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JAMES D. AND EDWARD S. DANA.

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WITH PLATE I.

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AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. I.—*Contributions to Meteorology;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College. Twenty-first paper. With plate I.

[Read before the National Academy of Sciences, April 21, 1885.]

Direction and velocity of movement of areas of low pressure.

IN several former papers I have examined the direction and velocity of movement of areas of low pressure. Since those papers were written, the materials for these investigations have been greatly multiplied, and I now present a summary of the results which I have obtained after an extended examination of the Signal Service observations, and of the observations made in other parts of the world.

The monthly maps of storm tracks, which are issued by the Signal Service in connection with the International Bulletin, give a distinct idea of the general direction of movement of areas of low pressure for all parts of the Northern hemisphere. Plate I was formed by a combination of these monthly maps for several years. It represents only a small part of the storm tracks delineated on the monthly maps, and does not attempt to represent the storm tracks of different regions in their relative frequency, but it is designed to afford a specimen of all the important storm tracks delineated on the monthly maps for all parts of the Northern hemisphere.

From this chart we see, that north of the parallel of 30 degrees, storm tracks in all longitudes almost invariably pursue an easterly course, but generally they show an inclination toward the north of east; while within the tropics, storm tracks almost invariably tend westerly, with an inclination toward the north of west. We also notice that none of the storm tracks reach down to the equator. The lowest latitude of any centre of low pressure which has been distinctly traced is 6·1° N., and there are eight cases of cyclonic storms whose paths have been traced to points south of lat. 10° N. Hard gales and violent squalls of wind sometimes occur directly under the equator, accompanied by sudden oscillations of the barometer; but within six degrees of the equator, the depression of the barometer has never been found sufficiently great, and the depression has not been maintained with sufficient steadiness, to enable us to identify an area of low pressure in its progress from day to day.

The tropical cyclones which have been found to pursue a westerly course are limited to two districts. 1. The Atlantic Ocean, and chiefly its western part near the West India Islands; and 2. the region south of the continent of Asia. Tropical cyclones have never been observed in any part of the Pacific Ocean, with the exception of its western portion near the continent of Asia and the neighboring islands. In my fifth paper I gave a table showing the leading particulars respecting the most violent cyclones originating near the West India Islands, whose paths had been investigated previous to 1875; and in my fourteenth paper I gave a similar table based on the observations contained in the International Bulletin. The average course of the cyclones enumerated in my fifth paper, while they were moving westward, was 26 degrees north of west; and the average course of those enumerated in my fourteenth paper, during the same part of their course, was 26½ degrees north of west.

According to Maury's Pilot charts of the North Atlantic, the average direction of the wind for that part of the Atlantic Ocean in which these cyclones most frequently occurred during the three months, August, September and October (which months include nearly all the cyclones referred to), is two degrees north of east. According to the charts of the United States Hydrographic Office, which include all the observations collected by Maury and also those collected by the British Meteorological Office, the average direction of the wind is 4½ degrees north of east. The average course of West India cyclones, while moving westward, differs therefore from 28 to 30 degrees from the average course of the wind. But if we make a comparison of the winds immediately succeeding each of the cyclones and continuing for at least 24 hours, we find that the direction of a cyclone's progress accords more nearly with the direction of the principal wind which prevails at the time of the cyclone.

The average course of the cyclones originating near the China Sea and Bay of Bengal, as enumerated in my fourteenth paper, was 38 degrees north of west, as long as they were moving westerly. The average course of the Asiatic cyclones indicated by the maps accompanying the International Bulletin (nearly all of which originated in the China Sea), was 27½ degrees north of west. Since the average direction of the wind in this region changes nearly 180 degrees from summer to winter, in order to make a satisfactory comparison between the average direction of the wind and that of the progress of storms, we must make a separate comparison for the different seasons of the year. Since nearly all the cyclones, whose tracks are shown on the International charts, originated in the China Sea, I will make the comparison for this region; and since nearly all of these storms occurred from July to November, I will make the comparison for these months.

According to Maury's Pilot Chart of the China Sea, the average direction of the winds in this sea for the month of July is south 22° west; for August it is south 39° west, and for September it is south 39° west. The average for these three months is south 33° west. The average direction of the wind for October is north 53° east, and for November it is north 41° east. The average for these two months is north 47° east. For the first three months, the average direction of progress of storms, is 35° north of west, and for the last two months it is 25½° north of west; that is during the first period the average course of storms differs 88 degrees from the average direction of the wind, and during the last period the difference is 68½ degrees. We also perceive that a change of 166 degrees in the average direction of the wind is accompanied by a change of only 9½ degrees in the average direction of the progress of storms. This fact clearly indicates that the direction in which storms advance is mainly determined by some other cause than the mean direction of the wind. If, however, we make a comparison with the winds immediately succeeding each of the cyclones, and continuing for at least 24 hours, we shall find that the direction of progress of a cyclone corresponds more nearly to that of the principal wind prevailing at the time of the cyclone. It is not, however, claimed that there is an exact agreement between these two directions.

An examination of the accompanying plate shows that in the middle latitudes of the Northern hemisphere there is a remarkable correspondence between the average direction of the progress of storm centers, and the average direction of the wind as shown by Coffin's wind charts. I have endeavored to ascertain whether this correspondence is exact, or whether there is a constant difference between these two directions. I first made a comparison of these two directions for the Atlantic Ocean.

In order to determine the average direction of progress of storm centers across the Atlantic Ocean, I measured with a protractor the bearing of the storm tracks delineated on the U. S. International charts. These bearings were measured for six points, viz: at the intersection of the storm tracks with the meridians of 10°, 20°, 30°, 40°, 50° and 60° west of Greenwich, and the measurements included the observations of four years, viz: 1878–1881. The following table shows the average results of these measurements for each month of the year, and for each of the six points above mentioned. The latitudes named at the top of the table are the average latitudes corresponding to the given directions.

Average direction of Storm tracks.

	Long. 60°. Lat. 46·9°.	Long. 50°. Lat. 48·9°.	Long. 40°. Lat. 51·3°.	Long. 30°. Lat. 53·9°.	Long. 20°. Lat. 54·9°.	Long. 10°. Lat. 55·5°.
January	N. 66° E.	N. 61° E.	N. 64° E.	N. 74° E.	N. 86° E.	N. 96° E.
February	66	67	60	60	74	82
March	73	69	68	65	71	79
April	63	68	72	79	91	97
May	62	67	68	71	76	76
June	76	63	64	67	71	71
July	72	62	59	68	76	80
August	69	74	74	77	72	71
September	67	72	78	75	75	73
October	67	64	72	68	73	72
November	70	67	62	69	68	69
December	65	66	62	67	73	80
Year	N. 68 E.	N. 67 E.	N. 67 E.	N. 70 E.	N. 75 E.	N. 79 E.

I have determined the average direction of the wind at several points on the Atlantic Ocean, as near as possible to the points corresponding to the preceding measurements. I have determined the directions according to Maury's Pilot Charts, and also according to the charts of the U. S. Hydrographic Office. Since the latter charts are based on the greatest number of observations, I have used them in the comparisons exhibited in the following table. The wind directions here given are deduced from the observations for January, April, July and October. Along the line of the storm tracks, the number of wind observations on the charts is very small, and I have therefore deduced the wind directions from the observations in the five degree squares a little south of the average storm tracks.

Comparison of Storm tracks with Wind directions, over the Atlantic Ocean.

Longitude.	Latitude of storm tracks.	Direction of storm tracks.	Latitude of wind directions.	Direction of wind.	Difference of latitude.	Wind most northerly.
60°	46·9°	N. 67·0° E.	42·5°	N. 79·3° W.	4·4°	−33·7°
50	48·9	63·7	42·5	S. 85·2 W.	6·4	−21·5
40	51·3	66·7	47·5	S. 61·6 W.	3·8	+ 5·1
30	53·9	72·2	47·5	S. 73·5 W.	6·4	− 1·3
20	54·9	81·5	50·0	S. 65·2 W.	4·9	+16·3
10	55·5	86·2	52·5	S. 58·6 W.	3·0	+27·6

Column 1st shows the longitudes for which the comparisons are made; column 2d shows the latitude of the points to which the direction of the storm tracks corresponds; column 3d shows the average direction of the storm tracks for the months of January, April, July, and October; column 4th shows the latitudes corresponding to the wind directions; column 5th shows the direction of the wind for the given latitudes and longitudes; column 6th shows the differences of latitude between the points to which the storm tracks correspond, and those to which the wind directions correspond; column 7th shows the difference between the average direction of the wind, and the average direction of the storm paths for the points of comparison.

It will be seen that there is an average difference of nearly five degrees between the latitudes of the points for which the wind directions are given, and those to which the storm tracks correspond. I have endeavored to determine the proper correction of the wind directions for this difference of latitude, but the corrections appear so questionable that I have made no use of them.

We see that for the middle of the Atlantic Ocean near the parallel of 50° the average direction of storm paths corresponds very closely with that of the average progress of the wind; but in the western part of the Atlantic, the average course of storms is 30 degrees more northerly than that of the wind, while in the eastern part it is nearly 30 degrees more southerly.

I next made a similar comparison for twelve of the Signal Service stations in the northwestern part of the United States, between the Rocky Mountains and the meridian of 90° from Greenwich, for the three winter months for the ten years from 1873 to 1882. The wind directions were deduced from the sum of the observations for each of the eight principal points of the compass and the direction of the storm paths on the Signal Service maps was measured with a protractor. The following table shows the result of this comparison. Column 2d shows for each of the stations the mean direction of the wind; column 3d shows the average direction of the storm paths; and column 4th shows the difference between these two directions. The directions are all measured from the north point toward the east.

Comparison of storm paths with wind directions, United States.

	Wind blows towards	Storms move towards	Storms most northerly.
Bismark	N. 162·7° E.	N. 107·7° E.	+55·0°
Ft. Sully	154·2	106·7	47·5
Breckenridge	126·2	105·5	20·7
North Platte	121·0	104·1	16·9
Keokuk	104·7	88·3	16·4
Yankton	121·6	106·3	15·3
Omaha	114·7	100·8	13·9
Davenport	96·8	87·0	9·8
Dubuque	92·7	88·3	+ 4·4
St. Paul	78·0	99·3	−21·3
LaCrosse	67·1	90·5	−23·4
Pembina	85·7	109·4	−23·7

We see that at all of the stations, except the last three, the average wind of winter blows toward a point somewhat south of east, and for four of the more western stations, the average direction is 51° south of east. We also see that at the more western stations, the average movement of storm centers is toward a point considerably south of east, but at the more eastern stations the direction is a little north of east. At Bismark and Fort Sully, the average course of the winds is 50° more southerly than that of storm paths, while at St. Paul, La Crosse and Pembina it is 22° more northerly.

The facts here stated afford a basis for some general conclusions respecting the movement of storm centers. Some meteorologists have claimed that the progressive movement of storm areas is satisfactorily explained by saying that they are carried forward by the general movement of the mass of the atmosphere within which they are formed; that is, they *drift* in a sense similar to that in which waves, eddies, etc., formed on the surface of a river, drift with the current. They advance as the water of the river advances, and in the same direction. But we have found that the average direction of movement of areas of low barometer does not generally coincide with the average direction of the wind for the same region. This is seen not only in the case of tropical storms, but also in storms of the middle latitudes.

But it may be claimed that the progress of storm areas is not determined entirely by the average movement of the atmosphere, but by that movement which is taking place at the date of the storm. There is some reason to think that in the case of tropical cyclones, particularly in the China Sea, the wind which generally prevails at the time of the cyclone accords more nearly with the direction of the storm's progress, than does the average wind for the same season of the year; but there is no evidence that there is an exact agreement between these two directions. If we claim that the progressive movement of a storm area is due to the progressive movement of the general mass of the atmosphere in which it is formed, it seems necessary to admit that a mass of the atmosphere, of considerably greater extent than the storm area, is advancing in the same direction and at the same rate as the storm advances. In order to decide whether such is the fact, we need only to consult a well-constructed weather map, of sufficient dimensions to include not merely a storm area, but a considerable margin beyond it. The storm maps which have accompanied my former papers are too limited to furnish the required information in a form which is entirely satisfactory, and it is desirable to have similar maps for several successive days. The Signal Service maps afford abundant materials for this purpose, and Hoffmeyer's maps are still better, since they include a much larger portion of the earth's surface. If we open a volume of these maps anywhere at random, we shall not find the general mass of the atmosphere surrounding a great storm moving forward in the same direction as that in which the storm advances.

If we follow the progressive movement of a great storm from day to day, by means of maps representing the phenomena at intervals not greater than eight hours, we shall find that in front of the storm the air appears to be drawn in toward the center, by which means the pressure on the front side of the storm is diminished. The air, thus drawn in toward the center, rises to a considerable elevation above the surface of the earth and its vapor is condensed. In the rear of the storm, the exterior air rushes in and restores the pressure on that side; and as the result of this double process, the point of least barometric pressure is carried forward. This movement of the exterior air in the rear of a storm, is not necessarily in the same direction as that in which the storm center advances. In the United States, storms almost invariably advance eastward, and generally toward a point a little north of east; but the wind which presses upon the rear generally comes from the north or northwest, which direction is often at right angles, or nearly at right angles, with the direction in which the storm center advances. This movement of the air, by which the center of least pressure is carried forward, bears some analogy to the movements which cause the advance of a wave upon the surface of the ocean, and hence we may with propriety say that the progressive movement of a storm area, is the movement of a great atmospheric *wave*.

Besides these general considerations, there are various special phenomena which indicate that the movement of areas of low pressure cannot be fully explained by the theory of a general drift of the atmosphere. We frequently find two neighboring low areas advancing in directions inclined to each other at an angle of 45 degrees, or even a greater angle. In the United States, while a low center is advancing from Florida along the Atlantic coast toward the northeast, another low center may be advancing eastward over the region of the Great Lakes, and the two low centers may coalesce somewhere in the neighborhood of Nova Scotia or Newfoundland. It will be seen from the accompanying plate, that the storms which proceed from the Gulf of Mexico and from the neighborhood of the West India Islands, generally advance toward Newfoundland; and the storms which come from the northwestern part of the United States, also tend toward the same region. Newfoundland becomes thus a point of convergence of storm tracks proceeding from regions quite remote from each other. In the vicinity of Newfoundland, there exists some influence which appears to act as an attractive force upon storm centers. This influence probably results from the great amount of precipitation near that island, arising from the proximity of the warm water of the Gulf Stream, to the colder air from the land. The accompanying plate shows other points toward which storm tracks seem to converge, particularly the Asiatic coast near Japan, and this fact probably results from a cause similar to the one just named. If the accompanying plate exhibited the storm tracks of different regions according to the relative frequency of their occurrence, other points of convergence of storm tracks would be exhibited. Along these converging storm paths, two storms often travel simultaneously and coalesce in a single storm area. Such a movement appears inconsistent with the drift theory.

For the convenience of those persons who may wish to investigate cases of this kind for themselves, I present the following list which shows some of the most decided cases in which two

Examples in which two centers of low pressure approach each other and coalesce.

1873.	March	29.1–29.2	1875.	Nov.	10.1–10.2	1879.	Jan.	1.3– 2.2
	Oct.	4.3– 5.1	1876.	March	25.3–26.1		Feb.	4.1– 4.2
	Oct.	11.1–11.3	1877.	Dec.	29.3–30.1		Oct.	16.1–17.1
1874.	April	19.2–19.3	1878.	Feb.	14.3–15.1		Oct.	28.2–28.3
	April	25.2–25.3		March	13.3–14.1		Nov.	20.1–20.2
	Aug.	30.2–31.1		May	2.1– 2.2	1880.	Feb.	13.1–13.2
	Sept.	25.1–25.3		June	18.1–18.2		March	7.2– 8.1
1875.	Jan.	22.2–22.3		Nov.	22.1–22.2		Oct.	29.2–29.3

centers of low pressure in the United States have coalesced. They are taken from the Signal Service Weather Maps for the years 1873–1880. These maps show a considerable number of other cases of like kind, some of which have been omitted because the depression of the barometer was small; and others because the position of the low center was not very sharply defined, or was situated near the margin of the weather map.

Among these twenty-four cases, there are only three in which the paths of the two low centers were not inclined to each other at an angle as great as 45°; in half of the cases the two paths were inclined at an angle considerably greater than 45°; in eight or nine of the cases the angle was nearly as great as 90°; and in three of the cases the angle was greater than 90°.

It sometimes happens that within an area of low pressure, having but a single center, a second low center is developed. The following list shows twenty-four such cases, selected from the Signal Service maps for 1873–1880. The maps show a large number of other similar cases, but in the cases here cited the depression of the barometer was generally considerable, and the position of the low centers was distinctly indicated.

Cases in which a second low center is developed within an area of low pressure.

1873.	Feb.	18.1–18.2	1875.	Jan.	30.3–31.2	1878.	Jan.	13.2–13.3
	Feb.	20.1–20.2		May	4.2– 4.3		Jan.	30.2–31.1
	March	28.2–29.1		Nov.	3.1– 3.2		March	12.3–13.1
1874.	April	25.1–25.2	1876.	March	5.3– 6.1		Nov.	23.3–24.1
	Aug.	29.1–29.2		March	25.2–25.3	1879.	March	29.1–29.2
	Aug.	30.1–30.2		May	7.1– 7.2	1880.	Jan.	21.3–22.1
	Nov.	23.1–23.3		May	7.2– 7.3		Feb.	12.2–12.3
1875.	Jan.	24.2–24.3	1877.	Dec.	29.2–29.3		April	17.1–17.2

In a majority of these cases, the two low centers appear to have subsequently coalesced; but in several of them, the two low centers moved off in directions inclined to each other at an angle of 90° or more, and with unequal velocities.

Over the Atlantic Ocean and Europe, cases similar to the preceding are of much more frequent occurrence than in the United States; the depression of the barometer is generally much greater; and the low areas have a much greater geographical extent. By consulting Hoffmeyer's Weather Maps, we may easily find examples in which two low centers move toward each other from nearly opposite directions and coalesce; and we may also find frequent cases in which a great area of low pressure, with but one center, undergoes a change by which two low centers are developed, and these new low centers recede from each other. Sometimes there is a further change by which three or four or even more low centers are formed, and these low centers have a progressive movement in different directions, and with unequal velocities.

On the contrary, within a large area of low pressure, showing several low centers, a low center may disappear from simple changes of pressure. In like manner a second low center may disappear and so on. In many cases the changes in the position and magnitude of the low centers are so rapid, that in comparing two weather maps for successive days, we frequently find it impossible to identify a low center on one of the maps, with its corresponding low center on the other map. Examples may be easily found to illustrate all of these different cases, so that it seems to be unneccessary to present a selected list. In cases like these, it surely will not be claimed that the movement of the low centers can be ascribed to a simple drifting of the general mass of the atmosphere in which the low areas were formed.

If we reject the drift theory, it will doubtless be asked how can we explain the fact that in the middle latitudes, storms almost invariably advance toward the east, and the opposite movement only occurs occasionally, and seldom continues longer than one or two days. This fact seems to result from the prevalent movement of the wind towards the east. The result however is not due to a general drifting of the mass of the atmosphere within which the low area is formed; but to the fact that the pressure on the west side of the low area is more steady and persistent than that on the east side. The characteristic features of a great storm movement are a motion of the air from all sides spirally inward, together with an upward movement resulting in the condensation of vapor at various places within the low area. Now if the air pressed in with equal force on all sides of the low center, and if there was an equal precipitation of vapor on all sides, there does not appear to be any reason why the low center should advance at all. It sometimes happens that the pressure on the west side is very small, while there is considerable pressure on the east side, and in such cases the low center moves toward the west. Many examples of this kind are shown by the Signal Service maps, and also by Hoffmeyer's charts. But this movement toward the west cannot be long maintained. In the middle latitudes, the east winds are exceptional, and result mainly from disturbances caused by storms. On the contrary, the west winds result from general causes which are permanent in their character, and are independent of storms; and if there were no storms the west winds would rarely be interrupted. During the prevalence of an east wind, the causes which produce west winds are not destroyed; their influence is only temporarily suspended; and they soon return with a force, not impaired, but rather augmented by their temporary suspension. The pressure on the west side of storm areas is thus a strong and persistent one, while that on the east side results from temporary causes, and cannot be long maintained. It occasionally happens, during a violent storm, that the east winds are stronger than the west winds. In such a case the low center may be pushed westward; but such a result does not necessarily follow, for a large part of the air which pushes in on the east side rises from the earth's surface, while the air which pushes in on the west side does not rise at all, or not to an equal extent. Thus the low area is filled up on the west side, and were it not for the continued precipitation of vapor, the low area would soon become obliterated.

Rate of progress of areas of low pressure.

In order to exhibit the average velocity with which centers of low pressure advance over the United States, I have prepared the following table which shows, in miles per hour, the average velocity of storm centers for each month during a period of thirteen years, according to the observations of the United States Signal Service.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1872	31·2	29·4	34·5	34·5	24·7	21·8	24·6	18·3	22·2	20·9	23·6	28·8
1873	25·8	32·7	28·1	22·3	23·5	20·8	24·6	17·8	23·1	28·1	27·9	26·7
1874	23·0	33·9	29·8	31·4	22·2	22·4	25·9	19·9	23·1	28·5	30·3	32·7
1875	32·1	32·8	30·0	26·4	29·2	31·5	25·3	17·1	30·5	23·4	30·0	31·1
1876	38·1	31·5	26·4	23·6	24·7	19·3	26·4	23·2	23·8	27·7	22·6	38·3
1877	37·7	26·5	32·6	25·2	27·3	25·2	24·2	20·0	17·4	20·2	25·5	24·7
1878	26·3	27·7	24·3	22·6	17·9	18·4	21·7	26·8	23·9	19·6	21·2	34·0
1879	35·5	33·3	35·1	27·8	25·3	29·4	26·4	21·0	21·7	30·8	40·7	38·7
1880	37·6	39·6	35·8	27·2	25·1	24·5	25·7	25·9	23·5	22·3	34·1	38·8
1881	32·3	35·4	26·8	37·1	32·6	32·8	26·6	25·4	30·6	37·5	30·8	33·6
1882	42·1	41·6	34·8	29·5	21·6	26·8	19·8	19·9	23·5	27·7	27·7	30·2
1883	39·8	36·4	38·0	28·4	30·0	24·2	25·8	28·0	25·0	37·3	39·4	33·0
1884	38·6	43·9	33·3	21·5	26·8	20·5	22·4	30·7	32·6	34·4	35·2	43·7
Mean	33·8	34·2	31·5	27·5	25·5	24·4	24·6	22·6	24.7	27·6	29·9	33.4

We see from this table that the average velocity of progress of storms for the entire year is 28·4 miles; also that the velocity is greatest in February and least in August, and that the former velocity is 50 per cent greater than the latter. We also see that the velocity varies very much for the same month in different years, the greatest mean velocity for the months of April and October being more than double the least mean velocity for the same months.

In order to study the movement of areas of low pressure under the greatest possible variety of circumstances, I have endeavored to obtain information from European observations. In the Uebersicht der Witterung for 1881, published by the Deutsche Seewarte, is given a table showing the mean velocity of movement of the barometric minima for the five years 1876–80, as deduced from the monthly charts of storm tracks.

The following table shows the average results deduced from the observations of these five years.

Rate of progress of Storm Centers in Europe.

	Kilom. in 24 h.	Miles p. hour.	Storms. in U. S.	Ratio.		Kilom. in 24 h.	Miles p. hour.	Storms. in U. S.	Ratio.
January	673	17·4	33·8	1·94	July	549	14·2	24·6	1·63
February	694	18·0	34·2	1·90	August	541	14·0	22·6	1·41
March	676	17·5	31·5	1·80	September	667	17·3	24·7	1·47
April	626	16·2	27·5	1·70	October	732	19·0	27·6	1·53
May	569	14·7	25·5	1·73	November	720	18·6	29·9	1·60
June	609	15·8	24·4	1·54	December	693	17·9	33·4	1·87
					Year	646	16·7	28.2	1.69

Column 2nd shows the velocity of movement for each month expressed in kilometers for 24 hours; column 3d shows the velocity expressed in English miles per hour; column 4th shows the velocity of movement of storm centers for the United States; and column 5th shows the ratio of the numbers in columns 3 and 4.

We see that in the United States the average velocity of movement for the entire year is about two-thirds greater than it is in Europe. This ratio is greatest in winter when it amounts to 1·9; and least in the autumn when it amounts to 1·5.

The following table shows, in miles per hour, the average rate of progress of storm centers over the Atlantic Ocean, as deduced from the monthly charts of storm tracks published with the International Bulletin for a period of four years from 1879 to 1882.

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
17·4	19·5	19·7	19·4	16·6	17·5	15·8	16·3	17·2	18·7	20·0	18·3

The average velocity for the entire year is 18 miles per hour.

If now we compare the preceding results with those heretofore found for the West India cyclones while pursuing a westerly course, and for the cyclones of the Bay of Bengal and China Sea for the same part of their course, we shall have a view of the movement of storm areas under a great variety of conditions. If we compare the average results for these five districts for the entire year, the numbers are as follows:

United States	28·4	miles per hour.
Middle latitudes of Atlantic Ocean	18·0	" "
Europe	16·7	" "
West India cyclones	13·7	" "
Bay of Bengal and China Sea	8·4	" "

Thus we see that the average rate of progress of storm centers over the Atlantic Ocean is about the same as over Europe, and is double the rate of progress for the China Sea; and the rate of progress for the United States is more than three times the rate for the China Sea. These results are derived from so large a number of observations, that they must be accepted as substantially correct, and they demand a clear explanation.

I have endeavored to determine how far these differences may result from a difference in the mean velocity of the wind for these several districts. For this purpose I determined the average velocity of the wind for that portion of the United States within which the storm centers are most frequently found, viz: that portion north of the parallel of 40°, and east of the meridian of 100° from Greenwich. A slight examination of the observations shows that at stations near the Atlantic Ocean, or near one of the Great Lakes, the velocity of the wind is greater than at stations in the interior of the country. I have therefore divided the observations into two groups, one including the stations near the ocean, or one of the Great Lakes, and called coast stations (twenty-five in number), the other group including the remaining stations, which are called inland stations (twenty in number); the second and third columns of the following table show, for each month of the year, the mean velocity of the wind in miles per hour for these two classes of stations, according to the Signal Service observations:

	Coast Sta.	Inland Sta.	Mean.	Storms.	Ratio.		Coast Sta.	Inland Sta.	Mean.	Storms.	Ratio.
Jan.	10·98	8·40	9·69	33·8	3·5	July	7·70	6·87	7·28	24·6	3·4
Feb.	11·33	8·94	10·13	34·2	3·4	Aug.	7·42	6·40	6·91	22·6	3·3
Mar.	11·76	10·18	10·97	31·5	2·9	Sept.	8·98	6·96	7·97	24·7	3·1
April	10·67	9·59	10·13	27·5	2·7	Oct.	9·96	7·84	8·90	27·6	3·1
May	9·22	8·55	8·88	25·5	2·9	Nov.	11·16	8·50	9·83	29·9	3·0
June	8·34	7·56	7·95	24·4	3·1	Dec.	11·30	8·20	9·75	33·4	3·4

The 4th column gives the mean between the numbers in the two preceding columns; the 5th column shows for each month the rate of progress of storm centers; and the 6th column shows the ratio of the velocity of storm centers to the mean velocity of the wind.

We see that this ratio is not the same for all months; but for that month in which the rate of progress of storms is greatest, the ratio is sensibly the same as for that month in which the rate is the least. This coincidence seems to indicate that the rate of progress of storms is in some degree dependent upon the mean velocity of the wind; but the considerable inequalities in the value of this ratio show that the rate of progress of storms cannot depend solely on the average velocity of the wind.

I next determined, as well as I was able with the means at my command, the average velocity of the wind for that part of

Europe within which storm centers are most frequently found, viz: between the parallels of 50 and 60 degrees. The following table shows the results which I have obtained, and the observations are divided into two groups as in the preceding table; the coast stations being twelve in number, and the inland stations being fourteen in number. The velocities are all expressed in miles per hour.

	Coast Sta.	Inland Sta.	Mean.	Storms.	Ratio.		Coast Sta.	Inland Sta.	Mean.	Storms.	Ratio.
Jan.	13·86	8·61	11·23	17·4	1·5	July	11·52	6·53	9·02	14·2	1·6
Feb.	13·98	9·13	11·55	18·0	1·6	Aug.	11·97	6·42	9·19	14·0	1·5
Mar.	13·60	9·31	11·45	17·5	1·5	Sept.	11·32	6·78	9·05	17·3	1·9
April	12·28	8·05	10·16	16·2	1·6	Oct.	13·33	7·87	10·60	19·0	1·8
May	11·86	7·83	9·84	14·7	1·5	Nov.	14·43	8·63	11·53	18·6	1·6
June	11·07	6·91	8·99	15·8	1·8	Dec.	13·80	8·63	11·21	17·9	1·6

The ratios of the velocities of storm centers to the mean velocities of the wind, are quite different from those found for the United States, and the correspondence between the rate of storm movements and the movement of the wind is not as distinctly marked. Nevertheless some degree of correspondence can be detected, and it is noticeable that in Europe the change in the wind's mean velocity for the different months of the year is only about half as great as in the United States. The inequalities in the value of the ratio for the different months are considerable, and indicate the operation of some other cause than the mean velocity of the wind.

I next determined the average velocity of the wind in the neighborhood of the Bay of Bengal and China Sea. The following table shows the results which I have obtained, the observations being mostly derived from the Report on the Meteorology of India for 1882. I have employed only stations south of lat. 20° and I have rejected all stations having an elevation greater than 3000 feet.

	Coast Sta.	Inland Sta.	Mean.	Storms.	Ratio.		Coast Sta.	Inland Sta.	Mean.	Storms.	Ratio.
Jan.	6·00	3·79	4·89	----	---	July	9·12	7·67	8·38	8·36	1·0
Feb.	5·46	4·13	4·79	----	---	Aug.	8·29	6·58	7·43	10 30	1·4
Mar.	5·33	4·54	4·93	----	---	Sept.	7·46	5·50	6·48	9·77	1·5
April	5·71	5·00	5·35	7·54	1·4	Oct.	5·79	3·46	4·63	9·26	2·0
May	7·17	6·21	6·69	8·54	1·3	Nov.	5·25	3·50	4·37	7·38	1·7
June	9·12	7·83	8·47	5·62	0·7	Dec.	5·71	3·75	4·73	----	---

The number of coast stations is 17, and the number of inland stations is also 17. The velocities are all expressed in miles per hour. The ratios of the velocities of storm centers to the mean velocities of the wind differ sensibly from those found for Europe, and differ very greatly from those found in the United States. Moreover we find no correspondence between the average rate of progress of storm centers for the different months of the year, and the average velocity of the wind. The inequality in the values of the ratio for the different months of the year is quite noticeable, but this may be partly due to the small number of the observations.

I next endeavored to determine the mean velocity of the wind in the neighborhood of the West India Islands, but found observations for only seven stations south of lat. 27°. The following table shows the results for August, September and October, which are the only months for which there is an observation of more than one cyclone, with the exception of June, for which month there are three observations.

	Velocity.		Ratio.
	Wind.	Storms.	
August	5·79	14·44	2·5
September	5·98	14·00	2·3
October	6·70	12·81	1·9

I next determined the mean velocity of the wind for that part of the Atlantic Ocean in the neighborhood of the usual tracks of storm centers, and have adopted the results contained in No. 3 of the Mittheilungen aus der Norddeutsche Seewarte. The first line of the following table presents a summary of these results for the four seasons of the year, the force of the wind being estimated in units of Beaufort's scale (1–12).

	Winter.	Spring.	Summer.	Autumn.
Beaufort's numbers	5·9	5·5	4·5	5·3
Miles per hour	33·0	30·8	25·5	29·7
Storms	18·4	18·6	16·5	18·6
Ratio	0·5	0·6	0·6	0·6

The second line shows the velocities denoted by Beaufort's numbers, reduced to miles per hour according to the table prepared by the British Meteorological committee; the third line shows the average rate of progress of storms, and the fourth line shows the ratio of the numbers in the two preceding lines.

If we group together the results now obtained, we shall have the following summary for the average rate of progress of storm centers, the average velocity of the winds, and the ratio of these two velocities:

	Storms.	Winds.	Ratio.
United States	28·4	9·5	3·0
North Atlantic Ocean	18·0	29·8	0 6
Europe	16·7	10·3	1·6
West Indies	13·7	6·2	2·2
Southern Asia	8·4	6·5	1·3

This table appears at first view to present a discouraging medley of anomalies, but some of the anomalies may appear less formidable after a careful examination. It seems highly probable that the slow progress of storm areas in southern Asia is partly due to the small velocity of the winds of that region. It is not obvious why storms should travel more rapidly near the West India Islands than in the China Sea. It is possible that this anomaly may disappear when the mean velocity of the wind has been determined by a more extended series of observations.

It seems to be established that over the Atlantic Ocean the mean velocity of the wind is considerably greater than the rate of progress of storms. This inequality is strikingly exhibited in numerous cases. Over this Ocean, we frequently find an area of low pressure, 2000 miles or more in diameter, with a pressure of about 28 inches at the center, attended by winds blowing with hurricane violence, while from day to day the center of the low area makes little or no progress eastward, showing that the movement of the atmosphere which corresponds to the average system of circulation, is almost entirely interrupted over this ocean.

The most noticeable anomaly shown in the preceding table is however presented by the United States, where the mean velocity of the wind is only one-third as great as over the Atlantic Ocean, but storms travel with nearly double velocity. This anomaly may be partly explained if we admit that the progress of storms is determined, not by the wind which prevails in close contact with the earth's surface, but by that which prevails at an elevation of several hundred feet, where the velocity is probably much greater than at the earth's surface. The same anomaly however is found when we compare the storms of the United States with those of Europe. In northern Europe the surface winds have a velocity greater than those of the United States, and we may infer that the same is true for elevations of 1000 or 2000 feet above the surface; yet storms in Europe advance with but little more than half the velocity of those in the United States. There must then be a powerful cause which accelerates the movement of storm areas in the United States, and which does not operate in Europe or over the Atlantic Ocean; and apparently the same cause does not operate in southern Asia, or in the West Indies, at least in an equal degree. This cause (or one of these causes) is probably the precipitation in the form of rain or snow which, in the United States, usually takes place on the east side of a storm area, greatly in excess of that on the west side, as I have shown in my seventeenth paper; but for the interior of Europe the same does not appear to be true, or certainly not in an equal degree, as I have shown in my twelfth paper.

ART. II.—*Note on some Paleozoic Pteropods;* by CHARLES D. WALCOTT, *of the U. S. Geological Survey.*

IT is with considerable reservation that I place the genera Conularia, Hyolithes, Hyolithellus, Coleoprion, Coleolus, Hemiceras, Salterella, Pterotheca, Phragmotheca, Matthevia and perhaps Palænigma under the Pteropoda. They form a group that, although representative, in a measure, of the recent Pteropoda, differ in other respects so much that it appears as though a division of the Gasteropoda, equivalent to the Pteropoda, might be consistently made to receive them.

I have had in my possession for two years past the specimens on which the genera Matthevia is founded, and it is of such interest that I take the opportunity of publishing it before bringing out the illustrations of the fauna with which it is associated.

Genus MATTHEVIA, n. gen.

Shell conical, aperture sinuous, transverse section ovate, elliptical or rounded subquadrate; interior with two elongate chambers diverging from the apex and opening into a large, single, terminal chamber; both of the interior chambers are crossed by a single imperforate septum; calcareous; surface papillose. Operculum calcareous, nucleus excentric, lines of growth concentric.

Type *Matthevia variabilis.*

The generic name is proposed in honor of Mr. G. F. Matthew, who is doing so much good work on the St. John fauna.

This peculiar shell is so distinct from all described forms referred to the Pteropoda, that a new family, Matthevidæ, is instituted to receive the one genus now known.

In form and surface markings it approaches the genus Conularia; the operculum may be compared to that of Hyolithes, and the imperforate transverse septum allies it to both Hyolithes and Conularia. Its thick shell is observed in the genera Conularia, *C. fecunda* Barr. (Syst. Sil. Boheme, vol. iii, pl. viii, fig. 8), and Hemiceras, *H. cylindricus* Eichwald (Leth. Ross., vol. i, Atlas, pl. xl, fig. 17; pl. xlii, fig. 29). When we come to trace a relationship to the two inner chambers, we are, at once, at a loss for comparisons.

There is a curious form described as *Tetradium** *Wrangeli* Schmidt (Mem. Acad. Imp. Sci. St. Petersbourgh, VII. Ser.,

* The genus Tetradium being preoccupied (Dana, 1846; Safford, 1856), I propose Palænigma in place of Schmidt's Tetradium, 1874, for the species under consideration—*P. Wrangeli.*

vol. xxi, No. 11, p. 42, figs. 3–8, 1874), which Lindström suggests is, by the thick-shelled *Conularia fecunda* Barrande, linked to Conulariæ and made to stand in affinity to them. (Sil. Gasteropoda and Pteropoda of Gotland, p. 41, 1884.) From our comparisons Matthevia appears still more to serve as a connecting link between Palænigma and the genera Conularia and Hyolithes. If *P. Wrangeli* had chambers running up into the shell, as is suggested by the cross-sections, and a septum that caused the upper portion of the shell to be decollated, as we are led to believe by the natural section, and the fact that each specimen figured has lost its apex, the relations between Palænigma and Matthevia are quite close and Palænigma may be provisionally grouped with the genera Matthevia and Conularia.

Matthevia variabilis, n. sp.

On a side view, the outline of the shell varies from broad to narrow conical, and the end view shows an elongate conical to a broad conical outline; the cross-section varies from elliptical to oval or rounded quadrangular; aperture varies in outline with the proportions of the shell; a sinus, varying in depth and curvature, extends across the ends of the shell; in the more elliptical apertures the sides are nearly straight and parallel, while, in those with a subquadrangular outline, they are strongly curved, and the sinus at the ends is very profound. The shell thins out at the edges and is not thick over the exterior of the interior chambers, but between them a connecting mass of shell unites the sides and gives strength and solidity; a section, crossing the center of the shell at right angles to the preceding, shows a solid shell to the outer chamber where it gradually thins out to the margin. The position of the two inner chambers vary in relation to each other, from subparallel to widely divergent; the chamber that is more at right angles to the aperture than the other, is usually larger, and is always prominent, while the oblique chamber is sometimes filled up by shelly matter and only the outer portion remains; both chambers are usually flattened on the inner side, and more or less expanded where they enter the large outer chamber. The septum crossing the inner chamber is thin, and varies in shape with the form of the chambers; it is usually slightly concavo-convex, concave towards the outer chamber, and marked, usually, by a raised scar of varying character; the septum is usually a short distance from the outer chamber—1^{mm} to 4^{mm}.

The substance of the shell is calcareous.

Surface marked by undulating lines of growth parallel to the margin of the aperture, a few radiating lines usually on the sides, and fine papillæ, arranged in lines that cross each other at right angles, on some shells; on others, the papillæ are arranged in lines parallel to the lines of growth and without reference to the order of those in the adjoining lines; the interior surface is covered with a fretted work brought out by depressed, irregular, inosculating lines; this surface varies in force and character, and some shells are almost smooth inside; a narrow, smooth space extends all around the margin of the inside of the aperture.

The associated operculæ vary in form and outline; the shell is calcareous, concavo-convex, rising to a blunt point more towards one end than the other; from this point long, narrow, radiating undulations extend to the margin, and it is the center of the concentric undulations of growth. Surface with concentric and radiating undulations, fine, inosculating lines, subparallel to the concentric undulations, and fine papillæ on the spaces between the inosculating lines; interior surface convex, smooth or showing the undulations of the outer surface; at the center, corresponding to the apex of the outer surface, a small, round scar appears to be indicated on some specimens.

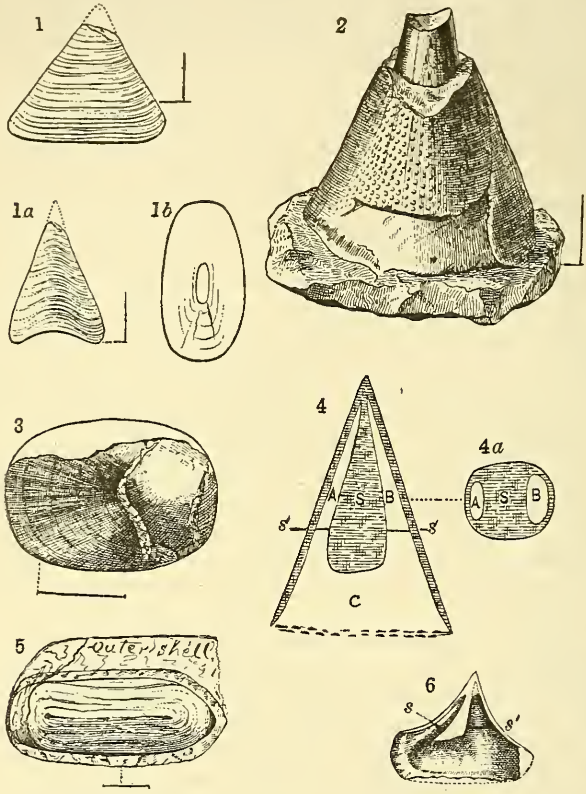
The only form known to me that corresponds in any way to this, is that figured by Eichwald (Leth. Ross., pl. xl, fig. 19, *a*, *b*, *c*), as *Hyolithes paradoxus*, which appears to be the cast of a portion of the outer chamber and one of the conical inner chambers; it may be only a superficial resemblance.

Formation and locality.—Cambrian. Limestone resting on Potsdam sandstone, one mile northwest of Saratoga Springs, N. Y.

The species is associated with *Cryptozoan proliferum* Hall (Thirty-sixth Ann. Rep. N. Y. State Mus. Nat. Hist., despt. of pl. vi, 1884), *Platyceras minutissima* W., *Ptychoparia* (L.) *calcifera* W., *Dicellocephalus Hartti* W., and *Ptychaspis speciosus* W. (Thirty-second Ann. Rep. N. Y. State Mus. Nat. Hist.)

Note on Hyolithes (*Camarotheca*) *Emmonsi* Ford.—When studying the species of Hyolithes from the Georgian Group, I found that the shell of *H. Emmonsi*, of Ford, was formed of three or more distinct layers: first, a thin outer layer with rather strong even striæ that cross the flattened ventral face nearly direct, and arch forward on the dorsal face, the flattened side, in this species, being the ventral face and not the dorsal, as in most species; the second layer appears to be of a smooth, even character, much like a filling between the outer and inner shell; the inner shell is thin and concentrically striated in a slightly different manner from the outer shell; a fourth layer appears to exist in one example, but it is too obscure for study.

Another character observed is one that, as far as I know, has not yet been noticed in any American species of the genus, although observed in some species of the genera Conularia and Matthevia. It is the presence of a transverse diaphragm in the tube towards the apex. This appears to have caused the shell



MATTHEVIA. DESCRIPTION OF FIGURES.

1, Side view of a small but characteristic form of the shell. 1*a*, End view of same. 1*b*, Outline, from the apex.

2, End view of one of the more circular shells enlarged to show the surface. The shell is broken away near the summit, and shows the cast of one of the inner chambers.

3, Operculum associated, in the same hand specimen of rock, with *M. variabilis.*

4, Longitudinal section of conical shell. *A*, *B*, inner chambers. *C*, chamber of habitation. *S*, thick shell between the inner chambers. *s'*, *s'*, position of the septa separating the chamber of habitation and the inner chambers. 4*a*, cross-section taken a little above the septa.

5, Enlargement of the surface of a septum.

6, Cast of the chamber of habitation and the inner chamber within the septa (*s*, *s'*) of the most common form of the shell.

to become deciduous in many instances, and we now find numerous examples showing the blunt terminal portion. Some shells show the rounded, smooth end without any constriction; others have a narrow concentric constriction just within the termination. The cast of the surface of the septum shows a slight central cicatrix or scar, but no evidence of a perforation in the septum could be observed. The average size of the tube, at the point of decollation, is 1^{mm}. The largest seen is $1{\cdot}5^{mm}$ and the smallest $\cdot 75^{mm}$.

When studying the septum, the close similarity between it and that of the first septum of the species of Orthoceras and Cytoceras, as figured by Barrande (Cephalopodes, Etudes generales, 1877, pls. 487, 488), were, at once, brought to mind, and also the interesting question of the relations of these shells to the cephalopoda.

Since the above was written I have received a paper from Mr. G. F. Matthew, in which he states that several of the Pteropod shells, from the base of the St. John group, have several distinct septa at the base of the tube. The genus and species is not mentioned (Nat. Hist. Soc. N. B. Bull., 10, p. 102, 1885).

Mr. Matthew quotes, in the same paper, from a letter written by Mr. Alpheus Hyatt, where the latter says: "These fossils with their distinct septa are startlingly similar to certain forms of Nautiloidea, but there is no siphon. They, however, confirm Von Jhernig's and my opinion that Orthoceratites and Pteropods have had a common, but as yet undiscovered, ancestor in ancient times."

In a letter dated April 9, 1885, Mr. Matthew states that he has several species of the Hyolithellidæ from the St. John Group that have several septa toward the apex of the tube for which he has proposed two generic divisions, one of which he calls Camarotheca; this will probably include *H. Emmonsi*, and I have used the name in a subgeneric sense, until I can learn more of the character of the septa of *H. Emmonsi.*

A single specimen of *H. primordialis*, from the Potsdam sandstone of Wisconsin, also shows evidence of a transverse septum.

NOTE OF CORRECTION.—In describing the figures accompanying the article on Linnarssonia (this Journal, vol. xxix, February, 1885, p. 117), figs. 1 and 2 are given as those of *Obolella chromatica*, and credited to Mr. Billings. The artist, in preparing the figures, copied, by mistake, those of *Obolella crassa* Ford. The fact escaped my notice at the time, and I unintentionally credited Mr. Billings with figures that had been published by Mr. S. W. Ford. C. D. W.

ART. III.—*A Determination of the B. A. Unit in Terms of the Mechanical Equivalent of Heat;* by LAWRENCE B. FLETCHER, Ph.D.

THE experimental work of the following investigation was completed in 1881, and forms the subject of a thesis submitted to the Johns Hopkins University in that year. In the present paper a more accurate method of calculating the currents from the deflection-curves is used, and some of the other calculations have been revised. The results of the two papers are substantially the same.

The experiment consisted of simultaneous thermal and electrical measurement of the energy expended by a current in a coil of wire immersed in a calorimeter. The result depends upon the values of the mechanical equivalent and the unit of resistance, and gives a determination of either in terms of an assumed value of the other.

The old determinations of Quintus Icilius and Lenz have no value, as the resistance is uncertain as pointed out by Rowland and H. F. Weber.

Joule,* in 1867, made a determination of the mechanical equivalent by this method, assuming the B. A. unit as determined by the committee in 1863–4 to be equal to 10^9 C. G. S. units. The value of the mechanical equivalent thus obtained is more than one per cent greater than Joule's water-friction value. H. F. Weber,† in 1878, used a similar method, employing the Siemens unit, the value of which he also measured in C. G. S. units. Weber's value of the mechanical equivalent is about one part in two hundred greater than Joule's water-friction value and one part in four hundred greater than Rowland's water-friction value.

In both Joule's and Weber's experiments a possible source of error seems to have been ignored. The wire was assumed to be at the temperature of the water in which it was immersed, and its resistance was calculated on this assumption. It is evident, however, that the wire was hotter than the water, inasmuch as it was giving heat to the water. The error due to this cause is of uncertain amount. If corrected for this error the value of the equivalent would be increased and their excess over the water-friction values would become greater than before. To avoid this source of error, the research described below was planned. The suggestion and general plan of the research I owe to Professor Rowland.

The theory of the method is as follows: A current c, flow-

* Report of B. A. committee on electrical standards, 1873.

† Phil. Mag., Series 5, vol. xxx.

ing through a wire of resistance R, for a time t, generates an amount of heat represented by $h = \frac{c^2 R t}{J}$ where J is the mechanical equivalent of heat. The wire being immersed in a calorimeter and put in a circuit with a galvanometer, h, c and t can be measured. Then if R is measured in B. A. units the experiment will give a relation between the value of that unit and the mechanical equivalent. In this research R was measured during the actual experiment by connecting its terminals with those of a large resistance R′ and measuring the current c', which flowed through the latter. With this arrangement $cR = c'R'$, or $R = \frac{c'}{c}R'$. Hence $J = \frac{cc'R't}{h}$, in which R does not appear, and the uncertainty attaching to its temperature has no effect.

The calorimeter was a cylindrical cup of sheet copper holding about 800^{cc}. On the bottom of the cup lay a sheet copper frame which supported three vertical glass rods. Around these the wire, R, was coiled, forming a helix. The ends of the wire were soldered to stout copper wires which, insulated by short vulcanite tubes, passed through the wall of the calorimeter and turned down so that they could be placed in mercury cups. The cover of the calorimeter rested in contact with the water to secure uniformity of temperature. The cover had an expansion tube and a smaller central tube which formed one bearing for the stirring apparatus, another bearing being given by a brass socket on the bottom of the calorimeter. The stirrer consisted of a spiral blade of sheet copper supported on a brass frame, the upper part of which was tubular, and passed through the central tube of the cover. The stirrer was kept in motion during the experiment by a silk thread, which passed over a vulcanite wheel at the top of the stirrer and ran to a driving clock. The stirrer formed the escapement of the clock, which ran very uniformly with this arrangement. I estimated the heat generated by the stirrer as two-thirds of the whole work of the weights. This is about one thousandth part of the heat generated by the current and only a rough determination of the correction is needed. The thermometer passed through the tubular upper part of the stirrer, and was clamped to a shelf above in such a manner that its bulb was in the centre of the calorimeter and surrounded by the stirring blade which, in turn, was surrounded by the wire which carried the current. The wire was composed of an alloy of platinum and iridium, and was varnished to prevent conduction to the water. Its resistance was about 1·8 ohm. The calorimeter was supported on legs of vulcanite within a copper vessel with double walls, the space between which was filled with water. This water-jacket was provided with a hollow cover, also filled with water, and its inner surface and the outer surface of the calorimeter were nickel-plated and polished. Thus the calorimeter was nearly surrounded by an envelope of fairly constant temperature, the thermometer, stirrer-thread and connecting wires passing through openings in the jacket.

From the mercury cups in which the electrodes of the calorimeter dipped, the wires of the main circuit ran to the battery and galvanometer. These wires were $2\cdot5^{mm}$ in diameter, cotton-covered, carefully paraffined and twisted together to eliminate direct action on the needle. The battery consisted usually of 24 one-gallon bichromate cells arranged 4 in series and 6 abreast, and gave a very steady current. In one experiment only 20 cells were used, 4 in series and 5 abreast. The galvanometer coil for the main current was a single turn of stout wire laid in a groove on a wooden circle of about 80^{cm} diameter. A sine galvanometer was so placed that its needle was in the axis of the single wire coil and about $\cdot1^{cm}$ distant from its plane. This excentricity was rendered necessary by the length of the suspending fibre. The coil of the sine galvanometer was connected with the calorimeter electrodes by a second circuit, in which a resistance coil of 30,000 ohms was included. The wires of this circuit were kept apart, as the current was too small to exert an appreciable direct action, and as great irregularity in some preliminary experiments in which the wires were twisted together was finally traced to leakage, although the wires had a double covering of silk. Both circuits were provided with commutators. The sine galvanometer had a horizontal bar parallel to the axis of the coil. To one end of this was attached a telescope, beneath which was a short scale which was seen by reflection in the mirror of the needle, and allowed a very accurate setting to be made without bringing the needle to rest. The needle consisted of two thin strips of steel $1\cdot2^{cm}$ in length separated by a piece of wood $\cdot6^{cm}$ in thickness. The circle of the galvanometer was graduated to half-degrees and read by verniers to one minute.

The needle was acted upon by both currents simultaneouly, and by means of the commutators the actions were caused to be in the same and in opposite directions alternately. The current through the sine galvanometer is c' in the formula $J=\frac{cc'R't.}{h}$ The current through the coil on the wooden circle is $c+c'$, and was assumed equal to c as c' was less than $\cdot00007c$. Let G denote the constant of the fixed coil, G′ that of the sine galvanometer, H the horizontal magnetic force, θ and θ' the deflections when the actions are in the same and in opposite directions respectively. Then

$$Gc\cos\theta + G'c' = H\sin\theta$$
$$Gc\cos\theta' - G'c = H\sin\theta'$$

Hence $c=\frac{H}{G}\tan\frac{1}{2}(\theta+\theta'),\ c'=\frac{H}{G}\ \frac{\sin\frac{1}{2}(\theta-\theta)}{\cos\frac{1}{2}(\theta+\theta')}$.

Let l denote the length of the wire in the fixed coil and b the distance of the needle from its plane. Then $G=\frac{4\pi^2}{l\left(1+\frac{6\pi b^2}{l^2}\right)}$.

Hence the equation for J becomes

$$J=\frac{R'l\left(1+\frac{6\pi^2 b^2}{l^2}\right)}{4\pi^2 G'}\ \frac{H^2 t}{h}\ \frac{\tan\frac{1}{2}(\theta+\theta')\sin\frac{1}{2}(\theta-\theta')}{\cos\frac{1}{2}(\theta+\theta')}$$

I shall discuss in order the quantities contained in this expression.

R′, the resistance of the secondary circuit, is the sum of the resistances of the 30,000 ohm coil, the sine galvanometer and connecting wires. The whole was measured by connecting the terminals of the circuit with a Jenkin bridge and comparing with other coils, using a high resistance Thomson galvanometer. The provisional standard was a 10 ohm coil, A, made by Warden, Muirhead and Clarke. From this coil the resistance of a 100 ohm standard, B, was obtained by means of a comparator, C, of 10 coils, each nearly equal to A, each coil of C being compared with A, and C in series then compared with B. Then A, B and C were arranged to form a bridge with D, a 1,000 ohm standard, whose resistance was thus fixed. E and F, two 1,000 ohm coils of a resistance box, were then compared with D. Finally a bridge was formed with A, B, D+E+F, and R′, the secondary circuit, giving R′ in terms of A. Elliott's coils were used in making the adjustments, which were always very small, and the temperatures were carefully observed. The result is

$$R'=30012{\cdot}4 \text{ at } 19^\circ{\cdot}3 \text{ C.}$$

R′ consisted principally of the 30,000 ohm coil and the variation of this only need to be considered. Its temperature varied from 19° to 24° when in use, the mean temperature being 22·°3 C. At this temperature, R′=30052, which value was used throughout.

The length of the wire in the fixed coil was determined by measuring with a steel tape the distance between two threads fastened on the wire before it was placed on the circle. When the wire was in position, the interval of a few centimeters between the threads was measured. The tape had been compared with standards. Care was taken to avoid difference of tension in the two positions of the wire. The result is $l=264{\cdot}49^{cm}$.

The quantity b, the excentricity of the needle, was estimated by holding the tape horizontally over the top of the circle and reading the positions of the center of the wire and the galvanometer fiber. For most experiments $b=1{\cdot}2^{cm}$. It was frequently re-measured and a correction applied when it varied. The method of measurement is not very accurate, but an error of 10 per cent in b, which could hardly occur, would only involve an error of 1 part in 3,000 in J.

G′ had been determined by Professor Rowland* by measurement during the construction of the coil, and also by comparison with another coil. The values are 1832·24 by measurement and 1833·67 by comparison. The mean, giving the second value twice the weight of the first, is 1833·19.

Hence the constant term $= 10996+10^7$.

G′ has recently been re-measured and found to be 1832·53. My final result is corrected to this value.

H was measured in the following manner: The circle bearing the fixed coil carried four smaller wires which could be connected with the battery and an electrodynamometer of the form described in Maxwell's treatise. These four wires with the needle formed a tangent galvanometer, the other coils being open. Eight pairs of simultaneous readings of the galvanometer and electro-dynamometer were taken comprising all possible combinations of signs of the currents in the galvanometer and the two electro-dynamometer coils. I am greatly indebted to Professor S. H. Freeman, then Fellow of the University, for assistance in these readings.

The expression for H is

$$H=\frac{4\pi^2 nC\sqrt{I}\left(1-6\frac{\pi^2 b'^2}{l'^2}\right)}{l'}\;\frac{\sqrt{\sin\alpha}}{T\tan\varphi}$$

where C is a function of the dimensions of the electrodynamometer coils, I the moment of inertia of the suspended coil, n the number of turns of wire in the galvanometer, l' the mean length of one turn, b' the mean distance of their planes from the needle, T the time of vibration of the small coil, α and φ the mean deflections of electrodynamometer and galvanometer.

C was known from measurements during the construction of the instrument, and I had been determined by observing times of vibration with and without the addition to the suspended coil of bars of known moment of inertia. These values of C and I had been verified by Dr. E. H. Hall and myself in connection with a previous research by comparing the values of H obtained by this method and by the magnetic method, the arrangement of the experiment being such as to make the

* This Journal, xv, 337, 1878.

two results obtain for the same point and time. The value of $C\sqrt{I}$ is 0·18567.

The measurements of l' and b' were made in the same manner as those of l and b.

The results are $l' = 263·91\ cm$, $b' = 2·07\ cm$, for most experiments. A correction was applied when b' varied. Hence

$$\frac{4\pi^2 n\, C\sqrt{I}}{l'}\left(1-6\frac{\pi^2 b'^2}{l'^2}\right) = ·11069.$$

Each of the angles, α and φ, is the mean of eight readings taken to 1′. The former was about 13°, the latter 6°. T was obtained by observing ten transits with a seconds clock, allowing the coil to vibrate for several minutes and then taking ten more transits. The difference between the mean times of the two series divided by the number of vibrations gives T very exactly. The difference between the values before and after the experiment never exceeded 1 part in 3000. The mean value is about 2·42 seconds. H was determined before and after the main experiment.

The quantities in the formula for J remaining to be discussed are t, h and the deflections. To treat these intelligibly I proceed to describe the method of experiment exactly.

First, a determination of H was made. The calorimeter was then weighed, filled with distilled water at a temperature usually 2° or 3° below that of the air, carefully wiped with a towel to remove moisture, again weighed and placed in the water-jacket. Its amalgamated electrodes were placed in the mercury cups with the terminals of the two circuits, the main circuit being broken at the commutator. The water-jacket was kept permanently filled and stood in a room of fairly constant temperature so that its temperature changed little during the experiment. The thermometer was placed in position and the stirrer started. During a few minutes readings were taken of the thermometer and of three auxiliary thermometers, giving the temperatures of the jacket, the 30,000 ohm coil and the air near the stem of the principal thermometer, the time of each reading being noted by a seconds clock. The circuit was then closed and a galvanometer reading taken, one of the commutators was reversed and another reading taken, the time of each reading being noted. The time of passage of the mercury of the thermometer over several successive scale-divisions was then taken, also readings of the other thermometers. Two more commutator reversals and galvanometer readings followed, then another set of thermometer readings, and this alternation was continued for about 40 minutes, during which time the thermometer rose about 12° C. Usually sixteen galvanometer readings were taken and seven groups of thermometer readings comprising 35 or 40 readings of the principal thermometer. Then the circuit was broken and the calorimeter allowed to cool for two or three hours, during which time groups of readings were taken as before, the stirrer being kept in motion. While this radiation experiment was in progress another determination of H was made. Finally the thermometer was removed and the calorimeter taken out and weighed.

The mean of each group of thermometer readings, corrected for stem error, gives very exactly the temperature of the thermometer for the mean time of that group. The difference between any two of these mean temperatures, corrected for radiation, gives by multiplication into the capacity of the calorimeter and contents the heat generated in the interval. Hence any two groups give a determination of J when combined with the proper values of θ and θ'.

I have combined groups taken 18 to 25 minutes apart, the rise of temperature being 6° to 8°.

In this calculation the differences of temperature of coil, water and thermometer are assumed to be constant for this interval. The water is cooler than the coil and the thermometer cooler than the water. Both differences depend upon the rate of generation of heat, and may be put approximately proportional to the square of the current. The rise of the thermometer after breaking the circuit is due to these differences and was found to be less than 0·°05. The variation of this quantity during the interval in question would be about 3 per cent, as the current changed 1·5 per cent. Hence the variation is 0°·0015, and as the rise of the thermometer is 6° or 8°, the error is negligible.

Two thermometers were used, designated as Baudin 6165, and Baudin 7320. The former is graduated in millimeters, of which about 12 equal 1° C. It had been used by Professor Rowland in his determination of the mechanical equivalent and compared several times with the air thermometer. Baudin 7320 is graduated to 0°·1 C. one degree occupying about a centimeter. It had been compared with standard thermometers, its errors plotted and the error for each degree obtained from the curve. The following tables give the reduction to the absolute scale.

The table for 6165 is condensed from Professor Rowland's paper* on the mechanical equivalent. Change in the zero point has no effect on the differences of temperature used, but the zero points were determined occasionally in order to get the mean absolute temperature.

The correction for radiation was made in the following manner: The groups of thermometer readings taken after breaking the circuit were reduced to mean readings at mean times. Any two of these mean readings gave the radiation for the

* Proceedings of the American Academy of Arts and Sciences, 1880.

intervening time. If t' and t'' are the temperatures at the beginning and end of an interval of T minutes, and τ is the mean temperature of the jacket during the interval, then $t'-t''=c\mathrm{T}[\frac{1}{2}(t'+t'')-\tau]$, where c is the coefficient of radiation. In the calculation of c, stem-corrections were applied and a correction made for the heat generated by the stirrer. Hence in the main experiment the temperature correction for an interval T$'$ is $\Delta=c\mathrm{T}'[\frac{1}{2}(s'+s'')-\tau']+\mathrm{K}$, where s' and s'' are the observed temperatures corrected for stem-error, τ' is the mean temperature of the jacket and K is the stirrer-correction. The sum of the corrections Δ from the beginning of the experiment added to the stem-corrected observed temperature at any point gives the temperature which would have been reached in the absence of radiation. The difference between any two of these theoretical temperatures multiplied by the heat capacity, gives the heat generated in the interval.

BAUDIN 6165.

Reading in mm.	Temperature on absolute scale from 0° C.	Reading in mm.	Temperature on absolute scale from 0° C.
35	0°·0	320	24°·547
50	1°·368	330	25°·365
100	5°·839	340	26°·174
150	10°·183	350	26°·981
200	14°·450	360	27°·782
250	18°·709	370	28°·584
260	19°·557	380	29°·376
270	20°·401	390	30°·170
280	21°·242	400	30°·965
290	22°·076	410	31°·786
300	22°·907	420	32°·581
310	23°·731		

BAUDIN 7320.

Reading.	Temperature on absolute scale from 0° C.	Reading.	Temperature on absolute scale from 0° C.
0°	0°·122	23°	23°·108
5°	5°·092	24°	24°·122
10°	10°·110	25°	25°·137
15°	15°·090	26°	26°·152
16°	16°·093	27°	27°·166
17°	17°·094	28°	28°·179
18°	18°·091	29°	29°·192
19°	19°·086	30°	30°·205
20°	20°·081	31°	31°·217
21°	21°·085	32°	32°·230
22°	22°·095		

The coefficients of radiation were found to decrease with decreasing difference of temperature between calorimeter and jacket. When this decrease was regular the corresponding value of c was used for each small interval of the main experiment. When the decrease was small and irregular, the mean value of c for that day was used throughout. In the revision of the calculations, stem and stirrer corrections were neglected in the calculation of both c and Δ, it being obvious that, both being small and quite regular, they are eliminated in this way, and the value of c corresponding to the difference between calorimeter and jacket for each small interval of the main experiment was used in all cases. The mean results of the two methods differ about 1 part in 1,000, and the figures in the table of results below are the means of both calculations

The mean values of c for the different experiments vary between 0°·0035 and 0°·0046, the general mean being 0°·0040.

The mean radiation correction is about 5 per cent, and is the most important source of variable error in the experiment, as the temperature differences are small and errors of reading have a large effect. But a ten per cent error in the radiation would only involve an error of 1 part in 200 in J, and as the errors are irregular they are in a great measure eliminated from the final result.

The calorimeter was composed of 246 gr. copper, 45 gr. brass, and 6 gr. solder. The specific heat of a mixture of these proportions was measured with Regnault's apparatus. Six determinations gave the value ·0899±·0005 for the mean specific heat between 24° and 100°. Reduced by Bède's law for copper to the mean temperature of my experiments, it becomes ·0877. The capacities of the coil and glass rods were calculated from published tables. The whole capacity is as follows:

Calorimeter	302·1 × ·0877 = 26·49
Coil	32·5 × ·0324 = 1·05
Glass rods	9·0 × ·177 = 1·59
Thermometer estimated at	1·25
Total capacity	30·4

The values of the deflections were obtained by a graphical method. The galvanometer readings fell into 4 groups, lying about 26° and 3° 45′ on each side of the zero point. The readings of each group were plotted separately as functions of the time. From each curve the theoretical mean readings for each interval between two temperatures used in the calculation of J were obtained by measuring a large number of equidistant ordinates, calculating the area of the curve and dividing by the base line. If a, b, c and d are the mean readings thus obtained, $2\theta=a-b$ and $2\theta'=c-d$ as the galvanometer was graduated from 0° to 360°. Thus the zero point was not used, though observed before and after each experiment.

Below are the results of one experiment in detail. Each value of J is calculated from the two temperatures found in the same horizontal line.

Series of December 9th.

	gr.
Weight of calorimeter and water before experiment	1157·2
Weight of calorimeter and water after experiment	1157·0
	1157.1
Weight of calorimeter	343·7
	813.4
Capacity of calorimeter	30·4
	843·8
Capacity reduced to weight in vacuo	844·8

Horizontal magnetic force before experiment	·1960
Horizontal magnetic force after experiment	·1963
Mean	·19615

Temperature of jacket	21°·5 to 21°·6
Temperature of air near stem	22°·8 to 23°·4
Temperature of 30,000 ohm coil	21°·5 to 22°·3
Thermometer in calorimeter	Baudin 7320

Time.	Reading 7320.	Stem.	ΣΔ.	Time.	Reading 7320.	Stem.	ΣΔ.	$2\theta°$.	$2\theta'$.	J.
m. s.				m. s.						
8 40	19°·45	−·010	0	25 14	25°·40	+·009	+·056	51° 8′·4	7° 35′·6	41810000
13 16	21°·15	−·007	−·018	31 9	27°·40	·021	·167	50° 57′·5	7° 28′·5	41640000
20 38	23°·80	·000	+·005	35 58	28°·98	·029	·299	50° 48′·5	7° 24′·4	41640000
25 14	25°·40	+.009	·056	42 54	31°·15	·039	·565	50° 40′·4	7° 21′·4	41840000

Time.	Galvanometer.	Time.	Galvanometer.	Time.	Galvanometer.	Time.	Galvanometer.
m. s.		m. s.		m. s.		m. s.	
7 50	237° 14′	15 40	237° 11′	28 5	237° 5′	39 0	237° 2′
7 0	259° 12′	17 5	258° 56′	29 15	258° 46′	41 0	258° 40′
10 20	229° 34′	22 0	229° 40′	32 45	229° 42′	44 15	229° 44′
11 30	207° 42′	23 25	207° 58′	34 0	208° 0′	45 25	208° 4′

Following is the general table of results:

Date.	Nov. 24.	Dec. 9.	Dec. 14.	Dec. 20.	Dec. 22.	Jan. 26.	Feb. 16.
Thermometer	7320	7320	6165	6165	6165	6165	6165
Temp. of water	24°·3	25°·4	27°·0	26°·6	26°·3	27°·2	26°·2
Temp. of R′	23°·0	22°·1	23°·8	21°·7	23°·4	19°·1	20°·6
$\frac{J}{10000}$	4240 4248 4229 4274 4216 4217 4204 4225 4225 4224	4181 4164 4164 4184	4180 4222 4217	4207 4195 4206 4204 4182	4198 4216 4232 4218	4194 4192 4205 4193	4207 4219 4174
Means	4230	4173	4206	4199	4216	4196	4200

The greater number of results on Nov. 24 is due to the fact that single thermometer readings were taken instead of groups. The two experiments with 7320 show the greatest variation from the mean, but the mean of these two agrees closely with the general mean. Experiments made on Nov. 29 and Dec. 6 were rejected on account of leakage of 3 and 6 grams respectively. The results, however, are 4220 and 4227, falling within the limits of the series. The duration of the experiment was less than one-fifth of the interval between the two weighings of the calorimeter, and probably the loss during the experiment was the same fraction of the whole loss. Furthermore, the leakage during the radiation experiment would affect the radiation coefficient in such a manner as to approximately compensate for the effect of leakage during the main experiment. For these reasons I have retained the experiments of Jan. 26 and Feb. 16, which showed a leakage of 1 gr. and 1·5 gr. respectively, but have given the results half the weight of the others. The remaining experiments are satisfactory in this respect, the loss being a few tenths of a gram, due principally to the removal of the thermometer.

The result of the experiment is J=42,039,000 O, where O is the value of one-tenth of the 10 ohm coil in earth-quadrants per second. Reduced to the new value for the constant of the sine galvanometer, it becomes J = 42,055,000 O.

I have also calculated the experiment from the formula $J=\frac{c^2Rt}{h}$, where R is the resistance of the calorimeter coil as measured in the ordinary manner, corrected to the mean temperature of the water and further corrected for superheating. I estimated the superheating from observations of the main and derived currents when the strength of the former was varied. The expression $\frac{c'R'}{c}$ should give the true resistance of the coil at any instant. When the currents are smaller the superheating is less and the comparison of the value of $\frac{c'R'}{c}$ for a zero current, obtained by graphical extrapolation, with its value for the full current, should give the superheating. The method is not very accurate, as the observations with the smaller currents are uncertain. I find the increase of resistance due to superheating to be about 1 part in 700, corresponding to a difference of temperature of 2° C. When this correction is applied the second method of calculation gives J= 42,156,000 O.

This result depends upon the square of the main current, and as the temperature of the coil changed 6° or 8° during the experiment, its mean resistance is somewhat uncertain. Hence this result has not the weight of the former.

The discovery of this discrepancy has greatly delayed the publication of this paper. It may be due to conduction to the water, which was guarded against by varnishing the wire and using distilled water, but was not proved not to exist.

For, let E be the difference of potential of the ends of the coil, e the E. M. F. of polarization, R and r the resistances of coil and water respectively. Then the current in the coil is $C=\frac{E}{R}$ and the current in the water is $c=\frac{E-e.}{r}$

The energy converted into heat is

$$C^2R+c^2r=\frac{E^2}{R}\left[1+\frac{R}{r}\left(1-2\frac{e}{E}+\frac{e^2}{E^2}\right)\right]$$

In the first method of calculation above the energy is computed as

$$E(C+c)=\frac{E^2}{R}\left[1+\frac{R}{r}\left(1-\frac{e}{E}\right)\right]$$

In the second method it is computed as

$$(C+c)^2R=\frac{E^2}{R}\left[1+\frac{2R}{r}\left(1-\frac{e}{E}\right)\right]+\text{smaller terms.}$$

E was over 6 volts, e is about 1·5 volts. Hence the second result is larger than the first, which agrees with the facts, and the *error* of the first is less than one-fourth of the difference between the two. The discussion shows that the first method of calculation is to be preferred, and I therefore take J = 42,055,000 O as the result.

Since the completion of my experiments, a 10 ohm Elliott standard in the possession of the University has been compared with the Cambridge standards and found correct at 20°·9 C. My standard has been compared with this with the following result:

$$\frac{\text{W. M. \& C.'s coil,}}{\text{Elliott's coil,}} = 1{\cdot}00168 \text{ in } 1878$$

$$\text{" \quad "} = 1{\cdot}00170 \text{ " } 1882$$

$$\text{" \quad "} = 1{\cdot}00173 \text{ " } 1883$$

In these comparisons the Elliott coil was taken at 16°·3 C., as marked. Also we have

$$\frac{\text{Elliott coil at } 20^\circ{\cdot}9}{\text{Elliott coil at } 16^\circ{\cdot}3} = 1{\cdot}0014$$

Hence $O=\frac{1{\cdot}0017}{1{\cdot}0014}=1{\cdot}0003$ B.A. units and J = 42,068,000 × value of B. A. unit in earth-quadrants per second.

Rowland* has discussed Joule's values and reduced them to the air thermometer and the latitude of Baltimore. The mean of the best results from the friction of water, in 1850 and 1878, thus becomes 426·55 kilogram-meters or 41,805,000 C.G.S. at 14·1° C. This, according to Rowland's results for the tem-

* Proceedings of American Academy of Arts and Sciences, 1880.

perature variation, corresponds to 41,608,000 at 26°, the mean temperature of my experiments. Rowland's value at 26° is 41,720,000. Combining the mean of these, 41,664,000, with my result, I find 1 B. A. unit $=\frac{41,664,000}{42,068,000}=$ ·9904 earth-quadrants per second.

This research cannot compare in weight with the elaborate determinations of the ohm by direct methods, which have been made in England and this country since the conclusion of my experiments, but as few results by this method are at hand, I publish it as a slight contribution to the history of this vexed subject.

Marlborough, N. Y., April 15, 1885.

ART. IV.—*Cause of Irregularities in the Action of Galvanic Batteries;* by HAMMOND VINTON HAYES and JOHN TROWBRIDGE.

IN the May No. of this Journal, 1885, is described an apparatus devised by Prof. John Trowbridge, for photographing the deflections of a galvanometer needle. A spot of light is reflected from the mirror of a galvanometer, and from a fixed mirror, on to a sheet of sensitive paper. When no current passes, the two spots of light coincide; when the mirror is deflected, one spot marks the zero of the scale; the distance between the two spots shows the amount the mirror has been deflected. In this way all variations of current are accurately registered. We have tested a number of batteries in this way, and have found that in some cases the current was comparatively constant, or if any variation occurred, it was of the nature of a gradual and regular fall. Examples of this action are to be seen in figures 1, 2 and 3. In other cases the action was exceedingly irregular; not only were there many marked variations in the strength of the current, but these variations were made up of a multitude of minor fluctuations. Both of these actions can be observed in figures 4 and 5. The variations in some cases are as great as twenty or thirty per cent of the total strength of the current. It seems, therefore, of interest to find to what causes these changes may be assigned, especially as such variations would seriously affect delicate experiments. Moreover, in batteries used for incandescent lighting it is absolutely necessary to obviate this difficulty.

It was observed that batteries without a porous partition were not subject to these fluctuations. Such batteries as the Leclanché exhibit smooth unserrated curves, whereas all batteries employing porous cups show irregularities more or less marked. It will be noticed that the variations may be divided into two classes, one a general undulation, the other a series of rapid fluctuations which make up the undulations. There are, likewise, two separate actions which cause these irregularities: first a diminution in the current strength, caused by the pores of the partition becoming filled with the base, and thus preventing action until it has been dissolved and fresh acid can attack the zinc; second, a diminution of the acid at the positive pole, and a consequent decrease in current. This action is due to electrical endosmose. The undulations are due to the first of these causes; the fluctuations to endosmose. It will be observed that the fluctuations in all cases begin as soon as the current is made, and before it is possible that the partition can have become so impregnated as to cause an interruption to the current. This can be well seen in figures 4 and 5. This shows that the fluctuations and undulations must be due to different causes. In figure 4 the undulation is very marked. This figure is the photograph of the action of a battery using a very dense porous cup. Figure 3 shows the action with a cup, the same size as that used in figure 4, made of ordinary unglazed paper. We find here no action whatever. We should suppose from the above experiments that the more dense the partition the more liable would it be to become clogged, whereas with a very porous cell there should be very little resistance offered. To prove this, a cup of very porous earthenware was compared with one much denser; figures 7 and 8 show the results. In the case of the porous cup the action is without undulations, while with the other the undulations are quite marked.

To investigate the action of endosmose, a very irregular open circuit battery was employed, consisting of a solution of bichromate of potassium and sulphuric acid, into which were plunged a piece of carbon and a porous cup containing zinc surrounded by mercury. In the action of a galvanic battery there are three separate actions which take place:

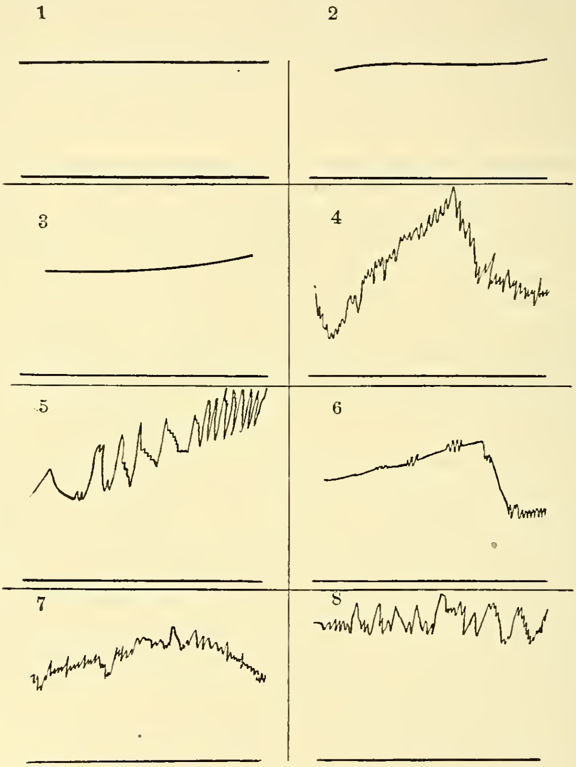
1st. A decomposition of the electro-positive ion at the positive electrode.

2d. A decomposition of the electro-negative ion at the negative electrode.

3d. The electric current carries whatever comes in its way from the positive to the negative electrode.

It is this last which is called endosmose, and which we wish to investigate.

It was discovered by Breda and Logeman that in a continuous liquid this third action disappeared, and only when a porous partition was introduced was this phenomenon observed. This agrees with the above stated theory; for no such fluctuations are observed in single fluid batteries. The laws which govern this action were very carefully studied by Wiedemann. Owing to this transporting force the heights of the liquid on the two sides of the porous jar are different, being higher on the side which is nearest the positive electrode. When a strong current acts, more liquid is driven through the partition than with a weak current; moreover, the greater the resistance of the liquid, the more is driven through. From this we find that the



porous jar increases the base or metal transported to the negative pole, and diminishes the quantity of acid at the positive pole in the case of sulphuric or nitric acids. Again, if we increase the surface of the jar, the force tending to transport the liquid is diminished, but it is increased if we increase the thickness or density of the partition.

If now we have a strong current, and a small, thick cup, there will be a maximum force tending to drive the liquid and base from the positive pole, and a consequent decrease in the strength of the current. The first case is well exemplified in figure 4, in which the fluctuations are very marked. The cup used was very small and dense. Figure 3 shows the action of a cell of ordinary unglazed paper of large dimensions. The current, which was as nearly as possible of the same strength as in figure 4, is perfectly uniform. Figure 1 is a photograph of the action of a cell made of a paper known as parchment paper. We again find a regular action. Wishing to make a slightly more dense cell, one was constructed of parchment of the same size as those employed above. The action of the battery with this cell is shown in fig. 6. It will be noticed that very slight fluctuations occur at the beginning, and in parts of the line.

Figure 7 shows the action of a cell presenting a large surface, but made of very dense earthenware. In this way great advantage is gained, for if the cell had been of the ordinary size, its fluctuations would have resembled those in figure 4.

We can, therefore, say that there are two causes of irregularity in the action of galvanic batteries, and that both difficulties are overcome by making a partition of as large surface dimensions as possible, and by using very porous material.

Jefferson Physical Laboratory.

Art. V.—*On the Sensitiveness of the Eye to Colors of a Low Degree of Saturation*;* by Edward L. Nichols, Ph.D.

[Read at the Philadelphia meeting of the American Association for the Advancement of Science.]

Every one who has had occasion to mix colors has noticed that an exceedingly small amount of any pigment will impart its hue to a very large quantity of white. One part of red lead, for instance, will color a million parts of a white powder like the carbonate of magnesium, and even a smaller proportion than that is distinguishable by the average observer, as will appear from the experiments to be described in this paper.

This observation is strikingly at variance with the results obtained by other methods of mixing colors. It has been shown, for example, by Aubert† that a disk, less than $\frac{1}{360}$ of which is painted (radially) with any pigment, the remainder being white, cannot when in rotation be distinguished from an entirely white disk.

We have attempted to measure the sensitiveness of the eye in this respect, by determining the smallest proportion of various coloring matters which, when mixed with a white powder, will

* This is one of a series of researches on the special senses by E. H. S. Bailey and E. L. Nichols.

† Rood: Modern Chromatics, p. 39.

give it a perceptible tint. The pigments selected were red lead, chromate of lead, chromic oxide and ultramarine blue. These were in the form of powder, the red and blue being the red lead and artificial ultra-marine of commerce, whereas the chromium compounds were freshly prepared by precipitation. Each of these pigments was mixed with white in the following manner. About ten cubic centimeters of the powder was mixed in the dry state with an equal volume of magnesium carbonate, the mixture was divided into two equal parts, half of it was again mixed with its own volume of the white powder, the product was again subdivided and the process of mixing with white by equal parts was repeated until all traces of color had disappeared. Since at each stage of the process only half the material was used for further dilution, there remained a series of colored powders of which the pure pigment formed the first, while the succeeding numbers were of less and less saturated hue, and finally could not be distinguished from white. These mixtures were put into small vials of white glass and labelled in such a manner as to ensure their recognition by persons acquainted with the code and at the same time to preclude the detection of the nature of their contents from the label, without such knowledge.

For the purpose of ascertaining the degree of saturation at which the presence of these pigments becomes perceptible to the eye, the four sets of bottles, containing mixtures of red and white, yellow and white, green and white, and blue and white, were mingled indiscriminately, and the observer whose eye was to be tested was requested to arrange those in which he could detect any trace of color, according to hue and degree of saturation. The bottles were afterwards inspected by some one acquainted with the code of labels, who threw out those not in the proper set and recorded the number of bottles remaining in each set and the number of each color which had been properly placed as to shade. From the former record the sensitiveness of the eye to colors of low saturation was determined; the latter data served to indicate the ability of the observer to detect small differences of shade.

Fifty four persons, all of them with two or three exceptions between the ages of fifteen and thirty, were examined in this way. The Holmgren worsteds had shown one of them to be completely green blind, three partially so and one partially red blind. Color-blindness was not found to affect in any marked way their ability to classify the colors.

This method of measuring the sensitiveness of the eye is not in all respects satisfactory. A method in which pure spectral tints mixed with white light could be compared with a field illuminated by white light alone and the amount of monochromatic light lessened until its effect was lost to the eye, would certainly be better; but ease and rapidity of execution were essential where so many individuals were to be tested and where the time of those who kindly presented themselves for the purpose was limited. Moreover, the study of spectral tints would not give results directly applicable to pigments, and it is the latter with which we have to do in many practical problems in the science of chromatics. An exhaustive study of this subject would involve the use of both methods.

Table I gives the general results of the fifty-four tests. The averages for males and for females are given separately for purpose of comparison. The numbers indicate in each case the amount of coloring matter present in one hundred million volumes of white, in the most dilute mixture which can be distinguished from a pure white by the average observer.

TABLE I.

Number of parts of coloring matter that must be mixed with 100,000,000 parts of white in order to affect the tint of the compound.

	Red lead.	Chromate of lead.	Chromic oxide.	Ultramarine
Average for 31 males	15·9	17·3	817·7	148·5
Average for 23 females	59·8	33·2	913·6	108·1
Average for both sexes	25·2	23·9	864·2	126·5

The popular impression that in woman the special senses are more finely organized and delicate than in man,* a view considerably strengthened so far as color-perception is concerned by her well authenticated exemption from color-blindness, finds no support from these experiments. As will be seen from the above table the average male observer is measurably more sensitive to red, yellow and green, while the female shows superiority in the blue alone. Quite as interesting, perhaps, is the manner in which the relative sensitiveness of the eye varies with the wave-length. If the corresponding data for mixtures of white and monochromatic light were obtainable it would be possible to indicate by curves the variations of the sensitiveness of the eye in this particular. The light reflected by pigments, however, is so far from being monochromatic† that it is out of the question to attempt to assign them any place in a pure spectrum, and curves constructed upon the assumption that pigments are representative of definite wave-lengths would be of interest only as illustrating in a very imperfect way the general character of the curves which might be obtained by a more precise method.

* Some experiments upon the sense of smell, carried on at the same time as and partly in connection with the tests described in the present paper, indicate that in the case of many common odors also, delicacy of perception is much more marked among men than among women. (E. H. S. Bailey: Proceedings of the Kansas Acad. of Sciences, 1884.)

† See "A spectro-photometric study of pigments," American Journal of Science, vol. xxviii, Nov., 1884.

The variation from these averages, in the case of individual observers, was very marked. Of the fifty-four persons tested, eight (five males and three females), could distinguish the presence of yellow in a mixture of three parts of that pigment in one hundred millions, while two individuals, both of them females, failed to detect it in mixtures containing less than one hundred and ninety parts in one hundred millions. Like differences were met with in the sensitiveness of the eye to other colors, and the relative sensitiveness to different colors was not the same for all observers.

The lack of delicacy in respect to green was a very general trait. Only three observers were as sensitive to green as to blue, and in the case of but one individual was the power of detecting the former color equal to the sensitiveness to yellow. The thought suggests itself that the failure to detect green may be due to a blunting of the nerves which respond to that color by continual exposure to green foliage. An investigation of the relation between the sensitiveness of the eye to colors mixed with white and the form of the three primary color-curves of the eye would add to our knowledge of this subject.

The striking discrepancy between these results and those obtained by the method of rotating disks, the eye recognizing with ease and certainty one part of coloring matter in many millions when mechanically mixed with white, and failing to detect one part in a few hundred parts (i. e., 360 parts) when mixed by rotation, shows, in our opinion, that the eye while watching a revolving disk is in an abnormal condition, and that quantitative results obtained by this favorite method of combining colors are not always comparable with those which we get by the actual mixture of white and colored light, or by the mechanical mixture of pigments. In view of the large number of researches upon Chromatics and Physiological Optics in which the revolving disk has been used, a special study of the condition of the eye during the observation of the disk, and a comparison of the results of this method with those obtained in other ways is greatly to be desired. In this manner alone can the limits of usefulness of this exceeding simple and convenient method be determined.

The tests of the power of recognition of small differences of shade were undertaken chiefly as a further means of comparing the attainments of the sexes in delicacy of color perception. The method was not adapted to the direct determination of the smallest difference of saturation which can be perceived, but our experience with the series of colors already described showed that the neighboring members were quite as closely allied in shade as was compatible with their recognition. Indeed, of fifty-four observers not one succeeded in placing all the vials, the colors of which were perceptible to him, in their proper places in the series. The two nearest approaches to complete accuracy consisted in the correct arrangement of ninety bottles out of ninety-two in the one case and of ninety out of ninety-three in the other. Although these records were made by male observers, the *average* for the other sex was noticeably higher than that of the males. It was found that of all the mixtures possessing appreciable color the average observer of each sex placed the following proportions correctly:

TABLE II.

Average accuracy of male and female observers in detecting the degree of saturation of mixtures of pigments with white. (Complete accuracy would be indicated by 100·00.)

	Red lead.	Chromate of lead.	Chromic oxide.	Ultramarine.
Males	86·86	87·16	92·81	78·13
Females	90·81	93·24	98·28	82·92

A comparison of tables I and II shows that the color (green) to which the eye is least sensitive, so far as the ability to detect small amounts of color is concerned, is the one in which the least difficulty is met with in noticing differences of shade. Possibly the circumstance already suggested as the cause of the deficiency in the one respect, i. e., continued exposure and consequent loss of sensitiveness to green, may be looked to as the cause of the increased facility in the other. If the detection of colors of low saturation depends upon the *delicacy* of the eye and the recognition of differences of shade upon *practice*, it would account equally well for both peculiarities.

An examination of some of the mixtures used in the foregoing tests under a half-inch objective magnifying about two hundred diameters, showed that the pigments consisted of well-formed, glistening crystals about $\frac{1}{100}^{mm}$ in diameter. These crystals were mingled with the magnesium carbonate without imparting *any trace* of their own color to the latter. Under the microscope the separation was perfect and the contrast of color a striking one. In the more dilute mixtures it was often necessary to search for some time before a single crystal of the pigment could be found, and the portion placed upon the slide did not contain, in some cases, more than five or six crystals altogether. To the naked eye, nevertheless, the mass appeared perfectly homogeneous, and unmistakably colored. Doubtless the power of a few isolated points of color, too small to be recognized individually by the eye, to impart their own hue to the entire colorless field in which they lie, is due to the persistence of the color-impression they produce upon the retina; this impression being fused with the impression of white from the remainder of the field of view by the continual movement of the eye in the process of observation.

University of Kansas, July, 1884.

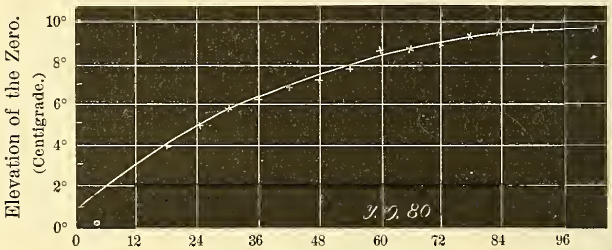
ART. VI.—*A Study of Thermometers intended to measure Temperatures from* 100°–300° *C.*; by O. T. SHERMAN.

IT is well known that when a thermometer is heated above a certain point, the mercury column is permanently displaced with regard to the scale.* The position of the point depends upon the constitution of the glass forming the bulb and upon the previous use of the thermometer. For certain glasses designated by the maker as German or American soda and Cornish the elevation upon a new thermometer begins at 111°. For a flint or crystal tube the point is nearer 200°. Mills records 256° as his highest observed limit, 48° as his lowest. Our experience presents nothing lower than 110°, nor higher than 255°. The latter point is obtained with English flint or French crystal.

By much use or long heating the displacement frequently amounts to ten degrees Centigrade, and may amount to 26°.† To assign corrections to points so easily displaced is evidently nugatory. The Observatory has therefore hitherto confined its corrections to points below that at which the ascent began.

If now the thermometer be exposed to a high temperature for some hours, the successive positions of the ice-point will be found to arrange themselves in a curve similar to that in the adjoining figure. Thus, for the first eighteen hours dur-

1.



Hours of heating at 300°.

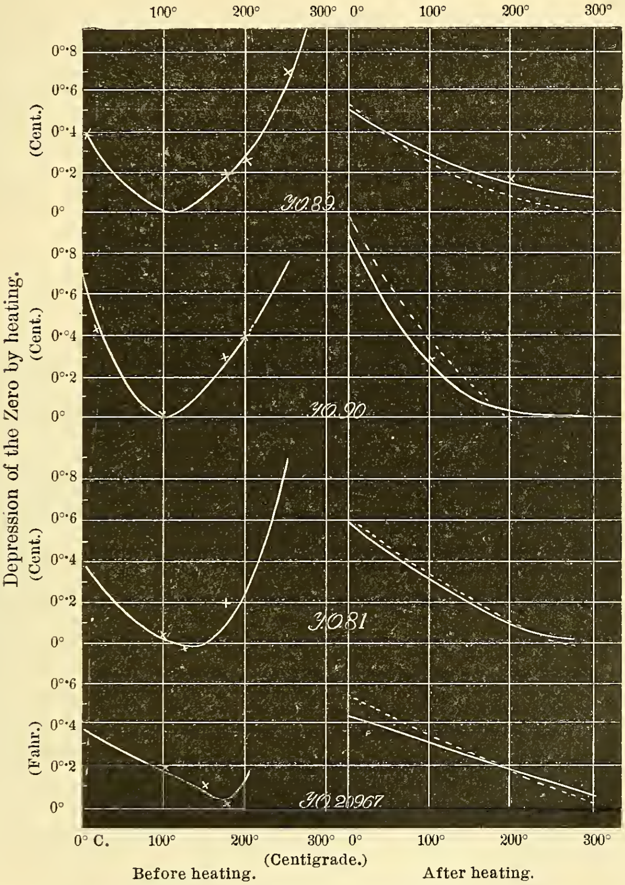
ing which the thermometer Y. O. 80 was held at 300° Centigrade, the zero point was elevated 3°; for the second eighteen hours the elevation was 2°·2; for the succeeding periods 1°·7, 1°·1, 0°·8, 0°·3 respectively.‡ The elevation evidently becomes less and less, and the curve becomes more nearly parallel to the axis of abscissas. This same thermometer placed in a bath at 200° immediately after the last observation rose two-tenths of

* Mills, Transactions Royal Society of Edinburgh, vol. xxix, part II.
† Crafts, Comptes Rendus, 1881 and 1882.
‡ Weber, Metronomische Beitrag, No. 111, pp. 126, 127.

a degree in the first twelve hours, but no change was detected in the following hundred and eight.

The question presents itself, What is the state of the thermometer after such treatment? First, as regards the action of the zero. In the adjoining cut we have compared the motion of the zeros of four thermometers before and after treatment.

2.



Movement of the Zero.

In the first series the influence of the rise is evident. The second series is free therefrom. The movement of the zero for the higher temperatures is similar to that for lower; or the mere fact of heating the thermometer now produces no distortion from which the instrument will not sensibly recover.

Does the instrument after treatment repeat its readings when exposed to similar conditions? Do its indications vary with time? We have observed the corrections to the following treated thermometers on Feb. 16th, March 9th and 22d, and April 15th. The record is given in the adjoining table:

	Feb. 16.	March 9.	March 22.	April 15.	Feb. 16–April 15.
Y. O. 81 (Cent.).					
0°	−6°·6	−6°·6	−6°·5	−6°·5	
100°	−5·85	−5·78	−5·76	−5·68	−0·16
200°	−6·3	----	----	−6·1	
Y. O. 89 (Cent.).					
0°	−5°·5	−5°·5	−5°·5	−5°·5	
100°	−4·1	−4·0	−4·1	−4·0	−0·1
200°	−5·3	----	----	−5·1	
Y. O. 90 (Cent.).					
0°	−8°·9	−8°·9	−8°·6	−8°·5	
100°	−6·8	−7·0	−7·0	−6·9	−0·06
200°	−6·3	----	----	−6·4	
Y. O. 20967 (Fahr.).					
0°	−16°·8	−16°·9	−16°·9	−17°·0	
212°	−18·6	−18·4	−18·6	−18·5	
387°	−25·2	−25·2	----	----	+0·17
420°	−27·8	----	----	−28·2	

On all of these instruments the closeness of the graduation renders an error of observation of a tenth not improbable, so that, with one exception, there is no difference which seems worthy of remark.

