy by weighing the water collected and correcting for temperature and pressure.

The apparatus for heating consists of a tube, 3 meters long and 9 centimeters in diameter, filled with copper turnings and immersed in boiling water. When the gas traverses this tube it is completely heated, even when 10 litres pass per minute. The calorimeter consists of three silver tubes, 63 mm. high and 9 mm. in diameter, filled with silver turnings and which the gas traverses successively. They are contained in a silvered copper vessel, 54 mm. high and 44 mm. in diameter, filled with water. To avoid radiation, the whole is enclosed in a double box of tin, which is filled with water and brought to a temperature midway between the initial and final temperature of the calorimeter. The small size of the latter, which contains only about 60 grams of water, enables us to obtain a considerable elevation of temperature. Thus 20 litres cooling from 100° to 20° raised the temperature 8°. To produce the same elevation Regnault required 200 litres, a serious objection, when it is difficult to obtain large quantities of the gas in a state of purity. With this apparatus the specific heat of several gases was measured with the following results, in which R. denotes Regnault's determination, and W. that obtained with the above apparatus. Air, R. ·2377, W. ·237; carbonic acid, R. ·2043, W. ·208; hydrogen, R. ·3409, W. ·3431; ethylene, R. gives ·4147 and ·3933 or ·4040 as a mean, W. ·4070 and ·3950 or a mean of ·401. The agreement is therefore very satisfactory.—*Bib. Univ.*, cci, 73. E. C. P.

II. Geology and Natural History.

1. *Coral reefs of Hawaii.*—Mr. Darwin, in the new edition of his work on Coral reefs, cites statements from Ellis respecting the existence of elevated beds of coral detritus "round several parts of Hawaii, about twenty feet above the level of the sea." The writer, as Mr. Darwin states, saw hardly any reefs about the island, the only point mentioned in my report being the vicinity of Hilo. In reply to an enquiry by me on the subject, the Rev. Mr. Coan, long a resident of Hilo, and, as missionary, a traveler over various parts of the island of Hawaii, makes the following statement in a letter dated Hilo, October 26th, 1874. Mr. Coan is a careful observer of natural objects and phenomena, aud has written much on the Hawaiian volcanos.

"With respect to your enquiry whether there is any elevated coral reef rock around the shores of Hawaii, I would reply that I think not. I have traveled the whole circuit of the island by land, and in boats, canoes and larger vessels, and there is hardly a point along the shores which I have not noticed carefully. Honolulu, on the island of Oahu, is built much of it upon the elevated coral-reef rock, and there are large areas in the district of Waianae and other portions of the Oahu shores; but there is nothing of this kind on Hawaii. You are aware that corals, even under the water, are on the weather side of this island [the eastern, near the middle of which is the harbor of Hilo], not abundant, and all the good specimens we get are obtained by diving. Small quantities of broken corals are washed ashore by the waves."

The Oahu reefs are described by me in my Exploring Expedition Geological Report, pp. 251–256. The facts are more briefly mentioned in my work on Corals and Coral Islands. J. D. DANA.

2. *Drift in Kansas;* by Rev. M. V. B. Knox. (From a letter to J. D. Dana, dated Sept. 24, Baldwin City.)—The drift in Kansas is confined mostly to the northern half of the State, little having been found any distance south of the great Kansas Valley. North of this river, especially in the region north and west of Topeka, there are drift rocks of vast size. The prevalent kind of rock is red and flesh-colored quartzite, with a mixture of conglomerate and trap; the mass is red quartzite. On the high prairie, these boulders are sometimes from six to fifteen feet in diameter; yet in the northeast fourth of the State, one may ride twenty miles over the prairies and not see one of so large size. Smaller boulders and pebbles are everywhere to be found in this part of the State.

North of the Kau, or as it is called far west, the Smoky Hill, in the Solomon River region, there are pebbles only of the size of quails' eggs—and these are confined to the beds of the streams. In the forks of the Solomon, about Cawker, large beds of these pebbles are found, washed from the soil, no doubt, in the whole course of the streams, and thrown into banks. These forks do not reach near the Rocky Mountains, but are entirely made up in the prairies. South of the Kau there are a few regions of drift. One in Wabamsu County, opposite Wamego, is the largest deposit I have seen in the State. Fifty or a hundred acres, on the top of high bluffs, are covered so thick as to take up all the space, and I judged that it might be 8–12 feet deep in places. None of the stones or boulders are more than three or four feet in diameter.

3. *Note on the Hawaiian Volcanoes;* by Rev. T. COAN. (From a letter to J. D. Dana, dated Hilo, Oct. 6.)—Kilauea has been very active for the greater part of the past year. The great South lake has been full and overflowing much of the time; and the great central depression of 1868, in the crater, has been filled up by deposits about 200 feet, while the region around the great south lake (Halemaumau) is a truncated mountain nearly as high as the outer upper edge of the crater.

Mokuaweoweo, the summit crater of Mauna Loa, has been in action for eighteen months. For the most of the time the action has been violent. Of late it has decreased, and there is the appearance that it will soon cease.

We have had few earthquakes at Hilo during the year, and these have been feeble. They are often felt near Kilauea in the district of Kau.