These observations indicate that after treatment the thermometer is as serviceable as a measure of temperature ranging from 0° to 300° C. as the standard to which we are accustomed is for the range 0° to 100°. In the curves representing the movement of the zero, the record of April 15th is represented by a dotted line, that of Feb. 16th by the full line. The former are slightly more curved than the latter. Again, in the final column of the preceding table are given the mean differences between the corrections due to Feb. 16th and April 15th. Both of these differences we would interpret as small effects still occurring in the bulb, such as occur in every new thermometer, rather than as evidence that the instrument does not repeat itself.

It is of interest to ask what is the nature of the change which has been effected in the glass. If we compare the errors before and after treatment, we obtain the following differences:

	Y. O. 81.	Y. O. 89.	Y. O. 90.
0°	4°·0	4°·9	7°·5
100°	2·8	2·9	6·6
200°	4·6	4·0	4·4

The differences for Y. O. 20967, upon which the points of comparison are more frequent, are given in the adjoining curve.

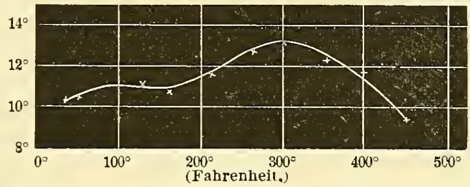
These differences indicate a change in the coefficient of expansion of the glass. The amount for the interval 0°–100° is readily calculated, and presents us with the following values:

Y. O. 81......	100. $\delta\beta$	$= -$ 0·000046
Y. O. 89......		$= -$ 0·000022
Y. O. 90......		$= -$ 0·000003
Y. O. 20967..		$= +$ 0·000026

Values similar to the first two have been previously observed by Weber and by Crafts. The latter two are, as far as my knowledge goes, without precedent.

Again, if the thermometer be preserved at ordinary temperatures, similar changes occur. We may instance the elevation of the zero with time. Its law is similar to that of the elevation produced by heating; or, analogies occur in the change in the fundamental length and the indicated change in the coefficient of expansion.

3.



Difference in the correction before and after treatment.

All of these facts—the condensing of the material forming the bulb, the consequent increase of its intermolecular attraction, the dependence of the point of rising upon its chemical constitution, the similarity of the changes produced by time, the regularity of these changes—seem to indicate the cause for the one as was long since suggested for the other, in a partial separation of the crystalline from the amorphous bulb-constituents. If the view is correct, it argues well for the stability of the treated instrument. The change will have been produced at the expense of its natural life. But then, few thermometers are permitted to die of old age. The correctness of the view is the subject of a separate research.

Yale College Observatory, April, 1885.

ART. VII. — *Notice of a new Limuloid Crustacean from the Devonian;* by HENRY SHALER WILLIAMS.

AMONG the fossils collected last summer for a comparative study of the Devonian faunas, an interesting form was discovered in Erie County, Pennsylvania, worthy of special notice.

The specimen was found in the bluish sandstone (which in places is a fine pebbly conglomerate) at Le Bœuf, called the "3d oil sand" by Mr. I. C. White in the Report Q^4 of the Second Geological Survey of Pennsylvania (p. 239), and regarded by him as the equivalent of the third oil sand of the Venango oil district of that State. In the same stratum and above it are typical Chemung fossils.

It occurs just at the junction between the sandstone and a stratum of soft, fine argillaceous shale, and, in the process of weathering, the fine shale has been washed away, leaving a sharply defined cast of the fossil in hard sandstone, though no portion of the original crust is preserved.

The associated species are *Spirifera Verneuilii* Murch. (=*Sp. disjuncta* Sow.), and *Rhynchonella contracta* Hall; and in the shales just above the sandstone occur *Chonetes scitula* Hall, "*Chonetes*" *muricata* Hall, an *Ambocoelia umbonata* Hall, a small Productus of the type of Hall's *Productella Boydii*, the coarse ribbed *Orthis Leonensis* Hall, and a Rhynchonella agreeing with some of the wider forms of *R. sappho* Hall.

The fauna is the characteristic Upper Chemung fauna of western New York and adjacent area. In this area some of the species occur among the earliest Chemung species; no characteristic Carboniferous types have been detected. The fauna may be considered, therefore, as a pure Devonian fauna.

The general form and structure of the specimen place it among the Merostomata with anchylosed thoracico-abdominal segments, but as only the under side is exhibited, its identification with *Prestwichia* must be regarded as provisional, since we are ignorant of the structure of the under surface of authentic members of that genus.

I propose as a name for it,

Prestwichia Eriensis, sp. n.

The following characters exhibited by the specimen are regarded as generic and as locating it in the genus Prestwichia of Woodward: (1), the elliptical head shield; (2), the genal spines which proceed backward more directly than in any described species of the genus; (3), the thoracico-abdominal segments anchylosed to form a buckler, to which is attached (4) a long telson. The general outline of the whole animal resembles that of the modern Limulus.

The evidence of a solid thoracico-abdominal buckler is found in the continuous surface across the body, from which proceed four (visible) short marginal spines each side the telson, and upon which are seen at least eight narrow ridges running longitudinally to near the margin.

The remaining characters may be, in part, of generic value, but they constitute the distinctive characters of the species, as far as these can be made out from the specimen.

The under side of the body presents three well defined tracts, viz: (A B), the cephalic shield which is evenly rounded in front and is laterally prolonged backward into two genal spines (C), which are nearly parallel with the axis of the body and reach nearly to a point opposite the posterior margin of the buckler. The cephalic shield along the median line is about a third the length of the body ; (M K) the space between the posterior margin of the cephalic shield and the anterior margin of the buckler containing the region of the mouth (M) and the gnathopods (K), and (F D H) the thoracico-abdominal buckler, marked over the surface by longitudinal ridges and by marginal spines, and terminating in a long stout telson (E). Traces of the gnathopods are seen, as also traces of the foliaceous appendages of the posterior pair (L), but in too imperfect condition for exact delineatión. Just anterior to the position of the mouth is seen a shield-like elevation (B), upon the edge of the cephalic shield, which has the appearance of an hypostoma. The condition of the specimen is not such as to give absolute certainty to this interpretation, though the symmetry of its form is strongly in favor of it. It is possible that it is merely outlines upon the surface produced by crushing during fossilization. There are faint indications of joints on each of the anterior set of gnathopods (K K).

Along the center of the thoracic region (H), there is a flattened depression, traversing longitudinally from the anterior edge of the plate F, backward to the middle of the telson E.

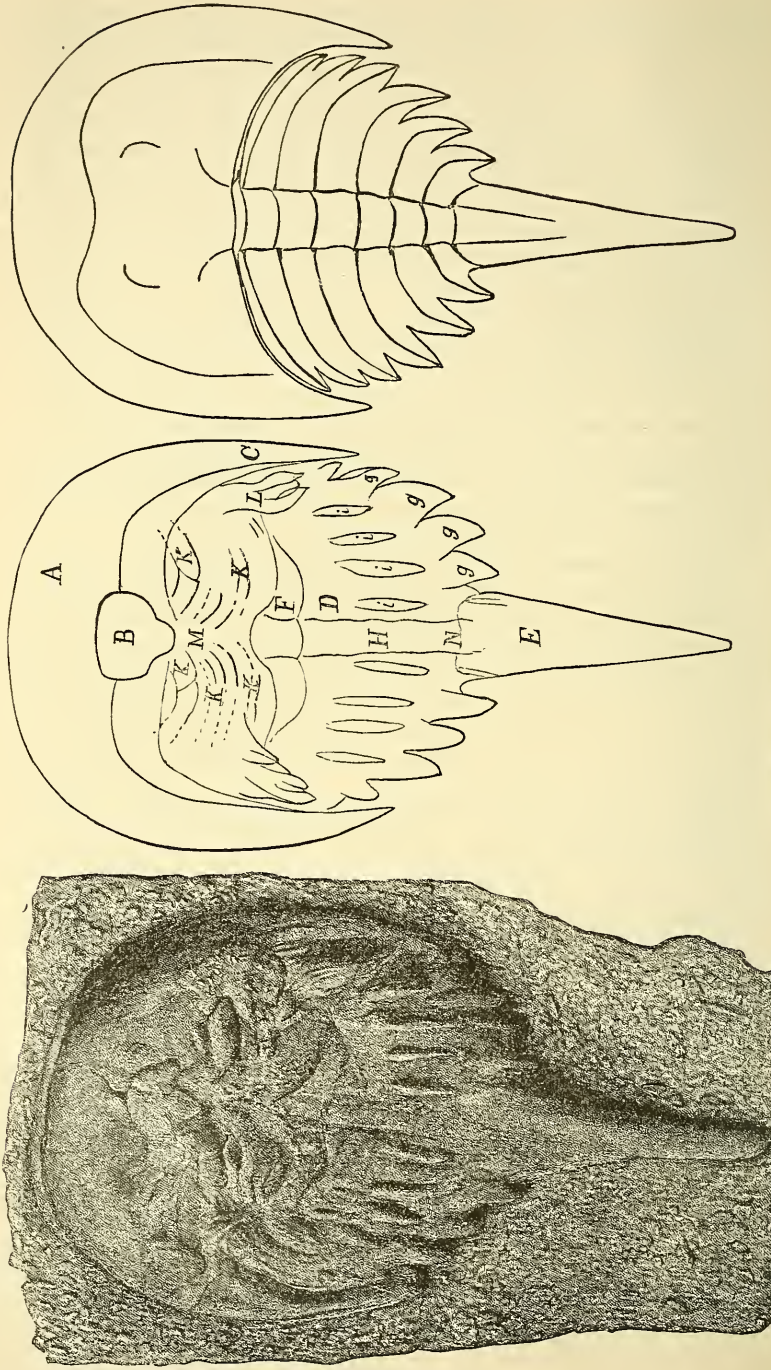
The terminal portion of the telson is evenly rounded. Each side of the median line of the buckler there are visible four clearly defined marginal spines (*g g*); there were probably more of them—six I have supposed, as in fig. 3, but concealed in the specimen by the filling between the buckler and genal spines.

There are also four rounded, longitudinal ridges on the buckler each side of the flattened depression H; these (*i i*) begin abruptly near the anterior margin of the buckler and run almost directly backward, tapering to a slender point near the margin of the buckler.

At the anterior margin of the buckler there is a narrow plate, divided into a median and two lateral parts (F), which appears to be separated from the buckler itself by a distinct furrow. Laterally this plate appears to curve inward and lies below (within) the surface of the buckler, and the median portion extends forward to a blunt point. I have interpreted this as probably representing the consolidated lamellar appendages of the "first and second" thoracic segments of Eurypterus, as defined by Hall in Paleontology of New York, vol. iii, p. 398.

The telson E, is nearly two-thirds the length of the body, is flattened at the base, but nearly cylindrical and tapering to a blunt point at the extremity. There is no indication of its articulation, but there is no reason, from the condition of the specimen, to presume that it was not articulated.

1. 2. 3.



Prestwichia Eriensis—H. S. WILLIAMS.

Above are given all the characters of which the specimen presents any reasonable suggestion. I have ventured to put an interpretation upon some of the characters for which the evidence is slight in the hope that those possessing specimens of any kindred forms may throw light upon this one by confirming the interpretation here given or suggesting a better one.

Fig. 1 represents very fairly the actual appearance of the specimen in size and details. It is a photo-engraving from a drawing of the original and photographs of it made by Professor E. C. Cleves of Cornell University.

Dimensions—	Total length	10·^{cm}
	Greatest width	5·7
	Length of telson (about)	4·
	Length of buckler (about)	2·
	Greatest thickness of telson	0·7

Horizon—Chemung Group, Upper Devonian; the "third oil sand" of I. C. White, 2d Pa. Survey.

Locality—LeBœuf, Erie County, Pennsylvania.

The original specimen is among the collections of the U. S. Geological Survey, and will be deposited ultimately in the National Museum.

Comments.—This specimen throws back the known range of *Prestwichia*, or at least the type to which this genus belongs, to an earlier stage than heretofore reported. The earliest previously known *Prestwichia* occurs in the Carboniferous.

If my interpretation of its characters be correct, Prestwichia bears closer relations to Limulus than is suggested by other known specimens, and also it possesses features linking it with Trilobites and Eurypterids.

Explanation of Figures.

Fig. 1. *Prestwichia Eriensis* Williams, sp. n. A sandstone cast representing the under surface; natural size.

Fig. 2. Diagram of the same.
- A. Cephalic shield.
- B. ? Hypostoma.
- C. Genal spines of the cephalic shield.
- D. Thoracico-abdominal buckler.
- E. Telson.
- F. First (and second ?) segments of thorax (? anchylosed).
- G*g*. Marginal spines of the buckler.
- H. Flat median depression extending across the buckler and upon the telson.
- *ii*. Longitudinal ridges of the buckler.
- KK. Portions of the gnathopods.
- L. ? Foliaceous terminations of the last gnathopods.
- M. Position of the mouth.
- N. Probable place of articulation of the telson.

Fig. 3. Theoretical diagram of upper side.

Cornell University, April, 1885.

Art. VIII.—*Gerhardtite and Artificial Basic Cupric Nitrates*, by H. L. Wells and S. L. Penfield.*

We shall describe in the present article a natural, crystallized basic cupric nitrate and a crystallized artificial salt of the same chemical composition but of different crystalline form. We also give an account of a re-investigation of two basic cupric nitrates to which have been ascribed different compositions, but which, as we shall show, have the same composition as the basic nitrates described by us and by other investigators, whose results will be briefly summarized.

Gerhardtite, *a new mineral.*

This mineral was first identified as a new species by Prof. Geo. J. Brush, who found it among a lot of copper minerals from the United Verde Copper Mines, Jerome, Arizona, which were left at the Sheffield Scientific School by Mr. G. W. Stewart, assayer, from that place.

The single specimen in our possession consists of a small piece of very pure massive cuprite, along a crack in which the crystals of the nitrate occur, together with acicular crystals of malachite. The crystals, 4–6^{mm} in diameter, were few in number and were almost wholly sacrificed to obtain material for investigation. An attempt has been made to obtain more of the material, but as yet no other specimens have been received, although we are in hopes that more may be found at the locality. From the abundance of crystals on the specimen in our possession, it would seem that there must have been a quantity of it found. It was probably regarded as malachite by the miners. Another specimen contains crystals of atacamite on the cuprite.

The crystals, which were carefully separated from the cuprite, were subjected first to crystallographic, then to chemical examination. About 0·8 of a gram was obtained almost perfectly pure, the only impurity being a few acicular crystals of malachite which sometimes penetrated the nitrate but were visible only under the microscope.

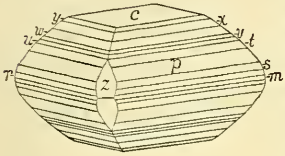
The hardness of the mineral is 2. Specific gravity 3·426. Color dark green. Streak light green. Transparent.

The crystals after being detached were only fragmentary. All those suitable for measurement were reserved. They were very fragile and had to be separated and handled with very great care. The crystals are orthorhombic, having the habit shown

* The chemical work is by the former, the crystallographic by the latter.

in fig. 1. There are two cleavages, which serve for orientation, one basal, parallel to *c*, as perfect as the most perfect cleavage of gypsum, a second, less perfect, parallel to the macropinacoid. The crystals can be readily bent, in which case they crack and separate along the latter direction. The most prominent forms on the crystals, besides the basal plane, are a series of pyramids occurring in oscillatory combination, which makes their indentification somewhat difficult. The best measurements were obtained from a small but very perfect macrodome which was found on two crystals. Owing to the fragmentary nature of the crystals and the difficulty of identifying the pyramidal planes, their orthorhombic form might be doubted were it not for their optical properties.

1.



The axial ratio was obtained from the following measurements:

c ∧ *z* 001 ∧ 201 = 68° 16′
z ∧ *p* 201 ∧ 111 = 39° 3′ 30″

giving ă : b : ċ = 0·92175 : 1 : 1·1562

The following forms were observed:

c,	001,	*O*	*t*,	778,	$\frac{7}{8}$
z,	201,	2–ī	*u*,	334,	$\frac{3}{4}$
m,	110,	*I*	*v*,	7·7·10,	$\frac{7}{10}$
r,	551,	5	*w*,	223,	$\frac{2}{3}$
s,	221,	2	*x*,	13·13·20,	$\frac{13}{20}$
p,	111,	1	*y*,	112,	$\frac{1}{2}$

The following is the table of measured and calculated angles, the measurements being made on eight crystals, the number of times each form was measured being given.

		Calculated.	Measured.	No. of times.
m ∧ *m*	110 ∧ 1$\bar{1}$0	85° 20′		
z ∧ *z*	201 ∧ 20$\bar{1}$	43° 28′	43° 34′	1
c ∧ *m*	001 ∧ 110	90°	90° 15′–90° 25′	2
c ∧ *r*	001 ∧ 551	83° 19′	83° 1′	1
c ∧ *s*	001 ∧ 221	73° 40′	73° 53′	1
c ∧ *p*	001 ∧ 111	59° 37′	59° 23′–59° 57′	6
c ∧ *t*	001 ∧ 778	56° 11′	55° 57′–56° 19′	3
c ∧ *u*	001 ∧ 334	51° 59′	51° 52′–52° 20′	2
c ∧ *v*	001 ∧ 7710	50° 3′	49° 46′–50° 38′	3
c ∧ *w*	001 ∧ 223	48° 40′	48° 8′–49° 12′	8
c ∧ *x*	001 ∧ 13·13·20	47° 57′	47° 11′–47° 56′	5
c ∧ *y*	001 ∧ 112	40° 28′	40° 13′–40° 18′	2
x ∧ *x*	13·13·20 ∧ 13·$\bar{13}$·20	60° 27′	60° 9′	1

Only distinct reflections were recorded, though other forms seemed to be present but were not definite enough to be determined. The variation in the measurements is large and may be due in part to an accidental bending of the crystals. The forms x and v with improbable indices would have been regarded as accidental had they not occurred repeatedly giving very distinct reflections. Tabular fragments, parallel to the basal cleavage, show under the polarizing microscope an extinction parallel to the macro-diagonal cleavage lines and in convergent light a bisectrix normal to c, 001. The optic axes lie in the brachypinacoid, the axial angle is large and could not be measured in air. Measured in the heavy solution of HgI_2 in KI ($n=1{\cdot}703$ for yellow, 1·722 for green).

$$2H = 76° \; 20' \text{ for yellow.}$$
$$2H = 80° \; 4' \text{ for green.}$$
$$\text{Dispersion } \rho < v.$$

A very thin section had to be used to obtain the character of the dispersion as moderately thick sections were practically opaque to yellow light. The indices of refraction could not be determined owing to the want of suitable material, the Kohlrausch total-reflectometer giving no total reflection.

Double-refraction is strong, negative.

Pleochroism is distinct:

For vibrations parallel to $\mathfrak{c}$, $\breve{a}$ blue.
" " " " $\mathfrak{b}$, $\bar{b}$ green.
" " " " $\mathfrak{a}$, $\acute{c}$ green.

Chemical composition.—Qualitative examination showed only the presence of CuO, N_2O_5 and H_2O.

I. ·3975 gram yielded ·0457 H_2O and ·2634 CuO.
II. ·3986 gram yielded ·0449 H_2O, ·2646 CuO, and 19·7 cc. dry N at 12·8° and 759 mm. (cor.).

	Found. I.	Found. II.	Calculated for $4CuO . N_2O_5 . 3H_2O$.
H_2O	11·49	11·26	11·56
CuO	66·26	66·38	66·22
N_2O_5	22·25*	22·76	22·52
	100·00	100·40	100·00

Pyrognostics, &c. B.B. fuses at 2, coloring the flame green. With soda on coal easily reduced to metallic copper with deflagration. In closed tube gives nitrous fumes and water which reacts strongly acid. Soluble in dilute acids, insoluble in water.

It is somewhat surprising that a mineral of this composition has not been found before, owing to the occurrence of nitrates in natural waters, the stability and insolubility of the compound and the ease with which it is made artificially.

* By difference.

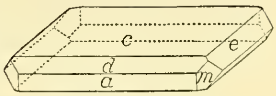
We propose for this beautiful and unique mineral, the only insoluble nitrate yet found in nature, the name Gerhardtite from the chemist who first determined the true composition of the same compound* made artificially.

Crystallized Artificial Basic Cupric Nitrate.

This is made by heating a solution of the normal nitrate with metallic copper in a sealed tube to about 150° for a day or more. The crystals form just above the surface of the liquid on the walls of the tube. When the contents of the tube are agitated the crystals fall to the bottom of the liquid where they remain undissolved while another crop is being formed. Cupric nitrite is apparently one of the products of the reaction: consequently the method is essentially a modification of that used by Vogel and Reischauer† in making the same compound in light, iridescent scales.

The crystals are of a beautiful dark green color, exactly the same as that of the mineral, with a very brilliant luster. Specific gravity 3·378. The largest which we succeeded in making were 7 or 8^{mm} in length. Their form is monoclinic as is shown both by measurement and by their optical properties. Their habit is tabular, lengthened out in the direction of the *b* axis, fig. 2. The axial ratio was determined from the following measurements:

2.



$$c \wedge a \quad 001 \wedge 100 = 85° \, 27'$$
$$c \wedge d \quad 001 \wedge 101 = 48° \, 25\tfrac{1}{2}'$$
$$e \wedge e \quad 011 \wedge 01\bar{1} = 82° \, 41'$$

giving $\grave{a} : b : \dot{c} = \cdot 9190 : 1 : 1\cdot 1402; \; \beta = 85° \, 27'$

The observed forms are

a,	100,	*i*–$\bar{i}$	*d*,	101,	1–$\bar{i}$
c,	001,	*O*	*e*,	011,	1–$\breve{i}$
m,	110,	*I*			

The following are the measured and calculated angles.

		Calculated.	Measured.
$m \wedge m$,	$110 \wedge 1\bar{1}0$	84° 59′	
$a \wedge m$,	$100 \wedge 110$	42° 29′	42° 36′
$c \wedge m$,	$001 \wedge 110$	86° 39′	86° 30′
	$00\bar{1} \wedge 110$	93° 21′	93° 40′
$c \wedge e$,	$001 \wedge 011$	48° 39′	48° 39′

Two twin crystals were found. They were not very perfect but the reëntrant angle $a \wedge a$ could be measured giving 9° 6′, calculated 9° 6′.

It may be said of the measurements on the prism *m*, that the face was usually quite imperfect and did not admit of very ac-

* See beyond. † See beyond.

curate measurement. Many trials were made before crystals suitable for measurement were obtained and a few crystals only were selected, which although small were very perfect. Many of the crystals have a different habit from that given in fig. 2, being terminated at the extremity of the *b* axis by the prismatic faces only, but none were found which admitted of measurement.

The cleavage is basal, perfect, a second cleavage is parallel to *a* (100). The crystals are brittle but do not bend like those of the natural nitrate.

Under the polarizing microscope the crystals show an extinction parallel to the *b* axis. In convergent light the axes are seen to lie in the plane of symmetry. One axis is visible in the field of the microscope, inclined about 40° to a normal to the basal plane, the other axis is outside of the field of the instrument. The bisectrix lies in the obtuse secant; its inclination could not be determined. One of the largest tabular crystals showed one axis in the axial angle apparatus, the other was totally reflected. In the solution of HgI_2 in KI ($n=1·703$ for yellow, 1·722 for green) both axes could be seen showing a marked inclined dispersion $\rho < v$, the axial angle being

$$2H = 59° \ 22' \text{ for yellow.}$$
$$2H = 63° \ 50' \text{ for green.}$$

Double-refraction, negative.

Pleochroism as in the orthorhombic crystals: ∥ *b* green.
⊥ *b* blue.

The crystals of the natural and artificial compound are very much alike, making a very interesting case of dimorphism. The points of similarity are repeated below.

Orthorhombic, natural, $\breve{a} : \bar{b} : \dot{c} = 0·92175 : 1 : 1·1562$
$\beta = 90$
Cleavage 001 and 100

Monoclinic, artificial, $\grave{a} : \bar{b} : \dot{c} = 0·9190 : 1 : 1·1402$
$\beta = 85° \ 27'$
Cleavage 001 and 100.

Plane of the optic axes in the brachypinacoid in the former and in the corresponding plane of symmetry in the latter. Dispersion almost of the same amount and $\rho < v$ in both cases. Double-refraction negative, and pleochroism similar with almost identical shades of color in both.

Chemical analysis:

I. 1·2373 grams yielded ·1388 H_2O and ·8202 CuO.
II. ·5314 gram yielded ·0615 H_2O, ·3519 CuO, and 25·4 cc. dry N at 12·9° and 764 mm. (corrected).

	Found. I.	Found. II.	Calculated for $4CuO . N_2O_5 . 3H_2O$.
H_2O	11·23	11·57	11·26
CuO	66·29	66·22	66·22
N_2O_5	22·48*	22·10	22·52
	100·00	99·89	100·00

Artificial Basic Cupric Nitrate made by other methods.

Graham† investigated a compound made by heating normal cupric nitrate. His formula was incorrect, owing to the fact that he merely determined cupric oxide and volatile matter, and, consequently, had insufficient data for determining it.

Gerhardt‡ showed the true composition of Graham's compound to be $4CuO . N_2O_5 . 3H_2O$. He made it a number of times under varying conditions and always obtained analytical results corresponding to this formula. He also showed that the precipitate formed by adding ammonium hydroxide not in excess to a solution of the normal nitrate has the same composition.

The following investigators have since confirmed Gerhardt's results: Gladstone§ by analysis of the compound made by heating the normal nitrate; Kühn‖ by examination of the precipitate formed by ammonium hydroxide; Vogel and Reischauer¶ by investigation of the light, iridescent, blue-green, crystalline scales made by boiling mixtures of the solutions of cupric nitrate and potassium nitrite and also by boiling the deep green solution made by passing nitrous acid gas through water containing cupric hydroxide in suspension; Field,** and Reindel†† by examining the precipitate formed by the addition of potassium hydroxide, not in excess, to solutions of normal cupric nitrate.

On the other hand, Casselmann‡‡ has described a basic nitrate which is precipitated by boiling solutions of cupric nitrate with acetates and various other soluble salts of organic acids and to which he ascribes a composition corresponding to the formula $2(4CuO . N_2O_5)7H_2O$. It will be noticed that this differs from Gerhardt's formula by only $\frac{1}{2}H_2O$; hence we thought it proper to reinvestigate the compound.

Sodium acetate was added to a hot, dilute solution of cupric nitrate until a copious precipitate was formed. The liquid was boiled until the precipitate became dense when the latter was thoroughly washed with cold water. No discoloration of the product was noticed either on boiling or washing, although Cas-

* By difference.

† Phil. Trans., 1837, 57.

‡ Jour. Pr. Chem., xxxix, 136.

§ Idem, xliv, 184.

‖ Jahresber., 1, Engl. transl., p. 340.

¶ Jahresber., 1859, 216.

** Idem, 1862, 216.

†† Idem, 1867, 304.

‡‡ Zeitschr. Anal. Chem., 1865, 24.

selmann states that blackening took place in both instances when he made the compound, so that he was unable to make it pure and unaltered. The product was composed of minute crystals, forming a powder of a light green color. Specific gravity 3·371. The following chemical analysis proves its identity with the basic nitrate made by other methods.

Of the substance, dried over sulphuric acid, 1·0631 gram yielded ·1229 H_2O, ·7035 CuO, and 52·4 cc. dry N at 18·9° and 764 mm. (corrected).

	Found.	Calculated for $4CuO . N_2O_5 . 3H_2O$.	Calculated for Casselmann's formula.
H_2O	11·46	11·26	12·89
CuO	66·17	66·22	64·99
N_2O_5	22·42	22·52	22·12
	100·05	100·00	100·00

A basic cupric nitrate mixed with metalic silver was made by H. Rose* by acting on cuprous oxide with silver nitrate solution. Rose not having determined the composition of the salt, Rammelsberg† attempted to do so and assigned to it a composition represented by the formula $Cu_{10}N_6O_{25}$. This differs so widely from the basic salt made by all other known methods that it was deemed advisable to reëxamine it.

Pure cuprous oxide was made by adding a dilute solution of pure glucose to an excess of Fehling's solution heated just to boiling. The precipitate was thoroughly washed, then treated while still wet with a large excess of silver nitrate solution, the liquid was boiled and the whole allowed to digest on a water-bath for several hours. The resulting dark gray voluminous powder was carefully washed with water and dried at 100°. In some preliminary experiments it was found difficult to completely decompose cuprous oxide after it had been dried, and it was also found that the reaction took place only very slowly in the cold; consequently the above mentioned method for making the mixture was adopted.

Supposing the mixture to consist of a basic cupric nitrate of Gerhardt's composition and metallic silver, the reaction would evidently be as follows:

$$3Cu_2O + 6AgNO_3 + 3H_2O = 4CuO . N_2O_5 . 3H_2O + 6Ag + 2CuN_2O_6.$$

Analysis proved this to be the case.

Of the substance dried at 100°,

I. 1·4958 grams yielded by ignition 1·2848 Ag and CuO,—the latter yielded 1·1459 AgCl.

II. 1·9329 grams yielded 1·6592 Ag and CuO, 1·4791 AgCl, ·0893 H_2O, and 40·55 cc. dry N at 18·2° and 758 mm. (cor.)

* Pogg. Ann., ci, 513. † Ber. Deutsch. Chem. Ges., 1877, 1780.

	Found. I.	II.		Calculated for $6Ag + 4CuO . N_2O_5 . 3H_2O$.	
Ag	57·66	57·60		57·46	
CuO	28·23	28·24	66·68	28·17	66.22
H_2O		4·62	10·91	4·79	11·26
N_2O_5		9·49	22·41	9·58	22·52
		99·95	100·00	100·00	100·00

Graham* states that the basic cupric nitrate will withstand a temperature near that of melting lead without decomposition. We find, on the other hand, that it begins to decompose when heated to 180–200°, blackening and giving off both water and nitric acid. The product made in sealed tubes and that made by Casselmann's method were both subjected to this experiment with like results.

The empirical formula for the compounds under consideration is $H_6Cu_4N_2O_{12}$ or $H_3Cu_2NO_6$. Cooke† developes this to $(HO)_3$, $(CuO_2H)_3$, CuO_2, viii, N_2O. A simpler formula is $H(HOCu)_2NO_4$. The latter has the argument in its favor that some other basic nitrates can best be formulated as derivatives of the hypothetical acid H_3NO_4 (corresponding to H_3PO_4), but the union of hydroxyl to both acid and basic radicals is possibly an objection to it as well as to Cooke's symbol.

It is an interesting fact that the monoclinic (?) mineral tagilite has the formula $4CuO . P_2O_5 . 3H_2O$, exactly corresponding to the nitrate.

Method of analysis.

Owing to the small quantity (less than ·8 gram) of the native nitrate at our disposal, it was important to use a method which would give a complete analysis on a single sample. The apparatus having been set up and tested, it was found to be so satisfactory and convenient that it was used for the other analyses given in this article.

The substance was ignited in a boat in a current of pure, dry CO_2 in a combustion-tube. The CuO remaining in the boat was weighed. The gases passed over a hot roll of copper gauze, then through a weighed calcium chloride tube, which absorbed the water, into an azotometer containing a concentrated solution of potassium hydroxide where the nitrogen was measured.

The carbon dioxide was made from crystallized calcite. The removal of the air from the apparatus before the ignition and the collection of the nitrogen afterwards was accomplished by passing the CO_2 only about ½ hour in each case. The CO_2 in the $CaCl_2$ tube was replaced by dry air before weighing.

Laboratories of Chemistry and Mineralogy, Sheffield Scientific School, June 6, 1885.

* Loc. cit. † Chemical Philosophy, 379.

ART. IX.—*On the occurrence of Fayalite in the lithophyses of obsidian and rhyolite in the Yellowstone National Park;* by JOSEPH P. IDDINGS, of the U. S. Geological Survey.

THE obsidian which forms the columnar cliff just north of Beaver Lake, on the road from Mammoth Hot Springs to the Geyser Basins, is especially rich in spherulites of various kinds, and of the more or less hollow forms called lithophyses by von Richthofen,* who first described those found in the rhyolites of Hungary.

These lithopyses, which vary in size from less than a quarter of an inch to a foot in diameter, when broken open often appear like spherulites whose central mass has shrunken and cracked apart like the pithy center of an over-ripe watermelon, or are composed of hollow, concentric shells, either hemispherically arranged in rose-like forms, or in segments like the chambers of an ammonite. The walls of these cavities are usually coated with transparent crystals of prismatic quartz and tabular tridymite, both appearing to have been deposited at the same time; scattered among these are small, opaque, black crystals about 2^{mm} and less in length. They are tabular in form, apparently with orthorhombic symmetry, and have most frequently a metallic luster, and sometimes a reddish color; they are found upon examination to be coated with ferric oxide, the interior of the crystals being transparent and of a light yellow color. Their optical behavior between crossed nicols is that of an orthorhombic mineral with high index of refraction, closely resembling olivine.

A chemical analysis was made by Dr. F. A. Gooch of the chemical laboratory of the U. S. Geological Survey, on a small amount of material, 0·24 gram, all that was at the time available. Under the microscope the crystals were seen to carry a small amount of adhering quartz, and to be coated with iron oxide, they were readily decomposed in hot hydrochloric acid with the separation of silica and yielded the following results:

SiO_2	25·61
Al_2O_3	trace
Fe_2O_3	14·92
FeO	51·75
MgO	1·66
CaO	none
Ignition	none
Insol. SiO_2	7·02
	100·96

* F. von Richthofen. "Studien aus den ungarisch-siebenbürgischen Trachyt-Gebirgen." Jahrb. k. k. geol. Reichsanstalt. Wien. 1861, xi, 153, et seq.

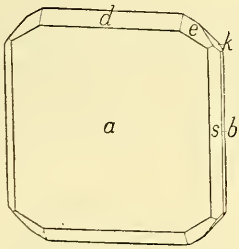
Considering the Fe_2O_3 as the opaque coating of alteration, and the insoluble SiO_2 as the adhering quartz, the composition of the unaltered mineral will be:

			Oxygen ratio.	
SiO_2	25·61	32·41	17·275	1·12
FeO	51·75	65·49	14·539 }	1·00
MgO	1·66	2·10	·840 }	
	79·02	100·00		

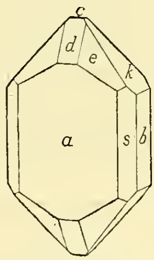
which is essentially the composition of the unisilicate fayalite.

Perfectly fresh, unaltered crystals about a millimeter long were subsequently found in small lithophyses in compact obsidian from half a mile north of Lake of the Woods. A very careful qualitative test showed that the crystals were an iron silicate containing no magnesia. They are in thin, square or rectangular plates, of a light honey-yellow color, perfectly transparent and free from inclusions of other minerals, but occasionally containing gas cavities. They show very slight pleochroism, pale greenish yellow parallel to the *b* axis and golden yellow parallel to the *c* axis. The cleavage parallel to the brachypinacoid is good, but a second at right angles to the first is less distinct and is probably in the plane of the macropinacoid as in olivine.

1.



2.



Mr. S. L. Penfield, of the Sheffield Scientific School, has kindly determined and figured the crystallographic forms presented by the rectangular, tabular crystals from the locality north of the Lake of the Woods, and the more elongated and pointed crystals, from Obsidian Cliff. The measurements were made on a thin tabular crystal 0·1^{mm} thick and 0·8^{mm} broad, which was broken across the prismatic zone. The observed forms were *a* (100, *i*-$\bar{i}$), *b* (010, *i*-$\breve{i}$), *s* (120, *i*-$\breve{2}$), *e* (111, 1), *d* (101, 1-$\bar{i}$), *k* (021, 2-$\breve{i}$). Arrangement of planes quite constant as in figure 1. For fundamental angles the two best reflections were chosen.

$$a \wedge s,\ 100 \wedge 120 = 42°\ 31'$$
$$d \wedge d,\ 101 \wedge \bar{1}01 = 103°\ 17'$$

$$\text{giving}\ \breve{a} : \bar{b} : \dot{c} = 0{\cdot}4584 : 1 : 0{\cdot}5791$$

The angles measured and calculated were:

		Measured.	Calculated.
$a \wedge b$	$100 \wedge 010$	90°	90°
$s \wedge s$	$120 \wedge \bar{1}20$	95° 8′	94° 58′
$a \wedge d$	$100 \wedge 101$	38° 20′	38° 22′
$d \wedge e$	$101 \wedge 111$	19° 52′	19° 46′
$e \wedge e$	$111 \wedge \bar{1}11$	95° 5′	95° 6′
$a \wedge e$	$100 \wedge 111$	42° 25′	42° 27′
$b \wedge k$	$010 \wedge 021$	40° 45′	40° 49′

The plane of the optical axes is parallel to the base, one of the bisectrices being normal to the macropinacoid as shown by a polarizing microscope. Owing to the minuteness of the crystal examined the divergence of the optical axes was not determined.

The opaque crystals from Obsidian Cliff show the same forms with the additional basal plane, *c*, but are mostly developed as in figure 2. With the exception of the macropinacoid the faces were too dull to give good reflections, and the forms were identified by approximate measurements only.

Similar crystals of fayalite are found in the lithophyses in rhyolite from several localities within the Yellowstone National Park, which will be fully described in the geological report on the survey of that region now in progress under the charge of Mr. Arnold Hague.

These brief notes are published at this time for the reason that well-developed, natural crystals of fayalite have not been found before, so far as we know, though they frequently occur as furnace products and in iron slags. Their natural occurrence in this instance bears a striking similarity to the artificial, the obsidian being as perfect a slag as any artifically produced, and the association of tridymite and prismatic quartz with the fayalite in the closed cavities of lithophyses leaving little doubt as to their igneo-aqueous origin.

Art. X.—*The Genealogy and the Age of the Species in the Southern Old-tertiary;* by Otto Meyer, Ph.D.

Part II.*

The Age of the Vicksburg and Jackson Beds.

The table, given in Part I, indicates that the Jacksonian stands between the Claibornian and the Vicksburgian, but it affords no evidence as to which is the most recent formation; this requires a special discussion.

The history of the age of the Vicksburg bed is closely connected with the literature of the orbitoidic limestone in the vicinity of Claiborne, as the following review shows.

*Errata in table of Part. I.—Nos. 3 and 13. Transpose names under Claiborne and Jackson. No. 17. Substitute *Lea* for *Mr.* after *Astarte sulcata.* No. 80. For *protracta* read *protractus.* Add No. 85 under Jackson and Claiborne *Terebra costata* Lea.

1833. When Conrad was in Claiborne he found in a stratum *lower* than the "Claibornian" a specimen of *Spondylus dumosus* (*Plagiostoma dumosum*). See "Fossil shells of the Tertiary formation," second edition, p. 34.

1834. "Observations on the Tertiary and more recent formations of a portion of the Southern States," by Conrad, Jour. Acad. Philad., vol. vii, pp. 116–157, read April 15, 1834.

Here Conrad mentions the limestone with *Nummulites Mantelli* Morton in the vicinity of Claiborne. As this limestone contains *Plagiostoma dumosum*, in abundance, he considers the stratum at the base of the Claiborne bluff as the upper part of this nummulitic limestone. Thus, he concludes, that the nummulitic limestone *underlies* the Tertiary formation of Claiborne and is Cretaceous.

March, 1846. The second number of vol. i, 2d series of this Journal (1846), contains the following two essays:

1. Conrad, "Observations on the Eocene formation of the United States, etc.," pp. 209–221. Here Conrad says that he no longer considered the nummulitic limestone of Alabama and Mississippi as a connecting link between the Secondary and Tertiary strata, but as Eocene. "Between Claiborne and St. Stephens it forms hills of considerable elevation and abounds in that fine fossil, *Plagiostoma dumosum*. At St. Stephens this limestone constitutes an elevated bluff and abounds in *Nummulites Mantelli*, *Plagiostoma dumosum*, *Ostrea cretacea*, etc. At Vicksburg the *Pecten Poulsoni* is common to this rock and to the Eocene sand." These words are important. Here the Vicksburg is mentioned the first time, and at the same time parallelized with the nummulitic limestone of Alabama.

2. As postscript to this number of the Journal, pp. 313–315, there is a letter from Charles Lyell, dated Claiborne, Ala., Feb. 4th. In this letter Lyell speaks on several subjects and says, of the nummulitic limestone, that it had been referred to a pre-Tertiary age, but he considered it even younger than the Claibornian, for it occurs on higher places than the Claiborne bluff, "a circumstance which, in a region where the stratification is horizontal, would imply a newer deposit. . . . I did not meet with the limestone in question in the bluff at Claiborne, which, I have no doubt, is owing to the fact that the calcareous strata are cut off at the top before they extend upward into the nummulitic beds."

We see, that Lyell does not show, why Conrad's opinion, based upon *Spondylus dumosus*, is wrong, but he states, in a few words, that the nummulitic limestone overlies the Claiborne strata. His reasons are: that the stratification is horizontal, and the nummulitic limestone occurs higher than the Claiborne bluff. If one of these two suppositions had been proved and the other was doubtful, there would be no right to draw the conclusion of Lyell, even if it had been proved besides that there are no faults. But Lyell proves none of them. As for the stratification, he was unable to bring forward any other fact, than that he did not notice any dip at the points where he made his observations. In a diagram, in his essay of the next year, he represents a dip, not as he observed it, but as he needs it for his hypothesis. As to the relative height, he also does not bring forward any proof; and I cannot imagine how he could possibly have obtained in Claiborne at that time any reliable data about it. In short, Lyell's new hypothesis was without any proof at all, and was in contradiction to an observed fact. Moreover, to sustain it, he had to make another hypothesis, also entirely without proof: that this limestone was totally eroded in Claiborne, but preserved in the vicinity.

May, 1846. Charles Lyell, "On the newer deposits of the Southern States of North America" (Quart. Jour. Geol. Soc. London, ii, pp. 405–410, read May 6, 1846). On this question Lyell writes, p. 408: "After visiting Claiborne and the country on the other side of the Alabama River in the fork of that river and the Tombecbee, I am persuaded that the nummulite limestone is Eocene, newer than all the beds of the well known Claiborne bluff. It is in fact more modern than the sandy deposit, from which the Eocene shells described in the publications of Messrs. Conrad and Lea were derived." We have here a more decidedly pronounced expression of his hypothesis without any proof.

July, 1846. Conrad, "Tertiary of Warren County, Miss." (this Journal, ii, 2d series, pp. 124, 125). Here Conrad briefly announces that he collected 103 species of fossils at Vicksburg; he enumerates the genera, but had not yet made descriptions of the species. To what age are these fossils attributed? He himself had parallelized this Vicksburg bed with the nummulite limestone of Alabama. Sir Charles Lyell claimed this limestone to be more recent than the Eocene of Claiborne. So, if Conrad did not wish to oppose this opinion of Lyell, he had only the choice of considering these Vicksburg fossils more recent than those from Claiborne. The fossils, considered alone, did not show their age, and Conrad seems not to have made even an attempt to question the decidedly pronounced opinion of the celebrated English geologist; for in this first announcement of the Vicksburg fossils we see distinctly the presupposition of their newer age, and he considers only whether they are more nearly related to the Miocene or to the Eocene. He says: "The Vicksburg group has decidedly more affinity with the Eocene group than with that of the Miocene, for there is only one species that closely resembles a Miocene fossil. The limestone of Clark County, Ala., and of St. Stephens contains *Nummulites crustaloides* and *Pecten Poulsoni* Morton, two fossils which abound* in the Vicksburg deposits, and this limestone is, therefore, *probably* of the same age as the Tertiary beds of Vicksburg. This formation marks a distinct era in the American Tertiary system, intermediate between the Eocene and Miocene formations, but more nearly allied to the former, etc."

In this way the age of the Vicksburg fossils was determined, and what Conrad says afterward about it is only the following out in detail of this once accepted supposition.

Sept., 1846. Conrad, "Eocene formation of the Walnut Hills, Miss." (this Journal, ii, 2d series, pp. 210–215). Conrad, speaking of the Vicksburg locality, says, p. 210: "One of the most abundant bivalves is *Pecten Poulsoni* Morton, a species occurring in the white limestone near Claiborne, Ala. A very thin wafer-shaped nummulite, described by Dr. Morton, is common in the limestone as well as in the strata above, and connects the formation of Vicksburg with the Eocene white limestone of St. Stephens." Here Conrad omits the word "probably," which he used in the preceding essay, and from this time the identity of the Vicksburg beds and the nummulite limestone in Alabama is an accepted fact in American literature. Though I consider this identity probable, it cannot be said to be proved.

June, 1847. Lyell, "On the relative age and position of the so-called limestone of Alabama" (Quart. Jour. Geol. Soc. London, iv, pp. 10–16, read June 9, 1847. Published also with a few alterations in this Journal, iv, 2d series, pp. 186–191, Sept., 1847.

Lyell says about this limestone, referring to his letter of the preceding year: "It was stated to be newer than all the beds of the well known Claiborne Bluff." Then he tries to prove this stated opinion by claiming that the Claibornian bed occurs also at the base of the St. Stephens bluff below the nummulitic limestone. The fossils which he cites from this bed, p. 15, are: "*Terebra costata* Con., *Cardita parva*, *Dentalium thalloides*, *Flabellum cuneiforme* Lonsd., *Scutella Lyelli* Conr., and several more." From the few species which Lyell selects to prove the Claibornian character of the beds, he cites, on the same page, *Dentalium thalloides* and *Terebra costata* as occurring in the Vicksburg bed, and *Flabellum cuneiforme* in the Jackson. So he

* As this is of some importance, and as it is repeated afterward, I have to object here to the word "abound" in relation to *Orbitoides Mantelli* in the sand of Vicksburg. It may be said to be "common," but is not at all "abundant," and is rarer than several other species.

might just as well have deduced a Vicksburgian or Jacksonian age, but he had to corroborate his stated opinion.

1850. M. Tuomey, "First biennial report of the geology of Alabama," Tuscaloosa, 1850. This report contains some interesting sentences, which show that the authority of Lyell must have been very great. Tuomey says, p. 149: "Sir Charles Lyell has proved that the white limestone is newer than the fossiliferous bed at Claiborne by showing that this bed, which contains identical fossils, underlies the bluff at St. Stephens. This is certainly the case, for although this bed is not seen at the base of the bluff, it is overlaid, as I have just stated, by a yellow limestone, which is a prolongation of that at St. Stephens."

The following is of special interest, pp. 156, 157: ". . . Mr. Conrad . . . referred the whole to the upper part of the Cretaceous system, supposing, as I have said in another place, that the Claiborne bed was newer, instead of being, as we now know it, older than the white limestone.

"It is curious to observe, after the difficulties have been cleared away that surround pioneer explorations of every description, how obvious everything appears and how difficult it is to account for the mistakes of our predecessors. But in the present instance it must be recollected that the Claiborne fossiliferous bed is nowhere in absolute juxtaposition with the overlying Orbitoides limestone, and even at St. Stephens I was unable to detect it at the base of the bluff, although I examined it at an unusually low state of the water. Nevertheless, the position of the bed above the bluff, together with its dip, leaves no doubt of its sinking below the white limestone."

The reason why Tuomey could not detect this bed at the base of St. Stephens bluff is very simple and was even known to himself, as will be seen by the juxtaposition of the following two sentences. Lyell says in his essay, p. 15: "The water of the river at the time of my visit was too high to enable me to collect fossils from the beds at the base of the cliff, but I was afterward furnished with them through the kindness of Professor Brumby of Tuscaloosa." Tuomey says, p. 158: "This is the bed described in the preceding pages as extending from Baker's Bluff to a point about half a mile above St. Stephens; and it was from this bed the fossils were taken which were sent by Professor Brumby to Sir Charles Lyell."

So we see that Lyell's Claibornian bed at the base of St. Stephens Bluff not only according to his own determinations need *not* be Claibornian, but that it is also *not* at the base of St. Stephens Bluff.

From this time, however, the higher position of the Orbitoidic limestone and the newer age of the Vicksburg fossils was considered a proved fact in the literature. As the Claibornian was considered Middle Eocene, Conrad called the Vicksburg bed "Newer Eocene." Afterward it appeared to him more proper to use the name of "Upper Eocene" for the intervening Jackson bed, and thus finally the Vicksburg fauna received the name of Oligocene.

1860. Interesting as it would be, I cannot review here the whole literature regarding this subject,* but it is necessary to criticize this point at least briefly, as it is presented in Hilgard's Geology of Mississippi (Jackson, 1860). Only a competent and careful examination of the fossils could indicate the relation of the Old-tertiary strata in Mississippi; but Hilgard seems to have studied this Tertiary paleontology very little, and when it was necessary to prepare a list of fossils he transferred the work to Professor W. D. Moore. The division: Claiborne, Jackson and, at the top, Vicksburg was accepted as a proved fact, as well as the identity in age of the Vicksburg and St. Stephens beds. On this basis he undertook to map the marine Old-tertiary. He found *Orbitoides* and *Pecten Poulsoni* in the east (Wayne County), in the west (Vicksburg), and besides in Rankin County. So he connected these localities, thus forming a "belt," which pointed admirably toward St. Stephens. But unfortunately this belt would pass right through Jackson, an older formation. Therefore Hilgard made this belt make a sharp curve to the south around Jackson. Then he drew a parallel Jacksonian belt; and farther north he found the Claibornian. All this would admirably agree with the general dip southward. An "irregularity"—the strata from Jackson to Canton indicating a northern dip†—seems not to have troubled him very much. I could not find a place in his work where an overlying of the Jacksonian by the Vicksburgian was shown. It is no wonder that Hilgard worked in this way, if we consider that he had to map an enormous territory without reliable preparatory work, almost without assistance and in so short a time, that he could not even by a glance examine large areas. The only criticism that can be made

* Attention might be called at least to the following sentence of Winchell's, (Proc. Am. Assoc. Adv. Sci., 1856, part II, p. 86: "The thick bed of limestone which underlies the sandy belt at Claiborne [the Claibornian] has not been recognized elsewhere. Perhaps its occurrence here is accidental." We see that Winchell, working on the accepted theory, comes to the conclusion that the stratum of more than fifty feet below the Claibornian may be accidental, as it cannot be found elsewhere.

Winchell determines, p. 85, a bed as Claibornian without citing any fossil to prove this character.

† Lyell represents in a diagram (1847) the dip from Jackson westward.

against him under these circumstances is that he represented suppositions too much like facts.

We find more reason to complain of the work which Professor Angelo Heilprin has published on the Old-tertiary. His first essay is:

1. "On some new Eocene fossils from the Claiborne Marine formation of Alabama."* I leave a determination of most of these "new" species till I have had an opportunity of examining at leisure the type specimens. Some of them, however, can be recognized very easily from the descriptions and figures. *Solarium striato-granulatum* Heilpr. is a specimen of *Solarium ornatum* Lea; *Natica bisulcata* Heilpr., a specimen of *Natica magno-umbilicata* Lea; *Tornatella bicincta* Heilpr. is *Actaeon lineatus* Lea; *Odostomia lævigata* Heilpr. is a fragment of *Actæon melanellus* Lea; *Delphinula solaroides* Heilpr. is either a young specimen of *Solarium elegans* Lea, or an old one in which the larger whorls are broken off.

2. "A comparison of the Eocene Mollusca of the United States and Western Europe, etc."† I have already in a former paper‡ excluded all identifications of Professor Heilprin which were made from figures. A collection of several hundred species of the French Old-tertiary, which I received from Mr. Cossmann in Paris, convinced me that the rest of his comparisons have also little value. In the French Old-tertiary as well as in the American there are quite a number of similar forms (connected by descent?), and only a large amount of material compared by an experienced observer can furnish reliable identifications or other results of comparison.

3. "On some new Lower Eocene Mollusca from Clarke County, Ala., etc."§ Specimens from Wood's Bluff, which I received from Professor E. A. Smith and Mr. Aldrich in Tuscaloosa, convinced me that no reliance can be placed upon these determinations and descriptions. I mentioned in part I of this essay that *Dentalium micro-striatum* Heilpr. has most probably a fissure, although Professor Heilprin says "there being no fissure." Where I have received the corresponding material I have found none of the Claibornian determinations correct. A striking example is the determination "*Corbula rugosa* Lam. (*C. oniscus* Conr. var. *C. gibbosa* Lea)."

The two species in Claiborne, *C. gibbosa* Lea and *C. Murchisoni* Lea (*C. oniscus* Conr.), are two entirely different species; neither of them is identical with the French *C. rugosa* Lam., and neither of the three is identical with the species in Wood's Bluff, which I have had in my collection for some time under

* Proc. Acad. Nat. Sci. Philad., 1879, pp. 211–216.
† Proc. Acad. Nat. Sci. Philad., 1879, pp. 217–225.
‡ Proc. Acad. Nat. Sci. Philad., 1884, p. 104.
§ Proc. Acad. Nat. Sci. Philad., 1880, pp. 364–375.

the name of *Corbula Aldrichi*, n. sp. It has radiating striæ on the umbonial part of the surface, a characteristic which I had not seen before in a Corbula.

4. "Contributions to the Tertiary Geology and Paleontology of the United States."* As the author is curator of the collection of the Philadelphia Academy, where so many type specimens of Tertiary invertebrates are said to be, I had looked with eagerness for this work, announced a long time since,† hoping to receive much information concerning many doubtful points, especially in connection with Conrad's descriptions. But I found that a study of the paleontology of the Southern States had not been attempted in it. The book contains nothing but reprints of previous essays of the author, increased by a "succinct statement of the Tertiary geology of each of the several States," and a map. How did Professor Heilprin map the Southern Old-tertiary? E. A. Smith has given a map of Florida,‡ where he carefully makes a distinction between the few localities where limestone with Orbitoides has been observed and the large area where this limestone is supposed to exist; all this is colored Oligocene by Professor Heilprin. The risk of mapping in this way can perhaps be fully appreciated only by one who has, like myself, tried to get a clear opinion of the Oligocene formation by studying for years its typical localities in Europe. Further, Professor Heilprin traces the Oligocene (p. 3 and p. 4), connecting link after link, from the Mississippi River through Florida, Jamaica, Antigua, Trinidad and St. Bartholomew, to the Vicenza deposits in Italy and the Mayence Basin in Germany, and ends this speculation by the words (p. 4): "we thus have the parallelism established between our Vicksburg or Orbitoidic bed and those of the typical Oligocene of Southern Europe." Having in my possession more than four hundred species, which I collected myself at both ends of Professor Heilprin's long chain, in the Mayence Basin and at Vicksburg, I ought to have found at least some identical forms, but as yet I have not noticed a single one. Considering the difficulty of determining the relative age of two localities only, such as Vicksburg and Jackson, which are situated near each other and contain hundreds of well preserved fossils,§ we may ask, what guided Professor Heilprin through thousands of miles? The answer is, the Foraminifera Orbitoides. It is scarcely necessary to repeat, what

* By A. Heilprin, Philadelphia, 1884, published by the author.

† C. A. White wrote 1880 (Am. Naturalist, p. 255): "Mr. Heilprin has begun the preparation of a monograph of the Tertiary fossils of Eastern North America."

‡ This Journal, III, vol. xxi, 1881, p 305.

§ Conrad described 34 species from Jackson; I collected there about 150. Having received during the printing of this article additional material from this locality, I think that this number must be increased, perhaps considerably.

is generally known, that Foraminifera of the most different formations often look extremely similar, and that the genus Orbitoides cannot be considered at all a "Leitfossil" for any certain Tertiary formation. But it may be of interest to quote what has been published about this same American Orbitoides. Lyell sent it to E. Forbes and A. d'Orbigny. Forbes writes to him,* June 14, 1847: "As the subject stands at present, then, we have no right to infer from the presence of an Orbitolite, however abundant, that the stratum in which it occurs belongs to one period more than another between the commencement of the Cretaceous epoch and our times." D'Orbigny writes, ibid., June 18, 1847: ". . . It is, in fact, of all genera, that perhaps which has been most often misunderstood, and I should call it the greatest culprit in geology, . . . the Orbitoides are found in the Cretaceous and Tertiary formations."

The gap between Mississippi and Florida, without the necessary data for a mapping, is filled by Professor Heilprin by prolonging the hypothetical belts of Hilgard through the States of Alabama and Georgia, till they meet the bulk of the Oligocene of Florida. My studies lead me to saying, that I consider Professor Heilprin's map, as far as the divisions of the Southern Old-tertiary is concerned, entirely imaginary.

Mr. Heilprin has not made himself a single observation in the Southern Old-tertiary formation, but is acquainted with its geology only by a somewhat superficial knowledge of a literature which needs much criticism. For this reason I do not wish to give any further criticism of the text, but we may see at least his views about that limestone, which interested us in the preceding pages. We find, p. 29:

"4. 'White limestone' (Jacksonian), best exhibited at Claiborne (upper portion of bluff) and St. Stephens (lower moiety of bluff), not very abundant in fossils—*Pecten membranosus*, *P. Poulsoni*, *Ostrea panda*, *Spondylus dumosus*, '*Scutella*' *Lyelli*, etc. —50—? feet."

While Lyell in the year 1846 had to confess that he could not find this white limestone in the upper portion of the bluff, and concluded that it must have been eroded, this upper portion of the bluff, according to Professor Heilprin, has become in 1884 typical for this limestone and best exhibits its characteristic features. The specimen of *Spondylus dumosus*, which Conrad found in 1833 in the lowest strata, must have migrated upward since that time, and the other species must have come from other places. They are indeed "not very abundant," for I could not find a single specimen.

Having now shown how the age of the Vicksburg and the Jackson beds was determined, I give in the following pages

* Quart. Journ. Geol. Soc. London, iv, p. 12.

observations of my own, which make it very probable to me that Vicksburg is the *most recent* bed.

1. *Profile of the bluff at Claiborne, Ala.*—This famous bluff has been described by Lea, Conrad, Lyell, Hale, Tuomey and Mell,* and yet none of the given profiles represents the facts correctly, at least at that point, where I observed. At a little distance above the "upper landing" there is a vertical exposure of the bluff of more than a hundred feet, showing the different strata so distinctly that the whole profile might be photographed. I was told by inhabitants of Claiborne that this exposure was caused by a landslide some years ago.† By a simple and yet reliable method (a string with a weight attached) I measured the thickness of the strata on one vertical line and then examined their characters, wherever they could be reached. The water at the time of my visit was so high that I could not determine the thickness of the stratum *b*, and moreover could not examine the stratum below it, which according to Conrad and Tuomey is only visible at very low water and contains fossils, the careful examination of which must be of importance.

The profile at the mentioned point is the following:

k.	Diluvium? red loam and pebbles.	———
i.	Mostly limestone.	33 feet.
h.	Glauconitic sands and clays.	11 feet.
g.	Gray sandy stratum with fossils.	6½ feet.
f.	Red sand, fossils badly preserved, similar to *e*, but *Scutella* very common.	3 feet.
e.	Ferruginous sand with the Claibornian fossils: Highest Claibornian. / Ferruginous sand with the Claibornian fossils. / Lowest Claibornian.	17 feet.
d.	Color a bluish gray.	26 feet.
c.	Mostly limestone with large ferruginous concretions.	8 feet.
b.	Calcareous limestone with green sand, *Ostrea*, *Pecten*, *Scalpellum*, *Scutella*.	30? feet.

In the lower limestone *b* I collected the following fossils: Casts of shells; teeth of sharks and rays; an otolite of a fish; *Endopachys Macluri* Lea sp., in bad condition; a fragment of a *Scalaria;* a specimen of an *Orbitoid;* a specimen of *Venericardia parva* Lea, showing more the typical form of the Clabornian specimens than that of the var. *Jacksonensis.* More frequent and in better condition are the following species:

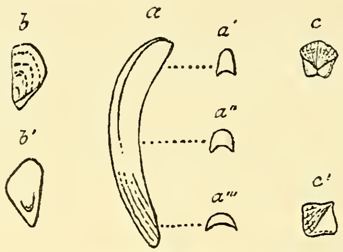
Ostrea sellæformis Conr., *O. Alabamiensis* Lea, *Pecten Deshaysi* Lea (includ. *P. Lyelli* Lea), *P. scintillatus* Conr., "*Scutella*" *Lyelli* Conr., *Scalpellum Eocenense*, n. sp.

* "The Claiborne group and its remarkable fossils," by T. H. Mell, Jr.—Trans. Am. Instit. of Min. Engin., 1880.

† Thus the Alabama River seems to be cutting its bed deeper. I observed a similar undermining of its banks in Moody's Branch near Jackson, Miss., and other creeks seem to do the same. This part of the American coast has risen more or less gradually during all the later formations, and the mentioned phenomenon may, perhaps, indicate that it is rising still at the present time.

Besides the figured piece *b*, I found valves of the same form but larger. The umbo of the carina is placed at the apex. As far as I know the literature, this is the first Lepadite from the American Tertiary.

The top of stratum *b* is formed by an oyster bank. In stratum *c* I did not find a trace of a fossil, and in *d*, *h* and *i* no determinable fossil. In *e* there must be made a distinction between the highest part and the lowest part of it, "*Highest Claibornian*" and "*Lowest Claibornian*," as there is a paleontological difference which, though not striking, cannot be neglected. I found in the stratum *e* also a specimen of an Orbitoid. The fossils in stratum *g* are very fragile. Larger specimens could be obtained only in poor fragments, and even



a. Carina of *Scalpellum Eocenense*, n. sp.
a', *a''*, *a'''*, vertical sections of this carina.
b and *c*, lateralia of the same species?, different views.

the smallest could be obtained only with difficulty. The most common of them seems to be *Alveinus minutus* Conr., which is apparently one of the most characteristic fossils of the Southern Old-tertiary. A specimen of *Venericardia parva* Lea shows no approach to the Jackson angular form, and is as rounded as the most rounded Claibornian specimens. A young specimen of *Corbula Murchisoni* Lea shows the sharp carina of the Clabornian form.

2. *Profile at Enterprise, Miss.*—The profile given by Hilgard* is not correct. The Chickasawhay River near Enterprise seems to be little fitted to furnish a reliable profile. Each of the creeks on the west side, however, shows distinctly *two* fossil-bearing strata. The upper one contains quite a number of species, but they are badly preserved. They have a Claibornian character (for instance, *Venericardia rotunda* Lea) and are apparently not the species which Conrad received from Enterprise,† which I could not find. The lower stratum is an indurated sand with glauconite; and, besides shark teeth, I found nothing else in it except

* Geol. of Miss., p. 125. † Am. Jour. Conch., 1865, p. 137.

Ostrea sellæformis Conr., *O. Alabamiensis* Lea, *Pecten Deshaysi* Lea (incl. *P. Lyelli* Lea), *P. scintillatus* Conr., "*Scutella*" *Lyelli* Conr., *Scalpellum Eocenense* Meyer.

These are exactly the same species as in the stratum *b* of Claiborne, and the bed in Enterprise is therefore evidently a *facies* of this stratum.

3. *Profile at Vicksburg, Miss.*—Directly in front of the national cemetery near Vicksburg there is a creek (bayou) forming a waterfall, at which the following facts can be observed: At the top there is a stratum with the "Vicksburgian" fossils; below this follows a stratum of limestone with *Pecten*, about thirty feet thick; at the base is a clayey stratum with fossils, which must be strictly separated from the upper ones. These strata may be called "Higher," "Middle" and "Lower Vicksburgian." The Higher Vicksburgian contains the fossils, generally known as Vicksburg fossils. The Lower Vicksburgian is characterized at first sight by the absence of *Arca Mississippiensis* Conr., which is abundant in the Higher Vicksburgian. It is very interesting as containing a species of *Cæcum* and two species of the Pteropod *Styliola*, two genera hitherto unknown in the American Old-tertiary.* Though I have not found as yet the species mentioned by Conrad, the Lower Vicksburgian is apparently identical with that stratum, which Conrad called Shell Bluff group,† in Vicksburg; but, if so, this name cannot be used for the Vicksburg stratum, because it implies a parallelism with Shell Bluff, which is as yet entirely without any proof.

After the description of the three preceding profiles, the following reasons for indicating as the true succession—Vicksburg, Jackson and Claiborne, with Claiborne at the top, may be mentioned:

1. The Orbitoidic limestone, parallelized by Conrad with Vicksburg, is characterized by *Spondylus dumosus* and *Orbitoides Mantelli.* As Conrad found a specimen of *Spondylus dumosus* in the lower limestone of the Claiborne bluff, he concluded that this lower limestone formed the top of the Orbitoidic limestone. Lyell, upon whose authority the contrary opinion was accepted, could adduce no facts at all against Conrad's theory, and this theory is made more probable by my finding a specimen of the second characteristic fossil, an Orbitoid, in this lower limestone.

2. If the stratum *b*, which occurs about a hundred feet below the surface in Claiborne, appears near the surface in Enterprise, a dip of the strata is indicated, which makes it probable that we find older beds as we go in the northwestern direction.

* Two other species of *Styliola* occur in Jackson.

† This Journal, 2d series, i, 1866, p. 96.

3. If the Jacksonian were younger than the Claibornian, the Highest Claibornian ought to be more nearly related to it than the Lowest. I found, on the contrary, the fauna of the latter more similar to the Jacksonian. For instance, *Lunulites interstitia* Lea is common in Jackson, not rare in the Lowest Claibornian, very rare in the Highest. *Mitra pactilis* var. *dumosa* is common in Jackson; *M. pactilis* is rarer in the Lowest Claibornian, and apparently absent in the Highest.

4. If Jackson were older than Vicksburg, the Lower Vicksburgian ought to show more relationship to it than the Higher. I found rather the contrary; for instance, *Astarte parva* Lea, in the lowest stratum, is more different from the Jackson form than in the highest stratum.

5. As a proof of this succession and derivation, the following facts are to be considered: *Venericardia rotunda* Lea in its young form resembles the allied Jacksonian species. *Fulgur filius* Meyer repeats in its sculpture *Fulgur Mississippiensis* Conr. *Tellina Vicksburgensis* Conr. resembles the young form of the allied variety in Jackson (see part I, Nos 22, 69, 35).

6. Among the fossils from Wood's Bluff, Ala., which I received, there is a Vicksburgian species (*Pleurotoma terebralis* Lam. var. *carinata* Conr.), a Jacksonian species (*Actæon*, sp.), but no characteristic Claibornian species. According to the determinations of Professor Heilprin, moreover, *Natica Mississippiensis* Conr. and *Pecten Poulsoni* Mort. occur in these strata, so that they show a decidedly Vicksburgian character. According to Professor E. A. Smith they are stratigraphically far *below* the Clabornian.

7. In Harper's Geology of Mississippi (Jackson, 1857, p. 141), a bluff in Wayne County is described and extensively figured. Harper's stratum on the top *b*, "containing Pectens of several species, Gallerites and several Ostreæ," is apparently my stratum *b* from Claiborne, which I showed to occur near the surface at Enterprise, a place not very distant from this locality. Harper's second stratum below, called *d*, "filled with *Orbitoides*, *Ostrea*, *Pecten* of several species, *Arca*, *Flabellum*, *Cardita*, *Gallerites*, etc.," seems to be of Jacksonian or Vicksburgian character.

Art. XI.—*On the probable occurrence of the Great Welsh Paradoxides*, P. Davidis, *in America;* by Geo. F. Matthew.

This, the largest and most remarkable species of Paradoxides occurring in the Primordial fauna of Europe, has not, so far as the writer is aware, been hitherto found in America. It was discovered by Dr. Henry Hicks near St. Davids in Wales about twenty years ago, and was subsequently found (by Professor O. Torell ?) in Sweden in 1869.

Through the kindness of Mr. J. P. Howley, Director of the Geological Survey of Newfoundland, the writer received, a few months ago a box of Cambrian fossils from that Island and among them fragments which appear to belong to this species. Its most important characters are well represented by the posterior half of the center piece of the buckler, by the free cheeks, the long cylindrical genal spines and the peculiar hypostome. The outlines and aspect of the parts preserved agree exactly with those of specimens from the Swedish beds figured by G. Linnarsson.* The Newfoundland fossil sent me is not *P. Bennetti* of Salter, nor is it Green's *P. Harlani;* from both it is distinct by the outlines and furrows of the glabella, by the short eyelobe and by the great extension of the facial suture behind the eyelobes.

A large species of Paradoxides is also found in the Cambrian slates at Saint John, New Brunswick, but the fragments recovered are not sufficiently large or perfect to make it clear that it is the species above referred to. This crustacean of the Saint John Group was nearly a foot and a half long (supposing it to have had extended spines next the pygidium like *P. Davidis*).

The American examples of *P. Davidis* occur in a hard black silico-calcareous shale at Highland's Cove, Trinity Bay, Newfoundland, in company with species of Agnostus—*A. punctuosus* Ang., *A. lævigatus* Dalm., *A. Acadicus* Hartt (var. *declivis* mihi.) These fossils indicate a new horizon in the Paradoxides beds of America somewhat above that of Braintree, or the known horizons of Newfoundland and New Brunswick.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On a simplified method of liquefying Oxygen.*—It is well known that liquid ethylene, boiling in free air, gives a cold sufficient to liquefy oxygen under pressure, only when the latter is suddenly expanded. By evaporating the ethylene in vacuo or by using liquid methane in free air, Cailletet produced the temperature of liquefaction, both of oxygen and nitrogen. Since, however, ethylene is more readily procurable in the liquid form than methane, the author has experimented on the use of this substance evaporated in free air and has succeeded in obtaining by its means a temperature sufficiently low to liquefy oxygen completely. This he effects by hastening the evaporation of the ethylene sim-

* De Undre Paradoxideslagren, Stockholm, 1883, Plate II.

ply by passing through it a current of air or of hydrogen previously cooled to a low temperature by the agency of methyl chloride. The steel cylinder containing the liquid ethylene is supported vertically with its orifice downward. To this a copper worm, 3 or 4 millimeters in diameter, is attached, and is closed at its lower end by a screw plug. On cooling this worm to −70° by means of methyl chloride, the ethylene contained in it has at this temperature only a feeble tension, and flows out when the screw plug is opened, without much loss. The liquid ethylene is received in a test tube of thin glass placed within a larger glass vessel containing dry air. It is necessary now only to accelerate its evaporation by passing through it a rapid current of the cooled air or hydrogen, in order to see oxygen compressed in a glass tube immersed in the ethylene, condense into a clear colorless liquid having a sharply defined meniscus. By means of the hydrogen thermometer, the temperature which was thus obtained with ethylene was measured and found to be −123°; a temperature below the critical point for oxygen. In this form the experiment is one well suited to the lecture table.—*C. R.*, c, 1033, April, 1885. G. F. B.

2. *On the preparation of Cyanogen in the wet way.*—While studying to learn the best conditions for the preparation of Bong purple, JACQUEMIN mixed a concentrated solution of copper sulphate with one of potassium cyanide and immediately obtained a tumultuous evolution of cyanogen gas, while the temperature of the liquid rose 40°. Although this fact had been already observed, the author has succeeded by a simple modification of the experiment, in making the reaction complete so as to obtain in the free state all the cyanogen of the cyanide. A solution of two parts copper sulphate in four of water is placed in a flask on the water bath, and by means of a funnel furnished with a stopcock, a concentrated solution containing one part of pure potassium cyanide is allowed to enter. The reaction begins actively even at the ordinary temperature, and when this diminishes, the temperature of the water bath is raised. In one experiment ten grams pure potassium cyanide gave 850 c. c. of pure cyanogen. If commercial cyanide be used carbon dioxide gas is obtained in addition. In the reaction, cupric cyanide is at first formed. This being unstable separates into cuprous cyanide and cyanogen. Two processes have been devised by the author for obtaining the cyanogen from the cuprous cyanide. On standing, the cuprous cyanide settles to the bottom of the flask and the supernatant liquid is decanted. In the first process, a slight excess of a 30° solution of ferric chloride is poured into the flask. The action begins even in the cold and an abundant evolution of cyanogen takes place on slightly warming the liquid. The ferric chloride is reduced to ferrous chloride the excess of chlorine uniting with the copper of the cuprous cyanide, producing cuprous chloride and setting the cyanogen free. By the further action of the ferric chloride the cuprous chloride is converted into cupric chloride. The second process consists in adding to the washed cuprous

cyanide, manganese dioxide and acetic acid, and slightly warming the mixture. Acetates of copper and of manganese are formed and the cyanogen is evolved. When the operation is terminated, the addition of sulphuric acid enables the acetic acid to be distilled off for a new operation. In separating the cyanogen which is evolved, from any other gaseous substances with which it may be mixed, Jacqueminfinds that this gas is very readily absorbed by aniline to form cyaniline, and hence recommends this substance as the best with which to absorb it, since neither CO_2, CO, nor air are appreciably absorbed by aniline.—*C. R.*, c, 1005, 1006, April, 1885. G. F. B.

3. *On two new Alkalimetric Indicators.*—VILLE and ENGEL have proposed two new indicators for alkalimetry, both of which are unaffected by carbonates; so that by means of these, the free bases may be determined volumetrically in presence of the alkali carbonates. The first of these is sulphindigotic acid. It is prepared by neutralizing with calcium carbonate the ordinary solution of indigo in fuming sulphuric acid, diluting with ten parts of water and filtering. While carbonates are without action upon the blue color of this liquid, caustic alkalies change it to yellow. In titering with it, a few drops of this blue liquid are added to the solution containing the caustic alkali, which turns it yellow. As soon as the neutralization is complete, the color of the liquid changes back to blue, passing through an intermediate green. By placing the beaker on a white paper each drop of acid as it enters produces a blue spot at the point of contact, which ceases on neutralization. The second substance proposed as an indicator is the soluble blue C. 4. B. of Poirier. This in solution in water (2 parts in 1000) is even more sensitive than the sulphindigotic acid. In presence of alkali carbonates this solution remains blue but becomes red under the influence of the free bases. If to a solution containing both carbonates and free bases, a few drops be added, a rose-red liquid is obtained in which each drop of the graduated sulphuric acid, as it enters, produces a blue color, which so long as neutralization is incomplete is transient, but which as neutralization is approached passes through violet into a permanent blue when no more caustic alkali is present. With these indicators, the determination of free alkali in presence of carbonated, is rendered as rapid and accurate as is ordinary alkalimetry with litmus or orange No. 3.—*C. R.*, c, 1073, April 1885. G. F. B.

4. *On the Separation of Nickel and Cobalt.*—ILINSKI and v. KNORRE have proposed a new method of separating nickel and cobalt founded on the properties of the compounds which these metals form with nitroso-β-naphthol. If to a neutral aqueous solution of sodium-nitroso-β-naphthol an excess of a cobalt salt be added in solution, a brownish red precipitate of cobalto-nitroso-β-naphthol $[C_{10}H_6O(NO)]_2Co$ is produced which is very sparingly soluble in water. By digestion with potassium hydrate, this precipitate loses slowly, by warming with acids rapidly, a portion of

its cobalt and is converted into cobalti-nitroso-β-naphthol, $[C_{10}H_6O(NO)]_3Co$, of a purple-red color. The same body is obtained when a neutral or acid solution of a cobalt salt is added to a solution of nitroso-β-naphthol in alcohol or acetic acid. It is remarkably stable, resisting acids, alkalies, oxidation and reduction reagents, etc., to a marked degree. It dissolves in fuming nitric acid and in strong sulphuric acid, but is precipitated unchanged on dilution. Boiled with strong alkalies, it is only slowly attacked. Hot acetic acid (50 per cent) dissolves it sparingly, but deposits it again completely on cooling. It is slightly soluble in strong alcohol, more so in dilute alcohol and readily in aniline and phenol. Nickel salts give under similar circumstances nickel-nitroso-β-naphthol as a brown-yellow precipitate difficultly soluble in water and alcohol and from which hydrochloric and sulphuric acids easily remove the nickel leaving the nitroso-β-naphthol, which in presence of sufficient acetic acid goes into solution. As therefore a nickel salt produces no precipitate in an acetic solution of nitroso-β-naphthol containing hydrogen chloride, the authors recommend the following method of separation: To the solution containing the nickel and cobalt as sulphates or chlorides, acidulated with hydrochloric acid and slightly warmed, a hot solution of nitroso-β-naphthol in 50 per cent acetic acid is added. The precipitate is allowed to deposit and the supernatant liquid tested with more of the reagent. After some hours, the precipitate is filtered off, washed first with cold, then with hot water, and then with a 12 per cent solution of hydrochloric acid to remove the whole of the nickel. After drying, a few grains of oxalic acid free from ash are added, and the precipitate is burned as usual in a tared crucible. The residue is ignited in a current of hydrogen gas and weighed as metallic cobalt. The nickel may be determined in the filtrate as usual. By determining both metals in an aliquot part of the solution by precipitation with potassium hydrate and reduction to the metallic state, and then estimating the cobalt in another similar portion as above, the nickel is readily determined by difference. In 20 c.c. of a solution containing 0·0382 gram cobalt, 0·0380, 0·0378, and 0·0379 gram were obtained in three experiments. Fifty c.c. of this cobalt solution, mixed with 100 c.c. of a nickel solution containing 0·2097 m., gave 0·0947 gram Co instead of 0·0955. As to delicacy, 0·2 c.c. of a cobalt solution containing 0·00004 gram Co, mixed with 5 c.c. of a nickel solution containing 0·0105 Ni, gave when mixed with the solution of nitroso-β-naphthol, an immediate turbidity, and after a time flocks of the cobalti-compound separated.—*Ber. Berl. Chem. Ges.*, xviii, 699, March, 1885. G. F. B.

5. *On a ready method of preparing Tartronic acid.*—Although many processes have been devised for preparing tartronic (oxymalonic) acid, yet owing to the cost of the crude material or the very small yield, this acid has been but little studied. PINNER has recently observed the formation of this acid by the action of sodium hydrate upon ethyl trichlorlactate according to the equa-

tion: CCl_3. CHOH. $CO_2C_2H_5+(NaOH)_5=CO_2Na$. CHOH. $CO_2Na+(NaCl)_3+C_2H_6O+(H_2O)_2$. To prepare it, he adds to a 10 per cent sodium hydrate solution warmed to 60° or 70°, the trichlorlactic ether slowly, in the proportion of one molecule of the latter to 5 of NaOH. After standing a short time, dilute acetic acid is added to weak acid reaction and then barium chloride solution so long as this gives a precipitate. After cooling the precipitate is filtered off and washed. It consists of pure barium tartronate. The yield of the barium salt was about 50 per cent of the ether employed. The trichlorlactic ether is readily formed from its nitrile, chloralcyanhydrin, by passing hydrogen chloride gas through its alcoholic solution, heated on the water bath. And the chloralcyanhydrin is produced by the action of hydrogen cyanide upon chloral hydrate. From 1800 grams chloral hydrate, the author obtained 2100 trichlorlactic ether.—*Ber. Berl. Chem. Ges.*, xviii, 752, March, 1885. G. F. B.

6. *Pocket Book of Mechanics and Engineering*, containing a memorandum of facts and connection of practice and theory, by JOHN W. NYSTROM, C.E. 18th edition, revised and greatly enlarged with original matter. 672 pp. 12mo. Philadelphia, 1885 (J. B. Lippincott & Co.).—From 1854 to 1885 this valuable work —small in size but large in the amount of matter it contains—has gone through eighteen editions and has increased two and a half times in size. It contains a vast number of facts conveniently arranged and covering a wide range of subjects. Personal experience with an earlier edition has shown the writer how useful a companion such a book can be.

II. GEOLOGY AND MINERALOGY.

1. *Coals and Lignites of the Northwest Territory;* by G. CHRISTIAN HOFFMANN, Chem. and Min. Canada Geol. Survey. From the Geol. and Nat. Hist. Survey of Canada. Montreal, 1884 (Dawson Brothers).—The coals and lignites of the Northwest Territory have been studied with great thoroughness by Mr. Hoffmann and with important results. The coal products are divided into Lignites, Lignite Coals and Coals, and the three groups are separately considered. The *Lignites* rather easily fall to pieces on exposure, the hygroscopic water ranges from 10 to 17 per cent, and they all communicate a brownish-red color to a boiling solution of potash; and they are all non-coking, in no instance affording a coherent coke. The *Lignitic Coals* do not easily disintegrate on exposure, the hygroscopic water varies from 8 to 9 per cent, they act with potash like the above but with less deep color; and none yield a coherent coke. The *coals* are hard and firm, bearing transportation without serious waste by reduction to pure coal; they give but slight coloration to a solution of potash; they yield by fast coking a good coherent coke though not by slow coking; they contain but a small proportion of hygroscopic water; and in general appearance and chemical com-

position resemble some varieties of coal of the Carboniferous system.

2. *Why are there no Fossils in the strata preceding the Cambrian?*—This question is answered, in the Proceedings of the Philadelphia Academy of Natural Sciences, by Mr. Charles Morris, by the suggestion that the earliest animals like the youngest stage of animal life generally had no hard parts to preserve; and that the sudden appearance of tribes was simply the appearance of species having hard or stony secretions. One difficulty in the way of the theory is presented by the existence of limestone formations of great extent in the Archæan which most geologists suppose to be of organic origin, and the existence also of phosphate of lime in large quantities which also is material of possible organic origin. These facts, although of uncertain bearing, throw doubts into all speculations on the subject.

3. *Cone-in-Cone Structure.*—Mr. John Young in a paper read before the Geological Society of Glasgow (Geol. Mag., June, 1885), arrives at the conclusion that the cone-in-cone structure, common in certain fine-grained sedimentary strata, consisting chiefly of calcareous material, clay and iron, is due to the upward escape of some gas generated in the deposit while it was in process of formation, each ebullition of gas producing a new layer. The cone is invariably found with the apex downward, and the author states that between the successive layers there is always a thin film of clay and also an axis of clay to the cone. The transverse wrinkling on the layers is attributed to a creeping downward of the plastic material through gravity. In the Scottish coalfield the structure occurs in strata of freshwater or lacustrine origin.

4. *Aerial formations.*—As the löss of China and similar deposits of other countries have been supposed to be of aerial or "Eolian" origin, the following facts from the Records of the Geological Survey of India, vol. xviii, p. 517, 1885, are interesting, as they illustrate the general truth that deposits so made never have for long distances a flat horizontal surface or horizontal stratification. "Aerial formations in the shape of blown sand cover large tracts in these wide valleys (in Beluchistan), and practically all the level country between Nushki and Helmund is covered with *sand-hills.* It is characteristic of them that they generally form low hills of crescent shape, with the horns and the scarp to leeward; the inclined plane formed by the currents of air are therefore generally dipping westward and show a rippled surface, resembling closely the accumulations of drift snow on the Himalayas."

5. *Irish and Canadian Rocks compared.*—In a paper by Mr. G. H. Kinahan, in the Geological Magazine for April, 1885, the characters of some Irish and Canadian Archæan rocks are discussed. He concludes that some of the gneissic Canadian rocks are very similar to Irish metamorphic rocks that are not Archæan but of different later periods, as Cambrian and Lower Silurian.

6. *Cambrian or Primordial rocks in the eastern part of British Columbia.*—Mr. H. H. Winwood describes, in the Geological Magazine for May, greenish micaceous, more or less calcareous rocks, containing remains of Paradoxides, Conocoryphe, and other allied forms, from a locality near Stephen, on the Canadian Pacific Railway, between the 116th and 117th meridians west of Greenwich.

7. *The folds in the Alps.*—The double folds described from the "Alpes Glaronnaises" by M. Heim have been a subject of much discussion, and one of those objecting to them, M. Vacek, has a paper in the Jahrb. der k. k. Geol. Reichsanst., xxxiv, 234, 1884. M. Bertrand has a paper bearing on the same subject in the Bull. Soc. Geol. de France, xii, 318, 1884.—*Archives Sci. Phys. et Nat.*, xiii, March 15, 1885.

8. *Disintegration in the Alps.*—M. A. Brun, after a microscopic study of the rocky crests of the Alps (Echo des Alpes, 1884), observes that they are penetrated with small fissures, leptoclases (joints) or microclases, having directions determined by the dislocations of the rock; that a mountain mass, like the Aiguille du Midi, in the Mt. Blanc chain, is traversed in certain directions by diaclases and paraclases—intersecting fissures, which divide the mountain into gigantic fragments, and were due to the pressures and tensions undergone by the rock since its formation. The microclases give the rock great porosity, permitting the absorption of water, which acts by corrosion as well as in other ways. The water charged with lime (from calcareous material in the granite) may contain even *three* milligrams of silica per liter.—*Archives Sci. et Phys. et Nat.*, xiii, March 15, 1885.

9. *The Amblypoda;* by E. D. Cope.—The American Naturalist for November and December, 1884, and January, 1885, contains an illustrated memoir by Professor Cope on species of the genera Coryphodon, Bathmodon, Pantolambda, Bathyopsis, Loxolophodon and Uintatherium.

10. *The Lenape Stone, or the Indian and the Mammoth;* by H. C. Mercer. 96 pp. 12mo, with illustrations. New York and London, 1885. (G. P. Putnam's Sons).—This little volume contains a review of the facts in Archæology bearing especially on the existence of man with the "Mammoth." The facts are well illustrated and appear to be fairly presented.

11. *United States Geological Survey.*—The following publications of the Survey have recently appeared:

Review of the Fossil Ostreidæ of North America and a comparison of the fossil with the living forms, by C. A. White, M.D., with appendices by Prof. A. Heilprin and Mr. John A. Ryder. From 4th Annual Report of the Director, 1882, 1883, Washington.

A Geological Reconnoissance in Southern Oregon, by Israel C. Russell. Ibid.

Bulletin of the Survey No. 10, on the Cambrian Faunas of North America. 56 pp., with 10 plates. Washington.

Bulletin No. 11, on the Quaternary and recent Mollusca of the Great Basin, by R. Ellsworth Call, introduced by a sketch of the Quaternary Lakes of the Great Basin, by G. K. Gilbert.

12. *Beiträge zur Kenntniss der Flora der Vorwelt, Band II.* Die Carbon-Flora der Schatzlarer Schichten, von D. Stur. K. K. Geol. Reichs. xi Band, I. Abtheilung.—With many lithographic plates and 48 zincotpyes. A grand work.

13. *Physikalische Krystallographie und Einleitung in die krystallographische Kenntniss der wichtigeren Substanzen* von PAUL GROTH. Zweite umgearbeitete und vermehrte Auflage, 710 pp. 8vo. Leipzig, 1885, (Wm. Engelmann).—This is a work which should be in the hands of every student not only of Mineralogy but of Chemistry also. The author is one of the most successful teachers in Germany, and his book is especially adapted, both in fulness of explanation and clearness of style, for the use of students. The new edition contains much new matter, more especially in regard to the description and use of the instruments employed in investigating the form and physical properties of crystallized substances. The chapter devoted to this subject extends to 130 pages and is all that could be desired, whether as regards the completeness of description or the abundance of illustrations.

14. *On the occurrence of native Silver in New Jersey;* by NELSON H. DARTON. (Communicated.)—The only locality in which metallic silver has been found in New Jersey hitherto is the Bridgewater copper mine, near Somerville, where it occasionally occurs as minute linings or blotches on the cuprite. Rogers called attention to this in his 1836 Report, in describing, for the first time, the copper deposits in the Mesozoic rocks of the State. Many of the copper ores from other parts of the formation have yielded small amounts of silver by assay, as noted by Schæffer.* Recently a small opening for copper ore has been made on the Westlake property, near the old Schuyler mine in Hudson County, and in cavities in the very rich chalcocite ore the writer found some very fine specimens, with thread silver associated with mammillary coatings of malachite. The ore occurs in red sandrock, adjacent to a dike of the trap sheet of the Hackensack upland and interbedded with carbonaceous strata, seamed with veins of anthracite of small size. These features I will describe in detail, in a memoir soon to appear.

The thread silver is easily identified, fusing readily to a bead, soluble in HNO_3, from which it precipitate as chloride with HCl. The threads are from 3^{mm} to 8^{mm} in length, and very uniformly about $\frac{1}{2}^{mm}$ in thickness; they are thinly coated with malachite, and when this is removed, are bright and crystalline in appearance; their regularity is sometimes interrupted by slight local thickenings, and the terminations are irregular and ragged. Several endeavors were made to cut sections for microscopic study, but without success. The threads are always separate from each other, extending from the walls out into the cavity. Assays of the average ore yielded about $12 per ton of silver and a trace of gold. The pocket was cleaned entirely out and the ore sent to Bergen Point.

* Eng. and Mining Jour., xxxiii, p. 90, 1882.

Careful examinations of the accessible portions of the Schuyler mine have never resulted in the discovery of thread silver. Metallic copper was found, however, in the Passaic mine in the same vicinity.

Laboratory, 112 Water St., New York City.

15. *On the Vanadates and Iodyrite from Lake Valley, Sierra County, New Mexico;* by F. A. Genth and G. vom Rath.—This interesting paper contains descriptions of vanadinite from the Sierra Bella and Sierra Grande mines, of endlichite, a vanadiferous mimetite, and of fine crystals of descloizite. The mineral called endlichite, after Dr. F. M. Endlich, superintendent of the Sierra Mines, is a sub-species intermediate between vanadinite and minetite. It occurs in crystalline groups with columnar structure sometimes radiating, also sheaf-like; the hexagonal form, characteristic of the group, is frequently distinct. The color varies from white to yellowish white or straw-yellow. The two analyses by Dr. Genth, proved that the vanadium and arsenic were present in the ratio of 1 : 1. One of these gave, after deducting impurities, and recalculating to 100:

As_2O_5	V_2O_5	Cl	PbO
10·73	7·94	2·18	79·15=100·00

The crystals of descloizite are exceptionally fine for the species. They include minute crystals mostly of a red to reddish-brown color, generally united in groups or forming incrustations; also a larger variety up to 8^{mm} in diameter and brownish-black to black in color. The former are octahedral, the latter rather prismatic in habit. Careful measurements by vom Rath go to prove that the species is orthorhombic in form, not monoclinic as made out by Websky. Several analyses of the different varieties conform to the generally accepted formula for the species, analogous to those of adamite, libethenite and olivenite. Iodyrite also occurs with the vanadates at the Sierra Grande mine in sulphur-yellow hexagonal crystals, or in crystalline masses.—*Amer. Philosoph. Soc.*, April 17, 1885.

16. *Hardness of the Diamond.*—At a recent meeting of the New York Academy of Sciences, Mr. G. F. Kunz called attention to some experiments made by the Messrs. Tiffany upon a diamond of extreme hardness. In the rough state it had a rounded form and was of composite crystallization ("round bort").

It had been cut into the rude outline form of a brilliant, and its table had been placed on a diamond polishing wheel for 100 days. The average circumference of that part of the wheel on which it was placed being about 2½ feet, and the wheel going at the rate of 2,800 revolutions per minute, the surface that traveled over the diamond table amounted to over 75,000 miles. At times, four and eight pounds were added to the usual 2¼ to 2½ pounds of the clamp or holder, and for a time forty pounds extra were added, this last causing the wheel to throw out scintillations for several feet. The diamond fairly ploughed the wheel, practically ruining it, so that it required planing before it could be further used. No polish was produced, however, sufficient to give the brilliancy necessary in any diamond gem.

17. *A transparent crystal of Microlite;* by W. E. Hidden. (Communicated.)—The crystal here described was found three years ago in the mica mine near Amelia Court House, Amelia county, Virginia, (see this Journal xxii, 82, and xxv, 335. This crystal weighed 0·877 gram and showed the planes O, 3-3, i and 1 about equally developed. It differed from others found at the locality in its hyacinth-red color, in its perfect transparency and high specific gravity, viz: 6·13. Dunnington obtained for the Amelia microlite 5·66, Shepard for that of Chesterfield 5·56, and Nordenskjöld for Utö crystals 5·25. The high specific gravity of the crystal now described suggests that it may consist more largely of calcium tantalate and less of the columbate than the others that have been examined. The perfect transparency and freedom from flaws prompted me to send this specimen to the lapidary. The result was a gem which had all the brilliancy and beauty of a fine hyacinth, or of an essonite garnet.

I might also add that the cabinet of Mr. C. S. Bement of Philadelphia contains some pyrope-colored microlites from the same locality of nearly one centimeter diameter, embedded in smoky quartz; they are transparent in part and have highly polished planes. They were at first, not unnaturally, mistaken for garnets by the finders.

18. *Emeralds from North Carolina.*—Mr. J. A. D. Stephenson of Statesville, N. C., in a recent letter to the editors, calls attention to the fact that the occurrence of emeralds in Alexander county, announced as a new discovery on page 250 of volume xxix of this Journal (March, 1885), was the same as that which had been already described on page 153 of volume xxvii (February, 1884).

19. *Uranium minerals in the Black Hills*—Mr. L. W. Stillwell mentions the finding of pitchblende and uranium mica (probably autunite), the latter as a thin incrustation, on Bald Mountain in the Black Hills, Dakota.

III. Botany and Zoology.

1. *The Woods of the United States, with an account of their Structure, Qualities, and Uses;* by C. S. Sargent. New York, D. Appleton & Co. 1885. pp. 283, 8vo.—A handy volume, prepared especially as a guide to the magnificent Jesup collection of wood in the American Museum of Natural History at New York. The matter is condensed from the author's volume on the forest wealth of the United States, forming the ninth volume of the Tenth U. S. Census, which was noticed in our March number. This compact and cheap volume should be useful far beyond its immediate design. A. G.

2. *Eucalyptographia: a descriptive Atlas of the Eucalypts of Australia and the adjoining Islands;* by BARON FERD. VON MUELLER, K.C., M.G., & S. Melbourne, 1879–1884, 4to.—The tenth decade of this great work and the one hundred plates besides the accessory ones having now been completed, the indefatigable author here closes the volume, giving title page, dedicatoin to the Prince of Wales, some general remarks, a detailed character to the genus, a synoptical view of the species, a geographic schedule, and three indexes. But the undertaking is not yet completed to the author's satisfaction. Twenty or thirty species are yet to be illustrated, none of them common or of known economic importance, and several still obscure for want of sufficient material. These Baron Mueller proposes to illustrate in "at least two more decades;" and he contemplates even a revision of the main body of the work, and the incorporation of recently accrued material; so that a second volume may in time be looked for. The courage, perseverance, and public spirit of the author are much to be admired. What an immense amount of work he has already done for the Australian flora! A. G.

3. *Les Organismes Problématiques des Anciennes Mers;* par le Marquis de SAPORTA. Paris: Masson, 1884. pp. 93, tab. 13, imp. 4to.—This follows up the author's *Apropos des Algues Fossiles*, in an equally sumptuous volume, even more richly illustrated. Besides the 13 plates there is a double one serving as frontispiece, representing *Bilobites prendo-furcifera*, of the natural size of the specimen, and a good number of wood-cuts in the letter-press. Four of the plates illustrate fossilization in demi-relief of *Brachyphyllum* and *Nymphæa*, the others *Gyrolithes*, *Vexillum*, and *Bilobites*,—vestiges of problematical forms of primordial seas, which in the letter-press are elaborately expounded. He brings new evidence and considerations to show that these are really casts of organisms. A. G.

4. *The Lythraceæ of the United States;* by E. KOEHNE. Bot. Gazette for May, 1885, with a plate.—It is gratifying to have articles from European botanists of mark in our own journals, and Dr. Koehne (of Berlin and Friedenau) may first of all be complimented for his idiomatic English. It is well-known that he has made an exhaustive study of the *Lythraceæ*, the full results of which appear in his monograph of the order published in Engler's Botanische Jahrbucher. Although this order is feebly represented in this country, yet we have more genera and species than any other part of the northern temperate zone, and there was a good deal to be done for the elucidation both of genera and species. The present article summarily presents the result so far as affects North American botany, and its publication in Coulter's Botanical Gazette renders it accessible to those most interested.

As to the propriety of re-establishing the Linnæan genus *Rotala* (which Bentham had referred to *Ammannia*), we could not now give a valuable opinion. Dr. Koehne says that, "it will be rather difficult, I fear, to convince North American botanists of the necessity of separating the genus *Rotala* from *Ammannia;* for, unfortunately, the only species met with in the United States (*A. ramosior L.*), has an Ammannia-like habit, which is not to be observed in the 31 other species of *Rotala*." Our bias, accordingly is against the separation. The restoration of *Didiplis* to *Peplis* may be more readily acceded to. It may be hoped that the species of *Lythrum* are well discriminated, also the genera *Nesæa*, *Heimia*, and *Decodon*. But we must enter a protest against the making of a new name, *Cuphea petiolata*, Koehne, for *C. viscosissima*, Jacq. It legitimately follows from the Laws of Nomenclature which Dr. Koehne generally adopts, that no new name should be made when there is already a fit one, that the earliest name under the proper genus should stand, notwithstanding any older specific name under some other genus. This has been well argued by Bentham, and we supposed that the general practice was conforming to it. A. G.

5. *Monographie der Gattung Clematis von* Dr. OTTO KUNTZE (*Separatabzug aus den Verhandl. Botan. Vereins, Brandenburg*, xxvi.) Berlin, 1885, pp. 83–202, 8vo.—There are two opposite extremes of departure from the Linnæan idea of species which are exemplified in recent phytography. One is that of Gardoger, who multiplies the European Roses into a thousand and more of species. Of the other extreme Dr. Kuntze is a type. For instance, in the present elaborate essay, he reduces *Clematis Virginiana*, *ligusticifolia*, *Catesbeyana* and *Drummondii* to forms of *C. dioica;* also *C. reticulata* as well as *C. coccinea* to *C. Viorna*, adding moreover Lavallée's *C. Sargenti*, which belongs to *C. Pitcheri*, which again is referred to the *C. cordata* of Sims. Then, under the Old World *C. Viticella* he assembles *C. crispa* and *C. Walteri*, even under the same subspecies, and will still have it that the erroneous *C. crispa* of DeCandolle, which he well refers to his var. *Campaniflora* is North American as well as S. European. The only reason for so attributing it is that it has been cultivated under the false name of *C. crispa*, and the genuine *C. crispa* L. came from North America. Also *C. verticillaris* is referred to *C. alpina*, and *C. ochroleuca* as well as *C. Fremontii* to *C. integrifolia!* To compensate for which extraordinary unions, Dr. Kuntze has made a new species, *C. pseudo-atragene*, of part of the North American *C. alpina* of our botanists (with the habitats Oregon, Colorado, New York, Canada, and with a var. *pseudo-alpina* for Fendler's New Mexican specimens), while he gives us *C. alpina, typica* from Lyell's and Bourgeau's collections. A. G.

6. *Recherches Anatomiques sur les Organes Vegetatifs de l'Urtica dioica;* par A. GRAVIS, D.S. Nat., etc. Bruxelles, 1885. pp. 256, tab. 23, 4to.—One of the *Memoires courronnés* of the Royal Academy of Sciences of Belgium, a goodly volume, as it were, devoted to the anatomy of the vegetative organs of the common Nettle, thus taken as a basis for a general and comparative study of the *Urticaceæ*. This exhaustive work was done in the botanical laboratory of the faculty of sciences of the University of

Lille, under the direction of Prof. Bertrand, and in the new botanical institute of the University of Liége, directed by Prof. Morren. The intimate structure of stem, root and leaf is worked out and exemplified under all modifications attendant upon age, level and biological conditions, and the importance of recognizing these conditions and states is insisted on. This treatise would be an excellent *vade mecum* and guide for any student who wished to undertake serious work in vegetable histology. The style of exposition is as clear as are the illustrative figures. Perhaps this is because it is French. A. G.

7. The following Catalogues should also receive notice :

A Preliminary List of the Plants of New Brunswick: Compiled by Rev. JAMES FOWLER, M.A., now of Queen's College, Kingston, Ontario. Published at St. John, N. B., 1885.—The author has aimed "to exclude every species of which he had not seen a specimen"; and this gives a real value to this catalogue. Moreover, the ballast-deposit plants and those just escaping from gardens are wisely placed in a separate list. The author modestly calls his work a preliminary list, and in the title states that it was compiled "with assistance of members of the New Brunswick Natural History Society." He is one of the best and most conscientious botanists of the Dominion; and this list has particular interest in relation to the most northern limit attained by many United States species.

Check-list of North American Gamopetalæ from Gray's Synoptical Flora; by H. N. PATTERSON, editor and printer. Oquawka, Illinois.—We have neglected till now to notice this neatly executed pamphlet, which follows the same author's List of North American Polypetalæ, from Watson's Index. Mr. Patterson does all his work tastefully and well.

Check-list of North American Mosses and Hepaticæ; arranged from Mosses of North America by Lesquereux and James and the Descriptive Catalogue of Hepaticæ by Underwood; by CLARA E. CUMMINGS, Wellesley College. An indication of the increasing interest now taken in Mosses and their allies through the facilities supplied by the two works from which this list is compiled.

Catalogue of the Phænogamous and Vascular Cryptogamous Plants of North America; by Dr. J. H. OYSTER, Paola, Kansas. 1885.—This is also a check-list for convenience of botanists in making exchanges. The species are numbered in succession, and run up to 9,750. It is better in print and proof-reading than would be expected. There is evidently a considerable demand for such lists, and the supply now seems adequate to the occasion. A. G.

8. *Eggs of Echidna hystrix.*—The discoveries of Dr. HAACKE and Mr. CALDWELL are briefly mentioned in this Journal, vol. xxviii, 475, and xxix, 74. Dr. Haacke gives additional results in a communication of January 8, 1885, to the Royal Society of South Australia. He states that the egg was about 15^{mm} long

and 13mm wide, and it had a parchment-like shell like that of many reptiles, which was ½mm thick. He inclines to the opinion of Professor Gegenbaur (Morphologisches Jahrbuch, 1884), who examined two adult females of *Echidna setosa*, that the pouches are periodically developed; and that "the pouch attains its greatest development during the time it serves as incubator for the egg, and that it consists of a pair of semi-lunar fossæ during the period the mother suckles her progeny, disappearing afterwards altogether."

In the examination in September of two male Echidnæ he discovered "the unmistakable homologue of the mammary gland of the female." He found "in situations corresponding to those in the female, similar tufts of short hair quite as plain as those indicative of the mammary areolæ in the female;" and, on skinning the animals, the rudimentary mammary glands were without difficulty discovered. "In the largest of the two specimens the one mammary gland forms a mass about 8mm long by 4mm wide, the lobules being about 2mm long. The other gland is a little larger. The glands are built after the same plan as the female glands, and contain a considerable number of lobules." Dr. Haacke adds: "In conclusion, I should not forget to mention that I am aware of Mr. W. H. Caldwell's discovery of the oviparity of the Monotremata, made about the same time as it seems at which my discovery was made, and that the British Association at their Montreal meeting received the information of Mr. Caldwell's discovery about the same date at which my *Echidna egg* was exhibited at a meeting of the Royal Society of South Australia. But this is all I know at present about Mr. Caldwell's discovery."

IV. Miscellaneous Scientific Intelligence.

1. *American Philosophical Society of Philadelphia.*—This society, whose first meeting was held in the year 1743, has published in a volume of 876 pages, the early correspondence and the minutes of the meetings, from the year 1744 to July, 1838, at which time the publication of the "Proceedings" heretofore printed, began. It is accompanied with copious indexes. The first letter with regard to the establishment of the society published in the volume is by Benjamin Franklin, bearing the date April 5, 1744, and a lithographic fac-simile of it makes a frontispiece to the volume. It states that "the society, as far as relates to Philadelphia, is actually formed, and has had several meetings to mutual satisfaction." The members, as stated in it, were "Dr. Thomas Bond, Mr. John Bartram, Botanist, Mr. Thomas Godfrey, Mathematician, Mr. Samuel Rhodes, Mechanician, Mr. Wm. Parsons, Geographer, Dr. Phineas Bond, General Natural Philosopher, Mr. Thomas Hopkinson, President, Mr. Wm. Coleman, Treasurer B. Franklin, Secretary," to whom others were later added. The expression "as far as relates to Philadelphia" is shown afterward to have implied that the original scheme in view was the

establishment of an "American Philosophical Society" with one of its component societies at Philadelphia. In January, 1768, the number of members was about fifty, and Mr. Winthrop of Massachusetts, Dr. Styles of Connecticut, Hon. Cadwalader Colden of New York, Dr. Lee of Virginia, and Dr. Garden of South Carolina were made Corresponding Members.

2. *American Association for the Advancement of Science.*—The next meeting of the American Association—the thirty-fourth—will be held in Ann Arbor, Michigan, beginning on Wednesday, 26th of August. The circular of the Local Committee states that the buildings and grounds of the University of Michigan will be placed at the disposal of the Association during the meeting. Arrangements are made for an excursion free of all expense to the Saginaw valley, and another, after the adjournment, to Detroit and Mackinack Island and return. The Secretary of the local committee of arrangements is Professor John W. Langley. Members who intend to be present are desired to inform the Secretary in advance as to the kind of accommodations they may prefer, in order that arrangements may be made accordingly. The Michigan Central Railroad will probably run a special train from Buffalo to Ann Arbor, leaving Buffalo, on Monday morning, August 25th, if a sufficient number of members signify their wish to avail themselves of it; and information on this point also should be sent to the secretary. This train will stop, if the party wish to do so, from one to two hours at the Falls View Station, Niagara Falls, "where ample facilities for visiting the Falls will be found."

The University offers its apparatus for any experimental illustrations that may require it, and will "furnish electricity either from a dynamo, from a storage battery, or from primary batteries as may be needed by members reading papers on electrical subjects.

3. *Meteorological Circular Letter.*—For the following circular letter, this Journal is indebted to General Hazen, to whom it was addressed by the International Committee of Meteorology, St. Petersburg and London, May 1, 1885:—

In compliance with the instructions of the International Congress on Meteorology at Rome, we have the honor to inform you that the International Committee on Meteorology instituted by this congress will meet for a third session in Paris in the beginning of the coming September and that up to the present time the following questions have been proposed for consideration during this session:

(1) Report of the Secretary on the labors of the Committee since the meeting at Copenhagen.

(2) Report of Messrs. Brito Capello, Hildebrandsson and Ley on the observation of the cirrus.

(3) Does it seem opportune to soon convene a third international congress of Meteorologists?

(4) Establishment of stations of the first order on the Congo.

(5) Discussion on the utility of the summaries of the state of the weather as

published in the different countries, and eventual preparation of plan for more uniformity.

(6) Discussion of the utility on the meteorological telegrams from America proposed by Mr. (General) Hazen, and of an eventual organization for their distribution in Europe.

(7) By what means can the timely receipt of meteorological telegrams be assured?

(8) Should the reduction of barometer readings to gravity under 45° latitude be generally introduced?

(9) Is it desirable to count also in Meteorology the hours of the day from 1^h up to 24^h in accordance with the resolutions of the international conference in Washington?

(10) Designation for a uniformly covered sky according to the form of the clouds.

(11) Definition of rain and snow days.

(12) Should not the general adoption of a uniform height above the earth for rain-gauges be recommended?

(13) What progress has been made lately in the more exact measurement of snow?

(14) International Meteorological tables.

(15) Modification of the rules for the administration of the International Committee.

In case you intend to submit to the Committee remarks on one or the other of these questions we request that you will please address them in good time to Mr. Robert H. Scott, (Meteorological Office, 116 Victoria street, London).

For the International Committee on Meteorology.

(Signed) N. Wild, Pres., R. H. Scott, Sec.

4. *Digestion Experiments.* Note to the Editors by H. P. Armsby.—After the proof of my article on Digestion Experiments in the May number of this Journal (p. 355, vol. xxix) had left my hands, my attention was called to the fact that Dr. E. Lewis Sturtevant, Director of the New York Agricultural Experiment Station, had published a preliminary account of digestion experiments made at that institution about a year previously, and about a month before the first account of my own experiment was printed. The opening sentences of my article should therefore be modified in accordance with this fact.

5. *A Catalogue of Chemical Periodicals;* by H. Carrington Bolton, Ph.D., Prof. Chem. Trinity College, Hartford, Conn.—This valuable catalogue by Professor Bolton is published in vol. iii of the Annals of the New York Academy of Sciences, pages 159 to 216. The same number of the Annals contains a paper by Albert R. Leeds, on the literature of ozone and peroxide of hydrogen, and by G. N. Lawrence on new species of birds of the Families Tyrannidæ, Cypselidæ and Columbidæ.

6. *When did Life Begin;* a monograph by G. Hilton Scribner. 64 pp. 8vo. New York, 1883. (Charles Scribner's Sons).—*Paradise Found*, by Wm. F. Warren, President of Boston University. Third edition, 496 pp. 8vo. Boston, 1885. (Houghton, Mifflin & Co.)—The first of these works aims to show that life, the earliest and latest, began in the Arctic region. The latter sustains the polar origin of man. Both are too far within the realms of speculation for further notice in this place.

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EDITORS

JAMES D. and EDWARD S. DANA.

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THE

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[THIRD SERIES.]

ART. XII.—*Origin of Coral Reefs and Islands;* by JAMES D. DANA. With a map (Plate II).

THE Presidential Address of Dr. Archibald Geikie, Director-General of the Geological Survey of Great Britain, before the Royal Physical Society of Edinburgh in 1883,* reviews the subject of the origin of coral reefs and islands. In the course of the discussion, the author sustains and enforces the objections which have been presented by others, and concludes that "the existence of such reefs is no more necessarily dependent on subsidence than on elevation." The existing state of doubt on the question has led the writer to reconsider the earlier and later facts, and in the following pages he gives his results.† That both sides may be fairly before the reader, the views of Darwin and the evidences in favor of his theory are first considered, and afterwards the arguments that have been urged against it. Part of the objections are based on misunderstandings of the facts, and hence a general presentation of the subject has been thought necessary.

* Proceedings Edinb. Roy. Phil. Soc., viii, 1, 1883.

† The writer's account of his original observations is contained in his Wilkes Expedition Geological Report, 1849 (756 pp. 4to), pages 29–154; and, less completely, along with a review of facts from other regions, in his Corals and Coral Islands, 398 pp. 8vo, 1872, 1875.

Part I.—The Darwinian Theory and its Evidences.

1. According to the Darwinian theory, islands with fringing reefs have been often changed through a slow subsidence of the region into islands with barrier reefs; and, as the last summit of the sinking land disappeared, the latter have become atolls, that is, barrier reefs enclosing simply a piece of the ocean (or a lagoon). Darwin added to this conclusion, a *second*, in view of the wide distribution of atolls and their relations to other islands: that the subsidence indicated involved the whole central Pacific, besides other large areas. He also expressed the opinion that a Pacific continent had disappeared through the subsidence. The proofs of the first and the second conclusions are partly different and should not be confounded. The third is no necessary part of the general theory, was not adopted in my Report, and need not be further considered.

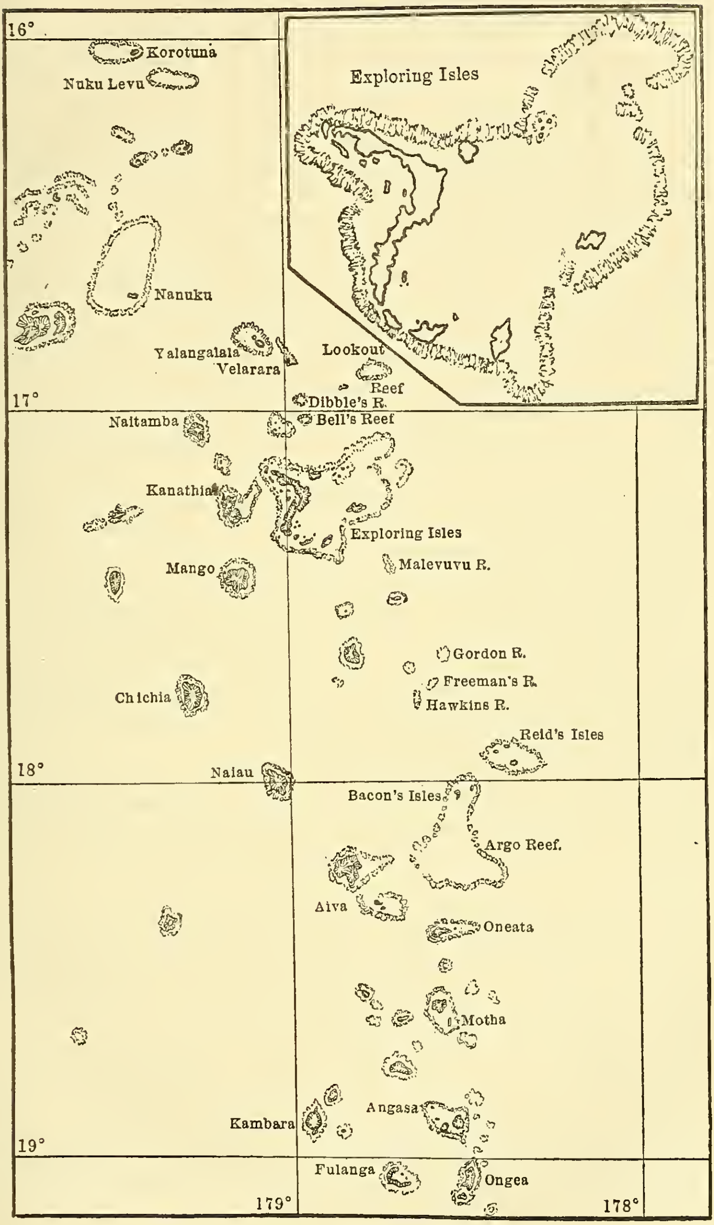
2. Darwin did not hold that atolls were necessarily evidence of a subsidence *now* in progress, but allowed that in some regions they may have reached a state of rest, and may perhaps have undergone an elevation since the cessation of the subsidence; and also that subsidence and elevation may have alternated.

3. Darwin found what he believed to be *almost certain proof of subsidence* in the features of the large barrier-islands and atolls. He perceived in the rocky islets that dot the great interior lagoon-like waters of the Gambier group, Hogoleu, and other similar barrier-islands of the Pacific, and the general resemblance of such islands to atolls, strong evidence, "leaving scarcely any doubt on the mind," that the islets were the emerged points of *sunken* lands; that such barrier-islands were no less lagoon islands than Keeling atoll (the atoll which he investigated); and if evidence of subsidence, the atoll was proof of further subsidence, that is, one that had continued to the disappearance of the sinking peaks.

The evidence which had satisfied him was satisfactory to me when I first learned of his views in Australia (in 1839), after a cruise among the Paumotu atolls and the Tahitian and Samoan reef-regions; and more decidedly so later, when I had been among the Friendly, Feejee and other Pacific Islands.

That the argument may be appreciated I here introduce a map of the eastern half of the Feejee Archipelago.* Several of the great barrier reefs in this map, 10 to 20 miles in length, have but one or two peaks of the sunken land remaining; Nanuku has but one little point near its southeastern angle, a mile of peak within a barrier island 200 square miles in area;

*Reduced from the general map of the archipelago in the Atlas of the Wilkes Expedition.



EASTERN PART OF THE FEJEE ARCHIPELAGO

Bacon's Isles are the last two little peaks of a still larger lagoon; and besides these and other examples of disappearing lands, a dozen of the easternmost islands are actual atolls—the last peak gone.

4. To this the chief of Darwin's arguments in his own view, another of like importance is added in my Report: *the existence of deep fiord-like indentations in the rocky coasts of islands, both of those inside of barriers and those not bordered by reefs.*

When making the ascent of Mount Aorai, one of the two high summits of Tahiti (September, 1839), where high narrow ridges, almost like a knife-edge along their tops, alternating with gorge-like valleys 1000 to 3000 feet deep, radiate from the central peaks but die out in a broad even plain at the shores, I was made to appreciate the consequence to such an eroded land of a partial submergence. At any level above 500 feet its erosion-made valleys would produce deep bays, and above 1000 feet, fiord-like bays, with the ridges spreading out in the water like spider's legs. Observing on the maps of the Marquesas Islands precisely this condition, it was a natural inference that the lands had undergone great subsidence, and perhaps were still subsiding.

With this criterion of subsidence in view, the evidence from the Gambier and Hogoleu islands is doubled in force: and that for the sinking of Raiatea of the Tahiti group, represented in fig. 3 of the plate of maps in Darwin's Coral Reefs, is as strong from each of the two enclosed islands as it is from the great breadth of the reef-grounds; and the same is true for Bolabola, another Tahitian island on the plate.

So it is also in the Feejees. The demonstration as to subsidence in the large barrier-island called the Exploring Isles, for example, is made complete by the form of the ridge of land along one side of the great barrier, and the positions of adjoining islets.

5. The general parallelism between the trends of coral islands and the courses of the groups of which they are a part, and the courses also of the groups of high islands not far distant, were regarded by Darwin as confirming his view that the coral islands were once high islands with bordering reefs.

6. Darwin uses also the argument that the larger coral islands have the diversity of form found in the barrier reefs of high islands; and also that they often have such groupings as would come from the sinking of a large island of ridges and peaks with encircling reefs. He describes the Maldives as one example of the latter, and the two loops of Menchikoff island in the Caroline archipelago, as another.

7. The depth of lagoons, and of the channels inside of large barrier reefs, afforded him further evidence of subsidence, it

being in many cases two to three times greater than the limiting depth (120 feet) of living reef-making corals.

8. The great depth of the ocean in near vicinity to the atolls is another source of evidence added.

9. He urged also, in supporting his views, the non-existence in the ocean now, and the extreme improbability of existence at any time, of submarine volcanoes or chains of mountains having their numerous summits within a hundred feet of the surface.

10. Darwin speaks of smallness of size in coral islands as a result of continued subsidence. In my Report I base an argument for subsidence on smallness in the proportion of dry land, and on smallness of size, when there is *gradation* toward either condition, and the seas beyond are free of islands. The facts bear on the general conclusion with regard to a Central-Pacific area of subsidence as well as on the fundamental point in the theory.

If an atoll-reef is not undergoing subsidence the coral and shell material produced that is not lost by currents serves: (1) to widen the reef; (2) to steepen, as a consequence of the widening, the upper part of the submarine slopes; (3) to accumulate, on the reef, material for beaches and dry land; and (4) to fill the lagoon.

But if, while subsidence is in progress, the contributions from corals and shells exceed not greatly or feebly the loss by subsidence and current waste, the atoll-reef, unable to supply sufficient debris to raise the reef above tide level by making beaches and dry land accumulations, would (1) remain mostly a bare tide-washed reef; (2) lose in diameter or size because the debris that is not used to keep the reef at tide-level is carried over the narrow reef to the lagoon by the waves whose throw on all sides is shoreward; (3) lose in irregularity of outline and thus approximate toward an annular form; (4) lose the channels through the reef into the lagoon by the growth of corals and by consolidating debris; and (5) become at last a small bank of reef-rock with a half obliterated lagoon-basin.

The Pacific contains reefs of the three kinds: (1) atolls with much of the reef under trees and shrubbery; (2) others, of large and small size, with the reefs mostly or wholly tide-washed; (3) others only two or three square miles in area, without lagoons. Further, the kinds are generally grouped separately and gradationally. (1) The islands of the Paumotu and Gilbert archipelagos have usually half or more of the reef dry and green; (2) the northern Carolines and the northern Marshall Islands, and the eastern Feejees, although in part of large size, are mostly bare reefs; while (3) the islands of the Phœnix Group, of the equatorial Pacific east of the line of 180°, are, with one exception (Canton or Mary), not over four miles long. The three more southern of the Phœnix Islands (see Map, Plate II), Gardner's, Hull's and Sydney, between 4° 25′ S. and 4° 35′ S., are two to four miles long and have lagoons; five, including Phœnix, Birnie's and Kean's, between 3° 10′ S. and 3° 30′ S., and Howland and Baker's north of the equator, are a mile and a half and less in length, and have depressions at centre but no lagoons. The depressions contain guano, and one of them, Kean's, has much gypsum mixed with the guano;* Kean's and Phœnix have a foot or two of water at high tide, the tide rising 6 feet. Another of the number, Enderbury's, is three miles long and has a half-dried lagoon which is very shallow and has no growing corals.† To the north of these islands for fifteen degrees of latitude, the sea is an open one, and in the next ten degrees, to the line of the Hawaian Chain, the only islets not marked doubtful are "Coral Reef, Awash" and Johnston Island.

A similar gradation in size takes place in the Ellice, Ratack and many other groups of the ocean. Smallness of size, and dried lagoon basins, with occasionally a deposit of gypsum from evaporated sea-water, are just the result that should have come, by the Darwinian theory, from subsidence; and gradation in size from gradation in the amount of subsidence. The positions of the Union, Gilbert, Ratack and Ralick Groups with reference to the Phœnix Group are shown on the Map, Plate II. All of the islands on the map are coral islands, and nearly all atolls; and the part of the encircling reef marked by fine dots is under water at high tide.

Adopting this view of the origin of these smallest of coral-made islands, I readily accepted Darwin's second conclusion as to a great central oceanic area of subsidence. The further inference, also, was deduced, for reasons stated in my Report, that the greatest amount of subsidence took place along a belt stretching southeastward from the southern half of Japan and passing south of the Marquesas Group toward Easter Island, and a line was drawn on a map among its illustrations representing the course of "the axial line of greatest depression."‡

* J. D. Hague, Amer. Jour. Sci., II, xxxiv, 242. Mr. Hague, in his valuable paper on the Guano Islands of the Central Pacific, mentions the existence of a bed of gypsum two feet thick under the guano of Jarvis Island, another small equatorial island, eleven degrees east of the Phœnix Group.

† Baker's Island has a height of 22 feet according to Mr. Hague, showing, he says, some evidence of elevation; and Enderbury's I found to be 18 feet in height, from which I inferred some elevation. But Howland's, Birnie's, McKean's, Phœnix, Gardner's, Hull's and Sydney are not higher than ordinary atolls would be in a sea of 6-foot tide.

The facts with regard to the "Reef" on the map, in long. 175° W. and lat. 2° 40′ S., I have been unable to learn.

‡ Report, pp. 399 and 432, and map between pages 8 and 9.

This line is reproduced on a chart of the World, in my Manual of Geology, where it is lettered A′ A′.

These deductions have been apparently sustained by the soundings of the Tuscarora and Challenger in 1874, 1875 and 1876. The soundings of the Tuscarora through the Phœnix Group in 1875, on its route from the Sandwich Islands to the Feejees (under the command of J. N. Miller, U. S. N., by the order of the U. S. Hydrographic Bureau), are shown on the map of the central Pacific herewith published (Plate II).

The soundings about these islands prove (1) that the islands are situated within the deep 3,000–4,000-fathom area of the ocean; and appear to indicate also (2) that along lines *transverse* to the trend of the islands (or to the direction of trend in other groups to the west), mean submarine slopes of 1 : 1·5 to 1 : 7 exist; while in the direction of the trend, the slopes are much less. The slope of 1 :1·5, or that of the angle 33° 41′, is nearly the maximum slope of the sides of Cotopaxi, Mt. Shasta and several other volcanic summits of Western America.

The facts are these.

Half way between Sydney and Birnie's islands, 60 English miles apart, a depth of 3000 fathoms (18,000 feet), was found. Off Enderbury's Island (40 miles northeast of Birnie's), (1) a depth of 2835 fathoms was obtained 20 miles to the southwest; (2) of 880 fathoms 2½ miles to the southwest; (3) of 1991 fathoms 3 miles to the northeast; and (4) of 2370 fathoms, 11 miles to the northeast. The mean slopes to the southwest, calculated from the soundings 1 and 2 are 1 : 6 and 1 : 3; and to the northeast, from 3 and 4, 1 : 1·5 and 1 : 4, fourteen miles south*east* of Hull's Island, at right angles to the above direction, a depth of 935 fathoms was found, which gives the slope 1 : 13.

Further evidence as to the submarine slopes about equatorial coral-reef islands is afforded by soundings made under the direction of the British Admiralty, near the very small Swain's Island, at the south end of the Union Group (see map); and others, by the Tuscarora under Commander Miller, in 1876, near the Danger Islands, about five degrees east of Swain's. Off Swain's Island, two soundings, one south of it and the other east, (the two directions at right angles to one another and the latter not diverging far from the trend of the other islands of the Union Group), give the slopes 1 : 7 and 1 : 13. Off Danger Island, as Commander Miller's Report states,* the depth of 660 fathoms was obtained half a mile (nautical) off the southwest corner of the reef near Southeast island, and 985 fathoms one mile east of the reef,—corresponding to slopes 1 : 1 and 1 : 0·75. 1 : 1 is a steeper slope than occurs even in small dry-made cinder cones; and 1 : 0·75 (53° 8′) is steeper still.

* I am indebted for the soundings about Danger Islands to Commander J. R. Bartlett, Superintendent of the U. S. Hydrographical Bureau.

The above facts are sufficient to authorize the drawing of the bathymetric lines for 1000, 2000 and 3000 fathoms *quite closely* about the islands of the Phœnix Group, and to give the areas a northwest–southeast elongation, corresponding with that of the neighboring Pacific islands to the west, as on the accompanying map, Plate II.*

It follows from the above mentioned facts that the deep-water areas adjoining the Phœnix Group, named provisionally by Petermann† the "Hilgard depths" and the "Miller depths," are parts of one large area 1200 miles broad. The greatest depth obtained in the part of the area southwest of the group (400 miles broad) is 3305 fathoms, and in the part northeast, 3448 fathoms.

Again: the soundings of the Tuscarora of 1875 here cited, taken in connection with those of the same vessel in 1874, under Commander George F. Belknap, along a line from the Sandwich Islands westward to Japan (mostly between the parallels of 20° and 25°), suggest the further conclusion: that the deep-sea area of the central equatorial Pacific, in which the Phœnix Islands stand, extends northwestward toward Japan, and that it was crossed by the Tuscarora between 171° E. and 150° E, where were found depths from 3009 to 3273 fathoms (with some alternations of smaller depths that isolated areas may account for). It is also probable that the soundings of the Challenger east of Japan between 153° E. and 143° E. and just northwest of those of the Tuscarora, were within the same deep-sea area. If this be so, a long deep-water area or trough extends from Japan southeast-

* The line on the map for 1000 fathoms is a simple dotted line; that for 2000 fathoms, - - - - - ; for 3,000 fathoms, – – – – .

† Geogr. Mittheil., 1877, page 125 and plate 7. The deep areas along the lines of soundings, were named by Petermann on his very valuable bathymetric map of the Pacific simply to facilitate reference.

The bathymetric lines about the islands, on the accompanying map, Plate II, have an unreasonable degree of regularity. But with no facts to indicate the actual irregularities, none could be reasonably introduced. The trends given them are the same as on Petermann's map. The actual steepness of slope is probably not exaggerated for either of the islands. If similar slopes exist about the smaller islands in other parts of the ocean, the final bathymetric map of the Pacific will have a very different aspect from that presented by the maps hitherto published, and the Central Pacific a much greater mean depth. About Wakes Island, a small atoll in latitude 19° 11′, standing alone in the ocean six degrees north of the Ralick Chain, the width of the area enclosed by the 2000 fathom line, as drawn on Petermann's bathymetric map, is nearly 100 nautical miles, while, in view of facts at the Phœnix Group, the actual width is probably not over 10 or 15 miles.

With but four lines of soundings for the part of the great Pacific Ocean, within 35 degrees of the equator, the author of a bathymetric map has to rely chiefly on his judgment or conjecture for the larger part of the surface. There are many great problems in physical, geological and biological science that would be elucidated by the facts which a thorough bathymetric survey of the ocean would afford; and the work is large and important enough to call for aid from each of the great nations of the world. Thus far, for the Pacific Ocean, the United States is first in the amount of work done.

ward through the Central Pacific, conforming well to the suggestion of the Darwinian theory; and corresponding in direction approximately to the "axial line of greatest depression" referred to above—the line AA on the accompanying map.

As regards the rest of the Central Pacific between the above defined 3,000–4,000-fathom belt and the Hawaian chain, the depths sounded by the Tuscarora are, with few exceptions, over 2400 fathoms; two-thirds of them are over 2750 fathoms, and a fifth (out of the fifty-five in this area) over 3000.*

11. Since a fringing reef is, by the theory of Darwin, the first stage in the origin of an atoll, it was naturally regarded by him as, in general, evidence of little or no subsidence, and even, at times, of elevation. But since (1) bold shores are an occasion for narrow reefs and for their absence; (2) submarine volcanic disturbances and eruptions about volcanic lands would destroy living reefs, or retard their progress where begun; (3) islands of active volcanoes have small or no reefs; and (4) abrupt subsidences of only 120 feet would put reef corals below a surviving depth and so lead to the beginning of a new reef, I was led to regard the evidence from a fringing reef for no, or little, change of level, as of very doubtful value. But the doubts, while making such evidence generally useless, do not affect the value of the preceding arguments for subsidence. Darwin used the evidence from fringing reefs only to mark off the limits of the area of Central-Pacific subsidence to which his coral-island theory had led him; and the same limits essentially are reached notwithstanding the doubt. Instead of concluding that the region along these limits was one of recent elevation or at least

* The same two lines of soundings by the Tuscarora suggest the existence of a second long deep-sea belt or trough in the Central Pacific just south of the Hawaian chain. This supposed trough was crossed by this vessel in 1875 between the parallels of 13° N. and 18° N. ("Belknap depths"), and in 1874 between the meridians of 172½° W. and 177½ W. (the "Ammen depths"); the greatest depth found on the former line is 3125 fathoms, and on the latter 3106 fathoms. Should the existence of these two troughs be sustained, the region between them would be a Central-Pacific plateau; having in it, along the 1875 line of soundings, depths of 2972 to 1325 fathoms, and along that of 1874, depths of 2836 to 1108 fathoms; the shallower portion is near the middle of each line of soundings, has a great descent (6,000 to 9,000 feet) on either side—suggesting the idea of a central ridge. Over this plateau-area, there are, south of the Hawaian chain, two or three small coral islands, and farther eastward, the Palmyra, Kingman, Washington, Fanning and Christmas reefs and islands which, although coral structures, make the idea of a central ridge in this part for 600 miles almost a manifest fact. Farther east, the Marquesas islands are in the same range. The deep belt lying on the south side of the plateau diminishes in depth to the eastward, the Challenger soundings from the Sandwich Islands to Tahiti finding no depth in the course of this belt greater than 2750 fathoms; but the belt on its north side may continue eastward of the Challenger route. Many more lines of soundings are needed to substitute sure conclusions for the above suggestions.

The existence in the ocean of parallel belts of deeper and shallower waters, such as are here inferred to exist, and such as are actually indicated by the parallel lines of high islands and atolls, is in accordance with the facts over the continents.

of no subsidence, I was led to speak of it as one either of no subsidence or of less subsidence than over the central area referred to. The difference between us is small.

12. The true value of fringing reefs as evidence in the question of change of level should be appreciated in this discussion, as is apparent from the objections to Darwin's theory which have been urged ; and I mention a few facts from the Pacific islands in its elucidation.

On Darwin's map, the Marquesas group is left uncolored, which means doubtful as to subsidence or not. The Tahitian group (Society Islands) is colored blue, that is, it is included within the area of coral-reef subsidence. The Navigator or Samoan Islands are colored red, or placed in the area of elevation ; the Feejees, blue ; the Sandwich Islands, red. The facts are these.

The Marquesas Islands are an example of absence of reefs to a large extent with only small reefs where any. But the shores are mostly too bold for reefs ; and hence their smallness bears no testimony as to elevation. Along the bold shores, there are deep indentations and fiord-like bays separated in some cases by narrow ridges sometimes in spider-leg style ; so that the proof of subsidence is positive, as explained in § 4.

Tahiti presents none of the Marquesan evidence of subsidence. Its erosion-made valleys, as already explained, die out at the broad shore plain, and the island is comparatively even in outline. I found over it, like Darwin before me, no evidence of elevation beyond one or two feet at the most. It has broad reefs ; and the channel inside the barriers between Papieti and Toanoa (2 miles) has a depth of 3 to 25 fathoms. From the width of the reef, and the slope (6 to 8 degrees) of the land and of the lava streams outcropping in the sides of the valleys, supposing this slope to be continued under water, I made the probable subsidence 250 or 300 feet. A slope of 6 degrees, and a width of reef of half a mile, gives 240 feet for the depth of the reef at the outer edge.

The Samoan (or Navigator group) includes (beginning at the east) Rose Island (an atoll), Manua, Tutuila, Upolu and Savaii.

Manua has bold shores, a height of 2500 feet, and a narrow reef where any.* Tutuila is of the Marquesan type in its bold indented sides and this suggests a probable subsidence. Pango-

* With regard to Manua, Mr. J. P. Couthouy, of the Wilkes Exploring Expedition for two-thirds of its cruise, in his paper on Coral Formations (p. 50 of Proc. Bost. Soc. Nat. Hist., Jan. 1842) reported the occurrence of fragments of corals at a height of 80 feet "on a steep hill-side rising half a mile inland from a low sandy plain." I was not on Manua. I found on Upolu fragments of coral limestone and shells in the tufa of a tufa cone at a height of 200 feet, which had evidently been carried up by the erupting action of a slightly submerged vent. (Report p. 328.) The facts on Manua need further study.

pango Bay, in which we anchored in 174 feet water, has a length of three miles. The coral reefs are of the *fringing* kind where any occur. Upolu, a few miles west, has bold shores and small or no reefs for fifteen miles of its north shore, east of its middle; but elsewhere there are broad reefs—mostly 5000 to 8000 feet in width—and a very gentle slope (three to six degrees) in the land above—which is about the slope of its underlying lava streams. The great width of the reef seemed to be evidence of subsidence. But the absence on the north side of the island of a channel in the reef deep enough for any craft larger than canoes made it essentially a great fringing reef. A calculation from the width and land-slope gave about 260 feet for subsidence; but I add (on page 332 of my Report) my doubt as to any subsidence. The facts known are against any elevation.

Savaii, the largest island of the group, is a gently sloping volcanic mountain, much like its name-sake, Hawaii, in its features, with lavas looking as if not long out of the fire. It has a broad reef for only 6 or 7 miles of its east shore; elsewhere on the east and northeast sides the reefs are fringing or wanting; and on the southern and western sides mostly absent. No evidence of elevation, and nothing certain as to subsidence, has been reported from the island.

The large Feejee Group bears abundant evidence of subsidence in its very broad reef-grounds, barrier islands, and atolls. Fringing reefs, or barriers with very narrow channels, occur about some of the islands of the group; but in view of the facts that have been stated, these are useless as evidence either way.

Thus the conclusions as to the changes of level about these large Pacific groups south of the equator are not far from Darwin's, although fringing reefs and the volcanic character of the islands are thrown out of consideration, and other conditions exist of varied interpretation.

But cases of actual elevation occur in the Central Pacific about several smaller islands as proved by *elevated* coral reefs. These occur in the Austral and Hervey Islands south and southeast of Tahiti, and in the Tonga or Friendly Islands. In none of these, however, thus far reported is the elevation over 300 feet: and the amount varies greatly in adjoining islands of the same group, *some affording proof of no elevation.* Hence only local changes of level, not a general elevation, can be inferred.

To the north of the equator at the Sandwich Islands some elevated reefs occur. But the amount of elevation is small and is not general in the group. Moreover, the reefs are small, where any occur, and the largest island of the chain, volcanic

Hawaii, the easternmost, is mostly without reefs, as well as the larger of the westernmost islands, Kauai, which has partly bold and indented shores.

13. To give a more adequate view of the changes of level, or the evidences bearing on the subject, along the "limits" of the central area of subsidence, I mention some additional facts.

Tahiti is the large eastern island of the Tahitian Group. To the *westward*, the islands (1) decrease in size; (2) increase greatly in relative breadth of reef-grounds; (3) become deeply indented in shores, as explained; and (4) include an atoll, Tubuai, as one of the last two of the chain. While the reef of Tahiti proves little subsidence at that end of the group, and its reefs and channels are extensive enough to make the proof good, the other islands indicate, on the Darwinian theory, that the subsidence increased much in amount westward. The western end of the chain is about a degree nearer the equator than the eastern.

In the Samoan Islands, the largest island, Savaii, is the westernmost; and from there the islands decrease in size *eastward*, and end in an atoll, Rose Island. The group is like Tahiti in gradation as to increase of subsidence, but the direction is the reverse; and this fact points apparently to a much deeper area between them.* Moreover, although such broad barrier reefs as those of Raiatea and Bolabola do not occur in the Samoan group, bold shores do in Tutuila and Manua, and indicate the participation of these islands in the subsidence, notwithstanding their contracted reefs. Further, the reef of Upolu is broad enough to be proof of little change in the region of that island; and there was little, probably, at Savaii, the larger island west of it. The evidence of increased subsidence *to the eastward* is strong, and narrowness of reef is no objection to it.

At the Sandwich Islands the case is similar and yet different; similar in the fact that the largest island of the chain, Hawaii, makes one of its extremities, the eastern, and a series of coral islands the other—the whole length being 2,000 miles; but different in that *no great reef exists* about the shores of either of the eastern islands to *prove* that the subsidence there was small or none. The elevated reefs are only a local phenomenon, and do not prove the absence of subsidence during the era preceding the elevation.

But we have other evidence of importance derived from soundings about the group by the Challenger in 1875 and the Tuscarora in 1874, 1875. These soundings show that the deep-sea area of 3,000 to 4,000 fathoms comes up quite closely about the eastern end of the chain. It was found within 300 miles of

* The distance between the remote extremities of these two groups is nearly 2,000 miles, and the interval between the nearer, over 800 miles.

northeastern Hawaii and 250 of southwestern, and within 80 miles of northeastern Oahu; and a sounding but 125 fathoms less than 3,000 was obtained by the Challenger within 40 miles of eastern Hawaii (or half its diameter). To the westward, along the north side of the chain, the deep-sea area appears to be two or three times more distant, according to the Challenger results; the condition on the south side is uncertain. It would seem from the great depth near Hawaii, that the region of this great island, although it is now actively *volcanic* and *has little growing coral* about it, had undergone more subsidence than the coral reef end of the chain, and that its height and steepness of submarine slopes are due to the fact that its outflows of lava have kept ahead of the subsidence, and also built up nearly 14,000 feet above the sea.

This height is large, but the mean pitch of the sides of the volcanic mountains of the island is between 5° and 7° 45′, and hence it is only the height which successive outflows should have produced over a vent at the sea-level; and it may be that the accumulation above tide level has been made since the supposed subsidence ceased. The depth of 2,875 fathoms found by the Challenger 40 miles east of Hawaii shows a mean submarine slope to that point of 4° 30′, as if here also was a slope made by flowing lava. But more soundings are needed to prove that the slope is a gradual one.

14. The facts reviewed show *the uncertainty of evidence* as to little or no subsidence, or as to recent elevation, from (1) narrow reefs, or from (2) the volcanic character of islands, and leave untouched the evidence of actual subsidence from the features of barrier and atoll reefs and from deeply indented coasts.

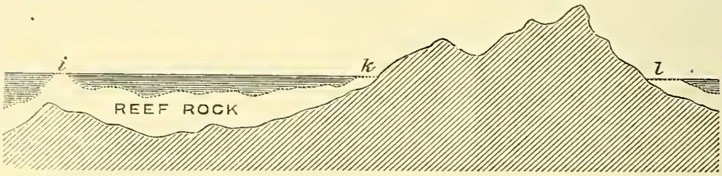
15. After the above considerations, it is clear that the theory of subsidence meets well the facts as to the varying extent of reef among reef-bordered high islands. According to it, (1) steepness of submarine slope may characterize the side of a barrier reef (as well as of an atoll) fronting east or west, north or south, as is true of high islands; but it is least likely to occur in the direction of the trend of the island or group, or that of current drift. (2) Fringing reefs, or no reefs, may characterize one side, that of bold bluffs, and wide barriers the opposite. (3) The barrier reef may be made on the submarine slopes of the land, or on a broad plateau or low-land area between ranges of elevations, one or both of which have disappeared in the subsidence. (4) By continued subsidence, the side having a fringing reef or no reef, may, later in the subsidence, be that of a very broad barrier reef, because of the form of the surface of the subsiding land; and *vice versa.*

The third of these propositions is well illustrated by the facts from the Maldives, as reported by Darwin. On account of its importance I add an illustration from the Feejees.

The great reef-grounds along the northwest sides of the two large Feejee islands, Vanua Lebu and Viti Lebu, do not indicate a subsidence proportional to their width.

Each of these islands is over ninety miles long, and together the trend is northeastward.* The northwestern reef-grounds are 10 to 25 miles wide; while on much of the southeast side of each island there is (according to the Wilkes Chart), only a fringing reef. The true explanation of the great width is found, not in the amount of subsidence alone, but largely in the existence there of a broad area of submerged land at relatively small depths. This inference is sustained by the fact that the outer barrier reef, after being simply a barrier reef for 125 miles with but two rocky islets in its course, becomes in the same line westward for 70 miles, a range of high narrow reef-bordered islands (called the Asaua Range), and then bends around southward through other rocky islands to meet the west end of Viti Lebu. The reef-grounds have thus a chain of islands as their boundary for a length of 100 miles, and simply a barrier reef with two rocky islets for the rest of the line (125 miles).

The following figure illustrates in a general way the above condition. It is a section across the reef-grounds, *i k*, and the



outer barrier reef *i*, with a fringing reef at *l*; and supposing it to have a rocky island at *i*, it represents a section (farther to the southwest) across the reef-grounds and the outer range of islands. The reason for the existence of only fringing reefs for much of the southeast side has not been particularly investigated.

16. Local elevations within the sinking area are not evidence against the Darwinian theory of subsidence. Local disturbances and faults, as both theory and the rocks of the continents show, are almost necessary concomitants of a slowly progressing change of level. Besides this, igneous conditions beneath a region are a common source of local displacements. Such displacements are therefore to be looked for in the tropical oceans, since the various high islands are volcanic, and the coral islands probably have a volcanic basement; and, moreover, the islands are not unfrequently shaken by earthquakes. The causes of

* A map of the Feejee group is contained in my Corals and Coral Islands, and of larger size in Wilkes's Narrative of the Expedition.

local displacements by either method would not necessarily interfere with any secular change of level in progress.

17. The shore-platform of an atoll, or the "flat" as called by Darwin, situated just above low-tide level, consists usually of the true reef rock, or the rock made by under-water consolidation; and its height is determined chiefly by the height of wave action, its general surface being produced by the chiseling effect of the inflowing waters. When found above its normal level it is probable evidence of an elevation; and on this kind of evidence the conclusion rests in several of the cases of supposed elevation which I mention in my Report. The width of the platform and its evenness of surface vary with the height of the tides. When the tides are five to six feet, the platform is narrow, more cut up by channels and less even in surface. After an elevation, if but a foot or two in amount, the surface of the platform becomes restored finally for a large part of its surface to its normal level and gentle slopes may connect the newer and older portions. But if the rise of an atoll is ten feet, great degradation takes place along the lifted edge of the reef, which may end in reducing the elevated coral barrier to a wall with numerous channels and broad spaces opening through to the lagoon, as observed by the writer (from ship-board) on the south side of Dean's Island.*

18. The differences in the *kinds of coral rocks* should be understood (as the recent discussions of Darwin's theory have shown) in order to appreciate the structural facts that bear on changes of level. The *beach-made* rock is of *above-water* consolidation, (through calcareous deposition about the grains as evaporation takes place), and is porous, often oölitic; and if a conglomerate it consists mostly of worn masses. The rock made of drifted sands is similar. But the true *coral-reef* rock is of *under-water* consolidation, and is usually very compact, like an ordinary limestone; and if a conglomerate it is commonly a

* Our cruise took us from the Paumotu atolls to Australia, and there, the sandstone bluffs making the capes of Port Jackson gave me my first understanding of the atoll's "shore platform." This bluff had its "shore platform," 50 to 150 yards wide, bare at low tide; it was the lower layer of the sandstone, a regularly jointed rock, lying like a loosely laid pavement. It seemed strange that it was able to keep its place in the face of the breakers. But the first waters of the incoming tide swelled quietly over it and served to shield it from the plunging waters of the later part of the flow; the waves, therefore, found nothing to batter against short of the base of the bluff.

A view of Dean's Island from the south is given in Wilkes's Narrative, i, 342; it fails only in not giving a nearly even top line to the columns. The view on p. 334 looks as if representing another example of similar erosion. But, as the text implies, the group of masses of coral rock was made by the artist by bringing into a single view the blocks that had been observed in an isolated way over the platforms of atolls. The size and shapes of the blocks are exaggerated. But, although isolated, such blocks are often so united to the coral platform that they appear to be a constituent part of it (my Report, p. 61), and suggest the question whether they may not be remnants of an overlying layer elsewhere removed.

breccia, and sometimes a very coarse breccia. Some masses of it lying on the shore-platform of Paumotu atolls (thrown up by storm or earthquake waves) 100 to 2000 cubic feet in contents, consisted of single pieces of massive corals—*Astræas, Porites*, etc.; and others were an agglomeration of fragments of corals. The fine-grained or impalpable kind made from coral mud may have few or no fossils, and be a magnesian limestone.

Another variety of the coral-reef rock, made in lagoons and sheltered channels, has the corals in the position of growth, and when formed of branching corals the spaces among the branches are often but partly filled. It is a weak rock; and the islets thus made in lagoons and inner channels are sometimes overturned by the heaviest of waves; and rising banks (as the experience of the Wilkes Expedition proves) may be crushed beneath the keel of a passing vessel.

Owing to the different modes of origin of the beach-made rock and the truc coral-reef rock, the occurrence of the former underneath the latter would be evidence of subsidence.

Deep borings in atolls with circular drills that would give a six-inch core would supply evidence as to the existence or not of beach-made coral rocks at levels below the surface. They would also determine the depth to which true modern coral-reef rock extends and the nature of the underlying beds, whether calcareous, volcanic, or of any other kind; this is hence a sure method for obtaining a final decision of the coral island question and should be tried.*

19. *Elevated* coral reefs afford an opportunity to search for

* The Wilkes Expedition carried out apparatus for boring. It was put into inexperienced hands, as Commodore Wilkes states in his Narrative (iv, 267, 268), and at a trial with it on Aratica (Carlshoff Island) in the Paumotus, it became broken and useless at a depth of 21 feet. Moreover, the granulated material brought up afforded no satisfactory evidence as to the kind of coral rock encountered. The statement in the Narrative that "the low coral islands, as far as they have been investigated, both by boring and sounding, have shown a foundation of sand, or what becomes so on being broken up," has been quoted and made more of than the facts warrant. The "soundings" reached only the sands of the sea-bottom; and the "boring," if it found sand at bottom, proved only that the beach-made rock may exist at the 21-foot level, in which case a small subsidence would be indicated.

Commodore Wilkes says on page 269 of the same volume: "The elevated coral islands which we have examined exhibit a formation of conglomerate composed of compact coral and dead shells, interspersed with various kinds of corals, which have evidently *been deposited after life has become extinct*. A particular instance of this was seen at the island of Metia, and the same formation was also observed at Oahu." As the corals of a conglomerate, whether consisting of rounded masses or angular, are "deposited after life has become extinct," no inference as to the particular kind of coral rock intended can be drawn from the remark. From my knowledge of the island I presume he meant the ordinary breccia conglomerate of the reef rock, which is one of the kinds of coral rock of the elevated island. Commodore Wilkes himself made no examination of the rock or special study of coral islands, as might be inferred from his theoretical views on p. 270 of volume iv. His Narrative was to a considerable extent made up from the Journals of his various officers.

layers of beach-made rock underlying true reef rock; and also, if over 120 feet in height, to ascertain directly the character of the rock below this level.

The elevated atoll, Metia, seventy-five miles northeast of Tahiti, whose maximum height (according to the measurement of officers of the Wilkes Expedition) is 250 feet, I have described as consisting of the true coral-reef rock. My examinations were made on the west side where it presents a nearly vertical front to the water. The white compact limestone was, in some parts, almost destitute of fossils, or had only an occasional mould of a shell or fragment of coral;* and in others, it was a fine or coarse coral breccia. My notes written out at the island include the statement that "large masses of corals make some lower layers." This observation, though not as complete as I now see that it should have been, favors the conclusion that the thickness of the reef rock is at least twice as great as the depth to which reef-corals grow, in which case the elevated reef is proof of a subsidence of 120 feet or more.

The island is so near the route to Tahiti that the doubt which remains could be readily removed.

20. The subsidence indicated, according to the Darwinian theory, by atolls and barrier reefs was *actual*, not apparent subsidence attributable to change of water-level. The difference in its amount between the Central-Pacific area of subsidence and its limits (§§ 10, 11, above), the gradation or variation in amount of subsidence along chains of islands (§§ 10, 12, 13), and the local character of elevations, like those of Metia, Mangaia and many others, are proofs on this point.

The preceding explanations have prepared the way for the consideration of the arguments urged against the Darwinian theory, to which I now pass.

[To be continued.]

ART. XIII.—*On the Meteorite of Fomatlán, Jalisco, Mexico;* by CHARLES UPHAM SHEPARD.

FOR my knowledge of the Formatlán meteorite I am indebted to Mr. Carlos F. De Landero, Engineer, of Guadalajara, Mexico. Along with a fragment weighing 142 grams came his letter of March 11th, stating that it formed portion of a

* It was this compact rock, white, flint-like in fracture, clinking under the hammer, that was found on analysis by B. Silliman to contain 38·07 per cent of magnesium carbonate.

stone that fell "about the end of the year 1883 on the farm of El Garganitello, near the coast in the State of Jalisco."

The fragment is somewhat prismatic in shape, two and a half inches long and one and a half in each of its two other dimensions. The broadest side retains the original black crust, which is rough and dull, with a thickness rather above the average in meteoric stones. It is broken with medium facility, showing a rather lighter color than common in these bodies. It abounds in pisiform grains of a pearl-gray or brownish color, varying in size from a pea downward to that of mustard-seed. The larger of these may be described as not strictly pisiform, but rather flattened globules or imperfect crystals, with rounded edges and angles. They often exhibit a single tolerably distinct cleavage in one direction, with traces of another perpendicular thereto.

The basis in which the globules are imbedded is rather peculiar. It is many shades lighter in color than the globules, and is fine granular resembling certain massive albites. Under the lens it appears an intimate mixture of the broken gray globules and a white mineral. This last in the field of the compound microscope is seen to consist of sharply crystalline, transparent or semi-transparent grains, and closely resembles the chladnite of the Bishopville meteorite. It should be mentioned that the pisiform globules situated within half an inch of the crust are much stained with iron-rust.

But the striking peculiarity of the Jalisco stone is the prevalence everywhere of octahedral crystals of nickeliferous iron. These are so distinct as to be recognizable with the naked eye, the brilliant equilateral triangular faces coming into view by every change of position in the specimen. Now and then a surface presents a pitted or dissected appearance, similar to what is common in quartz crystals. One or two instances were noted where a tendency to the arborescent structure showed itself, the octahedra being aggregated in a common line, and only touching by the tips of their pyramids. Neither the irregular globular form or the twisted pisiform shape of this substance, sometimes visible in meteoric stones, is recognizable in the present case.

This observation at first led me to suppose that the Jalisco stone offered the first instance of well defined crystals of nickeliferous iron. But on recurring to the stones of several other localities, I find their presence is by no means rare, though they have not hitherto attracted attention. The following of those showing occasional crystals may be instanced: Rochester, Sumner County, Little Piney, Richmond, Yorkshire, Montrejean, Daniel's Kuill, New Concord, Vouillé, Erxleben and Affianello.

The crystals are uniformly distributed, penetrating even to the center of the chrysolite globules, and are often so minute as to be invisible without the aid of a glass. To effect their entire separation by the magnet is wholly impossible. Neither can their estimation be accomplished by the aid of acids, since the chrysolite is more or less decomposed by the same action that dissolves the crystals. The nearest approximation to their percentage, as determined mechanically, gave it at about 7 p. c., though this is probably too high, through the adhesion of the pulverized chrysolite in the process of separation.

Particles of crystalline troilite of considerable size adhere occasionally to the nickeliferous iron, though on an average they cannot exceed 0·5 p. c. of the stone. The treatment of the metallic portion of the stone in aqua regia left undissolved a few very minute black shining scales of a plumbaginous nature, together with equally minute non-magnetic, dull, octahedral crystals of chromite, which gave with borax the characteristic chromium reaction.

Equally difficult, as in the case of the nickeliferous iron, is the determination of the proportions of the chrysolite and the supposed chladnite. The nearest estimate I can make would be eight of the former to one of the latter, thus presenting the following approximative table for the mineralogical constitution of the meteorite:

Chrysolite	80
Chladnite?	10
Nickeliferous iron	7
Troilite, Chromite, Peroxide of iron	3
	100

The specific gravity as determined upon two fragments, each having about one-third of its surface covered by crust, was 3·47–3·48.

In conclusion, it may be observed that the shape of the specimen indicated it to have belonged to a stone several inches in diameter. Additional particulars relating to the fall will probably be furnished hereafter, through inquiries promised by Mr. Landero.

Charleston, April 18, 1885.

Addendum.

The delay which has occurred in the publication of the foregoing enables me to append thereto the very interesting extract from a letter of Mr. De Landero, dated Guadalajara, May 30th, 1885:

"Respecting the exact date of the fall, I shall sooner or later be able to fix it with precision. The aerolite passed over the town of Fomatlán (40 miles S.S.E. of Cape Corrientes, a village of 800 inhabitants, belonging to the canton of Maseota in our state of Jalisco, at a height of some three to five thousand feet, between four and five P. M. It was a very clear day. Many persons saw the aerolite, and heard the explosion it made, which was very powerful. Its direction was from S.E. to N.W. It left a white cloud in its track. Two or three fragments fell, eight miles N.W. of Fomatlán, between the houses of the Gargantillo farm. The latitude of Fomatlán is 19° 44′ N. and its longitude near 6° 20′ W. of the city of Mexico. Its elevation above the sea is about 100 feet.

"The administrator of the Gargantillo farm, Cesareo Rodriguez, gathered two or three fragments of the meteorite a few minutes after their fall, when they were still at a burning heat. The largest of these weighed about two pounds. The main body of the meteorite, which must have been very large, continued on its path to the N.W. and fell into a large lagoon four or five miles distant from the farm.

"My uncle, Mr. Joaquin Castanos, who was at that time in Fomatlán, received one of the fragments from the hands of Cesareo Rodriguez, and kept it for me. I made a determination of the specific gravity of the meteorite upon a fragment weighing 28½ grams, the result of which was 3·49."

A late letter gives August, 1879, as the time of fall.

ART. XIV.—*On the widespread occurrence of allanite as an accessory constituent of many rocks*; by JOSEPH P. IDDINGS and WHITMAN CROSS, of the U. S. Geological Survey.

MORE than a year ago, while engaged in the microscopical study of rocks from widely separated regions, the writers observed in the thin sections occasional crystals or grains of a highly refracting, brown mineral, which was not referable to any of the more common rock-building species. The study of a number of sections showed that the crystals were monoclinic and apparently isomorphous with epidote, that is, elongated in the direction of the ortho-axis with the base and orthopinacoid well developed, two positive orthodomes less prominently, and with terminal planes belonging apparently to the unit prism. In a few instances imperfect cleavage was noticed parallel to *i-ī* and *O*, and in one case to *i-ì*. A twinning parallel to *i-ī*, as in epidote, was frequently observed.

The optical axes were found to lie in the plane of symmetry, one of the bisectrices making an angle of 35° to 40° with the vertical axis and 25° to 30° with the clino-axis. A strong pleochroism, from light yellowish brown to dark chestnut brown

ERRATUM.

Page 108, 24th line from top, for August, read September 17th, at 5 P. M.

was always noted, as also a high index of refraction. These characters strongly indicated the identity of this mineral with allanite, and a qualitative chemical analysis, made by Dr. W. F. Hillebrand in the Denver laboratory of the U. S. Geological Survey, fully confirmed the correctness of the microscopical determination. The material analyzed was derived from a biotite porphyrite of the Ten Mile District, Colorado, in which the mineral occurred in exceptional abundance. The process of separation and analysis is described by Dr. Hillebrand as follows: "The mineral supposed to be allanite was separated from the rock, together with much magnetite and some zircon, by means of the Sonstadt solution. The magnetite being removed by a magnet, the other minerals were left quite free from further admixture. They formed approximately 0·05 per cent of the whole rock.

"In order to preserve the zircon crystals intact, the mixture was treated, without pulverization, for many days in a platinum crucible with hydrochloric acid. The residue was zircon and silica retaining the original form of the allanite fragments. This silica after filtration was entirely soluble in hot sodium carbonate. After separating silica in the hydrochloric acid solution, the latter was boiled with potassium hydrate. The filtrate then showed much alumina and lime, and very little magnesia. The precipitate by potassium hydrate was dissolved in hydrochloric acid, diluted, and to it a concentrated solution of oxalic acid added. The precipitate, at first curdy, was ignited, dissolved in sulphuric acid, evaporated, and ignited gently, the sulphates dissolved in cold water, and crystals of potassium sulphate added. After washing the insoluble double sulphate formed with a potassium sulphate solution, ammonia formed no precipitate in the filtrate, showing the absence of erbium and yttrium. The double sulphates dissolved very readily in water acidulated with hydrochloric acid, and were then decomposed by oxalic acid; the ignited precipitate was brown, readily soluble in hydrochloric acid with evolution of chlorine, indicating thereby the presence of both cerium and lanthanum, and absence of thorium. No didymium absorption lines were visible in the spectroscope. The oxalates, when dissolved in strong nitric acid and boiled with lead dioxide, gave a yellow solution showing the presence of cerium. Further tests were rendered impossible by the loss of the material.

"Although in the beginning only a qualitative analysis was contemplated, owing to the small amount of material, nevertheless the silica, alumina, ferric oxide, mixed cerium and lanthanum oxides, and the lime were weighed, and found to bear to one another the avarage ratio of those constituents in allanite."

Considering the identity of the mineral as established, the following additional particulars as to its characteristics and distribution may be of interest.

Crystals which are large enough to be seen without the aid of a lens are easily recognized in the hand specimen by their brilliant black color, uneven fracture and oily luster. They vary greatly in form and size in the different rocks examined, in some cases being long, slender prisms, reaching a maximum length of 1 cm., though usually appearing in short, stout prisms, or in quite irregular grains of microscopic dimensions. In thin sections from a great number of localities the color is uniformly a rich chestnut brown with the strong pleochroism already mentioned, but in a few instances a zonal variation has been noticed, the color growing lighter from the center of the crystal outward. Cleavage is in most cases wholly absent.

A confusion with hornblende or biotite is only possible when these minerals are of nearly the same shade of brown, and are so cut by the section as to show no distinct cleavage. But the lack of pleochroism and the nearly uniaxial character of a basal section of biotite—the only one not exhibiting cleavage—sufficiently distinguishes it from the strongly pleochroic, biaxial allanite. In the exceedingly rare cases where hornblende possesses the peculiar chestnut-brown color, and shows neither characteristic cleavage nor outline, one can only distinguish the allanite by its higher index of refraction, which produces more brilliant interference colors between crossed nicols.

From its mode of occurrence and association allanite must be added to the group of primary, accessory rock constituents, similar to zircon, sphene, and apatite, though much rarer than any of these. In one instance it was noticed enclosing zircon, in others, sphene, apatite, and magnetite, but it has also been found in such connection with these minerals as to indicate a contemporaneous growth. Its nature as a primary constituent in eruptive rocks is further attested by its occasional inclusion in biotite, feldspar, and quartz.

In some regions it appears to be quite uniformly distributed through certain types of rock. Thus, in the porphyrites and allied porphyries of the Ten Mile district, Colorado, prismatic crystals of allanite may be seen in numbers upon nearly every hand specimen—one exhibiting as many as forty on its surface—and few thin sections of these rocks were examined without one or more being discovered. On the other hand, in the vicinity of Eureka, Nevada, it seems to be very irregularly disseminated through a considerable range of rock types, two or three microscopical individuals being recognized in quite a number of thin sections—and in one instance as many as eleven—but in the majority of sections it is absent altogether. In most of the rocks studied the mineral occurs very sparingly.

In almost every instance the allanite is perfectly fresh and unaltered. Macroscopic crystals are occasionally seen to be reddish brown in color, with a more or less deeply stained zone surrounding them. In one partially decomposed rock, a transition of allanite to epidote is indicated by crystals of the former enclosing scattered grains of epidote in such a manner as to leave little doubt that they result from an alteration of the enclosing mineral.

It is interesting finally to notice how extensively allanite is disseminated in minute quantities through rocks of a great variety of types and from widely separated localities. Without undertaking any special investigation of the matter we have found it microscopically in metamorphic rocks, in older crystalline eruptive masses and in glassy lavas; in hornblende gneiss and mica gneiss, in granite, granite porphyry, quartz porphyry, diorite, porphyrite, andesite (glassy), dacite, and rhyolite; and from localities in the States of Maine, Massachusetts, Rhode Island, Colorado and Nevada, and the Territories of Wyoming and Utah. Below is appended a list of the rocks and localities alluded to, which will no doubt be greatly enlarged by other observers.

Gneiss.—Utah: Ogden Cañon; Farmington Cañon, Wahsatch.
Wyoming: Medicine Bow Range.
Nevada: Clover Cañon, E. Humboldt Mountains.

Granite.—Maine: Vinalhaven, Biddeford, Wayne, Fox Island, Harrington.
Massachusetts: Lynnfield.
Rhode Island: Westerly.
Utah: Little Cottonwood Cañon.
Nevada: Eureka.

Granite porphyry.—Utah: Tooele.
Nevada: Eureka.

Quartz porphyry.—Colorado: Mosquito Range, Park County; Eagle River, Eagle County.

Diorite.—Nevada: Truckee Cañon.

Porphyrite.—Colorado: Mount Silverheels, Park County; Mosquito gulch, Park County; Ten Mile District, Summit County.
Utah: Henry Mountains.

Andesite (glassy).—Nevada: Eureka.

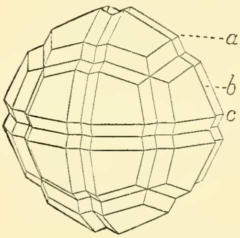
Dacite.—Nevada: Eureka, Shoshone Park; Washoe.
Utah: White Rock Mountain.

Rhyolite.—Nevada: Eureka.

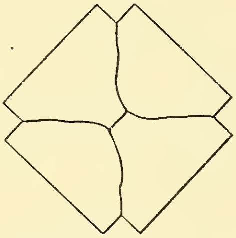
ART. XV.—*Crystals of Analcite from the Phœnix Mine, Lake Superior Copper Region*; by SAMUEL L. PENFIELD.

THESE crystals which have lately been received at New Haven, occur thickly grouped together on calcite and native copper, associated with tabular crystals of apophyllite, and numerous minute crystals of milky quartz scattered thickly over the specimens. The crystals are of all sizes from very minute up to one centimeter in diameter; the small ones are simply tetragonal-trisoctahedrons of the form (211), 2–2, the larger ones are of the same form but with the planes arranged as in fig. 1. Along every edge of what might be regarded as a simple crystal, there is a reëntrant angle or groove. The crystals are mostly of ideal symmetry of arrangement, and appear as if made up of twenty-four simple crystals, each equally spaced from the center. Occasionally at the extremity of the principal axes, a fifth solid angle can be seen symmetrically situated midway between the four represented in the figure. The planes of the crystals are uneven, as is so often the case with analcite, so that it was impossible to prove by means of the reflection goniometer that the individual parts of the crystal were arranged in exactly parallel position.

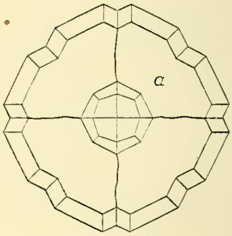
1.



2.



3.



In order to obtain some insight into the interior arrangement of the crystals, some sections were cut and examined with the microscope. A cross section parallel to (001), *i-i*, at the point *a*, fig. 1, is given in fig. 2. The section is plainly divided into four parts corresponding to the four crystals, so to speak, which are intersected at that point, the division lines running nearly parallel to the diagonals of the rectangle. The figure shows the irregular direction of these dividing lines in one of the sections. at times they run much more regularly. They were observed in all cases, and can be seen with the naked eye as soon as the top of the crystal is cut away. After grinding the sections thin it is impossible to transfer the same to an object-glass without the four parts separating along these division lines, and this is the case even when the sections are quite thick for microscopic purposes, so that in reality there are four separate individuals cut through at that point. Under the microscope the sections show a slight double refraction, but no division into definite fields, as observed by A. Arzruni and S. Koch,* could be observed. The patches of dark and light were very unequally distributed, but the extinction was in all cases at a maximum when the principal isometric axes were parallel to the planes of the polarizer or analyzer, as observed by the above mentioned authors. A section nearer the center of the crystals showed the four lines of separation as above, but no separation parallel to the dodecahedral symmetry planes, as might be expected below the point *b*, could be observed. In addition, a rectangular patch surrounded by an opaque rim was observed in the centre of the section, being the section through a central crystal or nucleus. A thick cross section between the points *b* and *c*, fig. 1, is given in fig. 3. The outer portion *a* represents the clear transparent material of the crystal with the four division lines, while in the interior a perfect tetragonal-trisoctahedron can be seen. The material of this inner crystal is transparent, but it has an opaque, white coating, and the whole outline of the crystal can be seen as distinctly as if it were a model mounted in some transparent material. After observing this in the section, it can readily be seen that all of the larger crystals have an opaque center. The transparent exterior portion shows with crossed nicols under the microscope colored patches running in radial directions from the center, the maximum of extinction being for all of the four secants when the principal axes are parallel to the polarizer or analyzer.

The outer portion would thus seem to be a secondary growth about an older analcite crystal serving as a nucleus. Each face of the original crystal seems to have acted separately in the orientation of the secondary deposit. The three crystals or parts of the crystal in each octant seems to be parallel in molecular arrangement because no lines of demarcation or separation between them can be detected, while the material of the eight octants seems to be not quite parallel in arrangement, as indicated by the division lines and easy separation which were in all cases observed.

Mineralogical Laboratory, Sheffield Scientific School, May 12th, 1885.

* Zeitschr. Kryst., v, 483.

Art. XVI.—*On a Differential Resistance Thermometer;* by T. C. Mendenhall.

The determination or registration of the temperature at a distant or not easily accessible point, is so extremely desirable that many methods for accomplishing this end have been proposed, and to some extent made use of during the past fifty years. Naturally enough electricity has been utilized in some way or other in the majority of these systems of telethermometry.

The requirements of the problem seem to be that the device or instrument used at the point, the temperature of which is to be ascertained, shall be of the greatest possible simplicity of construction involving little or no motion in its parts, so that the liability to "get out of order" shall be reduced to the minimum; and that at the observing or registering station, the necessary appliances shall possess a maximum of durability and simplicity—so that a minimum of time and skill will be demanded in making the observations. The whole system must be certain in its indications and correct within a reasonable limit.

The first of these conditions is apparently sufficiently well satisfied by the thermo-electric-junction which has probably been more extensively made use of than any other form of electric thermometer. It renders necessary, however, the use of a comparatively delicate galvanometer, and as the electromotive force of a single couple is small (it is difficult to use more than one in general practice) the results are subject to considerable errors arising from unknown or neglected sources of electromotive force. This source of error becomes more important as the range of temperature measured becomes smaller, although it may be almost entirely avoided by care and skill on the part of the operator. The well known resistance method of Siemens satisfies the same condition very perfectly, and is certainly capable of giving good results when skillfully applied, at least throughout moderate ranges.

The desire to possess some form of electric-thermometer which might be utilized in the study of certain problems connected with meteorology, especially the observation of soil and earth temperature, and the use of which would not demand greater skill than that of the ordinary meteorological observer, led to the device and construction of the instrument to be described which may be called a "differential resistance thermometer." It consists essentially of a mercurial thermometer, not unlike ordinary forms, except that the bulb is greatly enlarged so that the stem may have a diameter of something like a millimeter, and still leave the scale tolerably "open." In one of the instruments already made 1° C. corresponds to about 5^{mm} of the scale. Running down through the stem is a fine platinum wire about $\cdot 08^{mm}$ in diameter. The lower end may be secured in the bulb so that it is kept straight in the bore of the stem, and at the lower end a heavier wire is sealed in the glass so that metallic contact can be made with this wire both at the upper end and through the mercury at the lower. It is evident that the resistance between these two points will depend largely (but not entirely) on the length of the platinum wire which is above the mercury in the tube and this will depend on the temperature to which it is exposed. When this temperature rises the resistance is decreased by an amount equal to the difference between that of the platinum wire which disappears and that of the mercury which takes its place—less the increase in the resistance of the wire and mercury due to increase of temperature.

Let $l =$ length of platinum wire exposed at 0°.
$s =$ resistance per unit length (= length of 1°) of wire.
$g =$ resistance per unit length (= length of 1°) of mercury.
$k =$ temperature coefficient of platinum.
$h =$ temperature coefficient of mercury.
$R_0 =$ total resistance at 0°.
$R_t =$ resistance (including all) at $t°$.

Then

$$R_t = R_0 - \{s(1-kl) - g\}t - (ks - gh)t^2. \qquad (1)$$

This equation is not quite rigorous, but the approximation is very close. It is of the form—

$$R_t = R_0 - Bt - Ct^2. \qquad (2)$$

The simplest and best way of dealing with it is to determine the constants of the equation (2) by a series of observations making use of the method of least squares. From the result the resistance for any degree of temperature may be calculated, or better, a curve can be constructed from which the temperature corresponding to any resistance can easily be read.

The advantage of this method over the use of a simple resistance coil is that the change in resistance accompanying a given change in temperature, is much greater and in fact it may be made as great as one desires. As a result the telephone may be substituted for the galvanometer in the resistance measurements, thus greatly simplifying the apparatus as well as increasing the rapidity with which observations may be made; or if preferred, a much less sensitive galvanometer may be used. It also possesses the very great advantage of allowing an increase of delicacy as the range of temperature decreases. For earth temperatures this is very desirable and it will easily be seen that thermometers of this kind can be constructed for a few degrees of range with which, by comparatively rude processes the measurements may be correct within a very small fraction of a degree. For use in earth temperature measurements, the thermometer will be inclosed in a strong brass tube for protection and the connection with the point of observation made by means of a cable of heavy copper wire. The cable will of course form a part of R_0 in equation (2), but as it is a constant, the substitution of one cable for another, if necessary, will affect the position and not the form of the calibration curve. Its resistance must be small, relatively, and the influence of temperature upon it may be neglected.

With this device a temperature observation may be taken in less than a minute, no time being consumed in the preparation of liquids of known temperature at the observing station as in the use of the thermo-junction or the resistance coil.

Art. XVII.—*Impact Friction and Faulting*; by George F. Becker.

Bearing of the subject.—Some time since I submitted certain phenomena of faulting to analysis and endeavored to show how it might happen that a movement in the earth's crust, instead of being confined to a single surface, would be distributed over a great number of substantially parallel surfaces.* That such occurrences are frequent is certain. They are called step faults by Mr. Geikie† and are stated to be common in the coal fields bordering on the Forth. Professor Heim has also called attention to similar instances.‡ The parallel systems of veins found in a large proportion of mining districts, and even some forms of complex lodes, involve such a division of the country. Landslides frequently show a separation of the moving soil into extraordinarily regular sheets a foot or two in thickness, indeed during the spring of the past year I examined scores of such slides in the Coast Ranges of California, some of them covering forty or fifty acres in extent. Finally some monoclinal faults may probably be considered as step faults where the intervening masses are not too broad to permit of their displaying independent rigidity and, for these instances at least, the relations between monoclinal and anticlinal faults may be elucidated. It is clear that the subject is one of more than merely local importance and deserving of the attention both of

* The Geology of the Comstock Lode (Monograph U. S. Geol. Survey, vol. iii), p. 156.

† Text-book of Geology, p. 532.

‡ Mechanismus der Gebirgsbildung.

those interested from a scientific point of view in structural geology, and of those who desire to take advantage of such assistance as theory affords for economical purposes.

Plan of discussion.—While the extremely simple argument as to the distribution of energy in a system of material sheets in contact with one another and of the resulting geometrical effects, which was offered in the paper on faulting referred to, seems to me rigidly correct, the fact that friction plays a leading part in the problem lends it a somewhat unfamiliar character. It appears desirable, therefore, to subject friction itself to a closer examination and to show that a study of the character of this force leads to results embracing those formerly reached. Much of the material which will be presented in the following pages was prepared for my former discussion, but was omitted as not sufficiently germane to the subject of the report in which it was included. I shall first make an attempt to elucidate the distribution of energy in a rod, or any other system in which the centers of inertia of the members are arranged in the line of force, when subjected to an impact; next, to show that the results are immediately applicable to frictional problems, and then that the same results may be reached independently of any hypothesis as to the nature of a frictional surface. Finally, these results will be applied to a characterization of friction and their application to problems of structural geology will be indicated.

System of inelastic balls.—Suppose a series of inelastic bodies of equal mass, arranged in a straight line, at rest and unconstrained. If the first of these masses is started at a velocity v in the direction of its next neighbor, it will strike it, a loss of energy will ensue, the two will move off together and impinge upon the third, and so on. The loss of energy at each impact can be extremely easily calculated from the principle of the stability of the center of inertia of a system upon which no external forces act. If M is the mass of the moving body, the loss of energy when the first x bodies having coalesced and moving as one mass strike the $(x+1)$ body is say*

$$W=\frac{Mv^2}{2}\,\frac{1}{x(x+1)},$$

the equation of a locus belonging to the class of hyperbolic

* The momentum of the moving mass is constant. If therefore v_x is the velocity with which x masses move after they have coalesced

$$Mxv_x=Mv,$$

and therefore

$$v=xv_x=(x+n)v_{x+n}.$$

The kinetic energy of the moving mass before the x bodies strike the next is $\frac{Mv^2}{2x}$, and after they strike it $\frac{Mv^2}{2(x+1)}$, and the difference of these quantities is W, the energy expended.

curves. It is clear that W does not become zero until $x=\infty$, and consequently the energy is distributed over an infinite series of masses. The equation accounts for the entire energy and no more for

$$\sum_{1}^{\infty}\frac{1}{x(x+1)}=1.$$

This equation does not accurately represent the distribution of energy in the system ; for when x bodies strike the next in the series, although most of the energy will be expended at the contact, a portion will be propagated backward, and work will be done at previous contacts as well. On the other hand, it demonstrates that the distribution of energy in such a system may be discussed entirely apart from the effects which the energy exerted at any point produces upon the material of the system. If the masses of the members of the system remain constant, their density may vary regularly or irregularly without affecting the above equation. If two or more of the equal members of the system were joined together before impact, this would not in any way affect the distribution of energy, so that the equation holds good for bodies of which the mass varies regularly or irregularly, provided that x varies as the mass.

Distribution of energy in finite masses.—The transmission of energy in solids or liquids is not instantaneous. It follows that if two elastic bodies of the same material, but of unequal length, meet one another, a portion of the energy is converted into vibration. This is not the case with inelastic bodies, which remain permanently in the condition of maximum deformation. The transmission of energy however is extremely rapid, and Messrs. Thomson and Tait estimate that the entire impact of two balls a yard in diameter of copper, glass or steel, occupies a period within the thousandth part of a second. I shall consider the transmission instantaneous. On this supposition the centers of inertia of equal masses of a form varying from that of a cylinder will behave like the centers of inertia of such cylinders, and the distribution of energy among the centers of inertia in a rectilinear series of balls (for example) become reducible to that in a cylinder of invariable cross section but compressible in the direction of its axis. The distribution of energy in a perfectly elastic mass of constant temperature at the moment of maximum compression is also that which would be produced by the action of a constant force of appropriate intensity on the same body. It represents too the permanent effect of an impact upon a perfectly inelastic mass, so that the solution of one of these cases is the solution of all three.

From the instant at which an impinging mass first comes in contact with a passive mass, to the moment at which the centers of inertia come to rest relatively to the center of inertia of the system, the active and reactionary forces are in equilibrium. This is manifestly the case for elastic bodies, and since during this period elastic and inelastic bodies behave exactly alike, inelastic bodies are reducible to a conservative system for the same period. The principle of virtual velocities is therefore continuously applicable and the energy potentialized (or expended) in the two masses is equal in absolute value and opposite in direction. The kinetic energy at the moment of maximum compression on the other hand will be uniformly distributed (relatively to the mass) over the entire system.

Solution for finite compressible rod.—The geometrical methods of representing energy are as various as the corresponding algebraic notations, but perhaps the most natural is that in which the energy of a moving body is made proportional to the volume of the body and to its energy per unit of volume. If the energy potentialized throughout a given volume were uniformly distributed, the quantity of energy potentialized in a cubic unit would then be, say w, and that in an infinitesimal cube would be $w dx\, dy\, dz$. Suppose a finite compressible cylinder at rest to suffer impact from a second similar cylinder moving with a velocity v. It will be convenient to consider the mass of the moving cylinder as 2M and that of the passive cylinder as $2n$M. The entire energy potentialized in the passive mass at the moment of maximum compression will then be $\frac{Mv^2}{2}\frac{n}{n+1}$, and the kinetic energy of the whole system at the same instant will be $\frac{2Mv^2}{2}\frac{1}{n+1}$. The problem proposed is to find an expression for the distribution of potentialized energy throughout the passive mass, or to state w in terms of x, for the moment of maximum compression.

Between any two successive sections w will be diminished by two quantities, one representing the kinetic energy imparted to the mass between these sections, and the other the energy potentialized. Indeed these quantities may be thought of separately as if a certain amount of kinetic energy were first distributed uniformly over the passive mass and afterward a certain quantity of internal work were done in it. If the length of the entire system is unity, the diminution of w betwen x and $x+dx$ due to the uniform distribution of kinetic energy will be $\frac{2Mv^2}{2}\frac{dx}{n+1}$. The energy potentialized between these limits is of course $w dx$, but this quantity does not bear a simple relation to dw unless none of the energy assumes the kinetic form, or in other words, unless the passive mass is infinite. Were this the case w would be diminished by and could be diminished only by wdx; and $dw = wdx$, the differential equation of the simple logarithmic curve. But when a portion of the energy imparted to the section at x reappears as kinetic energy, dw must have a greater negative value. The equation might be written $dw = -wdx - Qdx$, but here Q must be some function of w. Now it is an elementary condition of equilibrium in the case under discussion that for a given displacement at any section, say the one at x, the energy potentialized between this section and the free end of the rod shall be a minimum, so that if a is the value of x for this end of the rod

$$\int_x^a wdx = \text{min.}$$

This minimum must have its maximum value when the entire energy is potentialized or when $a = \infty$. But if $dw = -wdx$ and a (or n) is infinite

$$\int_x^a wdx = -\frac{dw}{dx},$$

so that the infinite rod being merely an extreme case of a finite one, the integral for finite a must be less than $-dw/dx$ by a value which disappears when the kinetic energy of the system at the moment of maximum compression is zero. The effect of the uniform distribution of kinetic energy upon the value of dw has already been traced and the equation

$$\int_x^a wdx = -\frac{dw}{dx} - 2\frac{Mv^2}{2}\frac{1}{n+1},$$

therefore fully accounts for both the kinetic and the potentialized energy. For $x = a$, w must disappear, and this definite integral may therefore be written $C - \int wdx$. Introducing this value and differentiating

$$\frac{d^2w}{dx^2} = w.$$

If an arbitrary unit of measurement, c, is adopted in this equation and w and x are each divided by it,

$$c^2\frac{dw^2}{dx^2} = w,$$

which leads without difficulty to

$$w = A\varepsilon^{-x/c} + B\varepsilon^{+x/c},$$

where A and B are arbitrary constants to be determined both in sign and value by the conditions.

Determination of constants for general case.—For the free extremity of the passive mass $w = 0$ and therefore

$$B = -A\varepsilon^{-2a/c},$$

or in general,

$$W = A(\varepsilon^{-x/c} - \varepsilon^{-2a/c}\varepsilon^{x/c}).$$

Here $\varepsilon^{-2a/c}$ is independent of A, and the latter retains the same value even when $a = \infty$, or when the passive mass is infinite. In this case of course the entire energy of the system is potentialized (or expended if the material is inelastic). If m is the mass which the unit volume would possess were it compressed to the density which the impact produces at the face of the infinite rod and if c is so chosen that $cm = M$

$$\int_0^\infty w dx = Ac = \frac{mv^2 c}{2},$$

or

$$w = \frac{mv^2}{2}\varepsilon^{-x/c}.$$

It will be convenient to retain for c the signification which it assumes when the passive mass is infinite because of the simple relations which it then bears to the energy of the impinging body. The unit chosen in any case is of course entirely arbitrary, but the results are much simplified by establishing some rational relation between the units adopted for different cases. Let the product of the entire energy potentialized in any case into the corresponding unit be a constant; then if V is the energy potentialized in an infinite rod, and V_1 the energy potentialized in a finite rod, $Vc = V_1 c_1$, or

$$c_1 = c\frac{n+1}{n},$$

so that the equation for a finite rod may be written

$$w = A(\varepsilon^{-x/c_1} - \varepsilon^{-2a/c_1}\varepsilon^{x/c_1}).$$

Now for $n = \infty$, $c_1 = c$, and therefore the value of A already found is valid for the new equation. From

$$w = \frac{mv^2}{2}(\varepsilon^{-x/c_1} - \varepsilon^{-2a/c_1}\varepsilon^{x/c_1}).$$

it follows that

$$\int_0^a w dx = \frac{mv^2 c_1}{2}\left(1 - \varepsilon^{-a/c_1}\right)^2 = \frac{mv^2}{2}\,\frac{cn}{n+1}.$$

Reintroducing the value of c in terms of c_1, it will readily be seen that

$$\varepsilon^{a/c_1} = n + 1,$$

a value which can also be otherwise obtained. This also gives

$$c_1(1 - \varepsilon^{-a/c_1}) = c.$$

The equation of the distribution of energy in a finite rod, the contact being taken as origin, may therefore be written

$$w=\frac{mv^2}{2}\left(\varepsilon^{-x/c_1}-\frac{\varepsilon^{x/c_1}}{(n+1)^2}\right).$$

If the origin is transferred to the free extremity of the rod by substituting $x+a$ for x, this becomes

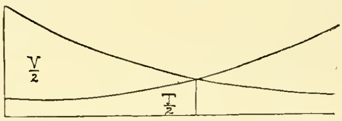
$$w=\frac{mv^2}{2}\,\frac{\varepsilon^{-x/c_1}-\varepsilon^{x/c_1}}{n+1}.$$

If $\frac{mv_2}{2}$ is taken as the unit of energy and c_1 as the unit distance,

$$w=\frac{\varepsilon^{-x}-\varepsilon^{x}}{n+1},$$

a form of very satisfactory simplicity. If $n=1$ $c_1=2c$, or the length which the entire impinging mass would assume were it uniformly compressed to the density at the contact when the passive mass is infinite.

In the following diagram the area marked $\frac{V}{2}$ is the energy potentialized in the passive mass, or half the entire energy potentialized in the system.



When the mass 2M strikes the mass $2n$M the kinetic energy of the entire system at the moment of maximum compression is, say

$$T=\frac{2Mv^2}{2}\,\frac{1}{n+1}=\frac{2mv^2}{2}c\varepsilon^{-a/c_1}=\frac{2mv^2}{2}c_1\varepsilon^{-a/c_1}\left(1-\varepsilon^{-a/c_1}\right).$$

and this is manifestly twice the integral of the area marked $\frac{T}{2}$ in the diagram. If T_1 is the kinetic energy of the passive mass alone at this moment, it is readily seen that

$$Tc_1=T_1c.$$

If V is the energy potentialized in the entire system and E the total energy, the fundamental energy equation

$$V=E-T,$$

becomes

$$2\int_{-a}^{0} w\,dx=\frac{mv^2}{n+1}\int_{-a}^{0}\varepsilon^{-x/c_1}dx-\frac{mv^2}{n+1}\int_{-a}^{0}\varepsilon^{x/c_1}dx.$$

$$=\frac{mv^2}{n+1}c_1\left(\varepsilon^{a/c_1}-1\right)-\frac{mv^2}{n+1}c_1\left(1-\varepsilon^{-a/c_1}\right)$$

$$=2\frac{Mv^2}{2}-2\frac{Mv^2}{2}\,\frac{1}{n+1}.$$

Case of the atmosphere.—A peculiar case arising under the general equation is that in which the passive body, though of finite mass is of infinite volume, and therefore, in its uncompressed state, of infinite tenuity. Here the value of a being infinite, the coefficient B disappears and the equation assumes the simple logarithmic form. The value of the energy at the contact, however, cannot be affected by the fact that there is no limit to the expansion of the material. A therefore assumes the value

$$A=\frac{mv^2}{2}\left(1-\frac{1}{(n+1)^2}\right)=\frac{m_2v^2}{2},$$

and the base of the equation diminishes in such proportion as to give the correct integral. If c_2 is the exponential constant for this case

$$\frac{m_2v^2}{2}c_2=\frac{mv^2}{2}\,\frac{cn}{n+1}$$

$$c_2=\frac{n+1}{n+2}c$$

$$w=\frac{m_2v^2}{2}\varepsilon^{-x/c_2}=\frac{mv^2}{2}\,\frac{n(n+2)}{(n+1)^2}\varepsilon^{\frac{-(n+2)x}{(n+1)c}}.$$

For a perfect gas under the compression produced by the impact of a body of its own weight $n=1$. The value m_2 is the actual mass of the unit volume at the contact of the masses, and c_2 is one-half the length which the volume of gas would have if uniformly compressed to m_2.

The average stress, due to the elasticity of a solid, when strained from its natural condition to that of a given strain, is just half the stress required to keep it in this state of strain.* If the energy potentialized in the passive mass by an impact at the moment of maximum compression is $\frac{Mv^2}{2}$ the effect of a constant force of corresponding intensity would be to potentialize an energy $\frac{Mv_1^2}{2}$ where $v_1=\frac{v}{2}$. If a column of uncompressed gas possessing the mass of a column of the atmosphere of the same section, were to strike the earth at a velocity g, the average potentialized would be $\frac{Mg^2}{2}$. This is four times of the energy actually potentialized in the atmospheric column. In general if the energy diagram for the impact of a mass 2M moving at a velocity v and impinging upon a mass $2n$M is reduced to half its dimensions, or if it is interpreted in terms of a unit twice that used in plotting it, the result is the distribution of energy

* Thomson & Tait, Nat. Phil., § 674.

due to the action of a constant force Mv on the mass $2nM$. If p is the pressure at any point and s the strain, we have in general

$$dw=pds.$$

If the stress and strain are in a constant ratio, say k, $ds=kdp$ and

$$w=\frac{kp^2}{2}.$$

If p_0 is the value of p for the contact plane of the masses, and w_0 the value of the energy for the same surface,

$$w:w_0=p^2:p_0^2.$$

For the atmosphere therefore

$$p=p_0\varepsilon^{-x/2c_2}$$

Here c_2, as already pointed out, is half the height which the atmosphere would have were it uniformly compressed to the density at the bottom of the column; $2c_2$ is therefore the familiar "height of the homogeneous atmosphere," and the equation is the barometric formula, introduced here merely as a check upon the reasoning.