4. *Permian in the Nova Scotia Coal Region.*—Dr. Dawson has a paper in the Quarterly Journal of the Geological Society, vol. xxx, in which he points out reasons for suspecting the upper part of the Carboniferous series to be of the Permian period. Several of the plants of the beds are in Europe, both Carboniferous and Permian, as *Calamites Suckovii*, *C. Cistii*, *Annularia longifolia*, *Neuropteris flexuosa*, *N. cordata*, *N. auriculata*, *Pecopteris aborescens*, *P. oreopteroides*, *P. abbreviata;* and "*Calamites gigas* is a decidedly and peculiarly Permian species." The evidence is not positive. No marine shells occur in the beds to aid in arriving at a true conclusion.

5. *Seventh Annual Report of the United States Geological Survey of the Territories for* 1873, Dr. F. V. HAYDEN, U. S. Geologist in charge: conducted under the authority of the Secretary of the Interior. Washington, 1874.—Dr. Hayden's Report for 1873, just now leaving the press, comes laden with valuable facts respecting the topography, geography and varied researches of the Rocky Mountain Region, and particularly of the Territory of Colorado. Dr. Hayden opens the volume with chapters on the features and general geology of the Colorado Range, South Park, Park Range, Sawatch Range, and the Elk Mountains, and on the remarkable glacial phenomena about the mountains. Next fol-

lows an excellent Geological Report by ARCHIBALD R. MARVINE, Assistant Geologist, directing the Middle Park division of the Survey. The topographical features are carefully described, and then the several geological formations in order. The Lignitic formation of Colorado he refers, like Dr. Hayden and Mr. L. Lesquereux, to the Lower Tertiary. Numerous analyses are given of the Lignitic coals, and much other information of economical value relating to the character and distribution of the coal beds is added. The granite and other granitic rocks of the mountain ranges in the region are shown by Mr. Marvine to be of metamorphic origin.

Pages 193 to 274 are occupied by the Report of Dr. A. C. PEALE, Geologist of the South Park Division. The Report contains a large amount of information respecting the Cretaceous, Lignitic and other formations of the region. In Pleasant Park, Carboniferous rocks were distinguished, overlying the granite. On Trout Creek, a few miles below Bergen Park, fossils of the Quebec group were collected, including species of *Obolus*, *Conocoryphe*, *Bathyurus*, etc. Beds referred to the Quebec group were found also on Horse-shoe or 4-mile Creek, near Horse-shoe Mountain, with also others of the Carboniferous. In the vicinity of Tennessee Pass, between the Arkansas and the head of Eagle River, on a large branch coming from the west, the beds are stated to range in age from the Silurian to at least the Upper Carboniferous. No mention, however, is made of Upper Silurian or Devonian fossils. The report closes with a catalogue of minerals and rocks.

Next follows a report by Mr. F. M. ENDLICH, on the mining districts of Colorado; another of 60 pages by LEO LESQUEREUX, on the Lignitic formation and its fossil flora; one of more than 100 pages by Mr. E. D. COPE, on the Vertebrate paleontology of Colorado, containing descriptions of many new species. These are followed by Zoological and Geographical reports of considerable length, the latter by Mr. James T. Gardner, Geographer of the Expedition, the sheets of which have not yet reached us.

Dr. Hayden, by adding to his own valuable work that of other scientific observers of known thoroughness, carries forward a vast amount of research each year, and is thus making extraordinary progress in extending our knowledge of the Rocky Mountain territories.

We learn that extended quarto Reports on the Fossil Plants by L. LESQUEREUX, and on the Invertebrate Fossils of the Rocky Mountain region by Mr. F. B. MEEK, both to be illustrated by numerous plates, are soon to be issued.

6. *Catalogue of Plants collected in the years* 1871, 1872 *and* 1873, *with descriptions of new species.* 62 pp. 8vo. *Report upon Ornithological specimens collected in the years* 1871, 1872 *and* 1873. 148 pp. 8vo. Geographical and Geological explorations and surveys west of the one hundredth meridian. First Lieut. GEO. M. WHEELER, Corps of Engineers, in charge, Engineer

Department, U. S. Army.—These catalogues of Plants and Birds, collected in connection with Lieut. Wheeler's expeditions, contain, besides a list of species, notes upon many of those imperfectly known, and descriptions of some new species. The Botanical Catalogue has been prepared through the assistance of various botanists, namely: Prof. Asa Gray, Mr. Sereno Watson, Prof. D. C. Eaton, Mr. Thomas P. James, Dr. George Vasey, Dr. George Thurber, Mr. Josiah Hoopes, Mr. S. T. Olney, and Mr. C. F. Austin, and also of Assistant Surgeon U. S. A., Dr. J. T. Rothrock.

The report on the Birds is by Dr. N. C. Yarrow and Henry W. Henshaw. It contains much valuable information respecting the characters and distribution of the species.

7. *On the Use of* "*Cyclosis*" *in America.* (Read before the Association for Advancement of Science at Hartford.)—The words *rotation* and *cyclosis* were first proposed in 1829 by C. H. Schultz, in a letter on the circulation of fluids in plants, addressed to the Paris Academy, and published in 1831 in the Ann. des Sciences Nat., 1st ser., vol. xxii. In this letter he defines rotation as the movement of sap round the axis of each particular cell, while cyclosis is the name he gives to the circulation in spiral vessels he supposed occurred in phanerogams. In a subsequent essay, "Sur la circulation," etc., published in Nova Acta, 1841, vol. xxiii, 2d supplement, the application of cyclosis is restricted to the movements of the latex in the lacticiferous vessels.

Mohl and others proved many of the theories of Schultz to be erroneous, but these words proposed by him were adopted and continue in use by most English authors in their original signification.