Case of a rivet.—If the coefficient B is positive instead of negative the entire energy will be potentialized within finite limits. This is possible only when the passive mass is subjected to two impacts in opposite directions, or when it rests against an infinite mass which may be regarded as rigid. In this case one-half of the entire energy will be potentialized in the finite passive mass. The general equation shows that the energy is to be considered as imparted to the passive mass from opposite directions, and it is evident that the result is the same as it would be if the energy were first distributed over an infinite mass, and the energy potentialized beyond $x=a$ were then restored to the finite cylinder from the opposite direction. The equation may therefore at once be written for the contact as origin

$$w=\frac{mv^2}{2}\left(\varepsilon^{-x/c}+B\varepsilon^{x/c}\right).$$

This curve must be horizontal at some point, say $x=a$, and if dw/dx is made equal to zero $B=\varepsilon^{-2a/c}$. According to the preceding

$$\int_0^a \varepsilon^{-2a/c}\,\varepsilon^{x/c}dx=\int_a^\infty \varepsilon^{-x/c}dx.$$

which gives

$$\varepsilon^{2a/c}=\varepsilon^{a/c}(\varepsilon^{a/c}-1).$$

Now in a former paragraph it was shown that

$$\varepsilon^{a/c_1}=\varepsilon^{na/c(n+1)}=n+1.$$

and this gives an expression for $\varepsilon^{a/c}$ which makes

$$\varepsilon^{2a/c} = (n+1)^{\frac{n+1}{n}}\left((n+1)^{\frac{n+1}{n}} - 1\right).$$

If the origin is removed to the base of the ordinate of the horizontal point of the curve the equation may be written

$$w = \frac{mv^2}{2}\,\frac{\varepsilon^{-x/c} + \varepsilon^{x/c}}{\varepsilon^{a/c}} = \frac{mv^2}{2}\,\frac{\varepsilon^{-x/c} + \varepsilon^{x/c}}{\varepsilon^{a/2c}(\varepsilon^{a/c} - 1)^{\frac{1}{2}}}$$

$$= \frac{mv^2}{2}\,\frac{\varepsilon^{-x/c} + \varepsilon^{x/c}}{(n+1)^{\frac{n+1}{2n}}\left((n+1)^{\frac{n+1}{n}} - 1\right)^{\frac{1}{2}}}$$

the equation of a "projected catenary." If the strain is proportional to the stress, this corresponds to the form assumed by a cold rivet, and it is under this law that the head of a drill spreads in use. For $n=1$ the equation assumes the simple form

$$w = \frac{mv^2}{2}\,\frac{\varepsilon^{-x/c} + \varepsilon^{x/c}}{2\sqrt{3}}.$$

Application to incompressible masses.—The problem of the distribution of energy in a finite or infinite cylinder, compressible in the direction of its axis only and subjected to an impact or a constant force, thus appears capable of entirely satisfactory solution on the supposition that the transmission of energy is instantaneous. The conditions as to compressibility answer to those of a gas confined in a rigid cylinder and are not those of solids. But since solid masses act as though concentrated at their centers of inertia, the formulas deduced are applicable to the positions of the centers of inertia of incompressible elastic or inelastic bodies. They therefore also represent completely the distribution of energy in incompressible rods capable of lateral deformation for infinitely small strains produced by impact, and approximately for small but finite strains. For constant forces acting in parallel lines or from a center at an infinite distance, in short when the equipotential surfaces are planes, the equations appear to represent the distribution of energy even for finite strains.

The character of the divergence when the equipotentials are not planes, or for central forces, is best seen by taking the extreme case of an impact acting at a point in the center of a thin sheet of elastic or inelastic material. Here the energy will be distributed at right angles to the direction of the impact, and the mass of matter over which it is distributed instead of increasing with x will increase with πr^2. If this area is denoted by z, and if the sheet is supposed infinitely thin or of finite thickness and infinitely little strained, the distribution of energy will be represented by

$$\frac{w}{c_1}=\frac{\varepsilon^{-z/c_1}-\varepsilon^{z/c_1}}{n+1}.$$

The resulting depression will be a figure of revolution, and if the strain and stress are simply proportional, the curve generating this figure will be of the form

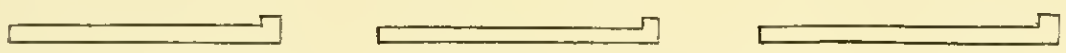
$$\frac{y}{c_1}=\frac{\varepsilon^{-x/2c_1}-\varepsilon^{x/2c_1}}{n+1}.$$

This result can be at least qualitatively tested by experiment. For this purpose I clamped a thin sheet of elastic rubber to a block of wood by a metal ring, and inserted a pointed instrument from beneath the block through a vertical hole at the center of the ring. The rubber was of course strained to conoidal shape, which was more or less sharply pointed according to the pressure. It was found that the generating curves for various pressures sensibly coincided with curves plotted from the above equation for various values of n. A form of the same character must be produced when a pointed instrument is being driven through a metallic sheet, just before penetration.

It is now easy to see the general character of the deformation which takes place when two spheres strike one another. If the sphere is supposed divided into segments of equal mass by planes at right angles to the direction of the impact, and each mass is considered as concentrated at its center of inertia the energy potentialized will be distributed among these centers as in a compressible finite cylinder. The plane section however, will not remain plane but will be indented in the direction of the force. At the point opposite that at which impact takes place, the surface will remain spherical while the contact of the two spheres will be a plane. An exact analysis of this case would probably be somewhat complex.

Effect of imperfect restitution.—Certain comparisons may easily be made between the behavior of totally inelastic or totally elastic bodies as hitherto treated and actual matter which is neither absolutely elastic nor perfectly plastic. Except under conditions which cannot be realized in practice, a portion of the energy received by any one of a series of bodies from an impact is always expended in internal work. Newton found that when the impact is not violent enough to produce sensible permanent deformation, the relative velocity of impinging bodies, after impact bears a proportion to their relative velocity before impact which is constant for the same two bodies. It is well known that at least a part of the energy not returned must be employed in producing elastic vibrations of the body struck. Messrs. Thomson and Tait therefore call the coefficient representing the relation of the velocities the coefficient of restitution instead of the coefficient of elasticity as it has commonly been designated. If this coefficient is e, the coefficient of energy potentialized is expressed by $1 - e^2$. If an infinite series of bodies, say spheres, not wholly inelastic, receive an impact they will be compressed as if totally without elasticity, a portion of the energy received by each will be expended in permanent deformation and in vibration and each will recoil with less force than that with which it was compressed. Now if e is constant for the system, the same proportion of the energy received by each member of the system (viz: $1 - e^2$) will be expended in it; and as the quantities of energy received stand as has been shown in a geometrical ratio, so also will the quantities of energy expended. The coefficient e is not constant for partially elastic bodies and so far as I know it has been but little studied. There is reason to suppose, however, that it varies slowly with the velocity and that it is therefore approximately the same for similar bodies within considerable ranges of velocity.

Passage to frictional sheets.—Having discussed the distribution of energy in a finite or infinite system when the centers of inertia are rectilinearly arranged in the direction of the active force and endeavored to check the results by reference to experience, I now pass to the application of these results to friction. Impinging bodies may be given any desired form under proper restrictions. Suppose for example a series of inelastic plates like the following:



let them be restricted to motion in horizontal planes and pass over one another without friction. If the first of this series is started in the direction of its neighbor and the system is left to itself, the momentum will remain constant, the energy introduced into the system will be distributed over the whole infinite series and, in short, the distribution of energy in a direction vertical to the line of motion will be exactly the same as it has been found to be for a rod in the line of force. Instead of a single lug at the extremity of a sheet an indefinite number of small teeth may be supposed to be distributed over the surfaces of the sheets, and if these teeth are very minute in size and very numerous, a frictional surface as I understand it is the result. It might for an instant seem an objection to this supposition that as such sheets pass over one another the teeth will be ground off and the frictional resistance will diminish. This fact however affords an argument in favor of the truth of the conception, for it is well known that machines, however well finished, tend to heat until the bearings have adjusted themselves to one another. The friction cannot be indefinitely reduced by this mutual action because the dust produced by the abrasion is sufficient to cause constant fresh inequalities in the surfaces.* The character here attributed to a frictional surface is substantially that which various writers have assigned to it; some of the logical results however seem to me to have escaped attention.

[To be continued.]

ART. XVIII.—*A Standard of Light*;† by JOHN TROWBRIDGE.

THE discussions in the Paris Conference of 1881–84 upon the subject of a standard of light, which resulted in the adoption of the light emitted by a surface of platinum at the point of solidification, seemed to assort ill with the discussions which led to a reaffirmation of the value of the C. G. S. system of absolute physical units, and a recognition of the relations between work and heat, and electrical energy.

The solidification point of platinum may be a fixed point in nature; but it has not been shown how this fixed point can be connected with the great web of physical measurements which has been woven by Weber, Helmholtz, Thomson, Maxwell, and other physicists. It is true that during the discussions of the Conference reference was made to a proposition of Schwendler, that the light emitted by a strip of platinum rendered incandescent by a known electrical current should be taken as a standard. This proposition, however, received little support; and the Conference finally adopted the light emitted by solidifying platinum as a standard.

It seems highly desirable that any standard of light which may be adopted should be connected with the present system of absolute measurements. The suggestion of Schwendler, therefore, seems to merit more attention than it has received. The suggestion of employing the light from a strip of platinum rendered incandescent by an electrical current is really due to Dr. John W. Draper, of New York, who in 1847 enunciated it as follows: "A surface of platinum of standard dimensions raised to a standard temperature by a voltaic current will always emit a constant light. A strip of that metal one inch

* As I pointed out in my former paper, page 158, the friction of ideally lubricated surfaces, in which the solid surfaces do not come in contact, is a very different matter. The statement as there given has since been amply confirmed by the report of a committee of the British Association.

† Read before the American Academy of Arts and Sciences, May 26, 1885.

long and one-twentieth of an inch wide, connected with a lever by which its expansion might be measured, would yield at 2000° a light suitable for most purposes."*

It has been urged against this standard that different specimens of platinum will emit different amounts of light with the same difference of potential; and that it would be difficult to carry out a measurement of the light and the strength of the current all at the same instant. With a view to obtaining a knowledge of the practical difficulties in this measurement, I interposed a fine platinum wire between the poles of a battery, and endeavored to measure the light emitted, together with the difference of potential at the extremities of the wire and the amount of current which passed through a tangent galvanometer. The difficulties, however, in using a fine platinum wire with a moderate battery power were great. The wire would fuse before the measurements could be satisfactorily made. I then employed a strip of platinum foil 5^{mm} wide, about 5^{cm} long, and about $\cdot02^{mm}$ in thickness. This was placed in a shunt circuit of a small Gramme machine in order that if the strip should fuse the dynamo machine might not race. With the proper speed and a suitable adjustment of resistances, the light from this platinum strip could be maintained very constant.

The strip was placed in a long Ritchie photometer box, which was provided with two mirrors inclined according to the plan of Ritchie. One half of the photometer disk was illuminated by the incandescent strip, and the other half by a sperm candle.

The electrical current was measured by a tangent galvanometer of which the reduction factor was ·44 in the C. G. S. system. The difference of potential at the ends of the strip was measured by a Thomson quadrant electrometer, the deflections of which were compared with that of a Daniell cell, the electromotive force of which was approximately 1·09. A Thomson voltmeter was also used. The indications of this instrument agreed with those of the electrometer. The following table gives the deflections of the instrument:

Deflection of Tangent Galvanometer in Degrees.	Deflection of the Electrometer in Centimeters.	
63	5·3	Light the color of a candle.
61	4·9	
59	4·6	
57	4·3	
54·5	3.8	
53·75	3·4	Light very dull red.

One Daniell cell gave a deflection with the electrometer of

* Scientific Memoirs, p. 45.

$1{\cdot}3^{cm}$. The resistance of the platinum strip when cold was ·2 of an ohm. It will be seen from the above results that the current varied approximately from 8 to 6 webers, with an electromotive force of from 3·8 to 2·6 volts, while the resistance varied from ·47 to ·44 of an ohm, the resistance when cold being ·2 of an ohm. The range of the indications of the electrical instruments was comparatively small, while the light varied enormously. It is evident that the chief difficulty of this method is in measuring a strong current with accuracy: for an increase in the current represented by a fraction of a degree of the tangent galvanometer will result in a very large increase in the light from the incandescent strip.

I next endeavored to ascertain if a thermal junction enclosed in an Edison incandescent lamp, at the center of the carbon loop, would be sensitive to changes in the heat radiation of the lamp. It is evident that, if this were the case, the carbon loop might be raised to the same point of incandescence in successive times, assuming that the thermal junction at this point of incandescence receives the same amount of radiant energy. Mr. Edison kindly provided me with a lamp in which one thermal junction of an alloy of iridium-platinum and platinum was inserted at the center of carbon loops. The other junction was placed in ice and water. The thermo-electric force of this combination, however, was extremely feeble. The difficulty of inserting wires of other metals into glass prevented me from carrying this idea further. Instead of the thermal junction a small loop of extremely fine platinum wire was placed at the center of a carbon loop in an Edison lamp. This fine wire constituted a bolometer strip and made one branch of a Wheatstone's bridge, it being my intention to place a similar strip in another branch of the bridge, thus making a bolometer. The lamp was placed in a photometer box, and its light was compared with that of a candle as it was raised from a red glow to a light of fifteen-candle power. At the same time the resistance of the fine platinum wire was measured by a Wheatstone's bridge. The following table gives the results:

Resistance of the Strip in Ohms.	Distance of Carbon Lamp from Photometer Disk.	Distance of Candle from Photometer Disk.
14·42	70^{cm}	40^{cm}
14·45	85	40
14·55	98	40
14·62	108	40

This method seems to be quite sensitive. The change in resistance is large when estimated by the number of ohms necessary to restore a balance to the bridge. It was noticed that at a certain point a comparatively small increase in heat radiations was accompanied by a large change in the amount of light emitted. This phenomenon had been noticed early by Dr. J. W. Draper. One Leclanché cell with five ohms in the circuit beside the resistance of the strip was sufficient to raise the latter to a red heat, and precautions were then necessary to prevent a change of resistance from the heating effect of the battery employed with the Wheatstone's bridge. Being desirous of ascertaining whether the resistance of the platinum wire changed after it had been heated to a red heat and had been allowed to cool, I arranged the resistance of the battery circuit outside the bridge, so that the wire could be raised to a red heat, and then, having quickly weakened the battery circuit, remeasured the resistance of the strip. No difference could be perceived in the resistance of the strip. This illustrated the fact discovered by Professor Langley, that thin strips of metal arranged as bolometer strips give up heat very quickly.

The results of this experiment led me to think that a bolometer strip of definite surface could be placed at a fixed distance from a carbon loop of definite dimensions inside an exhausted glass vessel. The amount of radiation which the bolometer strip receives could be calculated; and we might base our standard of light upon the point of incandescence which would give a definite radiation at a fixed distance. We could not distinguish by this method the energy produced by rays of different refrangibility. It seems desirable, however, to substitute for the uncertain estimation of colored lights by the eye an instrument which will measure the energy produced by the radiating source at a certain distance. Within certain limits I found that the bolometer strip would indicate an increase or decrease of the amount of radiant energy received while the difference in color of the incandescent lamp made the observer at the photometer entirely uncertain of his measurements.

Owing to the difficulty of obtaining the proper apparatus for the prosecution of the study of this method, I then studied the question of the practicability of employing a thermopile to measure the amount of radiation from an incandescent strip of platinum at a fixed distance. Within a long photometer box was placed a thin brass vessel containing water. Steam was passed by means of a rubber hose into the water of this vessel which was thus maintained at a constant temperature of about 94° C. The outside of the vessel was about 92° C. This was ascertained by making the side of the vessel constitute one metal of a thermal junction. Between this vessel and the platinum strip, which was made incandescent by a current of from 8 to 9 webers, was placed a thermopile. The face of the thermopile was thus exposed to the radiation from a given amount of heated surface at a constant temperature, while the other was exposed to the radiation of a given surface of platinum. The faces of the thermopile were provided with the customary cones, and a series of diaphragms of thick card-board extended between the radiating surface of the vessel containing the heated water and the platinum strip. The thermopile was connected with a short coil galvanometer, and was moved until the galvanometer needle came to zero. This arrangement was extremely sensitive—a movement of a centimeter in the position of the faces of the pile being sufficient to drive the spot of light from the galvanometer mirror off the scale, corresponding to a movement of nearly fifty centimeter scale divisions. There is no difficulty in effecting a balance as quickly as an ordinary photometric measurement is made. While one observer compares a candle or other source of light with the light from an incandescent strip of platinum, another could make the measurements with the thermopile, and could obtain the amount of energy radiated by the incandescent strip in terms of the constant source of heat. It is necessary to reverse the faces of the thermopile, or to place a second constant source of heat on the same side upon which the incandescent strip is placed. The following table indicates the character of the results:

Deflection of the Tangent Galvanometer.	Temperature of Water.	Distance of Face of Pile from Water.	Distance of Face of Pile from Strip.	Remarks.
°	° C.	cm.	cm.	
57·5	95	26·5	49·5	Dull red.
56·5	95	28·0	48	
61	96	25·5	50·5	Bright yellow.
62·5	96·5	24·5	51·5	" "
62·5	96	23·0	53	" "
60·5	97	25·7	50·3	" "
58·5	97·5	26·7	49·3	" "
60	95	24·2	51·8	" "
62·2	94	23·7	52·3	" "

The reduction factor of the galvanometer was ·44 in the C. G. S. system. When the photometric indications were the same, the thermopile indicated a large change in the amount of heat received. Thus the heat indications within the range in which the experiments were taken were far more sensitive than the photometric indications.

It seems possible, therefore, to assume as a standard of light an incandescent strip which radiates a definite amount of energy, this energy being measured at a fixed distance which will best agree numerically with the absolute system of measures now universally adopted in heat and electricity. The method of Draper and Schwendler could be combined with the methods I have described above. For a practical standard, a carbon loop in an exhausted vessel raised to such a point of incandescence that it will radiate a definite amount of energy—this energy being measured by a bolometer strip or the thermopile at a definite distance from the carbon loop, and also being measured by the formula $JH=C^2Rt$, would have a greater range than an incandescent strip of platinum placed in free air. The latter method, however, for the incandescence which produces a light similar in color to that of a sperm candle, is extremely sensitive, and can be made, I think, more exact than present photometric tests. Both methods have the great advantage of substituting a measure of energy for a relative indication by the eye, which is not connected with any absolute measurement.

These remarks apply to the question of a standard of light for practical purposes, which shall also be scientific in so far that more refined scientific investigation can connect this standard at any time with more precise methods of measuring the exact amount of heat given by radiations of definite wavelength. By means of a Rowland concave grating and with a bolometer strip, one can at present measure the energy of definite radiations. We can say that our scientific standards for light of different colors shall be based upon the energy received upon a definite surface at definite points in the diffraction spectrum.

Jefferson Physical Laboratory, Harvard College.

Art. XIX.—*On Hanksite, a new anhydrous sulphato-carbonate of sodium, from San Bernardino County, California;** by Wm. Earl Hidden.

In the very complete and attractive exhibit of California minerals brought to the World's Industrial and Cotton Centennial Exposition at New Orleans, by Professor Henry G. Hanks, State Mineralogist of California, were several species of unusual interest. Among these was the new borate, colemanite, in large and brilliant crystals, much resembling the finest of the Bergen Hill datolites; also the new vanadium mica, roscoelite, mixed mechanically with much native gold between the folia; borax crystals, clear and bright, of unusual size; stibnite in fine crystals almost equalling the late discoveries in this species in Japan, and many others equally noteworthy.

Of particular interest to the writer was a small lot of apparently hexagonal crystals to which had been given the name of thenardite. Now as thenardite crystallizes in the orthorhombic system, I was prompted to question the correctness of this

* Read before the New York Academy of Sciences, May 25, 1885.

determination. The results of measurements confirmed my first suspicions of their true hexagonal character, though only approximate, being made with a hand goniometer. Since, however, the hexagonal aspect of the mineral might possibly be due to complex twinning of orthorhombic individuals, it seemed advisable to have this question decided on the basis of an optical examination. For this purpose three of the best crystals were kindly given by Professor Hanks, and these were sent by me to Dr. Edward S. Dana; the crystals being quite clear. In a few days he reported them to be normally uniaxial with negative double refraction, and thus they were positively proved to be different from thenardite. An analysis being now necessary to settle the composition of the mineral, I placed sufficient material in the hands of Mr. James B. Mackintosh, E. M., for that purpose, and he has very kindly done the work, with the following results:

		Corresponding to	
SO_3	45·89	Na_2SO_4	81·45
CO_2	5·42	Na_2CO_3	13·06
Cl	2·36	NaCl	3·89
Na_2O*	46·34	Na_2O (excess)	1·08
			99·48

These results give the following molecular ratios for

Na_2SO_4	57·3	or	3·95
Na_2CO_3	14·5		1·00
NaCl	6·65		·46
Na_2O	1·75		·12

Or closely in the ratio of $4:1:\frac{1}{2}:\frac{1}{8}$. This points to the formula,

$$4(Na_2SO_4)+Na_2CO_3+\tfrac{1}{2}(NaCl)$$

as representing the composition of the crystals under examination. Neglecting the sodium chloride as non-essential, the formula becomes:

$$4Na_2SO_4+Na_2CO_3$$

which is probably the true one.

The observed excess of soda is either due to errors of analysis, as only a small quantity was used, or it may have been combined with boracic acid, as borax is very abundant at the locality.

The interesting anomaly of a sulphate and carbonate being in chemical combination reminds us of the rare sulphato-carbonate of lead, leadhillite, to which this alone bears relation as a natural species.

The angles obtained were as follows:

O on *I*= 90°. *O* on 1=130° 30′.
I on *I*=120°. *O* on 2=113° 30′.

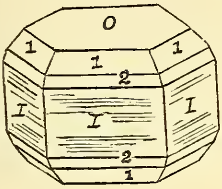
Accordingly, the value of the vertical axis is 1·014. Cleavage parallel to *O* nearly perfect, but difficult to obtain.

* All bases calculated as soda. Lime and magnesia were not present.

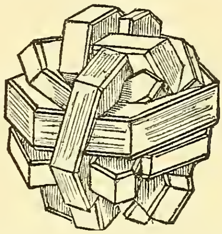
Crystals striated horizontally. They are commonly terminated at both ends of the prism and are very symmetrical in shape. They average, as thus far seen, about one centimeter in length and thickness, with *O* and *I* as predominating planes (fig. 1).

Sometimes the crystals are confusedly grouped (fig. 2), as from a common center, much like the aragonite from a noted European locality. For some late years mineralogists have received from several localities in the far West groups of tabular crystals that were hexagonal in appearance, very impure in composition, and to which the name of aragonite has

1.



2.



been attached. For the most part they are simply calcium carbonate mixed with sand and mud, and are without cleavage. It is very probable that they are pseudomorphs after the sodium sulphato-carbonate here described. In particular I refer to crystals which I have seen credited to Colorado and to Nevada.

The crystals here analysed were found with salt, thenardite, tincal, etc., at the works of the San Bernardino Borax Co., in San Bernardino County, California.

The density of this new California mineral is 2·562. Its hardness, 3·–3·5. It is readily soluble in water. Effervesces with acids. It affords, when dissolved in water, an abundant precipitate of barium sulphate when barium chloride is added to the solution. On addition of silver nitrate to a fresh solution chloride of silver is precipitated, showing that chlorine is also present. Gentle ignition develops no appreciable loss in the weight of the mineral.

The crystals are transparent to semi-opaque, with a white waxy color inclining to yellow. Surfaces never highly polished nor very smooth.

The definite formula deduced from Mr. Mackintosh's analysis, taken together with the form, warrants me in announcing this as a new mineral species. I therefore propose for it the name of Hanksite, after Professor Henry G. Hanks, of California, to whom we are so largely indebted for our knowledge of the minerals of the Pacific coast.

Newark, N. J., May 23, 1885.

ART. XX.—*Mineralogical Notes;* by EDWARD S. DANA and SAMUEL L. PENFIELD.

1. *A large crystal of Hanksite.*

SOME two years since Professor G. J. Brush placed in our hands for examination a large crystal, or rather group of crystals, of an anhydrous sulphate related to thenardite. The specimen had been received by him from Professor J. S. Newberry, who stated that he had purchased it in California but was unable to learn the exact locality from which it came. The examination proved it to be probably hexagonal in crystalline form, and in composition to consist of sodium sulphate and carbonate in the ratio of 4:1. Feeling reluctant to attach a new name to a mineral of which only one specimen was in hand, and that from an unknown locality, we postponed the publication of our results until some further facts should come to light. The same mineral has now been re-discovered and in specimens so satisfactory as to justify their receiving the name Hanksite, given by Mr. Hidden.

The specimen examined by us consisted of a low hexagonal prism, measuring transversely 75^{mm}, and in a vertical direction 20^{mm}; this prism is penetrated by several other similar tabular crystals but in varying positions, so that no general law of twinning can be given. The basal edges were irregularly replaced by pyramidal planes. Apparently the form is hexagonal, the prism and pyramid both being present, and the measured angles of the former showing very little variation from the required 60°. The analogy of the artificial sulphates of sodium and potassium suggested, however, that the form might be really orthorhombic, and the hexagonal aspect due to twinning. The optical examination made to settle the question was not satisfactory because the crystal contained so much mud as impurity as to be transparent only in spots. Some points were found, however, which gave an obscure uniaxial figure with negative double refraction; but this question might not be regarded as satisfactorily settled were it not for the excellent results which Mr. Hidden's crystals have afforded. The pyramidal plane spoken of was rough and rounded and was only distinctly seen on part of the edges. The approximate angle (supplement) measured on the basal plane is 43°, which, referred to the vertical axis assumed by Mr. Hidden, gives a symbol $\frac{4}{5}(40\bar{4}5)$; required 43° 8′.

An analysis of the mineral gave (Penfield) the following results, which are almost identical with those of Mr. Mackintosh:

		Ratio.				
SO_3	43·59	·545	and	·545	=	43·59 SO_3
Na_2O	40·86	·659		·536		33·23 Na_2O
				·123		7·63 Na_2O
CO_2	5·42	·123		·123		5·42 CO_2
K	2·33	·060				2·33 K
Cl	2·13	·060				2·13 Cl
Insol.	4·41					
Ign.	1·32					
	100·06					

This corresponds then to

Na_2SO_4	76·82
Na_2CO_3	13·05
KCl	4·46
Insol.	4·41
Ign.	1·32
	100·06

The insoluble portion is apparently clay; a section examined in the microscope showed the impurity densely distributed in bands parallel to the prismatic faces. The fact that in the analysis the potassium and chlorine are present in exactly the amounts required to form potassium chloride may be only a coincidence, and the chlorine may in fact be combined with sodium, and the potassium may in part replace the sodium in the sulphate. It is immaterial which explanation is adopted, but in any case it is quite certain that the potassium (or sodium) chloride is present as an impurity, for in the thin section numerous rectangular crystals, some of them apparently cubes, were visible. It seems proper, therefore, to deduct these elements from the analysis, leaving only the sodium sulphate and sodium carbonate. The result, calculated to 100 per cent, is

Na_2SO_4	85·48
Na_2CO_3	14·52

This corresponds, though approximately only, to the formula

$$4Na_2SO_4+Na_2CO_3$$

which requires

Na_2SO_4	84·27
Na_2CO_3	15·73
	100·00

2.—*An artificial crystallized Lead Silicate.*

We are indebted to the late Professor Silliman for a specimen of an artificial crystallized lead silicate, which had been obtained by him from the Desloge Lead Company, Bonneterre, St. Francois County, Missouri. The specimen consists chiefly of a brownish-red substance with resinous luster somewhat resembling sphalerite, associated with octahedral crystals of magnetite and cleavable galena. In small cavities in the mass and over the surface are numerous crystals, mostly stout hexagonal prisms, which, as is proved later, consist of the same material as the mass of the specimen. These crystals in color and form very closely resemble much of the vanadinite of Arizona, and, before the history of the specimen was known, they were referred to that species almost without question. Later, however, it was learned that the specimen was artificial in origin, and according to the chemist of the company was a lead silicate. A preliminary chemical examination proved that to be true, and a complete analysis was accordingly made of the hexagonal crystals and also of the massive substance.

The crystals spoken of vary in length and thickness from 1 to 3^{mm}. They show ordinarily only the planes of the unit prism I ($10\bar{1}0$) and the basal pinacoid O (0001); rarely, however, the basal edges of the prism are rounded and in a few instances distinct planes were observed. The best defined of these gave an approximate angle of 50° on the basal pinacoid

$$O \wedge p,\ 0001 \wedge 10\bar{1}1,\ =50^\circ.$$

This gives as the length of the vertical axis

$$c=1\cdot032.$$

A second pyramidal plane (q) gave an angle on the base of 25°, which corresponds tolerably to the symbol $\frac{2}{5}$ ($20\bar{2}5$), required 25° 29′. Apparently these pyramidal planes are holohedrally developed, so that the crystals would then be referred to the hexagonal system proper, but the material was too scanty absolutely to prove this point. There is some reason to doubt it, however, for on the other side of the same specimen, distant from them hardly an inch, are a few crystals apparently of the same material, but of quite different form. These are thin tabular crystals showing only the basal pinacoid and a rhombohedron; the measured angle for this is 67°, which, referring it to the above axis, would give it a symbol $2R$, or π ($20\bar{2}1$). The calculated angle is

$$O \wedge 2R,\ 0001 \wedge 20\bar{2}1,\ =67^\circ\ 12'.$$

The quantity of these tabular crystals was too small to admit of its being proved that they have the same composition as the hexagonal prisms, but everything points to that conclusion.

The substance was readily soluble in even very dilute, cold nitric acid: the solution upon evaporation yielded gelatinous silica. Before the blowpipe readily fusible. Specific gravity of crystals 5·92. For analysis the compound was dissolved in cold, dilute nitric acid and the insoluble magnetite filtered off. Two analyses were made (Penfield); for the first (I) only distinct crystals were taken, of which ·7073 grams were selected; the result after deducting 0·44 per cent of magnetite is given below. The second analysis (II) was of the crystalline part of the slag. The result is given after deducting ·207 per cent of magnetite. The presence of a trace of carbonic acid, and a very minute trace of phosphoric acid was distinctly proved.

I. Crystals.		Ratio.	
SiO_2	17·17	·286	
PbO	72·39	·325	·486 (PbO to Na_2O)
FeO	0·51	·007	
CaO	7·48	·134	
MgO	0·56	·014	
Na_2O	0·35	·006	
	98·46		

II. Massive portion.		Ratio.	
SiO_2	16·00	·267	
PbO	75·26	·338	·476 (PbO to Na_2)
FeO	0·74	·010	
MnO	0·17	·002	
CaO	6·15	·110	
MgO	0·50	·012	
Na_2	0·24	·004	
CO_2	trace		
	99·06		

The ratio in I for SiO_2 : RO = ·286 : ·486 = 1 : 1·70 = 4 : 7 nearly. For II the ratio is SiO_2 : RO = ·267 : ·476 = 1 : 1·78 = 4 : 7 nearly. The agreement between these shows that the formula, $R_7Si_4O_{15}$, must express very closely at least the true composition of this artificial lead silicate. The occurrence of so rare a compound is especially interesting in connection with the recent discovery of native lead silicates, notably at Långban, Sweden.

These natural lead silicates include the three following from Långban: ganomalite, a silicate of lead and manganese with small quantities of lime and magnesia; hyalotekite, a silicate of lead, barium and calcium; melanotekite, a silicate of lead and iron. To these should be added kentrolite, from southern Chili, a silicate of lead and manganese. All of the minerals named are crystalline in structure, but kentrolite alone has been found in distinct crystals. We do not know that any artificial crystallized lead silicate has been described hitherto.

ART. XXI.—*The amount of moisture which Sulphuric Acid leaves in a Gas;* by EDWARD W. MORLEY.

BERZELIUS and Dulong,* about 1820, and Erdmann and Marchand,† about 1842, employed calcium chloride in determining the atomic weight of oxygen, and were probably not aware that it leaves unremoved a comparatively large amount of water. Dumas,‡ in 1842, and Pettenkofer,§ in 1862, mentioned as well known, the fact that calcium chloride will not dry a gas as completely as sulphuric acid. Favre,‖ in 1844, proved that sulphuric acid at ordinary temperatures dries a gas so completely that neither sulphuric acid at −17° C. nor phosphorus pentoxide will absorb a sensible quantity of moisture from 40 liters of gas, nor even from volumes "bien plus considérables." He also attempted¶ in a most ingenious way to determine whether a gas dried by either of these agents was absolutely dry. He passed air dried as perfectly as possible over red-hot copper, and then again through a drying tube. In one experiment, 148 liters of air were reduced to 117 liters of nitrogen, and deposited ·0025 gram of water; in the other, 110 liters of air were reduced to 87 liters of nitrogen, and deposited ·0015 gram of water. Hence he concluded that a liter of gas dried by sulphuric acid or phosphoric oxide contained not more than ·00006 or ·00008 gram of water.

He also proved** that no other force than the tension of the vapor of water causes it to be retained in certain gases. Favre further proved,†† as did Regnault in 1845,‡‡ that drying tubes of no large dimensions are required to utilize the whole drying power of the drying agent with which they are filled. In 1864 and again in 1865, Fresenius§§ published experiments, which, if they were affected by no source of error at that time unsuspected, would show that sulphuric acid leaves one or two decimilligrams of moisture in a liter of gas. But in 1876, Dibbits‖‖ published experiments in which precautions were taken against the leakage of moist air through caoutchouc connectors, which showed that 308 liters of air dried by sulphuric acid at ordinary temperatures gave up but 7 decimilligrams of moisture to phosphorus pentoxide.

Dibbits also proposed a method for solving the remaining

* Ann. Chim. Phys., 2d Series, vol. xv, p. 388.
† Journ. Prakt. Chemie, vol. xxvi, p. 464.
‡ Ann. Chim. Phys., 3d Series, vol. viii, pp. 193, 210.
§ Lieb. Ann. Supp., vol. ii, p. 29.
‖ Ann. Chim. Phys., 3d Series, vol. xii, p. 223.
¶ Ibid., vol. xii, p. 225.
** Ibid., vol. xii, p. 227.
†† Ibid., vol. xii, 228.
‡‡ Ibid., vol. xv, p. 152.
§§ Zeitschr. Anal. Chem., vol. iv, p. 180.
‖‖ Zeitschr. Anal. Chem., vol. xv, p. 160.

question: How much moisture is left in a gas dried by phosphorus pentoxide? He proposed to evolve a gas of which we might be certain that it contained no water, to pass it into water, and then to dry it with phosphorus pentoxide. He suggested the evolution of dry oxygen by heating fused potassium chlorate. Whether perfectly dry oxygen could be thus obtained remains to be seen; the task of keeping up a suitable current of oxygen till a few hundred liters shall pass the absorption tubes would involve a good deal of labor.

Desiring to know the amount of water which sulphuric acid or phosphorus pentoxide fails to remove from a gas, I succeeded in devising a method which has made the solution of the problem easy. It permits the determination of the absolute amount of moisture left in a gas by any drying agent; the maintaining a slow current of air for days or weeks demands attention for only some five minutes each day, so that very large volumes of air may be used, at small velocities, and even if the residual moisture is as little as a hundredth or a thousandth of a milligram in a liter, it may be determined with any needed accuracy.

I devised the method with the intention of applying it first to phosphorus pentoxide. But in the third number of the Zeitschrift für analytische Chemie for 1884, Mathesius made certain statements about the use of sulphuric acid in drying tubes, in consequence of which I first undertook the study of the absolute amount of moisture left in a gas by this drying agent.

The paper of Mathesius raised a preliminary question which had to be answered. He found that certain drying tubes filled with sulphuric acid, of specific gravity 1·84, when used to absorb moisture as in organic analysis, lost weight at the rate of five or more decimilligrams an hour. This statement must be taken as referring to sulphuric acid supposed to be pure; because a statement that impure sulphuric acid contained some volatile impurity would hardly be worth publication; and also because, in order to lessen the loss of weight in his drying tubes, Mathesius diluted the acid somewhat, probably supposing that the vapor of sulphur trioxide escaped and occasioned the loss of weight.

It is difficult to believe that either water or sulphur trioxide can be given up by pure sulphuric acid to a current of gas in any such quantity as Mathesius observed. Regnault* determined the tension of the vapor of water given off at 20° C. by sulphuric acid of the formula SO_3+2H_2O. This is ·15 millimeter, so that a liter of absolutely dry air passing through such acid would take up at this temperature ·16 milligrams of water.

* Ann. Chim. Phys., 3d Series, vol. xv, p. 179.

He gives no results for acid more concentrated than this; but from a comparison of the results for more dilute acids, it is difficult to believe that an acid containing half a molecule of water and one molecule of the monohydrated acid would give up to a liter of absolutely dry air as much as the twentieth of a milligram of water at ordinary temperatures.

And as to the evaporation of sulphur trioxide from sulphuric acid: Dumas* passed 20 liters of air through pure sulphuric acid, and into solution of barium chloride, which preserved "une limpidité absolue." But if the loss of weight observed by Mathesius was due to the escape of sulphur trioxide, Dumas should have obtained not only a visible but a weighable precipitate.

But while we may dismiss the idea that sulphur trioxide escapes from sulphuric acid in drying tubes in ordinary conditions in any such quantities as several decimilligrams an hour, it was necessary for the determination of the absolute amount of moisture left unabsorbed by sulphuric acid that the amount of sulphur trioxide volatilized should be accurately determined. For this purpose I made several experiments. In one of them, a wash bottle and a Winkler's absorption tube were filled with pure sulphuric acid. This acid I distilled from a pure acid, rejecting the first and the last fifth. Its specific gravity at 22° and at 16·8° C., compared with water at 4°, weights being reduced to a vacuum, and the thermometer being corrected for error of zero point, was found to be 1·8344 and 1·8394. A current of air was aspirated through a gas-meter, through the wash bottle of acid, through the absorption tube with acid, through an empty tube two meters long, through a plug of glass wool, and through an absorption tube with pure water. The acid in the absorption tube occupied about two meters and a half; the water in the other absorption tube occupied about a meter. The parts of the apparatus were fused together. When 6800 liters had passed, not too rapidly, the sulphuric acid in the water was determined as barium sulphate, and found to be 3·1 milligrams. In a second experiment at a somewhat lower temperature, ·7900 liters were passed and 2·5 milligrams of acid were found in the second absorption tube. Several experiments were made in which air passed, at the rate of two liters an hour, into a solution of barium chloride; in which experiments neither myself nor Dr. Spenzer, my assistant, could detect any trace of a precipitate till the third day.

With the degree of approximation thus far obtained, therefore, we may conclude that a liter of air passed through sulphuric acid of the specific gravity of 1·84 will take up something like the two thousandth or three thousandth part of a milligram of sulphur trioxide at ordinary temperatures.

* Ann. Chim. Phys., 3d Series, vol. viii, p. 204.

This being determined, the way was clear to determine the absolute amount of moisture left in a gas by sulphuric acid. To Liebig's potash bulbs I fused a sixth bulb, connected with the others by a capillary tube of so small bore that when a vacuum was maintained at one end, one or two cubic centimeters of air would pass through it in a minute. In this sixth bulb I placed sulphuric acid so diluted with water that air passing through it would take up a certain small amount of water. In the bulbs which belonged to the original apparatus was placed sulphuric acid of specific gravity 1·8381 at 18° C., compared with water at 4° C., weights and thermometer being corrected.

In use, a partial vacuum was maintained in the five bulbs containing strong acid, while the dilute acid was in contact with air at ordinary pressure. Air in passing from the dilute acid through the constriction towards the strong acid would therefore expand a number of times depending on the pressure in the partial vacuum. The air before entering the dilute acid was made as dry as sulphuric acid can render a gas; it took up water from the dilute acid; it was expanded; the increased volume was made as dry as sulphuric acid can render a gas.

Let us imagine, for the sake of clearness, that the expansion in passing the constriction was ten times, that five liters of air entered the dilute acid, and that therefore fifty liters passed out of the strong acid. Let us also make two alternative suppositions in order; first, that sulphuric acid makes a gas perfectly dry, and second, that it leaves a hundredth part of a milligram of water in a liter of gas.

If, according to the first supposition, sulphuric acid makes a gas perfectly dry, the five liters of dry air which enter the six-bulb apparatus carry into it no water. In the sixth bulb they take up a small quantity of water. Passing the constriction, they become fifty liters. The sulphuric acid makes the fifty liters perfectly dry, and no water is carried out of the apparatus. Therefore the only effect changing the weight of the apparatus is the escape of sulphur trioxide, the amount of which is approximately known.

But, secondly, if sulphuric acid leaves a hundredth of a milligram of water in a liter of gas, the five liters of air entering the apparatus carry into it one-twentieth of a milligram of water. In the sixth bulb, more water is taken up. The five liters expand to fifty. Now the drying such a gas as air is simply a process of reducing the vapor tension of the accompanying vapor of water. One liter of air dried by sulphuric acid will contain water possessing a certain tension, whatever be the pressure of the air. At least Regnault proved this to be true within one per cent. The fifty liters of air will therefore carry out ten times as much moisture as the five liters brought in. Neglecting for a moment the evaporation of sulphur trioxide, the apparatus will lose nine twentieths of a milligram in weight. Conversely, if we knew the expansion to be ten times, and the entering air to be five liters, and the loss of weight to be ·45 milligram, we could compute the water remaining in each liter to be 0·01 milligram.

In applying this principle, it was necessary to avoid errors due to leakage of moist air into the apparatus during the long time through which an experiment lasted, and to provide for weighing the six-bulb apparatus so accurately as to make certain the detection of a total effect of a few tenths of a milligram. I secured sufficient accuracy in weighing by using as a counterpoise an apparatus of the same shape and same kind of glass, filled with the same acid, and interposed in the same current of air. To these I fitted ground glass caps as accurately as I could, so that I could leave the two apparatus on the balance for several days without any change in their relative weights; of course after applying corrections for the state of the barometer and thermometer.

I also fitted to the two apparatus just named, a third, which prevented the diffusion of moist air backward from the vacuum, and a fourth which dried the air before it entered the first and second apparatus, all, to each other, by glass tubes with joints carefully ground which were made tight with a fat from which all matter volatile at common temperatures had been removed. In this way, I could leave the apparatus for weeks with the certainty that moist air could not enter the apparatus.

The measure of the volume of air which enters the apparatus and of the expanded volume which leaves it is easy. The third drying tube which prevents the diffusion of moist air backward was fused, together with a barometer gauge, to a tube leading to an air-tight reservoir of 54·1 liters capacity. When all the drying tubes were in place, the pressure in the reservoir was reduced to such a fraction of an atmosphere that the air passed through the last five bulbs of the six-bulb apparatus at the rate of about two liters an hour. This pressure was observed on the barometer gauge. Call the pressure a. On the next day, or sometimes in twelve hours, the pressure was again observed. Call the second pressure b. The pressure in the reservoir was then again reduced, and the apparatus was ready for another day of action.

Now, disregarding variations of temperature and supposing the barometer constant, remembering that the expansion of the air takes place so slowly that no cooling effect is sensible, we can easily compute the volume of rarefied air which has passed out of the six-bulb apparatus while the pressure has increased from a to b. For this purpose put

w=the weight of the air which would pass through the constriction in the unit of time, if a perfect vacuum were maintained on one side, and the barometric pressure on the other.
x =the pressure in the reservoir.
h =the height of the barometer, assumed constant.
t =the time.
u =the weight of air in the reservoir at the pressure x.
c =the capacity of the reservoir.
l =the weight of a liter of air at the temperature and pressure of the air during the experiment.
V=the volume of rarefied air passing out of the apparatus while the pressure rises from a to b.

The weight of air passing out of the six-bulb apparatus in the unit of time, when the pressure in the vacuum is not zero, but x, may be written $wf(x)$. At normal pressure, its volume would be $\frac{w}{l}f(x)$, but under the pressure x, its volume would be $\frac{h}{x}\frac{w}{l}f(x)$, and we therefore have $dV=\frac{h}{x}\frac{w}{l}f(x)dt$. Also it is obvious that $\frac{x}{h}=\frac{u}{cl}$, so that $du=\frac{cl}{h}dx$; and obviously $du=wf(x)dt$. From the last two equations, we find $dt=\frac{cl}{hwf(x)}dx$; and substituting this value we get $dV=c\frac{dx}{x}$.

Therefore $$V=c\int_{x=a}^{x=b}\frac{dx}{x}=c\log\frac{b}{a}.$$

This is the volume of rarefied air which passes the constriction while the pressure rises in the reservoir from a to b. Its volume on entering is computed in an obvious manner.

I have so far made three experiments, as follows:

	1.	2.	3.
Air entering apparatus = n	27 l.	23 l.	58 l.
Mean temperature = t	16° C.	15° C.	19° C.
Air leaving apparatus	286 l.	228 l.	757 l.
Excess leaving	259″	205″	699″
Liters an hour	1·6″	1·4″	1·2″
Dilute acid, specific gravity	1·707	1·707	1·566
Vapor tension of water from such acid at temperature t	·48^{mm}	·43^{mm}	1·85^{mm}
Milligrams water taken up by n liters air, at t } computed	13^{mg}	10^{mg}	106^{mg}
Specific gravity strong acid at $\frac{18°}{4°}$	1·8381	1·8381	1·8388
Loss, decimilligrams	5½	4½	18

Experiments 1 and 2 were parts of the same experiment which was interrupted to see if the loss of weight were proportional to the amounts of air in the line marked "excess leaving." In all three experiments, 1163 liters more passed out than entered the apparatus, the sum of the losses in weight is 2·8 miligrams. The amount of sulphur trioxide which escaped may be computed at ·4 milligram. The remaining 2·4 milligrams is the weight of aqueous vapor carried out of the apparatus by 1163 liters of air. The quantity subtracted is affected with some uncertainty, since the air used in the experiment on the evaporation of sulphur trioxide was not purified from organic matter, and there may have been reduction of the acid to the dioxide, and reoxidation to sulphuric acid. But with the approximation so far obtained, the water which strong sulphuric acid fails to remove from a slow current of air is about the four hundred and fiftieth or five hundredth part of a milligram in a liter of air.

Dibbits* showed that 308 liters of air dried by sulphuric acid gave up ·7 milligram to phosphorus pentoxide. It is curious that this is the quantity which my experiments show to be left by sulphuric acid in that quantity of air. The obvious inference may be true, but is not safe. I shall hope to repeat these experiments on the evaporation of sulphur trioxide from sulphuric acid with purified air, and those on residual moisture left in a gas by the acid with some form of apparatus permitting more accurate weighing than Liebig's bulbs.

749 Republic Street, Cleveland, Ohio, May 25th, 1885.

Art. XXII.—*Local Deflections of the Drift Scratches in Maine*; by G. H. Stone.

On almost every glaciated surface in Maine may be found isolated drift scratches aberrant both in direction and outline. Often these are somewhat curved and may grow deeper toward their south ends where they usually terminate abruptly. In a few cases I have found reversed curves making the scratch resemble the mathematical sign of integration. They sometimes vary 10° or even 20° from the average direction of the glaciation and hence they often intersect other scratches near them. Indeed it is the exception to find the glacial scratches exactly parallel even when adjacent, and it is often by no means an easy task to decide which of a given series of scratches most nearly represents the true direction of the glacial movement at that point. The writer is persuaded that the time has come for glacialists to unite in adopting some more

* Zeitschr. Anal. Chem., vol. xv, p. 160.

accurate system of measurements than has heretofore prevailed. A full discussion of this subject is reserved for another occasion. For our present purpose it is assumed that in order to prove by means of glacial scratches, that the ice at a given place moved in different directions at different times, it is needful to prove that there are intersecting series of scratches at that point.

The following instances of local changes in the direction of the ice-flow in Maine have been reported. In the Maine Geological Report for 1861 (p. 262), Professor C. H. Hitchcock reports intersecting drift striæ at Chamberlain Lake in northern Maine, and perhaps his observations on the drift of the upper Saint John Valley may indicate local glaciation. Professor Hitchcock has also described a *roche moutonneé* in York County, Me., on the stoss side of which the drift scratches bear southeastward, while on the lee side they bear southwestward. Professor G. L. Vose was the first (American Naturalist, vol. ii, p. 28) to report the signs of a local glacier in the Androscoggin Valley. This glacier extended from the White Mountains eastward to near West Bethel, Me., as is proved by the moraines and by the glacial scratches which, near the bottom of the valley, are nearly parallel with the course of the valley, while the glaciation of the higher hills and the surrounding region is nearly at right angles to this direction. These observations regarding the local Androscoggin glacier, the writer has carefully verified. The other localities named have not been visited by me. Professor F. C. Robinson of Bowdoin College, has also informed me that he has found intersecting drift scratches on the bank of the Penobscot River, a few miles west of Chesuncook Lake.

During the past summer the writer has discovered three other localities showing intersecting drift striæ. That the secondary glaciation was not sooner discovered is due to the fact that the slates east of Waterville weather quite readily. The later scratches are shallow, and long after they have been obscured by weathering, the earlier and deeper ones remain quite distinct. This gives an appearance of freshness to rock exposures, which is very deceptive.

1. *The Sebasticook Valley Locality.*

The Sebasticook River flows from the east into the Kennebec River at Waterville. Numerous observations of the drift striæ on both sides of the Sebasticook show that there is an area several miles long in the towns of Winslow, Benton and Clinton, where there are two and in a few places three superposed systems of scratches. In a few places very near the Sebasticook, the later scratches are very confused, pointing in many directions, whether from berg drift or ice gorges of the river is yet uncertain. The intersecting systems of scratches were traced to within one-half a mile of the Kennebec River, and if any fresh exposures could have been found, they might perhaps have been traced quite to that river. No opportunity has yet offered for excavation, which will be necessary for determining the limits of the area showing deflections in the direction of the ice-flow. The facts will be sufficiently indicated by a few typical examples. All the angles given in this paper are corrected for the magnetic variation, and are therefore azimuths, not bearings.

North Vassalboro, S. 11° W.

S.E. part of Winslow, older striæ S. 4° E.; newer, S. 24° E.

S. part of Benton, older S. 6° W.; newer S. 26° W.

Three miles S. of Clinton village, S. 24° W.

Benton, N. side of Sebasticook, oldest series S. 14° W.; next later S. 24° W.; and a very faint series comes last, S. 34° E.

Winslow, one-half mile E. of Kennebec, older S. 24° W.; later, S. 69° W.

These observations plainly show movements of the ice in more than two directions. The eastward flow in the S.E. part of Winslow probably indicates a local movement down the Sheepscot Valley. The valleys of the Kennebec, the Sheepscot and the Penobscot are more favorably situated for local movement than any other of the larger river valleys of Maine, though perhaps the valley of the St. Croix should be added to this list. The shorter valleys of the St. George, the Union, the Narraguagus, the Machias, and the rest of the small coast streams are equally favorably situated in the matter of direction, as they lie nearly north and south, but they do not penetrate the high ranges of hills which formed a barrier to farther ice-flow southward after the ice became less than about 500 feet thick. Near the Sebasticook, the latest scratches are quite shallow, and have not effaced the earlier ones as they have done in the track of the local Androscoggin glacier. A fair inference is that at the time when these latest scratches were made, the ice was becoming thin and that the flow was not long continued in the direction of the latest glaciation. Further exploration will be needed in order to bring out all the facts; thus far we are justified in regarding these local deflections as an incident of the decay of the great ice-sheet, at a time when the local topography affected the ice-movements much more than during the time of maximum glaciation.

2 *and* 3. *The Belfast and the St. George River Localities.*

These localities are so closely connected that they may best be described together. For about ten miles north of Union Village, the St. George River (St. George on the maps but locally known as George's River) flows in a narrow valley between steep ranges of hills. The ridge on the west side of the valley rises from 200 to 500 feet above the river, that on the east from 600 to 1100 feet above it. The last named range chiefly lies in the towns of Hope and Union, and rises to an elevation of 1240 feet above the sea. East of it is an irregular clump of high peaks and ridges in Camden and Lincolnville.

Near Union Village the observed direction of the drift scratches is S. 15° W. Near the St. George River from the north line of Union to the north line of Appleton, S. 25° to 30° W., approximately parallel with the valley. Near North Appleton the valley series of scratches is crossed by a later series at an angle of about 75°. These later scratches were found on the east side of the valley, about 100 feet above the river, and run obliquely down the hill toward the bottom of the valley and almost due west. The limits of this area of local ice-movement have not been determined.

If we leave the valley of the St. George at Searsmont and pass eastward, our course will lie two or three miles north of the high hills of Hope and Lincolnville before referred to. The westward deflection of the glaciation increases as we proceed. In the south part of Belmont, about five miles east of Searsmont, the direction of drift striæ is S. 35° to 37° W. Yet this place is in the broad valley of a stream which flows southeastward to Lincolnville Village, while to the south lies a narrower low pass through Hope and West Camden. Here then, although a natural drainage slope lay off to the southeast, and a favorable pass to the south, yet the ice bore off over a rolling plain to the southwest into the narrow valley of the St. George. Three miles farther east in Belfast, the scratches follow the valley of Little River, S. 10° to 20° E. Numerous exposures of the rock in Belfast and Morrill show uniformly an eastward trend of the drift-scratches, sometimes as much as S. 40° E.

About three miles N.W. of Belfast City there are several places where are preserved portions of a system of drift-scratches which has been nearly effaced by a later glaciation. The local rock is a hard quartzitic slate, well adapted both for resisting glacial corrasion and for preserving the scratches under atmospheric exposure. Usually, when traceable, the earlier scratches have been intersected and nearly obliterated by the later, but at one place the earlier ones were perfectly preserved in a depression of the rock at the southeastern base of a little ledge or steep escarpment barely from half an inch to two inches in height. The line or front of the little precipice happened to lie parallel with the earlier ice-movement and hence offered no resistance to it. The gentle depression was glaciated S. 10° E. to the very base of the scarp. But when the direction of the ice-flow changed to S. 30° to 35° E., then the steep scarp protected a portion of the depression, and a strip from two to eight inches wide was left in its lee wholly untouched by the later glaciation. Over all the rest of a rock exposure fifteen feet in diameter, the later glaciation had wholly effaced the earlier.

It thus appears, that at one time, the high hills of Hope, Lincolnville and Camden (the "Camden Hills" of the mariners) formed a great barrier and forced the ice to flow around them both to the east and the west. In doing this they were aided by the topographical features of the country lying to the north of them, where is a great gently rolling plain. During Champlain time, an arm of the sea extended up the St. George Valley to Searsmont and thence eastward through Belmont and Morrill to Belfast. The average elevation of the country lying between the St. George Valley at Searsmont and the head of Belfast Bay is rather less than 200 feet. Megunticook and its associated peaks rise from 600 to 1000 feet above this plain. When the ice was thickest the flow was directly over these hills, but when the thickness was 1000 feet or less, this would manifestly be impossible. My exploration reached only the northern and western bases of these hills, and a thorough exploration will be needed in order to trace all the deflections of ice-movement which would naturally take place in so uneven a country. A single item of detail will be referred to in closing. As has been stated above, in the southern part of Belmont the flow was southwestward down the St. George Valley, rather than south or southeastward, though the latter route presents a more favorable slope through a somewhat level valley from one to two miles wide. No lateral moraines are found near the north ends of the valleys which lie between the high hills of Hope, Camden, etc. Apparently, then, these valleys were filled with embayed ice. That is, instead of a series of local glaciers penetrating the hilly region, the valleys were filled with stationary ice, at least it was stationary enough to unite with the hills to form a great barrier which caused the main ice-flow to take place around the eastern and western bases of this system of hills. It will be proper to add that the direction of the drift-scratches in the vicinity of Belfast bears a significant relation to the glacial gravels of the region. Five systems of these gravels converge to Belfast Bay, all roughly parallel with the glaciation. These deposits are in several respects different from the other Kame or Osar gravels of Maine which I have heretofore described, at least from those of the interior of the State where no evident signs of local ice-movements have been discovered.

Colorado College, Oct. 29, 1884.

ART. XXIII.—*Successional relations of the species in the French Old-Tertiary;* by OTTO MEYER, Ph.D.

IN a former article* I have shown that in the American Old-Tertiary formation many species can be traced along through the succeeding strata, and that the similar succeeding forms are apparently connected by descent. In the second part of this article† I said that the same phenomenon is manifest also in the French Old Tertiary formation. The following table may illustrate this. For the principles which have guided me in making out this genealogy, I refer to this Journal, vol. xxix, p. 458. In preparing it I have used only the material in my own collection, and for this reason the table is very incomplete. Naturalists with more material from French localities than I have may complete it and perhaps correct it in some points. In comparisons like these I think it best to avoid the use of descriptions and figures.

The determinations of the species enumerated in the table were made by Mr. Cossmann, in Paris; and I have adhered to his determinations, even where I am of different opinion; in many cases I would not make specific differences where they are made by Deshayes. My purpose here is only to call attention to the fact that evolution is illustrated also by the species of the French Old-Tertiary. Mr. Cossmann is now occupied with a revision of the species of Deshayes.

	LOWER EOCENE.	MIDDLE EOCENE.	UPPER EOCENE.	OLIGOCENE.
		Lamellibranchiata.		
1		Ostrea flabellula *Lam.*	Ostrea cubitus *Desh.*	
2	Ostrea angusta *Desh.*	Ostrea cyathula *Lam.*	Ostrea cucullaris *Lam.*	
3	Anomia primæva *Desh.*	Anomia tenuistriata *Desh.*		
4	Arca modioliformis *Desh.*	Arca modioliformis *Desh.*	Arca Rigaultiana *Desh.*	
5		Arca condita *Desh*	Arca planicosta *Desh.*	
6	Pectunculus polymorphus *Desh.*	Pectunculus pulvinatus *Lam.*		
7	Pectunculus terebratularis *Lam.*			Pectunculus obovatus *Lam.*
8	Limopsis lentiformis *Desh.*	Limopsis granulata *Lam.* sp.		
9		Trigonocœlia deltoidea *Lam.*	Trigonocœlia media *Desh.*	
10		Nucula Parisiensis *Desh.*	Nucula lunulata *Nyst.*	
11	Cardita planicosta *Lam.*	Cardita planicosta *Lam.*		
12	Cardita Prevosti *Desh.*	Cardita asperula *Desh.*		
13	Cardita imbricata *Lam.*	Cardita imbricata *Lam.*	Cardita propinqua *Desh.*	
14		Crassatella compressa *Desh.*	Crassatella rostralis *Desh.*	

* This Journal, xxix, 457–468, 1885. † Ibid. xxx, 66.

	LOWER EOCENE.	MIDDLE EOCENE.	UPPER EOCENE.	OLIGOCENE.
15	Lucina discors *Desh.*		Lucina Ermenonvillensis *d'Orb.*	
16	Lucina proxima *Desh.*	Lucina Saxorum *Lam.*		
17		Lucina elegans *Defr.*	Lucina elegans *Defr.*	
18	Diplodonta duplicata *Desh.*		Diplodonta elliptica *Desh.*	
19		Cardium obliquum *Lam.*	Cardium Bouei *Desh.*	
20	Cardium hybridum *Desh.*	Cardium porulosum *Lam.*		
21		Cardium granulosum *Lam.*		Cardium scobinula* *Mer.*
22	Cytherea proxima *Desh.*		Cytherea striatula *Desh.*	
23	Cytherea fastidiosa *Desh.*			Cytherea incrassata *Sow.*
24		Cytherea semisulcata *Lam.*†	Cytherea polita *Lam.*†	
25		Cytherea analoga *Desh.*	Cytherea cuneata *Desh.*	
26	Tellina pseudorostralis *d'Orb.*	Tellina rostralis *Lam.*		
27		Tellina donacialis *Lam.*	Tellina parilis *Desh.*	
28	Tellina decorata *Watel.*		Tellina symmetrica *Desh.*	
29	Mactra Levesquei *d'Orb.*	Mactra semisulcata *Lam.*		
30	Corbula gallicula *Desh.*	Corbula gallica *Desh.*		
31	Corbula striatina *Desh.*	Corbula Lamarcki *Desh.*		
32	Corbula muricina *Lêvesq.*	Corbula minuta *Desh.*		
33	Corbulomya seminulum *Desh.*		Corbulomya complanata *Sow.*	
		Glossophora.		
34	Delphinula turbinata *Desh.*	Delphinula turbinoides *Lam.*	Delphinula turbinoides *Lam.*	
35	Bifrontia Laudunensis *Desh.*	Bifrontia serrata *Desh.*		
36	Turritella Dixoni *Desh.*	Turritella carinifera *Desh.*	Turritella copiosa *Desh.*	
37	Calyptræa Suessoniensis *Desh.*	Calyptræa lamellosa *Desh.*		Calyptræa labellata *Desh.*
38		Hipponix elegans *Desh.*	Hipponyx patelloides *Desh.*	
39	Natica semipatula *Desh.*	Natica patula *Desh.*		
40	Natica intermedia *Desh.*	Natica depressa *Desh.*		
41		Natica abscondita *Desh.*	Natica abscondita *Desh.*	
42		Natica epiglottina *Lam.*	Natica Noæ *d'Orb.*	
43	Natica separata *Desh.*	Natica semiclausa *Desh.*		
44		Natica acuta *Desh.*	Natica ponderosa *Desh.*	
45	Diastoma variculosum *Desh.*	Diastoma costellatum *Lam.*	Diastoma interruptum *Desh.*	
46		Pyramidella terebellata *Fér.*	Pyramidella inaspecta *Desh.*	
47	Odostomia turbonilloides *Desh.*	Odostomia turbonilloides *Desh.*		Odostomia obesula *Desh.*
48	Turbonilla polygyrata *Desh.*	Turbonilla fragilis *Desh.*		
49		Pseudomelania hordana *Lam.* sp.	Pseudomelania hordana *Lam.* sp.	
50		Pseudomelania lactea *Lam.*		Pseudomelania semidecussata *Lam.*

* Compare the specimens of the Mayence Basin.

† If these two species occur near each other, they must have a very near common ancestor.

	LOWER EOCENE.	MIDDLE EOCENE.	UPPER EOCENE.	OLIGOCENE.
51	Cerithium breviculum *Desh.*			Cerithium Boblayi *Desh.*
52		Cerithium echidnoides *Lam.*	Cerithium pleurotomoides *Lam.*	
53	Cerithium subacutum *d'Orb.*		Cerithium Bouei *Desh.*	
54	Cerithium involutum *Lam.*	Cerithium conoideum *Lam.*	Cerithium Cordieri *Desh.*	
55	Cerithium plicatulum *Desh.*			Cerithium limula *Desh.*
56	Cerithium deceptor *Desh.*	Cerithium perforatum *Lam.*		
57	Cerithium turbinatum *Desh.*	Cerithium interruptum *Lam.*		
58			Cerithium mixtum *Desh.*	Cerithium conjunctum *Desh.*
59		Rostellaria fissurella *Lam.*	Rostellaria lubrosa *Sow.*	
60			Buccinum sabrandei *d'Orb.*	Buccinum Gottardi *Nyst.*
61		Marginella crassula *Desh.*	Marginella Edwardsi *Desh.*	
62		Marginella ovulata *Lam.*		Marginella Stampinensis *St.-M.*
63		Mitra cancellina *Lam.*		Mitra perminuta *Braun.*
64		Voluta spinosa *Lam.*	Voluta depauperata *Desh.*	
65		Oliva nitidula *Desh.*	Oliva Baumontiana *Lam.*	
66		Ancillaria buccinoides *Lam.*	Ancillaria obesula *Desh.*	
67	Ringicula minor *Desh.*	Ringicula ringens *Lam.* sp.	Ringicula ringens *Lam.* (var. Meyer.)	

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On a new general Method for the Determination of Nitrogen.*—The determination of nitrogen in nitro- and azo-compounds is effected by the addition of some reducing material to the soda-lime, by which the nitrogen is yielded as ammonia. Tamm-Guyard proposed sodium acetate, Ruffler, sodium thiosulphate, and Goldberg, stannous sulphide and sulphur. All these are somewhat unsatisfactory as general methods, and Arnold has experimented with a view to improve them. He first used a mixture of acetate and thiosulphate, and afterward two or three per cent of amorphous phosphorus; but without advantage. He then substituted sodium formate for the acetate, using it in connection with sodium thiosulphate and soda lime, and obtained excellent results. The combustion tubes used were from 10–12

mm. diameter. The ammonia was determined by means of a normal hydrochloric acid, standardized by analysis and by pure sodium carbonate. For titering back, one-third normal ammonia solution was used, controlled by oxalic acid. Fluorescein was employed as an indicator, since it can be used in daylight and with artificial light as well. The results of the mixture used in four different proportions, are given. In A, equal parts soda-lime, sodium thiosulphate and sodium formate were employed. In B, two parts soda-lime, one part sodium formate and 10 per cent of sulphur. In C, equal parts soda-lime and formate, with ten per cent sulphur. And in D, two parts thiosulphate, mixed with one part of soda-lime and sodium formate mixed. The latter mixture is preferred by the author who suggests the following points to be observed: (1) A canal in the mass in the front of the tube should be avoided. In filling the tube, the finely pulverized mixture containing the substance is first introduced, the tube held vertically and jarred to compact the mass. Then a mixture (either A or D) is placed in it, and then coarsely pulverized soda-lime. (2) The mass in front must have a definite length. In the analyses given this length was 15 cm., but 20 cm. would be better. (3) The combustion in a 45 cm. tube requires about one hour, a bubble of gas per second passing through the receiver. (4) The process is a failure if the acid becomes turbid or acquires more than a yellow color. (5) Not more than 0·5 gram, or, if the substance contains over 20 per cent nitrogen, not more than 0·3 gram is to be used. (6) The mixture containing the substance should occupy 12 to 15 cm., the mass in front of this 15 to 20 cm. and the soda-lime 5 to 10 cm. of the tube. By this method potassium nitrate gave 13·85, 13·77 per cent N, instead of 13·86; sodium nitroprusside 28·20 and 28·15, instead of 28·13; trinitromethyl-toluidine 21·90, cal. 21·87; ethyl nitrocinnamate 6·01, cal. 6·33; nitrosalicylic acid 7·56, cal. 7·65; strychnine nitrate, 10·60, cal. 10·60; morphine 4·50, cal. 4·62; *m*-nitrocinnamic aldehyde 7·89, cal. 7·91.—*Ber. Berl. Chem. Ges.*, xviii, 806, April, 1885. G. F. B.

2. *On a new method of determining the Heat of combustion of Carbon and of Organic compounds.*—Two difficulties attend the determination of the heat of combustion of carbon and its compounds: one arises from the length of time required, the other from the incompleteness of the oxidation. Berthelot and Vieille have obviated these difficulties by effecting the combustion in oxygen compressed to about seven atmospheres, in a calorimetric bomb; using a weight of combustible such that the oxygen consumed by it does not exceed 30 to 40 per cent of the whole quantity. The ignition is accomplished by a platinum wire heated to redness by electricity and is completed in a few seconds, sometimes with the characteristic noise given by an explosion in a closed vessel. The entire operation does not require more than three or four minutes and is applicable to all substances whose vapor tension at the ordinary temperature is incon-

siderable. The completeness of the combustion was verified by an examination of the products. The heat of combustion thus obtained is of course the heat at constant volume. For carbon this is the same value as that at constant pressure, since the carbon dioxide formed replaces the oxygen volume for volume. For hydrogen compounds, however, the usual corrections are necessary for the condensation of the water vapor. When cellulose in the form of cotton was burned in this way, the ash being deducted, one gram gave 4·2 calories; or one equivalent (162 grams) 680·4 calories. The heat of combustion calculated at constant pressure, the water being in the liquid state, is 681·8 calories. Comparing this value with that of the carbon contained in the cellulose (referred to diamond) 564 calories, it appears that that of the cellulose is in excess 117·8 calories, or about one-fifth. It follows from this that the hydrates of carbon, so-called, contain an excess of energy above that given by the carbon and water which their decomposition would furnish. The authors call attention to the fact that this is also true of incompletely burned charcoal, as for example the *charbon roux* used in making gunpowder; and hence that the energy of a sample of gunpowder due to the carbon it contains cannot be accurately calculated from its percentage composition.—*Bull. Soc. Ch.*, II, xliii, 262, March, 1885. G. F. B.

3. *On a new absorbing agent for Oxygen.*—VON DER PFORDTEN has suggested the use of chromous chloride as an absorbing substance for oxygen. To prepare it, chromic acid is heated with hydrogen chloride until the liquid is perfectly free from chlorine. The green solution of chromic chloride is then reduced by means of zinc, to the blue solution of chromous chloride. This is then poured, in an atmosphere of carbon dioxide, into a saturated solution of sodium acetate, whereby a red precipitate of chromous acetate is produced, which is washed by decantation with carbonic acid water. The precipitate is quite permanent and may be preserved for some time without change in closed vessels filled with carbon dioxide. For use it is converted into chromous chloride by placing it in a test-glass closed by a rubber stopper bored with three holes. Through one of these a funnel passes, furnished with a cock. The others have entrance and exit tubes as usual. When the air is displaced by carbon dioxide, the acetate is placed in the glass in excess, the gas stream continued and hydrochloric acid allowed to flow in and convert it into a blue solution of chromous chloride. When a gas containing oxygen is passed through it the color changes from blue to green. Experiments have shown that an alkaline solution of pyrogallol is entirely unaffected by gas which has passed through the solution of chromous chloride. Hence, hydrogen, carbon dioxide, hydrogen sulphide and the like may be freed from oxygen in this manner.—*Liebig Ann.*, ccviii, 112, March, 1885. G. F. B.

4. *On a Simple Quantitative method of separating Selenium and Tellurium.*—DIVERS and SHIMOSÉ have proposed a method of separating selenium and tellurium founded on their behavior toward sulphurous oxide in presence of sulphuric acid, no hydrochloric acid being present. A mixture of the two substances in the free state is heated with concentrated sulphuric acid in a covered beaker until it is entirely oxidized and converted into a colorless solution, with a deposit perhaps, of tellurium sulphate. A moderately strong solution of sulphurous oxide is then gradually added until the volume is increased at least five-fold, and the solution is heated on the sand bath for some time. The precipitated selenium soon darkens in color and becomes dense without aggregating into masses difficult to wash as it does when heated with hydrochloric acid. After dilution, the solution is filtered through a tared filter, and the precipitate washed, dried and weighed. The tellurium is precipitated from the filtrate by heating with hydrochloric acid and adding sulphurous oxide in the usual way. To test the process, 0·3115 gram Te and 0·3867 gram Se were mixed, dissolved and separated. The tellurium weighed 0·3107 and the selenium 0·3865 gram. In a second experiment the quantities taken were 0·2515 gram Te and 0·3395 gram Se; the quantities recovered were 0·2513 gram Te and 0·3395 gram Se.—*J. Chem. Soc.*, xlvii, 439, June, 1885. G. F. B.

5. *On the Illuminating power of Ethane and Propane.*—P. F. FRANKLAND has been engaged for some time in determining the illuminating power of the hydrocarbons supposed to be present in coal gas. He had previously shown that the illuminating power of ethylene calculated for a consumption of five cubic feet per hour from a referee's burner, is 68·5 candles; and that of benzene was estimated to be six times as great. He has now experimented with ethane and propane, the second and third terms of the paraffin series. The ethane was prepared by the action of the copper-zinc couple upon ethyl iodide in presence of alcohol, and was made to pass slowly, first through bromine and water and then through a solution of sodium hydrate and finally over slaked lime. The product showed on analysis 97·88 per cent of ethane. Burned from a referee's burner and comparison being made with the Methoen standard and with standard candles, the mean result (corrected for five cu. ft. of gas) was 34·99 candles. The propane was prepared by the action of zinc on isopropyl iodide, being passed first through a scrubber containing zinc-copper, then through alcoholic soda, bromine and water, sodium hydrate and slaked lime. The gas contained 95 per cent of propane, and, burned in the same way as ethane, gave 53·91 candles as its illuminating power. Hence this value appears to be proportional to the number of carbon atoms in the molecule.—*J. Chem. Soc.*, xlvii, 235, April, 1885. G. F. B.

6. *On the Illuminating power of Methane.*—WRIGHT has made a series of experiments to determine the illuminating power of pure methane. The gas was prepared by Gladstone & Tribe's

method, by the action of the copper-zinc couple on methyl iodide, and was passed through three copper tubes 12 inches long, 1·5 inches in diameter, placed horizontally, fitted with corks and connecting glass tubes and filled with copper-zinc and as much alcohol as they would contain in this position, to remove the vapor of the iodide; and then through a glass tube containing beads moistened with sulphuric acid to absorb the alcohol vapor. The methane was collected in a small gas-holder 5 liters in capacity graduated to 5 c. c. and passed thence to the photometer, which was furnished with a Methoen standard giving a light of two candles when supplied with eighteen candle gas. The methane was burned in a London argand with a six-inch chimney, provided at top with a metal cap capable of vertical adjustment by a screw. This cap is to limit the air supply and is necessary at low rates of consumption. Two specimens of marsh gas were tested. In the first 2·78 cubic feet (corrected) were consumed per hour and gave a light of 2·9 candles; or 5·2 candles per cubic foot per hour. In the second 4·56 cubic feet were consumed per hour, giving 4·6 candles; or 5·15 candles per cubic foot per hour.—*J. Chem. Soc.*, xlvii, 200, April, 1885. G. F. B.

7. *On Toughened Filter-papers.*—FRANCIS has observed that filter paper is remarkably toughened by the action of nitric acid, the product being pervious to liquids and quite different from parchment paper made with sulphuric acid. By immersing the paper in, or even by moistening it with, nitric acid of density 1·42 and subsequent washing, the result is attained. The toughened paper can be used like ordinary paper, filtration being but little retarded. It may be rubbed without injury, like a piece of cloth. To compare its strength with that of ordinary paper, a strip of Swedish paper 25mm. wide was made into a loop and the ends held in a vertical clamp. A glass rod was passed through the loop and from its ends a scale pan was suspended. The paper after wetting broke with a weight of 100 to 150 grams, while the same paper after the above toughening treatment supported 1·5 kilograms. Filters of this paper are very useful with the filter pump, the apex only being toughened. When treated the paper contracts slightly, filters 11·5 cm. in diameter being diminished to 10·4 cm. The ash was reduced from 0·0026 to 0·0011 gram. The treated paper contains no nitrogen, and it suffers only a slight decrease of weight.—*J. Chem. Soc.*, xlvii, 183, April, 1885. G. F. B.

8. *On a Crystallized Tricupric sulphate.*—The basic copper sulphates hitherto known have been in general amorphous powders of doubtful purity. SHENSTONE has now prepared a crystalline basic copper sulphate artificially. In the course of experiments on the solubility of salts in water at high temperatures, he observed in the tubes in which copper sulphate and water had been heated deposits of bright green crystals. By heating the crystals of ordinary copper sulphate $CuSO_4(H_2O)_5$ at about 200° for a few hours in tubes containing a few grams of the salt, he was

able to prepare the new salt in larger quantity. By draining off the mother liquor while still hot, and washing the solid part of the contents of the tubes with water, a green product consisting of crystalline fragments was obtained, about 0·25 gram being yielded for each tube. On analysis, the salt afforded 26·98 per cent of SO_4 and 54·49 of Cu; giving the formula of a tricupric sulphate $CuSO_4\ (CuO)_2(H_2O)_2$ or $Cu_3SO_6(H_2O)_2$, which requires 53·75 per cent Cu and 27·07 SO_4. The salt is permanent when heated in the air to 190°, is insoluble in water, soluble in dilute sulphuric acid. The crystalline form was determined by Miers to be orthorhombic.—*J. Chem. Soc.*, xlvii, 375, June, 1885.

G. F. B.

9. *On the Molecular Weight of liquid Water.*—Thomsen has called attention to the fact that the conclusion reached by Raoult in his researches on the freezing point of saline solutions, that water possesses, in the condition of liquid, twice the molecular weight which it has in the condition of vapor, coincides with the conclusion to which he himself had come from his investigations on the constitution of hydrated salts. In his thermochemical researches, Thomsen says: A glance at the table of heat of hydration of hydrated salts shows that the water molecules enter often in pairs with the same heat-change; a fact explicable either by supposing that the molecules of water are symmetrically placed in the molecule of the salt, or, and perhaps more probably, that the molecular weight of liquid water is twice that of water vapor. The similarity of these conclusions, from widely different fields of investigation, is noteworthy.—*Ber. Berl. Chem. Ges.*, xviii, 1088, April, 1885. G. F. B.

II. Geology and Mineralogy.

1. *The volcanic nature of a Pacific island not an argument for little or no subsidence.*—In the remarks on this point in § 13 (p. 100) of my paper on the Origin of coral reefs and islands, I refer to the great depths found in the ocean by soundings in the vicinity of Hawaii, and speak of the facts as favoring the idea of more subsidence about that southeastern end of the group than along the northwestern, although the latter is the coral island end. Another example of similar character, but more striking, is afforded by the region of the Ladrones. This north-and-south range of islands has its largest volcanic islands in the southern part, and dwindles in the opposite direction to islands which are little more than tufa cones; and 200 miles south of Guam, the largest island, the Challenger found a depth of 4,475 fathoms (26,850 feet), one of the deepest regions of the ocean. It hence may be that Guam, like Hawaii, is a large island, not because of small subsidence, but because of continued eruptions that made it large in spite of the sinking that was in progress. The question arises how far the depths in these particular cases are due to the undermining effects of volcanic eruption. There are coral

islands both northeast of the deep region, near Guam, and also of large size, to the southwest and southeast, not three degrees off; the former, those of an extension of the Pelew range, and the latter, islands of the Caroline Archipelago. J. D. DANA.

2. *The Physical Features of Scotland*, by Professor JAMES GEIKIE.—This excellent paper is illustrated by an orographic map of Scotland which is necessary to its full appreciation. The author condemns the "statement so frequently repeated in class-books and manuals of geography, that the mountains of Scotland consist of three (some say five) ranges." He observes that it is divided into three parts "the Highlands," the Central Lowlands and the Southern Uplands; and defines the Lowlands (which are the southern limits of the Highlands and northern of the Southern Uplands) as extending from Stoneham in a nearly straight southwest direction along the northern outskirts of Strathmore to Glen Artney, and thence through the lower reaches of Loch Lomond to the Firth of Clyde at Kilcreggan. The mountains "are merely monuments of denudation," "relics of elevated plateaus which have been deeply furrowed and trenched by running water and other agents of erosion." The straightness of the southern boundary of the Highlands "is due to the fact that it coincides with a great line of fracture of the earth's crust; on the north side are hard and tough slates and schists, on the south sandstone strata prevail." Looking across Strathmore from the Sidlaws or the Ochils, the mountains seem to spring suddenly from the low grounds at their base, and to extend northeast and southwest as a great wall-like rampart. "The mean height of the Highlands above the sea is probably not less than 1500 feet;" peaks rise to a height of nearly 3500 feet. Any wide tract of this Highland region "viewed from a commanding position looks like a tumbled ocean in which the waves appear to be moving in all directions. But the masses are broad, generally round-shouldered, often somewhat flat-topped, with no great disparity of height among the dominant points. The relationship and the forms are the result of denudation; the mountains are "monuments of erosion,"—they are the wreck of an old table land—the upper surface and original inclination of which are approximately indicated by the summits of the various mountain masses and the directions of the principal water-flows.

A similar general conclusion is drawn from the Southern Uplands; that "the area is simply an old table-land furrowed into ravine and valley by the operation of the various agents of erosion."

In view of such facts it is not surprising that Scotch valleys and mountains should have given to Hutton right ideas on mountain sculpturing, and have led him to the opinion he brings out in his memoir on the Theory of the Earth (R. Soc. Edinb. 1778, and 8vo, 1795), that mountain forms are due to subaerial denudation after an elevation of a region by the earth's central heat.

Professor Geikie illustrates the subject with much interesting detail, and discusses also other points connected with the physical history of the region. He speaks also of the shaded sheets (maps) issued by the Ordnance Survey as reproducing with marvelous skill the surface features of the country even to minor points of glacial origin; and asks in conclusion: "With such admirable cartographical work before them, how long will intelligent teachers continue to tolerate those antiquated monstrosities which so often do duty as wall-maps in their school-rooms."

3. *Pennsylvania Geological Survey.* — This Survey has recently issued the following reports:

Report on the Coal Mines of the Monongahela River Region, from the West Virginia State-line to Pittsburgh, including the mines on the Lower Youghiogheny River (No. K 4), by J. Sutton Wall. Part I. Description of the Mines, 232 pp., 8vo, with maps and plates. Harrisburg, 1884.

Report on Perry County (No. F 2), by E. W. Claypole. 438 pp., 8vo, with many plates, maps and sections—a geological report preliminary to an account of the Paleontology. Harrisburg, 1885.

Atlas of the Northern Anthracite field, Part I (No. A A).

Maps and charts of the Report on Cameron, Elk and Forest Counties (No. R R).

Grand Atlas, Division III. Petroleum and Bituminous Coal Fields, No. I.—The first part issued of this "Grand Atlas" of the Geological Survey of Pennsylvania was noticed on page 340 of the last volume of this Journal. The Atlas is to appear in five Divisions. Division I is to contain the geological maps of the counties constructed on a scale of two miles to the inch, and will consist of 49 sheets, covering fifty-six counties; Division II, the sheets of the Anthracite Survey, including mine sheets on a scale of 800 feet to the inch, topographical sheets on a scale of 1600 feet to the inch, cross-section sheets and columnar-section sheets, besides miscellaneous sheets; Division III, of which Part I has recently been issued under the above-mentioned title; Division IV, topographical maps from the surveys of A. E. Lehman, of the South Mountain, in Adams, Franklin and Cumberland Counties, and others of the Great Valley, between Easton and Reading, from the surveys of Messrs. d'Invilliers, Berlin and Clarke, on a scale of 1600 feet to the inch, with the contour lines 10 or 20 feet apart; Division V, the geological maps and sections relating principally to the Silurian and Devonian formations in Central Pennsylvania, and cross-sections of the Philadelphia rocks.

The maps on the Petroleum and Bituminous coal fields, making the first part of Division III, contain topographical maps, plans, cross-sections, and others, illustrating the geological structure of the oil region, the distribution of the areas, the underlying rocks down to the oil-producing sand beds in a large number of sections obtained in boring for wells, and are thus full of instruction. The general map of the oil-region, prepared by C. A. Ashburner, which comprises Western Pennsylvania, shows that the many areas lie in a belt of country 20 to 25 miles broad, extending in a northeast and southwest direction from Allegheny County, New York, to Beaver County, Pa. The belt to the northward bends to west-northwest, and thus exhibits its parallelism to the mountain system lying to the southeastward.

An announcement accompanies the recent reports stating that they may be obtained by purchase of Mr. J. S. Africa, Secretary of Internal affairs, Harrisburg, Pa. The State has directed that they shall "be furnished at cost to all applicants."

The legislature of Pennsylvania at its late session appropriated $50,000 to continue the State Geological Survey for the years 1885 and 1886, and the Governor has signed the bill. The survey was begun in 1874, and the work remaining to be done is chiefly in the anthracite coal regions. A summary of the whole in a final report of one or two volumes by Professor J. P. Lesley, the chief State Geologist, a most important work is intended to complete the set of reports.

4. *Geological Survey of New Jersey*: *Report for* 1884; G. H. Cook, State Geologist.—Fine views of the remarkable locality of columnar trap in O'Rourke's Quarry, on Mount Pleasant Avenue, Orange, make a frontispiece to this year's Report. The most singular feature is the obliquity of the columns in divergent directions about the center of the quarry while nearly vertical elsewhere. The nearly vertical columns are large, some 6 or 7 feet in diameter while the oblique are small. The columnar surface exposed has a length of about 700 feet and a height of 20 to 100 feet; and underneath it, 6 to 8 feet below the exposed surface, there lies the red sandstone. The outer surface of the columns is horizontally banded, but there is no regular cross-fracturing. Professor Cook suggests that after the main extrusion of trap, there may have been a later outflow along the central part where the small divergent columns occur. The locality may be reached by going out on Pleasant Avenue in Orange about a mile from the railroad station, and then following the quarry wagon road on the left for 400 or 500 feet.

An interesting exhibition of a buried forest is reported as recently opened at the Earnst Clay pits in South Amboy. The burial must have taken place since the settlement of the country, not more than 280 years ago; the place until recently has been under a swamp and a forest of cedars. "The ground which was formerly enough above the sea-level to sustain a growth of upland timber, is now so low that every high tide could cover it with salt water."

Artesian wells have proved to be a success along the low coast region of Southern New Jersey. Abundant pure water has been thus obtained at Ocean Grove, Asbury Park, Red Bank, Ocean Beach, Lakewood, Marlton and other points. Each passes down to the sandy stratum under the Lower Marl bed of the Cretaceous. The wells at Red Bank and Marlton have a depth between

80 and 90 feet. That at Ocean Beach, Monmouth County, about 400 feet from the ocean, is 485 feet deep, is 3 inches in diameter inside of the wrought iron casing pipe, and soon after completion yielded 36,000 gallons a day; that at Lakewood is 475 feet deep. The Lakewood water contains but 6½ grains of solid matter to the gallon.

An artesian well at Newark went down through 110 feet of earth and 505 feet of red sandstone; the bore was 8 inches in diameter and the daily yield 800,000 gallons; the temperature 55½° F. The amount of mineral matter is about 150 grains to the gallon, over two-thirds of which is calcium sulphate (gypsum). An analysis, made in 1879, afforded sodium sulphate 15·94, magnesium sulphate 25·87, calcium sulphate 106·98, magnesium carbonate 1·55, sodium chloride 2·47 = 152·81. In a trial in 1882 the amount of solid matter was found to be 151·79 grains per gallon, and in 1884 148·83 grains.

The above are a few of the facts to be found in the Report.

5. *Contributions to the Knowledge of the Older Mesozoic Flora of Virginia*, by W. M. FONTAINE. 144 pp. 4to, with 54 plates. Washington, 1883. U. S. Geological Survey. (Received in June, 1885.)—Professor Fontaine, after a brief review of the beds in Virginia, describes the fossil plants in detail. His specimens were obtained from different openings on the Richmond belt, and from the Cumberland belt, about 30 miles west of the former. The plants were found in the layers of shale and sandstone adjoining the coal beds,—not far from the middle of the sandstone formation. In the Richmond belt there appear to be 500 to 600 feet of sandstone above and below the coal beds.

Forty-two species are described and figured. Half of them have no distinct relations to European species. Of the rest, twelve are decidedly Rhætic in their relations, and four identical with Rhætic species, while only four are Triassic in character and three of these are as closely Rhætic. Besides, eight have Jurassic relations. Professor Fontaine concludes, therefore, that the beds are probably of Rhætic age rather than Triassic, that is, of the epoch between the Triassic and the Lias. The descriptions and figures of the plants of the North Carolina Mesozoic described by Professor Emmons are reproduced in the volume and compared with those from Virginia. Professor Emmons's specimens were sought for without success. Of the thirty-nine species identified, fifteen are also Virginia species, and they point to the same age for the beds. The Cycad genus *Ctenophyllum*, of which there are four species, is highly characteristic of the Rhætic Lias. The genera *Acrosticides*, *Laccopteris*, *Cycadites*, *Podozamites* also are Rhætic, or Rhætic and Jurassic. The North Carolina fauna is much richer than the Virginian in Conifers; but this is attributed to drier conditions of growth and not to actual difference in flora.

This new volume from the U. S. Geological Survey is a worthy successor to the excellent Reports which have preceded it.

6. *Syenite and Gabbro in Essex County, Massachusetts.*—Dr. M. E. Wadsworth has an article on this subject in the Geological Magazine for May last. It describes the coast from Salem to a point beyond West Manchester as occupied by a typical reddish or grayish syenite. The granite of the same region, according "to the preponderance of evidence, is the younger rock, unless it is cotemporaneous with the syenite." A gneissoid schist on Woodbury's Point contains "gabbro" in masses—called "irregular dikes"—approximately parallel to the foliation of the schist. The "gabbro" varies from a whitish rock consisting of feldspar with "a few grains of diallage" to one in which the feldspar and diallage are in nearly equal proportions, and in some of it the latter ingredient is in individuals two or three inches long. Zircon-syenite similar to that of Marblehead occurs also on Salem Neck, containing sodalite and elæolite.

7. *Thermal effect of the action of aqueous vapor on feldspathic rock (kaolinization).*—Dr. Carl Barus discusses this question mathematically, after some careful experiments, in a paper published in the School of Mines Quarterly, November, 1884. A definite result was not reached; yet the discussion is one of much interest, as the author states in conclusion, "showing in how far very small increments of temperature, increasing continuously, through infinite time, are accurately measurable;" and "containing the first direct attack upon physical problems of this character, many of which have an important bearing on geological and metallurgical subjects."

8. *New localities of Erythrite;* by Wm. P. Blake. (Communicated.)—There are two localities of *erythrite* in the West which deserve mention. One of these, lately opened near Lovelock's station on the Union Pacific Railway in Nevada, has yielded considerable quantities of nickel and cobalt ore. The cobalt bloom occurs in crusts and aggregations of very small crystals in the seams of a calcareous rock, containing also brilliant brass yellow acicular crystals of millerite. The ore as mined and shipped contains an unusually high percentage of both nickel and cobalt. There are also masses of a black earthy aggregate consisting largely of black oxide of cobalt. These masses do not appear to carry manganese oxide in any appreciable quantity and can not properly be referred to the ores of manganese, as with asbolite, but are rather entitled to a separate place as black oxide of cobalt, for which the name "asbolite" may be retained if the description is amended so as to make the presence of manganese unessential.

The other locality is in Los Angeles County, California, at the Kelsey Mine, Compton, where the erythrite is associated with an ore of silver and of cobalt in dark-colored earthy masses in a gangue of heavy spar. This occurrence was noted in 1881 and is described in the Report of the State Mineralogist of California for 1882, p. 207, and in the fourth report, p. 179.

III. Botany.

1. *A Course of practical instruction in Botany*, by F. O. Bower and S. H. Vines, Part I. (Macmillan & Co., London, 1885.)—This handy book of only 226 pages, is the best introduction in English to the practical investigation of flowering plants and ferns. After giving a chapter on the preparation and effects of reagents for microscopic use, the authors deal with the micro-chemistry and microphysics of the vegetable cell. The student is led along slowly and safely over the least attractive part of the field, and is there shown sound methods for the examination of organs of the higher plants. Nearly all of the methods are described concisely, yet so as to leave no important point untouched. Theoretically it would be better to show a student how to examine any object, and then demand of him direct answers as to what he sees, rather than to tell him what he ought to see. But this plan consumes more time than most beginners have at their command; therefore the "personally conducted" plan is generally most in vogue. The present work tells the student all that he can be reasonably expected to see under the microscope, *if* he is successful in making his sections, etc. Therefore, the student who follows out faithfully all the exercises laid down in this work may be sure of acquiring the essentials of histology, so far as the higher plants are concerned. A second volume, devoted to the plants lower than the ferns and their allies is promised. With Strasburger's German treatise, "Das botanische Practicum," or with this excellent English work as a guide, the student can have no excuse for lack of practical acquaintance with histological manipulation.

G. L. G.

2. *Text-book of Structural and Physiological Botany*, by Otto W. Thomé and Alfred W. Bennett. (Longmans, Green & Co., London, 1885.)—This is the fifth edition of a useful book. The revision brings portions of the treatise fairly down to the eve of publication, and makes it a well proportioned treatise. It is, however, a text-book, in the strictest sense of the term, and in no way supplies the help in practical manipulation which is afforded by the work noticed above. The student preparing for an examination finds that Thomé gives in small compass exactly the information required by the question-papers set in the earlier examinations for the degrees of M.B. and B.Sc. To the general student, the chapters on the geographical distribution of plants and on fossil plants are especially useful. G. L. G.

3. *Le Potager d'un Curieux: Histoire, Culture, et Usages de 100 Plantes comestibles peu connues ou inconnues.* Par A. Pailleux et D. Bois. Paris, Libraire Agricole de la Maison Rustique; pp. 294, 8vo. 1885.—This volume is a reprint from the *Bulletin de la Société Nationale d'Acclimatation.* One of the authors is a councillor in that society, the other is an assistant in the Museum at the Garden of Plants. The now classical volume of Alphonse de Candolle treats of the esculent plants of large

cultivation and investigates their history and origin. This book treats of a hundred less known or only locally known alimentary plants, records the results of trials in cultivating a considerable number of them, indicates those which promise well, and tells us how they may best be cooked. It aims to extend the domain of *l'horticulture potagère* in Europe by introductions from foreign parts of plants which various people regularly use for food, trusting that some of them may prove to be valuable acquisitions. Let us note, as having for us a certain interest, the North American plants which our authors take into consideration. They are *Apios tuberosa*, which was vaunted as a most hopeful substitute for the potato in the days of potato-rot in France, but which, naturally came to nothing, which, indeed, would never have been thought of except for the tradition of its use by the aborigines of New England. *Camassia esculenta*, the *Quamash*, upon the bulbs of which the Indians of Oregon were largely nourished. Introduced into France, these bulbs were pronounced to be a dainty dish; but our author's attempts to cultivate them on a large scale completely failed. *Claytonia perfoliata*, which, as a succedaneum for spinach in summer, is said to furnish "un aliment acceptable." *Melothria pendulà*, the berries of which have been highly recommended for pickles! Naturally our authors do not recommend them. *Phytolacca decandra*, the spring shoots just pushing from the ground, used in the manner of asparagus, so used indeed in some parts of the United States. It seems that of late they have been largely supplied to the Paris market. M. Paillieux reports that some people seem to like them, that he finds them rather tasteless, and that he has eaten them, in small quantity indeed, without any ill effects. Probably the largest use of our Poke in France is for the rich coloring matter of the berries in wine-making. *Psoralea esculenta*, of the Upper Missouri region, the tuberous roots of which, being very farinaceous, were introduced into France, forty years ago, to be a substitute for the failing potato, but which, as might have been expected, proved an utter failure. In fact the contributions of North America to the kitchen-garden (deduction made of *Helianthus tuberosus*) amount to nothing. The authors of this work evince a wonderful hopefulness by asking (in a private communication) to be supplied with the means for making trial of some other Indian food plants, such as *Lewisia*, the tuberous-rooted species of *Callirhoë*, *Balsamorrhiza* and *Peucedanum*, *Valeriana edulis*, etc. They even ask for *Asclepias tuberosa*. This reminds us that—judging from the recollections of boyhood—the vernal shoots of *Asclepias Cornuti*, our common milk-weed, make the best of substitutes for asparagus.

More important plants than these, mainly from tropical or subtropical regions, are reported on in this book, some of them, such as the various kinds of Yam, at full length. The volume is rather a series of reports, than a treatise. The practical and also truly scientific work is Vilmorin's Plantes Potagères. A. G.

4. *Contributions to American Botany,* XII; by SERENO WATSON. Extr. Proc. Am. Acad., xx. Feb. 21, 1885, pp. 324–374, with a full index.—This last particular is one of the good points of Dr. Watson's papers, an extra finish which botanists are thankful for, none the less because they cannot generally expect it. A most important "contribution" indeed, one in which the essential results of very prolonged study and critical toil are condensed into less than thirty pages, is the *History and Revision of the Roses of North America.* The "History" any one can read with interest; the "Synopsis of Species" (18 in number, which European treatment might have quadrupled, and which "the extreme of possible reduction" might condense into nine) presents the botanist with a convenient view of the leading differential characters; then we have, under proper divisions, sufficiently detailed descriptions, habitat, and a particular mention of localities and collectors. Thanks to our botanists and curators, nearly all the principal herbarium-material in the country was in the monographer's hands, and a part of that in the Gray herbarium had previously been examined by Crépin in Belgium. Let us hope that our Roses may now be fairly well and readily understood by our botanists, that the attention which, with such help, they will generally receive may lessen rather than increase the remaining doubts and ambiguities, and that this judicious monograph may do its part in preserving our American rhodology from the fearful state which that of the Old World presents.