In America, with the exception of Dr. Darby, who in his "Flora of the Southern States" has retained the original meaning, the word rotation appears to be ignored, and cyclosis substituted for it, to denote the intra-cellular motion of protoplasm,—a transfer not justified by either the origin or present use of the word in Europe.

It is perhaps true, as remarked in the Botany of Maout and Decaisne, that Schultz did not make a happy application of the word cyclosis, and if it were established that latex circulation did not exist, the transfer of use might be accomplished.

But many competent observers believe in an independent latex motion, the old meaning is still adhered to in England, and hence an unnecessary confusion of terms results from applying the name cyclosis to the cell circulation properly called rotation.

It should be remembered that both these movements of plant fluids are distinct from the interchange between roots and branches, known as the general circulation. WM. H. SEAMAN.

Note.—The case stands thus: Von Mohl, the highest authority, is responsible for the statement (which Le Maout and Decaisne say he demonstrated) that Schultz's pretended latex circulation has no existence; the present writer took up the word *cyclosis* for the actual phenomenon which it well describes; and finally, Le Maout and Decaisne say: "C'est à le mouvement intra-cellulaire qu' on a donné le nom de *rotation*, terme tout a-fait impropre, auquel il serait convenable de substituer celui de *cyclose*, aboli par Hugo Mohl, et qui exprime plus exactment le mouvement circulaire du suc dans la cellule" (p. 117). And we learn that the term *cyclosis* is defined accordingly in England, in the last edition of the Micrographic Dictionary. So there is some good authority for the American use of the term in Europe. A. G.

8. *Notice of Papers on Embryology by A. Kowalevsky;* by A. AGASSIZ. (Communicated.)—A. Kowalevsky has published, unfortunately in Russian, two capital papers on Embryology. The one continues the investigations he had been carrying on regarding the existence of an ectoderm and entoderm layer in the early embryonic stages of Invertebrates. In the present paper he has given a summary of the early stages of a Campanularia, confirming the observations of Wright and A. Agassiz. For Rhizostoma and Cassiopea he shows that the digestive cavity is formed by the invagination of the ectoderm. This is contrary to the results of previous observers, except Schneider. For Pelagia he shows a direct development from the egg remarkably similar to that of the Geryonidae as we know it from Hæckel, Fol and Metschnikoff. He adds nothing to the embryology of Actinia not already known from the magnificent monograph of Lacaze-Duthiers. He then passes on to the development of Alcyonium, of which he gives an extremely interesting sketch supplemented by fragments on the embryology of Astræa, Gorgonia and Cerianthus, the development of the latter is strikingly similar to that of Edwardsia, as we know it during its passage from Arachnactis to Edwardsia. He has added a few observations on the earlier embryonic stages of Eschscholtzia, Beroë and Eucharis, completing deficiencies in his earlier papers on the embryology of Ctenophoræ. These supplementary observations agree completely with the observations of A. Agassiz on the embryology of Ctenophoræ.

The second Memoir is a very complete history of the development of Brachiopods, strikingly in accordance with the views of Steenstrup and of Morse on the affinities of Brachiopods with Annelids. The homology between the early embryonic stages of Argiope with well known Annelid larvæ is most remarkable, and the resemblance between some of the stages of Argiope figured by Kowalevsky and the corresponding stages of growth of the so-called Lovén type of development among Annelids is complete. The number of segments is less, but otherwise the main structural features show a closeness of agreement which will make it difficult for Conchologists hereafter to claim Brachiopods as their special property. The identity in the ulterior mode of growth between the embryo of Argiope and of Balanoglossus, in the Tornaria stage, is still more striking; we can follow the changes undergone by Argiope while it passes through its Tornaria stage, if we may so call it, and becomes gradually, by a mere modification of the topography of its organs, transformed into a minute pedunculated Brachiopod differing as far from the Tornaria stage of Argiope as the young Balanoglossus differs from the free-swimming Tornaria. In fact the whole development of Argiope is a remarkable combination of the Lovén and of the Tornaria types of development among Worms. His paper also includes the history of a less vermiform type of development, that of Thecidium and of Terebratula, in which the observations of Kowalevsky fully agree with the previous well known memoir of Lacaze-Duthiers on Thecidium, and of Morse on Terebratulina. It is not out of place to recall the very ungenerous treatment which Morse received at the hands of many Conchologists for the heresies of his papers on the systematic position of Brachiopoda; and it certainly is a striking proof of the sagacity of Morse to have announced so positively, from the history of the American Brachiopods alone, the vermiform affinities of Brachiopods, now so conclusively proved by the development of Argiope in Kowalevsky's paper.

The close relationship between Brachiopods and Bryozoa cannot be more fully demonstrated than by the beautiful drawings on Plate v. of Kowalevsky's history of Thecidium. We shall now have at least a rational explanation of the homologies of Brachiopods, and the transition between such types as Pedicellina to Membranipora and other incrusting Bryozoa is readily explained from the embryology of Thecidium. In fact, all incrusting Bryozoa are only communities of Brachiopods, the valves of which are continuous and soldered together, the flat valve forming a united floor, while the convex valve does not cover the ventral valve, but leaves an opening more or less ornamented for the extension of the Lophophore.* A. AG.

9. *Embryology of the Ctenophoræ;* by ALEXANDER AGASSIZ. 4to, with five plates. From the Memoirs of the American Academy of Arts and Sciences, vol. x, No. iii, August, 1874.—In this memoir we have a complete embryology of *Idyia roseola*, and a nearly complete one of *Pleurobrachia rhododactyla*, with observations on other genera. The memoir concludes with a discussion of the systematic position and affinities of the Ctenophoræ, from which we make the following extracts:

"The question of the systematic position of the Ctenophoræ can now, thanks to the greater knowledge we have of their embryology, be treated more intelligently. The position taken by Vogt who follows Quoy in removing them from the Acalephs altogether, and associating them with the Mollusks on account of the apparent bilaterality so strongly developed in some families (Cestum, Bolina and Mertensia), seems not untenable. The nature of their relations to Echinoderms, Polyps and Acalephs, as well as the general relations of the Cœlenterata to Echinoderms, may be discussed again, especially as having an important bearing not only on the value of the Cœlenterata as a primary division of the animal kingdom, but also on the limits of Radiates, and the possible affinities of the Sponges and Cœlen-

* Mr. B. P. Mann translated for me the explanation of the plates of the two Memoirs of Kowalevsky.

erata suggested by Hæckel.* A still more important point developed from this embryology is its connection with the Gastræa theory of Hæckel,† for which he claims that it will supplant the Type theory, and give us in its place a new system based upon the homology of the embryonic layers and of the primitive digestive cavity. Hæckel attempts, in his Gastræa theory, to find an explanation for the natural development of species from a purely mechanical cause, and has been bold enough not only to name, but also figure, the primitive ancestor from which all types of the animal kingdom have been developed! This unknown ancestor, he says, must have been built much like his gastrula, only another name for what has long been known to all students of Invertebrates as the Planula of Dalyell. Hæckel would lead us to believe that this gastrula is a newly discovered embryonic stage; all he has done in reference to it, is to recall the existence of planulæ among Sponges which had previously been discovered by N. Miklucho Maclay.‡ Since the publication of Hæckel's article, his special interpretation of fanciful affinities and homologies existing only in forms conjured up by Hæckel's vivid imagination, have been sufficiently criticised by Metschnikoff,§ so that until we know something more of the development of Sponges we may leave the discussion of their affinities with Cœlenterates out of the question, in spite of the ingenious arguments advanced to support Leuckart's views on the subject.

"The existence of planulæ, the walls of which consist of an ectoderm and entoderm, has been distinctly proved for Acalephs, Echinoderms, Polyps, Worms, Arthropods, Tunicates, Mollusks,‖ and finally for Amphioxus, the papers of Johannes Müller, Krohn, Agassiz, Kowalevsky, Sars, Allman, Claparède, Kupfer, Metschnikoff and others, are too well known to need citation in this connection. So far we are in perfect accordance with Hæckel and cordially agree with him in his estimate of the systematic value of this early embryonic stage, whether we call it planula or adopt his later name of gastrula. But let us follow his subsequent steps and separate what is known from what is stated as known by Hæckel. It is known that the planula consists of an entoderm and of an ectoderm. It is known that the primitive digestive cavity is in the case of Echinoderms, of Ctenophoræ and of some Discophoræ, formed by the turning in of the ectoderm, so that the walls of this primitive cavity is, in their case at least, invariably formed by the ectoderm. It is known, on the other hand, that in Actiniæ, in Worms, in Hydroids,¶ this primitive digestive cavity is hollowed out of the inner yolk mass of the embryo, and has its

* Hæckel, E. Die Kalkschwämme, Berlin, 1872.

† Hæckel, E. Die Gastræa Theorie, Jenaische Zeitschrift, ix, 1874.

‡ Maclay, N. Mikulcho. Jen. Zeitschrift, iv, 1868.

§ Metschnikoff, E. Zur Entwickelungsgeschichte d. Kalkschwämme. Zeits. f. Wiss. Zool., xxiv, 1874.

‖ Lankester, E. R. On the primitive cell layers of the Embryo. Ann. Mag. N. H., Feb., 1873.

¶ Fol, H. Die erste Entwickelung d. Geryonideneies. Jen. Zeitsch., vii, p. 471.

walls formed by the entoderm. We must lay great stress on this point, which is alluded to by Hæckel as of no consequence, * for this seems to us to destroy the very base of his argument. If the gastrula can in one case, and in such closely allied classes as Actiniæ and Hydroids on one side, and Echinoderms and Ctenophoræ on the other, be built so differently that in the first case the walls of the primitive cavity are formed by the entoderm, and in the other of the ectoderm, what becomes of all of his subsequent generalizations of the value for systematic purposes of these two layers? The distinction of entoderm and ectoderm is, as Hæckel himself acknowledges, and as is sufficiently shown by Kowalevsky, of the greatest anatomical value, yet how is it possible that these differently constructed planulæ should have the genetic connection claimed for them by Hæckel, if in their very embryonic stages the differences are of so radical a nature that according to the very theory of embryonic layers so strongly insisted upon by Hæckel, they could have no possible relation, the one being a product of the entoderm, the other of the ectoderm, the two primitive embryonic layers.