The other article of the present contribution consists of "*Descriptions of some New Species of Plants, chiefly from our Western Territories,*" which have recently been brought to light by our various zealous collectors, and which are examined in the course of preparation of the Flora of North America. A few already published plants are mentioned. Among them is *Atamisquea emarginata* of Miers, a rare Capparideous shrub of a peculiar genus, which Miers discovered in the province of Mendoza, in about the same latitude in the southern hemisphere that Mr. Pringle found it in the northern, namely in the northwestern borders of the Mexican State of Sonora. As far as we can see there is no difference between the specimens from these widely disjoined stations, the only two known. Dr. Kellogg's *Œnothera arborea,* which he long ago figured in the Hesperian is taken up and described aright as *Hauya Californica.* It would have been better to follow the general rule of retaining the original specific name, and also to have avoided "Californica." For, although the country which this shrub inhabits was the original California, it is not our California.

Tetracoccus, the only new genus described in this paper is interesting as being the last plant studied and named by Engelmann. It was discovered by Dr. Parry in Lower California, in the winter of 1883, but male flowers and mature fruit were obtained by his young friend, Charles R. Orcutt, a year and a half later. Both sent materials to the Gray Herbarium, and it was supposed that it was left to be published from here. But meanwhile Dr. Parry, the discoverer, thought best to bring out the genus in Southern California, in Orcutt's "Western Scientist;" and so *Tetracoccus monoicus*, Engelm. and Parry, was published while the *Tetracoccus* (Engelmann) *Engelmanni*, Watson, was here in press.

It should perhaps be here noted that there is a somewhat earlier paper in the same (twentieth) volume of the Proceedings of the American Academy of Arts and Sciences, by the present writer (pp. 257–310), comprising *A Revision of some Borragineous Genera* (*Omphalodes, Krynitzkia, Plagiobotrys*), *Notes on some American Species of Utricularia, New Genera of Arizona, California*, etc., and *Gamopetalæ Miscellaneæ*. A large share of the latter, as also of Dr. Watson's Miscellaneæ, are from the interesting collections made last year by Mr. Pringle, with no small hardship and suffering, along the frontiers between Arizona and Sonora. This year Mr. Pringle, with his usual zeal and with excellent prospects, undertook the exploration of the State of Chihuahua; but when about to enter into the most alluring yet hazardous field, that of the Sierra Madre Mountains, he was prostrated by a return of last year's fever, and has been obliged to return home for recuperation. Let us hope that the air of his native Vermont will soon restore him to wonted health, and to the botanical explorations for which he is remarkably fitted, and in which he bears the palm. A. G.

5. *Talks Afield about Plants and the Science of Plants*, by L. H. Bailey, Jr. (Boston: Houghton, Mifflin & Co., 1885, pp. 173, 12mo), are pleasant talks, well adapted to inspire an interest in plants and botany, sensible and instructive in what is said, equally sensible in the omission of the technical and recondite matters which are too commonly crowded into books of this sort. A. G.

IV. Miscellaneous Scientific Intelligence.

1. *Report of the Secretary of the Smithsonian Institution*, Professor Spencer F. Baird, *for the year* 1884. 98 pp., 8vo.— The Smithsonian Institution has become a great center for the collection and diffusion of knowledge. Considering the amount of labor and investigation carried forward, the expenditure for 1884 appears very small—43,613.36 dollars out of the total income, which was 68,994.20 dollars. The explorations promoted by the institution, which have in past time done more for "increasing our knowledge of the physical condition and natural history of various parts of the globe, especially on the continent of America," than any other single agency in the land, were carried on in 1884 partly in coöperation with the U. S. Signal Service, the Geological Survey and the Fish Commission, in Greenland, Labrador, Alaska, British Columbia and Washington Territory, Arizona, New Mexico, Mexico, Central America, and other regions. The quarto volume of the Smithsonian Contributions to Knowledge, published during the year, is that of Dr. C. Rau on

"Prehistoric Fishing in Europe and North America," a work already noticed in this Journal. Not the least of the benefits conferred by the Smithson gift is the free system of international exchanges of scientific publications carried on by it. During 1884 over 65,000 packages were thus distributed, of an aggregate weight of nearly 154,000 pounds. The institution has also been a chief reliance in State and International Exhibitions and has brought great credit to the country and done it great service by its labors in this direction.

Further, the care and enlargement of the National Museum, although supported by appropriations from Congress, have become a prominent object with the institution. The grand museum is an expression of the efficiency of the present secretary, Professor Baird.

2. *American Association for the Advancement of Science.*—The arrangements made by the local committee for the coming meeting are noticed on page 87. For all matters pertaining to membership, papers, and business of the Association the permanent Secretary, F. W. Putnam, should be addressed, at Salem to August 20, and at Ann Arbor, Michigan, from August 20 to September 2.

The President of the session is H. A. Newton of New Haven, Ct.; the Vice Presidents, J. H. Van Vleck of Middletown, Ct., in Mathematics and Astronomy; C. F. Brackett of Princeton, N. J., in Physics; W. R. Nichols of Boston, Mass.; J. B. Webb of Ithaca, in Mechanical Science; B. G. Wilder of Ithaca, in Biology; S. H. Gager of Ithaca, in Histology and Microscopy; W. H. Dall of Washington, in Anthropology; and E. Atkinson of Boston in Economic Science and Statistics.

3. *Report on the Museums of America and Canada;* by V. Ball. 34 pp. 8vo.—Mr. Ball, formerly connected with the geological Survey of India, is now Director of the Science and Art Museum, Dublin. His observations on American Museums were made during his visit to the country last summer, and with special reference to the improvement of the arrangements in the museum under his charge, and not without some profit, as he states in his report.

The Sun: A familiar description of his phenomena, by the Rev. Thomas W. Webb. 80 pp. 12mo. New York, 1885. (Industrial Publication Company.)

An Introduction to Practical Chemistry, including Analysis, by John E. Bowman; edited by Charles L. Bloxam. Eighth edition. 248 pp. 12mo. Philadelphia, 1885. (P. Blakiston, Son & Co.)

An Introduction to Practical Organic Analysis, adapted to the requirements of the first M. B. Examination, by George E. R. Ellis. 72 pp. 12mo. London, 1885. (Longmans, Green & Co.)

OBITUARY.

Titian R. Peale, of Philadelphia, died on the 13th of March, 1885, in his 86th year. Mr. Peale was one of the Naturalists of the Wilkes Exploring Expedition. He was for twenty-four years connected with the Patent Office at Washington.

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THE

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THIRD SERIES.

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[THIRD SERIES.]

Art. XXIV.—*Origin of Coral Reefs and Islands;* by James D. Dana.

[Continued from page 105.]

Part II. The Objections considered.

The objections to the Darwinian theory may be considered in the following order:

I. Darwin's insufficient knowledge of the facts bearing on the subject.

II. Subsidence not ordinarily a fact because methods of producing barrier reefs and atolls have been brought forward that do not require its aid.

III. The occurrence of cases of elevation in regions of atolls and barrier reefs inconsistent with the subsidence-theory.

IV. No ancient coral reefs in the geological series have the great thickness attributed by the subsidence-theory to modern reefs.

V. Other methods of explanation and their supporting evidence.

The adverse remarks directed against the idea of a sinking *continent* in the Pacific as the initial condition in the coral-reef subsidence are outside of the present discussion for the reason stated on the first page of this paper. In the following pages the objections are first explained, under the above-mentioned heads, and then follow, in paragraphs lettered *a*, *b*, *c*, etc., the writer's discussions of the several points.

I. *Darwin's insufficient knowledge of the facts.*

In the Address referred to in the opening page of this article, Dr. Geikie, speaking of Darwin, observes: "It should be borne in mind that, compared with more recent explorers, he did not enjoy large opportunities for investigating coral reefs." "He appears to have examined one atoll, the Keeling Reef, and one barrier-reef, that of Tahiti." "By a gradually widening circle of observations a series of facts has been established which were either not known, or only partially known, to Darwin."—The authors appealed to for the views that are presented as a substitute for Darwin's are Prof. Karl Semper, who has examined and described reefs of the Pelew and Philippine Islands; Dr. J. J. Rein, who has published on the physical geography of the Bermudas; Prof. Alexander Agassiz, who has written on the Florida reefs and others in that vicinity; and Mr. John Murray of the Challenger Expedition, whose investigations were made at Tahiti: all able men in science whether more learned or not than Darwin on the special subject under discussion. The facts from "a widening circle of observations" referred to comprise the physical and biological results of deep-sea exploration. The writer is mentioned as one of the "competent observers" who had given "independent testimony" in favor of Darwin's views after "at least equal opportunities of studying the subject," and as he has, in these later years, looked into the new facts, he has at least a claim to a hearing.

As to Darwin's knowledge, it appears to the writer that the apology offered in the above citations was not needed. In his detailed investigation of Keeling atoll—a good example of atolls and like all the rest in its principal features—and in his examination of the Tahitian reefs, followed up by a careful study of other atolls and reefs of the ocean through the maps and descriptions of former surveying expeditions, he had a broad basis for judgment and right conclusions. When the second edition of his work was published in 1874, many of the important facts from deep-sea exploration were already known; and later he learned of the more recent results; and he did not recant. A letter of his, of October 2nd, 1879, published by Mr. Semper, while admitting with characteristic fairness the interest of the facts collected by the latter, expresses his continued adherence to the opinion "that the atolls and barrier reefs in the middle of the Pacific and Indian oceans indicate subsidence."

The writer, as his expositor, may be excused for adding here that his own "independent testimony" was based on observations among coral reefs and islands in the Pacific during parts of three years, 1839, 1840 and 1841; that, besides working among the reefs of Tahiti, the Samoan (or Navigator) Islands, and the Feejees (at this last group staying three months), he was also at the Hawaian Islands; and, in addition, he landed on and gathered facts from fifteen coral islands, seven of these in the Paumotu Archipelago, one, Tongatabu, in the Friendly Group, two, Taputeuea and Apia, in the Gilbert Group, and five others near the equator east of the Gilbert Group, Swain's, Fakaafo, Oatafu (Duke of York's), Hull and Enderbury Island.* The writer may, therefore, be acquitted of presumption if he states his opinion freely on the various questions that have been brought into the discussion by other investigators. Sympathising fully with the sentiment expressed in the words, "The example of Darwin's own candor and over-mastering love of truth remains to assure us that no one would have welcomed fresh discoveries more heartily than he, even should they lead to the setting aside of some of his work," and knowing that we are all for the truth and right theory, he has reason to believe that those who have been led to object to Darwin's conclusions will be pleased to have their objections reviewed by one who has a personal knowledge of many of the facts.

II. *Subsidence not ordinarily a fact because methods of origin have been brought forward that do not require its aid.*

It is urged that, while subsidence may have happened in several cases, it is not at all necessary to the making of barrier reefs and atolls; that "subsidence has been invoked because no other solution of the problem seemed admissible;" that the "solution" by subsidence "is only, an inference resting on no positive proofs."†

a. Darwin's usual methods were not such as these words imply, and we think that he was true to those methods in his treatment of coral island facts. Darwin can hardly be said to have "invoked" subsidence. Subsidence forced itself upon his attention. He saw evidence that it was a fact, and the theory came ready-made to him. The proof of subsidence from the relations in form, structure and history between atolls and the large barrier islands, like the Gambier Group, Raiatea, Bolabola and Hogoleu, scarcely admitted, he says, of a doubt; and other facts were all in harmony with it. This, his chief argument, with the enforcing evidence in my Report, (see §§ 4 and 10 of Part I of this paper) is not set aside and not mentioned in the Address from which the above sentences are cited.

b. Darwin observes that "from the nature of things it is scarcely

* These five islands are on the map of the Central Pacific accompanying Part I of this paper. Hull's Island, is "Sydney" of the writer's Expedition Report.

† Address, page 24.

possible to find direct proof of subsidence," recognizing the fact that subsidence, unlike elevation, puts *direct* testimony out of sight. But still it has left evidence which he perceived and thought convincing: and this stands, whatever virtue there may be in other explanations.

Moreover, we have now *direct* testimony for subsidence, from the facts brought forward (for another purpose) by Mr. Murray, as is set forth beyond.

III. *The occurrence of cases of elevation in regions of atolls and barrier reefs.*

The fact that elevated reefs and other evidences of elevation occur at the Pelews, a region of wide barrier reefs and atolls, has been presented by Prof. Karl Semper,* after a study of those islands, as an objection to the theory of subsidence; for we have thereby (in the words of the Address), "a cumbrous and entirely hypothetical series of upward and downward movements." Prof. Semper reports the existence of reefs raised 200 to 250 feet above the sea-level in the southern third of the larger of the islands, while the other two thirds exhibit evidence of but little, if any, elevation.

a. Such facts are of the same general character with those of other elevated reefs and atolls discussed in §§ 12, 13, 16 of Part I, and the same explanation covers them. The Pelew region is one of comparatively modern volcanic rocks and this renders local displacements a probability.

b. The occurrence of great numbers of large and small masses of coral rock, in some places crowded together, upon the *western* or *leeward* reef of the several Pelew islands, and of none on the eastern reef, is mentioned as evidence against subsidence and in favor of some elevation: because, Professor Semper says, the strongest wind-waves on the western side are too feeble to break off and lift on the reef so large masses, some of them (as his words imply rather than distinctly state) ten feet thick.

But the difficulty does not exist in fact; for earthquakes may have made the waves. The region just west of the Pelews is one of the grandest areas of active volcanoes on the globe. It embraces the Philippine Islands, Krakatoa and other volcanic islands of the Sooloo sea, Celebes, etc. The agents that could do the work were there in force. To the eastward, in contrast, lie the harmless islands of the Caroline Archipelago, mostly atolls, serving, perhaps, as a breakwater to the Pelews.

The small elevation referred to is therefore not proved by the

* First in 1868, Zeitschr. Wissensch. Zool., xiii, 558; additions in Die Philippinen und ihre Bewohner, Wurzburg, 1869; and still later in his "Animal Life" published in Appleton's International Scientific Series in 1881.

evidence adduced; and yet it may be a fact without affecting the theory of Darwin, as I have fully illustrated.*

It is important to have in mind that the coral-reef era probably covered the whole of the Quaternary and perhaps the Pliocene Tertiary also, and hence the local elevations that have taken place in the ocean were not crowded events of a short period.

Moreover, these local elevations in coral seas are spread over an area of 25,000,000 square miles. As an example of the long distances: the Paumotu Archipelago, consisting of more than eighty atolls and two barrier-islands, and covering about 450,000 square miles, contains only three or four atolls that are over twelve feet high; and of these, Metia is 250 feet in height, Elizabeth, 80 feet, Dean's probably where highest, 15 or 20 feet. Metia is one of the westernmost, near 148° 13′ W. and 15° 50′ S.; Dean's is 60 miles to the north-northeast of Metia, and Elizabeth is far to the southeast, in 128° W. and 25° 50′ S., or nearly 1450 miles distant from Metia. Locate these points on a continent, and Pacific distances and the length of Pacific chains of atolls will be appreciated.

IV.—*No ancient coral reefs have the thickness attributed by the subsidence-theory to modern reefs.*

An argument against the subsidence-theory is based by Prof. J. J. Rein † on the alleged fact that the thickness attributed to modern reefs is far beyond that of any such reefs in earlier time; that is, the thickness is unprecedented. The argument decides nothing. The question is one of geological fact, not to be settled by a precedent. Whether, then, there are precedents or not it is not necessary to consider.

Besides this, it implies a distinction between coral-made and shell-made rocks which does not exist. The coral-reef rock is largely made of shells, and the process of formation for a limestone of shallow-sea origin is essentially the same whether shells or corals are the predominant or the sole material. No thick formation of any kind of rock was ever made, or could be made, by shore or shallow-sea operations without a slowly continued subsidence or a corresponding change of water-level.

* Mr. Semper's objection to the theory of subsidence based on the coëxistence of all kinds of reefs in the Pelews, atoll, fringing and barrier, with no reefs about one island, and from the relative steepness of the submarine slopes on the east and west reefs of an island have been sufficiently met in Part I

† Dr. Rein's first memoir on Bermuda appeared in the Senckenberg Ber. naturforsch. Gesellschaft, 1869–70, p. 157, and the later in the Verhandlung des I. deutsch. Geographentages, 1881, Berlin, 1882. The above argument is from the latter paper, and is given here from the citation by Dr. Geikie, the publication not being accessible to the writer.

V.— *Other methods of explanation, and their supporting evidence.*

A. Mr. John Murray, one of the able naturalists of the Challenger Expedition, reports the following important results from soundings off northern Tahiti, made under his supervision and that of the surveying officer.*

Along a line outward from the edge of the barrier reef there were found: (1) for about 250 yards, a shallow region covered partly with growing corals, which deepened seaward to 40 fathoms; (2) for 100 yards, between the depths of 40 and 100 fathoms, a steeply but irregularly sloping surface which commenced with a precipice of 75° and had a mean angle exceeding 45°;† then (3) for 150 yards a sloping bottom 30° in angle; (4) then a continuation of this sloping surface, diminishing in a mile to 6°, at which distance out the depth found was 590 fathoms (3,540 feet). Over the area (2), or the 100 yards between 40 and 100 fathoms, the bottom was proved to be made of large coral masses, some of them "20 to 30 feet in length," along with finer debris; outside of this, of sand to where the slope was reduced to 6°; and then of mud, composed "of volcanic and coral sand, pteropods, pelagic and other foraminifers, coccoliths, etc."

These observations have great significance. They show (1) that the feeble currents off this part of Tahiti carry little of the coral debris in that direction beyond a mile outside of the growing reef; (2) that a region of large masses of coral rock and finer material occurs at depths between 240 and 600 feet; (3) that, a mile out, the bottom has the slope nearly of the adjoining land, and in this part is covered with the remains of pelagic life.

From the second of these facts—the great accumulation of coral blocks below a level of 240 feet—Mr. Murray draws the conclusion that, in the making of fringing, barrier and atoll reefs, the widening goes forward (*a*) by making first upon the submarine slopes outside of the growing reef a pile of coral debris up to the lower limit of living reef-corals; and then (*b*) by building outward upon this accumulation as a base.

He also announces, after speaking of other causes influencing the growth of corals, the more general conclusion that "it is not necessary to call in subsidence to explain any of the characteristic features of barrier reefs and atolls," and concludes that his views "do away with the great and general subsidences" appealed to by Darwin.

a. The widening-process, in the first conclusion, had previ-

* Proc. Edinburgh Roy. Soc., Session 1879-80, p. 505.

† Dr. Geikie gives in his paper a section of the soundings, "on a true scale, vertical and horizontal," and in it the upper steepest part of this 100 yards has a slope of about 75°.

ously been a part of the Darwinian theory; for, as stated in §10 (Part I), a fringing reef, where no subsidence is going on, widens above and steepens its seaward-slope, and it could do this only by the process described: that is, by building out upon a base of debris, or, more correctly, upon true coral-reef rock made by the gradual consolidation of the debris.*

b. The broader conclusion Mr. Murray does not sustain by a mention of special facts from the soundings, tending directly to meet the question of change of level, but by attempting to show that through the eroding action of currents and other means (as had been argued by Prof. Semper), in connection with the process already explained, reefs of all kinds can be made from submarine banks without aid from subsidence.

In this place I confine myself to the question as to the fact of subsidence. The only direct argument presented against subsidence is contained in the statement, that the very broad shore-plain of Tahiti shows that "the island has not in recent times undergone subsidence," and may indicate a slight elevation; and in this he sustains the earlier statement of my Report, which says (p. 293) that the broad shore-plain of Tahiti probably overlies in some parts the fringing reef, and (p. 300) the shore-plain, if built upon reefs, as I was assured, may afford proof of a rise of one or two feet." But this admission, as I have explained for other cases of local elevation, is in no way opposed to the theory of subsidence.

c. The kind of submarine slopes to be looked for off reefs is illustrated by the soundings, as Dr. Geikie indicates. But it is interesting to note that the facts, while very important, sustain instead of correcting those announced by earlier observers. Beechey and Darwin make the mean slope about 45°, and my Report says 40° to 50°. I have assumed for the slope of the bottom outside of the reef-limit the same angle as for the surface-slope of the island just above the water level: 5° to 8° off Tahiti, of which 5° is accepted as most correct, and 3° to 5° off Upolu;† and the assumption as regards Tahiti is sustained by the Challenger soundings. My Report states (from the Expedition surveys) that off Upolu, the bottom "loses more and more in the proportion of coral sand till we finally reach a bottom of earth," and introduces this as an argument against the indefinite drifting of coral sands into the deep ocean;‡ and this argument the Tahiti soundings sustain.

With reference to the occurrence off some shores of precipitous submarine slopes, the Challenger soundings give definite facts as to one case. It leaves undisturbed the previously re-

* My Expedition Geological Report, pp. 131, 132, where figures are given illustrating the effect of widening.

† Ibid., page 47. ‡ Ibid., page 154.

ported cases of like steepness at greater depths; for example, the sounding of Captain Fitzroy at Keeling atoll (while Darwin was there), 2200 yards from the breakers, in which no bottom was found at a depth of 1200 fathoms, but the line was partly cut at a depth between 500 to 600 fathoms; the sounding by the Wilkes Expedition off Clermont Tonnerre (Paumotu Archipelago), where the lead brought up an instant at 350 fathoms, and then dropped off again, descended to 600 fathoms without reaching bottom, and came up bruised, with small pieces of white and red coral attached; a sounding by the same Expedition, a "cable's length" from Ahii, in which the lead struck a ledge of rock at 150 fathoms and brought up finally at 300 fathoms.* All the older soundings need to be repeated; but there must be enough truth in those quoted to warrant the remark that the force of Darwin's argument for subsidence from the steepness of the submarine slopes about atolls is not weakened by the Challenger results.

d. But the chief interest of the Challenger soundings consists in their affording "direct" proof, "positive" proof, of *much subsidence;* a kind of proof that subsidence sinks out of sight, and which soundings may yet make available in many similar cases.

That belt of coarse debris—including "masses 20 to 30 feet" long—was found over the steeply sloping bottom at depths between 240 and 600 feet. These depths are far below the limit of forcible wave-action. They are depths where the waters, however disturbed above by storms, have no rending and lifting power, even when the bottom is gradually shelving; depths, in this special case, against a slope which for 100 yards is 75° in its upper part, and in no part under 45°, the vertical fall being 360 feet in the 100 yards. Strokes against the reef-rock thus submerged, and under such conditions, would be extremely feeble. Waves advancing up a coast, whether storm-driven waves or earthquake-waves, do little rock-rending below the depth to which they can bare the bottom for a broadside plunge against the obstacle before them, although the velocity gives them transporting power to a greater depth. It is the throw of an immense mass of water against the front, with the velocity increased by the tidal flow over a shelving bottom,—the rate sometimes amounting, according to Stevenson, to 36 miles an hour or 52·8 feet a second,—together with the buoyant action of the water, that produces the great effects.

A vertical surface below the sea-level of 20 feet made bare for the broadside stroke is probably very rarely exceeded even

* Ibid., page 55.

in the case of earthquake-waves; and with storm-waves, or recorded earthquake-waves, the displacement of the water at a depth of 240 feet would be at the most only a few inches. I saw on atoll reefs no upthrown masses of coral rock over ten feet in thickness and twenty feet in length or breadth. It is therefore plainly impossible that such a belt of debris should have been made at its present level, or even at a depth of 20 feet; and hence the debris affords *positive proof of a large subsidence during some part of the reef-making era.*

The existence of the belt of debris may be explained as follows: If the reef now at a depth of 240 feet were at the sea-level as the sea-level reef, and subsidence were not in progress for a period, the very steep front of the reef now just below the 240-foot level might have resulted from the widening that would have gone forward. And, under such conditions, the action of the occasional extraordinary waves might have torn off masses from the front which would have tumbled down the steeply sloping surface until the belt of debris had been formed. Then, with a renewal of the slow subsidence, the thickening of the reef would have been resumed and gone on to its final limit, and the rendings of the great waves found lodgment at higher levels. The masses now on atoll reefs must be from comparatively recent upthrows.

This *direct* evidence of subsidence from Tahiti renders it reasonable to make subsidence in atoll-making a general truth. It is nevertheless desirable that facts of the kind should be multiplied. The abrupt descent in the submarine slopes of reefs detected by Fitzroy at a depth below 3000 feet, and those reported by the Wilkes Expedition at depths of 2100 and 900 feet, seem to indicate a similar rest at the sea level and consequent reef-widening, in the course of a progressing subsidence; and proof of this may yet be found in belts of coarse coral-rock debris at the foot of the precipices. Such a period of rest would lead to the forming of submarine precipices in different regions contemporaneously at different depths according to the rate of subsidence of the part of the subsiding area.

B. From facts observed about the Florida reefs, Lieutenant E. B. Hunt, U. S. N., announced, in 1863,* the conclusion that these reefs had received their westward elongation through the westward "sweep" of an eddy current to the Gulf Stream. The subject, nearly twenty years afterwards, was more thoroughly investigated by Mr. Alexander Agassiz, and the same

* This Journal, II, xxxv, 197.

conclusion reached.* Mr. Agassiz made also another important observation—that this current is an abundant carrier of marine life for the feeding of the coral animals, and so accelerates the coral growth and accumulation in its direction. Combining with these effects others considered beyond, Mr. Agassiz expresses, like Mr. Murray and Mr. Semper, the further conclusion, that all kinds of reefs, atoll, fringing and barrier, may be made without aid from subsidence.

a. The facts, presented by Lieutenant Hunt, and more fully by Mr. Agassiz with regard to the effects of the eddy current of the Gulf Stream, show that coral reefs may be elongated, and also that inner channels may be made, by the drifting of coral sands. But the action with coral sands is essentially the same as with other sands; and illustrations of this drifting process occur along the whole eastern coast of North America from Florida to Long Island. We there learn that drift-made beaches run in long lines between broad channels or sounds and the ocean ; that they have nearly the uniform direction of the drift of the waters, with some irregularities introduced by the forms of the coast and the outflow of the inner waters which are tidal and fluvial and have much strength during ebb tide. The easy consolidation of coral sands puts in a peculiar feature, but not one that affects the direction of drift accumulation.

b. The great barrier reef off eastern Australia, a thousand miles long, has some correspondence in position to the sand-reefs off eastern North America. But it is full of irregularities of direction and of interruptions, and follows in no part an even line. In the southern half, it extends out 150 miles from the coast and includes a large atoll-formed reef ; in the northern half, the barrier while varying much in course is hardly over 30 miles from the land. There is very little in its form to suggest similarity of origin to the drift-made barriers of sand.

c. In the Pacific Ocean, the trends of many of the coral island groups, and of the single islands, do not correspond with the direction of the oceanic currents, or with any eddy currents except such as are local and are determined by themselves.

* On the Tortugas and Florida Reefs, by A. Agassiz, Trans. Amer. Acad., xi, 1883.

Professor Louis Agassiz's account of the Florida reefs was published in the U. S. Coast Survey Reports of 1851 and 1866, and reproduced in vol. vii of the Memoirs of the Museum of Comparative Zoology. It gives an excellent description of the Florida reefs, and of the action of boring animals and other injurious agents on corals, and reaches the conclusion that the reef has been raised to its present level and thickness by wave and current action, without the aid of elevation or subsidence. The argument is based on such observations as could be made over the surface of the reefs and the adjoining sea-bottom, and bears on the question of the necessity of subsidence and not on the fact of subsidence.

Near longitude 180°, as the map of the Central Pacific (see Part I) illustrates, the equator is crossed by the long Gilbert (or Kingsmill) Group, at an angle with the meridian of 25° to 30°, and not in the direction of the Pacific current which is approximately equatorial. This obliquely crossing chain of atolls is continued northward in the Ratack and Ralick Groups (or the Marshall Islands), making in all a chain over 1200 miles long; and, adding the concordant Ellice Islands on the south, and extending the Ratack line to Gaspar Rico its northern outlier, the chain is nearly 2000 miles long. Nothing in the direction of the long range, excepting local shapings of some of the points about the atolls, can be attributed to the Pacific currents. Moreover, the diversified forms of the atolls have no sufficient explanation in the drift process.

d. Further: drifting by currents may make beaches and inner channels whether subsidence is going on in the region or not, and are not evidence for, or against, either a movement downward or upward. Sandy Hook, the long sandy point off the southern cape of New York harbor, has been undergoing (as the U. S. Coast Survey has shown) an increase in length, or rather variations in length, through the drifting of sands by an outside and an inside current; and this is no evidence that Professor G. H. Cook is wrong in his conclusion that the New Jersey coast is slowly subsiding.

e. But even *in this region of Florida we have strong evidence of a great subsidence during the coral-reef era*, and all the subsidence that the Darwinian theory demands.

In a very valuable paper by Mr. Agassiz, published in 1879 in the Bulletin of the Museum of Comparative Zoology,* the author points out that the South American continent, in comparatively recent geological times, had connection with the West India islands through two lines: (1) one along a belt from the Mosquito Coast to Jamaica, Porto Rico and Cuba; and (2) the other through Trinidad to Anguilla, of the Windward Islands. He sustains the conclusion by a review of the soundings made by the Steamer Blake under the command of J. R. Bartlett, U. S. N., and a consideration of the facts connected with the distribution of marine and terrestrial species. As the soundings show, the former of the two connections requires for completeness an elevation of the region amounting to 4060 feet over the part south of Jamaica, 4830 feet between Jamaica and Hayti, and 5240 feet between Hayti and Cuba. The other line of connection requires an elevation of 3450 feet. An open channel, as he observes, would thus be left between Anguilla and the Virgin Islands, where there is now a depth of 6400 feet. The close relations in the existing fauna

* An abstract of the paper is contained in this Journal, III, xviii, 230, 1880.

of the Gulf to that of the Pacific waters prove that it continued to be a salt-water gulf through the era of elevation.

Mr. Agassiz infers that the connection of the West India Islands with South America existed before the Quaternary era. But there are other facts which seem to prove that it was continued into, or at least was a fact, in the Quaternary.

The opinion as to a connection of the Windward Islands with South America in the Quaternary was presented by Prof. E. D. Cope in 1868, and earlier, as he states, by Pomel, on the ground of the discovery in the caves of Anguilla of a species of gigantic Rodent related to the Chinchilla, as large as the Virginia deer, and nearly equaling the Quaternary *Castoroides* of Ohio.* Further, De Castro, as cited by Dr. J. Leidy in his "Mammalian Fauna of Dakota and Nebraska," 1869, announced, in 1865, a gigantic sloth of the "Quaternary," from Cuba, which he referred to the genus *Megalonyx*, and Dr. Leidy named *Megalocnus rodens*, proving a Quaternary connection between the continent and Cuba.

The fact of an elevated condition of the region sufficient to make Cuba and Anguilla part of the continent during the earlier Quaternary, if not in the Pliocene also, is thus made quite certain. This is fully recognized by Wallace.† Such a condition could hardly have existed without a large elevation also of Florida, though probably not, as Mr. Agassiz holds, to the full amount of the depression between it and Cuba—nearly 3000 feet—because Cuba is most closely related in fauna to South America. The subsidence which brought the region to the present level was consequently within the coral-reef period. It is hence hardly to be doubted that the making of the Florida, Bahama and other West India coral reefs was going on during the progress of a great subsidence. None of the facts mentioned by observers are opposed to this view.

It is of interest to note here that on Cuba and Jamaica there are *elevated* coral reefs, the highest on Cuba 1000 feet above the sea, according to Mr. Agassiz, and probably at one point 2000, according to Mr. W. O. Crosby's observations,‡ and on Jamaica 2000 feet, according to Mr. Sawkins; indicating that there have been upward movements subsequent to the down-

* Proc. Philad. Acad. Nat. Sci., 1868, 313, and Proc. Philad. Amer. Phil. Soc., 1869, 183; also Smithsonian Contributions to Knowledge, 30 pp. 4to with 5 plates, Washington, 1883. The last paper (prepared in 1878) contains descriptions of the following species from the Anguilla bone-cave. *Amblyrhiza inundata* Cope (the large Rodent announced in 1869), *A. quadrans* Cope, *A. latidens* Cope, an Artiodactyl apparently of the *Bovidæ* and a little smaller than *Ovis aries*. With them was obtained an implement ("a spoon-shaped scraper or chisel") made of the lip of the large *Strombus gigas*.

† Geograph. Distrib. of Animals, ii, 60, 78.

‡ Proc. Boston Soc. Nat. Hist, xxii, 124, 1882, and in abstract in this Jour. xxvi, 148, 1883.

ward. Mr. Crosby argues that the great thickness of the now elevated reefs could have been produced only "during a progressing subsidence," so that "we have apparently no recourse but to accept Darwin's theory."

C. It has been urged by Mr. Semper, Dr. J. J. Rein, Mr. A. Agassiz, Mr. Murray, Dr. Geikie and others, that since the growing calcareous deposits of the sea-bottom are slowly rising toward the surface by successive accumulations of the shells and other debris of marine species, they may have been built up locally in various regions of the deep seas (as they actually are now about some islands) until they were near enough to the surface to become next a plantation of corals; and that in this way, atolls became common within the area of the tropical oceans. The method is regarded as setting aside subsidence.

a. The advocates of this hypothesis have not pointed to such a mound now approaching the ocean's surface on the western border of the Gulf Stream, where the depth over the remarkably luxuriant region is least; and none over any part of the tropical Pacific. It is suggested that the Chagos Bank may be one example; but it is not known to be so. Professor Semper states that he found evidence of pelagic life instead of modern corals in the lower part of the elevated reefs of the Pelews. Dr. Geikie cites from letters by Dr. Guppy in Nature of Nov. 29, Dec. 6, 1883, and Jan. 12, 1884, the fact that in elevated reefs on the Salomon Islands, 100 to 1200 feet high, the coral rock forms a comparatively thin layer over impure earthy limestone abounding in foraminifers and other pelagic organisms, such as Pteropods. Such observations have great interest, but they only prove that, in coral-reef seas, corals will grow over any basis of rock that may offer where the water is right in depth, and do not nullify any of the evidences of subsidence. This point should be kept before the mind in all future study of coral-reef regions. Borings in coral islands, as recommended on a former page, are the true means of investigating it.

b. The old hypothesis that atolls may have been built upon the summits of submerged mountain-peaks or volcanic cones at the right distance under water for growing reef-corals, or, if not at the right level, brought up to it by other organic depositions, or down to it by abrasion, is urged by Mr. Murray.

This writer observes that "the soundings of the Tuscarora and Challenger have made known numerous sub-marine elevations: mountains rising from the general level of the ocean's bed at a depth of 2500 or 3000 fathoms up to within *a few hundred fathoms* of the surface." But "a few hundred fathoms," if we make few equal 2, means 1200 feet or more, which leaves a long interval yet unfilled.*

It is also urged that some of the "emerged volcanic mountains situated in the ocean basins" may have been wholly swept away and left with a few fathoms of water above them. But this is claiming more from the agents of erosion than they could possibly have accomplished, as the existence of an atoll in the ocean and the examples on coasts of wave and tidal action prove.

D. To give completeness to the hypothesis which makes barrier and atoll islands out of sub-marine banks (whether these banks have a basis of volcanic or other rocks, or of calcareous accumulations), it is necessary to show that the waters of the waves and currents can make barrier islands and atolls out of such banks without subsidence; and explanations to this effect have been given.

It is urged, in agreement with Darwin, that the outer portions of reefs increase faster than the inner, owing to the purer water about them and the more abundant life for food; that the inner parts are not only at a disadvantage in these respects but suffer also from coral debris thrown over them. They add to these causes of unequal growth mentioned by Darwin, the solvent and abrading action of the waters.

It is hence concluded that, under these conditions, the simple bank of growing corals may have a depression made at center, which, as the process continues, will become a lagoon basin, and the reef, thereby, an atoll with a shallow lagoon; that the atoll, so begun, may continue to enlarge through the external widening of the reef and the further action of current-abrasion and solution within; or, in the case of fringing reefs, that the change may go on until the reef has become a barrier-reef with an inner channel and inner reefs. It is admitted that subsidence may possibly have helped in the case of the deepest lagoons.

Dr. Geikie expresses his opinion on the subject thus: "As the atoll increases in size the lagoon becomes proportionally larger, partly from its waters being less supplied with pelagic

* The actual depths over the elevations in the Tuscarora section between the Hawaian Islands and Japan, numbering them from east to west, are as follows: 1, 11,500 feet; 2, 7500 feet; 3, 8400 feet; 4, 12,000 feet; 5, 9000 feet (this seven miles west of Marcus Island); 6, 9600 feet. Whether ridges or peaks the facts do not decide; probably the former. No. 1 has a base of 185 miles with the mean eastward slope 40 feet per mile (=1:132) and the westward 128 feet per mile. No. 2 has a breadth of 396 miles, with the mean eastern slope mostly 37 feet per mile, but 51 feet toward the top, and the westward, 55 feet per mile (=1:96). No. 3 was the narrowest and steepest, it being about 100 miles broad at base and having the mean eastern slope 192 feet per mile and the mean western 200 feet.

food, and therefore less favorable to the growth of the more massive kinds of corals, partly from the injurious effects of calcareous sediment upon coral growth there, and partly also from the solvent action of the carbonic acid of the sea-water upon the dead coral."

Mr. Semper gives examples of the effects of currents at the Pelew Islands, stating that by striking against or flowing by the living corals they make the reef grow with steeper sides and determine its direction, and urging that abrasion and solution have made not only the deep lagoon-like channels, but the deeper channels between the islands. He holds that in Kriangle, which he describes as a true atoll with no channel leading into the lagoon from the sea, that the lagoon may have been "the result of the action of currents on the porous soil during a period of slow upheaval.* He says, further, that the large channel in the main island of the group "forty fathoms deep and many miles wide," "finds an easy explanation on the assumption of an upheaval;" it became "wider in proportion as the enclosed island consisting of soft stone [tufa] was gradually eaten away, and during slow upheaval it would continue to grow deeper in proportion as the old porous portions of the reef and the rock in which it was forming were more and more worn down by the combined action of boring animals and plants, and of the currents produced by the tides and by rain." Mr. Semper refers to the dead depressed tops of some masses of Porites near tide-level as the effects of the deposit of sediment over the top of the living coral and of erosion by the waves and exposure to rains while the sides continued to grow; and the fact is made an example on a very small scale of atoll-making. Other examples of the action of currents, sediment, boring species, and the solvent action of carbonic acid in the waters, are mentioned by Mr. Agassiz, in his excellent account of the "Tortugas and Florida reefs."

a. The theory, if satisfactory, accounts not only for the origin of an atoll, but for the origin of atolls of all sizes, shapes and conditions, and for great numbers of them in archipelagos and chains; not only for channels through fringing reefs, like those that abrasion in other cases makes, but for all the irregular outlines of barriers, for the great barriers reaching far away from any land, and for the positions and indented coasts of the small included lands. Is it a sufficient explanation of the facts?

b. The currents that influence the structure of reefs are: (1), the general movement or drift of the ocean, in some parts varying with seasonal variations in the winds; (2), the currents

* Animal Life," pp. 269, 270.

connected with wave-action and the inflowing tide over a shelving bottom; and (3), the currents during the ebb, flowing out of channels; together with (4) counter-currents. Each region must have its special study in order to mark out all the local effects that currents occasion. Such effects are produced whether a secular subsidence is in progress or not, and hence a particular review of the subject in this place is unnecessary.

The shaping of the outside of the reef and the determination of the width and level surface of the shore-platform are due chiefly to the tidal flow and the accompanying action of wind-waves, as explained in §17 of Part I.*

The current that accompanies the ebb is locally the strongest. Owing to the great width of many barrier reefs and of the channels and harbors within them, the tide flows in over a wide region. At the turn in the tide the waters escape at first freely

* Since the first part of my paper was published I have observed in an article by Mr. A. R. Hunt, in the Scientific Proceedings of the Royal Dublin Society, iv, 254, January, 1885, the remark, referring to a statement of the above fact in my Manual of Geology, that the "statement though strictly in accordance with Mr. Russell's theory, has so far as I can ascertain, no foundation in fact." The statement, as I have said (and as I illustrate in my Geology) was but the statement of a fact observed by me first in 1839 on the coasts of Australia and New Zealand, without a thought of any theory; and part of the explanation is overlooked by Mr. Hunt. I *observed* that the first waters of the incoming tide swelled over the sandstone platform (which was a hundred yards or more wide off the Port Jackson heads), and became thus a protector of the sandstone *platform* from breaker strokes; and that the lower part of the sandstone *bluff* to a height a little above high tide, was hollowed out by the strokes of the breakers. A similar erosion near high tide level of the great coral masses standing on the coral-rock platform of atolls I also observed while among the Paumotu Islands. Prof. A. E. Verrill informs me that he has seen examples of the same action on a grand scale about the island of Anticosti in the Gulf of St. Lawrence. The observations do not appear to me to be at variance with the principles laid down in Mr. Hunt's valuable paper; they require only his recognition of a tidal effect which he does not fully consider, and which British seas cannot illustrate.

To produce a platform, (1) the rock-material exposed to the flow of the tide and the breakers must be firm enough to resist wear during the early part of the flow, and at the same time soft enough to allow the striking breakers to cut into the base of the bluff, or shear off the projecting ledge; and (2) the region must not be one of very high tides or stormy seas, for, in such regions of forceful waves and tides, the movements are too often of the destructive kind through the whole continuance of the flow, leaving no chance for the protection a platform needs. Loose sand-deposits are too soft; they are worn off below the sea-level and changed in surface by storms; but some firmer kinds may make a low-tide flat in a bay where the tides are small. Coral-reef rock, the material of the atoll platform, has the hardness and solubility, in carbonated sea-water, of ordinary limestone. The rock of the Port Jackson Heads is a friable sandstone. At the Bay of Islands, New Zealand, the platforms occur in an argillaceous rock, which becomes soft and earthy above by weathering, but is unaltered and firm below because kept wet (loc. cit. p. 442). At the Paumotus the tides are two to three feet high, and the platform usually 100 yards or more wide; at the Phœnix Group the tides are five to six feet high and the platform mostly fifty to seventy yards wide; at the Port Jackson Heads, the ordinary tides are six feet high and the platform fifty to one hundred and fifty yards wide; at the Bay of Islands (in the sheltered waters of the bay), the tides are eight feet high and the platform is under thirty yards wide.

over the same wide region; but, with a tide of but two or three feet, there is but little fall before the reef—which lies at low tide level and a little above it—retards it by friction; and thus escape by the open entrances is increased in amount and in rate of flow. The facts are the same in atolls where the lagoons have entrances.*

c. Examples of massive corals having the top flat, or depressed and lifeless, while the sides are living, are common in coral-reef regions, wherever such corals are exposed to the deposition of sediment, and where they have grown up to the surface so that the top is bare above low tide. A disk of Porites, having the top flat and the sides raised (owing to growth) so as to give it an elevated border, is figured on plate LV. of my Report on Zoophytes. Many such were found in the impure waters of a shore reef at the Feejees. At Tongatabu one flat-topped mass of Porites was twenty-five feet in diameter; and both there and in the Feejees, others of Astræids and Meandrinas measured twelve to fifteen feet in diameter.

Over the dead surfaces, as Mr. Semper observes, the coral may be eroded by the solvent action of the waters, and especially where depressions occur to receive any deposits, and boring animals may riddle the coral with holes or tubes. But generally the erosion is superficial; the large masses referred

* The currents of the tropical Pacific Ocean are of very unequal rate in its different parts, and very feeble in the Paumotu Archipelago and the Tahitian and Samoan regions. Capt. Wilkes reports that in the cruise of the Expedition through the Paumotu Archipelago to Tahiti, a distance of a thousand miles, during a month from August 13 to September 13, 1839, the drift of the vessels was only 17 miles; and that during fourteen days in the first half of October, between Tahiti and Upolu of the Samoan group, nearly 1800 miles, the drift was only 43 miles.

The Challenger, on her route from the Hawaian Islands to Tahiti, found. between the parallel of 10° S. and Tahiti, "the general tendency of the current westerly, but its velocity variable;" between the parallel of 10° S. and 6° N., the direction was westerly with "the average velocity 35 miles per day, the range 17 to 70 miles per day," the maximum occurring along the parallel of 2° N. Farther west. about the Phœnix group, the equatorial current, as described by Mr. Hague (loc. cit., p. 237), has "a general direction of west-southwest and a velocity sometimes exceeding two miles per hour." At times it changes suddenly and flows as rapidly to the eastward. The drifting of the sands about Baker's Island (in latitude 0° 13′ N., longitude 176° 22′ E.) has much interest in connection with this subject of current action, and the facts are here cited from Mr. Hague's paper. The west side of the little island ($1 \times \frac{3}{5}$ m. in area) trends northeast, and the southern east-by-north, and at the junction a spit of sand extends out. During the summer the ocean swell, like the wind, comes from the southeast, and strikes the south side; and consequently the beach sands of that side are drifted around the point and heaped up on the western or leeward side, forming a plateau along the beach two or three hundred feet wide, and eight or ten feet deep over the shore platform. With October and November comes the winter swell from the northeast, which sweeps along the western shore; and in two or three months the sands of the plateau are all drifted back to the south side, which is then the protected side, extending the beach of that side two or three hundred feet. This lasts until February or March when the operation is repeated.

to showed little of it. Such dead surfaces in corals are generally protected by a covering of nullipores and other incrusting forms of life, and the crusts usually spread over the surfaces *pari passu* with the dying of the polyps.

d. Every stream, says Mr. Semper (when explaining, as cited on a preceding page, the origin of the deep channel of the large Pelew island, whose depth is "35 to 45 fathoms"), "has a natural tendency to deepen its bed." But there is a limit to this action. The eroding or deepening power of a stream through abrasion and transportation is null or nearly so below the level of its outlet. A basin or channel 45 fathoms (270 feet) deep with an outlet of much less depth could not be deepened by such means or protect itself from shallowing. The depth of the outlets is not stated except that they are said to be ship-channels. Moreover, with a tufa bottom, solution could not contribute to the removal, since carbonated waters, although decomposing the tufa, dissolve very little of its ingredients. An elevation in progress would result in making of the channel a closed lake and finally dry land.

For the same reason, the small atoll, Kriangle, having, as described, a *closed* lagoon, could have no deepening of the lagoon from abrasion by tidal currents or wave-action during the progress of an elevation. And if a lagoon have an outlet, the rapid current of the ebb would be confined to the narrow passage-way and a portion of the bottom near it; through the larger part of the lagoon, as in any other lake, the waters would have scarcely perceptible motion, and therefore slight energy for any kind of work. Hence a lagoon would lose very little by this means, and shallowing would go on unless there were great loss through the *solvent action* of the waters. An elevation would only hurry the shallowing and end in emptying the lagoon.

e. Erosion through solvent action is promoted by the presence in the waters both of carbonic acid and organic acids. The material within reach of the tides or waves exposed to this action is dead corals and shells, or their debris, and bare coral rocks, occurring over: (1) the outer region of living corals and for a mile or so outside; (2) the shore platform and the reef, bare at low tide, on which there is comparatively little living coral; and (3) the lagoon basin. There is nothing in the material within the lagoon to favor solution more than in either of the other two regions; in fact, the platform and bare reef are most exposed to the action because of the small amount of living corals over them. The outside waters take up what they can through the carbonic acid they contain, and supply thereby the wants of the lime-secreting polyps, shells, etc., and carry on the process of solidification in the debris; the same waters move on over the atoll reef and take up more lime as far as the acid ingredient is present; and then they pass to the lagoon for work similar to that outside, with probably a diminished amount of free carbonic acid, on account of the loss over the reef-ground previously traversed.

The lagoon-basin is not, therefore, the part of the atoll that loses most by solution, any more than by abrasion and transportation. The outer reefs suffer the most; and yet, if the island is not subsiding at too rapid a rate, they keep extending and encroaching on the ocean, instead of wasting through the drifting into the ocean at large of calcium carbonate in grains and solution; and the shore-platform also preserves its unvaried level notwithstanding the daily sweep of the tidal floods, and the holes that riddle its outer portion.

The remark: "It is a common observation in atolls that the islets on the reefs are situated close to the lagoon shore;" such "facts point out the removal of matter which is going on in the lagoons and lagoon channels,"* I know nothing to sustain. The width of the shore-platform on the seaward side is always greater than that on the lagoon side; but the outside shore-platform has its width determined by tidal and wave action, and this action is powerful on the ocean side, and feeble on the lagoon side; it produces a high coarse beach on the outside as the inner limit of the platform, and a finer, lower and much more gently sloping beach on the inside. The amount of erosion is far greater, as it should be, on the side of the powerful agencies.

f. The loss to the lagoon by abrasion and solution *is reduced to a minimum, in the majority of atolls, by the absence of lagoon entrances*, which leaves them with only concealed leakage passages for slow discharge.

Nine-tenths of atolls under six miles in length (or in longer diameter), half of those between six and twenty miles, and the majority of all atolls in the Pacific ocean, have no entrances to the lagoon a fathom deep; and the larger part of those included in each of these groups have no open entrances at all.

For evidence on this subject, I refer to the Wilkes Expedition Hydrographic Atlas. This atlas contains maps of nearly sixty coral islands from the surveys of its officers, drawn on a large scale (one or two miles, rarely four, to the inch).

Out of the number, nine, ranging from $1\frac{1}{2}$ to 3 English miles in the longer diameter of the reef, *have no lagoon*, but only a small depression in its place; two of these take in water at high tide, and the rest are dry.

Of those *under six miles in length having lagoons*, seventeen in number, sixteen are represented as having *no entrances to the*

* Mr. Murray, loc. cit., p. 515.

lagoon at low tide; and the one having an entrance is 5×4 miles in size. The smallest is about a mile in diameter.

Of those that are *six miles or over* in length, twenty-nine in number, seventeen have channels and twelve have none. Those having channels are mostly over ten miles in length. A list of them is here given with their sizes, and also the proportion of the reef around the lagoon which is under water above third tide, and bare at low tide, a feature of much interest in this connection.

Ellice Group.—Depeyster's: 6×6 m.; three-fourths of the encircling reef bare. Ellice's: 9×5 m.; three-fourths bare.

Gilbert Group.—Apia: 17×7 m.; half bare. Tarawa: 21×9 m.; half bare. Taritari: 18×11 m.; two-thirds bare. Apamama: 12×5 m.; half bare. Taputeuea; west side mostly submerged.

Marshall Islands (northern).—Pescadores: 10×8 m.; four-fifths bare. Korsakoff: 26m.; four-fifths bare.

Paumotus.—Peacock: 15×7 m.; nearly all wooded. Manhii: 13×5 m.; nearly all wooded. Raraka: 6×9 m.; three-fourths wooded. Vincennes: 13×9 m.; mostly wooded. Aratica: 18×11 m.; three-fifths bare. Tiokea: 18×4 m.; two-thirds wooded. Kruesenstern's: 16×10 m.; mostly wooded. Dean's (or Nairsa): 53×18 m.; half or more bare.

g. The absence of open channels in so large a proportion of lagoons, and especially in lagoons of the smaller atolls, appears to be fatal to the abrasion-solution theory. The method of enlarging atolls through currents and solution can act only feebly if at all where waters have no free outlet; and this is eminently so with the smaller atolls which have been assumed by the theory to be most favorable in purity of water and in abundant life for progress; if the small cannot grow, the large lagoons cannot be made from them by the proposed method.

Reverse the method, letting the large precede the small (as under the subsidence theory), and then we have a consistent order of events. We have large atoll reefs with several large entrances (like the great barrier reef about a high island in this and other respects) gradually contracting, and the entrances concurrently narrowing through the growing corals and the consolidating debris, in spite of the efforts of abrasion and solution to keep them open and make them deeper; and, afterwards, the atoll becoming still smaller until the entrances close up; and finally the lagoon-basin is reduced to a dry depression with nothing of the old sea-water remaining except, perhaps, some of its gypsum.

h. Instead of small lagoons having the purest waters, the reverse is most decidedly and manifestly the fact, and this accords with the reversal in the history just suggested. Since atolls of middle and larger size commonly have one-third to two-thirds of the encircling reef covered with the sea at one-third tide, making the ocean and lagoon for more than half the time continuous, the large lagoon in such a case has as pure water as the ocean, and commonly as good a supply of food-life, and sometimes as brilliant a display of living corals. But in the smaller atolls, the area of the lagoon has little extent compared with the length and area of the encircling reef; coral sands and other calcareous material consequently have possession of the larger part of the bottom, and the waters, since they are less pure than those outside, contain fewer and hardier kinds of corals and less life of other kinds. They are exposed, also, to wider variations of temperature than the outer, with injury to many species, and at lowest tides may become destructively overheated by the midday sun, as many a plantation of corals with dead tops for a foot or more bears evidence. In the smallest atolls, the lagoons are liable also to alternations of excessive saltness from evaporation and excessive freshness from rains, and consequently no corals can grow inside, though still flourishing well in the shallow sea about the outer reef. The above are the facts, not the suggestions of theory.

i. We read: "So great is the destructive and transporting influence of the sea under the combined or antagonistic working of tides, currents and wind-waves that the whole mass of the reef, as well as the flats and shoals inside, may be said to be in more or less active movement."* This description of the Tortugas reefs is not applicable to the atolls of the Pacific. Notwithstanding the testimony of Captain Beechey and others about occasional catastrophes—which are mostly catastrophes to the islets and banks within the lagoons—I was led to look upon a coral island as one of the most stable of structures. The waves and currents have shaped its reef, shore platform, and beaches, fitting it well in all respects for its place by means of the forces that were to assail it; and an air of placid repose, as it lies amid the breakers, is its most impressive feature. Through the wind-made and tidal movements, the loose sands are drifted along the shores and over the reef; edges of the reef are broken off in gales or by earthquake waves; and occasionally a mushroom islet in the lagoon, where growing corals are not compacted by wave-action, is overthrown by the same means; but beyond this the structure is singularly defiant of the encroaching waters. Earthquakes may bring devastation; and so they may to other lands.

* Address, p. 23.

V. *Conclusion.*

With the theory of abrasion and solution incompetent, all the hypotheses of objectors to Darwin's theory are alike weak; for all have made these processes their chief reliance, whether appealing to a calcareous, or volcanic, or mountain-peak basement for the structure. The subsidence which the Darwinian theory requires has not been opposed by the mention of any fact at variance with it, nor by setting aside Darwin's arguments in its favor; and it has found new support in the facts from the Challenger's soundings off Tahiti that had been put in array against it, and strong corroboration in the facts from the West Indies.

Darwin's theory therefore remains as the theory that accounts for the origin of coral reefs and islands.

VI. *Central-Pacific Subsidence.*

Darwin, as has been said, took a step beyond direct observation in his inference that the subsidence attested to by each atoll extended over the intermediate seas and characterized a large central area of the ocean. He may be wrong here (and the writer with him) while not wrong in his theory. But, considering the distribution of the Pacific atolls in the ocean, their relation in this respect to the distribution of other Pacific lands, and the facts connected with the history of coral reefs and islands, the generalization appears to be well sustained. The question is here left without further argument, to be considered over the best geographical map of the ocean to be had, and the best bathymetrical map that can be made, only asking that the doubts which physical theory has set afloat may not be allowed by the geologist to warp the judgment or cripple investigation.*

My own agreement with Darwin as to the area of Coral-reef subsidence was promoted by an early personal study of the oceanic lands. For more than five years previous to passing my third decade I was ranging over the oceans—receiving im-

* One point often encountering an *a-priori* doubt is the slowness of the required subsidence. The subsidence over the Appalachian region which preceded the making of the Appalachian mountains amounted, according to well ascertained facts (as stated by Hall and Lesley), to at least 30,000 feet. The great trough, nearly a thousand miles long, was in progress through all of Paleozoic time. If the Paleozoic ages covered only 20,000,000 years (a low estimate) the mean annual rate was 0·018 inch, which is less than half a millimeter per year. Such a fact is no evidence as to the rate of the atoll-making subsidence; but, whatever the cause to which the Appalachian subsidence is to be attributed, it is suggestive as to possibilities and probabilities connected with the earth's movements.

pressions from a survey of the earth's features. I was made to see a system of arrangement in the Pacific islands, instead of a "labyrinth;" to appreciate the vast length of the island chains in the great ocean with their many parallelisms, and the accordant relations subsisting between them and long lines of atolls. I was thence led to observe the corresponding system in the features of the continental lands, and the more fully so when afterwards it was proved that geology was not in America merely the study of strata and fossils, but of the successive stages in a growing continent. Thus a conception of the earth as a unit became early implanted, and the idea also of its development as a unit under movements as comprehensive as the system in its feature-lines. My faith in any mountain-making theory hitherto proposed is weak. But that idea of system in structure and progress stands, and, however much ignored by students of the earth's stratigraphy, it must have its explanation in a true theory of the earth's dynamics.

ART. XXV.—*On a quartz-twin from Albermarle County, Virginia;* by W. G. BROWN.

IN 1851 G. Rose* described a group of quartz crystals from Reichenstein in Silesia, and announced that he had observed among them a new twinning law which he states as follows:

"Die Zwillingsebene ist nämlich, eine Hauptrhomboëderfläche; die Krystalle sind aber nicht mit dieser, sondern mit einer darauf senkrechten Fläche verbunden, und die Krystallgruppe besteht auch nicht aus zwei, sondern aus vier Individuen, indem an einen mittleren Krystall drei Individuen so angewachsen sind, dass eine Hauptrhomboëderfläche von jedem der letzteren mit einer der drei Hauptrhomboëderflächen des mittleren Krystalls in gleicher Ebene liegt."

In a note to this paper he mentions a specimen from the same place in the possession of Weiss, of which the transparent quartz crystals possessed a slight amethystine color; a color which seems to be common with the other occurrences to be mentioned. J. D. Dana† calls this group of Rose "a three-rayed twin, consisting of a central crystal twinned to three others by each R of one extremity," and gives the figures (figs. 198 A. B.) copied from Rose's paper.

Eck‡ took however a different view of the nature of this group and showed that it was not a twin according to the law

* Poggen. Annalen, vol. lxxxiii, p. 461, 1851.
† A system of Mineralogy, 5th Ed., p. 161, 1869.
‡ Zeitschr. d. deutsch. geol. Gesellschaft, vol. xviii, p. 428, 1866.

composition face R. Indeed he supposes that it was formed by the growth of quartz crystals upon the $-\frac{1}{2}R$ faces of calcite in a certain definite manner which he explains.

A little later Jenzsch* in a paper on the twinning laws of quartz, under the second law, which he calls the "Reichensteiner" or G. Rose's, seems to admit the explanation of Eck as applied to the whole group described by Rose, but says that for each two crystals in twinned positions G. Rose's law holds good, and gives an ideal figure representing two crystals twinned according to the law, twinning plane R. This figure (197 B) is in Dana.† Jenzsch seems to consider the statement of Naumann‡ that the twinning plane in these crystals instead of R may be $-\frac{5}{8}R$ "R (oder auch $-\frac{5}{8}R$)" as a quibble or attempt at a complete bibliography which it was hardly worth noticing.

That the view of Eck was the correct one is strengthened by the paper of vom Rath and Frenzel§ and that of E. S. Dana,‖ in both of which regular growths of quartz on calcite are described which, while not exactly agreeing with the groups described by Rose, seem to leave no doubt but that groups of quartz, "pseudo-twins," are formed by regular growth upon the rhombohedron $-\frac{1}{2}R$ of calcite. In the paper of vom Rath and Frenzel it is said that the faint reddish color of the Reichenstein quartz which reminds one of pale amethyst is due to disseminated particles of hematite. They further redescribe the group of G. Rose and give an exact drawing of the crystals (naturtreue Zeichnung).

Hare¶ says the form described by Rose is not excessively rare at Reichenstein and mentions, as do both vom Rath and Frenzel and E. S. Dana, that Breithaupt first noticed the regular growth of quartz on calcite and gives full references to Breithaupt's papers.

Since Jenzsch** says "Obgleich nun das G. Rose'sche Gesetz mit Evidenz bis jetzt nur von Reichenstein in Schlesien bekannt ist," and in the last edition (11th) of Naumann-Zirkel the view of Eck with regard to this kind of twinning is accepted, it appears justifiable to describe a group of amethystine quartz crystals found near Mechums River, Albemarle County, Virginia, in which there is twinning according to the law. Twinning plane and composition face $-\frac{5}{8}R$.

The group, which is transparent and of decidedly amethystine color is represented in nearly actual size by the accompanying figure. The sub-individuals marked *a* and *c* in par-

* Poggen. Annalen, vol. cxxx, p. 600, 1867. † Loc. cit.

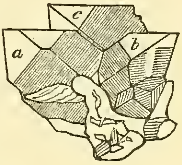
‡ Elemente der Mineralogie, 6. Ed., p. 190, 1864.

§ Poggen. Annalen, vol. xvii, p. 155, 1875.

‖ American Jour. of Science, III, vol. xii, 448, 1876, and Zeitschr. für Krystall., vol. i, p. 39, 1877.

¶ Zeitschr. für Krystall., vol. iv, p. 244, 1880. ** Loc. cit.

allel juxtaposition have the face opposite *a*, over the apex of the crystal, in common, along with a third sub-individual in part shown in the figure on the lower left hand side of *a*. These three sub-individuals have also the ∞R plane parallel to that under *a*, seen in the figure in common. The second individual *b* is also composed of three sub-individuals in juxtaposition with the principal axes parallel. Using *a* to indicate one individual and *b* the other, it is seen that *a* and *b* are in a twinned position. The faces R of both individuals which are perpendicular to the plane of the paper give a single reflection and the faces of the sub-individuals *a* and *b* lie nearly in the same plane, not parallel planes which is the general case and which is represented by the faces R of *c* and *b*. The faces R which lie nearly in one plane, indeed all the well developed faces in the group, are slightly curved and striated so that a sharp reflection cannot be obtained for the purpose of exact measurement. Such being the case the following details are given.



The faces R are not *absolutely* in one plane, careful examination showing that the faces are inclined at an angle a little less than 180°. Further, when one face is horizontal the other is very slightly twisted around an axis joining the apices of the two crystals. Moreover the faces R and −R of the two individuals are not *exactly* in a zone, or what is the same thing the planes normal to R containing the principal axis in each individual would form a very small angle at their intersection. So nearly do the individuals approach the theoretical conditions that these points are mentioned to avoid giving the impression that the group possesses ideal symmetry.

The two conditions are practically fulfilled:

1. The faces R parallel to the same plane.
2. Principal axes contained in a plane perpendicular to R.

Under these conditions the angle between the principal axes would be 103° 34′ (the supplement of the angle between R and −R over the apex) and the twinning plane perpendicular to R, which is $-\frac{5}{8}$R.

Supposing $-\frac{5}{8}$R to be the twinning plane and calculating backwards it is found the faces R, of the two crystals, would form an angle at their common intersection of 179° 34′ an angle a little less than 180° which is the case with the group here described, and that the inclination of the principal axes of the two crystals would be 104°.

It may be stated that the lower portion of the group, represented in the figure by irregular lines, consists mostly of planes having a definite orientation to the dominant individuals.

The group was found loose in the soil and there are no signs of its having been broken off another piece or that there have been any crystals attached after the manner of those in the groups from Reichenstein.

The only planes that can be definitely recognized are R, −R, ∞R. The faces R and −R possess essentially the same luster, so that there is no physical means of distinguishing the one or the other.

The foregoing article was prepared while the writer held one of the Morgan Fellowships in Harvard College, and this opportunity is taken to express the gratitude he feels to the Director of the Boylston Laboratory, Professor J. P. Cooke, for the unvarying kindness with which Professor Cooke gave aid, counsel and encouragement.

University of Virginia, May 12th, 1885.

Art. XXVI.—*Impact Friction and Faulting;* by Geo. F. Becker.

(Continued from page 128.)

The first portion of this article was devoted to a discussion of the distribution of energy in a rod of finite or infinite length, and of elastic or inelastic material, when subjected to an impact in the direction of its axis. It was also pointed out that such a rod might be divided into short portions supposed to remain in contact with one another without affecting the result. It was then shown, that if for such short rods, sheets or plates lying one upon another are substituted, each plate being constrained to move in its own plane, but presenting on its upper and lower surfaces an indefinite number of extremely small inequalities or teeth interlocking with the similar inequalities of the adjacent plates, the distribution of energy through the system of sheets caused by the impact of these teeth when the uppermost sheet is moved is the same as in a rod subjected to impact except that the axis of distribution is vertical to the line of motion instead of coincident with it. A surface armed with such minute inequalities or teeth, is the conception of a frictional surface temporarily adopted.

Application of principles of impact to friction.—Considering friction as a form of impact under particular geometrical conditions, it is easy to make applications of the results obtained for impact to frictional problems. Were the material of sheets offering frictional resistance perfectly elastic and without elastic limit, the movement of one plate over another would in general be impossible. The projections on the two surfaces would interlock and any force applied would deform the two plates as a single elastic mass. It appears indeed not beyond the bounds of possibility, that the inequalities of a surface might be perfectly elastic but of such a form as to yield passage to a body passing over them by bending and compression. Such a case would correspond to the impact of perfectly elastic bodies in which a portion of the energy is converted into vibrations; in other words to a case in which, in spite of perfect elasticity, the coefficient of restitution is not unity. Such cases, if they exist at all, must be very rare; for it is a matter of daily experience that highly tempered steel-cutting instruments are sharpened upon, and are therefore abraded by, friction upon soft and greasy leather. If on the other hand the material were inelastic, or partially elastic, work would be done on the contact and relative motion would take place. This relative movement would be greatest for total inelasticity. Partially elastic sheets would not separate like partially elastic balls, but would move relatively by an amount corresponding to the work done in members of a system of partially elastic balls. The material remaining the same and the coefficient of restitution constant, the number of minute projections on any pair of contact surfaces would make no difference in the work done at that contact though it would determine the ultimate relative positions of the centers of inertia of two sheets. The tiny projections of material surfaces are certainly not prisms with vertical sides, but irregular in shape and larger at their bases than at their summits. The closer surfaces are brought together therefore, the more such projections will interlock and the greater will be the expenditure of energy requisite to grind them off.

Corresponding interpretation of Morin's laws.—The results obtained by Morin from his own and Coulomb's experiments on friction are expressed in rules known to be approximately and only approximately true. They are that friction is proportional to the area of the rubbing surfaces, proportional to the pressure, and independent of the velocity. The first relation is evident, the second states that the interlocking of projections increases with the pressure, as it certainly must, and furthermore that this interlocking is such that the amount of work to be done increases in simple, direct ratio to the pressure; this is only approximately true. The third relation, that the amount of work done is independent of the velocity, manifestly implies the assertion that the coefficient of restitution is independent of the velocity, or that it is constant for partially elastic bodies and beyond the limit of elasticity as well as under conditions where no sensible deformation occurs. It was of course never claimed that this rule was exact or that it was applicable except for moderate differences of velocity. A great advance towards the elucidation of the true relation between friction and velocity might evidently be made by a study of the relation between the coefficient of restitution and the velocity of imperfectly elastic impinging spheres or cylinders.

Effect of extreme velocities.—If there is relative translation between rubbing masses there must always be more or less "play," or a slight possible movement normal to the frictional contact, so that the centers of inertia of such masses cannot be confined to motion in absolutely straight lines. This is of importance in the extreme cases of zero velocity and very great velocity. The coefficient of friction for rest appears from experiments to be merely the maximum value of the coefficient for motion, and its excess is almost beyond a doubt due in part to the fact that, when at rest under a given pressure, the projections of the opposing surfaces interlock more deeply than they can under the same pressure when in motion. In the case of high velocities the mean distance of the centers of inertia of rubbing masses must be greater than for low velocities, owing to the increase of the elastic rebound, resolved perpendicularly to the contact plane, when the more or less irregular inequalities impinge.* These will therefore interlock to a smaller extent, and the friction will diminish from this cause independently of any variation of the coefficient of restitution. This coefficient also stands in some not precisely known inverse relation to the velocity so that friction should be expected to be notably less for very high velocities than for moderate ones as experiments have shown is the case.

Results for a system of sheets.—If then a system of material sheets is arranged say in a vertical pile, there being perfect contact throughout, and impulsive energy is imparted to the uppermost of them, this energy according to the deductions drawn will be distributed throughout the system exactly as in a series of partially elastic spheres. Assuming that the coefficient of restitution is constant and not unity, every sheet which receives energy must move and must impart kinetic energy to the next following. The distribution of energy must be logarithmic (simply or compositely) quite irrespective of any differences of frictional resistance which may exist at different contacts. On the other hand if the friction at all contacts is the same, the movements of the sheets will be logarithmic and if the friction varies according to any law, this combined with the law of the expenditure of energy will give the locus. Morin's law that

* Imagine for example a mass of india-rubber dragged over a street pavement; the elastic mass would constantly rebound from the irregular paving stones, and the more rapid the translation the higher would be the mean position of the center of inertia of the rubber mass. Morin shows that the resistance offered by pavement to a vehicle provided with springs is less than to one without springs.

friction is independent of velocity not being rigidly correct however, if two partially elastic sheets are in contact one of them may as a matter of fact receive an impulse so slight that the elastic resistance of the interlocking projections will not be overcome. An elastic strain but no motion on the contact plane will be the consequence.

Independent kinetic theory of system of sheets.—While there thus seems every reason to suppose that friction is the resistance due to the impact of the minute projections which distinguish material surfaces from mathematical planes, the kinetic theory of a system of sheets is not dependent upon it, but merely upon the existence of some resistance which allows of relative motion yet converts "molar" energy into molecular energy. Whatever may be the nature of this resistance it cannot conflict with the principle of the stability of the center of inertia of a system upon which no external forces act and it is a necessary conclusion that, if no obstacle exists to intercept the transmission of energy, the motion of the first of an infinite series of sheets necessitates the motion of all. If these sheets are infinitely thin as well as infinite in number, the diminution of the energy between two successive surfaces, or dw can be neither more or less than wdx, and the distribution of energy is simply logarithmic. The argument may be extended to the case of a finite system of sheets exactly as the behavior of a finite rod under compression was inferred from that of an infinite rod. In short

$$\frac{c^2 d^2 w}{dx^2} = w.$$

But if the results derived from the consideration of friction as mere resistance of unknown character coincide mathematically with those deduced from considering friction as a form of impact, this conclusion is certainly strongly confirmatory of the physical hypothesis.

Elastic mass under shearing strain.—Exactly as a series of inelastic cylinders in contact (or an inelastic rod) and a similar series of elastic cylinders accurately coincide in behavior during the period which elapses between the first contact of an impinging mass and the moment of maximum compression, so also does the behavior of a series of sheets in contact represent the behavior of a solid completely elastic mass. In other words precisely as the energy actually expended in the deformation of inelastic substances during an impact may be conceived as potentialized, so also may the energy expended in friction be treated as potentialized. This statement which is almost self-evident has ample authority.* If an elastic rod of uni-

* Thomson & Tait, Nat. Phil., 1st ed., § 452.

form cross section with its axis placed parallel to X were artificially prevented from any deformation in planes parallel to its terminal surfaces and one of these surfaces were strained by a suitable stress into a position in which its central point no longer coincided with the axis, the same differential equation would express the distribution of energy. This result follows quite independently of any analogy, but the steps necessary to show it are a mere repetition of those given in detail for a compressive strain. Now imagine such an elastic rod thus strained to be suddenly intersected by any number of planes parallel to its ends. If these surfaces presented no frictional resistance the centers of inertia of the intervening sheets of elastic material will have no tendency to move and they will remain on the simple or composite logarithmic locus. Or suppose them to present a frictional resistance independent of the velocity. At the instant when the partings are formed the system is in equilibrium and the center of inertia of each sheet may be regarded as held in check by the forces exerted upon it by its two neighbors. Two suppositions may then be made: the frictional resistance may be conceived either as that presented by inelastic substances or as that presented by partially elastic substances. (If the interlocking particles were wholly elastic no motion would take place). In the former case no energy could be transmitted from one sheet to the next and the whole energy potentialized between the planes of the centers of inertia of two adjoining sheets would be expended upon the contact between them and the curve of the centers would coincide with that found when the surfaces are frictionless. If the resistance is partially elastic, e remaining constant, a certain proportion of the energy potentialized would be expended at each contact and the locus of the centers of inertia would vary from the preceding only in the value of the constants.

Coefficient of friction—If, as in the experiments to be described presently, a rigid mass W moves over a system of sheets with a fixed quantity of energy, it will move precisely as far as if the upper sheet of the system were fixed and no energy were communicated to the remainder of the system. If A is the distance which W moves and f is the frictional resistance per linear unit, the energy expended in friction is Af. If the mass of W is M and its initial velocity v and were the system of sheets totally inelastic, Af would equal $\frac{Mv^2}{2}$. Were the sheets wholly elastic (or more exactly wholly restitutional), no energy would be expended in friction and there would be no permanent deformation. (The whole energy would be first converted into "molar" vibrations, and in any actual case would pass over by friction against the atmosphere, internal friction, etc., into molecular energy). If the system is partially restitutional it is clear that

$$f \, . \, \mathrm{A} = \frac{\mathrm{M}v^2}{2}(1-e^2),$$

where e is the coefficient of restitution for velocity and $(1-e^2)$ the coefficient of expended energy. If k is the coefficient of friction and the normal pressure is $\mathrm{M}g$ (where g is the acceleration of gravitation if the pressure in the system is due to the weight of the mass W, or a corresponding velocity if the pressure is otherwise induced),

$$f = k\mathrm{M}g,$$

and if $\mathrm{A}=1$

$$k = v^2\frac{1-e^2}{2g},$$

so that the coefficient of friction may be reduced to two determinable velocities and the coefficient of restitution. Here however the extent to which the inequalities of the frictional surfaces overlap is supposed constant, or the elasticity of the rubbing masses considered as represented by their centers of inertia, is not taken into account, nor shall I attempt to discuss it.

The equation of the distribution of energy in an infinite system of sheets will then be

$$w = \frac{\mathrm{M}v^2}{2} \, . \, \frac{1-e^2}{c} \, . \, \varepsilon^{-x/c},$$

c being the subtangent. If the coefficient of friction is uniform throughout the system, the differences of the movements of successive sheets will be in precisely the same ratio as the absolute movement of the sheets, so that if y is the distance moved by any sheet and μ the ratio of the movement of adjoining sheets,

$$y = \mathrm{A}\varepsilon^{-x/c} = \mathrm{A}\mu^{-x}.$$

For this case therefore

$$\frac{f}{c} = \frac{w}{y} = \frac{\mathrm{M}v^2}{2}\frac{1-e^2}{\mathrm{A}c}.$$

The frictional resistance being uniform its actual value will affect the zero value of w, while μ and c are entirely independent of f, or, if the subscript indices 1 and 2 are used to indicate successive sheets in any portion of the system,

$$\mu = \varepsilon^{1/c} = \frac{y_1}{y_2} = \frac{\Delta y_1}{\Delta y_2} = \frac{f\Delta y_1}{f\Delta y_2} = \frac{w_1}{w_2},$$

and f appearing both in the numerator and denominator does not affect μ so long as f is constant. On the other hand if

f increases with x (as it really does, though slowly) μ and c are functions of f. If f is not uniform, the strain curve will be the result of the combination of the logarithmic distribution of energy with the varying coefficient of friction and will not therefore in general be simply logarithmic. But were the relation of f to x logarithmic, so that $f_2=\nu f$, ν being a constant

$$\mu=\frac{f_1\Delta y_1}{\nu f_1\Delta y_2}, \text{ or } \mu\nu=\frac{\Delta y_1}{\Delta y_2}.$$

It readily follows that y_1 is to y_2 in the same ratio for this case, and therefore the strain equation would be

$$y=\mathrm{A}\,(\mu\nu)^{-x/c}.$$

Now according to Bochet the locus representing the relation of the coefficient of friction to velocity is represented approximately by the equilateral hyperbola referred to axes parallel to the asymptote.*

For a series of velocities standing to one another in a constant ratio and for a uniform total pressure at all contacts, the ratio of successive values of the coefficient of friction will therefore be nearly constant, or in other words ν will vary very slowly for the range of velocities for which Bochet's formula fairly represents the facts, and the locus of the edges of the sheets will vary but little from a simple logarithmic curve. It will be shown later that while this is true for very moderate velocities, it is possible to experiment with velocities so low as to reveal the variation of ν. If f varies at different contacts, of course the ratio of successive values of w is not the same as the ratio of successive values of y.

Coefficient of restitution—If the strain is proportional to the stress in a system of uniform sheets the strain equation may be written

$$y=\mathrm{A}\varepsilon^{-x/c}=\mathrm{A}\mu^{-x},$$

where μ is the ratio of the movement of two successive sheets. The quantity c is therefore the length of the subtangent expressed in terms of the thickness of a sheet. If c is taken as unity (making $\mu=\varepsilon$) and if the coefficient of restitution is zero

$$\frac{dw}{dx}=-w.$$

* Ann. des Mines, 1861. If k_0 is the statical coefficient, and $k\infty$ the value of k for an infinite velocity and v the velocity in metres per second, Bochet's formula may be written

$$k=\frac{k_0-k\infty}{1+0.3v}+k\infty.$$

To illustrate the statement made in the text, let k_0 be 0·5 and $k\infty$ be 0·1. If the value of k is computed for $v=\frac{1}{16}, \frac{1}{8}, \frac{1}{4}, \frac{1}{2}$, 1, 2, 4, 8, 16, 32, 64, and each of these values is divided by the next following, the ratios or values of ν obtained are in the same order 1·01, 1·03, 1·06, 1·09, 1·17, 1·25, 1·27, 1·29, 1·23, 1·17.

If this last case is compared with another in which the coefficient of restitution is not zero, w' for the same value of w will evidently be smaller than the above; and, by a similar argument to that adopted in determining the total expenditure of energy, it follows that

$$\frac{dw}{dx} = -(1-e^2)\,w.$$

Here then the subtangent instead of being unity is

$$c = \frac{W}{W^1} = \frac{1}{1-e^2},$$

so that c is a function of e which can become unity only when $e=0$. The unit of measurement in this equation is the value which the subtangent would have were $e=0$ or the system wholly irrestitutional, but it is also the length of the subtangent in terms of the thickness of a sheet and consequently if the system is irrestitutional the length of the subtangent is the thickness of a sheet, and in general if the thickness of a sheet is δ,

$$c\,(1-e^2) = \delta.$$

Now from an experimental case it is extremely easy to determine c and δ so that the coefficient of restitution may readily be found for a given case from the equation

$$e = \sqrt{1-\frac{\delta}{c}} = \sqrt{1-ln\mu}.$$

For $e=1$ or for a perfectly restitutional system $c=\infty$ and $\mu=\varepsilon^0=1$ while for $e=0$, $c=\delta$, or if δ is the unit, $\mu=\varepsilon$, and these are the limiting values of μ.

Friction in terms of elasticity—The energy equation for an infinite system of sheets may therefore be written

$$w = \frac{Mv^2}{2}(1-e^2)^2\,\varepsilon^{-x(1-e^2)}.$$

and if the functional relations of e and the velocity were known w could be immediately stated in terms of velocity. On the other hand, experiments on friction are capable of yielding values of e, though whether this would be convenient and expedient method of investigation is another matter. Be this as it may, the above equations establish a simple and rational relation between friction and restitution; they show that friction may be removed from the anomalous position it has hitherto occupied, and brought under the great problem of elasticity.

Geometrical differences between systems under friction and impact.—There is a formal difference between the behavior of a finite system of sheets resting against a rigid obstacle and that of a finite compressible rod. In the latter case I have shown that the energy of an impact is to be regarded as divided into two portions acting from opposite directions upon the passive mass and that the energy curve is a projected catenary, while if the strain is proportionate to the stress, the geometrical result is of the same character. In the same way, if a finite system of sheets rests on a rigid mass and the topmost sheet is moved against friction by an impulsive application of force, this force is to be regarded as decomposed into two shearing stresses acting on the upper and under surfaces respectively in opposite directions. The energy curve is the same as for a rivet, but the geometrical result differs because, as the rivet may be conceived as at first forming part of an infinite rod and then as receiving the energy which would be distributed over the whole mass beyond the point $x=a$ from the opposite direction, so a finite system of sheets is to be regarded as at first of infinite thickness and arranging itself on the simple logarithmic curve and then as receiving the energy potentialized beyond $x=a$ in a direction parallel to that of the original impulse, but differing from it by an angle of 180°. If the strain is proportionate to the stress, the geometrical result will clearly be a curve of the form

$$y = \mathrm{A}\,(\varepsilon^{-x/c} - \varepsilon^{x/c}),$$

the origin being taken at the point of inflection which will be on the same ordinate as the horizontal point of the energy curve.

There is a corresponding difference between the results of the impact of two compressible masses and the mutual action of two systems of sheets, if the two systems move relatively on the only plane common to both. In each case the energy curves of the two systems will meet in a salient point on a line corresponding to the contact plane. For the impinging compressible masses the geometrical result will be of the same character as the distribution of energy. The two systems of sheets on the other hand will arrange themselves on two (simple or composite) logarithmic curves with opposite curvature, so that if the material of the two systems is the same the junction will be a point of inflection instead of a salient point.*

A similar reversal takes place in certain cases of what may be called negative impact. If an iron rod is brought in contact with the surface of a mass of melted glass and withdrawn, the thread obtained will be logarithmic (for evidently dw will equal minus wdx). If instead of an iron rod a glass rod is substituted, and this is allowed to remain in proximity to the heated mass long enough to become somewhat softened, the hot end of the glass rod will be drawn down by the resistance

* The subject is treated at length and illustrated in Geology of the Comstock Lode, p. 173, *et seq.*

of the weight depending from it and the curve shown by a longitudinal section of the rod will be continuous with that of the logarithmic column, instead of showing a salient point at the junction as would be the case in a corresponding positive impact. Similar principles apply under the slightly more complex conditions existing when a glass rod is softened in the middle before a lamp and drawn out from the cold ends.

Experiments on friction.—More or less exact experiments can readily be made to test the statements as to friction here made. If a flat bar (such as a standard steel foot rule) be laid upon a frictional surface (blotting paper for instance), and a yielding mass (say a rubber pencil eraser) be drawn along its upper surface, it will be found that a slight pressure suffices to set the bar in motion and that there is simultaneous relative motion between the rubber and the bar and between the bar and the blotter. If a board about as thick as the bar is broad is covered with thick paper of the same kind as that on which the bar rests, and the edge (thus prepared for the sake of a better contact) is pressed upon the bar and moved in the direction of its longer dimension, relative motion on each side of the bar will again take place. In these experiments if the pressure applied is very light, no relative movement will occur below the bar because a certain amount of energy is needful to strain the minute projections of the paper beyond their elastic limit. These trials show that motion is really produced by friction if under that term the whole phenomenon is understood, or concomitantly with friction if the word is limited merely to the dissipation of energy accompanying the action. Still further to test the character of this transmission of energy, I arranged a pile of seven steel bars one foot long $1\frac{1}{4}''$ wide and $\frac{1}{12}''$ thick, inserting between each a slip of blotting paper of the same width and length as the bars. These slips of paper were considered necessary to effect fairly complete contact which could have been attained otherwise only by grinding the bars together with polishing powder. To adjust the strips of blotting paper to the steel surfaces and to remove grosser inequalities in the paper, each bar was rubbed a few times backward and forward on the underlying slip. A long strip of wood was then laid upon the strip of paper covering the topmost bar and this was uniformly weighted over the bar with some ten pounds of lead. The wooden strip was then drawn away by a horizontal traction for three or four inches. The energy was distributed throughout the system, relative motion taking place from the first instant on 13 out of the entire 15 contacts and the ends of the bars were manifestly arranged on a curve asymptotic to their original plane and almost certainly recognizable as the logarithmic curve to an eye familiar with its form.

Still more satisfactory is an experiment with slips of paper.* If a considerable number of slips of uniform paper are arranged in a pile held down by a weight, and a blunt edge, such as that of a ruler, pressed upon them near the weight and drawn forward at right angles to the direction of the edge the fraction of an inch, the edges of the sheets will be found to occupy a curve. If the operation is repeated a number of times the curvature becomes very sensible. This curve can be plotted from the experiment with great ease and will be found to coincide most remarkably both as to curvature and the position of the asymptote with the logarithmic curve.

If the sheets were perfectly flexible the pressure throughout the system under the narrow area in contact with the ruler would be confined to nearly the same area at each contact. The weight of the slips of paper, it is true, causes an increment in pressure from the surface downward, but this is inconsiderable as compared with the combined pressure of weight and ruler. If the slips are not perfectly flexible, the pressure is spread over a wider and wider area from the top downward. This however makes no difference in the result, since the frictional resistance varies directly both as the surfaces under pressure and the intensity of the pressure per unit of area, in other words, as the total pressure which is necessarily the same (neglecting the weight of the slips) at each contact, however the pressure may be disseminated. For a single edge many edges or points may be substituted. If for instance a few dozen grains of sand are strewn over the upper sheet and a bit of plank be pressed upon the system and moved, the sheets will still arrange themselves on the same curve. Even a single point (a pencil point) will bring about the same result. On the other hand, if the weight is uniformly distributed over the whole upper surface and drawn away, the system will break at one or a few contacts only. The experiments indicate what might have been supposed *à priori* the reason of this failure. Films of air occupy the by no means infinitesimal intervals between the sheets and, unless these are broken by localized pressure, such as is produced by a point, edge or narrow weight, these films are shifted irregularly and prevent the uniform action of friction. The phenomena are then comparable rather with those of lubricated surfaces rather than with dry friction. An almost self-evident modification of the experiment confirmatory of this explanation may be effected by setting the blunt-edged instrument in the line of motion instead of at right angles to it, so that the pressure is uniform from end to end of the sheets beneath the edge. If the edge slips too easily, or is too smooth to communicate the necessary amount of energy to the

* Described at length in Geol. Comstock Lode, p. 165.

system, sufficient friction can be induced by fastening round it paper of the same kind used for the sheets. The resulting curve is the same as that produced by applying the edge at right angles to the direction of motion.

In experiments with slips of paper it is easy and desirable to apply the pressure and draught mechanically, although an excellent coincidence with the logarithmic curve can be obtained with a ruler merely held in the hand. If in mechanically conducted experiments the velocity is about that which one would naturally employ in a trial by hand, say six or eight inches per second, the coincidence with the logarithmic curve is almost perfect.* If, however, by a very light pressure and slow mechanical movement with an edge or soft blunt point the uppermost sheet is given an extremely slow motion so that the rate of movement of say the tenth sheet is scarcely perceptible, the curve produced has a subtangent which increases rapidly from the first sheet onward. So also must therefore the quantity I have called ν and with it the coefficient of kinetic friction for these extremely low velocities, or as the value of the statical coefficient is approached. Judging from this indication it would appear that a good empirical approximation to the relation of k to v might be obtained by adopting the tractory (or "anti-friction" curve $k = c\,\varepsilon^{-s/c}$) rather than the hyperbola to represent experimental results, but the value of k in terms of v might then be too complex for practical use. The value of the coefficient of restitution indicated by experiments on paper is as it should be, very high. In my former paper I noted a case in which μ was 1·4. This answers to $e = \cdot81$. With other qualities of paper I have obtained values of μ indicating a restitution of considerably above 90 per cent.

Conclusions reached.—In the foregoing pages the attempt has been made to approach the subject of friction as a problem in elasticity. I began by discussing the distribution of energy in an elastic compressible rod and showed that this is represented in general by the simple equation $w = c^2 w''$. Applications of this equation to some special cases were made and some of the modifications necessary for incompressible substances were pointed out. By regarding friction as the impact of the minute inequalities of material surfaces, a passage was effected from impact to friction and from a compressive to a shearing strain. The significance of Morin's laws under this supposition was enlarged upon. It was then shown that the same conclusions followed independently of the physical hypothesis adopted, a fact regarded as confirmatory of this hypothesis. It was at last

* See Geol. Comstock Lode, p. 167, fig. 4.

found that the formulas for the distribution of energy in a system of sheets could be reduced to terms of the total energy and the coefficient of restitution which brings friction under the head of the general elastic problem and accomplishes a main purpose of the inquiry. Various experiments designed to test the analytical results were then described. It only remains to draw some general conclusions as to the character of friction and to point out the application of the results to the geological problems which led to the investigation.

Character of Friction.—The term friction seems to be somewhat variously applied in literature, sometimes indicating the whole resistance offered by a body to the passage of another over its surface and sometimes only the surface resistance. It appears to me more convenient to restrict it to the latter sense, because in this case it is measured by what is known as the coefficient of friction. When one body tends to move over another, the contact being frictional, the resistance of the passive mass, as in any other case of motion, is the resistance which it offers to acceleration, or its reaction against the active force. The active force in this case is a shearing stress and its effect is a shearing strain which is distributed throughout the entire passive mass. The resistance as a whole, therefore, is a counter shearing stress, while friction considered as a surface resistance is the value of this counter-stress for a particular plane. If the passive mass is nowhere strained beyond its elastic limit no relative motion can take place under ordinary circumstances and the electric resistance of the minute inequalities of the frictional surface is what is known as statical friction. If the stress is sufficient to strain the minute inequalities beyond their elastic limit, or to bend and compress them out of the way, relative motion takes place. The conceivable case of surfaces wholly elastic but only partially restitutional, which would allow of relative motion, does not seem to occur, though in some instances it is approached. The imperfectly restitutional resistance of the minute projections is kinetic friction, and as a matter of experience is always attended by permanent deformation. It must in all cases be accompanied by a strain of the entire passive mass, though this strain may be elastic and commonly is so. But if the passive mass is itself a system presenting planes in which resistance is inelastic, motion and permanent deformation must occur on them also. The supposed entire inelasticity, like perfect elasticity, is ideal. The fact that any surfaces, however inelastic, produce sound when undergoing friction shows that the material is capable of molar vibrations. It is fair to conclude that the coefficient of restitution also never reaches its limiting value, unity, which is of course also inferable from experiments on impact. If the resistance on the planes above referred to is partially elastic, motion and permanent deformation would always occur on them also were the coefficient of restitution and therefore also the coefficient of friction independent of velocity. Motion and permanent deformation will actually occur on such planes unless the strain falls short of the elastic limit or, practically unless the strain is extremely slight. The frictional resistance depends upon the geometrical form and absolute dimensions of the minute projections on the rubbing surfaces, the extent to which they interlock, and upon their coefficient of restitution. To a certain extent surfaces rubbing will polish one another and friction will progressively diminish, but the dust resulting from attrition will always suffice to produce new inequalities, while the smoother the surfaces become the greater must be the influence of the welding which according to Messrs. Thomson and Tait occurs even in the mere impact of polished spheres. A tearing of the surface must follow cohesion at any point, so that this cause also must continually interfere with the reduction of the surfaces beyond a certain limit of smoothness. The character of the surface of minimum friction resulting from the action of friction itself is probably open to investigation. It will evidently be closely connected with several of the physical properties of the material. In any inquiry into friction it should not be forgotten that the extent to which the inequalities overlap is in part determined by the elasticity of the whole masses in contact, which must therefore be taken into consideration as well as the coefficient of restitution of the inequalities of the surfaces.

Geological application.—Suppose a mass of rock in place is divided by any means into parallel sheets: let the country adjoining one side of this system of sheets be solid; and let the whole system be held together by a pressure normal to the fissure planes. If the solid mass adjoining the system of sheets is forced to rise, the conditions discussed at such length in the foregoing pages are present. The energy of the moving mass will be distributed through the system logarithmically. If the system is infinite, or contains a considerable number of sheets, and if the coefficient of restitution is constant, the distribution of energy will be represented by the simple logarithmic curve however the friction may vary. If the friction is the same on all the contacts, the geometrical result will be represented by the same curve. If the friction remaining constant the system is regarded as including a finite number of members the geometrical results will be represented by the difference of two simple logarithmic curves. These are the three fundamental propositions asserted, and so far as I can see rigidly proved in my former paper on faulting, but reached by a wholly different method from that here adopted. New experiments as well as new proofs are given above and the analysis is further extended. That a fault under a compressive stress in a mass of rock cut by a single fissure will tend to produce a system of parallel fissures, I showed in my former paper; I have nothing to add on that subject. This fact explains the formation of parallel veins, the parallel fissure of step-faults and the sheeted structure of landslides which are merely superficial faults.

Faults are known to be in a large proportion of cases the result of a tendency to fold carried beyond the limit of elasticity of the rock. The simplest case of faulting is therefore an anticlinal fault passing over into a fold at its extremities. I have shown that under certain conditions instead of a single fissure a system of parallel fissures will be formed, and that the movement will be distributed over all of them. The upper end of any sheet will then remain parallel to its original position. In a stratified country intersected by such a fissure system the result is, strictly speaking, a system of monoclinal faults. The divisions of the step-faults which I have personally inspected, however, are all too insignificant in thickness to be ordinarily or conveniently classified as monoclinal faults. That this is not always the case is apparent from Mr. Geikie's illustration* of a step-fault in the coal fields between Linlithgow loch and the Frith of Forth. The logarithmic character of this fault is manifest from the illustration, while, if I understand the geography aright, the separate benches must be as much as a quarter of a mile in width. Even if the section is not to scale, the width of the sheets must evidently be such that no one would hesitate to include the case among monoclinal faults. In this particular instance the strata are practically horizontal. This is a matter of no consequence however, for, had they been inclined previous to the intersection by the fissure system, they would still be inclined at the same angle. Thus it is certain that some monoclinal faults are explicable on the theories set forth above and their relation to anticlinal faults is at once evident. They result from the substitution of a system of fissures for a single fissure in an anticlinal fold followed by the distribution of motion over all the contacts. In all such cases it is unnecessary to suppose a tilting of the monoclinal blocks in a plastic magma after separation from the adjoining country, or to conclude that the fissures have gaped to allow of such tilting; for step-faults involve the compressive strains which most geological phenomena lead us to suppose the rule in geological dynamics. A series of blocks tilted after separation from the surrounding country also leave a triangular space at each end of the system which must apparently fill with the plastic magma, a complication which will not be met with in step-faults. This

* Textbook of Geology, p. 532.

fact affords a distinction which may in some cases serve to decide the character of a particular fault.

Since different lines of research lead to the principle of the logarithmic distribution of energy in systems of sheets connected by friction, and since this result is confirmed by experiments and observation it appears to me that it may fairly be claimed as established and as affording a valid explanation of the structural problems presented by step-faults, parallel systems of veins, and of some monoclinal faults and landslides.*

I shall take advantage of the first space which may be allowed me in this Journal, to show that the same principles apply to another and very different problem in structural geology, and to present an entirely fresh proof of the fundamental equation.