"It is not known, as is stated by Hæckel, that the walls of the primitive digestive cavity are invariably formed of the entoderm, and when Hæckel states the result (the gastrula) to be the same whether formed by the ectoderm or entoderm he states what is known to be exactly the contrary. It is not known, as is stated by Hæckel, that the mere fact of a planula fixing itself by one extremity or not, will in one case lead to a radial type, in another to a bilateral type. What becomes of all the free-swimming embryos of Echinoderms, of Acalephs, of Polyps? Are they bilateral? It is true Hæckel is obliged, to suit his theory, to consider the Echinoderms as an aggregation of individuals, but he has not the countenance of a single zoölogist whose opinion on Echinoderms is of any value; when he says that Sars, whose knowledge of the development of Echinoderms was so accurate, agreed with his peculiar views, we can only reply that his agreement must be based upon a misunderstanding. We have equally as many radial and bilateral types developed either from fixed or from pelagic gastrulæ, and to cite this as a *causa efficiens*, the mechanical reason of the genetic descent of all radiates from a fixed gastrula, and of all bilateral types from a free-swimming one, is simply fantastic. How is it that so many Actiniæ and Acaleps have their radiate structure developed long before they become fixed? It is not known that the embryonic layers of Acalephs are truly homologous to those of the higher Vertebrates. Huxley simply speaks of their bearing the same physiological relation to one another, but until we know the gastrula of other Vertebrates than Amphioxus it is idle to talk of the continuity existing between the Ontogeny of Amphioxus, and the remaining members of the Vertebrate branch, and to say that hence there is *no doubt left* that the

* Hæckel and Lankester both seem to think that because the result is a similar form it must be homologous.

ancestors of the Vertebrates must have passed through in the beginning of their development the gastrula form! Neither Hæckel nor any one else has seen this; it is a pretty hint which may or may not be proved.

"Considerable confusion arises in Hæckel's classification from his adopting at one time as of primary importance the development of the cavity of the body and making it the main point in his phylogenetic classification, while previously the relations of the phylum to Protascus and Prothelmis (names he gives to the unknown ancestors of the radial and bilateral types) formed the basis of his classification. This places him in the awkward predicament of having a phylum of the animal kingdom (the radial) which has lost the capacity of forming a body cavity, and yet its descendants have in some unaccountable manner (entirely against the rules of Hæckel's theory) managed to get one by some unexplained method. We do not see how it can be so confidently stated by Hæckel that Echinoderms have lost their original central nervous organ; there is no proof whatever of its once having existed. There is, as yet, no proof whatever that the organs of sense (which, as had already been so often insisted upon by Agassiz, are not homologous in the different branches of the animal kingdom) have the same phylogenetic origin. When Hæckel says that the mouth of Echinoderms is not homologous to the primitive mouth, we can only refer him to the memoirs of Müller, Metschnikoff and myself on Echinoderm embryos for proof to the contrary.

"There seems no doubt, as Hæckel insists, that to the majority of zoölogists of the present day the idea of type is a very different one from that of type as understood by Baer and Cuvier. The probability of their original community of origin is hinted at from the many so-called intermediate forms, both living and fossil, which, though we may enroll them either in one great branch of the animal kingdom or another, yet show that we can no longer consider the great types of the animal kingdom as closed cycles, but must hereafter regard them as holding to one another relations similar to those which the remaining categories of our systems have to one another. This change has principally been brought about by a better knowledge of the embryology of a few well known types.

"But what becomes of all the assumptions of Hæckel which form the basis of his Gastræa theory? They are totally unsupported, and with their refutation must fall his theory; it can only take its place by the side of other physiophilosophical systems; they are ingenious arrangements laboriously built up in the interests of special theories, which fall to the ground the moment we test them by our actual knowledge. That the time has not yet come for embryological classifications, the attempts of Hæckel plainly show, for they are in no ways in advance of the other embryological classifications which have preceded them; we get new names for somewhat different combinations, but a truly scientific basis for a classification based upon the value of embryonic layers is at present impossible; such attempts can be only speculations, to be proved or disproved on the morrow.

"What Hæckel substitutes in the place of the accepted types of the animal kingdom is simply another view of these same types, and his Gastræa theory is in no danger of upsetting, at present at least, zoölogical classification as now understood. Indeed, if we need an ancestor for our phylum, why not at once go back to the cell? There we have a definite starting point, a typical element which underlies the whole of the animal kingdom and which forms the walls of Hæckel's gastrula. Then we shall all be agreed, and when we frankly state that all organisms are derived from a primitive cell and from its subsequent increase, we come within the range of positive knowledge, but we are unfortunately as far as ever from having for that reason been able to trace a mechanical cause for the genetic connection of the various branches of the animal kingdom. We must meet the direct issue raised by Hæckel,—that such a genetic connection either does or does not exist,—by repeating what has so often been said by others, this genetic connection may exist, but we have at present no proof that it does exist, and at any rate his gastræa theory does not bring us any nearer to a mechanical explanation of such a genetic connection however probable it may be. * * * * *

"Here we must call attention to a marked difference between Acalephs and Polyps on one side, and Echinoderms on the other, that while in the former the connection between the digestive cavity and the water system always remains open, it is at one time disconnected in the Echinoderms, though it is eventually reopened through anastomoses of the water tubes. The anal opening holds in Ctenophoræ very much the same relation which it holds in Echinoderm larvæ, in which the water tubes are still connected with the primitive digestive cavity. When we find, as we do, that in Ctenophoræ as well as in Echinoderms, the primitive digestive cavity is formed by the inturning of the ectoderm, that in both classes the water system is developed as diverticula, from this digestive cavity, we fail to see how we can separate the Ctenophoræ from Echinoderms and place them with Polyps in a separate subkingdom of the animal kingdom. No one questions the relationship of Ctenophoræ to Acalephs, yet from embryological data it would be more natural to associate Echinoderms and Ctenophoræ into one subkingdom characterized by the mode of formation of the water system as diverticula, forming eventually chymiferous tubes in both classes, and to associate the other Acalephs with the Polyps* where the chymiferous tubes and cavities are formed by the liquefaction of the interior of the planula. Any one who will compare the figures of the embryos of starfishes (A. Agassiz, Embryol. Starfish, pl. II, fig. 8) and Ctenophoræ (pl. III, figs. 6–10; pl. V, figs. 5, 11) at the time when the chymiferous tubes are reduced to mere diverticula, cannot fail to feel satisfied of their complete identity of plan. Metschnikoff has made, in addition to the homologies I have just recalled, a most interesting comparison

* See Allman's views on the position of the Ctenophoræ as contrasted to the Actinozoa. Trans. R. S. Edinb., xxvi, pl. II, p. 466, 1871.

between an Echinoderm larva and a Ctenophore; he shows that even in the adult Ctenophore the identity of plan is not destroyed and is carried out to the smallest details. The only point in which I would differ from him is in his comparison of the abactinal cœliac openings to the actinostome; he seems to forget that in Echinoderm larvæ what at first performed the part of anus and mouth eventually becomes the mouth alone, so that his figures should be reversed, and then the identity will be found complete between an Echinoderm larva (see A. Agassiz, Embryol. Starfish, pl. III, fig. 6, and pl. VII, fig. 8) with its œsophagus, digestive cavity, alimentary canal and its chymiferous pouch (water system) from which run the diverticula eventually to become the water tubes, and a Ctenophore (pl. III, fig. 25) with its lateral tubes on the sides of the digestive cavity (*g*), leading into the chymiferous pouches, *w*, branching into the chymiferous tube. The cœliac opening (pl. III, fig. 45, *ca*) of the funnel he looks upon as representing the madreporic body, while I look upon them as the anal openings. In this view of the case, the Ctenophore is rather more in the embryonic condition of the Echinoderm larva, when the actinostome leading into the digestive cavity should perform at the same time the function of mouth and anus, which it occasionally does, although at other times the cœliac opening of the funnel seems to be the true anal opening, while according to Metschnikoff, it is the madreporic body, which performs the part of an anal opening. He says it only acts to introduce water into the system, which is contrary to my observations.

I may here recall former statements* concerning the affinities of the Ctenophoræ, when describing some of the younger stages. It could only be after a careful comparison of Ctenophorous and of Echinoderm embryos, that undoubted evidence of their identity of plan might be obtained. The Ctenophoræ retain the permanently embryonic features of Echinoderm embryos, in which the water system is still connected with the digestive cavity. The formation of a funnel as a sort of alimentary canal, opening externally through the cœliac apertures at the abactinal pole, corresponds to the existence of a short alimentary canal in Echinoderm larvæ. The Ctenophoræ are from their embryology, more closely related to the Echinoderms than to the other Acalephs, and it seems natural to separate the Acalephs into two orders, the Ctenophoræ characterized by the presence of locomotive flappers, and the Medusidæ, including the Discophoræ, and Hydroids.

10. *Development of Marine Sponges.*—Mr. H. J. Carter has a paper in the Annals and Magazine of Natural History for November (to be continued in the following number), giving an account of the results of his microscopic investigations of marine sponges, with regard to the development from the earliest recognizable appearance of the ovum to the perfect individual sponge.

* Agassiz, Alexander. Ill. Cat. M. C. Z., No. 2, p. 12, 1865.

III. Astronomy.

1. *On the apparent Connection between Sun-spot and Atmospheric Ozone;* by T. Moffat.—At the last meeting of the British Association, Mr. Smith, of Cirming, gave me a record of new groups of sun-spots which appeared in each year for a number of years, and he asked me to compare the mean daily quantity of ozone in each year with the number of groups. I have done so, for nineteen years, 1851–1869.

It would appear from the results that the maximum of sun-spot gives a maximum of ozone, and that the minimum of sun-spot gives the minimum of ozone. The years 1854 and 1863 appear to be exceptional. In 1854, however, ozone observations at Hawarden were suspended for three months, which may account for the irregularity in that year. There is, I think, in these results, sufficient to induce others to observe.—*Proc. Brit. Assoc., Nature,* Sept. 17.

IV. Miscellaneous Scientific Intelligence.

1. *Permanent Ice in a Mine in the Rocky Mountains;* by R. Weiser of Georgetown, Colorado. (Communicated.)—Geologists have been not a little perplexed with the frozen rocks found in some of our silver mines in Clear Creek Co., Colorado. I will first give a statement of the facts in the case, and then a theory for their explanation.

There is a silver mine high up on McClellan Mountain, called the "Stevens Mine." The altitude of this mine is 12,500 feet. At the depth of from 60 to 200 feet the crevice matter, consisting of silica, calcite, and ore, together with the surrounding wall-rocks, is found to be in a solid frozen mass. McClellan Mountain is one of the highest eastern spurs of the Snowy Range; it has the form of a horse-shoe, with a bold escarpment of feldspathic rock near 2,000 feet high, which in some places is nearly perpendicular. The Stevens Mine is situated in the southwestern bed of the great horse-shoe; it opens from the northwestern. A tunnel is driven into the mountain on the lode, where the rock is almost perpendicular. Nothing unusual occurred until a distance of some 80 or 90 feet was made; and then the frozen territory was reached, and it has continued for over two hundred feet. There are no indications of a thaw summer or winter; the whole frozen territory is surrounded by hard massive rock, and the lode itself is as hard and solid as the rock. The miners being unable to excavate the frozen material by pick or drill to get out the ore (for it is a rich lode, running argentiferous galena from 5 to 1,200 ounces to the ton), found the only way was to kindle a large wood fire at night against the back end of the tunnel and thus thaw the frozen material, and in the morning take out the disintegrated ore. This has been the mode of mining for more than two years. The tunnel is over two hundred feet deep and there is no diminution of the frost; it seems to be rather increasing. There is, so far as we can see, no opening, or channel through which the frost could possibly have reached such a depth from the surface. There are other mines in the same vicinity in a like frozen state.