Office of the U. S. Geological Survey,
San Francisco, February, 1885.

* Mr. Ross E. Browne, of the University of California, has published an attack upon the theory of step-faults given in my report upon the Comstock Lode (this Journal, vol. xxviii, p. 348). I feel great diffidence in opposing my opinions on this subject to those of an author who writes in so authoritative a tone, for my own knowledge of the science of mechanics is rudimentary. I am nevertheless forced to the conclusion that Mr. Browne has failed to perceive the full significance of Morin's laws. To grant that friction is independent of velocity and yet to deny that there will be motiou on each contact of a system of sheets, such as that discussed above is simply to deny the presence of a stress, for this independence implies movement (a strain) however slight the stress. But to deny a stress under such conditions would be to deny the principle of the equivalence of action and reaction. So, too, Mr. Browne disputes the applicability of my experiment on sheets of paper as a test of the theory proposed, because the pressure of a blunt edge will not be confined strictly to the same area at each contact. Yet this can make no difference under Morin's laws, since under them the frictional resistance at any contact depends upon the total pressure exerted upon the entire area of the frictional surface and not in the least upon its distribution. Morin's laws formed the basis of my analysis as was stated over and over again in my former paper, though I endeavored to experiment in such a way as to be as little dependent upon them as possible.

Rejecting my explanation of the curve obtained in the experiment Mr. Browne offers as an improvement one founded on what appears to me a strange physical hypothesis and which leads him to the hyperbola $y=c/x$. In my report, p. 167, I compared the experimental curve with this locus, and showed that the position of the asymptote is fatal to the supposition. If this is not regarded as a sufficient objection the incorrectness of the solution will be evident when it is considered that even if the equation is given the form $y=c(1+x)^{-1}$, $\sum_{0}^{\infty}(1+x)^{-1}=\infty$; so that the hyperbolic curve could only result from the application of an infinite amount of energy. In short, without going into further detail, I fail to see that Mr. Browne has pointed out any error in my work or that he has thrown any additional light on the subject.

Art. XXVII.—*Note on the Transmission of Light by Wire Gauze Screens;* by S. P. Langley.

In the beginning of the present year, a friend sent me a series of wire-gauze screens, which he used to diminish the apparent brightness of stars in making meridian observations, with a request that I would determine photometrically the amount of light transmitted by them. As such screens are occasionally employed in astronomical work, particularly in the use of the heliometer, I have thought the following account of our experience of sufficient interest to make public.

I used for the measurements a photometer box originally constructed for another purpose, and an opaque wheel or disc, having radial slits of variable width, which placed in the path of a ray of light and rotated with sufficient velocity, can be made to reduce the light to any desired fraction of its original intensity. (This I have employed for some years for photometric measurements when it is desirable to avoid the use of polarizing apparatus.) In the center of the photometer box was a sliding Bunsen disc which could be viewed from above by a suitable arrangement of mirrors. The open ends of the box were directed to two opposite windows, and the disc placed in such a position that its sides were equally illuminated. The wire screen was then placed over one end of the box, the wheel photometer in front of the other end, and the apertures of the latter altered until the equality of illumination of the Bunsen disc was restored. The screen then cut off the same amount of light as the wheel. From several series of measurements made in this way it was found that

1 screen transmitted	·395 ± ·004	of the incident light.
2 screens superposed transmitted	·144 ± ·004	" "
3 " " "	·052 ± ·003	" "

These numbers, as was to be expected, are nearly in geometrical progression. The screens were returned to the sender and the results communicated to him, but he wrote that upon trial, he found the reduction of light very much greater than the above values, three superposed screens reducing the light of a star by 7·1 magnitudes, which corresponds to a transmission of only ·0014.

I was at that time absent, and my assistant, Mr. J. E. Keeler, undertook the investigation of the cause of the discrepancy, which he attributed to loss of light by diffraction under the circumstances in which the screens were used by their owner, i. e. in front of the object-glass of a telescope directed upon a star. With diffuse light such as was used in the measurements with the photometer box, no loss due to this cause was possible.

In the apparatus devised by Mr. Keeler for an experimental determination of the loss by diffraction, the star was replaced by an illuminated pinhole in the focus of a 3-inch collimating telescope. This was viewed by an observing telescope of nearly equal size, in the eye-tube of which was an unsilvered plane-glass mirror, which reflected into the eye-piece a comparison star—the image of an illuminated pinhole produced by a collimating telescope at right angles to the other two. In the path of the rays from this telescope could be interposed the wheel photometer. The light before entering the first pinhole suffered reflection from an unsilvered glass surface in order to reduce its intensity to that of the comparison star.

The two images in the field of view having been adjusted to equality, the wire-gauze screen was interposed between the object glasses of the collimating and observing telescopes, reducing the light of the star and producing around it the well known diffraction image of a network. The wheel photometer was then introduced, and the intensity of the comparison star reduced until it was equal to the central image of the other. By enlarging the pinholes until the superposition of the colors produced white light, the intensity of the diffraction images could also be estimated.

It was thus found that the central image had only ·175 of its original brightness, which would therefore be the proportion transmitted by the screen under these conditions, and that the brightness of each of the four first spectra was ·05 of that originally possessed by the central image. Two thicknesses of the wire-gauze transmitted barely ·02 as measured by the intensity of the central image.

The screens with which these experiments were made were much coarser than the original ones, and it was expected that the effect of diffraction would be less pronounced. The transmission of one thickness. measured by the photometer box, was ·47, of two thicknesses ·21.

Finally the apertures of the screen and the diameter of the wire were measured by a micrometer microscope and the apertures found to occupy ·465 of the total area of the screen.

It was concluded therefore as the result of the experiments:

1. That the transmission as measured by the photometer box was equal to the ratio of the sum of the areas of the apertures of the screen to its total area, and therefore could be considered to be the true transmission of the screen, and

2. That the much smaller transmission of the screen, when used in front of the object-glass of a telescope to diminish the apparent brightness of a star, is satisfactorily accounted for by the loss of light caused by diffraction under these circumstances.

3. That screens used for this purpose should have their con-

stants determined by special experiments of the nature of those just detailed, and that their photometric use should then be limited to the reduction of the light of bodies possessing a small angular magnitude.

Allegheny Observatory, July 13, 1885

ART. XXVIII.—*Geological Relations of the Gypsum Deposits in Cayuga County, N. Y.;* by S. G. WILLIAMS.

IT is well known to geologists that the strata containing the gypsum deposits of New York, ranging from Madison County westward, at the time of the State Geological Survey were all assigned to the third of the four groups into which the Salina Period was divided; and that these gypsum deposits were described as isolated masses of possibly concretionary character contained in the enclosing beds. The gypsum beds a short distance north of Union Springs, Cayuga County, are much the most extensive of these deposits, and as they afford a fertilizer of approved quality, they have been largely laid open by workings prosecuted during more than fifty years. A rock cutting made by the Cayuga Lake Railroad, along the bank of the lake within eighty rods of the largest quarries, has also laid open the strata which underlie the gypsum beds. A recent examination of the gypsum beds and their enclosing strata, made with these more recently afforded aids to investigation, and accompanied by fortunate discoveries of somewhat abundant though often badly preserved fossils, has not only shown that the gypsum in this locality exists in continuous beds of a considerable degree of regularity, but has also afforded reasons for believing that this portion of the gypseous series belongs rather with the Lower Helderberg than with the Salina.

A section at this point eastward, and thus nearly in the line of strike, from the level of Cayuga Lake to the top of the Oriskany sandstone, is about 114 feet thick, and consists of the following members, numbered upward from the lake level:

No. 8.	Oriskany sandstone in a single fossiliferous seam,	3 feet 8″.
" 7.	Drab limestone, upper beds with thin undulating laminæ: exposed,	10 feet.
" 6.	Limestone revealed only in occasional outcrops, leveled with Locke-level, about	46 "
" 5.	Thick-bedded blue limestone, containing *Meristella lævis*, *Orthis oblata*, small form, *Rhynchonella semiplicata*, *Strophodonta varistriata*, etc.,	10 "
" 4.	Drab limestone, holding a branching fucoid and *Nucleospira ventricosa*,	4 "

No. 3. Measures concealed, leveled with Locke-level, 20 feet.
" 2. Thin-bedded blue limestone terminating below with a seam 2 feet thick, 4 "
" 1. Drab limestone unevenly bedded with two or three thin blue seams, to Lake-level, 16 "

The mean of four readings with the aneroid agrees well with the total thickness here given. The beds No. 1 have yielded fossils at three points, all at about the same level near the middle of the series, viz: one head of *Eurypterus remipes*, and tolerably abundant *Leperditia alta, Nucleospira ventricosa* and *Meristella bisulcata.* Although the two species last named are but imperfectly preserved, still the comparison of a considerable number of specimens, among which are some fair casts of the interior, leaves no good reason to doubt their specific identity; besides which, my own opinion is confirmed by that of the experienced paleontologist, Mr. R. P. Whitfield, to whom a number of the specimens were submitted.

Besides the irregularities in the bedding of No. 1 mentioned in the section, the strata in this region are occasionally affected by local disturbances. The most common of these is a sudden tilting of the beds at a low angle, continuing sometimes a number of rods, which is caused apparently by a failure of support from below. One such disturbance occurs near the middle of No. 1, causing the beds to dip very perceptibly to the southeast for a short distance. More considerable disturbances of like character affect the Corniferous limestones of Union Springs, about two miles south of the plaster quarries, two of which have come under my notice. One of these, which was mentioned and figured in the Report on the 3d District, and by which the limestone is caused to pitch suddenly south at an angle of 13°, has within the last two years, been more fully developed by the opening of a large quarry immediately south of the disturbed beds. In this quarry, which is capped by a considerable thickness of Marcellus shales with a band of concretionary limestone, is revealed a flat-topped anticlinal arch with an E.-W. strike, the southern limit of which has not yet been reached in quarrying, while the northward dipping side with an angle of 20° is near the junction with the southward dipping beds described by Vanuxem. In one of the most extensive plaster quarries also, there occurs a gentle anticlinal with meridional strike, through which the present working floor of this quarry and the one to the north of it, is made to dip eastward at a small angle as far as the workings extend; and if this dip continues, it will increase by a number of feet the space between Nos. 2 and 4 which was found by leveling to be 20 feet. The knowledge of these occasional irregularities demanded caution in assigning the gypseous series, with an average thickness of about 25 feet, to its proper place in the section, especially since the space of nearly a quarter of a mile between the fossil-bearing beds on the lake shore and the gypsum quarries, is concealed by drift. Fortunately the valley of a brook, separated only by a single field from the nearest quarry at this point, affords a continuous line of outcrop from the fossiliferous limestones on the lake to the top of No. 2, by which its continuity is assured. The top of No. 2 is 20 feet above the lake-level, which is also the height of the floor of the nearest plaster quarry. Add to this the fact that the character of No. 2 corresponds with that of the bed of tough blue limestone which forms the bottom of all the southern quarries, and there is no reason to doubt that the plaster series belongs in the covered space between Nos. 2 and 4, widened probably by the local easterly dip mentioned above,—a position to which, in the absence of any local irregularities, it would be unhesitatingly assigned by any geologist.

There is then no doubt that the gypsum deposits here form a part of the fossiliferous series, lying above beds containing *Eurypterus*, *Leperditia alta*, *Nucleospira ventricosa* and *Meristella bisulcata*, and below, or in close connection with, beds containing well-marked fossils of the Pentamerus and Shaly Limestone horizons of the Lower Helderberg. It should also be borne in mind that, apart from the gypsum beds, the entire section from the lake level to the Oriskany sandstone, is made up of drab limestones with frequent blue seams, sometimes of considerable thickness, as in No. 5. Some of these limestones, both at the bottom and top of the series, are highly laminated, showing thin layers of slightly different colors, and nearly all hold a considerable amount of impurities. A test which was made of No. 1 showed 25½ per cent of insoluble matter, so fine as to cause difficulty in filtering; while some of the higher beds above No. 5 are said to have been burned for hydraulic lime at an early day, yielding a cement of good quality, thus showing that they are probably no purer than No. 1. There is indeed, even if we set aside the fossil evidence now gained as to geological age, no such lithological change in the limestones as to warrant the reference of the lower portion of them to the Salina Period, and the upper part to the Lower Helderberg. Nor is it likely that any such reference would ever have been made had it not been for the presence of the gypsum deposits. These deposits in the regions both east and west of Cayuga county appear, from the state reports, to occupy a pretty definite place in strata bearing intimate relations with the shales of the salt group; to occur in irregular masses enclosed in marly shales whose lamination they sometimes share and sometimes disturb; and to be divided often into two "ranges" by a peculiar porous or vesicular lime rock, or by shaly limestones, holding indications in hopper-shaped accretions of the action of saline waters. It was natural, therefore, in the absence of any evidence to the contrary, that the occurrence of gypsum in any additional locality not obviously removed from its usual horizon, should lead to the reference of both gypsum and the accompanying strata to that usual horizon. A brief description of the gypseous series here however will show, I think, that its structure bears no very close resemblance to that of the gypsum of the Salina Period; that its character is intimately related with that of the accompanying limestones; and that both character and structure tend to indicate for it a different geological horizon, if not a different origin, from that of most of the other gypsum deposits.

It may be said at the outset that the gypsum deposits of this region are not irregular masses: they have no relations therefore with marly strata surrounding and enclosing them; they are not associated with any "vermicular lime rock" within them, nor with anything answering to the 4th or Magnesian division of the Salina lying above them. In one point only do they bear a superficial resemblance to the deposits elsewhere: they occur in two "ranges," or rather beds, separated from each other, however, not by shaly or vesicular lime rock, but by a bed called *slate* by the quarrymen, made up of thin seams of gypsum, interlaminated with other layers of shale.

The gypseous series here, as revealed by workings mostly confined to the vicinity of the lake, but extended northeastward nearly three miles by occasional pits, from Yawger's Quarry on the south, the bottom of which is 25 feet below the lake level, to the northernmost ones of Mr. Fitch and others which are more than 100 feet above the lake, has a very uniform character, consisting as it does of three persistent and tolerably regular members. *First* occurs the lower seam of gypsum, highly laminated and separating into several distinct layers, somewhat harder than the upper seam, and of an usual thickness of seven feet, varying but little in this respect. The existence of this seam appears, from the statement of the owner of most of the quarries, not to have been suspected forty years ago. *Second:* upon this rests a stratum usually about three feet thick called *slate* by the quarrymen, consisting of alternating laminæ of gypsum and shaly matter, and said to be gypseous enough in the northernmost quarries (which are now little worked) to be ground for plaster. *Third:* the upper gypsum bed which closes the series, varies much in thickness, from nothing to upwards of 20 feet, averaging possibly 15 feet. Its variability in thickness is probably due mostly to denudation, since it is capped by yellow drift clay in nearly every place where it is laid open by workings; the only exception that I could find being in the two northern quarries called the Fitch quarries and which are but a few rods distant from each other. Here the gypsum, which occurs in several thick layers not well enough revealed to admit of definite measurement but showing at least 12 feet, has above it about six feet of black *dirt* resembling an impure leached gypsum which it probably is, eight feet of very shaly drab limestone, and three feet of somewhat firmer drab limestone containing *Spirifera Vanuxemi*, a Lingula apparently undescribed, somewhat smaller and more oval than *L. spathata*, fragmentary impressions of a Rhynchonella not sufficient for determination but having some resemblance to *R. mutabilis*, and a slightly tapering fragment $2\frac{4}{5}$ inches long, nearly an inch wide at the widest end, and $\frac{7}{8}$ inch wide at the narrowest, and marked lengthwise with 12 shallow furrows; this may possibly prove to be a plant allied to Calamites. These fossils have been discovered since the first part of this paper was written; but their bearing on the question of geological age will be obvious. The upper gypsum bed shows little disposition to separate into distinct layers save in the northern quarries, and is softer and somewhat less dense than the seven-foot bed. It was thought also to be of better quality until analysis showed it to be nowise superior. All the members of the series show occasionally small spots and thin scale-like laminæ of sulphur, more especially on dirt seams. The upper bed, I am told, contains more of this sulphur than the lower, and the slate seam more than either. The gypsum of both seams varies from a light to a somewhat dark gray.

The gypseous series here shows therefore no tendency to form isolated masses, save where denudation may give it that appearance, in which case it is enveloped in drift clay. Two proprietors of long experience however inform me that the entire set of beds is occasionally cut across by what are called "mud seams" from one to five feet wide, that the mud seams are often of thin-laminated structure and sometimes contain a little gypsum and selenite, and that the gypsum beds abut against them regularly on both sides. The only example of this kind of replacement that has come under my notice, was in the edge of one of the quarries where, at the time of my last visit, the lower gypsum bed and the "slate," the only members there present, had suddenly given place below to black thinly-laminated mud, and above to harder thin-bedded ferruginous shale, the "mud seams" abutting against the gypsum and "slate" in a reëntrant fashion. The lamination of the mud appeared to correspond to that of the gypsum against which it abutted, and one block was hard gypsum at one end and black laminated mud at the other, although elsewhere the gypsum and mud were separated by irregular joint-like cracks. I was inclined to attribute the change to the action of water penetrating to the beds through crevices in the clay cover.

The limestone of that part of the series in question which encloses the gypsum beds is of a prevailing drab or ash color, with a few blue seams, of which No. 2 of the section is the only important one. It is often highly laminated; has a considerable amount of impurities as has already been said; and by reason of the earthy character of these impurities, it shows such a disposition to absorb water as to unfit it for all but the roughest purposes. A fragment of No. 1 gained in weight 3 per cent by two hours soaking, while a like fragment of the blue limestone No. 2 showed no appreciable gain in the same time.

The character of the limestones just described seems to me to throw light on the question of the origin of the gypsum beds; these I think have obviously been formed from the earthy drab limestones of the horizon at which they occur, as the result of the action upon them of acid waters originating in sulphur springs, which are still somewhat abundant in this region, and which it may be presumed were more abundant at an earlier geological date. The porous character of the drab-colored limestones would facilitate such a transformation, under favorable circumstances; while the imperviousness of the blue limestone which underlies the series would limit it below. Reasons for this opinion as to origin may be found: first, in the striking similarity in structure between the lower gypsum and the associated drab limestones, both having the same highly laminated character, while both the lower gypsum and the northern part of the upper, are also distinctly bedded; second, in the structure of the intermediate bed, containing as it does alternate layers of gypsum and shale, as if whatever was lime in an impure shaly limestone had been transformed to gypsum, leaving the remainder unchanged; third, in the presence in all the gypsum beds of native sulphur which would be difficult to account for on any theory of origin which should not include the action of sulphuretted waters; fourth, in the composition of the gypsum itself, which is gray, and like the limestones somewhat impure, containing in commercial samples an average of 80½ per cent of lime sulphate, with 14 per cent of earthy matter, 5 per cent of lime and magnesian carbonates, and, quite significantly, .6 per cent of lime phosphate and organic matter, these last ingredients suggesting an organic origin, while the residual lime and magnesia point to the probable original condition of the deposit. It may be added in this connection that in the limestone No. 1, in close proximity to one of the fossil localities and nearly at the same level, occurs a small isolated mass of decomposed gypsum possibly 10 cubic feet in dimensions, which is due apparently to the agency of a small sulphurous percolation now extinct.

I believe, therefore, that the structure of the great gypsum deposits of Cayuga Co. separates them sharply from those existing elsewhere in New York in strata of the Salina Period; and that their association with limestones, both below and above them, containing fossils of the lower divisions of the Lower Helderberg, as well as the nearly uniform character of the rock series from the lake level to the Oriskany Sandstone, vindicates for them a place in the lower portion of the Lower Helderberg in which I include the Water Lime Group.

A word in conclusion as to the dip of the strata at this point. It will be seen that the top of the Lower Helderberg limestones in the line of section is 110 feet above the lake. About 2½ miles south of this point, these limestones disappear beneath the lake, giving a dip of more than 40 feet per mile. This corresponds with some determinations of dip published in this Journal, October, 1883, made on a belt fifty miles long from east to west, and from six to ten miles wide, lying six miles south of Union Springs. I have at present no data for ascertaining whether the effective dip in the intermediate space corresponds with these determinations.

Art. XXIX.—*On the variation of the Magnetic Permeability of Nickel at different temperatures;* by Charles A. Perkins.

In the year 1820 Oersted published his discovery of the action of an electric current upon a magnet and Arago discovered that the current would magnetize an iron bar. The same year Ampère proposed his theory of molecular currents to explain both phenomena. Poisson soon after elaborated a theory based on the existence of magnetic fluids, but the tendency has always been toward the acceptance of Ampère's hypothesis and later physicists have striven principally to modify and enlarge it, to make it correspond with the known facts of magnetism.

It was upon this as a basis, that Weber constructed his theory assuming the existence of molecular currents which an inductive force could so arrange as to produce a magnet, while they resisted this tendency to a certain degree.

With Maxwell's additions this theory gives a rational formula which agrees much better with the known facts than do many of the empirical formulas that have been proposed.

All the formulas introduce certain constants depending on the relation between the magnetizing force and the magnetism produced, to find the value of which, as well as to test the theories themselves, experiments have been made repeatedly.

The first series of experiments undertaken in a really scientific manner are those made by Lentz and Jacobi.*

The method employed by them was to place a bar of iron in a helix of wire and to pass through this a current whose strength was measured by the attraction between two spirals through which it passed. The magnetization was measured by the induced current produced in a coil surrounding the iron bar and the magnetizing spiral. In this way they arrived at the result that the total magnetization was proportional to the strength of the current.

Joule† discovered, in experimenting on the strength of electromagnets in 1839 and 1840, that he could not indefinitely increase their carrying power by an increase in the current, but that it approached a maximum as the iron became "saturated." He also clearly stated the law that the maximum is independent of the length of the magnet and varies directly with the smallest cross-section.

But although Joule's experiments established the fact that the magnetization of iron was not proportional to the current strength, yet the principle was already stated in a paper by Ritchie‡ in 1833, six years before the publication of Lentz and Jacobi's results.

He made use of two horse-shoe electromagnets of very different lengths and an armature, all of the same metal. Then putting a different number of coils of wire about each magnet, he connected both at the same time with a battery and found the shorter one had twice the power of the longer. Then keeping the connections the same he joined a larger battery and found that the long magnet had increased in proportion much more than the short one. He states that this is because the particles in the short magnet were arranged by the first current in the position to give the best effect, so that the stronger one could not add much to it.

Plücker§ found in 1848 that bodies which at a distance were attracted by a magnet were repelled when brought very close. This was true of charcoal and a number of organic substances. This was ascribed by him to the different rate of increase in magnetization as the magnetizing force increased, and proved not only that the magnetization was not proportional to the force but that it might be entirely reversed.

* Pogg. Ann., vol. xlvii, 1839.

† Phil. Mag., [4], vol. ii, 1851. Annals of Elec., vol. iv, 1839.

‡ Phil. Trans., vol. cxxiii.

§ Pogg. Ann., 1848.

Three years later, Faraday* in attempting to classify magnetic substances: glass, water, etc., found that the order was different at different distances from the magnet, showing that the increase of magnetization was not directly as the magnetizing force and that the law was different for different materials.

In 1849 Müller† undertook an extensive series of experiments fully establishing the fact that the magnetization of iron was a complicated function of the magnetizing force and proposed the equation $I=220d^{\frac{3}{2}} \tan \frac{m}{00005} d^2$ as sufficiently representing the experiments. His method consisted in observing the deflection of a needle by a bar of iron magnetized by a helix through which a measured current was passed.

In 1852 W. Weber‡ stated his theory of induced magnetism and proposed an equation based upon the same. He also made experiments to find the connection between the theory and observation. The method employed by him was the deflection of a needle by a long thin rod closely surrounded by a spiral which extended also beyond the rod. The magnetizing current was measured and its force calculated so that the whole might be reduced to absolute measure. He found a fair agreement with the theory and determined the maximum magnetization possible for iron.

Beetz§ in 1860 showed most conclusively that iron had a maximum magnetization and he perhaps approached it as nearly as is possible. A fine line was made on a varnished silver wire and iron deposited electrolytically while under the influence of a magnetizing force and thus the molecules were so arranged as to give the greatest effect. In this way a high degree of permanent magnetization was given to the filament of pure iron and a stronger magnetizing force only served to slightly increase the temporary magnetization.

More recent experiments‖ have showed that the law of variation proportional to the current was not true even for weak currents, the increase being at first more rapid, then less so, than this law would allow. This was most noticeable in the experiments of Quintus Icilius¶ to which attention was forcibly called by Stoletow in a paper which appeared in Pogg. Ann., vol. cxlvi. Instead of a cylindrical bar he used a very long ellipsoid, thus avoiding the error of distribution neglected by his predecessors. The magnetism was measured by the direct action on a needle or by the induced current produced in a helix about the inducing spiral.

Stoletow's paper contains valuable discussions of previous experiments and important results obtained by himself. The

* Exp. Res., vol. iii, p. 503, 1851.
† Pogg. Ann., vol. lxxix.
‡ Pogg. Ann., vol. lxxxvii.
§ Pogg. Ann., vol. ciii.
‖ Wiedemann Galvanismus, vol. ii, p. 350.
¶ Pogg. Ann., vol. cxxi.

method used by him was suggested and developed by Kirchhoff* in 1870, and consisted in magnetizing a ring of metal by a coil about its entire length and then measuring the current produced in a secondary coil when the primary current was reversed. This method possesses decided advantages over all previously used in important particulars and is capable of giving results of great regularity. He arrived at results similar to those of Icilius, but found certain irregularities for which he was not able to account.

The same year Prof. Rowland† had made experiments using the same general method, but introducing various modifications. An earth inductor was placed in the secondary circuit by which the induced currents could be directly compared with those produced by a coil rotated under the influence of the earth's action and thus reduced immediately to absolute measure. In the same circuit was included a small coil of wire, sliding upon a magnet. By the motion of this coil the galvanometer needle is under perfect control and can be instantly stopped at the middle of the scale. He wound the primary coil directly upon the ring, while in Stoletow's experiments wooden rings were placed inside the coil.

Rowland also suggested plotting the results with respect to the magnetization and the magnetic permeability as coördinates, thus obtaining a curve which gave a definite value for the maximum magnetization possible.

The results reached by him in the experiments on iron were as follows:

For weak forces all the magnetism is temporary.

The value of the permeability rises very rapidly at first, reaching a maximum when the magnetization has the value $\mathfrak{B}=6000$ and then diminishes, approaching 0 as $\mathfrak{B}=17500$. This, therefore, is about the maximum magnetization of ordinary iron, giving a sustaining weight of 354 lbs. per square inch of least cross section.

(This is in C. G. S. units. The original results were on the M. G. S. system, and as the dimensions of $\mathfrak{B}$ are $[L^{-\frac{1}{2}}M^{\frac{1}{2}}T^{-1}]$, the original figures are to be divided by ten. μ is of zero dimensions.)

The curve in form resembles a slightly inclined parabola, but is more accurately represented by the equation

$$\mu=2475 \sin\left(\frac{\mathfrak{B}+1{\cdot}094\mu+500}{100}\right).$$

In some experiments the curve finally became curvex toward the axis, which was ascribed to unhomogeneity in the ring but

* Pogg. Ergz. Bd., vol. v.

† Phil. Mag., August, 1873.

may have been due to a departure from the above law which takes place on approaching the limit.

The same thing has since been observed by Fromme, who made use of the same method and ascribed the result to the latter cause.

In these experiments Rowland also investigated the magnetism of nickel and a law for the variation of μ was discovered similar to that holding in the case of iron. More recently* he has made further experiments on nickel and has added cobalt to the list of substances following the same general law. On account of the difficulty of procuring these metals in a pure condition, fewer experiments have been made with them than with iron, but the results, as far as found, agreed with those for iron.

In 1853 Plücker† experimented on a large number of bodies, and although the experiments were complicated by using an electro-magnet as the magnetizing force, he attempted, as far as possible, to eliminate this effect by using the law of increase in the case of iron. Only a few points were determined to fix the curve of magnetization, but the results found showed that all the substances experimented on approached a definite maximum which for iron, nickel and cobalt was relatively: Fe 1000, Co 918, Ni 322, values which are in very good agreement with those since found.

Arndtsen‡ also, using Weber's diamagnetometer, found that nickel soon approached a maximum magnetization and reducing the values to absolute measure stated that for very weak magnetizing forces the magnetism of nickel was five times that given by Weber's formula for iron.

All the experiments above mentioned were made at ordinary temperatures, but Faraday§ found that by heating in olive oil, the electro-magnetic properties of iron were hardly changed. Nickel suffered a considerable decrease, while cobalt increased in heating.

The later experiments by Rowland, referred to above, were made especially with reference to this point and show a very interesting property of nickel, viz: that for weak magnetizing forces its permeability is much increased by heating from 15° to 220° Centigrade, while for stronger forces it is decreased. The effect on iron is slight and cobalt has its permeability increased for all magnetizing forces used, showing the accuracy of Faraday's conclusions.

More recent experiments by Fromme, Riecke, Ettinghausen and others have confirmed the accuracy of these conclusions and at the same time have developed new facts in relation to the subject.

* Phil. Mag., November, 1874.
† Pogg. Ann., vol. xci.
‡ Pogg. Ann., vol. civ.
§ Exp. Res., xxx, 3424, 1855.

Fromme* has shown that by repeatedly magnetizing in the same direction, the permanent magnetism of an iron bar is increased, while the total magnetism is not much altered, according with Maxwell's idea of a force which causes a molecule to return to the position from which it has been displaced by the magnetizing force, unless the force exceeds a certain amount.

In 1880 Wassmuth,† in a research on the effect of heat on the magnetization coefficient of iron, found that for weak forces it was increased by heating from 20° to 146°, but it reached a maximum sooner and the maximum magnetization was less by about three per cent.

This result corresponds to that found by Rowland and shows that the difference between this metal and nickel is only one of degree.

Bauer ‡ also, using both a ring and a bar of iron and measuring the induced current produced by reversing the magnetism, found that the same law continued at much higher temperatures. For weak magnetizing forces the coefficient rises rapidly, reaching a maximum as the metal becomes red hot and then rapidly falls.

Following the suggestions made in Rowland's paper, the present series of experiments upon nickel was undertaken with a view to secure complete curves at a number of temperatures, showing the relation between the permeability and the magnetization, and thus if possible, determine the law by which it changed on passing from one temperature to another.

The rings used were from commercial rolled nickel§ and therefore not as pure as those used by Rowland. Still they were quite pure, as a chemical examination showed.

Their physical and chemical properties were as follows:

Ring.	Spec. grav.	Weight.	Cross sec.	Mean diam.	
I.	8·731	34·855	·196	6·440	Contain some C and Si.
II.	8·746	25·514	·198	4·696	A small precipitate was produced by H_2S.
III.	8·394	7·814	·0624	4·427	Contain no Co or Fe.

Rings I and II were from the same piece. Before using they were carefully annealed by heating to redness in a crucible filled with sand, which was allowed to cool as the fire died out.

As the temperatures employed were so high as to destroy ordinary insulation the rings were covered with a thin layer of asbestos paper, then a coil of wire was wound, then another

* Pogg. Ergzbd., vol. vii.

† Sitzb. der k. Akad., vol. lxxxii.

‡ Ann. der Phys. u. Chem., vol. xi.

§ Made by Wharton, Philadelphia.

layer of asbestos, etc., by which means an insulation was produced which was not affected by heating to a red heat.

The primary coil was in two layers, and as the second layer was not wound immediately upon the ring, a certain error was introduced, the magnetism being apparently increased by an amount which was in one case $\frac{1}{40}$, and varied from this to $\frac{1}{550}$, but in rings I and II it in no case exceeded $\frac{1}{130}$. Whenever it did so in ring III a correction was made.

The magnetizing current was measured by a tangent galvanometer constructed by Rowland. Its constants were computed from its dimensions. The circle was 21 cm. in diameter and graduated to quarter degrees. Six coils were used, each containing about three times as many turns as the next preceding. From six to twenty-four cells of Bunsen chromic acid battery furnished the current, which was further modified by the introduction of a variable resistance.

The galvanometer for measuring the induced currents had a sensitively adjusted astatic needle whose directive force was given by a magnet placed below. It had a period of about nine seconds, and was deflected by one turn of the earth inductor through nearly four degrees. It was read by a telescope and scale one meter distant. The earth inductor was introduced for the purpose of comparing the induced current with one of known strength and the winding included a total area of 20,716 sq. c. m. It was adjusted in a plane perpendicular to the magnetic meridian and could be turned through an arc of 180°. As in Rowland's experiments, a coil of wire sliding upon a magnet was introduced into the circuit, to control the vibrations of the needle.

The ring was placed in a bath of kerosene for low temperatures, for 100° in one of paraffine and heated in a steam bath. At higher temperatures it was placed in an oil bath which was designed for the comparison of thermometers at high temperatures. This was made of very heavy copper castings and held several liters so that its temperature, when kept up by a Bunsen burner, would remain constant for a long time, rarely varying more than four or five degrees during the experiment which usually took several hours. The temperature was determined by a mercury thermometer.

The course of each experiment was as follows:

The current was allowed to run for some minutes through a shunt circuit of about the same resistance as the ring, then turned through the ring and the readings of the tangent galvanometer made.

Then the deflections of the mirror galvanometer by the induced currents were measured, four readings being taken for the temporary magnetism and four for the total. Then another measurement of the primary current was made.

In the case of very strong currents which would heat the ring rapidly the measurements of temporary magnetism were omitted and the tangent galvanometer reading was taken between the measurements of the induced current. In measuring the temporary magnetism, the current, instead of being broken, was turned into the shunt, so that the battery strength should remain constant.

Readings of the thermometer were made before and after each observation and the deflection produced by the earth inductor was taken each time, though in most of the experiments this was so uniform that the mean of all the values was taken as the true value.

Since the rings used in this experiment were of rectangular section, the solution given by Kirchhoff does not accurately hold, but the theory is equally simple.

Let $\mathfrak{B}$ = magnetic induction through unit area.
μ = magnetic permeability.
H = horizontal component of earth's magnetism at the earth inductor.
H′ = horizontal component of earth's magnetism at tangent galvanometer.
i = intensity of the primary current.
A = total area included by the earth inductor.
A′ = area of the section of the ring.
n = whole number of windings about the ring in the primary circuit.
n' = whole number of windings in the secondary circuit.
Q = current due to reversing earth inductor.
Q′ = current due to reversing magnetism of the ring.
ϑ = deflection of galvanometer produced by Q′.
ϑ' = deflection of galvanometer produced by Q.
R = mean radius of the ring.

Then the magnetic induction at any point in the ring is $\frac{2ni\mu}{\rho}$ and the total induction through any section is:

$$2ni\mu b\int_{-\frac{a}{2}}^{\frac{a}{2}} \frac{1}{\mathrm{R}-x}\,dx$$

where ρ is the distance of any point from the axis of the ring and a and b are the sides of the rectangular section.

The value of the integral is

$$\log\frac{\mathrm{R}+\frac{a}{2}}{\mathrm{R}-\frac{a}{2}} = \frac{a}{\mathrm{R}}\left(1+\tfrac{1}{3}\frac{a^2}{(2\mathrm{R})^2}+\tfrac{1}{5}\frac{a^4}{(2\mathrm{R})^4}+\text{etc.}\right)$$

$$\therefore \mathfrak{B}=\frac{2ni\mu}{\mathrm{R}}\left(1+\tfrac{1}{3}\frac{a^2}{(2\mathrm{R})^2}+\text{etc.}\right)$$

The value of the second term of the series never reaches $\frac{1}{300}$ in the rings used and hence is entirely imperceptible.

$$Q \propto 2AH \qquad\qquad = C \sin \vartheta'$$

$$Q' \propto 2n'A'\mathfrak{B} \propto \frac{4nn'\mu i A'}{R} = C \sin \vartheta$$

$$i = \frac{H'}{G} \tan \delta$$

$$\frac{\sin \vartheta}{\sin \vartheta'} = \frac{2nn'\mu}{R} \frac{A'}{A} \frac{H'}{H} \frac{\tan \delta}{G}$$

$$\therefore \mu = \tfrac{1}{2} \frac{R}{nn'} \frac{A}{A'} \frac{H}{H'} \frac{G}{\tan \delta} \frac{\sin \vartheta}{\sin \vartheta'}$$

$$\text{and } \mathfrak{B} = \frac{HA}{n'A'} \frac{\sin \vartheta}{\sin \vartheta'}$$

All measurements given in this paper are in C. G. S. units.

The values of some of the quantities used in the experiment are as follows:

$$H = \cdot 1873$$
$$H' = \cdot 1883$$

The values of n are

Ring	I	205
Ring	II	183
Ring	III	292
		169

To vary n', several sets of coils were wound about each ring containing from eight to ninety-five turns, according to the currents used and the cross section of the ring. The different coils in the same ring were of the same resistance, but any slight changes in the adjustment would be eliminated by the earth inductor.

In one case the ring was cooled to $-17°$ by means of ice and calcium chloride, but the change produced by this was so small that it was not considered worth doing in the other cases.

The results obtained for the three rings were as follows: (μ_1 is the permeability for temporary magnetism and μ_{1+2} for total.)

TABLE I.—Ring No. I.

Temperature=22°.			Temperature=100°.			Temperature=205°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
80·	83·9	29·2	105·	107·	58·	127·	141·	87·2
74·	74·	33·4	118·	160·	563·	148·	202·	559·
83·	109·	278·	125·	178·	724·	149·	240·	977·
92·	149·	593·	120·	217·	1720·	138·	250·	1790·
98·	199·	1460·	115·	204·	2400·	130·	228·	2280·
102·	200·	2180·	107·	187·	2890·	117·	188·	2960·
100·	194·	2510·	94·	151·	3710·	100·	150·	3580·
90·	155·	3700·	----	100·	4940·	----	131·	3880·
----	102·	5260·	----	----	----	----	97·4	4370·

Temperature=262°.			Temperature=314°.			Temperature high.	
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_{1+2}	$\mathfrak{B}$
147·	156·	90·5	185.	205·	93·9	169·	3120·
168·	243·	583·	210·	353·	677·	132·	3580·
173·	278·	906·	197·	359·	1050·	170·	2920·
160·	291·	1400·	173·	319·	1360·	123·	3300·
144·	246·	2170·	137·	209·	2080·	133·	2900·
121·	189·	2850·	109·	154·	2360·	103·	2820·
97·	140·	3300·	78·	104·	2460·	117·	2120·
----	111·	3630·	----	85·	2490·	63·1	1780·
----	84·1	3690·	----	----	----	23·6	428·
----	----	----	----	----	----	5.9	107·

See plate (p. 229) figure 1 where the continuous lines correspond to the total magnetism, and the broken lines to the temporary. The points for the different curves of temporary magnetism, have distinctive marks.

TABLE II.—Ring No. II.

Temperature=21°.			Temperature=100°.			Temperature=197°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
64·	63·9	49·6	87.	90·5	59·3	111·	114·	70·7
65·	72·6	137·	92·	125·	402·	118·	165·	485·
71·	93·	303·	98·	157·	806·	120·	208·	850·
79·	136·	710·	99·	184·	1570·	119·	209·	1730·
67·	168·	1460·	98·	176·	2150·	113·	194·	2210·
86·	164·	2050·	93·	158·	2790·	102·	163·	2810·
83·	147·	2710·	81·	126·	3820·	84·	119·	3660·
78·	125·	3030·	----	84·1	4910·	81·	119·	3710·
----	83·	5090·	----	----	----	----	80·1	4420·

Temperature=269°.			Temperature=298°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
126·	128·	41·3	140.	161·	60·3
129·	150·	163·	161·	184·	186·
138·	215·	560·	157·	273·	759·
145·	235·	844·	155·	275·	1050·
141·	248·	1110·	151·	268·	1230·
127·	222·	1860·	124·	218·	1820·
114·	184·	2390·	110·	169·	2330·
80·	110·	3350·	77·	104·	2930·
----	83·9	3620·	----	69·2	2810·

Table III.—Ring No. III.

Temperature = −17°.			Temperature = 15°.			Temperature = 100°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
58·	63·3	92·4	69·	83·2	117·	127·	143·	181·
69·	106·	378·	80·	135·	438·	139·	212·	677·
78·	165·	970·	95·	193·	1120·	152·	248·	1390·
87·	201·	2070·	105·	217·	2290·	139·	259·	2280·
91·	192·	2820·	104·	200·	3040·	130·	219·	3270·
87·	166·	3580·	103·	206·	2860·	113·	186·	3750·
73·	129·	4320·	100·	178·	3580·	90·	139·	4380·
60·	100·	4670·	80·	130·	4320·	70·	105·	4750·
37·	58·2	5280·	67·	106·	4640·	42·	59·4	5160·
----	----	----	42·	62·1	5220·	----	----	----

Temperature = 210°.			Temperature = 300°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
191·	191·	139·	268·	271·	193·
187·	227·	339·	245·	349·	557·
197·	266·	727·	245·	388·	1040·
190·	295·	1890·	218·	337·	1820·
175·	286·	2300·	169·	274·	2140·
161·	246·	2800·	158·	226·	2380·
111·	157·	3740·	83·	113·	2780·
100·	139·	3890·	----	----	----
54·	68·3	3800·	----	----	----

Ring III was re-wound between the third and fourth experiments, and owing no doubt to a contact in the primary coil, the later values of μ were too small. In the fourth and fifth series, as given in the table, μ is multiplied by $1\frac{1}{6}$. On this account these experiments are of less value, but the error does not affect the determination of the maximum value of $\mathfrak{B}$ as given later.

The last series in table I was taken at varying temperatures above the previous. To secure this the ring, properly insulated, was placed on a plate of copper in a brass vessel and sand piled upon it. The whole was then heated slowly, and at intervals very rapid observations were made of the magnetic condition of the ring, and this was continued till the metal was nearly non-magnetic. The points thus found are indicated on the diagram. At the end the bottom of the vessel seemed slightly red.

An examination of the curves shows—

I. A rise nearly in a straight line at first and more rapid as the temperature is higher.

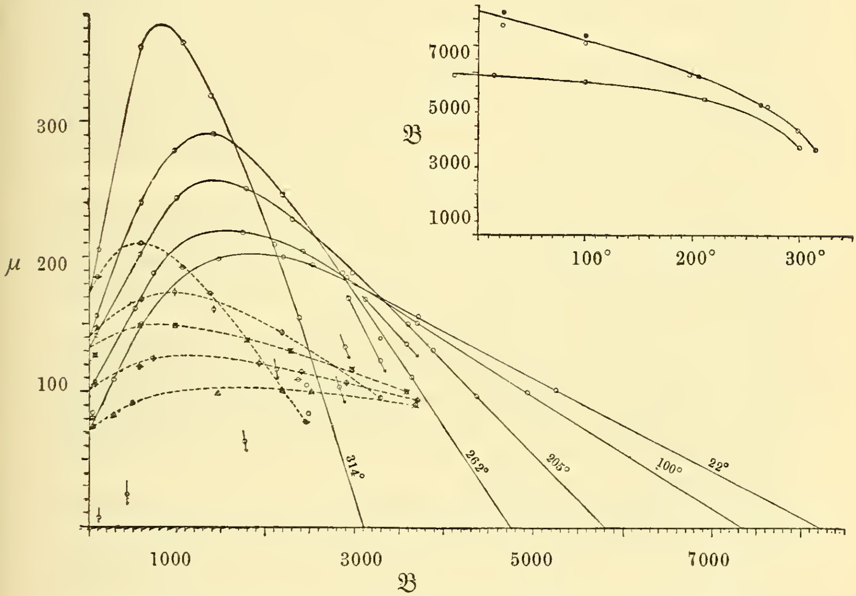
II. A sharp curvature, the sharper as the temperature is higher.

III. A descent which also is more abrupt as the temperature rises.

There was a limit to the extent to which the current could be increased on account of the heating of the ring. In one or two cases where a strong current was used this effect was so marked that a smaller value of 𝔅 was found for the stronger current than for the weaker, which could not be due simply to the increase of the magnetic field.

The latter part of the curves is a nearly straight line, and when this is prolonged to cut the axis it gives the value which 𝔅 approaches as a maximum for all magnetizing forces here available. It is to be expected that the form of the curve will change as it approaches the axis, to which it no doubt becomes finally parallel, since $\mu=1$ for non-magnetic bodies, and in no known case is as low as zero.

1. 2.



Such an inflection was observed in the curve for iron by Rowland and by Fromme, as before stated.

Faraday* also found that at very high temperatures nickel still possessed the power of being attracted by his electro-magnet, so that it is doubtful whether the curve crosses the line $\mu=1$.

But although 𝔅 may never reach a maximum, yet the quantity found by prolonging the curve to the axis gives the law of the change of 𝔅 with varying temperatures and is the highest value of 𝔅 that can be reached by ordinary magnetizing forces,

* Exp. Res., vol. iii, p. 55.

the metal becoming practically non-magnetic in this neighborhood.

The values thus found for these rings are given here:

I	𝔅,	8230	7250	6020	4810	3300
	temp.,	22°	100°	205°	262°	314°
II	𝔅,	7820	7180	5950	4740	3820
	temp.,	21°	100°	197°	269°	298°
III	𝔅,	5860	5860	5660	5000	3200
	temp.,	−17°	15°	100°	210°	300°

It will be seen that the values for rings I and II are as nearly equal as can be determined by this method, and they are therefore shown on the same curve where the values found for ring I are indicated by the crossed circles. Those for ring III vary somewhat less rapidly and lie constantly below the others.

This difference seems to belong to the metal itself, as it will be seen on examination of the curves for the rings that μ of ring III goes down more rapidly than the others, though it rises to the same height as ring I and higher than ring II at the maximum point.

If this curve be continued it must not be allowed to meet the axis at a large angle, since this would indicate that the ring resisted infinitely the passage of the lines of force, but it must nearly coincide at the last part with the curve representing $\mathfrak{H}$, the magnetizing force.

This has been shown to be true by some observations made by A. Becquerel. H. Becquerel* states that in some unpublished experiments his father found that nickel is still slightly magnetic when heated to about 600°, though only about $\frac{1}{4000}$ as much so as at ordinary temperatures. Faraday's observations indicate the same thing.

The additional series made with ring I shows that the change is continuous beyond the points indicated in the diagram.

The tables given for μ and 𝔅 include also the permeability for temporary magnetism, the curves of which are drawn on the same diagram† with the total permeability. (Fig. 1.)

The permanent permeability may be readily found by subtracting one of these from the other.

These results are not nearly as regular as the others because the temporary magnetism varies with many circumstances, depending on the amount and direction of previous magnetization and on the number of times the magnetism has been reversed.

If the ring has previously been strongly magnetized the first few reversals cause a greater change of magnetism than the subsequent, and the same is true for temporary magnetism.

* Ann. de Chimie et de Phys.

† In broken lines.

Also, if the ring has been for some time magnetized in one direction the temporary magnetism in this direction is less than in the opposite by an amount reaching twenty per cent in some cases, thus presenting an analogy to the so-called "fatigue" of metals under strain.*

The curves, however, serve to show that at the first all the magnetism is temporary. The permeability rises to a maximum at about the same place as that of total magnetization, then falls less rapidly and approaches the total, though it is evident that it can never reach it, since the permanent magnetism cannot be less for a high magnetizing force than for a lower at any given temperature.

It is to be noticed that the magnets used have no free poles. Hence the transient nature of the magnetism is due to the structural properties of the material and not to any demagnetizing influence of the poles.

Art. XXX.—*Enlargements of Hornblende Fragments;* by C. R. Van Hise.

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The nature of the process or processes by which fragmental rocks have become indurated is a question of great interest. The enlargement of grains of quartz, as first described by Sorby,† in 1880, has since been shown‡ to explain in large measure the consolidation of quartzose sandstones to quartzites. This enlargement consists in the deposition of silica about rolled grains of quartz, the new silica being optically continuous with the old grains, or in other words forming with each of the original fragments a single quartz individual. The secondary growth of rolled grains of feldspar in feldspathic sandstones in a corresponding manner, i. e., the addition of feldspar to rolled grains of feldspar, the two being parts of the same individuals, was also announced by me some time since,§ since which time the study of many sections of feldspathic sandstones from various localities has made it certain that fragmental particles of feldspar frequently enlarge by renewed growth just as do grains of quartz. It would seem that the enlargement of feldspar fragments plays an important part in the consolidation of many feldspathic fragmental rocks.

* Righi has observed the same effect (Beibl. vol. v), and it is also similar to the observation of Fromme in the case of repeated magnetization (p. 223).

† Proc. Geol. Soc. London, 1880, p. 62.

‡ R. D. Irving, Am. Jour. Sci., June, 1883. R. D. Irving and C. R. Van Hise in Bull. U. S. Geol. Sur., No. 8, 1884.

§ Am. Jour. Sci., III, xxvii, May, 1884.

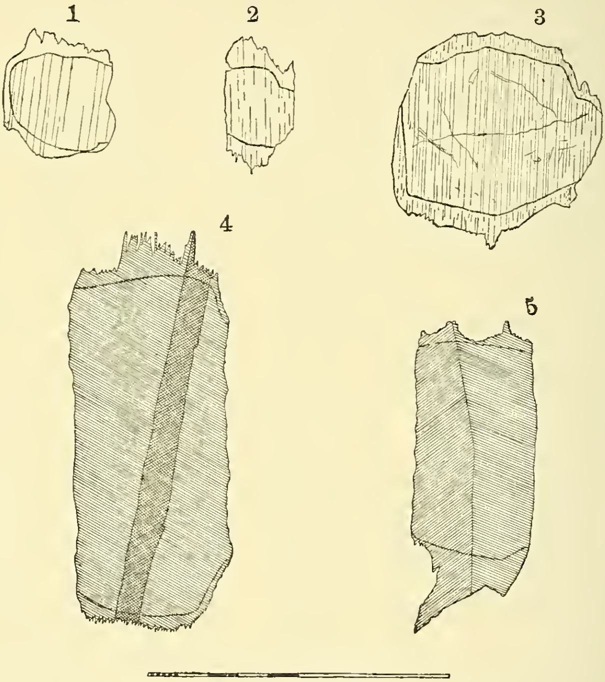
Hornblende is one of the most important of the remaining rock-forming minerals. The object of the present paper is to describe briefly some unmistakeable enlargements of hornblende fragments that I have recently met with in studying certain peculiar conglomerates under the direction of Professor R. D. Irving.

At Ogishke Muncie and Cacaquabic lakes, in the northeastern part of Minnesota (T. 66 N., R. 6 W., and T. 66 N., R. 7 W.), occurs a great belt of conglomerate forming part of the Archæan schists of that region. The following extracts from field notes by Mr. W. M. Chauvenet, of the U. S. Geological Survey, are descriptive of this conglomerate: On Ogishke Muncie "the great conglomerate formation or bed shows no apparent dip or strike, but massive walls of green color with joints and great cracks. . . . The pebbles protrude in masses, the great pink, granite pebbles being most numerous, often as large as three feet through. Bright red jasper pebbles give the rock a handsome appearance. Quartzite and slate pebbles are also present." At another place on Ogishke Muncie Lake: "The conglomerate and greenish conglomeratic rock rise in contact forming a cliff 30 to 40 feet high, broken or irregular. There is no marked division between the two, the one blending with the other. No red jasper pebbles are here seen, which fact distinguishes this from the coarser conglomerate. The pebbles are black and green in a dense matrix of crystalline greenish rock." At Cacaquabic Lake: "The conglomerate appears on the shore. The conglomerate differs little in appearance from that at Ogishke Muncie. The pebbles are of many kinds but not so thickly scattered. Jasper pebbles are very common, granite, clear quartz, and slate pebbles are present. This was especially apparent upon weathered surfaces." At times the exposures become "a green, sub-crystalline, exceedingly tough rock, but appearing like a bedded sandstone in place. It is finely but distinctly banded, cleaving most readily along the bands. . . . Above this rock is a distinctly crystalline, greenish rock, weathering to green and red, having a conglomeratic appearance in places where red granitic grains are thickly scattered."

The best illustrations of the enlargements of hornblende yet found are in the denser parts of the conglomerate where the pebbles are comparatively rare. But these enlargements are present in nearly all sections cut from the matrix of this conglomerate throughout its entire known extent of a number of miles. As is to be expected, thin sections from different localities are quite different in appearance and composition. In many cases the finer parts of the sections are so exceedingly fine as to make it difficult to determine accurately the various mineral constituents. However in these cases there are always present coarser grains of hornblende and feldspar, and the former commonly show enlargements.

The following is a brief description of one of the thin sections from the Ogishke Muncie rock, the mineral constituents of which are in the main quite clearly defined. The section is composed of a rather sparse fine ground-mass, through which are abundantly scattered medium-sized particles of hornblende and feldspar. Although cut from a conglomerate, at a first glance the section somewhat resembles that of a basic porphyry, but upon a closer examination its fragmental character is plain. With a high power, the ground-mass, which makes perhaps one-fourth of the section, appears to be composed of quartz and feldspar, with some kaolin and cryptocrystalline silica. The other four-fifths of the section are composed almost wholly of medium-sized grains of feldspar and hornblende in about equal proportions. The feldspar is in part orthoclase and in part plagioclase, the grains being usually more or less rounded and varying in size from those several millimeters across to those so small as to be lost in the fine matrix. Some of these grains of feldspar have apparently received enlargements. The hornblende is green or greenish yellow in color. In most cases it shows one cleavage and frequently is so cut as to develop nicely its two cleavages at the characteristic angle. In unpolarized light these individuals appear to be bounded by well defined lines, their apparent forms being ordinarily oval, rounded or rectangular. Much more rarely they possess crystalline outlines. In the polarized light, however, each grain of hornblende is found to extend beyond its apparent outline, as seen in the ordinary light, and to terminate commonly in a ragged outline with long projections. These present outlines are believed to be due to enlargements of the hornblende, the inner rounded, rounded rectangular, or crystalline outlines, bounding fragments of hornblende which after being deposited in the positions now occupied have taken a new growth. The lines of division between the fragmental cores and the exterior parts are distinct. They are due to numerous gas cavities and inclusions, the latter being mostly minute particles of ferrite, i. e. these lines of division are of the same nature as the corresponding lines in the enlargements of quartz fragments. When viewed in unpolarized light, in many cases there is little difference in the appearance of the cores and exterior portions of hornblende while frequently there is a marked difference. The added portions are paler in color, at times so much so as to give the impression that this part is quite different from the cores; yet, between crossed nicols the lighter colored, most irregularly outlined exterior parts are optically continuous with the interiors of the grains. Quite frequently the individuals of hornblende are twinned, and in this case the added portions are also twinned in a corresponding manner, the *twinning bands cutting directly across both the cores and the new bordering material.*

The proof that these are really enlargements of hornblende fragments is, then, much the same as that hitherto advanced



One millimeter.

of the enlargements of quartz and feldspar. The more important of these are indicated by the figures in the accompanying plate.

Figures 1 and 2 are enlarged rounded grains of hornblende. The curved lines represent the broad lines of division between the old and new hornblende. This line is not an unbroken, sharply continuous one, but is built up of small gas cavities and particles of ferrite. With a low power the ferrites and cavities make an apparently unbroken line, but with a higher power their true character is plain. The sharply serrate outlines of the enlargements are noticeable. At points where the lines are not thus serrate the grains have grown until in contact with other grains of hornblende or feldspar. It will be noticed that the cleavage is continuous in places from the inner into the outer hornblende, while at times these cleavage lines break abruptly at the division line between the old and new material. This fact alone is an almost decisive proof that the exterior and interior parts of the individuals are of the same nature, and that the growth of the whole has been an interrupted one. The regular, worn, rounded outlines of the interior parts show that this interruption has been a great one. It will be noticed that the enlargements are narrow or wanting in the direction transverse to this cleavage. This cleavage direction, as is well known, is that in which hornblende individuals commonly have their greatest length. It is most often the case that the entire enlargements have occurred in the direction of the greatest magnitude of the particles of hornblende.

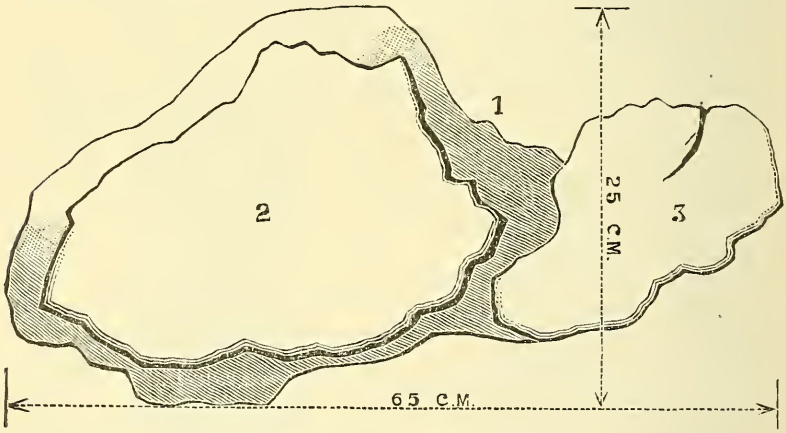
Figure 3 shows an enlarged, crystal-outlined grain of hornblende. These crystal-outlined grains are sparsely present in many of the sections from the conglomerate at Ogishke Muncie and Cacaquabic Lakes, but are commonly enlarged. The enlargements themselves never have crystal outlines. How it chances that such grains occur in a clastic rock, I leave for another to discuss. In figures 1, 2, and 3 all lines are structure or form lines.

Figures 4 and 5 represent enlarged twinned crystals of hornblende. Parts of like shading in each case represent parts which become dark simultaneously in polarized light. It is plain that twinning bands cut through the lines which separate the clastic cores and newly formed hornblende. The cleavage lines, which are not shown in the figures, are parallel to the greatest length of the grains. These grains then also represent the tendency to enlarge in this direction as mentioned above. They also show finely the sharply angular outlines of the added hornblende.

ART. XXXI.—*On three Masses of Meteoric Iron from Glorieta Mountain, near Canoncito, Sante Fe County, New Mexico;* by GEORGE F. KUNZ. With four Plates.

THE meteorite, described in this paper, was discovered by Mr. Charles Sponsler, a prospector, and was supposed by him to be a mineral of peculiar value. It was found on some unclaimed land on Glorieta Mountain, about half a mile from a house in the woods, one mile northeast of Canoncito, Sante Fe County, in May (?), 1884. The mass was lying on a rock, upon which it had fallen, in three fragments, and judging from the few marks of weathering, had not been long exposed. The exact date of the discovery I am unable to give at this time, for since it came into my possession I have not succeeded in communicating with Mr. Sponsler, he being absent out of the reach of the mails. In due time I expect to be able to settle this point and to announce it with other facts of interest.

The weight of the entire mass is 317 lbs. (143·76 kilos.). Perhaps one kilo had been chipped off before it came into my possession, so that the original weight was probably about 145 kilos. The diagram accompanying this will give a general idea of the former relative position of the three pieces; No. 1 projecting above and below No. 2, as indicated by the shading, and No. 3 fitting in at the lower right hand end. The dimen-



sions of the whole were approximately as follows: length 25 inches (65 cm.), height 10 inches (25 cm.), thickness 15 inches (37 cm.) It is curious that so large and compact a mass of iron should have been so completely broken asunder, and in this respect the fall is quite unique. The fractures are very clean considering the size of the fragments, although the edges are somewhat irregular. No. 1 is filled with elongated hollows, proving that it evidently was disturbed, and the twistings in No. 2 at the point of impact would lead to the conclusion that the falling body was partly semiplastic; but Professor R. H. Thurston, who kindly examined the iron, compares the fracture to the effect that is produced by a sudden heavy blow on cold iron and has observed the same violent wrenching in an iron target used in heavy gunning practice and now at the Stevens Institute, Hoboken, N. J.

No. 1 weighs 148½ lbs. About one-third of the whole surface shows the disjuncture very plainly, as also the exact point where this began. The mass measures 15½ inches (39 cm.) in length, 12 inches (30 cm.) in width, and 8¾ inches (22 cm.) at the thickest part, and at the thinner 5 inches (12 cm.) One portion has a peculiar bubbled pasty appearance, as if the mass had been cooled in water at this point. Some of the depressions on the surface, or pittings, are 5 cm. across, and quite deep and well-marked. The upper figure on plate V represents the torn side of this mass. Plate VI shows the Widmannstätten figures produced by etching a surface of a fragment cut from No. 1; this plate is printed from an impression taken directly from the etched slab.

No. 2 weighs 115 lbs. (52·38 kilos.), and measures 16½ inches (41 cm.) in length, 10 inches (24 cm.) in width, and 6½ inches (16 cm.) in thickness. About one-third of the surface of this piece shows the remarkable rupture, the remainder being covered with the pittings. On one corner there is a portion, 10 inches by 6, which is evidently the spot where the mass struck the rock. Here the pittings are flattened and the whole mass distorted and curled over, giving it a radiated or fan-like appearance. The front, or pitted, side of No. 2 is well exhibited in Plate III, and the torn side in Plate V.

No. 3 weighs 53½ lbs. (24·263 kilos.), and measures 12 inches (30 cm.) in length, 8½ inches (21¼ cm.) in height, and 6 inches (15 cm.) in thickness in the thickest part. Over five-sixths of the entire surface is pitted, some of the depressions being 5 cm. across and nearly 2 cm. deep. The place of rupture is plain, and the iron here is coarsely fibrous, possibly because it was farther from the point of impact. There is also a fissure about 4 inches (10 cm.) deep and nearly 1 cm. wide, opposite the broken face (see plate IV). In this fissure are two broken ends of chisels which were broken in the attempt to pry off this piece, and which may have enlarged the opening. The front side of No. 3 is shown in Plate IV.

This iron is one of the Holosiderites of Daubrée, and comes under the general group of Caillite of Meunier; it is related to the irons of Augusta County, Virginia, Whitfield County, Georgia, and Washington County, Wisconsin. The iron is of characteristic octahedral structure, and the Widmannstätten figures are made up of kamacite (Balkeneisen or beam-iron), i. e. iron with little nickel, enveloped in tænite (Bandeisen), rich in nickel, and plessite (Fülleisen).* On the single cut made, one field of dark plessite measured 17 mm. by 8 mm., the kamacite from ·5 mm. to 2 mm. in breadth. The tænite was abundant and brilliant.

*Die Meteoriten Sammlung des k. k. mineralogischen Hof-Kabinetes in Wien. Am. 1. Mai, 1885, by Dr. Aristides Brezina; (Separat-Abdruck aus dem Jahrbuch der k. k. geol. Reichsanstalt, for 1885, vol. xxxv, No. 1).

The following is the result of an analysis of a compact piece of iron from No. 3, made by Mr. James B. Mackintosh, E. M. of the School of Mines, New York City.

Fe	87·93
Ni	11·15
Co	0·33
P	0·36
	99·77

Carbon, sulphur and other constituents were not determined. The specific gravity of the entire mass No. 2 was taken on a common steel-yard, and found to be 7·66+. The figures may be of interest, as showing the homogeneity of the mass, although the method was not delicate.

Troilite was observed in several places on the crust of No. 1, also traces of olivine, and perhaps schreibersite? But as yet the investigation of the mass is incomplete; all facts regarding the included minerals will appear in a succeeding note on this fall, together with some approximate data as to the velocity at the moment of impact.

I will add that this meteorite was brought to my notice by Messrs. F. Alfred Reichardt & Co., and Dr. H. G. Torrey, who have courteously given me information on the subject.

SCIENTIFIC INTELLIGENCE.

I. Physics.

1. *On "Transfer-resistance" in Electrolytic and Voltaic Cells;* by G. Gore. (Abstract.)—The existence of this phenomenon has been a matter of doubt ever since the year 1831, and the question has been examined by many investigators. In the present paper are described a series of methods by means of which its reality has been determined. Other methods are given for measuring the amounts of such "resistance," either collectively at the two electrodes of an electrolytic cell, or separately at each electrode. Modes of obviating the interference of polarization, and of securing success in the measurements, are also described.

The influence of various circumstances upon the phenomenon were investigated, viz: strength and density of current; total resistance; density of current and size of electrode; composition of the electrolyte; strength of the same; combined electrolytic cells; temperature; and chemical corrosion. The relations of the phenomenon to size of plate in voltaic cells, to the positive and negative plates respectively, and to strength of current in those cells, were also examined, and the results are given.

The following are the chief facts established by this research: That a species of electric "resistance," distinct from that of polarization and of ordinary conduction-resistance, varying greatly in amount in different cases, exists at the surfaces of mutual contact of metals and liquids in electrolytic and voltaic cells. That this "resistance" varies largely in amount with different metals in the same solution, and with the same metals in different solutions, in dilute solutions of mineral acids of different strengths, or of different temperatures; and is usually small with easily corrodible metals which form quickly soluble salts, and large with those which are not corroded; and is disguised in the case of those which by corrosion form insoluble salts.

The results of the experiments also show that the same voltaic current was "resisted" in different degrees by every different metal when employed as an anode, and when used as a cathode; also by the same metal when used as an anode and cathode respectively; and that the proportions of such "resistance" at an anode and cathode of the same metal, varied with every different metal in every different electrolyte (and strength of electrolyte), and at every different temperature; and that the resistance at the anode was usually smaller than that at the cathode; in some cases, however, where a film was formed upon the anode, an apparently reverse effect occurred; that a current from a given positive plate of a voltaic cell was differently resisted by every different metal used as a negative plate in that cell; and that by rise of temperature "transfer-resistance" was usually and considerably reduced.

They further show that this species of "resistance" was largely reduced by increasing the strength of current; and was thus conspicuously distinguished from ordinary conduction-resistance of the electrolyte. In consequence of this effect, "transfer-resistance" was greatly influenced by every circumstance which altered the ordinary resistance, and thereby the strength of current. The usual effect of diminishing the density of current alone, by enlarging both the electrodes and keeping the strength constant, was to diminish the "transfer-resistance;" and of enlarging one only, was to diminish it at that electrode and increase it at the other, the effect being greatest at the altered electrode; but the influence of density was very much smaller than that of strength of current. The current was usually less "resisted," and larger with a small positive plate and a large negative one, than with those sizes reversed. Alterations of size or kind of metal at one plate of an electrolytic or voltaic cell affected the "transfer-resistance" at the other, by altering the strength and density of the current.

"Transfer-resistance," therefore, appears to vary, not only with every physical and chemical change in the metals and liquids, but also with every alteration in the current. Such "resistance" throws light upon the relative functions of the positive and negative plates of voltaic cells, and illustrates the comparatively small influence of the negative one in producing strength of current. Nearly all these conclusions are based upon results represented by average numbers obtained by series of experiments.—*Proc. Roy. Soc.*, No. 236.

2. *Electrical Resistance of the new alloy Platinoid.*—The new alloy Platinoid is essentially German silver with the addition of 1 or 2 per cent of metallic tungsten. It has a specific gravity of 8·78 (at 20° C.); the color is white, and when polished is hardly distinguishable in appearance from silver; it is especially remarkable for being practically untarnishable, resisting to great degree the ordinary tarnishing effect of the air. BOTTOMLEY has carried on a series of experiments having as their object the determination of its electrical resistance. He finds that it possesses the same properties of high specific resistance and small variation of resistance with change of temperature that make German silver wire so suitable for the galvanometer and resistance coils, only in a higher degree. The specific resistance of platinoid is about one and a half times that of German silver. The average percentage variation of resistance per 1° C. between 0° and 100° was found to be 0·02087 in one case and 0·022 in another; the corresponding values, as obtained by Matthiessen are at 20°, for copper 0·388, platinum-silver alloy 0·031, gold-silver alloy 0·065 and German silver 0·044.—*Proc. Roy. Soc.*, No. 237.

3. *Annual change of the Aurora Borealis.*—A recent number of *Nature* contains an interesting review of a work by Tromholt on the Aurora. After remarking the fact that Weyprecht was the first to advance the view that the auroral zone is farthest south at the equinoxes and farthest north at the solstices, the following quotation in regard to this point is given from Tromholt.

"My researches have led me to endorse Weyprecht's theory. I feel satisfied that the Aurora Borealis moves toward the autumnal equinox southward, and then northward, reaching its farthest northern limit about solstice. After this it again moves southward, being in its most southern position at the vernal equinox, when the movement is again in a northerly direction.

"From this it follows that the two maxima occurring in the temperate zone at the equinoxes must approach each other more the farther north the point of observation is situated. This is, in fact, the case. As some examples, I may mention that, while the two maxima occur in March and September in St. Petersburg, Åbo, Stockholm, Christiania, Worcester (Mass.), and New Haven, they occur in February and October in Aalesund, Newberry, Quebec, and Newfoundland; in December to January in Hammerfest, and in January at Fort Reliance. Very instructive in this respect are also the observations from the three Greenland stations: Upernivik, Jacobshavn, and Ivigtut. At Ivigtut, the southernmost of the stations, the yearly maximum must certainly be said to occur in January, but there is a second maximum toward the autumnal equinox. At Jacobshavn, eight degrees farther north, there is but one distinctly marked maximum in January, and at

Upernivik the northernmost of the stations, the maximum falls at the winter solstice more marked and dominant than anywhere else in the world."

4. *Note on a preliminary Comparison between the Dates of Cyclonic Storms in Great Britain and those of Magnetic Disturbances at the Kew Observatory;* by Balfour Stewart and Wm. Lant Carpenter.—We took the dates of thirty storms from Mr. Scott's paper entitled "The Equinoctial Gales; do they occur in the British Isles?" in the "Quarterly Journal of the Meteorological Society" for October, 1884, and by the kindness of Mr. Whipple, of the New Observatory, were enabled to make the comparison mentioned above.

Out of these thirty storms, in twenty-three cases there is a distinct magnetic disturbance, for the most part preceding the storm by somewhat more than a day. We do not, however, imagine that we have thus proved the fact of such a connection, but think the results we have attained sufficient to justify us in pursuing the subject.—*Proc. Roy. Soc.*, No. 236.

5. *Properties of Matter;* by P. G. Tait. 320 pp. crown 8vo. Edinburgh, 1885 (Adam and Charles Black).—The excellent volumes by Professor Tait on the subjects of Heat and Light, already noticed in this Journal, are now followed by a third on the Properties of Matter, and, as we are informed, the series is to be completed by three others on Dynamics, Sound, and Electricity. The value of such works as these to the student of physical science can hardly be overestimated. Instead of the dull mechanical style of many of the older text-books, only enlivened by the too elaborate illustrations, these volumes of Professor Tait are fresh, bright and suggestive, and calculated to tempt a student on to do independent thinking for himself. The present work is especially welcome since it is devoted to a series of topics which have not often been discussed connectedly in a single volume.

6. *The Mathematical Theory of Electricity and Magnetism;* by H. W. Watson and S. H. Burbury. Vol. i, Electrostatics. 268 pp. 8vo. Oxford, 1885 (The Clarendon Press).—This volume is intended, as the authors state, as an introduction to, or commentary upon, Maxwell's work on Electricity and Magnetism. The three opening chapters are devoted to a series of introductory mathematical propositions on Green's theorem, spherical harmonics and the potential. The fundamental electrical phenomena are then briefly described, much as given by Maxwell, and afterward the usual series of topics in electrostatics discussed mathematically in succession. Students who find difficulty in reading Maxwell's larger treatise will be assisted by this volume, in which his views and methods are more or less closely followed.

II. Geology and Natural History.

1. *Report of Progress of the Geological and Natural History Survey of Canada during the years* 1882–83–84, Alfred R. C. Selwyn, Director. Montreal, 1884. (Dawson Brothers.)—Among

the valuable Reports in this volume is one on the Labrador and Hudson Bay region by Mr. ROBERT BELL. The author observed glacial striæ in many places and the following are some of the facts. About the Southern Cape of Hudson's Straits the direction (magnetic, and the same beyond) of the striæ was southeastward—S. 35° E.—though varying among the hills with the trends of the valleys; on the north side of Hudson Strait, near Cape Prince of Wales, the direction was S. 40° E. to S. 60° E.; across the south end of Nottingham Island S. 30° E. (and the bowlders were largely limestone, indicative of a limestone formation to the westward); near Marble Island on the west side of Hudson's Bay (where occurs quartzyte having often ripple-marked surfaces), the direction of the glacial striæ is S. 10° E. The remark is made in the concluding summary that on both sides of Hudson's Bay the movement of the ice was to the southward and eastward; that an extensive glacier moved eastward down Hudson Strait, which had its head in Fox's Channel and terminated in the Atlantic Ocean. Glaciers are said to exist now in this channel and to be the source of the small icebergs that float down the strait. Mr. Bell also concludes that throughout the Glacial period "the top of the coast range of Labrador stood above the ice and was not glaciated, especially the high northern part." In the southern part of the Labrador peninsula the general course of movement "appears to have been southward, varying to the eastward and westward with the courses of the valleys;" but over Newfoundland "from the center toward the sea on all sides." The rocks met with along Hudson Strait and Bay were mainly Archæan. But on Mansfield Island and Cape Southampton, to the west, a fossiliferous limestone formation was seen, which was probably Lower Silurian.

In the report of Mr. R. W. ELLS, on the Gaspé Peninsula, it is stated that the later investigations show that much the larger part of the island instead of being under Triassic rocks, is Permo-Carboniferous, as shown by the fossil plants collected at various places; and that the part of the coal formation affording the great coal beds of Cape Breton, Pictou and Spring Hill is probably wanting.

Mr. A. P. LOW describes the rock of a ridge on the Gaspé peninsula, including the prominent peak Mt. Albert, averaging two and a half miles in breadth, as an olivine rock, more or less changed to a dark green serpentine. Chromic iron occurs in the serpentine, but not in sufficient quantities for profitable mining.

Mr. R. CHALMERS has a report on the Quaternary geology of Western New Brunswick. The directions of the glacial striæ (corrected for variation) in Carleton and York Counties were mostly between S. 15° E. and S. 30° E. The many lake-basins of the region are attributed to the partial filling of preglacial valleys by drift during the Glacial period, and a subsequent scooping out by local glaciers and currents. Terraces, kames and other glacial phenomena of the region are described.

The Report of Mr. HUGH FLETCHER on the Geology of Northern Cape Breton gives interesting facts with regard to the relations of the Carboniferous beds to the associated pre-Cambrian rocks. The volume is accompanied by a series of colored maps of the Province of Nova Scotia to illustrate a report by Mr. Fletcher of 1879 to 1884.

The Reports of Mr. G. M. Dawson on the region of Bow and Belly Rivers, and of Mr. Hoffmann on the coals and lignites of the northwest have already been noticed in this Journal.

2. *Aralo-Caspian and Mediterranean Basins.*—A paper on the Inland Seas and Salt Lakes of the Glacial period, by Mr. T. P. JAMIESON, is contained in the Geological Magazine for last May (III, ii, 193). It illustrates the great extension of such seas during the progress and decline of the era of ice by a review of the facts connected with the Great Salt Lake of Utah, the Dead Sea, the Aralo-Caspian Basin, the Pangong Lake and the Mediterranean Basin.

Speaking of the *Aralo-Caspian Basin* it states that the Caspian is 84 feet below the Black Sea, and that a rise of 107 feet would cause its waters to flow westward into that sea; that a rise of 220 feet would make the Caspian waters to flow northward into the Tobolsk and down the Obi into the Arctic Ocean. Its former recent connection with that ocean is sustained by the existence in its region of mollusks, crustaceans, fishes, the Beluga and seals, some of the species closely like or identical with Arctic kinds. The seal, *Phoca caspica*, is by some made a variety of the Arctic *P. vitulina.* The fishes include the sturgeon, herring, sterlet and salmon; the crustaceans, *Idotea entomon* and *Mysis relicta*, both Arctic species.

With regard to the Mediterranean, Mr. Jamieson, after referring to the opinion that a land communication must have existed between Spain and Africa during some part of the Quaternary, and that this would become a fact by a rise of a thousand feet, since a ridge crosses from Cape Spartel to Cape Trafalgar with no greater depth above it than 167 fathoms, urges that not only migrations across from Africa to Spain and the rest of Europe would thus have become possible, but also, in his view, migrations to Malta, Sicily and other islands within the Mediterranean Basin. He argues that the sea without an outlet would lose its water by evaporation, inasmuch as the average rainfall within the watershed is but 30 inches, while the evaporation is stated (Encyl. Brit., Art. Mediterranean) to exceed 60 inches, until an equilibrium was established between the loss and the supply, and that in this way the sea would be reduced to two or more lakes. He speaks of a dry climate intervening between the two Glacial eras of Europe, and favoring such a result. Thus there would have been made a dry path over to Sicily for African elephants of two species, two also of hippotamus, and other species, and for a similar migration to Malta; a dry way also, for foxes to Minorca, and hares, martens, deer, foxes, etc., to Corsica and

Sardinia. The depth of water between Africa and Sicily is now about 200 fathoms.

This theory of making dry passage-ways over the Mediterranean bottom and lakes of the salt sea requires elevation only at Gibraltar. The relations between the present salt-water life of the sea and that of Glacial and pre-Glacial time, found fossil in shore-deposits and elevated beds, ought to decide its merits.

3. *Union Group, Pacific Ocean.*—In an article on the composition of some coral limestones, etc. from the South Sea Islands, by A. Liversidge, published in the Journal of the Royal Society of New South Wales for 1880, p. 181, one of the specimens analyzed is stated to have been brought by Dr. Messer, R. N. of H. M. S. Pearl, from a raised reef on the Duke of York Island, one of the Union Group, *at a height of* 110 *feet above* the sea-level. There is an error here, for the Duke of York Island is a low atoll, and so are the others of the Union Group, the greatest elevation being 14 or 15 feet.

The analysis afforded 1·97 per cent of alumina (with traces of iron sesquioxide) and 0·789 of silica, and the amount of alumina suggests that the coral-reef rock may have come from some one of the barrier-islands of the ocean. The whole analysis is as follows: Carbonic acid 41·68, lime 52·09, magnesia 0·86, potash 0·98, soda 0·85, alumina and traces of iron sesquioxide 1·97, manganese *trace*, silica 0·79, organic matter 0·50, chlorine *trace*, hygroscopic moisture ·02=99·75. J. D. D.

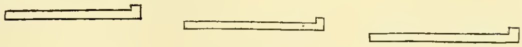
4. *Spiraxis major and Sp. Randalli of Newberry; large Screw-like fossils from the Chemung group of Northern Pennsylvania and Southern New York.*—These singular fossils are described and figured by Dr. J. S. Newberry in the Annals of the N. Y. Academy of Sciences, vol. iii, no. 7, p. 217. They are three to seven inches long, tapering slightly in either direction from the middle, half an inch to over an inch in greatest diameter, and are marked with a broad and deep spiral groove. But they are without any trace of organic structure, and Dr. Newberry describes the specimens and mentions possible biological relations, without venturing any decided opinion.

5. *Geological Map of the United States;* by W. J. McGee, of the U. S. Geological Survey.—This map, although a small one (18×28 inches), will be found of great value to the geological student. It has been carefully prepared and is colored in the best style of the art.

6. *Macfarlane's Geological Railway Guide.*—Dr. James Macfarlane is preparing a new edition of his valuable Geological Railway Guide, corrected to correspond with the recent discoveries, which will soon be published by Appleton & Co. The part for Canada has already been issued.

7. *Impact Friction and Faulting;* by George F. Becker.—The following misprints occur in the portion of this article which appeared in the August number:

Page 120, line 3 from top, $dw = wdx$ should read $dw = -wdx$.
Page 121, line 2 from top, for W read *w*.
Page 122, line 8 from top, for mv_2 read mv^2.
Page 123, line 7 from bottom, for *average* read *energy*.
Page 127, the figure should be as follows:



8. *Plantes à Fourmis.*—Under this title M. Levier has reproduced in the French language, in the *Archives Italiennes de Biologie*, the substance of Beccari's *Pianti Ospitatrici, ossia Piante Formicarie della Malesia e della Papusia*, (that of the introductory portion), from the second volume of Beccari's *Malesia*. The first part of this, with its copious figures, is devoted to the consideration of plants which hospitably shelter and in part feed colonies of ants in a peculiar and quasi-pathological growth. The first-known plant of this kind is *Acacia cornigera* of Central America. Hernandez, about the middle of the seventeenth century, described the huge stipular thorns of this tree and the way in which ants of a particular species eat into them from the apex, feed upon the pulpy interior substance, and make there their dwelling, feeding also upon the sweet secretion of the leaf-glands. As Belt (The Naturalist in Nicaragua) describes, the stipular thorns or horns, thus inhabited, grow still larger and in a different shape from those of the species which are unoccupied; and the ants are said to pay for their food and lodging by effectually keeping off herbivorous animals and other species of insects which otherwise would attack the tree. The ant-inhabited plants next made known were two woody *Rubiaceæ*, of Sumatra, a *Hydnophytum* and a *Myrmecodia*, which, in 1750, Rumphius described and illustrated, the first as *Nidus formicarum niger*, being inhabited by a kind of black ant, the second as *Nidus formicarum ruber*, because it harbored a red ant. Both shrubs are epiphytic, and both make a large tuber-like growth at the base where the attachment to the foster-tree takes place;—an abnormal and pathological growth, in the sense that the development is stimulated and aggrandized by the irritation of the colonizing ants which find food and lodging in the cavernous interior, yet natural because the tuberous enlargement begins in the germination of the plant before the ants attack it. It is the caulicle, or hypocotyledonous stem, which thus swells out when the embryo germinates, and this appears to form the whole of the large tuber. All this is beautifully illustrated by Beccari, in plates 8, 13, and 21 of the second volume of Malesia; where it is moreover shown that the swelling caulicle becomes hollow by its own growth. So it appears that these insect-colonized Rubiaceæ prepare in advance for their peculiar guests, although the amplitude of the lodging is doubtless increased by their subsequent action. Indeed Beccari states that the seedling plants which fail to be inhabited soon cease to grow. This, however, contradicts the

opinion of Treub, that the ants have nothing to do with the formation of this organ. Be this as it may, it is most supposable that this extraordinary formation was acquired gradually; that the normally fleshy caulicle of the ancestral plant, made a nidus by an insect, developed under the disturbing stimulus somewhat as a gall develops, until at length the tendency became hereditary and the singular adaptation of plant to insect was established. In formulating such an hypothesis, which falls in so naturally with prevalent conceptions, Signor Beccari, in his elaborate exposition, goes back very far for his starting point, even to the properties of protoplasm, the development of protista into protophytes and protozoa, the development of the former into higher-organized forms, and so on; coming down at length to the hypothesis that the various adaptations of flowers to insects, (irregularities in form, the development of nectaries, the growth of these into hollow spurs or sacs, etc.), have resulted from the irritant or disturbing action of visiting insects. The whole dissertation is interesting and ingenious.

Beccari, in Malesia, indicates 16 species of *Myrmecodia* and 29 of *Hydnophytum*. This association with ants is established in many of them. He also describes and figures less remarkable cases of ant-lodgings in stems thereby more or less distorted by enlargements, in a *Myristica*, in an *Endospermum* and another of the Euphorbiaceæ, a *Clerodendron*, and in three Palms of the genus *Korthalsia*. A. G.

9. *Lloyd's Drugs and Medicines of North America.* — The sixth part, issued in June, 1885, extending from p. 177 to 208, brings to a close the elaborate medical and botanical history of *Hydrastis*, with 43 bibliographical references to the botany alone. It devotes a good figure and a page of letter-press to *Trollius laxus*, and for the rest is occupied with *Coptis*, which is left unfinished. There are excellent figures and dissections of *C. trifolia* and also of *C. occidentalis* and *C. asplenifolia* of the Pacific side of the continent, and an elaborate account of the minute anatomy of *C. trifolia* by Louisa Reed Stowell, with copious and admirably drawn illustrations. The main active principle of the plant is said to be berberine. So the single order *Ranunculaceæ* is likely to fill a volume. A. G.

10. *Transactions and Proceedings of the New Zealand Institute*, for 1884, vol. xvii, 1885.—The Botanical papers, p. 214–306, show great activity among the resident botanists of New Zealand, consisting of articles by T. Kirk, W. Colenso, D. Petrie, J. Adams, H. C. Field, and W. S. Hamilton. Many new species are characterized; and there are still ample harvests to be gathered in these large and diversified islands. A. G.

11. *Revision of the North American Species of the Genus Scleria;* by N. L. Britton.—This forms pages 228–237 of the third volume of the *Annals of the New York Academy of Sciences.* As the New York Academy is a continuation and amplification of the former Lyceum of Natural History, which in

former days brought out distinguished work in *Cyperaceæ*, this continuation of its Annals upon the same line is opportune and welcome. No new species are proposed in this critical revision, but several new varieties are characterized. A. G.

12. *Beitrag zur Kenntniss der Sarraceniaceen.*—An inaugural Dissertation for the philosophical doctorate at the University of Erlangen, May, 1885, by PAUL ZIPPERER of Munich, octavo, with one double plate of anatomical detail; worked out upon *Sarracenia purpurea, flava,* and *variolaris, Darlingtonia* and *Heliamphora.* A. G.

13. CHARLES WRIGHT, our associate of many years, a veteran botanist and extensive explorer, the most kindly of men, died suddenly, of heart disease, at his home in Wethersfield, Conn., August 11th, at the age of 74. A. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Transactions of the Connecticut Academy of Arts and Sciences*, vol. vi, Part 2. New Haven, Conn.—This part closes the volume with the 516th page. It contains the following memoirs: J. H. EMERTON, on New England Spiders of the family Epeiridæ, with 8 plates of crowded figures; and on the Lycosidæ, with 4 plates; R. H. CHITTENDEN and H. E. SMITH on the diastatic action of Saliva, as modified by various conditions, studied quantitatively; S. W. WILLISTON, on N. A. Conopidæ (conclusion); A. E. VERRILL, Third Catalogue of Mollusca recently added to the fauna of New England coast and the adjacent Atlantic, consisting mostly of deep-sea species, with notes on others previously recorded, with 4 plates; K. J. BUSH, Mollusca of Cape Hatteras, with one plate.

Professor Verrill's paper reports that the Cephalopoda here described were obtained at depths between 600 and 2574 fathoms; the Gastropoda between 43 and 2574 fathoms; and one species *Lamellaria pellucida* has this great range; the *Scaphopoda* from 70 to 1594 fathoms; the *Lamellibranchiata* at various depths down to 2221 fathoms, of which, species of Pecten occur down to 1525 fathoms, of Arca at 2021 fathoms, and of Limopsis at 2221 fathoms. Two Brachiopoda are included, one *Atretia gnomon* 1525–1594 fathoms, and *Discina Atlantica* 1198–2021 fathoms.

2. *A Catalogue of Scientific and Technical Periodicals* (1665 to 1882) *together with Chronological Tables and a Library Check-List*, by H. CARRINGTON BOLTON. 774 pp. 8vo. Washington, 1885. Smithsonian Miscellaneous Collections, No. 514.—This catalogue with its chronological tables and check list is the result of a number of years of work, and its completeness is the more remarkable that the best libraries of the world do not exist this side of the Atlantic. The volume is of great value for its carefully prepared alphabetized list of scientific and technical periodicals of all nations; but this value is very much enhanced by the addition of the Chronological Tables, which give a student

the means of finding the date of any volume cited in a reference and thus ascertaining the time of the investigation or discovery referred to. Such references ought always to have the date affixed, but, unfortunately, the great majority fail of this. The work has been handsomely and generously printed under the auspices of the Smithsonian Institution, and makes a compact and convenient volume. The science of the world owes much to Professor Bolton, and also the Smithsonian Institution for the volume.

3. *Contributions to North American Ethnology*, volume v, 4to. Washington, 1882. (U. S. Geographical and Geological Survey, J. W. POWELL in Charge.)—This important volume contains three extended memoirs, illustrated by many excellent plates. The subjects are: Observations on cup-shaped and other lapidarian sculpture in the Old World and in America, by Charles Rau, 112 pp.; On prehistoric trephining and cranial amulets, by Robert Fletcher, 30 pp.; A study of the manuscript Troano, by Cyrus Thomas, with an introduction by D. G. Brinton, pp. xxxvii and 237.

4. *The Microscope in Botany: A Guide for the Microscopic Investigation of Vegetable Substances;* from the German of Dr. JULIUS W. BEHRENS; translated and edited by Rev. A. B. Hervey, assisted by R. H. Ward. 466 pp. 8vo, Boston, 1885 (S. E. Cassino & Co.).

OBITUARY.

M. HENRI FRESCA, member of the French Academy since 1872, and distinguished for his physical researches and in mechanical engineering, died on the 21st of last June.

M. HENRI MILNE EDWARDS, the eminent zoologist of France, author of works on general zoology and the Invertebrata, and particularly the departments of crustacea and corals, died in July, in his 86th year, having been born in Belgium, at Bruges, on the twenty-third of October of the year 1800.

PROF. W. C. KERR, State Geologist of North Carolina for eighteen years, and more recently connected with the United States Geological Survey, died at Asheville, N. C., on the ninth of August, of consumption. Prof. Kerr was an excellent observer in geology, and in his few publications brought out results of great interest. He was the first in the country to call attention to, and rightly explain, the unequal steepness in the opposite banks of streams, where flowing through yielding deposits (Rep. Geol. N. Carolina, vol. i); and the first to appreciate adequately and describe the action of frost in producing the deep movement and bedded arrangement of loose material on slopes (this Journal, III, xxi, 1881), the depth in North Carolina being such as to indicate, in his view, the unusual conditions of a Glacial era. Owing to deficient appropriations, only one volume of the State Survey Reports has been published. He was occupied with investigations under the United States Geological Survey when his failing health brought his labors to an untimely close.

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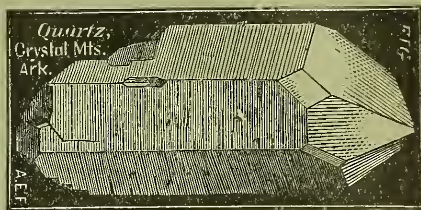

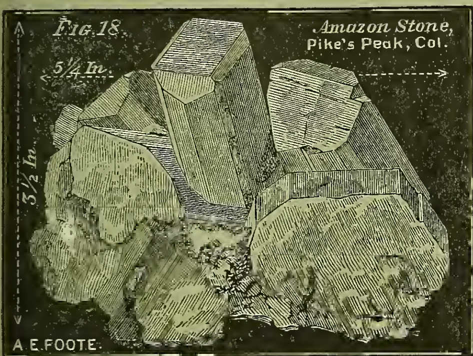

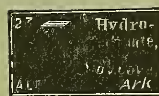

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No. 178, Vol. XXX. OCTOBER, 1885.

Established by BENJAMIN SILLIMAN in 1818.

THE AMERICAN JOURNAL OF SCIENCE.

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THIRD SERIES.

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THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

Art. XXXII.—*On the Crumpling of the Earth's Crust*;* by William B. Taylor.

The causes and conditions of the vast series of movements which have taken place throughout the earth's crust, present perhaps the most fundamental problem of physical or dynamical geology. And yet this problem is one of the most difficult and unsettled in the range of geological inquiry. It is indeed a very complex one, embracing several quite distinct features. The enormous lateral compressions which have everywhere crushed, folded, and contorted the stratified rocks, culminating in vast uptilted and fractured mountain ranges, may or may not have been instrumental in the elevation of continents, and the depression of ocean-beds. The alternate subsidence and emergence through miles of vertical oscillation of continental areas—successively repeated (by the agency of internal or external influences)—may or may not be correlated with equivalent upheavals and submergence of the present oceanic areas. And whether we infer with some geologists that ocean and continent have more than once changed places, or on the other hand conclude with Dana, and others, that the skeletons of the existing continents were rudely blocked out and the foundations of the great deep laid down in pre-archæan times, we are on either supposition beset with grave and peculiar alternative perplexities. The hypothesis of Joseph Le-

* Read before the Philosophical Society of Washington, May 23, 1885.

Conte, and others, that the tangential stresses of secular lateral compression have resulted in very great differences of thickening in the crust, deepening simultaneously the foundations of the mountains as they were slowly protruded upward, while leaving the great depressions proportionately attenuated, so that the internal topography of the shell is an obverse corresponding closely with its external contours,—although an hypothesis framed for the avoidance of supposed hydrostatical difficulties in the conception of a layer of approximately uniform thickness, floating on the liquid abyss,—is yet on both geological and physical grounds exposed to at least as weighty objections.

It is proposed in this communication to consider only the probable origin of that great contraction of the terrestrial crust so widely manifested in its plications or crumplings, increasingly marked as we approach the great oceans.

The liquidity of our globe, and the relative thinness of its incrusted envelope,—as attested by all legitimate *geological* induction,—will be assumed without misgiving or hesitancy; and the supposed mathematical arguments for its solidity—ignored as essentially fallacious and wholly inconclusive. It must be lamented that the professional mathematicians have as yet contributed nothing to the advancement or to the extension of geologic theory:—not from lack of analytic skill in a Thomson, or a Darwin, but from the want of proper data to justify the conclusions they have so boldly hazarded.

On the universal recognition from numerous evidences that our earth is a cooling globe, still highly heated within, the suggestion by Elie de Beaumont, in 1830, that "the inequality of cooling [between the interior and exterior portions] would place the crusts under the necessity of continually diminishing their capacities . . . in order that they should not cease to embrace their internal masses exactly," and that this condition "may with great probability completely account for the ridges and protuberances which have been formed on the external crust of the earth:"*—this seemed so natural and obvious an explanation, that it commanded a very general assent among geologists.†

* Phil. Mag., Oct., 1831, vol. x, pp. 263, 264.

† "Elie de Beaumont and some other geologists have attributed these effects and especially the elevation of mountains to the contraction of a cooling globe, and this appears to be the only one adequate for the results. . . . In attributing the plications of the earth's crust and the elevation of most mountains to a lateral pushing movement within the crust, there is nothing hypothetical. The statement is the expression simply of a fact. The conclusion that this tension [compression] is due to the contraction of a cooling globe has not yet been fully established. It is here adopted, because no other that is at all adequate has been presented."—*Prof. J. D. Dana, Manual of Geology*, 1863, part iv, chap. vi, sect. 1, pp. 721 and 725.

There were not wanting some however who questioned the sufficiency of the cause to accomplish the observed results; and our colleague Captain C. E. Dutton some ten or twelve years ago with great acuteness pointed out the entire inadequacy of the "Contractional Hypothesis," from an engineering stand-point.*

Mr. Robert Mallett, a zealous supporter of the contractional hypothesis, estimated that the diameter of the earth is now about 189 miles less than it was when entirely fluid.† That is, that the surface has collapsed ninety-four miles. Liberal as is this allowance (a surface contraction linearly of $\frac{1}{42}$), it would still be utterly insufficient to represent the actual average of compression. But in point of fact, the whole amount of cooling, and therefore of possible contraction by cooling, has been very much less than has generally been assumed.‡

1. No permanent crust could have been formed upon the molten globe until convective currents had well nigh ceased; that is, until the entire mass had cooled down very nearly (say —within a hundred degrees F.) to the congealing point.§

2. After the formation of a permanent crust, the greater part of the heat escaping through it would probably have been the "latent heat" from consolidation of new accessions to its inner surface, with little reduction of the sensible temperature of the inner mass.

3. Whatever *expansion* by freezing or consolidation of such accessions to the inner surface might occur, would to their extent counteract the effects of interior shrinkage.

4. The further escape of the encased heat through several miles of so poor a conductor as the earth's crust, must have since gone on with extreme and ever lagging slowness; and even through the lapse of a million centuries, must have been

* This Journal, 1874, vol. viii, pp. 113–123. The Rev. Osmond Fisher, in his Physics of the Earth's Crust (8vo, Lond., 1881), has also successfully attacked the hypothesis, chapter vii, pp. 73–75.

† Phil. Trans. Roy. Soc., June 20, 1872, vol. clxiii, pp. 147–227.

‡ "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."—*C. E. Dutton*, this Journal, 1874, vol. viii, p. 121.

§ Mr. William Hopkins, the author of the unfortunate "precession" argument against a fluid earth (or rather against a flexible earth-crust), in the same memoir, well remarks: "If the matter composing the globe was originally in a high state of fluidity from heat, the process of cooling would undoubtedly in the first instance be by circulation." And even supposing solidification to commence at the center, he maintains: "The superficial parts of the mass must in all cases cool the most rapidly; and now (in consequence of the imperfect fluidity) being no longer able to descend, a crust will be formed on the surface; from which the process of solidification will proceed far more rapidly downward, than upward on the solid nucleus."—*Phil. Trans. Roy. Soc.*, January 17, 1839, vol. cxxix, pp. 381–384.

largely compensated by the frictional heat continually generated by the tidal waves in the superior layers.*

Indeed we may venture the opinion that the ultimate refrigeration of a body like the earth must require a period of time whose curve would be almost hyperbolic to the asymptote; and that the entire solidification of a planet has not yet occurred in the solar system. Even our Moon, with a mass less than one-eightieth of our own, and with an exterior cooled probably below the freezing point of mercury, may still be very hot if not partially molten within.†

The volumetric contraction of our globe since the formation of its crust may therefore be regarded as practically very insignificant. On the other hand if we attempt to form an idea of the amount of average shortening of the strata in a given zone about the earth, from the amount of corrugation and displacement observed (assuming from the analogy of submarine contours that a corresponding degree of compression exists throughout the ocean beds), an allowance of one-eleventh, or of a primitive excess by one-tenth of the present girth, would probably not be regarded by the geologist as an undue assumption. Professor Alphonse Favre of Geneva in detailing some experiments to reproduce by lateral compression in layers of tenacious clay, the plications of strata, has estimated a linear contraction of one-third, as exhibited by portions of the Swiss Alps.‡ Our own Appalachians and western Sierras probably indicate as great an amount of shortening.

But an expansion of the circumference of a sphere by one-tenth, involves an increase of its volume of one-third; of course a wholly inadmissible assumption on any hypothesis of the formation of a crust. The conception of a gaseous or vaporous *intumescence* of a body like the earth may be dismissed at once, as simply opposed to physical possibility.§

* Mr. George H. Darwin has estimated that the internal heat developed by "the lengthening of the sidereal day from 5h. 36m. to 23h. 56m. would be sufficient (if applied all at once) to heat the whole mass about 3000° F., supposing the earth to have the specific heat of iron."—*Phil. Trans. Roy. Soc.*, Dec. 19, 1878, vol. clxx, pp. 495 and 535. And in a subsequent memoir, computing this past transformation of energy as equal to thirteen and a half times the whole remaining kinetic energy of the present rotation, he arrives at the somewhat startling conclusion: "Thus it appears that at the present rate of loss, the internal friction gives a supply of heat for 3560 million years." (Same volume, pp. 561 and 592.) He infers however that only about one-fiftieth of the present downward increase of temperature is referable to the past internal friction.

† This is of course in very strong antagonism to the conclusions reached by Sir William Thomson's elaborate calculations.

‡ La Nature, Sept. 28, 1878, vol. vi, part ii, pp. 278–283. A full abstract of the paper is given in Nature, Dec. 5, 1878, vol. xix, pp. 103–106.

§ This suggestion is broached by Mr. Fisher in his Physics of the Earth's Crust, and appears to be in some sense maintained in a recent German work (not seen by the present writer), on Terrestrial Physics (1884), by Von Siegmund Günther.

What solution of this seeming paradox have geologists to offer, after the demolition of the "contraction hypothesis?" As yet none has appeared. It would seem then that the problem must be attacked from a different quarter.

For a century it had been observed by astronomers that the moon's path exhibited a small acceleration (amounting to about 10″ or 11″ in a century), not accounted for by theory. Laplace investigating the problem, showed that as a result of the secular diminution of the earth's orbital eccentricity, there should be an acceleration of the moon of about 10″ in a century. And he announced it as established from a computation of the ancient eclipses that our "sidereal day has not changed by so much as one-hundredth of a second since the time of Hipparchus."*

That remarkable man, Dr. Julius R. Mayer of Heilbronn, in his very original essay on "Celestial Dynamics" published in 1848, showed that as the oscillations of the pendulum require for their maintenance a constant expenditure of power, so equally do the oscillations of the ocean tidal wave. "The moving waters rub against each other, against the shore, and against the atmosphere; and thus meeting constantly with resistance, would soon come to rest, if a *vis viva* did not exist, competent to overcome these obstacles. This *vis viva* is the rotation of the earth on its axis; and the diminution and final exhaustion thereof will be a consequence of such action. *The tidal wave causes a diminution of the velocity of the rotation of the earth.*" In the next chapter, discussing the consequence of Laplace's demonstration of the constancy of the length of the day in historic times, Mayer continued: "This result, as important as it was convenient for astronomy, was nevertheless of a nature to create some difficulties for the physicist. With apparently good reason it was concluded that if the velocity of rotation had remained constant, the volume of the earth could have undergone no change [by loss of heat]. . . . The earth's radius measures 6,369,800 meters, and therefore its length ought not to have diminished more than 15 centimeters [6 inches] in 25 centuries. . . . Considering what is known about the expansion and contraction of solids and liquids by heat and cold, we arrive at the conclusion that for a diminution of one degree [C.] in temperature, the linear contraction of the earth cannot well be less than the hundred-thousandth part. . . . The reason why in spite of this accelerating cause, the length of the day has nevertheless remained constant since the most ancient times, must be attributed to an opposite retarding action. This consists in the attraction of the sun and moon on the liquid parts of the earth's surface, as explained in the last chapter. According to the cal-

* Sir John Herschel's Outlines of Astronomy, chap. xviii, sec. 908. (See Laplace's Traité de Mécanique Céleste; tom. ii, liv. 5, and tom. iii, liv. 7.)

culations of this chapter, the retarding pressure of the tides against the earth's rotation, would cause—during the lapse of 2,500 years—a sidereal day to be lengthened to the extent of one-sixteenth of a second. As the length of the day however has remained constant, the cooling effect of the earth during the same period of time, must have shortened the day one-sixteenth of a second. A diminution of the earth's radius to the amount of 4½ meters in 2,500 years, corresponds to this effect. Hence in the course of the last 25 centuries, the temperature of the whole mass of the earth must have decreased one-fourteenth of a degree [C.]."* In this ingenious adjustment, the eminent philosopher doubtless greatly over-estimated the earth's cooling and consequent contraction, in order to compromise the "difficulties of the physicist" with the authority of the astronomer.

In 1853, our colleague Mr. William Ferrel in a paper "On the effect of the Sun and Moon upon the rotatory motion of the Earth," independently undertook the mathematical investigation: the very name of Mayer being then (it is scarcely necessary to state) as wholly unknown in this country as it was in England. In this important paper it is shown that external forces of the second order—neglected by Laplace as producing no sensible effect upon the rotation period, are—though very small—decidedly appreciable, and "must, if not counteracted by some other effect, produce a sensible variation in the earth's rotatory motion." Assuming a lunar ocean tide of two feet, with a lag of two hours, Mr. Ferrel estimated an equatorial retardation of 37 miles per century, increased by the solar tide to 44 miles per century, which would give an apparent lunar acceleration of 1′ 24″ in a century. "As no such acceleration has been observed—above what is accounted for otherwise, the rotatory motion of the earth must be nearly uniform; and the above effect of the sun and moon must be accounted for by the gradual contraction of the earth through a loss of temperature." This would require a subsidence of the surface, or a reduction of the earth's radius (supposing the contraction equable through the mass), of about one foot per century.†

In the same year, a few months earlier, Mr. John C. Adams in a careful memoir "On the secular variation of the moon's

* L. E. D. Phil. Mag., May and June, 1863, vol. xxv, pp. 403, and 423–427. Also Am. Jour. Sci., 1864, vol. xxxviii, pp. 398, and 409–413.

† Gould's Astronom. Jour., Dec. 8, 1883, vol. iii, pp. 138–140. In this paper (p. 141), Mr. Ferrel shows that the similar retarding effect of the earth upon a primitive rotation of the moon (supposed originally fluid) must be 562 times greater than the converse earth retardation; and that this therefore sufficiently explains the present reduction of the moon's rotation to the period of a lunar month. This is believed to be the first suggestion of the now accepted cause of this peculiarity of the moon. Mr. Ferrel's paper is dated September, 1853. A similar explanation was offered by Helmholtz in the following year.

mean motion," pointed out an incompleteness in Laplace's investigation by the neglect of a certain series of terms; and he showed that the effect of this series would be to reduce the actual acceleration due to the diminution of eccentricity in the earth's orbit to about 6″; leaving the residual discrepancy of 4″ or 5″ of acceleration (as compared with the positions indicated from the ancient eclipses), to be still accounted for.*

Herman Helmholtz, in a lecture on the "Interaction of Natural Forces," delivered at Königsberg, Feb. 7, 1854 (without knowledge of Adams' work), pointed out to his hearers that "The motions of the tides, however, produce friction; all friction destroys *vis viva*, and the loss in this case can only affect the *vis viva* of the planetary system. We come thereby to the unavoidable conclusion that every tide—although with indefinite slowness, still with certainty—diminishes the store of mechanical force of the system; and as a consequence of this the rotation of the planets around their axes must become more slow."†

Charles Delaunay, who with fine analytic skill had for several years been engaged on the theory of the moon, in 1859 undertook a careful examination of the results obtained by Mr. Adams, which impeached the accuracy of his great countryman, Laplace; and in a memoir "On the secular acceleration of the mean movement of the Moon," presented to the French Academy, he fully confirmed the bold criticism of Mr. Adams.‡ And he affirmed that the necessary elongation of the day by tidal retardation is quite competent to account for the apparent residual acceleration of the moon.

In 1862 the eminent mathematician, Arthur Cayley, independently investigated the problem; and in a paper "On the secular acceleration of the Moon's mean motion," he also corroborated the correctness of Mr. Adams' great discovery.§

In 1864, eleven years after his first suggestive memoir, Mr. Ferrel returned to the subject of the elongation of our day. In a "Note on the influence of the Tides in causing an apparent secular Acceleration of the Moon's mean motion," he remarks: "At the time of the original publication of Mayer's paper, and also at the time of my own, Laplace's result of

* Phil. Trans. Roy. Soc., June 16, 1853, vol. cxliii, pp. 397–406. Also Monthly Notices, Roy. Astr. Soc., 1853, vol. xiv, pp. 59–62. And Reply to Objections, M. N., 1860, vol. xx, pp. 225–240, and 279–280.

† L. E. D. Phil. Mag., June, 1856, vol. xi, p. 513. Also Am. Jour. Sci., 1857, vol. xxiv, p. 212.

‡ Comptes Rendus Acad. Sci., Jan. 17 and April 25, 1859, vol. xlviii, pp. 137, 138, and 817–827. Also numerous papers on the subject in succeeding volumes. Especially a paper "On the apparent acceleration of the mean motion of the Moon, due to the actions of the Sun and Moon on the waters of the sea."—Comptes Rendus, 1866, vol. xlii, pp. 197–200, 575–579, 704–707, etc.

§ Monthly Notices, Roy. Astr. Soc., 1862, vol. xxii, pp. 171–230.

about 11′ per century for the amount of acceleration of the moon's mean motion arising from a secular change of the eccentricity of the earth's orbit was supposed to be correct; but since that time it is known that Adams and Delaunay, and also quite recently, Mr. Cayley, have all obtained less than 6″ for the amount of secular acceleration per century. This determination, compared with the most reliable determination of the acceleration deduced from the discussion of the recorded observations of ancient eclipses, leaves about 6″ to be accounted for by tidal action or some other *unknown cause.*" He then proceeds to state that Mayer's hypothesis of a one-and-a-quarter hour retardation, and his own of a two-hour retardation of the tidal wave, "are both much too great to be reasonable hypotheses, or to be necessary to account for the balance of the moon's secular acceleration over and above the late determination." And he concludes "that tidal action is adequate to account for 6″ of secular acceleration upon a very reasonable and probable hypothesis with regard to the magnitude and retardation of the tidal wave by friction after making due allowance for the effect of a probable contraction of the earth's volume."*

In 1867, Prof. William Thomson, discussing the same subject says: "The tides must tend to diminish the angular velocity of the earth's rotation, and this tendency is not counterbalanced to more than a very minute degree by the tendency to acceleration which results from secular cooling and shrinkage."†

This brief historic summary has been introduced to emphasize the strengthening conviction among physicists of the necessary variability of that standard unit of time, which to the astronomers had seemed the most permanent constant of our solar system. And these references serve to establish beyond a reasonable doubt the remarkable fact that our present day is considerably longer than the day of early geologic times. For nearly two centuries has the moon been seen in advance of its periodic position—to the small amount (as estimated from ancient eclipses), of about ten or eleven seconds of angle per century. Of this relative displacement about one-half is now known to be due to the actual falling behind of the terrestrial observer, and the other half to Laplace's great discovery—the influence of the diminishing eccentricity of the telluric orbit.‡

Mr. George H. Darwin (as is well known) has in recent years been engaged in elaborate investigations of this subject, and he has drawn some important consequences from his discussion.

* Proceedings Am. Acad. Arts and Sci., Dec. 13, 1864, vol. vi, pp. 379–383.

† Thomson and Tait's Natural Philosophy, 1867, vol. i, Sec. 830, p. 686.

‡ An influence which must reach its inferior limit in about one precession period.

He says: "As we look at the whole series of changes from the remote past, the ellipticity of figure of the earth must have been continually diminishing, and thus the polar regions must have been ever rising, and the equatorial ones falling; but as the ocean always followed these changes they might quite well have left no geological traces. The tides must have been very much more frequent and much larger, and accordingly the rate of oceanic denudation much accelerated. The more rapid alternations of day and night would probably lead to more sudden and violent storms, and the increased rotation of the earth would augment the violence of the trade winds, which in their turn would affect oceanic currents. Thus there would result an acceleration of geological action."*

In this suggestive retrospect one important physical condition, however—which probably *has* "left its geological traces" on our globe—does not appear to have attracted the author's attention. A shell formed upon the spinning earth when its oblateness was considerably greater than at present obviously could not fit the spheroid as the ellipticity of its meridians diminished. Not only would the crust be quite sensibly too large, as a whole, but especially would its equatorial girth have to be notably reduced.

If in imagination we may carry back the formation of a consistent crust (of some few miles thickness) to an epoch when the rotation of our planet was at four times its present rate—that is, when the day measured but six of our hours,—the equatorial radius (assuming a true ellipsoid of revolution, and neglecting the small amount of contraction by loss of heat), would have been about one-tenth greater than it now is, or 4359 miles; and the polar radius about one-sixth less, or 3291 miles. In other words, the poles would have been about 658 miles nearer the center of the earth than they are at present, and the equatorial protuberance about 396 miles higher than at present.

With an equatorial shell one-tenth greater in circumference than the present dimensions, it is evident that from the very slow but never ceasing contraction due to diminution of rotatory motion, this crust would be subject to an unremitting stress of lateral compression as relentless as that from the old hypothetic shrinkage of volume by reduction of temperature. Is it not precisely this *morphologic* contraction whose effects and records are everywhere apparent in the crumpling of the earth's crust? Here is a true cause: here is a sufficient cause: here is a necessary cause—so importunate that it cannot readily be dispensed with or explained away.

An objection to this hypothesis, based on the supposed

* Phil. Trans. Roy. Soc., December 19, 1878, vol. clxx, p. 532.

solidification of our planet since its present form has been reached, requires a notice here, rather from the character of its source, than from its own intrinsic force or importance.

Professor William Thomson, adopting Mr. Adams' estimate of tidal retardation, thus argues: "If the rate of retardation had been uniform since ten million centuries back, the earth must have been then rotating faster by one-seventh than at present, and the centrifugal force greater in the proportion of 64 to 49. If the consolidation took place then, or earlier, the ellipticity of the upper layers of equal density must have been $\frac{1}{230}$ instead of about $\frac{1}{300}$ as it certainly now is. It is impossible to escape the conclusion that the date of the consolidation is considerably more recent than a thousand million years ago.*

Professor Peter G. Tait (as if not to be outdone), is still more emphatic and positive in his enunciation. He says: "It being thus established that the rate of rotation of the earth is constantly becoming slower the question comes: How long ago must it have solidified in order that it might have the particular amount of polar flattening which it shows at present? Suppose, for instance, it had not consolidated less than a thousand million years ago. Calculation shows us that at that time, on the most moderate computation, it must have been rotating at least twice as fast as it is now rotating. That is to say, the day must have been 12 hours long instead of 24. Now if that had been the case, and the earth still fluid throughout, or even pasty, that double rate of rotation would have produced four times as great centrifugal force at the equator as at present, and the flattening of the earth at the poles and the bulging at the equator would have been much greater than we find them to be. We say then, that because the earth is so little flattened it must have been rotating at very nearly the same rate as it is now rotating, when it became solid. Therefore, as the rate of rotation is undoubtedly becoming slower and slower, it cannot have been many millions of years back when it became solid, else it would have solidified into something very much flatter than we find it."†

The supposition that a granite mountain or equatorial protuberance 400 miles high or 100 miles high, could for a moment support itself would hardly be entertained by a practical civil engineer.‡

It is now nearly forty years since Herbert Spencer, with a juster physical insight, contended and satisfactorily showed that a solid earth (of any shape) would assume the oblate

* Nat. Philos., 1867, vol. i, Sec. 830, p. 687.

† Lect. on recent advances in Phys. Sci., 1876, lect. vii, pp. 173, 174.

‡ The limiting modulus of height of a granite pyramid (equaling one side of its square base), is somewhat less than eleven miles.

spheroidal form due to its rate of rotation, as certainly and promptly as if it were liquid.*

Granting then that the earth were solidified with an oblateness such as has just been assumed, its present dimensions—and probably its present topography—would not be very different from what we now find them.

But another very serious error in the estimates of both these quotations should not be overlooked. While it is quite impossible to calculate the time when the equatorial radius was one-tenth greater than at present, it is very certain that any estimates based on the existing retardation from the ocean tides, must be utterly fallacious. Not only has this tidal action been continually decreasing (by some small exponent) from the earliest times, but the bodily tides of the liquid or solid earth have undoubtedly played a far greater part than the superficial ocean tides—in the absorption and destruction of its rotatory momentum.† It is not at all probable therefore that it has required "a thousand million years" to effect the present equatorial contraction of one-eleventh from the former diameter of 8,718 miles.

The much debated question of the probable degree of rigidity of our planet is therefore quite irrelevant to the problem before us. And yet with so much confidence and persistence has the pet-hypothesis of its entire solidity been maintained,—the temptation is strong to waste upon it a collateral glance.

Professor William Thomson contends: "Had the solid part of the earth so little rigidity as to allow it to yield in its own figure very nearly as much as if it were fluid, there would be very nearly nothing of what we call tides, that is to say, rise and fall of the sea relatively to the land; but sea and land together would rise and fall a few feet every twelve lunar hours. This would (as we shall see) be the case if the geological hypothesis of a thin crust were true. The actual phenomena

* "However great in a given portion of matter may be the excess of the form-preserving force over the form-destroying force, it is clear that if during augmentation of bulk the form-preserving force increases only as the *squares* of the dimensions, while the form-destroying force increases as their *cubes*, the first must in time be overtaken and exceeded by the last; and when this occurs the matter will be fractured and re-arranged in obedience to the form-destroying force."—*Herbert Spencer*. And the author estimates that "the most tenacious substance with which we are acquainted—when subjected to the same forces that are acting upon the earth's crust—would exceed the limit of self-support determined by the above law, before it attained the thousand-millionth of the earth's bulk."—*L. E. D. Phil. Mag.*, March, 1847, vol. xxx, pp. 194–196.

† Mr. G. H. Darwin remarks: "Whatever may be thought of the theory of the viscosity of the earth and of the large speculations to which it has given rise, the fact remains that nearly all the effects which have been attributed to the action of bodily tides would also follow (though probably at a less rapid rate), from the influence of oceanic tides on a rigid nucleus."—*Phil. Trans. Roy. Soc.*, Dec. 19, 1878: vol. clxx, p. 538.

of tides therefore give a secure contradiction to that hypothesis."*

That a siliceous crust of 20 miles average thickness, and an overlying aqueous ocean of three miles average depth, should have (as required by the argument) so equal a coefficient of mobility, that sea and land could thus "together rise and fall," might well be pronounced incredible. Professor Thomson himself proceeds to very seriously damage his "secure contradiction" of "the geological hypothesis," by adding immediately after the passage just quoted: "We shall see indeed presently that even a continuous solid globe of the same mass and diameter as the earth would if homogeneous and of the same rigidity as glass, or as steel, yield in its shape to the tidal influence, three-fifths as much, or one-third as much, as a perfectly fluid globe."†

When we have learned the elements of depth, inertia and fluidity of the disturbed layers beneath the geologic crust, then and not till then may the beginnings of a mathematical theory of the telluric tides be attempted; and some plausible measure of their "lagging" be suggested. But in our profound and insuperable ignorance of these interior conditions determining the magnitude and direction of the tidal crests, it is idle for the mathematician (with whatever array of formidable differential equations) to gravely assure us that "no very considerable portion of the interior of the earth can even distantly approach the fluid state."‡

It is in fact quite incontrovertible, that whether the geologic crust have for its content a glowing lava, or Thomson's ideal steel, it is equally subject to lunar and solar tides fully comparable to the more apparent ocean tides. This last surviving argument for solidity, from the theory of the tides, should therefore be dismissed as being no less futile than the abandoned argument, from the theory of precession.§

* Nat. Philos., 1867, vol. i, sect. 833, p. 690. Retained in the new edition.

† Same work; sect. 833, pp. 690, 691, and sect. 834, pp. 691–694.

‡ Mr. George H. Darwin in a very abstract memoir: "On the bodily tides of viscous and semi-elastic spheroids, and on the Ocean Tides upon a yielding nucleus," read before the Royal Society of London, May 23, 1878, arrives at the conclusion: "Unless the viscosity were very much larger than that of pitch, the viscous sphere would comport itself sensibly like a perfect fluid, and the ocean tides would be quite insignificant. It follows therefore that no very considerable portion of the interior of the earth can even distantly approach the fluid state."—*Phil. Trans. Roy. Soc.*, vol. clxx, p. 28.

§ Professor Thomson, who in 1862 had re-enforced and exaggerated the Hopkins argument, by maintaining that the earth's defect (by deformation) from the theoretical amount of precession, is "much smaller for instance than it would be if its effective rigidity were no more than the rigidity of steel," in consequence of an oral discussion with our colleague Professor S. Newcomb, in 1876, with a frankness worthy of all praise, made a full surrender of the position, in his address before the mathematical and physical section of the British Association: Report Brit. Assoc., Sept., 1876, vol. xlvi, part II, pp. 1–12), and has since con-

Returning to the consideration of the varying oblateness of the terrestrial ellipsoid, and its geological consequences, we naturally inquire what indications remain to corroborate or to impair our hypothesis. Could we ascertain the approximate amount of lateral excess of shell by its corrugations, say for each parallel zone of ten degrees, these enlarged circumferences divided by the cosine of the latitude would give us the variations (if any) of envelope, throughout a given meridian. This is of course entirely beyond our reach. We may, however, infer that the altitudes of mountain ranges should bear some rude proportion to the general amount of crumpling in neighboring districts; and that at least some approximate indication might thus be afforded or suggested. We might expect that circumpolar regions should be free from mountains or plications; and that the inter-tropical region should contain the highest elevations.

Dr. Arnold Guyot, in his excellent summary of the Physics of the Globe, prepared for Johnson's Cyclopædia, gives the following characteristic sketch: "In the New World therefore the highest lands are piled up in the southwest, in the Old World in the southeast. . . . On the whole, the reliefs begin with the vast low plains around the polar circle, and go on increasing from the shores of the Arctic Ocean toward the tropical regions. The highest elevations however are not found at the equator, but north of the Tropic of Cancer in the Old World, in the Himalayas 28° N., and north of the Tropic of Capricorn in the New World, in the Andes of Bolivia 16° S."*

The same eminent and conscientious physiographer, in his paper on the Ocean, remarks: "On the whole, the ocean beds become less deep toward the north pole, just as the lands become lower toward the same region."†

In this significant epitome—presented certainly with no view of supporting an hypothesis, we have perhaps as striking a suggestion of greater oblateness in former times as we could expect to find preserved to us. The curious circumstance that the highest lands on either hemisphere are found much nearer to the Tropics than to the Equator, it must be admitted, is difficult of explanation. Theoretically we should expect no sensible differences throughout the inter-tropic zone. We may however remember that the tides of the comparatively thin and yielding crust exert their greatest influence on the tropical zones; being for the larger portion of the year very near to

ceded "that a perfectly fluid spheroid has a precession scarcely differing from that of a perfectly rigid one."—Mr. Darwin, in *Phil. Trans. Roy. Soc.*, Dec. 19, 1878, vol. clxx, p. 525.

* Johnson's Cyclopædia, 1875: art. "Earth," vol. i, p. 1455.

† Same work, 1877, art. "Ocean," vol. iii, p. 918.

those circles; while the equatorial tides—during the equinoctial periods—occur when the sun (and the mean lunar plane) have their most rapid change of declination. If it be said that a tidal elevation of two or three feet is too utterly insignificant a space, and a cycle of half a year too utterly insignificant a time to affect the vast erections of secular progression, the reply is that this minute relaxation of particular zones—unceasingly repeated and continued—may well facilitate the slow but gathering movements of the crust from its strong compression, and may even to some extent give direction to their energies. As a concurrent though perhaps unimportant circumstance, the fact may be mentioned that while the northern tropic encounters 35 per cent of dry land, and the southern tropic nearly 25 per cent, the equator embraces but 21 per cent of land.

George Darwin, discussing the differences of tidal energy and lagging at different latitudes (in a viscous spheroid), by reason of which the retarding influence is greatest at the equatorial zone, suggests the possibility of this tendency causing a westward drift of mountain folds and continents in the lower latitudes (as compared with polar regions) and remarks: "There can be little doubt that on the whole, the highest mountains are equatorial, and that the general trend of the great continents is north and south in those regions. The theoretical directions of coast line are not so well marked in parts removed from the equator."* Here again, the striking significance of this physiographic generalization appears to be strangely overlooked.

It has been an occasion of some surprise that in all the geological literature to which I have had access, I have been able to find but a single allusion to the hypothesis here advanced in explanation of the palpable tangential compression of the earth's exterior strata.

This one allusion is given in Mr. O. Fisher's interesting work on the "Physics of the Earth's Crust," and is by him referred to, only to be dismissed in a brief paragraph. His reference is as follows: "The friction of the tides—whether oceanic or bodily—must necessarily have diminished the rotational velocity, and lessened the oblateness. The parts of the crust about the poles will consequently have been subjected to stretching, and those about the equator to compression. There is however no apparent reason immediately to connect the inequalities with this cause, for the continents do not occupy an equatorial belt—as they would do under this hypothesis, nor have the polar regions been free from the compression which all continental areas have experienced."†

* Phil. Trans. Roy. Soc., Dec. 19, 1878, vol. clxx, p. 589.
† Physics of the Earth's Crust, 8vo, Lond., 1881, chap. xiv, p. 183.

The expansion or stretching of the polar regions—referred to, though probably less than Mr. Fisher would imagine, would be attended with no dislocations, and would leave no traces. A crust of ten or twenty miles depth pressing upon its interior bed with a weight of five thousand or ten thousand tons to the square foot, would flow (excepting its exterior film of a mile or two),—on any relief of lateral pressure—as quickly and as uniformly as so much plastic clay.

To Mr. Fisher therefore belongs (so far as I am aware) the credit of the first suggestion of a possible connection between variation of oblateness and mountain-building;—even though by him discarded. He does indeed on the following page recur to the topic; and admitting that this variation *ought* to have produced some appreciable effect, he suggests that this apparent want of relation between the occurrence and the phenomena, favors the idea of a change of latitudes.

So strongly impressed is the writer with the inevitable operation and potency of this unquestioned retardation of rotation, that were all traces of any differential action masked and obliterated, he would still hold to it as the one efficient cause (alone—as yet suggested) to account for the prominent constriction of the crust—displayed in every land. But the differential traces of oblateness have *not* been obliterated;—masked though they may be to some extent, by other perturbations.

The suggestion of a change of axis, is one which will be entertained by the physicist with extreme hesitancy and caution. Professor William Thomson indeed has stated that "the axis of maximum inertia, and axis of rotation—always very near one another—may have been in ancient times very far from their present geographical position; and may have gradually shifted through 10, 20, 30, 40 or more degrees, without at any time any perceptible sudden disturbance of either land or water."* George Darwin also has admitted that if the earth be not solid, "As in successive periods the continents may have risen and fallen, the pole may have worked its way in a devious course some 10° or 15° away from its geographical position at consolidation; or may have made an excursion of smaller amount, and have returned to its old position."†

That under the continued stresses of precession and nutation—there should have been a slight slipping of the crust on its fluid interior, is not improbable. The poles of the shell in such case might describe a small spiral about the "axis of maximum inertia," but could probably never diverge therefrom more than a very few degrees. Nor could such gyration

* Report Brit. Assoc., 1876, vol. xlvi, part ii, p. 11.

† "On the Influence of Geological changes on the Earth's Axis of Rotation." Phil. Trans. Roy. Soc., Nov. 23, 1876, vol. clxvii, p. 305.

in any degree affect the sidereal axis of rotation—or its angle of obliquity with the plane of the ecliptic. Any such *geographical* change of the axis therefore as admitted by Thomson and Darwin, could only be a shifting of the mass of the earth (so to speak) upon an axis fixed in angular direction, with a corresponding shifting within its substance of the equatorial plane of oblateness.

Were we at liberty to imagine a translocation of the northern geographical pole—in the remote past, as far as to the arctic circle near Bering's Straits, the equatorial region would nearly correspond with Guyot's line of demarkation of the three northern—from the three southern continents.* This re-arrangement would also bring the highest portions of the South American Andes and of the Asiatic Himalayas much nearer to the equator, and the remarkable elevations of southern Europe—the Pyrenees, the Alps and the Caucasus (now all near the 45th degree of latitude), to the northern tropic. This, however, is mere speculation, for which no scientific warrant can at present be given.

An objection urged with considerable force by Captain Dutton against the hypothesis of the earth's contraction through secular cooling, should not be overlooked since it lies equally against the supposition here presented, and indeed against any hypothesis of general contraction. He remarks: "The determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated. . . . . 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. . . . . 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 on 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 Bering's Sea is a continuous belt, very narrow for most of the distance, but extremely disturbed throughout."†

* Professor Guyot, referring to the general direction of the line of separation marked out by the Caribbean and Mediterranean Seas, with their adjacent isthmuses, remarks: "These regions are parts of a broad transverse band whose position can be traced from Bering's Straits as a center, with a meridian arc of 80° radius, and which we would call the *central zone of fracture.*"—Johnson's Cyclopædia, 1875, vol. i, pp. 1449, 1450.

† Am. Jour. Sci., August, 1874, vol. viii, pp. 121, 122.

It cannot be denied that the difficulty here set forth is a very puzzling one. Especially inexplicable appears the instance last referred to—of an unbroken range of mountain foldings extending over more than a third of the globe's circumference. While the *force* at the command of the rotating planet is abundantly sufficient to accomplish the result evidently some supplementary considerations are requsite to give the observed *direction* to this force. Beyond all question, the surface of the earth *has* been subjected to a compressing stress of tremendous energy. To account for the stress is one problem; to account for its resultants is another and probably much more complicated one.

The mere mechanical difficulty, however, of transmitting stresses through comparatively undisturbed areas of hundreds of miles of a flexible, friable, and practically plastic crust—with a large coefficient of viscous friction beneath—is not so formidable as might at first sight appear. It must be borne in mind that the pressures derived from an action so slow as from century to century to be scarcely sensible, are of an order of very great intensity, but of very small quantity. Under the continued urgency of rapidly revolving tidal waves (though also of a very minute order), there does not seem any improbability in the supposition that with the long time element such stresses may gradually be equalized or transmitted by what might be termed a process of "conduction" almost indefinitely.

From various considerations we may infer that in all geological ages the progress of elevation has been in excess of that of degradation by erosion; that in all ages mountain building has been at a maximum; that is, the uplifted heights have been the greatest which the average thickness of the crust at the time was capable of supporting; so that the former has been a constant function of the latter, the ratio being probably not far from one-fifth.

We may also infer that this increasing maximum of elevation must now have practically reached its limit, since both the processes of equatorial contraction and of internal temperature reduction are going on with extreme and lengthening slowness; and the whole remaining subsidence of the inter-tropical oblateness cannot exceed five miles, during the vast ages in which the earth's rotation shall be entirely arrested.

Looking back through the long vista of the unmeasured past we behold in imagination our planet in its early youth, with its expanded tropical surface as yet unmarked by wrinkles, endowed with superfluity of rotary activity, and subject to far more energetic tides and winds and tempests than we know at present. Its primitive and lowly organisms with as yet unspecialized sensations, accustomed to a dusky day of a few brief hours succeeded by as short a night. The moon—perhaps much nearer and of ampler but paler disk—speeding through its phases in the quick period of a week or less. The sun—of enormously greater volume than we behold it—diffusing its hazy beams, and probably expending a much smaller amount of radiant light and heat upon the flat and dreary face of nature than it now does after many hundred million years of waste.* Such is the outline of early meteorological conditions which the geologist should take into his account when theorizing on the grand dynamics of his science.

But of that primeval period of fleeting days and of shortened axis, perhaps the only physical record and memorial left us is the wide array of distorted crumplings and ruptured foldings (culminating apparently in the lower or middle latitudes) which have formed the needful condition and environment for man's advancement, and which have never ceased to excite the wonder and admiration of his observant and inquiring intelligence.

Art. XXXIII.—*The Old Tertiary of the Southwest;* by E. W. Hilgard.

In an article published in the June and July numbers of this Journal, Dr. Otto Meyer undertakes to show, not only that numerous supposed species of fossils heretofore described from the Tertiary of the southwestern States should be canceled as being mere variations of no specific value, but also that there is good reason to suppose that the stratigraphic succession of the several stages, as heretofore understood and accepted, is incorrect and actually requires to be turned upside down. As regards the former part of his thesis I thoroughly agree with him, if not in detail at least in the general issue. As regards his second point, it is simply incorrect; and it is difficult to understand how, if Dr. Meyer took the pains to do more than look over the lists of fossils in my report on the geology of Mississippi, he could entertain such a proposition for a moment. The only explanation of his error can be found in the fact, evident from the whole of his article, that he is unacquainted with the methods of field geologists, and imagines that the paleontologist is the final arbiter in all questions of geological age. There was a time when this idea was current even among geologists; but at least on this side of the Atlantic it has for a number of years counted among the "*überwundene Standpunkte.*"

* It is well known to physicists that radiation is not in proportion to temperature. The "lime-light" radiates a far greater amount both of heat and of light than the simple oxy-hydrogen flame at a higher temperature.

Dr. Méyer commits a fundamental error of judgment in another matter, namely, in the assumption that after Lyell, Conrad and Tuomey had issued their (well grounded) dicta in regard to the succession of the Tertiary stages, those following them in the investigation of the subject calmly took these things for granted, and made their observations conform to "the masters' words." Here again, Dr. Meyer assumes a state of affairs which not long ago was widely prevalent in the old world, but has not, within my recollection, been a fault of American observers. On the contrary, young men have rather tended to distinguish themselves by making startling discoveries of mistakes in the work of their predecessors, and have left nothing unchallenged and unverified. Contrary to Dr. Meyer's expressed opinion of my method of work in Mississippi (see his paper in July number, page 65), I was even then sufficiently Americanized to subject every point of my predecessors' work and conclusions to the closest and most elaborate scrutiny, as he would have found out had he done me the honor to study my report. Hence I have no comment to make on his historical presentation of the growth of opinions, except that those opinions served me merely as convenient working hypotheses. But I differ from him more fundamentally in his sweeping statement (*Ibid.*), that "only a competent and careful examination of the fossils could indicate the relations of the Old-tertiary strata of Mississippi," and that I "studied this Tertiary paleontology very little." I had not time, it is true, for a thorough study of *all* the forms occurring in the several stages; and it is also true that under pressure of work, I "transferred the making of the *lists* of fossils to Prof. W. D. Moore." But every one of these fossils had been collected by myself personally, and in so doing I had acquired a very competent knowledge of the leading fossils of each of the stages; it had also convinced me of the fact that Conrad had made a large number of spurious species, and that the several stages are intimately interconnected by community of species from Claiborne to Vicksburg. This conviction I have repeatedly expressed in my Mississippi report, and it is emphasized by the list of fossils from the "Red Bluff" locality, which shows an obvious transition from the Jackson to the Vicksburg fauna. But it, as well as a great many other observations, also emphasized the extreme *localization* of certain fossils and groups of fossils; a circumstance easily accounted for by the shallowness of the depositing sea, evidenced not only in the materials and littoral fauna, but in the constant recurrence of brackish and lignitic facies where, stratigraphically, the continuation of marine beds was to be expected. This made me extremely cautious in relying on any single or few fossils for the identification of stages in distant localities; and similar caution would have prevented Dr. Meyer from coming to many false conjectures on insufficient premises.

It is difficult to characterize in terms altogether courteous Dr. Meyer's supposititious account of my mode of constructing my geological map of the formations of Mississippi (*Ibid.*); for he had before him, in print, the record of the localities through which the lines were drawn, and in stating that I made the acute southward curve of the Vicksburg belt around Jackson simply by way of getting out of a difficulty engendered by a hasty adoption of my predecessors' views, he states what the printed record shows to be false. This curve is necessitated by the occurrence of characteristic Vicksburg fossils in the following localities: Brownsville, Marshall's Quarry, Byram, German Berry's (then Monterey Post Office), Brandon; and no other delineation is possible. How is it that with these observations plainly in the text before him, and with the results of his own examinations at Jackson itself, it did not occur to him to walk down Pearl River nine miles, as I did, to see the Jackson strata sinking out of view, to be replaced, first by brackish and more or less lignitiferous deposits, which in their turn sink out of view and are capped by the Vicksburg rocks in their most characteristic development, in a magnificently fossiliferous outcrop two miles long? How does Dr. Meyer expect that in a level or merely rolling country, underlaid by strata having a dip not exceeding ten feet per mile, he will ever see stages separated by strata sixty or seventy feet in thickness exposed in any one outcrop? and in the present case, is any fossil whatsoever needed to establish the order of superposition?

There are at least two other lines of section across the Tertiary belt of Mississippi, in which the order is just as plainly established by stratigraphy alone, independently of fossils, as it is on Pearl River. One of these lines lies along the Yazoo Bluff, from Carrollton via Vicksburg down to Grand Gulf; the other, from Meridian via Enterprise and Quitman down to Winchester, on the Chickasawhay River. The former section has been completely explored between Professor E. A. Smith (then my assistant) and myself; the latter was in the first place traversed by myself, by land, and then, to make assurance doubly sure, and in order to observe details, the entire trip was made in a canoe by Professor George Little, later State Geologist of Georgia and now at the University of Mississippi. In all cases alike, *going down streams flowing to the southward, the strata successively sink below the water's edge as the observer progresses, in the order Claiborne, Jackson, Vicksburg, Grand Gulf,* as identified by their leading fossils. If then, there is any virtue in geometry, this is the inevitable order of succession from below upward.

But, outside of the State of Mississippi, I can satisfy Dr. Meyer's postulate of "seeing the Vicksburg rocks actually superimposed upon the Jackson strata." I have seen this in Louisiana, on the Bayou Funne Louis, where I have stood on a ledge of Vicksburg limestone showing a southward dip and containing abundance of *Orbitoides*, *Arca Mississippiensis* and *Pecten Poulsoni*, looking down some eighty feet, to northward, upon a level prairie country in which the bones of the Zeuglodon have been plowed up.

Without discussing paleontological details for which in the absence of adequate literature and collections I should now have to rely on memory alone, I must remark that I cannot attach much importance to *Plagiostoma dumosum* as a significant fossil. Of hundreds of localities examined by me in Mississippi, only two have yielded this shell; and both belong to a level intermediate between the Jackson and Vicksburg groups. But no Vicksburg locality has failed to furnish what I have been led to consider the decisive mark of the age, viz: *Arca Mississippiensis;* it is more constant than either the *Orbitoides* or *Pecten Poulsoni*, although the latter is rarely absent. For the Jackson age the most constant fossil is the *Zeuglodon*, bone fragments of which can nearly always be found by diligent search; and besides, an excellent and constant criterion is the presence of *Venericardia planicosta*, which has nowhere been found associated with the characteristic Vicksburg fauna. Through this widely diffused and universally recognized shell, as well as through the almost equally constant *Gastridium vetustum* and *Calyptrophorus velatus* as common fossils, the Jackson fauna connects strikingly with the Claiborne and Buhrstone beds; and I have found this *Venericardia* in the latter, in almost immediate contact with the Upper Cretaceous rocks of North Mississippi. Upon Dr. Meyer's assumption, the Vicksburg beds, void of both of the above types, would actually be intercalated between this oldest post-Cretaceous fauna and the Claiborne and Jackson beds. However, his assumption is abundantly and conclusively disproved by the most direct stratigraphical evidence; which it is to be hoped he himself will undertake to verify before he again ventures to re-classify the Southwestern Tertiary.

ART. XXXIV.—*Remarks on a paper of Dr. Otto Meyer on "Species in the Southern Old-Tertiary;"* by EUGENE A. SMITH.

IN this paper, one of the objects of the author is to show that the relative position of three of the subdivisions of the Tertiary formation, viz: Vicksburg, Jackson and Claiborne (given in descending order), is not the true one, but that it should be Claiborne, Jackson and Vicksburg, with Claiborne at the top and Vicksburg at the bottom.

This conclusion of Dr. Meyer cannot be allowed to pass without comment; for although no geologist who has ever been across the country where these rocks occur in Alabama, could for a moment be in doubt as to their relative position, yet those unacquainted with the facts in the case might be led by Dr. Meyer's paper to doubt the accuracy of the observations of Lyell, Tuomey, Winchell and others.

So far as I am able to make it out, Dr. Meyer's conclusion is based upon two observations, and upon a number of inferences derived from what he thinks should have been the course of evolution of several species of shells.

The observations are these: (1) Conrad found in one of the lower strata of the Claiborne bluff, below the ferruginous sands which bear the greater part of the well known Claiborne fossils, a specimen of *Spondylus dumosus;* and (2) Dr. Meyer himself, found a specimen of an *Orbitoid* in one of the lower strata at Claiborne. I shall say nothing of the possibility of these specimens having been washed down from a higher level, for it is to be supposed that these two observers were not mistaken as to the actual occurrence of the fossils in the rocks mentioned, nor will I refer to the fact that these two observations, so far as they prove anything, merely show that the two species named have a greater range geologically than has usually been assigned to them, for I know from personal observations near St. Stephens and elsewhere, that they occur in great numbers, *above* the Claiborne sands also, but shall confine myself to stratigraphy, since superposition is after all the only absolute test of relative age.

The term White limestone, has been applied in Alabama to a series of calcareous rocks over 200 feet in thickness. A portion of the upper part of this is a chalky rock quite soft and easily cut when freshly dug, and much used as material for building chimneys. This upper part also in places contains great numbers of *Orbitoides Mantelli.* The lower fifty or sixty feet are more clayey, give rise on disintegration to a limy soil which resembles closely the limy soil of the Rotten limestone of the Cretaceous. This lower part of the limestone contains also numbers of *Spondylus dumosus*, and vertebræ of *Zeuglodon cetoides.* These facts concerning the character and contents of the White limestone I give from personal observations. By many writers the upper part of this limestone has been considered equivalent to the Vicksburg, and the lower, to the Jackson divisions, but for our purposes it will be sufficient to define as above what we term the "White limestone."

Before offering any proof of the relative position of the White limestone and the Claiborne sands, it may be well to call attention to very important omissions of Dr. Meyer in his quotations of Sir Charles Lyell.

In Dr. Meyer's paper, page 61, we find the following remarks on an extract from a letter of Lyell published in this Journal, II, vol. i, pp. 313–315. "In this letter Lyell speaks on several subjects and says, of the Nummulitic limestone, that it had been referred to a pre-Tertiary age, but he considers it even younger than the Claibornean, 'for it occurs on higher places than the Claiborne bluff, a circumstance which, in a region where the stratification is horizontal, would imply a newer deposit, *even if the section near Suggsville, and Clarkesville*, SHOWING SUPERPOSITION *had been less satisfactory.* I did not meet with the limestone in question in the bluff at Claiborne which I have no doubt is owing to the fact that the calcareous strata are cut off at the top before they extend upward into the nummulitic beds.'"

Dr. Meyer omits all the words in italics, and thus carefully excludes the *proof of superposition* which Lyell brings forward; and then charges him with putting forth an hypothesis without proof. Moreover, in allusion to the diagram published by Lyell, in this Journal, II, vol. iv, pages 188–189, showing a section from Bettis' Hill in Clarke County to Claiborne, in which the White limestone is very distinctly represented as occurring at the top of the Claiborne bluff, Dr. Meyer insinuates that Lyell represented the dip, etc., not as he observed it, but as he needed it for his hypothesis. That Sir Charles, in saying that he did not observe at Claiborne the limestone in question, meant the *upper* or *nummulitic part*, is very evident from the figure referred to as well as from the words of Lyell quoted.* Indeed, Lyell's whole description of the relative positions of the various strata and their composition, in the region between the two rivers in the latitude of Claiborne, is remarkably accurate, as any one who is at all familiar with the country cannot fail to see.

Thus, when Dr. Meyer says Lyell has put forward his hy-

* This nummulitic part does, however, actually occur on the hill one mile south of Claiborne, as shown below in § 1.

pothesis of the newer age of the nummulitic limestone without any proof, he does so in contradiction to Lyell's direct and specific statement of what he had actually seen. (This Journal, 1847, pp. 188, 189, 190.) It is because the evidences of superposition are so numerous and unmistakable, that all geologists who have ever examined the country have never questioned it.

I have some sections to present which will remove all doubt, if only we can agree upon what we mean by the terms White limestone, and the Claiborne ferruginous sands. I have already said what I mean by the former. By the latter, I mean the stratum about 15 to 17 feet thick, occurring about midway of the Claiborne bluff (equal to *e* of Meyer's section) consisting of a reddish ferruginous sand full of the fossils described by Lea and Conrad as the Claiborne fossils. This sand is in its appearance and fossil contents unmistakable by any one who has ever seen it once.

The proofs which I wish to offer are of two kinds, viz: (*a*) direct superposition, and (*b*) the geographical position of the outcrop.

a. From direct superposition.

Immediately overlying the red ferruginous sands containing the Claiborne fossils—there are about three feet of ferruginous sands with hard ledges filled with *Scutella Lyelli.* This Scutella bed is indicated by *f* in Meyer's section at Claiborne, and it may be seen at all the localities cited below. Close above this Scutella bed is a White limestone—in places very argillaceous—sometimes sandy and glauconitic, which is demonstrably the lower part of the White limestone series as above limited. This is seen

1. At Claiborne.* Here there are to be seen between 50 and 60 feet of the clayey and glauconitic limestone (*h* and *i* of Meyer's section)—overlying the Claiborne ferruginous sands and the Scutella bed. On going from the plateau on which the town of Claiborne stands—up to Perdue Hill, the upper strata of the White limestone, containing *Orbitoides Mantelli*, may be seen in many places near the road, thus proving the correctness

*The lower part of the Claiborne bluff below the ferruginous sand, which is spoken of by Tuomey, Winchell and others as a limestone, has no resemblance whatever to the "White limestone." It is in fact rather a series of fossiliferous calcareous sands, and calcareous clays, than a limestone.

I can further assert from personal knowledge, that there is, below what I have just defined as the White limestone, nothing whatever in the whole series of Alabama Tertiary strata (at least from the Alabama river westward), which could with any propriety be called a limestone, unless an occasional bed of indurated shell marl, seldom if ever exceeding 15 to 20 feet in thickness, might be so designated.

Meyer's section on p. 69 which he says must supersede all previously made sections, describes *d* as "color a bluish gray" 26 feet, but whether the material is sand, clay, or limestone, he does not say.

of Lyell's statement that at the bluff the upper part of the limestone had been cut off by erosion.

2. At Gosport, a few miles below Claiborne, there is an exposure very similar to that at Claiborne Bluff, and the White limestone appears on the hills a short distance from the river just as it was when Sir Charles Lyell observed it many years ago, and at a higher level than the Claiborne sands—as Lyell said it was.

3. At Rattlesnake Bluff, a few miles below Gosport, we have the Claiborne ferruginous sands with their overlying Scutella bed—about 18 feet above the water level. Above the Scutella bed occur some 15 feet or more of the White limestone.

4. At the mouth of Cedar Creek, several miles farther down the river, the Scutella bed is only about three or four feet above the water level with a few feet of the White limestone over it.

In Meyer's section of Claiborne—the ferruginous fossiliferous sands are 64 feet above the river level; at Rattlesnake Bluff they are 18 feet above, and at the mouth of Cedar Creek, just below the water level. These observations establish the fact of a southerly dip of the strata.

Mr. Meyer questions the fact of such dip, but there is no doubt about it, as may be seen below in 5. C. S. Hale, in this Journal, II, vol. vi, No. 18, Nov., 1848, in an article on the Geology of South Alabama, not quoted by Meyer, likewise demonstrates the fact of the southerly dip of these strata. He also distinctly says that the White limestone overlies the Claiborne ferruginous sands, and brings forward observations of his own to prove it.

5. If we go down the river from the mouth of Cedar Creek, during a low stage of water, we can have the direct evidence of superposition, of the relative places of the Claiborne sands and the White limestone, for the few feet of this latter rock above the Scutella bed, mentioned as forming the upper part of the bank at the mouth of Cedar Creek, can be followed down the river till they sink below the water level, but before one of these beds sinks out of sight another appears above it, and the succession of these low bluffs (hardly ever more than 10 to 15 feet high) is so nearly continuous, that practically all of the strata intervening between the Scutella bed at Cedar Creek, and the Orbitoidal limestone at Marshall's Landing, may be seen. These strata are mostly argillaceous limestones, glauconitic in places. At Marshall's Landing, this argillaceous limestone is distinctly overlaid by 10 feet or more of limestone filled with *Orbitoides Mantelli.* There is no trace of faulting or disturbance along this part of the river, and the uniform dip of the strata and their succession are unmistakable.

6. The most positive and irrefutable evidence of the relative position of the White limestone and the Claiborne ferruginous sands is to be seen at the St. Stephens Bluff. There can I imagine be no reasonable doubt that this bluff is made up of the White limestone. In its upper part this limestone is in places a mass of *Orbitoides Mantelli*, and in the lower part of the bluff about 15 feet above the water level is a hard projecting ledge, a foot thick, beneath which is a layer consisting of the shells of *Spondylus dumosus.*

A short distance—about half a mile—above the St. Stephens landing, this bluff is interrupted by a ravine, on the other side of which it may be seen again with identical characters; Orbitoides limestone forming the upper part as at the landing below. At the base of this part of the bluff occurs a red ferruginous sand full of fossils—among which are *Melongena alveata*, *Crepidula lirata*, *Infundibulum trochiformis*, *Corbula Murchisoni*, *Turritella lineata*, *Cytherea æquorea*, *Oliva Alabamensis*, *Turbinolia Maclurei*, *Voluta Defranckii*, *Astarte sulcata*, and a number of other forms which are found at Claiborne. Just over this bed occurs the Scutella bed above referred to, and over that, *in direct contact*, the white clayey limestone passing upward into the Orbitoidal limestone. This ought to set the matter at rest forever, so far as the relative positions of the Claiborne sands and the White limestone are concerned. I shall not here attempt to show that the White limestone of Alabama is the representative of the Jackson and Vicksburg strata of Mississippi; but I can assert from personal observations that the lower part of the White limestone of Alabama with its Zeuglodon bones and its limy soils corresponds with what Hilgard marks as Jackson on his map, and that the upper part of our White limestone is what Hilgard marks as Vicksburg. In p. 227 of Hilgard's Geology of Mississippi it is stated that the Orbitoides limestone is directly overlaid by the sandstone of the Grand Gulf group. This it seems to me is evidence that the Orbitoides limestone (which Hilgard considers Vicksburg) is at the top of the series with which we are concerned.

In paragraph 222, p. 144 of Hilgard's work, there is a distinct statement that the Vicksburg strata overlie those of the Jackson. The same thing is shown in his diagram No. 32.

b. From geographical position.

The Tertiary formations of Alabama have a southerly dip (S. or S.W.) which will average some 25 to 30 feet to the mile. This dip has been ascertained by L. C. Johnson and myself at several places, and that it is very nearly the average may be seen from the correspondence between our river sections and the records of a boring made at Bladon Springs. These data are very soon to be published.

I might also add in further proof of southerly dip, that all the streams flowing in easterly or westerly directions in the Tertiary territory of Alabama, have steep escarpments on the southern banks, and gentle southerly slopes on the northern.

That with a general dip toward the south, the Tertiary strata will outcrop across the country in approximately parallel belts, the newer beds appearing farthest south, is plain enough. Now in fact we have, going southward, the outcrops of the Tertiary formations in the following order:

1st, Lignitic; 2d, Buhrstone; 3d, Claiborne; and 4th, White limestone. There is but one exception to this order known to us, and that is of very limited extent, and plainly due to a flexure which can be traced out. In the same way, the Cretaceous outcrops lie to the northward of those of the Tertiary.

It seems to me that this relative position geographically of the outcrops of the formations, taken together with the general direction of the dip, would, to say the least, go far to prove their relative age.

Indeed the stratigraphical evidence of the superposition of the White limestone above the Claiborne sands is so abundant, so conclusive, and so completely without contradiction from a solitary fact, that it has, so far as I know, never been called in question by any geologist familiar with the field of their occurrence.

Art. XXXV.—*Native Antimony and its Associations at Prince William, York County, New Brunswick;** by George F. Kunz.

The Brunswick antimony mine is situated in Prince William Parish, York County, New Brunswick, on the right bank of the St. John River, 24 miles from Fredrickton, and 96 miles from St. John, New Brunswick. The locality was discovered about 1860, and a few years later the Prince William, the Hubbard and the Lake George mines† were opened; all of these were ultimately absorbed by the Brunswick mine. At the present time this company controls a tract of land three miles by five, containing many veins of stibnite, and forming one of the largest antimony deposits known.

In the early working of the mines, native antimony was only rarely observed, and was not even mentioned in the reports

* Read at the meeting of the Amer. Assoc. Adv. Science, August, 1884.

† This locality is briefly mentioned by Dr. L. W. Bailey in a letter to Professor B. Silliman, see this Journal, xxxv, 150, 1863. Messrs. Hayes and Jackson also mention this locality in the Geology of New Brunswick by Dr. L. W. Bailey, 1865. See also Report of the Geology of New Brunswick, 1865, by Henry Youle Hind.

of Professor G. I. Chase and Wm. Petherick, M. E., in their prospectus of the Brunswick Mining Co.* It is mentioned however in the fifth edition of J. D. Dana's System of Mineralogy, page 18, as of rare occurrence at the Prince William mine. The mining then was all merely on the surface, but within the last two years it has been carried on to a considerable depth. At a depth of from 100 to 150 feet the native antimony is occasionally found in large pockets, some of which contain fully one ton of the pure mineral; it is associated with stibnite, valentinite and kermesite.

In its most common form the mineral is very compact, and at times finely granular, in appearance, resembling very closely the native antimony of South Ham, Canada; in this form it breaks at times with a slightly conchoidal fracture and has a decided steel-blue color. The forms in which it occurs are usually either rounded or elongated masses, from ten to twelve inches across, and weighing from 20 to 50 lbs.; it passes into a coarse granular variety where it very closely resembles the mineral from Sarawak, Borneo. The most remarkable form, however, is that where it occurs in radiated masses of crystalline plates. The single blades are from one to two and occasionally four inches in length and ⅛th of an inch across, being nearly as large as those of the artificial metal of commerce. The radiations seem to have massed around a common center, as if the mineral had cooled or crystallized slowly from without. One fine mass of this radiated tin-white metal measured over six inches across. Very small microscopic crystals about .05 mm. in size were developed by removing the calcite with acid, and mention was made of some large ones that had been observed, but these had been lost. Some of the crystalline cleavages have striations similar to those described by Laspeyres † that are also observed on the artificial antimony, and on the coarsely grained native variety from Allemont; these he refers to a polysynthetic twinning with $24R$ as the twinning-plane. The specific gravity of a pure piece of the coarsely crystalline bladed mineral was found to be 6·606 at 15°, and of the very compact, finely granular mineral 6·693. The following chemical results were obtained by Mr. James B. Mackintosh, E. M., School of Mines, New York City.

Coarse grained variety.

Gangue	0·84	per cent.
Iron	0·11	"
Arsenic	0·86	"

* Reports of Professor George I. Chase and Wm. Petherick, M. E., to the Brunswick Antimony Co., Boston. 1864.

† Zeitsch. deutsch. geol. Ges., 1875, p. 574.

Fine grained variety.

Gangue	5·04	per cent.
Iron	0·34	"
Arsenic	0·47	"

The difference in both cases is metallic antimony free from other admixture in large enough amount for detection, in the quantity used for the examination (1 gram.) In working the stibnite and accompanying ores on a large scale, to make the golden sulphuret (Sb_2S_5) gold and silver were observed in sufficient quantities to suggest the working of the ore for these minerals.

Stibnite occurs largely in the quartz veins in a massive form and in small diverging blades, and also in large masses with blades from four to six inches in length, and from one-quarter to one-half inch across. It occasionally occurs also in very small crystals in cavities, usually all of a very dark graphite color. In the seams between the layers of native antimony the valentinite occurs in massive and granular forms; also in beautiful radiations of bunched crystals, the radiations measuring over one and a half inches across. It occurs also at times in small hemispheres and in small isolated imperfect crystals not over 3 mm. in length.

Kermesite occurs in the cavities in stibnite and the antimony in small tufts of crystals, none of which were observed over one-half an inch in length; it occurs also in small hemispheres as a rule in the cavities in the native antimony. The color is from a dark cherry red to nearly black, and the fracture of the antimony is often streaked with this mineral. Dyscrasite, allemonite, senarmontite and native arsenic were looked for but not observed, although it is highly probable that some of these may yet be found.

The country rock is a black argillite, and the veins of quartz and calcite with stibnite which traverse it, vary from one to thirty feet; they are similar to the South Ham, Canada, deposits.* At the surface one of the principal veins had a dip of S. 80° E. with a depression of 55° N. Several other veins were N. 70° W. and S. 70° E. In following it from the points where the shafts were sunk in a westerly direction an increase of dip was observed.

I take pleasure in expressing my thanks to Mr. C. E. Parsons, of the Brunswick Antimony Co., for his kindness in furnishing information as well as specimens of the minerals for examination.

* Geology of Canada, 1863, p. 876. Report by Charles W. Willimott, Geol. Survey of Canada, 1880, 1881 and 1882, p. 3, G. G.

ART. XXXVI.—*The Crystalline Rocks of Alabama;* by C. H. HITCHCOCK, Hanover, N. H.

IN March and April, 1885, at the request of Professor E. A. Smith, State Geologist of Alabama, I studied the crystalline rocks of Eastern Alabama, and was thus enabled to compare them with the similar exposures in Virginia and New England. This region of exploration lies between the Coosa river and the east line of the State. It is the southern end of the long belt of crystalline rocks continuous from New Brunswick and seen farther east in Newfoundland. This area, as is well known, contains three groups of mountains, termed the northern, middle and southern sections. Viewed literally the western border is the most strongly marked, constituting the Green Mountains in New England, the Highlands in New York and New Jersey, and the Blue Ridge in Virginia. The more eastern portion rises to greater altitudes in the White Mountains of New Hampshire and the Black Mountains of North Carolina, while the depression between them is equally conspicuous and profound. The Alabama area lies wholly upon the southern slope of the last section, it being higher near the passage of the Georgia line by the Coosa river than about Auburn and Columbus. The country seems like an ancient table land worn to shallow depths by the tributary streams.

The geographical area to the west of this crystalline district is marked off definitely by that remarkable boundary, known as the Great Appalachian Lower Silurian Limestone valley, continuous from the Gulf of St. Lawrence to Eastern Tennessee and Northern Alabama. The contrasts on both sides of this boundary are well marked. On the east are the crystalline rocks, folded, inverted, faulted, traversed by igneous intrusions of the whole pre-Tertiary family of eruptives; on the west all the rocks are fragmental, posed in graceful folds or split by gigantic faults and are very rarely penetrated by unstratified rocks; on the east the rocks are principally Archæan; on the west they are Paleozoic; on the east the ranges are commonly short, with obtusely pointed summits; on the west the ranges are long barrows passing into plateaus. The eastern are the Alps and the western the Jura of America. It is this American Alpine section or the "Atlantic Primary Chain" of Featherstonhaugh which crowds the Coosa River in Alabama.

The geology of this region in Alabama may be best understood by a sketch of the order and position of the strata in a northwest-southeast section crossing them, say from Talladega to Columbus, Ga.

In Talladega the ubiquitous number II of Rogers — the

Lower Silurian limestone—is abundant. Near its eastern border are quarries of white marble not distinguishable in its appearance and general geological position from that of western Vermont. Not far removed from it is the familiar quartzite with its *Scolithus*—the same rock whether in Vermont, Pennsylvania, Virginia or Alabama, and it everywhere forms mountain ranges, as about Talladega and Anniston. As in Vermont, so in Alabama it requires close study to discover the true relations between this sandstone, the marble and the various limestones. The calcareous portion attains a greater thickness than in the north and it has been well divided by Professor Safford in the various members of the Knox group, which have not yet been identified *seriatim* (to my knowledge) north of Virginia.

The range of mountains immediately adjoining these limestones has the Ocoee group of Safford upon its western flank and summit, and it attains the highest elevation of any of the summits, and is to be compared with the foot hills of the Blue Ridge which sometimes surpass the main ridge in magnitude. The rocks are greenish and drab slates and schists, argillaceous, nacreous and hydromicaceous, together with layers of quartzite which in some localities attain great thicknesses. Grains of quartz presumably water-worn, are disseminated through many of the schists. All the strata dip southeasterly, more at first than afterwards, so as to give the aspect of an overturned anticlinal. Small veins of quartz, cavernous through decomposition and with bunches of chlorite, further characterize this group. I find from Safford's descriptions that the rocks of the typical area of this formation in Tennessee are similar. I have not seen much of this member in Virginia. Whatever of it is to be found there was placed by W. B. Rogers in his formation No. 1. But the exact analogue of this group is to be found in the magnesian slate of Emmons in western New England and eastern New York, whether it be Lower Silurian, Acadian or Huronian; a hundred typical specimens collected in Alabama could not be distinguished from a corresponding number obtained in the Taconic Mountains of western New England. This character is uniform in the three places where I observed it, near Anniston, Talladega and Syllacauga.

The next band of rock, usually just to the southeast of the crest of the mountains, is a feldspathic mica schist, or an imperfect gneiss. It resembles the inferior grades of the Green Mountain gneiss of Vermont. Near Ashland a few ledges remind one of the schists of Mt. Washington, N. H., but no more so than does the Green Mountain rock. There is not enough of this rock to enable us to call it Montalban.

Two bands of a newer schist are intercalated within this mica schist. The first is developed at Chandler's Springs as a heavy greenish chloritic schist: the second may be seen near Millersville or Hillabe and is a garnetiferous argillaceous schist: the first is like the familiar green rock east of the Green Mountains in Canada, Vermont and Massachusetts, holding beds of steatite and serpentine—the second is a genuine synclinal. Passing farther along we find next a broad belt of well defined gneiss between Pinckneyville and Alexander City. Part of it is reticulated by the segregated veins which are characteristic of the Lake Winnipiseogee gneiss of New Hampshire. Another portion is a genuine granite of oval shape about six miles in length. It is much like the Amoskeag quarry granite of Manchester, N. H., and its presence is marked by immense bowlders of decomposition, a novel sight to northern geologists. A few miles beyond Alexander City, at the crossing of the Tallapoosa River the rock is full of pyroxene, and farther along it is associated with gneiss standing vertically.

Midway between Alexander and Dadeville is a breadth of one or two miles of the augen gneiss. Between this and Auburn, gneiss of diversified aspect and considerable hornblende schist prevail. In the vicinity is a good locality of the mineral corundum. At Ragen's mill on Songahatchie creek near Loachapoka a massive gneiss is quarried for underpinning and piers, and is cut by a dike of gabbro similar to that in the White Mountains.

Southeast from Auburn are repetitions of the augen gneiss. Beyond them are the Chewackla quarries of limestone which are associated with itacolumite. These are very like the quartzites and limestone of Thomaston, Rockland and Camden in Maine, and probably those of Smithfield, R. I. A large business is done in the manufacture of quicklime at Chewackla and other localities near by. These northern rocks were referred to the Taconic system by Emmons. All of them, including similar deposits in the Carolinas near the eastern border of the crystallines, evidently originated in the same geological period, whether that be Cambrian or Silurian.

The remaining thirty miles of the section, from Opelika to Columbus were traversed along a railroad. Gneisses predominate with variable dips. Midway and toward the southeastern end the heavy massive gneiss near Auburn is repeated. At Columbus, the exposures are closely similar to the saccharoidal gneisses of Manchester, N. H., and to the thoroughly crystalline schists of typical Laurentian areas. Between Alexander City and Columbus, any geologist familiar with the Canadian or Adirondack gneisses will constantly recognize the peculiar features of that ancient fundamental gneiss. Our examinations terminated at Columbus, as we here reach the Atlantic Tertiary plain, at the head of navigation for large steamboats. A return trip northward near the State line enabled us to correct and confirm the impressions derived from the study of the section.

Certain obvious conclusions may be briefly summarized. First as to structure: (*a*) The contact between the Silurian limestones and the Ocoee group is not the natural one. There is no deep cut like that across the Blue Ridge at the James River to show the two groups in their undisturbed primal position. (*b*) The Ocoee slates dips are small, and when protracted upon a scale there seem to be an inverted anticlinal in the Syllacauga section. This position is due to the tangential motion of the elevating force and there is no reason to doubt Safford's assignment of these slates to the horizon suggested by the geographical position,—between the Potsdam and the ancient gneiss. (*c*) The first mica schist or gneissic band, as in so many sections farther north is an inverted anticlinal with many subordinate folds. As a careful study of this group at the north develops unexpected normal anticlinals, so it will probably be the case in the south. (*d*) Belts of chloritic and argillaceous schists, when conforming to the adjacent monoclinal gneisses or coarsely crystalline mica schists, are supposed to indicate compressed folded synclinals. Hence besides the western anticlinal between the Ocoee range and Ashland there must be at least one inverted synclinal of the chloritic schists if not others in the older group. (*e*) The second or argillaceous belt is synclinal in attitude and so are the underlying gneisses, between Ashland and Alexander City. It may be convenient to term the chloritic belt the Lower and the argillaceous belt the Upper Huronian. (*f*) Dadeville, in the midst of the indisputable Laurentians, is the center of a very large synclinal.

Second, as to mineral resemblances: (*a*) No one will hesitate to accept the identity of the limestones and quartzite in the edge of the great Appalachian valley with the corresponding rocks in Virginia and New England known as the Lower Silurian and Potsdam sandstone. (*b*) The Ocoee corresponds best with the later Huronian or the earlier Cambrian of the north. There is no good reason for calling it Acadian. (*c*) A section across northern New England has many points of resemblance to the one in Alabama described above. In northern Vermont after leaving the Potsdam there is a band of argillitic schists very like the Ocoee. This lies west of the Green Mountains and by Zadock Thompson in an unpublished map was once called the Magnesian slate of the Taconic system. The Green Mountain gneissic anticlinal follows with a repetition of the Ocoee-like slates and also the heavy chloritic group with serpentines referred to the Huronian by most authors.

Within this Huronian synclinal, between the Green Mountains and Connecticut River, lies a micaceous, argillaceous often calcareous group, which is the latest formation in that section of country. The east side of the Huronian synclinal is underlaid by better characterized gneisses than those upon the west side—and on reaching the watershed between the Connecticut and Merrimack Rivers in New Hampshire we find the augen-gneiss, which is regarded as the oldest rock in the north. Between this ridge and the ocean there are several synclinals filled with slates, quartzites or limestones of various descriptions. The order in Alabama is the same, save that there is a peculiar development of mica schist at several localities, particularly in the White Mountains, and called Montalban in my writings, which is not yet recognized. (*d*) Throughout the Atlantic crystalline area the oldest of the gneisses lies from thirty to fifty miles east of the Green Mountains and Blue Ridge range. (*e*) Having had the opportunity to examine a copper vein in Alabama, said to belong to the Ducktown type, I recognize the familiar mineral character of the copper regions of Ely, Strafford and Corinth in Vermont. The locality is the Woods Mine, Cleburne County, fully described in Professor Smith's report of progress for 1874. These veins occur quite near the junction of hornblendic schists with mica schists. The iron pyrites is the pyrrhotite. These veins are very different from those of Lyman and Milan, N. H., and those near Lennoxville, P. Q., which lack the pyrrhotite and are imbedded in the Huronian. The Ducktown and Vermont copper regions were of later origin than the others, and might be termed later Huronian, corresponding to the Montalban of Dr. T. Sterry Hunt.

Final scheme of classification.—Perhaps for the present it may not be needful to depart from the distinction laid down by Logan in 1855 for the crystalline series, that of Laurentian and Huronian. The beds commonly recognized as gneiss belong to the former, while the chloritic, hydromicaceous and argillaceous schists are chiefly Huronian. The latter are largely of sediment derived from the pre-existing gneisses and granites.

It has been necessary to speak of different sections of these two great groups, and the terms upper and lower have been employed. No term of geographical origin need be employed to define the places of the several divisions. The Laurentian of the east may be subdivided into lower, middle and upper or the augen gneiss at the base, followed by well characterized heavily bedded gneisses for the middle division, and coarse mica schists or imperfect gneisses for the upper part. The Lower Huronian would be the massive chloritic division, while the upper part is more varied, being argillaceous, hydromicaceous, nacreous and quartzose.

We may regard the primary division into Laurentian and Huronian as thoroughly established, and hence are warranted in using lithological distinctions to guide us in discriminating the age of crystalline rocks so far as they can be derived from the predominant features of these great systems. For a long time to come their further subdivisions will be open to question.

ART. XXXVII.—*The Geometrical Form of Volcanic Cones and the Elastic Limit of Lava;* by GEORGE F. BECKER.

General character of a volcanic cone.—If a fluid or partially fluid substance, such as lava, issues from an orifice in a plain and congeals about the mouth, the accumulation will form an elevated mass more or less nearly resembling a cone. If the resistance of this solid at the base is greater than the load, matter will or may continue to accumulate on the upper portion, while if the resistance towards the base is smaller than the load, a readjustment of material will necessarily take place. It is to be inferred, therefore, that under favorable circumstances volcanic cones will approximate to some definite form, and this inference is strengthened by the well known fact that the form of craters has been closely imitated by experiments on plastic material. Observation, too, shows that volcanic cones are characterized by a remarkable regularity of outline and that the outlines of the more regular cones of volcanoes wherever observed are strikingly similar.

Milne's results.—Professor John Milne has discussed the form of volcanic cones,* but his papers have not received the attention they deserve, containing, so far as I know, the first attempt to assign a definite geometrical form to a natural surface.† Though his papers are cited in some recent works on geology, I have seen no reference to this fact and it had entirely escaped my own attention until the material for a paper on the subject was completed. My results are closely

* Geol. Mag., vol. v, 1878, p. 338; and vol. vi, 1879, p. 506.

† Geology is one of the least exact of sciences, and opinions still differ among the ablest specialists as to its most fundamental doctrines; but, so far as geological relations are exactly known, they are necessarily capable of mathematical expression, and all relations are essentially exact, however imperfectly we may be acquainted with them. Per contra, the investigations of Sir William Thomson, Professor G. H. Darwin and other physicists show what grand additions may be made to geological science by the application of mathematical reasoning, and it appears to me that geologists should be on the watch for every opportunity to achieve for any geological relation, however insignificant it may appear, that final intellectual conquest which is symbolized by its correct expression in mathematical language. As in other branches of theoretical physics blunders will be made and progress will be slow, but opinion will gradually yield to certainty over a portion of the infinite field.

analogous to Professor Milne's but differ in some important particulars, and the data accumulated by him enable me to carry the subject a little further. Professor Milne regards the problem of a volcanic cone as that of the form which would be assumed by a mass of loose ash, and he is led to the conclusion that the form which would be assumed by such material would be that generated by the revolution of a logarithmic curve round its asymptote.* His experiments on sand, however, as he points out, gave him only right cones with straight sides, excepting when material of different sizes was employed and the action was such as to involve a sorting of the particles. I cannot think that the form generated by the revolution of a logarithmic curve about its asymptote is the figure of stability for a mass of loose material. Without any analysis it seems beyond question that were a long trumpet-shaped tube filled with dry sand, inverted on a horizontal plane and the tube withdrawn, the column of sand would collapse, and that a cone or some figure very similar to it would result. On the other hand it is a well known fact that the solid produced by the revolution of the logarithmic curve about its asymptote is the form which a loaded column of uniform strength must have when the material of the column is continuous and coherent like metal or stone. Such a column may be cut at any point and the load it will bear is the weight which the infinite column above this section would possess were the material uniform.† This, however, is not exactly the problem of the volcanic cone, which is an unloaded finite column, and neither an infinite one nor, what amounts to the same thing, a finite one supporting an extraneous weight.

Solid unloaded column of "least variable resistance."—A part of the ejecta of a volcano is melted lava and congeals to rock. A large part is also in the form of sand or "volcanic ash," but of this much is indurated to a tolerably firm material after ejection by one cause or another and, although weathering and frost will often cover the slopes of a mountain such as Fusiyama with detritus, it appears to me that volcanic cones must be regarded as essentially continuous masses.

Suppose a columnar mass of uniform coherent material, the surface of which is generated by the revolution of some curve about a vertical axis. Let the height of this column be a, its radius y, the distance of any horizontal plane from the base x, the specific gravity of the material ρ, and the coefficient of resistance to crushing stress at the elastic limit $\varkappa$. If such a

* Professor Milne does not give his conclusion in this form, but states that the sum of equally spaced ordinates will be to their difference in a constant ratio. which is equivalent to the statement in the text.

† The volume of this solid of revolution is of course finite in spite of the fact that its length is infinite.

column is in a condition of stable internal equilibrium, the total resistance of any section to crushing stress must at least equal the pressure of the load resting upon the same section, so that if p is the pressure of a load which would strain a given section to its elastic limit, or the possible load, and if l is the actual load, $p \geqq l$ and $\frac{dp}{dl} \geqq 1$. The square of this differential coefficient, if not unity, must also exceed it, so that

$$\frac{dp^2}{dl^2} = 1 + \mathrm{F},$$

where F denotes some positive quantity the limiting value of which is zero. It follows from the definitions that

$$p = \pi \varkappa y^2; \qquad l = \pi \rho \int_x^a y^2 dx;$$

and therefore

$$\frac{dp^2}{dl^2} = \left(\frac{2\varkappa}{\rho}\right)^2 \frac{y'^2}{y^2}$$

the accent as usual denoting the differential coefficient taken with regard to x. The volume of the solid of revolution above the section at x is therefore

$$\pi \int_x^a y^2 dx = \pi \int_x^a \left(\frac{2\varkappa}{\rho}\right)^2 y'^2 dx - \pi \int_x^a \mathrm{F} y^2 dx.$$

For the limiting value $\mathrm{F}=0$, or when the load everywhere exactly equals the carrying power, these equations give for the generating curve

$$y = \mathrm{A}\, \varepsilon^{-x\rho/2\kappa},$$

a well known formula for this condition. Since this logarithmic curve meets the axis only at infinity, the condition $l=p$ answers only to an infinite value of a.

In the equation for the volume, either term may be regarded as varying independently. If the term containing y' is supposed constant, the volume above any and every section will be a maximum, for stated values of the constants, when the term containing F is a minimum; and this must be true even when a is not given the special value ∞. Consequently when this term is a minimum, each section of the column will be loaded as nearly to the elastic limit as the conditions will permit and the entire column will contain less material than any other stable column of the same height, or, for a given amount of material, will be the highest stable column which can be generated by the revolution of a continuous curve, or which can be formed under the action of a uniform law. This postulate of continuity of course excludes from the possible finite forms the truncated logarithmic column, with or without an imposed load. It appears to me that a volcanic cone, formed as indicated at the beginning of this paper must fall under this problem.

For the sake of brevity, I propose to call the solution of this problem the form of *least variable resistance*, a term intended to include as a special case the form of uniform resistance or the infinite logarithmic column. The form of least variable resistance then is a solution of the equation

$$\int_x^a \left(y^2-\left(\frac{2\kappa}{\rho}\right)^2 y'^2\right)dx=\text{min.},$$

or by the method of variations

$$\delta\int_x^a \left(y^2-\left(\frac{2\kappa}{\rho}\right)^2 y'^2\right)dx=0,$$

giving

$$y=\left(\frac{2\kappa}{\rho}\right)^2 y'',$$

or

$$y=\text{A}\varepsilon^{-x\rho/2\kappa}+\text{B}\varepsilon^{x\rho/2\kappa},$$

where A and B are arbitrary constants.

If the origin of the curve is now taken at the center of the base of the solid of revolution, and if for convenience $\frac{2\kappa}{\rho}$ is made equal to c, y must disappear for $x=a$ and therefore

$$\text{B}=-\text{A}\varepsilon^{-2a/c};$$

$$y=\text{A}(\varepsilon^{-x/c}-\varepsilon^{-2a/c}\varepsilon^{x/c}).$$

When $a=\infty$, $\varepsilon^{-2a/c}=0$ and the equation reduces to the simple, well-known logarithmic form of uniform resistance. If the origin is taken at the summit of the cone, and both x and y are multiplied by c, the equation may be written

$$y=b(\varepsilon^{-x}-\varepsilon^{x}).$$

Here b must be determined to correspond to the equation of condition which becomes

$$\int_{-x}^{0}(y^2-y'^2)\,dx=4b^2x=\text{min.}$$

The more b exceeds b^2 therefore, the smaller will be $4b^2x$. Now it is well known property of $\frac{1}{2}$ that it exceeds its square more than any other number, and consequently $b=\frac{1}{2}$. Introducing this value and restoring c the equation becomes

$$\frac{y}{c}=\frac{\varepsilon^{-x/c}-\varepsilon^{x/c}}{2}$$

and this is the equation of the curve which by its revolution about the x-axis will generate the finite unloaded column of "least variable resistance."*

This is the same curve barring the value of constants which I have shown twice elsewhere,† characterizes step faults. It might have been deduced directly from the discussion presented in the latter paper on the distribution of energy in compressible masses under the action of a constant force, or that discussion might have been evolved from this. The arrangement of sheets of rock in a complex fault, the distribution of pressure in the atmospheric column, the form assumed by a cold rivet, and the shape of a volcanic cone, as well as some other important cases, and possibly some vegetable forms‡ are mere variations of a single problem and find their solution in different readings of the equation

$$w = A\varepsilon^{-x/c} + B\varepsilon^{x/c}$$

where w is the energy potentialized per unit volume.

The quantity $\frac{2\kappa}{\rho}$ is the natural unit of the volcanic curve corresponding to the constant sub-tangent of the infinite form and to the constant of the catenary curve. It is of course constant for any given homogeneous material and different for different materials. Consequently solids of different materials

* Whether this proposition is new or not my reading is insufficient to determine. I can only say that I have looked for it in vain in a number of treatises in which it might have been expected to be mentioned if known.

† Geology of the Comstock Lode, chap. iv; Impact Friction and Faulting, this Journal, vol. xxx, p. 116.

‡ The case of a loaded column of uniform strength seems unlikely to be met with in inorganic nature for it appears to imply an adjustment of the column after the imposition of the load. I strongly suspect however that the simple logarithmic column is the form to which tree trunks tend in forests where the influence of winds is but little felt. Where such trees reach a large size and especially where the wood is soft, the increase in diameter near the ground is very marked. Thus in the red-wood forests of California the largest trees are generally cut some ten or more feet above the ground to save the inconvenience of handling a trumpet shaped log. This increase of diameter is less marked in trees of moderate size than in very large ones and less among hard wood trees than in species the wood of which is soft. Forest trees of course seek the light, and one can scarcely doubt that they reach it as rapidly as it is possible to do so consistently with stability. If so the load at any section below the branches per unit of area of this section will be a maximum and will be the same at all sections, and if this is true the form is the simple logarithmic column. For if F is the area of the section or πy^2, and if F_0 is the value of F for the datum plane, the equation may also be written

$$F = F_0\varepsilon^{-x\rho/\kappa}.$$

This would lead to a simple means of testing the question under favorable circumstances. If one were to cut a well developed forest tree just below the branches and divide the trunk into two or more portions, weigh the branches and each log, and measure each cross-section, it could of course be determined in a moment whether the load per square inch of all sections were uniform or not.

corresponding to this equation will differ only in scale or will be geometrically similar. The value of $\frac{2\varkappa}{\rho}$ can easily be expressed in terms of x and y. It will be found that

$$\frac{4\varkappa^2}{\rho^2}y'^2 - y^2 = \frac{4\varkappa^2}{\rho^2}$$

which gives

$$\frac{2\varkappa}{\rho} = \frac{y}{(\tan^2 \vartheta - 1)^{\frac{1}{2}}}$$

ϑ being the angle which the tangent at any point makes with the x axis.*

In the figure of least variable resistance the radius becomes zero when $\vartheta = 45°$. Below the point of the cone ϑ increases with the radius. In comparing the theoretical form with actual occurrences this angle is especially significant.

Comparison with actual cases.—Whether or not the figure of least variable resistance is that of a volcanic cone, can, I take it to be, determined only by comparison in spite of the apparently good reasons which have been stated for such a supposition. The first step for such a comparison is the reduction of each drawing or of corresponding numerical data to the same unit, which can be done by help of the formula for $\frac{2\varkappa}{\rho}$ just given. Professor Milne gives diagrams and tabulated measurements of Fusiyama and Kumagatake which I have attempted to reduce in this way. The diagrams were taken from selected photographs and are probably slightly but certainly not greatly distorted. Their actual scale is unfortunately not given. For the left side of Fusiyama Professor Milne gives in centimeters the position of eleven points on the section referred to the axis of the volcano. Of these I rejected the two uppermost for reasons to be mentioned presently, and calculated $\frac{2\varkappa}{\rho}$ for each of the others, assuming that the cord connecting any two points was parallel to the tangent at a point half way between them. By reference to Professor Milne's diagram it will be seen that his points are so close that

* For the infinite logarithmic column on the other hand

$$\frac{2\kappa}{\rho} = \frac{y}{\tan\vartheta}$$

while for the catenary

$$y = \frac{c}{2}\left(\varepsilon^{-x/c} + \varepsilon^{x/c}\right)$$

$$c = \frac{y}{(\tan^2 \vartheta + 1)^{\frac{1}{2}}} = y\cos\vartheta.$$

no sensible error is involved in this assumption. The average value of $\frac{2\kappa}{\rho}$ obtained was 2·86 centimeters. To reduce the diagram to any desired unit, *c*, it is only necessary to redraw it to a scale in which $\frac{c}{2 \cdot 86}$ is treated as unity. The value of *c* which I happened to find convenient was 4^{cm}. I drew both sides of Fusiyama to the same scale to which I had plotted the equation of the solid of maximum stability and give the plots below. I treated Kumagatake in the same way, getting the value of the natural unit in Professor Milne's diagram as $2 \cdot 61^{cm}$.

In fig. 1 may be seen the theoretical locus and Professor Milne's outlines of these cones reduced as described. They are drawn to the same axial line but to different bases, so that for purposes of exact comparison a tracing of one should be made and shifted vertically until it more or less nearly coincides with the other. If this is done, a similarity will be revealed between the results of theory and the facts which seems to admit of but one explanation.

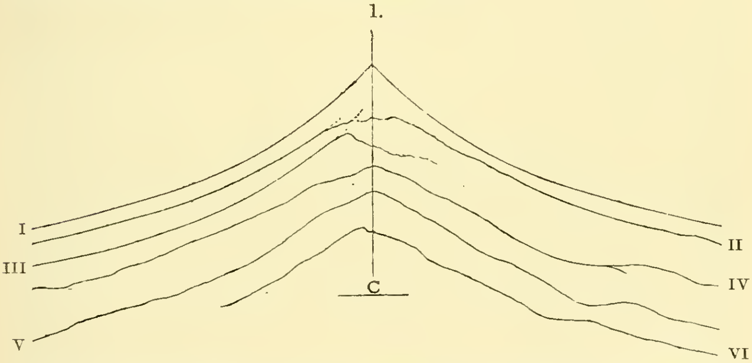


FIGURE 1.—Theoretical curve and outlines of actual volcanoes. The comparison is to be made by vertical transposition. I, is $y=\frac{\varepsilon^{-x}-\varepsilon^{x}}{2}$; II, Milne's outline of Fusiyama; III, Milne's outline of Kumagatake; IV, Shasta, enlarged from Taber's stereoscopic view from north side, No. 1542; V, Hood from the Dalles, enlarged from Watkins' New Boudoir Series, D. 61; VI, Popocatapet'l enlarged from A. Briguet's photos of Aqueduct of Tomacoco Mill, No. 19; *c*, the unit or $\frac{2\kappa}{\rho}$.

I may mention that the slopes as given by Professor Milne are reproduced as accurately as it is practicable to draw them to an altered scale, and that the natural units obtained as stated were used without any correction or adjustment. That for Fusiyama might be changed a trifle, for, according to my computations, Professor Milne has not taken the axis quite at the center. On recomputing it however for the slight discrepancy mentioned, I find that the difference in the cross section of the mountain would be imperceptible on the scale of the figure, and I therefore prefer to present it exactly as it results from Professor Milne's measurements. A small part of the lower portions of the left sides of the mountains as given by Professor Milne are omitted because they could not be included without unduly reducing the scale of the cut. These portions correspond as well as the remainder with the theoretical form. Professor Milne gives no more of the right side of Kumagatake than the cut shows.

In computing the value of the natural unit I rejected the two measurements nearest the peak because the summit is not only most subject to erosion when not snow-capped but should not theoretically coincide with the figure of least variable resistance; for such a coincidence would imply an infinitesimal crater while the existence of an actual crater implies the presence of less matter, or a smaller load, close to the summit and consequently a more rapid convergence of the sides. Strictly speaking, the presence of a crater would affect the whole figure, but the influence of this diminution of the load will manifestly be extremely slight excepting near the crater itself whenever the crater is small compared with the volume of the cone.

For the sake of comparison with these very perfect examples I have introduced outlines of Mt. Shasta, Mt. Hood and Popocatapet'l carefully reduced to appropriate scales from photographs. These mountains are all rather irregular but will serve at least to show a striking similarity in the curvature of volcanic cones, and a pretty close agreement with the theoretical form. This likeness can best be judged of by making a tracing of the theoretical cone and placing it upon the outlines of the mountains. Professor George Davidson has kindly lent me a sketch of Mt. Renier, which he made for the special purpose of recording its slopes. Long practice in this kind of work makes him confident that this sketch is correct as to angles to something like one degree. The sketch coincides most remarkably with the theoretical form but is not added to the diagram because its evidence is scarcely comparable with that obtained mechanically by photography. In the case of a very large and deep crater it might be interesting to compare the form of greatest stability with that of the wall of the crater. If R were the radius of the outer surface of this wall at any level and r the radius of the inner surface the area of the ring would be $\pi(R^2-r^2)$ and $(R^2-r^2)^{1/2}$ would have the same value as y in the equation given for a pointed cone.

Professor Milne states that the highest slope he has observed upon a volcano is 40° upon Kumagatake, while he notes in pictorial representations of volcanic cones angles often exceeding 50° and one reaching 69°. He is inclined to think that artistic feeling may have induced exaggerations in these cases. Other geologists have of course also called attention to such misrepresentations. If my theory of the form of volcanic cones is correct these objections are well founded, since 45° is reached only at the (impossible) point of a solid cone. If the thin walls of large craters, however, are sufficiently solid to take a form of least variable resistance higher angles than 45° will occur.

The elastic limit of the average lava of cones.—Besides its geometrical importance in the equation of the volcanic cone the quantity $\frac{2\varkappa}{\rho}$ possesses a further property of at least equal interest. The coefficient of resistance at the elastic limit of the material of the cone is

$$\varkappa = \frac{\rho}{2}c.$$

Now $\varkappa$ is a constant which it is peculiarly difficult to determine experimentally for any material, while it is one of prime importance in the grand question of geology, upheaval and subsidence. The value of c, however, can be immediately derived from observations on volcanic cones or from drawings to scale or from photographs of which the scale and the angle of the plane of projection to the vertical are known, while ρ is determinable for uniform material with the utmost accuracy and ease and a close approximation to its average value could doubtless be obtained by a considerable number of experiments for the materials of almost any volcanic cone. The value of ρ is capable of being further checked by the results of pendulum observations. The form of the more regular class of volcanic cones will therefore enable geologists to determine the modulus of resistance for the elastic limit on an enormous scale for an extremely important class of the constituents of the earth's "crust," and if the scale should prove not to compensate for the uncertainty as to the value of the density, the method cannot fail to afford a valuable check on those obtained from laboratory experiments.

Mr. Mark B. Kerr of the U. S. Geological Survey has kindly furnished me with a surveyed section of the "Sugar Loaf," Siskiyou County, California. It is shown in fig. 2 with the theoretical curve on a larger scale than that employed in fig. 1. The agreement is very good and the scale being known gives

$$c = \frac{2\varkappa}{\rho} = 2560 \text{ feet} = 780{\cdot}26 \text{ meters.}$$

The height of a uniform column of this material which will strain its lower surface to the elastic limit is

$$\frac{\kappa}{\rho} = 1280 \text{ feet} = 390 \text{ meters.}$$

If its specific gravity is 3, the load it will bear at the limit of elasticity per square centimeter is 117 kilos. This is a very reasonable result, for I find in a table compiled from a number of trustworthy sources the following values for the pressure at the breaking point (which is of course greater than that at the elastic limit). Good brick, 100 kilos. pr. sq. cm.; sandstone, 200; limestone, 300; granite, 600. It is probable that the mean specific gravity of the cone is below 3 and its carrying power not much above that of good brick. A good suite of specimens would be necessary to give this determination much value because of the uncertainty as to the density of the material, but it at least exhibits the method.*

2.

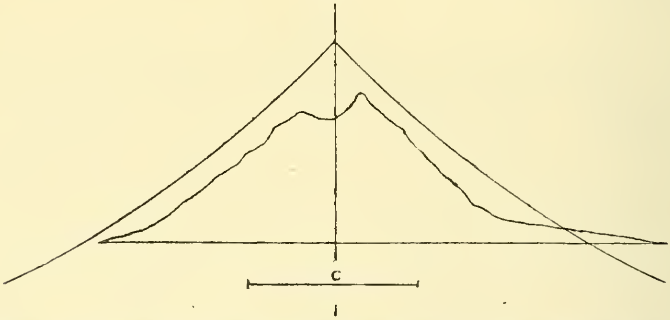


FIGURE 2.—Theoretical curve and surveyed section of Sugar Loaf, Siskiyou County, California. The comparison is to be made by vertical transposition. Elevation of base, 4000 feet; of summit, 6399 feet. $c = \frac{2\kappa}{\rho}$.

Lunar volcanoes.—If ρ is given the form mg, where m is the mass of the unit volume and g the acceleration of gravitation, c or $\frac{2\kappa}{mg}$ is inversely proportional to g. Hence, if the attraction of gravitation at the earth's surface were to diminish c would

* Since this paper was completed I have had access to the maps of Mt. Shasta, recently made by the U. S. Geological Survey. For the sake of testing the theory set forth above four vertical sections through the summit were prepared by the topographical assistants on lines running north and south, east and west, northwest and southeast, northeast and southwest. All of these profiles showed a very satisfactory general agreement with the theoretical form and yield a value for κ/ρ of about 1320 feet or within 40 feet of that found for Sugar Loaf, so that considering the roughness of the method the two results are to be regarded as substantially identical. The main mass of Shasta appears to consist of andesites, somewhat "trachytic" in texture.

increase and, *ceteris paribus*, loftier volcanic cones would result. If, therefore, the material of the volcanic cones on the moon closely resembles that of those on the earth, the enormous height of lunar volcanoes is in part ascribable to the feebleness of the attraction which the moon exerts upon bodies at its surface. On the other hand, studies of the form and dimensions of lunar volcanoes would lead to values of $\frac{\varkappa}{\rho}$, from which it might be determined approximately whether the height of a column of lunar lava which would strain its lower surface to the elastic limit does or does not correspond to that of columns of terrestrial lavas; a step of some interest in lunar physical geology, and one which might even lead to a guess as to the lithological character of the lunar rock, since different lavas probably have different characteristic values of $\frac{\varkappa}{\rho}$.