From what we know of the depth to which frost usually penetrates into the earth, it does not appear probable that it could have reached the depth of two hundred feet through the solid rock in the Stevens Mine, nor even through the crevice matter of the lode, which, as we have stated, is as hard as the rock itself. The idea, then, of the frost reaching such a depth from the outside, being utterly untenable, I can do no other way than to fall back upon the Glacial era of the Quaternary. Evidences of the Glacial Period are found all over the Rocky Mountains. Just above the Stevens Mine there are the remains of a moraine nearly a mile long, and half a mile wide. The debris of this moraine consists of small square and angular stones, clearly showing that they have not come from any great distance. And just over the range, on the Pacific slope, there are the remains of the largest moraine I have ever seen, consisting of feldspathic boulders of immense size. I conclude, therefore, that it was during that period of intense cold that the frost penetrated so far down into these rocks; and that it has been there ever since, and bids fair to remain for a long time to come.

2. *Franz-Joseph Land.*—Two papers are published in the current volume of Petermann's Geographical Journal (p. 381), on the Austrian Arctic discoveries, one by Petermann, and the other by Dr. Joseph Chavanne. The accompanying map represents the Francis-Joseph Land as a group of large and small islands, between the paralells of 79° 50′ and 83° 10′, and between the meridians of 49° and 65° east of Greenwich. The limits of the larger masses of land remain yet unascertained.

3. *Physiology;* by M. Foster, M.A., F.R.S. Part VI of Science Primers, edited by Professors Huxley, Roscoe and Balfour Stewart. 132 pp. 16mo. (D. Appleton & Co., New York.—An excellent little work, simple in language, clear in its explanations and illustrations, and good in its science.

4. *The Transit of Venus;* by George Forbes, B.A., Prof. Nat. Phil. in the Andersonian University, Glasgow. 100 pp. 12mo, with numerous illustrations. London and New York (Macmillan & Co.).—This volume is a revised issue of a series of articles published in Nature, which give in detail the objects and methods of the observations, with an account of the expeditions sent out by the different governments. The explanations of the subject are made as popular as the case admits of, and are well illustrated by figures.

5. *Swedish Iron Ores.*—A collection of the Swedish ores has been received by Lafayette College, Easton, Pennsylvania, from the iron department under the Swedish government.

Tidal Researches: by William Ferrell, M.A., Assistant U. S. Coast Survey. 268 pp., 4to, with 3 plates. From the U. S. Coast Survey Report for 1874.

Fig. 8.

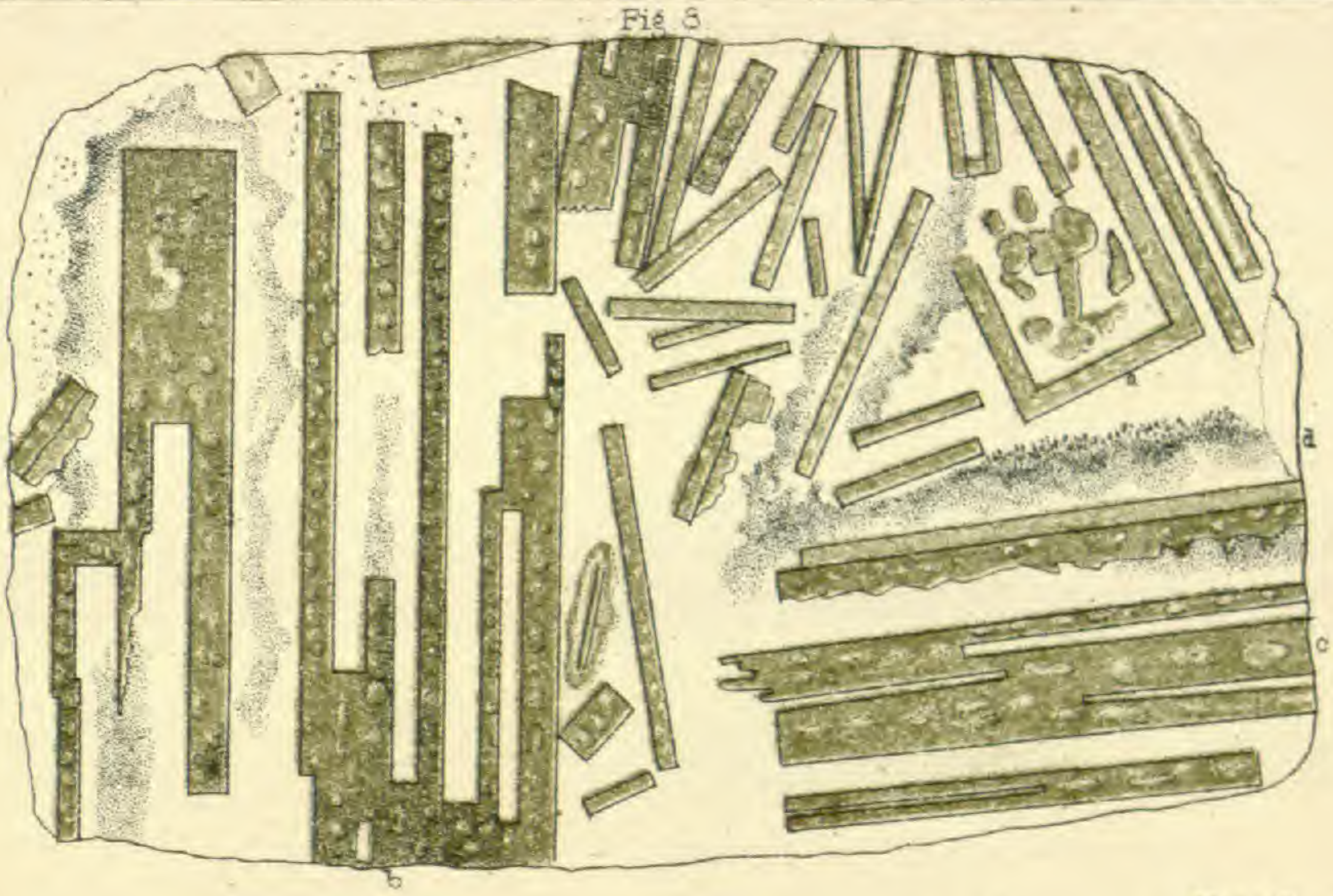


Fig. 12.

Fig. 9.

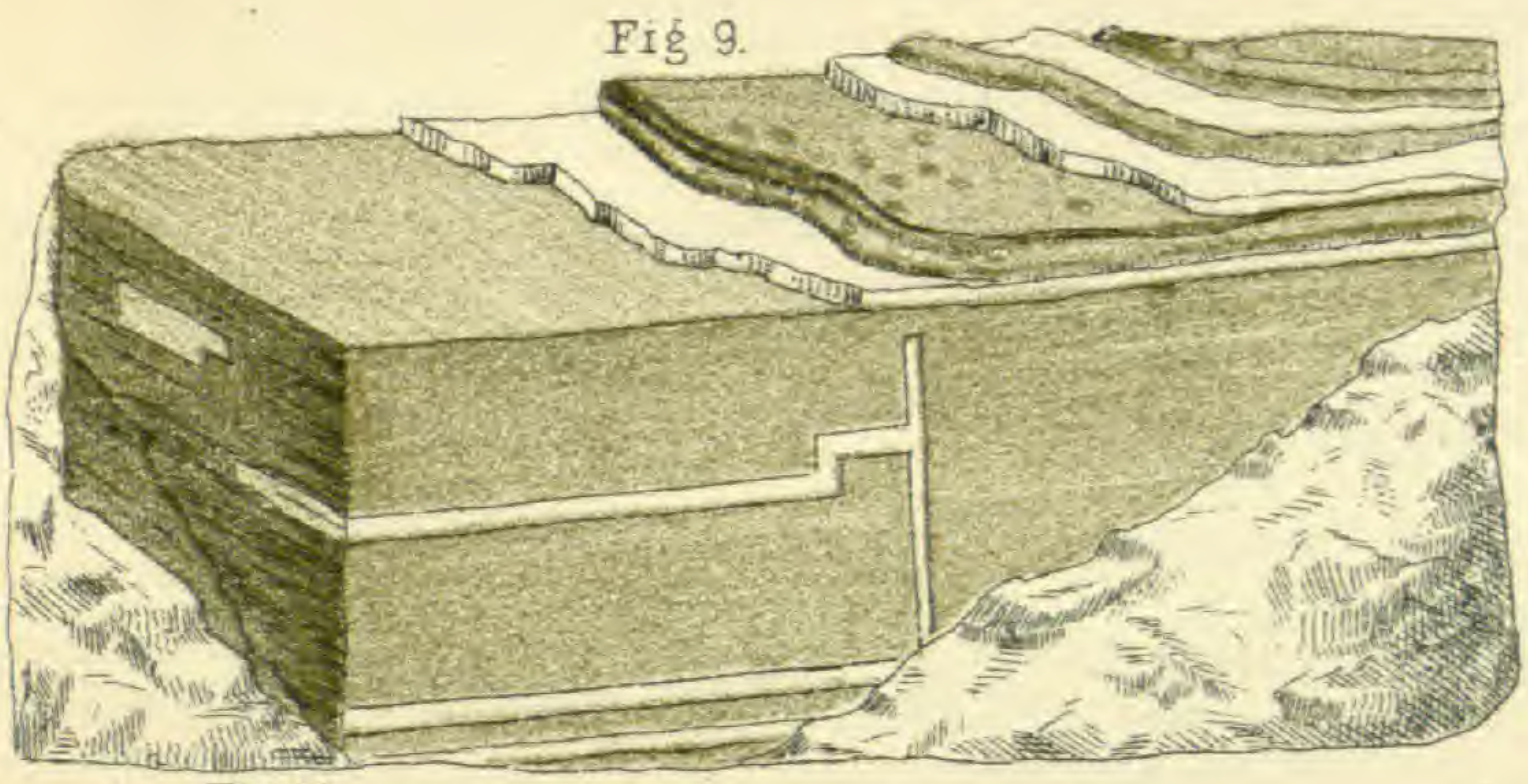


Fig. 11.

Fig. 13.

Fig. 10.

Punderson & Crisand, New Haven, Ct.

SERPENTINE PSEUDOMORPHS

INDEX TO VOLUME VIII.*

* The Index contains the general heads Botany, Geology, Mineralogy, Zoology, and under each the titles of Articles referring thereto are collected.

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