Office U. S. Geol. Survey, San Francisco, Feb., 1885.

ART. XXXVIII.—*Notice of a new genus of Pteropods from the Saint John Group* (*Cambrian*); by GEO. F. MATTHEW.

IN studying the organic remains of the St. John Group the writer has met with a new genus of Pteropods which is of interest as showing the relation of the ancient genus *Hyolithes* and its allies to the Cephalopods.

Eichwald's genus *Hyolithes* was based on a species which is camerated near the apex; but the new genus not only has this chambered area near the apex, but is also divided by diaphragms on one side of the shell, nearly to the aperture, somewhat in the manner of *Phragmotheca* of Barrande. It may be described in the following terms:—

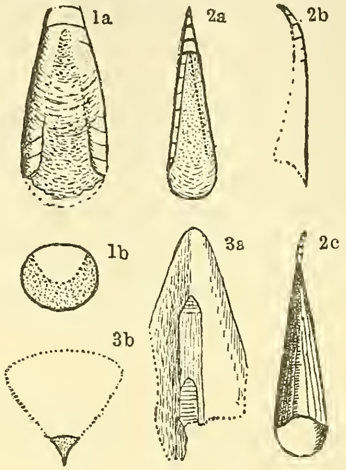
DIPLOTHECA n. gen.

Slender oval cones somewhat triangular in section with abbreviated or attenuated apices. In the narrower part of the tube or cone there are several septa that divide off segments of the tube from the body cavity (chamber of habitation). The body cavity is separated from one side of the outer shell by a thin partition supported by delicate transverse septa or diaphragms. The apex in one species is prolonged into a narrow attenuated flexible tubule with transverse annulations (diaphragms ?) at regular intervals.

This genus differs from *Camerotheca** (another group of *Hyalithoid* shells associated with it in the same measures) in the more rapid enlargement of the shell during growth (which

* Described in the Canadian Record (Canadian Naturalist), 1885.

thus formed a shorter cone than *Camerotheca*), and in having a firmer and, as preserved in the shales, rounder side, where it has the support of the lateral diaphragms. This feature of the endo-skeleton is most distinct in two species, varieties of which are found in the sandstones near the base of Div. 1 of the St.



John Group. The shells of these pteropods are here found scattered over the surface of the sandy layers of an old sea bottom, close to the shore line; and occur mingled with the material forming the casts of worm burrows and imbedded in phosphatic nodules. They thus occupied locations where in later formations lamellibranchs might be looked for: but the remains of numerous individuals of the species of this genus are also found in fine shale at a higher horizon, showing that the genus inhabited deeper waters as well.

Further particulars of these species will be found in an article read before the Royal Society of Canada, 1885.

Reference to the figures:

1a. *Diplotheca acadica* Hartt, sp. var. *crassa*, dorsal view, showing the diaphragms of the endoskeleton. The diaphragms near the aperture and the septa near the apex are exposed by the abrasion of the shell.

1b. Same, transverse section, showing the body cavity partly enveloped by the phragmated part of the tube (represented by the shaded area).

2a. *Diplotheca Hyattiana*, $\frac{4}{1}$, vertical section, showing the septa at the apex of the tube, and the diaphragms at the side.

2b. Same, section of a flattened shell, from front to back, showing the curved apex.

2c. *Diplotheca Hyattiana* var. *caudata*, $\frac{3}{1}$, showing the annulated flexible tubule attached to the apex.

3a. *Phragmotheca Bohemica* Barr., showing along the axial line the place where the phragmated sheath is found and the closeness of its diaphragms—figured for comparison.

3b. Same, transverse section of the shell, the shaded portion marks the narrow phragmated sheath.

St. John, N. B., July, 1885.

ART. XXXIX.—*Cope's Tertiary Vertebrata;** by J. L. WORTMAN.

THE exceptional facilities which this country affords for researches into the history of its extinct vertebrate inhabitants have been well known ever since the rich fossiliferous deposits of the Rocky Mountain region were first brought to the attention of students of geology.

The unusual qualifications necessary for a successful prosecution of investigations in this branch, as well as a general lack of the requisite facilities in the way of large osteological collections in this country, have no doubt prevented many from entering this field of study.

The expense likewise attendant upon the collection, preparation, and illustration of material is so considerable that only those who have a comparatively large amount of means at their disposal can make much headway in it. While these causes have necessarily limited the number of investigators to an extent by no means commensurate with the material to be investigated, activity has nevertheless been proportionately great since Leidy, Cope and Marsh began making collections of fossil remains in the West. Later Scott and Osborne of Princeton have been more or less actively engaged in the same pursuit.

As a result of the study of these collections great contributions to our knowledge of the extinct vertebrate fauna have been made. Those of Leidy were the first, and their superior excellence must always remain a monument to his scholarly attainments.

Contributions by Cope and Marsh have followed from time to time, some of which have been complete and handsomely illustrated; their value is of high order. Up to within a comparatively short time however, the greater part of their immense collections, especially those from the Tertiary horizons, have been made known only through preliminary descriptions and stray papers in which little else was attempted than a brief and hasty description of the most salient characters of the new species and genera discovered.

Within the present year two notable quarto volumes have been issued by the U. S. Geological Survey, forming the most considerable contributions to the subject which have yet appeared in this country. One of these is by Professor E. D. Cope of Philadelphia, and is devoted to the vertebrate fauna of the older Tertiary deposits of Western America. It comprises somewhat over one thousand pages of text, which is illustrated

* U. S. Geolog. Surv. Territories. The Vertebrata of the Tertiary formation of the West. Book I. By Edw. D. Cope. Washington, 1883–1884.

by one hundred and thirty-three beautiful lithographic plates. As it covers but little more than one-half of the entire Tertiary fauna, it is soon to be followed by another volume of equal dimensions which will be devoted to the later horizons.

To students of Mammalogy, this work will prove of unusual interest on account of the flood of light which it throws upon the origin and relationship of many groups which have hitherto proven puzzles to the best zoologists. One of the leading questions in the study of the Mammalia is their origin and succession. It is well known that they make their first appearance in rocks of Triassic age; that they continue to be represented by a few small marsupial-like creatures up to the beginning of the Cretaceous period, where, with a single exception,* the record is lost until the Eocene is reached. It is likewise well-known that the fauna of this period is comparatively highly specialized and comes into existence, so far as we now know, without announcement in the preceding formation.

Previous to the discoveries of Professor Cope, the Wasatch was the oldest Eocene deposit of which we had any knowledge. These beds contain the remains of an extensive fauna, a large proportion of which is composed of Perissodactyle ungulates. They likewise contain Rodents, Lemurs, Creodonts, Taxeopods, etc.

In the present volume we have brought to our attention an Eocene fauna which antedates that of the Wasatch, viz: that of the Puerco. Here, so far, no Perissodactyles have been discovered; neither have Rodents as yet been found, although it appears to be quite rich both in species and individuals. Since these groups have always been hitherto regarded as a constant feature of any early Eocene fauna, this is somewhat remarkable.

The Perissodactyles appear to be replaced by a most extraordinary group to which an ordinal rank has been assigned by the author under the name *Taxeopoda.* The Rodents are represented by the *Tæniodonta*, an assemblage of extinct forms with large scalpriform incisors in both jaws, while Lemurs, Creodonts and primitive allies of the Coryphodonts go to make up the list of the mammals.

Altogether, the *facies* of this fauna is much more primitive than that of any other group of Eocene Mammalia so far known, and its discovery may be justly regarded as the most important that has been made in this subject within the past decade. To those who await with confidence the discovery of the connecting links between the mammals of the Jurassic and

* Messrs. Wortman and Hill discovered the remains of a small Stereognath marsupial in the Laramie deposits of Dakota in the summer of 1883, associated with the bones of huge Dinosaurs. To this species Cope gave the name *Meniscoëssus.*

the rich fauna of the Eocene as heretofore understood, the discovery of the Puerco constitutes a bold advance.

As regards the important additions to our knowledge of special groups, which this work contains, they are so numerous that it is impossible to mention more than a few of the leading ones in this connection. We have here presented for the first time any thing like a broad and comprehensive generalization of the relationships of the hoofed Mammalia. Assuming as a basis of understanding the fact that the limb structure has been all important to these animals in the struggle to survive, Professor Cope has divided them into four groups according to the degree of modification of the carpal and tarsal articulations.

He has shown that in all primitive ungulates the carpal and tarsal bones are serially arranged; that is to say, those of the proximal row are directly superimposed upon the corresponding elements of the distal set, a condition whose mechanical advantage in sustaining the weight of a bulky body in rapid movement, is much inferior to that of the higher types in which the carpal and tarsal bones interlock.

The four orders which he thus constructs are the *Taxeopoda*, *Amblypoda*, *Proboscidea* and *Diplarthra*. The first of these is the most primitive, being pentedactyle, probably semi-plantigrade, bunodont, and in many ways approaching the clawed orders. It attained its greatest development in the Puerco epoch, where it is represented by numerous species and genera. According to the views of Professor Cope, which seem to be unusually well-founded, this group forms the central stem from which the others have been derived, having as a cotemporary in the Puerco horizon the *Taligrada*, a sub-order, which establishes a close connection between it and the *Pantodonta* of the later Eocene.

It is here also that the Hyrax, that anomalous nondescript, for the first time finds fellowship, being at the same time the only living representative of this remarkable order. It appears to be an oversight on the part of Professor Cope that he did not detect the ancestral connections of the tree Hyrax with his *Meniscotheriidæ*, a family of the *Taxeopoda*. It is likewise somewhat questionable whether he is correct in giving the *Toxodontia* a position in this order.

With reference to the immediate connections between the *Diplarthra*, a group corresponding to the *Ungulata* of most authors, and the Taxeopoda, comparatively little has been made out; neither do we receive any additional light upon the direct ancestry of the Proboscidians in the present contribution. The internal arrangement of the *Taxeopoda* is thoroughly considered and many genera and species described. The characters of the order are established principally upon unusually perfect skeletons of *Phenacodus primævus* and *P. Wortmani*, which were obtained by one of his collectors in the valley of the Big Horn in Wyoming Territory.

A fact of no small significance is ascertained with regard to the superior molar teeth of some of the Puerco representatives. They are shown to be tritubercular and are therefore the simplest pattern which is known to exist in any ungulate. From this the evolution of the teeth of all the *Ungulata* from a simple type appears to be demonstrated.

In the Perissodactyle division of the *Diplarthra* much is added to our knowledge. The suborder is for the first time divided into families whose exact limits are defined and the genera systematically arranged. The osteology of the four-toed equine representative, *Hyracotherium*, is thoroughly described, and an almost complete skeleton figured. The osteology of the Lophiodont genera, *Triplopus* and *Hyrachyus*, are also described from exceptionally perfect skeletons. As already indicated, the discovery of the *Taligrada*, a new suborder of the *Amblypoda*, constitutes the principal advance in our information respecting this division, nevertheless numerous genera and species related to the Coryphodonts as well as one extremely interesting genus *Bathyopsis*, near to *Loxolophodon*, are also described and figured.

Another generalization of scarcely less importance is that which relates to the arrangement of the clawed Mammalia. From the previous publications of Marsh and Cope we have been made acquainted with the remains of several extinct groups which display characters intermediate between those of orders now living. These are the *Tillodontia* of Marsh, which has both Insectivorous and Rodent affinities, the *Tæniodonta* of Cope, which seems to connect the Tillodonts with existing Edentates and the *Creodonta* of Cope, which apparently blend the modern *Carnivora* with the ancient *Insectivora*. These, together with all existing Insectivores as well as the Lemurs, are grouped into a single order under the name *Bunotheria* and their relationship defined.

This has appeared indeed necessary since the additional evidence which paleontology affords unquestionably demonstrates the close affinities of these groups and strongly suggests a community of origin. Only three of these divisions are found in the Puerco, and these are the *Creodonta*, which are nothing more than slightly specialized Insectivores, the *Tæniodonta* and the Lemurs. It is evident therefore that the Tillodonts, Edentates Bats, Carnivores and Rodents must have been derived from these three, and seeing that the latter are so closely related in this epoch it would not do very great violence to the system to unite under this order an even greater number of these divisions than is done by Professor Cope. The origin of the *Carnivora* from the *Insectivora* through the specialized offshoot *Creodonta* is demonstrated beyond all peradventure, it seems to us, while many interesting and important facts have been discovered which throw a great deal of light upon the philogenetic history of the Dogs and Cats of modern times.

Although not indicated by Professor Cope, it seems in the highest degree probable that in his Insectivorous genus *Esthonyx*, we have the ancestor of the *Tillodontia*, which in turn gave origin to the *Toxodontia*.

Among the Lemuroids many new and interesting genera are added to the list, as well as much important information respecting them. Prominent among these is the description of the skull of *Anaptomorphus*, a remarkably specialized form for so early a period as the Wasatch from whose rocks it was derived. Others of scarcely less importance are described and figured.

Another discovery of importance, which is here recorded, relates to the probable ancestry of certain of the marsupials, including the very curious genera *Plagiaulax* and *Thylacoleo*. According to Professor Cope, *Ctenacodon* of Marsh, from the American Jurassic, is the ancestral type from which these Plagiaulacid Marsupials were derived. The line of development, as indicated by him, is as follows: *Ctenacodon*, *Plagiaulax*, *Ptilodus*, *Catopsolis* and *Thylacoleo*. *Ptilodus* and *Catopsolis*, from the Puerco Eocene, are the important links which have been added by Professor Cope, establishing not only an interesting fact of phylogeny, but adding at the same time another link to the chain between the Jurassic and Eocene Mammalia.

Altogether this ponderous volume forms one of the most substantial contributions to the subject which has ever been made, and certainly marks an epoch in the history of paleontological science. The genera and species considered are well systematized and defined, the descriptions clear and accurate, while the illustrations are for the most part well done. That which, however, will in all probability commend the work most to thoughtful students of paleontology, is the unusual grasp of its philosophic deductions which are in every way worthy of the marked ability of its author.

U. S. Army Med. Mus., Washington, D. C.
July 16, 1885.

Art. XL.—*Observations upon the Tertiary of Alabama;* by T. H. Aldrich.

In the following article, the results of a personal examination of the Tertiary of Alabama are given so far as is necessary to reply to the papers of Dr. Otto Meyer in the June and July numbers of this Journal, entitled "The Genealogy and the Age of the species in the Southern Old-tertiary."

The proof of the stratigraphical relationship, as worked out by Dr. Meyer, rests upon quotations from previous writers upon the subject, and upon a theory of descent and resemblance, which Dr. Meyer applies to the fossils of the different groups. There does not seem to be any positive statement in his articles that the superposition of the beds as given by him came under his actual observation, therefore I shall proceed by giving the stratigraphy at different points, and then some remarks upon his identifications of species.

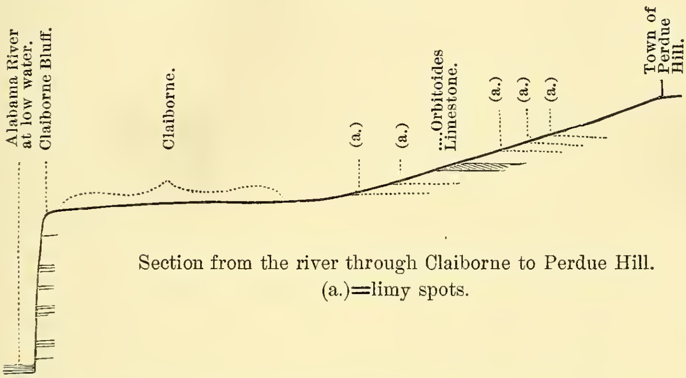
The old town of Claiborne, Ala., is built upon what is locally known as "second bottom" of the Alabama River, a level sandy plain over a mile wide at this place; Jackson on the Tombigbee River, and Selma and part of Montgomery, both upon the Alabama River, are built upon the same terrace and present almost precisely the same topographical features as Claiborne.

This terrace apparently is the oldest upon these rivers and has been subject to extensive erosion. The drift following filled up the depressions to a level plain, on the remains of which Claiborne now stands.

Going toward Perdue Hill through the main street of Claiborne (which runs in a southeasterly direction) at a point about one and a half miles back we reach the base of the hill; as we ascend it we find, in the road and gullies alongside, limy spots indicating the presence of the "White Limestone" below, and at a point about two miles from the bluff, on the side of the road, there is an outcropping of the White Limestone highly charged with Orbitoides and casts of several different forms of shells; by the aneroid barometer this point is 110 feet above the top of the bluff at the river. These limy spots continue to show still higher, with an occasional outcrop nearly to the top of the hill. The crown of the hill is covered with the red loam, and is 180 feet above the top of the bluff.

The dip of the strata at Claiborne seems to be a little west of south, therefore this street is very nearly upon the strike. This fact, together with the difference in level, plainly proves that at Claiborne itself the White Limestone is above the Claiborne group. The dip of the strata can be shown by observations made by a careful writer, the Rev. C. S. Hale;* on page 359 of that article he states that the Claiborne sand (No. 7) appears at low water at a point four miles south; the bottom of the bed at the Claiborne bluff is about 90 feet above low water; this shows a southerly dip of at least 22 feet per mile. The true dip is more than this, as the river runs W.S.W. between these places.

The following section with the above explanation will give a clear view of the vicinity of Claiborne.



Section from the river through Claiborne to Perdue Hill.
(a.)=limy spots.

South of Claiborne where the road crosses Randon's Creek, I found fine specimens of *Pecten Poulsoni* Mort., *Orbitoides Mantelli* Con., a species of Echinus and several casts of other shells. All these species are also found at Perdue Hill.

The section of Claiborne bluff following shows (No. 9 and 10) strata not given by Dr. Meyer, probably owing to a higher stage of water at the time of his visit. There is otherwise no material difference. The measurements were made at the lower landing with the exception of the two lowest beds which rise up at the upper landing and above (another proof of a southerly dip).

I wish here to correct an error which seems to be made by nearly every writer who has given a profile of the bluff at Claiborne,† and that is the designation of the lower part of the Claiborne section as *limestone.* It is a calcareous clay and not limestone, the average proportion of lime being about 12 per cent,‡ and Winchell's statement that this bed has not been recognized elsewhere is also erroneous, as shown further on.

Beds Nos. 9 and 10 are particularly interesting from the fact

* Geology of South Alabama, by C. S. Hale, this Journal, II, vol. vi, pp. 354–363. 1848.

† Conrad, J. A. N. S., 1st Series, vol. vii, p. 122. 1834. Winchell, P. A. A. S., 1856, Part I, p. 86. Tuomey, 1st Bien. Rept., 1850, p. 152.

‡ Lea, Contrib. to Geol., 1833, p. 22. Mell, Tr. Am. Inst. M. Engrs., 1880.

Section of the Bluff at Claiborne, Ala.

LOWER LANDING.	40 ft.	DRIFT.	
	45 ft.	White Limestone bed, containing Scutella and casts of shells. Zeuglodon bones found in this bed.—Hale, this Journal, 1848, p. 361	D
	3 ft.	Scutella beds	C
	6 ft.	Coarse ferruginous sands, indurated at bottom	B
	17 ft.	Claiborne sand—thin layers of lignite show about 10 ft. down from top. In places this layer becomes over a foot thick	A
	4 ft.	Indurated sandy ledge	1
	18 ft.	Calcareous clayey strata becoming sandy in lower part	2
	1 ft. 6 in.	Indurated sandy ledge	3
	5 ft.	Calcareous clay sandy at bottom	4
	5 ft.	Light yellowish gray calcareous sand, lower part indurated containing casts of shells	5
	27 ft.	Light yellowish gray calcareous sands containing *O. sellæformis* Con.. *Scutella Lyelli* in fragments, *Scalpellum Eocense* Mr., *Pecten Deshayesii* Lea, *Pecten scintillatus?* Con., etc. Shows indurated ledges in some places	6
	3 ft.	Layer of comminuted oyster shells	7
UPPER LANDING.	2 ft.	Dark blue black sandy clay	8
	10–15 ft.	Bluish green clayey sands, very few fossils in upper part. crowded below, a large number of the fossils distorted by pressure containing *O. sellæformis*. Con., *Venericardia rotunda* Lea. *Nucula magnifica*. *Arca rhomboidella* Lea. *Anomia* — *n. s.*, *Amphidesma limosa* Con. — many other bivalves	9
	6 ft.	Dark bluish greens and containing a peculiar small form of *Vener. planicosta* Lam., *Turritella Mortoni* Con., *Turritella* —*n. s. Crassatella—sp?*, *Corbula—sp?*, and many bivalves.	10

Alabama River at low water.

that they contain species in abundance that are rare in the Claiborne sand. *Turritella Mortoni* is found very large here. Owing to illness a complete list of species has not yet been prepared. The geographical distribution will be given in the future; the range of many species will be a surprise to those who rely upon paleontological evidence alone to prove the age of the various beds of the Tertiary of Alabama and Mississippi. In many cases actual superposition seems to be the only sure guide.

Now if Dr. Meyer's theory is correct we should find underneath the Claiborne beds the Jackson and Vicksburg formations.

We give below what we actually found. Proceeding up the river from the Claiborne Landing we lose all trace of the Tertiary rocks in about a mile and a half; then till we reach Lisbon, on the west side of the river about five miles up, only the recent bottom lands appear; here is a fine exposure of the lower Claiborne beds in a nearly vertical bluff about a mile long.

Section at Lisbon.

(*a*) Surface soil or loam	20 feet.
(1) Sandy strata with clay streaks, no fossils observed,	10 feet.
(2) Sandy clays, dark brown, badly weathered, highly fossiliferous, equals No. 9 and 10 of Claiborne section, contains some new species, also *Amphidesma limosa* Con., *Arca rhomboidella* Lea, *Turritella*, n. s., *V. planicosta* Lam., *V. rotunda* Lea, *Lucina compressa* Lea, *Ancillopsis vetustus* Con., *Rostellaria Whitfieldi* Heilpr., etc.	12′ 0″
(3) Hard sandy ledge	0 8″
(4) Calcareous clayey sands, light yellow when wet, nearly white when dry*	6′ to 8′ 0
(5) Coarse grained ferruginous sands, fossils numerous	3′ 0″
(6) and (7) Light yellow sand with a hard ledge on top, lower five feet dark blue when wet	20 feet.
(8) Bluish-black clay with remarkable fucoidal looking raised rib-like concretions upon the exposed bedding planes	8 feet.

No. 8 is the top of the Buhr-stone series which are exposed higher up the river. *Lapparia dumosa* Con., = *Mitra pactilis* of Claiborne sand, is found in No. 5. This is the only Jackson fossil in addition to those already known found here.

About one mile from McCarty's ferry on the Tombigbee

* There is a remarkable difference in color between beds when wet and when dry. At Prairie Bluff. Ala., we noticed that deep blue clayey beds weather out higher upon the bluff, where perfectly dry. to a nearly pure white sand. The distinctive characters are so few between hundreds of layers that it is hard to characterize them so as to be recognized by a future observer.

River, we measured the Buhr-stone with the aneroid, making it 270 feet thick from our camp on top of the hill to the river. Dr. Smith's observations at other points indicate at least 70 feet more to go on top of this, giving at least 340 feet of thickness.

All the fossils found in the Buhr-stone were Clabornian in specific character; I mention *Venericardia parva* Lea, *T. obruta* Con., *V. rotunda* Lea and *Corbula Murchisoni* Lea.

Continuing our section downward we find at White Bluff on east bank of the Tombigbee River, the following, viz: at top.

(1) Hard Buhr-stone, vertical bluff	90–100 feet.
(2) Clays	30 feet.
(3) Clay with lignite stems distributed throughout	3′ 0″
(4) Clay	5′ 0″
(5) Lignite in fine streaks	0′ 6″
(6) Barren clays	100′ 0″

Coming up the river toward Wood's bluff, we find, rising from under this exposure:

(7) Clays, some layers very sandy. Found one specimen of *Athleta Tuomeyi* Con. here	50′ 0″
(8) A thin streak of fossiliferous sand containing Wood's bluff fossils	0′ 2″
(9) Clays, barren	12′ 0″
(10) 2–4 feet Green sand, brown outside, dark green when freshly cut, fossiliferous, *Lævibuccinum lineatum* Heilpr., seems confined to this bed	4′ 0″
(11) Clay, dark grayish-blue, containing *Athleta Tuomeyi* Con., abundant	8′ 0″
(12) Indurated greensand	2′–3′ 0″
(13) Greensand marl (fine fossils here)	3′
(14) Indurated Greensand marl crowded with shells, large proportion Turritellas	6′ 8″
(15) Indurated Greensand marl ledge making a shoal in the river, showing	5′ 0″

Nos. 12, 13, 14 and 15 really form one marl bed, the distinctions are mainly in hardness and in being more or less fossiliferous. This group (Wood's bluff) is very extensive, and can be traced easily to the western border of the State. We found it at Butler, Choctaw Co. Professor Heilprin has described most of the species found here.*

It is unnecessary to continue this series farther to prove the object in view; but nearly every bed down to the Cretaceous has been examined without revealing the Jackson or Vicksburg groups.

Dr. E. A. Smith and Dr. Lawrence Johnson have made sec-

*P. A. N. S., 1880, p. 364.

tions of the whole Tertiary. Their report is now in course of publication by the National Survey.

Having personally collected, in company with Dr. Smith this summer, in nearly every bed known below this (Wood's bluff). horizon, I can testify that no evidence of much value can be obtained from the fossils. The Vicksburg and Jackson have not been found down 710 feet below Claiborne as the above sections will testify; if they are below this, then Dr. Meyer has got to sandwich in between Claiborne and Jackson the whole of the Buhr-stone formation. This formation is found only in the N.E. of Mississippi, a long distance from the position of the two groups mentioned.

Dr. Meyer is equally unfortunate in his quotations from authorities. I review as follows:

1833. Conrad, *Spondylus dumosus* Mort. is here spoken of as a stumbling block in Conrad's way and especially to Lyell. We have lately found it at Hatchitigbee bluff, 25 feet beneath the buhr-stone.

1834. Observations, etc., Conrad. In this section Conrad distinctly states that he saw only the "White Limestone" and the "Bluish Limestone," Nos. 7 and 8 of his section *near* Claiborne, and probably made his erroneous determination from the fact, as stated by Lyell, that the Claiborne beds are worn away largely in places and have been replaced by the "White Limestone." If Conrad had made a trip to Perdue Hill at the time of his visit the error would not have been made.

March, 1846. Lyell's general statements are correct and proved by all subsequent observers to have been very carefully made.

1850. M. Tuomey, 1st Biennial Rept. of the Geol. of Ala. Dr. Meyer quotes Tuomey as follows, p. 149: "Sir Chas. Lyell has proved that the White Limestone is newer than the fossiliferous bed at Claiborne by showing that this bed which contains identical fossils underlies the bluff at St. Stephens. This is certainly the case, for although this bed is not seen at the base of the bluff it is overlaid, as I have just stated, by a yellow limestone which is a prolongation of that at St. Stephens."

By a juxtaposition of sentences Dr. Meyer evidently proves satisfactorily to himself "that Lyell's Claibornian bed at the base of St. Stephens bluff according to his (Lyell's) determination need not be Claibornian, but that it is also *not* at the base of St. Stephens bluff." Having personally examined this exposure within the past month in company with Professor Smith, State Geologist of Alabama, I consider that Lyell's statement is correct. The same bluff that is at St. Stephens is over the Claiborne sand bed at the point Tuomey speaks of. A rough section taken at this place, which was hurriedly done owing to a heavy shower at the time of our visit, is as follows:

Section half a mile north of St. Stephens.

No. 6. White limestone, highly charged with *Orbitoides Mantelli* .... 40 feet.
No. 5. Hard ledge limestone containing spines of a species of Cidaris .... 2 feet.
No. 4. White limestone containing *P. perplanus* Mort., etc., no Orbitoides found, ... 50 feet.
No. 3. Scutella bed. Yellow sand indurated in places,.. 2 feet.
Also containing Osteodes — sp. ?
No. 2. Claiborne sand, 15 feet thick in places measured, containing the well known Claiborne fossils .... 15 feet.
The sand is a trifle redder than at Claiborne, but a single glance is enough to show they are the same. I noted the following species among many others : *Rostellaria velata* Con., *Pecten Deshayesii* Lea, *Crassatella protexta* Con., *Venericardia rotunda* Lea, *V. transversa* Lea, *Turbinolia Maclurii* Lea, *Dentalium thalloides* Con., *Astarte sulcata* Lea, *Corbula Murchisonii* Lea, *Cytherea perovata* Con., *Cyth. æquorea* Con., *Fusus protextus* Con., *Melongena alveata* Con., *Crassatella alta* Con.
No. 1. A blue sandy clay containing a few Claiborne fossils and a species of Osteodes .... 10 feet.
Tombigbee River level.

This should be convincing.

Dr. Meyer in his opening quotation from Tuomey which is given previously should have added the succeeding paragraphs. I quote:* "Another locality occurs a few miles from Clarksville on the land of Mr. Chambers, on one of the branches of Satilpa Creek, where this fossiliferous (Claiborne) bed is laid bare by the denudation of the upper beds and appears in the bottom of a ravine, in the very midst of the White Limestone, at a locality too where the latter rock is rich in the remains of Zeuglodon." Dr. M. leaves out of his authorities the Rev. C. S. Hale.† The reader is referred to p. 360 where he gives two localities near Claiborne (below) showing a section from the Scutella bed (C. of my Claiborne profile) up to the Orbitoides limestone.

Winchell,‡ on page 84–85, gives two localities where the Claiborne sand has the White Limestone above it, namely, Stone Creek and in Clark County, Ala.

The northern dip mentioned by Hilgard has its counterpart in Alabama in several places ; in fact there is a large basin in the Tertiary of Alabama, first spoken of by Tuomey,§ probably with a smaller sub-basin north of it. Professor E. A.

* 1st Bienn. Rept., 1850, p. 148.
† This Journal, 1848, p. 354–363.
‡ P. A. A. S., 1856, pp. 82–93.
§ 1st Rept., 1850, p. 150, and in Hilgard. this Journal, new series, 1867, vol. xiii, p. 37.

Smith has confirmed this, and is now engaged in completing his observations.

A locality north of Barrytown, Ala., at and in the vicinity of a mill spoken of by Tuomey,* furnishes another section from the Lisbon beds up to the White Limestone, a small patch of which is left on top of Womac hill. The bed at the mill contains: *O. sellæformis* Con., very large and fine; *Teredo* — n. s.; *Scalpellum Eocense* Meyer; *Pecten Deshayesii* Lea, and corresponds with the bed (*b*) of Dr. Meyer's section. Winchell's statement that this bed is not found elsewhere is disproved.

The species described by Conrad† which were received from Dr. Spillman in which he gives the locality "Enterprise, Miss.,". Dr. S. writes me (Aug. 14, 1884) were not found there; he says: "I have no recollection of sending T. A. Conrad any fossils from near Enterprise. I sent him some from Garland's Creek, three miles east of Shubuta, Miss., in the southern part of Clark County." This removes one question, as these shells are undoubtedly Jacksonian and Dr. Meyer is no doubt correct in calling the beds at Enterprise Claibornian.

One more point remains to be quoted, and this is, that the crystalline limestone of the Vicksburg group has never been found in Alabama below the Claiborne sand, while crystalline limestone over 150 feet thick shows above it.

Reviewing Dr. Meyer's summary of reasons:

1st. He speaks of the lower limestone, which is not a limestone; also sections already given show beyond question that Lyell was correct in his general statements.

2d. This is no argument whatever; differences in level of 100 feet between points nearly 100 miles apart have no stratigraphical value. If the general dip is southerly in Alabama and southwest to west in Mississippi, all the beds are sure to outcrop at the surface *somewhere.*

3d. The Jackson group in Mississippi presents a mixture of Vicksburg and Claiborne forms, and this very fact is a strong argument in favor of its true position, being between the two groups. *Mitra pactilis* Con. is common at Jackson as *M. dumosa* Con., yet it also is found at Lisbon, still lower than Claiborne sand by 100 feet. The relationship is therefore to be largely widened.

4th. *V. parva* Lea has a still larger range, probably through 1000 feet of strata.

5th. *Venericardia diversidentata* Mr., from Jackson, is nothing more nor less than *V. rotunda* Lea. Conrad at one time evidently considered it new, as he gives a name in Wailes (Geol. of Miss.) "*Cardita tetrica*," but afterward abandons it. This

* 1st Rept., p. 148. He also mentions finding *O. Sellæformis* Con. here.
† Am. Jour. of Conchology, 1865, vol. i, p. 137.

species ranges through nearly the whole Tertiary, the Vicksburg group included. I have it from Vicksburg.

Fulgur Mississippiensis Con. and *Tellina Vicksburgensis* Con. are found in both groups, also in the intermediate Red Bluff strata. Why they could not *ascend* from the Jackson to Vicksburg passes my understanding. *Fulgur filius* Mr. seems from the description to be *F. Mississippiensis* itself; the differences pointed out are trifling, and there are intermediate forms.

6th. *Pleurotoma terebralis* Lam. = *P. cristata* Con. I have from the Greggs Landing marl which is several hundred feet below the Wood's bluff group and not far from the Cretaceous, therefore its origin is below all the groups in question. The Acteon spoken of has a greater range than given.

In reference to *Natica Mississippiensis* Con.: As the Wood's bluff is over 700 feet below the Claiborne its origin is simply shown to be below the three groups under discussion.

7th. Here Dr. Meyer is guilty of assuming a parallelism which he has not *seen* and of which he gives no *proof* whatever.

In conclusion, let me state that the Tertiary is a great deal thicker than has been before supposed, and arguments based upon the upper quarter of its thickness are very likely to be upset by the paleontology of the three-fourths as yet unknown. An enormous territory remains here for the paleontologist, both in Mississippi and Alabama, entirely outside of the three groups, Vicksburg, Jackson and Claiborne, full of beautiful new species and gigantic forms of little Claiborne shells, that would cause one to exclaim, what could have led to such degeneration!

There is no doubt that there is a relationship existing between fossil species the same as in living forms, but, until the great unknown territory is more fully explored, comparisons are not apt to be of much value. Dr. Meyer has shown great industry in his papers, and apart from his unfortunate mistake in stratigraphy, they are well worth especial study.

ART. XLI. — *On the Electrical Furnace and the reduction of the Oxides of Boron, Silicon, Aluminum and other metals by Carbon*; by EUGENE H. COWLES, ALFRED H. COWLES and CHARLES F. MABERY.*

THE application of electricity to metallurgical processes has hitherto been confined chiefly to the reduction of metals from solution, and few attempts have been made to effect dry reductions by means of an electric current. Sir W. Siemens endeavored to utilize the intense heat of an electric arc for this purpose, but accomplished little beyond fusing several pounds of steel. A

* Read at the Ann Arbor meeting of the American Association.

short time since Eugene H. Cowles and Alfred H. Cowles, of Cleveland, conceived the idea of obtaining a continuous high temperature on an extended scale by introducing into the path of an electric current some material that would afford the requisite resistance, thereby producing a corresponding increase in the temperature. After numerous experiments that need not be described in detail, coarsely pulverized carbon was selected as the best means for maintaining a variable resistance, and, at the same time, as the most available substance for the reduction of oxides. When this material, mixed with the oxide to be reduced, was made a part of the electric circuit in a fire-clay retort and submitted to the action of a current from a powerful dynamo machine, not only was the reduction accomplished, but the temperature increased to such an extent that the whole interior of the retort fused completely. In other experiments, lumps of lime, sand and corundum were fused, with indications of a reduction of the corresponding metal; on cooling, the lime formed large well defined crystals, the corundum beautiful red, green and blue hexagonal crystals.

Following up these results with the assistance of Charles F. Mabery, Professor of Chemistry in the Case School of Applied Science, who became interested at this stage of the experiments, it was soon found that the intense heat thus produced could be utilized for the reduction of oxides in large quantities, and experiments were next tried on a large scale with a current from two dynamos driven by an equivalent of fifty horse-power. For the protection of the walls of the furnace, which were made of fire-brick, a mixture of the ore and coarsely pulverized gas carbon was made a central core, and it was surrounded on the sides and bottom by fine charcoal, the current following the lesser resistance of the central core from carbon electrodes which were inserted at the ends of the furnace in contact with the core. In order to protect the machines from the variable resistance within the furnace, a resistance box consisting of a coil of German silver wire placed in a large tank of water was introduced into the main circuit, and a Brush ammeter was also attached by means of a shunt circuit to indicate the quantity of current that was absorbed in the furnace. The latter was charged by first filling it with charcoal, making a trough in the center and then filling this central space with the ore mixture, which was covered with a layer of coarse charcoal. The furnace was closed at the top with fire-brick slabs containing two or three holes for the escape of the gaseous products of the reduction, and the entire furnace made air-tight by luting with fire-clay. Within a few minutes after starting the dynamo, a stream of carbonic oxide issued through the openings, burning usually with a flame eighteen inches in height.

The time required for complete reduction was ordinarily about an hour.

The furnace at present in use is charged in substantially the same manner, and the current is supplied by a Brush machine of variable electromotive force driven by an equivalent of forty horse-power. A Brush machine capable of utilizing 125 horse-power, or two and one-half times as large as any hitherto constructed by the Brush Electric Company, is being made for the Cowles Electric Smelting and Aluminum Company, and this machine will soon be in operation. Experiments already made show that aluminum, silicon, boron, manganese, magnesium, sodium and potassium can be reduced from their oxides with ease. In fact there is no oxide that can withstand temperatures attainable in this electrical furnace. Charcoal in considerable quantities is changed to graphite; whether this indicates fusion or solution of carbon in the reduced metal has not been fully determined. As to what can be accomplished by converting enormous electrical energy into heat within a limited space, it can only be said that it opens the way into an extensive field for pure and applied chemistry. It is not difficult to conceive of temperatures limited only by the capability of carbon to resist fusion. The results to be obtained with the large Brush machine above mentioned will be of some importance in this direction.

Since the cost of the motive power is the chief expense in accomplishing reductions by this method, its commercial success is closely connected with the cheapest form of power to be obtained. Realizing the importance of this point the Cowles Electric Smelting and Aluminum Company has purchased an extensive and reliable water-power, and works are soon to be erected for the utilization of 1200 horse-power. An important feature in the use of these furnaces, from a commercial standpoint, is the slight technical skill required in their manipulation. The four furnaces in operation in the experimental laboratory at Cleveland are in charge of two young men 20 years of age who, six months ago, knew absolutely nothing of electricity. The products at present manufactured are the various grades of aluminum bronze made from a rich furnace product that is obtained by adding copper to the charge of ore, silicon bronze prepared in the same manner, and aluminum silver, and alloys of aluminum with several other metals. A boron bronze may be prepared by the reduction of boracic acid in contact with copper.

As commercial results, may be mentioned a daily production in the experimental laboratory averaging fifty pounds of 10 per cent aluminum bronze; and it can be supplied to the trade in large quantities at prices based upon $5 per pound for the aluminum contained, the lowest market quotation of this metal being at present $15 per pound. Silicon bronze can be furnished at prices far below those of the French manufacturers.

The alloys, which the metals obtained by the methods above described form with copper, have been submitted to careful study. An alloy containing 10 per cent of aluminum and 90 per cent of copper forms the so-called aluminum bronze, with a fine golden color, that is retained in the atmosphere for a long time. The tensile strength of this alloy is usually given as 100,000 pounds to the square inch; but castings of our 10 per cent bronze have stood a strain of 109,000 pounds. It is a very hard, tough alloy, with a capacity to withstand wear far in excess of any other metal in use. All grades of aluminum bronze make fine castings, taking very exact impressions, and there is no loss in remelting as in the case of alloys containing zinc. The 5 per cent aluminum alloy is a close approximation in color to 18 carat gold and does not tarnish readily. Its tensile strength in the form of castings is equivalent to a strain of 68,000 pounds to the square inch. An alloy containing 2 or 3 per cent aluminum is stronger than brass, possesses greater permanency of color and would make an excellent substitute for that metal. When the percentage of aluminum reaches 13 an exceedingly hard, brittle alloy of a reddish color is obtained; and higher percentages increase the brittleness and the color becomes grayish-black. Above 25 per cent the strength again increases.

The effect of silicon in small proportions upon copper is to greatly increase its tensile strength. When more than 5 per cent is present the product is exceedingly brittle and grayish-black in color. It is probable that silicon acts to a certain extent as a fluxing material upon the oxides present in the copper, thereby making the metal more homogeneous. On account of its superior strength and high conductivity for electrical currents, silicon bronze is the best material known for telegraph and telephone wire.

The element boron seems to have almost as marked an effect upon copper as carbon does upon iron. A small percentage in copper increases its strength to 50,000 or 60,000 pounds per square inch without diminishing to any extent its electrical conductivity.

Aluminum increases very considerably the strength of all metals with which it is alloyed. An alloy of copper and nickel with a small percentage of aluminum, called Hercules metal, withstood a strain of 105,000 pounds and broke without elongation. Another grade of this metal broke under a strain of 111,000 pounds with an elongation equivalent to 33 per cent. It must be remembered that these tests were all made upon castings of the alloys. The strength of common brass is doubled by the addition of 2 or 3 per cent of aluminum. Alloys of aluminum and iron are obtained without difficulty; one product was analyzed containing 40 per cent of aluminum. In the furnace, iron does not seem to be absorbed readily by the reduced aluminum when copper is present; but in one experiment a mixture composed of old files 60 per cent, nickel 5 per cent, and of 10 per cent aluminum bronze, 35 per cent was melted together and it gave a malleable product that stood a strain of 69,000 pounds.

When the reduction of aluminum is conducted in the absence of other metals it forms a compound with carbon analogous to pig iron as it comes from the blast furnace; and products are frequently analyzed that contain sixty or seventy per cent of aluminum. If the ore contains silicon the latter is absorbed by the aluminum and compounds of the two elements containing ten or fifteen per cent of silicon are often taken from the furnace in considerable quantities. These important products are at present under examination.

Art. XLII.—*The Grand Rapids Meteorite*; by R. B. Riggs.

In a recent number of this Journal (October, 1884), I. R. Eastman describes a meteorite found in Grand Rapids, Michigan. A preliminary analysis was made at the time, but of a very inadequate amount of the oxidized material, taken from the surface. Since then the meteorite has come into the keeping of the National Museum, and a more complete analysis gives the following results:—

Fe	88·71
Ni	10·69
Cu	·07
Mg	·02
P	·26
S	·03
C (combined)	·06
Graphite	·07
	99·91

It is a mass of great apparent homogeneity, weighing originally about 50 kilograms. One of the sections, however, on being polished, discloses a nodule about a centimeter in diameter, like troilite in appearance, which remains to be investigated.

A polished surface of the meteorite etched with nitric acid developed very handsome Widmannstättian figures somewhat like those on the iron from Robertson County, Tennessee.

Chemical Laboratory U. S. G. S., Washington, Aug. 26, 1885.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Sensitiveness of Selenium and Sulphur to Light.* — The remarkable property possessed by selenium of having its electrical resistance varied by the action of light has been the subject of many investigations, since its first announcement in 1873. The cause of this phenomenon is discussed by Shelford Bidwell in a recent number of the Philosphical Magazine (August, 1885, pp. 178–191). Remarking upon the ingenious method of forming selenium cells described by C. E. Fritts, who melted the selenium as a thin film on a plate of metal with which it forms a sort of chemical combination, he suggests the similarity of some of the phenomena described by Fritts to those of electrolysis. The arrangement of the two metallic plates with the third substance between them, that is, of the selenium between the metal plate upon which it is melted and the gold leaf film by which it is covered, is suggestive of this; while the unequal resistance of the two surfaces and the generation of an independent electromotive force, in conjunction with the polarization effects observed, make the conduction of selenium seem truly electrolytic. The objection that the selenium itself is not a electrolyte is met by the suggestion that in the process of making the cell a metallic selenide may have been formed, and the apparently improved conductivity of the selenium, and the accompanying phenomena, may be accounted for by the existence of this selenide, rather than by any change in the crystalline condition of the selenium.

As bearing upon this question the author made a series of experiments with sulphur. Five parts of sublimed sulphur and one of precipitated silver were heated together, and a cell constructed by spreading some of the melted sulphur, containing particles of silver sulphide, evenly over a surface of mica, and then laying a piece of thin silver-foil on its surface. The cell was found to vary in resistance to a marked degree when exposed to the light of a burning magnesium wire. Other experiments went to show the same result and to prove that the effect of the light was due to the action of radiation proper and not to any incidental rise in temperature. Other cells, constructed in a somewhat different manner, behaved in the same manner though in different degrees; with all the resistance diminished to a marked extent under the action of radiation. One of them, the resistance of which was small, was connected with ten Leclanché cells and a telephone, and exposed to a rapidly interrupted beam of light; it gave a musical note nearly as loud as would be obtained from a good selenium cell. All of the sulphur cells resembled selenium in giving polarization-currents after being detached from the battery. The author urges that the effects of radiation, in the case of the sulphur-silver cell, are to be looked for principally at the surface of the electrodes, though not necessarily confined entirely to it. It is shown that light favors the union of sulphur and silver to form silver sulphide, and it is suggested that the same influence which would assist the union of two substances when they have a tendency to unite might act conversely when they have a tendency to separate. Radiation, therefore, acting upon a thin layer of silver sulphide might exert a material influence upon the conductivity of the sulphide by facilitating the molecular rearrangement of the atoms of sulphur and silver. The bearing of all this upon the explanation of the action of light on selenium is obvious, though experimental proof is needed to establish it.

2. *Molecular Shadows in Incandescent Lamps.* — The formation of metallic deposits in incandescent lamps with a carbon filament is described by J. A. FLEMING, the metal coming from the wire to which the carbon is attached. A sage-green deposit of copper is observed in an Edison lamp not infrequently; and in one case a silvery transparent metallic film of platinum was deposited on the inside of the glass bulb. These deposits recall the experiments of Wright (this Journal, xiii, 49; xiv, 169, 1877), who formed brilliant specula by deposition from a metallic electrode in exhausted tubes. An interesting point in the lamp deposits was the existence, under some circumstances, of a line of no deposit on the surface in the plane of the filament. This is to be regarded as the shadow of the loop in which the trajectory of the molecules is interfered with by the carbon filament. The deposit near the neck of the bulb is thicker than at the crown because of the greater number of molecules which here strike the glass. In the case of the carbon deposit, often observed, it was found that the molecular shadow of the filament, or the line of no deposit, could be formed by suddenly raising the filament to a very high temperature, as for instance by placing a 50-volt lamp for an instant on a 100-volt circuit; but when the deposit went on slowly no line was observed, only a general smokiness. In the former case the projection of the molecules from the carbon is violent enough to prevent their paths from being altered by collision with molecules of the residual air, so that a larger proportion reach the envelope in the direction of projection, thus causing a deposit on all parts except as shielded by the loop. The best shadows were obtained from a lamp with a single loop.—*Phil. Mag.*, August, 1885, pp. 141–144.

3. *Disintegration of the carbon filament in an incandescent Electric lamp.*—Some experiments carried on by BUCHANAN, having as their object the deciding of the question as to whether the observed breaking of the carbon filament at the negative end was due to a local excess of temperature or a "Peltiér effect," led to a negative result; the local heating detected being too small to account for a break at one end rather than the other. The author concludes that the disintegration of the carbon filament preceding complete destruction, as described by Preece, is doubt-

less the result of molecular changes in its structure produced by maintaining it at very high temperature; and he finds manifestation of the alteration in the change of thermo-electric power of the carbon.—*Phil. Mag.*, August, 1885, pp. 117–126.

4. *On changes produced by Magnetization in the Length of Rods of Iron and Steel.*—An abstract of an article on this subject by SHELFORD BIDWELL, after mentioning the results reached by earlier experimenters, goes on to remark that by using thinner iron rods and greater magnetizing forces the curious fact was established that if the magnetization be carried beyond a certain critical point, the consequent elongation, instead of remaining stationary at a maximum, becomes diminished, the diminution increasing with the force. If the force is sufficiently increased, a point is reached where the original length of the rod is totally unaffected by magnetization; and if the magnetization be carried still further, the original length of the rod is reduced. It also appeared that the position of the critical point in steel depended in a remarkable manner upon the hardness or temper of the metal. The author embodies his results for iron, steel and nickel in a series of formal laws.—*Proc. Roy. Soc.*, No. 237, p. 265.

II. GEOLOGY AND MINERALOGY.

1. *Notes on some of the Geological Papers presented at the Meeting of the American Association at Ann Arbor:* *

(1.) A. WINCHELL. On the Geology of Ann Arbor. The following is a summary of the geological facts within easy reach of Ann Arbor. Drift covers the nearest outcrop of rock 20 miles away; depth of drift at the University 200 feet, at the Observatory, 230 feet; on hills west and north probably 375 feet. Indications of terminal moraine. Kettle hole within a few rods. The drift accumulations rest on the outcropping belt of Marshall sandstone, trending northeast-southwest. Sheets of impervious materials included, forming water basins, and this source of supply is the basis of the water-works of Ann Arbor. Tabular limestone masses imbedded in upper part of drift as formerly described by the writer in this Journal, II, xl, 331–8; ascribed to action of ice-floes in Scientific Monthly, Toledo, Oct., 1875, this Journal, III, xi, 225–8; with additional facts in Proc. Amer. Assoc., xxiv, 1875, pp. 27–43; differently explained by T. C. Chamberlin in Ann. Rep. U. S. Geol. Surv., 1881–2, pp. 326–330.

(2.) S. G. WILLIAMS. Traced the Lower Helderberg strata into western New York.

(3.) A. H. WORTHEN. On the Quaternary deposits of Illinois. The author exhibited sundry sections obtained by borings in central and southern-central Illinois. They showed generally that the lower portion of the Quaternary formation is strictly stratified; that no bowlder beds exist, but that bowlders are sparsely scattered through the middle portions; that a dirt bed generally

* For these notes this Journal is indebted to Professor Alexander Winchell.

separates the lower and middle portions; that finally, the explanations usually applied to the northern drift will not apply in these parts of Illinois.

(4.) A. Winchell. On sources of trend and Crustal Surplusage in mountain structure. [The first part embodied views embraced in a paper sent Professor J. D. Dana in 1881.] The second part traced the consequences of slow subsidence of the earth's equatorial protuberance resulting from the secular retardation of its axial velocity of rotation caused by the action of the moon on the lagging tide. [The section voted to request a fuller abstract of this communication, the paper being orally presented.]

(5.) Wm. B. Taylor. On a probable cause of the shrinkage of the Earth's Crust. By a singular coincidence this paper considered the second cause mentioned in the preceding paper. It was, however, only to explain *surplusage* of circumference; while the other employed the principle for both surplusage and trend.

(6.) H. S. Williams. On the classification of the Upper Devonian. Presented numerous studied sections of strata ranging east-and-west in southern New York, and pointed out the progressive changes in the faunas. He thought there was some ground for admitting that the equivalent of the Catskill group might be sought within the range of the Waverly series of the West. To this Professor Hall sharply demurred, and claimed that if the Catskill is made Carboniferous, then all, to the bottom of the Corniferous, must be so made, since traces of a Catskill fauna are found in eastern New York below the Corniferous. A. Winchell recalled the doctrine of "Colonies" maintained by Barrande, and instanced the case of an Upper Silurian fauna of over 3,000 feet occurring in the midst of the Lower Silurian in Bohemia.

(7.) Edward Orton. Exhibited the records of a deep well at Cleveland, Ohio, in which over 200 feet of rock-salt were passed, with a parting of 15 feet of shale and 81 of limestone, at a horizon apparently *below* the Niagara limestone. But the final interpretation of the section was left for future study.

(8.) E. W. Claypole. On the Materials of the Appalachians. Held that the vast volume of the deposits and their increasing coarseness toward the southeastern part of Pennsylvania imply the former existence of a lofty pre-paleozoic range to the east of the present Appalachians.

(9.) N. H. Winchell. On *Lingula* and *Paradoxides* from the Red Quartzites of Minnesota. Exhibited a large slab from the "pipestone quarries," which was covered with small shells named by him *Lingula*. The remains of the shells on chemical testing showed a distinct phosphatic reaction. From the same quarries was exhibited a form regarded as an imperfect *Paradoxides*, showing the central axis and part of the pleuræ of the right side. As this pipestone bed is included in the quartzite of Wisconsin (at Baraboo, etc.) described by the Wisconsin geologists as Huronian, the discovery is important.

A. Winchell stated that the so-called shells appeared to be undoubtedly brachiopods of the pleuropygian order, but with characters intermediate between *Lingulidiæ* and *Discinidæ*. He reminded the section that he had years before assigned the Baraboo quartzite to the Lower Silurian on lithological grounds, though Professor Irving had subsequently attempted to invalidate the reference. E. W. Claypole thought the objects exhibited were undoubtedly organic and coincided as to the affinities of the shells.

(10.) A. Winchell. On *Cœnostroma* and *Idiostroma* and the comprehensive character of Stromatoporoids. Enumerated the structural characters found in the group, and traced the morphology of each structure separately, showing on what grounds generic distinctions had been based. He then indicated the assemblages of characters which distinguish *Cœnostroma* and *Idiostroma* from other genera, and gives them validity. He finally pointed out the fact that the Stromatoporoids possess a very wide range of affinities: with *Sponges*, in external aspect, curdled tissue, water canals and oscula, though none of these characters are present in all; with *Foraminifera*, in their laminæ and interspaces; in their radial, branching canals, seen in Eozoön, and in the concentric layers, interspaces and curdled structures of foraminiferal *Parkeria;* with *Anthozoa*, in their laminæ (compared with *Heliolitidæ* and some *Favositidæ*), their radial structures, their individuation (incipient in *Cœnostroma*, completed in *Idiostroma*), and the (by some) supposed tabulæ of *Caunopora;* with *Hydrozoa*, in the resemblances of *Hydractinia* and more strikingly, *Labechia*, which may be regarded really a Stromatoporoid; with *Polyzoa*, in the tubes and (supposed) tabulæ of *Caunopora*, and some further resemblances of *Eschara nobilis*. It is vain, therefore, to seek to place Stromatoporoids within the bounds of any recognized class-type. The author exhibited numerous specimens, thin sections and photographs illustrating generic distinctions, and circulated a printed synopsis of Stromatoporoids.

For a full list of the geological papers presented to the Association see page 324.

2. *Can underground heat be utilized?*—J. Starkie Gardner has an article on this subject in the Geological Magazine for September. The author concludes from the increase of heat downward, from metamorphism, from volcanoes, and from the earth's up and down movements, that there is abundant reason for believing that the earth has a thin crust. The movements would be physically impossible in an earth solid throughout. If the principal mass is kept solid at a temperature beyond the fusing point of rock through the pressure of the external envelope, "the pressure must become relaxed as the surface is neared, and at a certain point the rock must obey its impulse and melt," and thus make a layer in a state of fusion. The movements of the crust are more compatible with a crust of ten miles thickness than with one of fifty miles. "The deepest artesian well in the world

is being bored at Pesth, and has already a depth of 951 meters. The work is undertaken by the brothers Zsigmondy, partially at the expense of the city which has granted £40,000 for the purpose, with the intention of obtaining an unlimited supply of warm water for the municipal establishments and public baths." The present temperature is 161° F.; and it will be prosecuted until water of 178° is obtained. About 175,000 gallons of warm water stream out daily, rising to a height of 35 feet. "It needs no seer to pierce the not-distant future when we shall be driven to every expedient to discover modes of obtaining heat without the combustion of fuel, and the perhaps far more remote future when we shall bore shafts down to the liquid layer and conduct our smelting operations at the pit's mouth."

3. *A gigantic bird of the Lower Eocene of Croydon, Gastornis Klaassenii;* E. T. Newton. (Geol. Mag., August, 1885.)—The remains of this bird indicate a species as large as the New Zealand Dinornis. The most perfect tibiotarsus when complete must have had a length at least of 20 inches, and its trochlear extremity is 3½ inches wide; while in another specimen the latter is 4 inches wide. The remains are from the "Blue Clay" and lignite patches of the Woolwich beds. The original specimen of Gastornis—*G. Parisiensis*—was from the Lower Eocene beds of Meudon, near Paris. The Anserine affinities of Gastornis, as regards the tibiotarsus, held by some writers, are confirmed by the detailed comparison of the Croydon bones with recent forms. In other parts of its organization the genus is regarded by Dr. Victor Lemoine as having affinities with the Ratitæ.

4. *Comstock Mining and Miners;* by Eliot Lord. U. S. Geol. Survey, Clarence King, Director. 452 pp. 4to. Washington, 1883. (Recently issued; bearing the date of March 1, 1882, in the letter of transmittal.)—This report is a history of the development of the Comstock mines to the close of the year 1880, and, as the preface observes, it is the story of the birth of the silver-mining industry in this country as well as of its vigorous growth. On account of the great productiveness of the lode, the rapid movement in population it occasioned, the quick succession of events, and the later decline and depopulation, the history has unusual social and political interest. It is full of surprising incidents, and of vivid descriptions of scenes and occurrences, and contains much in the way of social and mining statistics. The interesting volume is illustrated by three excellent maps.

5. *Materialien zur Mineralogie Russlands von* N. von Kokscharow. Vol. ix, pp. 81–272. St. Petersburg, 1885.—A continuation of Kokscharow's great work on Russian Mineralogy is always a welcome and valuable addition to mineralogical literature. The species discussed at length in this part of the 9th volume are turquois, wulfenite, topaz, vesuvianite, nepheline, sanidine, linarite.

III. Botany.

1. *The Microscope in Botany: a Guide to the Microscopical Investigation of Vegetable Substances. From the German of Dr. Julius Wilhelm Behrens.* Translated and edited by Rev. A. B. Hervey, A.M., assisted by R. H. Ward, M.D., F.R.M.S. Illustrated by thirteen plates and 153 cuts. Boston: S. E. Cassino & Co. 1885. pp. 466, 8vo.—This is a large and full book, on the microscope and its use in the investigation of vegetable structures and products. For the translator, while it has evidently been a labor of love, it must have been a long and serious task; and the publisher has brought it out in the best style, one would say upon superfluously fine and thick paper, which, however, allows the illustrations to appear at their best. One-third of the volume is devoted to the microscope and its appliances. The preparation of microscopical objects and microscopical reagents are discussed in about the same number of pages; and the microscopical investigation of the principal vegetable substances is treated with similar fullness. Dr. Ward has borne a part in the earlier chapters. Dr. Corwentz of Danzig contributed the short and very useful section upon the preparation of fossil plants. There is a good section on drawing under the microscope. In respect to the more important vegetable substances copious bibliographical references are appended. The microscope in this country is in many hands, and there is an increasing disposition to turn it to real scientific account;—for which this volume should be helpful. A. G.

2. *Bulletin of the California Academy of Sciences*, San Francisco.—The new departure made by the issue, in February, 1884, of the first number of this Bulletin, has been followed up with spirit by the publication, last February, of the still ampler No. 3, pp. 61–177, in direct continuation of No. 1. A No. 2, if it exists, is therefore out of pagination, and we believe was only a fly leaf. And now, in September, we receive No. 4, or at least the first part of it, continuing the volume to p. 228. The papers are all botanical, except two short ones by Dr. Behr on *Lepidoptera*. Dr. Harkness, still zealously devoted to the mycology of the Pacific coast, here gives us only a few pages, noting additional known Fungi and characterizing some new ones:—among them his *Lycoperdon sculptum*, well said to be "a curious and strikingly beautiful species," having a singular tuberculated cortex, the like of which has never been seen before. Plate I. gives a good representation of it.

Mrs. Curran, the efficient curator of the botanical collections at the Academy (which, happily, are at length being well cared for and in the way, as they should be, of steady augmentation), who published, as her first paper, three new species of Californian plants in the earlier part of the Bulletin (among them a second *Acanthomintha*, confirming the genus, with a difference), contributes another to No. 3, chiefly from her own discoveries. The most interesting one is her *Nemacladus rigidus*. She has also done excellent service—bibliographical and critical—by looking up all the extant materials of the various genera and species published, during a series of years, by the venerable Dr. Albert Kellogg in the several volumes of the Proceedings of the Californian Academy, as well as in some out-of-the-way and quite unscientific and ephemeral journals or newspapers,—comparing such specimens as could be found with the neat drawings which Dr. Kellogg delighted to make. These drawings are very much better than the rude reproductions of them which were given in the Proceedings, are more numerous, and are generally more helpful than the descriptions in the work of determination. The labor of looking up these scattered publications and of digesting the bibliography must have been very considerable. But Mrs. Curran, with what help she could obtain, has brought them all together in 23 pages of the Bulletin, with the needful references, appending the synonymous name, where there is any known to her. Botanists who have to do with this troublesome matter must heartily thank Mrs. Curran for this conscientious piece of work. Without this exemplification some botanists might have found it difficult to believe that Dr. Kellogg's *Linum trisepalum* is *Helianthemum scoparium*, his *Ludwigia scabriuscula* the *Ammannia latifolia*, his *Gnaphalium Nevadense* the *Antennaria dioica*, his *Egletes Californicus* the common *Bahia lanata*, alias *Eriophyllum cæspitosum*, and his *Heterocodon minimum* the *Alchemilla arvensis*, not "*Specularia biflora*." Also, that the new genera *Melarhiza*, *Partheniopsis*, *Tesseranthemum*, and *Ranapalus*, are founded respectively upon the *Wyethia helenioides*, *Venegazia*, *Frasera speciosa*, and *Herpestis rotundifolia*. It is helpful, also, to have in the Bulletin copies of the dozen plates, the greater part colored and of new Lower Californian plants, long ago prepared for the Hesperean, but we believe not published. If they did appear, along with the descriptions, in this "monthly magazine published in San Francisco in earlier years," it is unlikely that the botanical world knew or could have known anything of them. And the same must be said of "the columns of the San Francisco Rural Press,"—hardly a scientific vehicle.

In thus noticing, as it comes in our way, the botanical work of a scientific pioneer on the Pacific coast, we should not withhold our tribute of respect and admiration for this zealous, wholly disinterested, and simple-hearted lover of nature, who merely wished to do what he could for the advancement of our knowledge of the Californian flora, under conditions—such as the want of books and collections—which would not improperly have kept back almost any other equally ardent naturalist.

The ample remainder of the third number and the whole of the recent issue of the fourth consists of "Studies in the Botany of California and parts adjacent," by Edward Lee Greene. They show a quickness quite equal to the author's well known quickness and acuteness in observation. Besides the interesting new material here elaborated—much of it gathered in an enterprising expedition by boat to the islands of Lower California—there is a good deal of reconstruction of old species, a large number of new ones, and several new or restored genera of plants. Valuable as these contributions to our botany must be, we suppose that more time for elaboration, less confidence as to specific distinctions, and a more restrained judgment about genera might have made them better. Yet opinions will naturally differ in botany as well as upon other subjects. The present writer, for one, would not willingly found a genus upon an outlying plant which appears to differ from *Draba* only in its late-dehiscent or possibly indehiscent silicle, and another upon a wingless *Thysanocarpus* (which even Nuttall with all the loose ideas of his later years about genera had no thought of separating); still less would he have thought of a probable junction of these two proposed genera into one upon a "half-anticipation" of an unseen second ovule in the *Thysanocarpus*. Nor would he accede to the restoration of Nuttall's genus *Eucrypta*, nor readily believe that the genus *Eschscholtzia* comprises as many as ten definable species. As to *Mimulus*, although Mr. Greene's discovery that the capsule of *Diplacus* dehisces first and mainly by the upper suture certainly strengthens the claim of the latter to generic rank, there are no new reasons for reinstating *Eunanus*, nor for setting up *M. pilosus* (or *M. exilis*) as a genus. On going over the whole ground anew, with all the extant material, and with all the impartiality the present writer can muster, he still is of the opinion that *Mimulus* is best treated as a multiform genus.

On the other hand there cannot be a better genus than *Bebbia*, Greene (and our associate Mr. Bebb has well earned the honor); and it is not Mr. Greene's fault nor that of Dr. Cooper (who both long ago stated that *Carphephorus junceus*, Benth., had yellow flowers) that the genus had not already taken its place. If the writer was slow of belief, with only the dried specimens before him, he was at once convinced when he came upon this striking plant, full of golden bloom, in the Grand Cañon of the Colorado. Our idea of the affinity of this genus, however, is quite unlike that of Mr. Greene, and will in due time be recorded. Mr. Greene refers to a "sunflower-like odor," apparently of the herbage; but he makes no mention of what was to us a most attractive characteristic, namely, the delicious aroma, like that of *Acacia Farnesiana*, which its blossoms exhale. A. G.

3. *A Systematic Catalogue of the Flowering Plants and Ferns indigenous to or growing wild in Ceylon:* Compiled by Henry Trimen, M.B., F.L.S., Director of the Royal Botanic Gardens, Ceylon. Colombo, 1885. pp. 137, 8vo. Separately issued from the Journal of the Ceylon branch of the Royal Asiatic Society.—Mr. Trimen, the successor of the late Mr. Thwaites at the noted and charmingly situated establishment at Péradeniya, has set himself actively to the work of mastering the botany of Ceylon, and has now brought out this catalogue of 1071 genera, phænogamous and vascular cryptogamous, besides two of *Characeæ*, and

of their known Ceylonese species, with some synonyms. The naturalized species are enclosed in brackets; the endemic species, by a happy thought, are in small capitals, which at once catch the eye. A. G.

4. HON. GEORGE W. CLINTON, probably the oldest native botanist of this country, a man of remarkable personality and attractiveness, died at Albany—the city of his birth and boyhood—September 7, at the age of about 78. Some biographical notice of Judge Clinton, and of the late Charles Wright, may be expected in the January number of this Journal. A. G.

IV. ASTRONOMY.

1. *Identity of Denning's and Biela's comets.*—In the *Observatory* Mr. Denning and Captain Tupman discuss the question whether these two comets may not be the same body, and are led to believe their identity probable. The two comets have their line of nodes nearly coincident, the ascending node of the one being very nearly the descending node of the other. The earth is supposed to have thrown the comet from one orbit into the other, the radius vectors being made to equal that of the earth by minor disturbances of Jupiter.

If however the radiants for the two comets be compared they will be found to differ in position by a distance of 125° or 130°, and this distance is a measure of the necessary disturbing power of the earth in order to throw the comet from one orbit into the other. Either comet, it will be found by a simple computation, would have to come much nearer to the earth's center than 4000 miles in order to suffer such a perturbation of orbit.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Meeting of the American Association for the Advancement of Science at Ann Arbor, Michigan.**—The thirty-fourth meeting of the American Association for the Advancement of Science convened in Ann Arbor on Wednesday, August 26, and adjourned on Tuesday evening, September 1. The sessions were held in the halls and apartments of the University, which were found unusually commodious, being spacious, accessible and quiet, and the large University hall seating 3,000 persons. The total number of members registered during the meeting was 364, and the total number of new members elected was 153. A very large number of visitors attended the meeting in addition to the above lists. The total number of papers entered for the meeting was 179; and of these 176 were read, including one illustrated evening lecture. Of these papers there were read before the section of Mathematics and Astronomy, 12; before that of Physics, 23; that of Chemistry, 18; of Mechanical Science, 12; of Geology and Geography, 27; of Biology (which was made to include all paleontological

* The editors are indebted for this sketch to Professor A. Winchell, of Ann Arbor.

papers), 32; of Histology and Microscopy, 4; of Anthropology, 26; of Economic Science and Statistics, 21. The lecture was on Friday evening by Capt. E. L. Corthell, on "The Interoceanic Problem and its Scientific Solution," with lantern illustrations. The address of retiring President Lesley was delivered on Wednesday evening to a large audience in University Hall, the vice-presidential addresses having been mostly given during the afternoon.

The arrangements effected by the local committee appeared to have been perfect, and were the subject of universal commendation. All the usual preparations of rooms, blackboards, tables, sign-boards and the like were provided, and there was opened, next door to the Permanent Secretary's office, a post-office, express office, telegraph office and telephone office. On the same floor was the office of the committees on excursions and on transportation. The insufficiency of the hotel accommodations led to the opening of many private houses—not a few for both lodgings and board—and a common restaurant with a capacity of 300 was organized for such as desired it. Many citizens were also free entertainers. On Thursday evening a public reception was given at the Court House—a new fine building with imposing staircases—whose two stories and basement had been decorated with elaborateness and artistic taste, which won praise from the thousand guests in attendance. On Friday afternoon the city gave also an elegant lawn party on the campus. On Saturday 400 guests, invited by the liberality of Detroit, enjoyed, free of all expense, an excursion to Detroit and thence by the steamer Northwest up the Detroit River and Lake St. Clair, viewing the government improvements in the lake and the unique line of summer houses on the islands; and thence to Marine City. At this point are very extensive salt works, based on an enormous supply of rock-salt of the Salina Group, found here 1633 feet below the surface, and having a known thickness of over 115 feet. After an inspection of these works, for which every facility was provided by the proprietor, Mr. C. McElroy, the excursion proceeded to St. Clair, where an elegant dinner was served at the "Oakland House," a vast summer hotel, whose business is based on a mineral-well supplied from the Huron Group and rich in sulphur. Returning from here, a lunch was served on board, speeches of greeting were made and replied to, and a delightful day was ended with the return at 9 o'clock. A general excursion was also arranged for September 2d to Mackinac and thence in various directions. Entertainments of less general character were numerous. A reception was given to the geologists and many of their friends on Friday evening by Professor A. Winchell, and one to the chemists and their friends on Monday evening by Dr. Prescott. The botanists were taken by Professor Spaulding on an excursion to the Tamarack Swamp. Invitations to lunch and dinner were abundant; and carriage rides about the city and suburbs were quite general. The principal interest in these details is the demonstration that the Association can obtain ample conveniences and enjoyments in a city with a population of less than ten thousand. Indeed, Ann Arbor could have accommodated a thousand guests as easily as five hundred.

It was generally remarked that the scientific work of the meeting was good. An unusually large proportion of Fellows was present. Fewer papers than usual had to be rejected; and there were several papers of great and permanent importance, as will appear from the particular reports. The section of Histology and Microscopy was merged in that of Biology by request of the section itself. Resolutions were adopted expressing a high appreciation of the value of the work of the "Coast and Geodetic Survey," and a hope that criticisms of the work might be left by the government to competent scientific experts.

The subjects of the addresses before the several sections by the vice-presidents were as follows: Professor W. R. Nichols, to the Chemical section, on Chemistry in the service of public health; Professor Edward Orton, to the Geological section, on unfinished problems relating to the geology of coal; Professor J. Burkitt Webb, of Ithaca, to the Mechanical section, on the second law of Thermo-dynamics; Dr. B. G. Wilder, of Ithaca, to the Biological section, on an educational museum of Vertebrates; Professor S. H. Gage, of Ithaca, to the section of Microscopy and Histology, on the limitations and value of histological investigations; Mr. W. H. Dall, of Washington, to the Anthropological section, on the native tribes of Alaska; Mr. Edward Atkinson, of Boston, to the section of Economical Science, on the application of Science to the production and consumption of food. [Abstracts of these addresses, together with notes on many papers read at the meeting, are given in the number of Science for September 11 (No. 136) and the address entire of the retiring president, Professor Lesley, in the number for August 28.]

Buffalo, New York, was selected for the meeting of the Association in 1886—where the meetings of 1866 and 1876 were held—and Wednesday the 18th of August appointed for the opening session.

Professor Edward S. Morse, of Salem, Mass., was elected President, and the following for Vice-Presidents of the different sections: Professor J. W. Gibbs, of New Haven, Mathematics and Astronomy; Professor C. F. Brackett, of Princeton, Physics; H. W. Wiley, of Washington, Chemistry; O. Chanute, of Kansas City, Mechanical Science; Professor T. C. Chamberlin, of Washington, Geology and Geography; Professor H. P. Bowditch, of Boston, Biology; Horatio Hale, of Clinton, Ontario, Anthropology; Joseph Cummings, of Evanston, Ill., Economic Science and Statistics.

List of Papers accepted for Reading.

1. *Astronomy. Mathematics, Physics.*

H. A. Newton: Effect of small bodies passing near a planet upon the planet's velocity.

D. P. Todd: On a rare sun-spot, observed 1885, May 19, 21 and 22; The audible circle,—a new device whereby the settings of an astronomical instrument may be made by the ear.

W. Harkness: On the flexure of transit instruments.

G. W. Hough: Description of a printing chronograph.

J. Burkitt Webb: Polar versus other coördinates.

J. Haywood: The visible shadow of the earth.

S. P. Langley: Spectra of some sources of invisible radiation, and on the recognition of hitherto unmeasured wave-lengths.

J. A. Brashear: A practical method for working rock-salt surfaces for optical purposes.

J. W. Moore: The direct optical projection of electro-dynamic "lines of force;" The optical projection of electro-dynamic phenomena.

H. S. Carhart: On surface transmission of electrical discharges.

E. L. Nichols: A spectro-photometric analysis of the color of the sky; Chemical behavior of iron in the magnetic field.

H. E. Alvord: Telemetric aid to meteorological records.

W. Ferrel: Psychrometry.

C. H. Chandler: A new harmonograph.

T. F. Jewell: Apparent resistance of a body of air to a change of form under sudden compression.

H. W. Eaton: The relation of vanishing and permanent magnetism.

T. C. Mendenhall: Note on electrometers and atmospheric electricity.

A. E. Dolbear: On the contact theory of electricity; On an incandescent electric lamp for projection; On a new galvanic element.

A. J. Rogers: Electrolysis of salt of the alkalies and alkaline earths.

C. K. Wead: Exhibition of a combined spectro-photometer and ophthalmo-spectroscope.

C. J. Reed: Exhibition of an apparatus for demonstrating the laws of falling bodies.

C. H. Rockwell: Some practical results in determining time and latitude with the almucantar.

H. H. Clayton: Weather changes of long period.

T. Bassnett: Parallax of the sun.

S. S. Haight: Rapidity of calculation.

2. *Chemistry.*

T. Taylor: On the crystals of butter and other fats.

A. B. Prescott: Control analyses, and limits of recovery in chemical separations.

E. D. Campbell: A colorimetric method for the estimation of phosphorus in iron and steel.

W. A. Noyes: On para-nitro-benzoic-sulphinide.

W. H. Wiley: Estimation of acetic acid occurring with lactic acid in sour milk or *kumys;* Composition of *kumys* made from cow's milk; Honey and its adulterations.

E. H. Cowles, A. H. Cowles and C. F. Mabery: On the electrical furnace and the reduction of the oxides of boron, silicon, aluminium and other metals by carbon.

W. A. Withers: Chemical conversion scales.

H. B. Battle and F. B. Dancy: Consideration of the atomic weights of some of the elements found in agricultural analysis, and their application to conversion tables to facilitate calculation.

Note on the estimation of water in glucoses, honeys, etc.

Helen C. DeS. Abbott: A chemical study of Yucca angustifolia.

A. V. E. Young: Thermo-chemical analysis of the reaction between potassic hydrate and common alum.

F. P. Dunnington: A sponge-like mass containing titanic acid; A simple method of fixing crayon drawings on paper.

J. W. Langley: On the concentration of certain acid radicles by the differential action of chemism.

O. C. Johnson: Negative bonds and rule for balancing equations.

J. W. Pike: Chemical and thermo-chemical relations of the gases of the atmosphere in the disintegration and metamorphism of rocks.

C. J. Reed: Graphical representation of the relation between valence and Mendelejeff's periodic law.

C. G. Wheeler: Action of carbonic anhydride in aqueous solution on some minerals, and modifying influence of sodium chloride.

3. *Mechanical Science.*

L. S. Randolph: Strength of stay-bolts in boilers.
D. P. Todd: On a universal form of pressure motor.
S. S. Haight: Use and value of accurate standards for surveyors' chains.
J. Burkitt Webb: The lathe as an instrument of precision; The economy of accurate standards.
C. J. H. Woodbury: Experiments upon the coefficient of efflux of automatic sprinklers.
F. C. Wagner: Electric light tests.
M. E. Cooley: Testing indicator springs; A new smoke-burning device.
R. H. Thurston: Cylinder condensation in steam engines; A Prony brake for governing powerful steam engines.
A. Hogg: Deep water at Galveston, Texas, and how to secure it.

4. *Geology, Paleontology, Mineralogy.*

A. Winchell: Geology of Ann Arbor; *Cœnostroma* and *Idiostroma*, and the comprehensive character of Stromatoporoids; Sources of trend and *crustal surplusage* in mountain structure.
W. B. Taylor: A probable cause of the shrinkage of the earth's crust.
S. G. Williams: Westward extension of rocks of the Lower Helderberg period in New York.
J. D. Dana: Lower Silurian fossils in a limestone of the original Taconic.
L. E. Hicks: Structure and relations of the Dakota Group.
F. D. Chester: Results from a study of the gabbros and associated amphibolites in Delaware.
A. D. Crandall: Occurrence of trap rock in eastern Kentucky.
A. H. Worthen: The Quaternary deposits of Illinois.
G. K. Gilbert: Post-glacial changes of level in the basin of Lake Ontario.
H. S. Williams: Classification of the Upper Devonian.
E. Orton: The gas and oil wells of northwestern Ohio; Record of the deep well of the Cleveland Rolling Mill Co., Cleveland, O.
W. McAdams: The Lœss and drift-clays.
W. B. Dwight: Discovery of fossiliferous Potsdam limestone at Poughkeepsie, N. Y.
E. W. Claypole: The materials of the Appalachians.
T. Sterry Hunt: Apatite deposits in Laurentian rocks.
J. C. Branner: Glaciation of the Lackawanna Valley.
C. Wachsmuth: The presence or absence of underbasals can be ascertained from the columns.
E. D. Cope: On the brain and auditory organs of a Permian Theromorph Saurian.
A. S. Tiffany: The Corniferous or Upper Helderberg Group of Scott County, Iowa and Rock Island, Ill., with a list of fossils; The Chemung Group at Burlington, Iowa. with a list of its fossils.
N. H. Winchell: Notice of Lingula and Paradoxides from the red quartzites of Minnesota.
G. F. Kunz: A new mass of meteoric iron from Charleston, Kanawha County, West Virginia; Mineralogical notes: Tourmaline locality at Rumford, Maine; Pseudomorph of feldspar after leucite (?) from Magnet Cove; On a remarkable rough collection of diamonds; Native antimony and its associations at Prince William, New Brunswick.

5. *Botany, Zoology, Microscopy.*

E. Sturtevant: Observation on the hybridization and cross-fertilization of plants; Germination studies.
C. E. Bessey: Further observations on the adventitious inflorescence of *Cuscuta glomerata;* Question of bisexuality in the pond-scums (*Zignemaceœ*).
C. R. Barnes: Process in fertilization in *Campanula Americana.*

H. G. BERGER: Biological deductions from a comparative study of the influence of Cocaine and Atropine on the organs of circulation; Structure of *Glattidea pyramidata* (Stien) Dall.

C. V. RILEY: The song-notes of the periodical Cicada (*Cicada septendecim* L.), and the mechanism by which they are produced; Some popular fallacies and some new facts regarding *Cicada septendecim;* On the parasites of the Hessian-fly (*Cecidomyia destructor* Say).

A. W. BUTLER: The periodical Cicada in southeastern Indiana; Observations on the musk-rat.

J. C. ARTHUR: Proof that Bacteria are the direct cause of the disease in trees known as pear-blight.

S. H. GAGE and S. S. PHELPS GAGE: Aquatic respiration in soft-shelled turtles (*Aspidonectes* and *Amyda*).

E. D. COPE: Phylogeny of the placental Mammals.

T. J. BURRILL: The mechanical injury to trees by cold.

J. M. COULTER: On the appearance of the relation of ovary and perianth in the development of dicotyledons.

C. S. MINOT: A new membrane of the human skin; Organization and death; Morphology of the supra-renal capsules; The structure of the human placenta; Evolution of the lungs.

D. H. CAMPBELL: The development of the prothallium of ferns.

J. B. STEERE: The importance of individual facts of environment in the formation of groups of animals.

B. G. WILDER: Experiments antagonizing the view that the serrulæ (serrated appendages) of *Amia* are accessory organs.

H. AYERS: On the structure and functions of Sphæridia of the *Echinidii;* On the carapax and sternum of the Decapod Crustacea.

W. G. FARLOW: Notes on some injurious Fungi of California.

D. E. SALMON and T. SMITH: A new Chromogenous Bacillus (*Bacillus luteus suis*).

J. B. WEBB: Entropy; The life of the universe.

W. H. WALMSLEY: Photo-micrographs on gelatine plates for lantern projection.

T. J. BURRILL: Photo-micography work with high powers.

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2. *U. S. Coast and Geodetic Survey.*—The resolutions with regard to the United States Coast Survey, passed at the recent meeting of the American Association at Ann Arbor, without a dissenting voice, are in accord, we believe, with the views of scientists throughout the country. The survey has had great influence in promoting the progress of high science in the land through its demand for the best abilities in the departments of mathematics, physics, hydrography and geodesy, in order to carry forward its work, and through the investigations it has been compelled to undertake for the improvement of its methods, the elaboration of its observations and the perfecting of its results. No department of work under the government requires greater exactness and a wider and profounder range of knowledge. For thirty years and more, commencing under Professor Bache, its annual reports have contained, not only charts of hydrographic work in great numbers, but also papers of high scientific merit bearing on the various questions arising out of the investigations in progress, and others fundamental to those investigations; and this has continued to be true to the latest issue under Mr. Hilgard. The work of the Coast Survey, as outlined by Bache and carried forward by his successors, has a unity and a completeness which should be preserved in its future scope and management. Besides elevating the science of the country, it has tended also to exalt in foreign lands the standing of American science. Any crippling of the Survey in the present unfinished stage of its work would therefore be a national calamity.

The demand, in the vote of the Association, that the head of the Coast Survey (and by inference the superintendents of other scientific work) should have the highest possible standing among scientific men, and should command their entire confidence, is in accordance with the "civil service" principles of the country. Sure destruction to the usefulness and reputation of the scientific departments—for example, those of the Coast Survey, the Smithsonian Institution and the Geological Survey—would follow their subjection to the control of persons without thorough scientific education, for it would be quite sure to end in subjection to the debasing influences of political ambition.

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And other objects of NATURAL HISTORY.

A. E. FOOTE, M. D.,

No. 1223 Belmont Avenue, Philadelphia, Penna.

(Professor of Chemistry and Mineralogy; Fellow of the American Association for the Advancement of Science; Life Member of the Academy of Nat. Sciences, Phila., and American Museum of Nat. History, Central Park, N. Y. City.)

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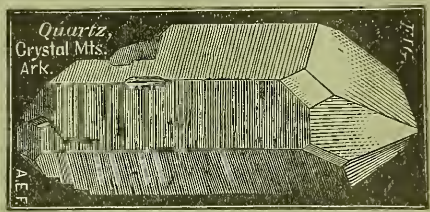

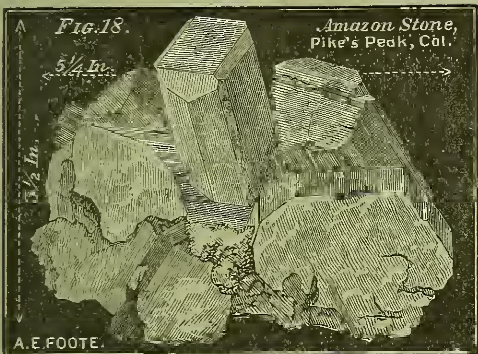

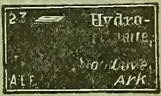

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SCIENTIFIC INTELLIGENCE.

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An *extra number* of this Journal, containing an index to volumes xxi to xxx, of sixty pages or more, will be ready in January. The publication of this number involves a large extra expense to the editors, and it will be sent, therefore, to those only who specially order it. The price is seventy-five cents per copy. Orders are solicited, as the edition will be a small one.

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[THIRD SERIES.]

Art. XLIII.—*The Quantitative determination of Niobium;* by T. B. Osborne.

Niobium occurs in nature almost always in connection with tantalum and titanium, in the presence of which it has been heretofore impossible to obtain more than an approximate determination on account of the close analogy of the behavior of these three elements, when occurring together, toward reagents.

Marignac proposed in 1866 (Arch. de Sc., xxv, p. 17) the process now generally employed for the determination of niobium, but as he says in describing his method, only approximate results can be obtained by its use. His method depends on the difference in solubility of the potassium fluorine salts of tantalum, niobium and titanium. Tantalum forms a salt of the composition $TaF_5 2KF$ dissolving at ordinary temperatures in 150–200 parts of water acidulated with hydrofluoric acid and crystallizing in fine needles. Niobium on the other hand forms a salt of the composition $NbOF_3 2KFH_2O$, dissolving in twelve parts of water and crystallizing in scales isomorphous with the corresponding titanium salt. The potassium titanium fluoride dissolves in ninety-six parts of water.

Marignac adds bifluoride of potassium to the solution of the fluorides of tantalum, niobium and titanium, and concentrates till scales of $TiF_5 2KF$ or $NbOF_3 2KF$ appear. The $TaF_5 2KF$ is filtered and washed with the aid of a pump until the washings no longer give any orange red precipitate with solution of galls after standing two hours. On account of the solubility of the $TaF_5 2KF$ the mother liquor containing the $NbOF_3 2KF$ must always be a saturated solution of $TaF_5 2KF$ and likewise a loss must occur on washing, so that even if the process of crystallization be several times repeated, as is usually necessary, a very considerable loss must occur. Moreover, the tantalum potassium fluoride cannot be entirely freed from niobium in this way, for I found in preparing TaF_62KF that the niobium cannot be entirely removed by simply crystallizing and washing, as I was compelled to recrystallize several times before the TaF_62KF gave no evidence of the presence of niobium, when tested by reducing with zinc and hydrochloric acid. Titanium will, when present in any considerable quantity, render the application of this method still more uncertain on account of the intermediate solubility of the potassium titanium fluoride and its close resemblance to the $NbOF_32KF$.

From this description it will readily be seen that this method is open to serious objection and can only find acceptance in want of a better. I therefore undertook an investigation of the reduction with zinc in acid solutions containing niobium with the hope of discovering a more accurate and less difficult method of determination. The salts of tantalum and niobium I prepared from columbite from Branchville, Connecticut, in the following way: The mineral was ground to pass a sieve of 100 meshes to the inch, and 500 grams fused with bisulphate of potassium in a platinum dish, 100 grams at a time. When thoroughly fused the mass was immediately poured into cold water which caused the separated tantalic and niobic acids to granulate and expose a large surface to the action of the water, whereby the solution of the bisulphate of potassium was very materially promoted and the lumps broke up easily on stirring, the tantalic and niobic acids separating partly in a flocculent and partly in a granular form.

After washing by decantation the residue was treated with ammonium sulphide and then filtered and washed to remove any tin or tungsten which might be present. Hydrochloric acid was then added and after washing, the residue was obtained free from iron and manganese. The residue was next treated with hydrofluoric and sulphuric acids, in order to remove any silica, precipitated with ammonia and washed free from sulphates. The tantalic and niobic acids thus obtained were dissolved in an excess of hot hydrofluoric acid and carbonate of potassium added gradually, and after cooling, the TaF_62KF filtered off. The crude TaF_62KF was recrystallized several times and washed till no reduction whatever took place on testing with zinc and hydrochloric acid. The mother liquor from the crude TaF_62KF was then nearly neutralized with carbonate of potassium which caused the solution to nearly solidify from the formation of $NbOF_62KF$. This product was then recrystallized several times and tested for tantalum by boiling with pure water, when, according to Marignac, if the slightest trace of tantalum were present, it would separate as an oxyfluoride. None was found. Analyses of the $NbOF_3 2KFH_2O$ thus obtained gave the following results.

			Calculated.
H_2O	5·92	5·90	5·97
NbO	36·92	36·84	36·87
K_2	26·07	26·13	25·97
F_5	31·09	31·13	31·19
Ti	trace	trace	----
	100·00	100·00	100·00

This analysis shows that the salt was practically pure. The trace of titanium was found by testing with H_2O_2 and amounted to only ·0066 per cent.

Tantalum when treated with zinc and hydrochloric acid does not reduce. Niobium on the other hand forms apparently several reduction products or at least exhibits colors varying according to treatment. When zinc is added to a dilute hydrochloric acid solution of niobium, a blue color is formed in the cold; if the acid is strong a brown color is obtained which, if the acid is allowed to act on the zinc until nearly neutralized, changes to blue and an indigo blue precipitate separates out. When heated to 100° only quite dilute acid solutions give a blue color which speedily turn brown. Those containing more acid give a brown color at once. If sulphuric acid is used instead of hydrochloric, the blue color appears at first, but on heating with much acid this passes into a brown differing slightly in color from that produced in the hydrochloric acid solution. A grass-green color is often obtained in the cold, with both sulphuric and hydrochloric acids, which appears to be intermediate between the blue and the brown as the blue passes into green and this into brown. All solutions strongly acidified with sulphuric or hydrochloric acid give on heating a dark brown color, which evidently indicates the lowest reduction attainable, while the blue color marks a higher oxide than the brown or else an incomplete reduction.

The solution reduced with sulphuric acid and zinc is much less stable than that with hydrochloric acid, for on pouring it while warm into water, if the amount of reduced niobium is sufficient, an evolution of hydrogen takes place from the decomposition of the water. With hydrofluoric acid and zinc a violet color is produced similar to that given by titanium with zinc and sulphuric acid. Under similar conditions titanium gives a green color.

The first attempts at a quantitative determination of niobium were made with sulphuric acid solutions of $NbOF_3 2KF$, as sulphuric acid is better suited for titration with potassium permanganate than hydrochloric. It was found, however, that the amount of reduced niobium was very far from constant. More constant results were obtained by pouring the reduced solution into ferric sulphate, but these varied too widely to give any promise of a satisfactory method. Hydrochloric acid was then tried and iodine solution added till a drop gave a blue color with a drop of starch solution, then sodium thiosulphate solution in slight excess, then a few drops of starch solution and iodine added till a blue color appeared. It was found, however, that a dark brownish color was formed near the end of the titration which so obscured the blue of the starch that very unsatisfactory results were obtained.

I then tried potassium dichromate for titrating, pouring the reduced solution into ferric chloride but without success.

Potassium permanganate was then returned to and manganese sulphate or magnesium sulphate added to prevent the action of the hydrochloric acid on the permanganate. It was afterward found that by sufficient dilution the end point could be obtained with all the accuracy desirable without the addition of either magnesium or manganese sulphates. The results are shown in the following table. The reduction was assumed to be to Nb_2O_3.

Amount of acid used.	Amount of $NbOF_32KFH_2O$.	Time of reduction.	Percentage of Nb_2O_5.
50 c. c. HCl sp. gr. 1·1	·5155	¾ hour	34·24
"	·4652	"	35.21
"	·4932	"	35·58
"	·5900	"	34·90
"	·5561	"	34·50
"	·6614	"	35·24
"	·4926	1½ hour	34 01
"	·5896	"	34·60
40 c. c. "	·4982	¾ hour	34·27
"	·4161	"	34·13
30 c. c. "	·5291	½ hour	33 56
50 c. c. " (conc.)	·5219	¾ hour	37·68
"	·8122	"	38·41
"	·4256	"	36·74
"	·7152	"	37·65
25 c. c. " (conc.) (Contained about ·5 gr. TaF_52KF.)	·4421	"	36·97
"	·5162	"	37·54
"	·3910	"	37·40
"	1·0592	"	38·07
"	·8890	1 hour	37·56
"	·9274	"	35·90
"	·6734	¾ hour	38·58
"	·6861	"	38·78
"	·5980	"	38·30
"	·5828	"	38·70
"	·7611	"	37·68
"	·6530	"	38·07
In the above analyses the fluorides were dissolved in water and the acid added. In the following concentrated acid was added directly to the fluorides	1·1023	"	39·03
	·8395	"	38·86
The amount of Nb_2O_5 corresponding to the niobium in the $NbOF_32KFH_2O$ is			44·48

The foregoing series of determinations show that the reduction of the niobium is quite constant for a given strength of acid depending not on its amount but on its strength. The determinations were made by dissolving $NbOF_3 2KF$ in water and adding the indicated amount of acid. The first eight determinations, where 50 c. c. of dilute acid were used, give an average of 34·78. With 40 c. c. the same amount of water was used to dissolve the fluoride, the strength was consequently less and the average lower, being 34·20. With 30 c. c. 33·56 was found. When concentrated acid was used the amount of water required to dissolve the fluorides was small in comparison with the amount of the acid in solution, so that the amount of water used exerted but little influence on the reduction, but the increased strength of acid raised the average to 37·80. The results obtained when concentrated acid was used to dissolve the fluoride are higher still, the average being 38·94.

Thus we have

For 30 c. c. HCl sp. gr. 1·1	33·56	per cent	Nb_2O_5
40 c. c. " "	34·20	"	"
50 c. c. " "	34·78	"	"
Concentrated HCl	37·80	"	"
Fluorides dissolved in conc. HCl	38·94	"	"

It appears therefore that the amount of reduction increases with the concentration of the acid. The formula for an oxide corresponding to the reduction in the last case would be Nb_8O_{13}. Marignac sought to obtain a formula for this oxide in the same way and obtained Nb_3O_5. This is very nearly equal to Nb_8O_{13} since, for eight atoms of niobium there would be thirteen and a third atoms of oxygen according to his formula. It is probable that neither of these oxides represents the oxide formed, but that a partial reduction has taken place as will appear from the results obtained from niobic acid in the following manner. Ten grams of $NbOF_3 2KF$ and five grams of $TaF_5 2KF$ were accurately weighed and heated with sulphuric acid in a platinum dish till all the hydrofluoric acid was removed. The acid solution was poured into water and precipitated with ammonia, washed by decantation and then thoroughly dried at 100° C. Weighed portions were then ignited and the total amount of mixed oxides of tantalum and niobium determined. The percentage of niobic oxide in these mixed oxides was calculated from the relative amount of the fluorides of each metal taken. In this way it was found that the mixture of tantalic and niobic acid precipitated and dried at 100° contained 47·36 per cent of Nb_2O_5. Weighed portions were then dissolved in as small an amount of hydrofluoric acid as possible and 50 c. c. of concentrated acid added and the reduction carried on at a temperature of about 80° C. for three quarters of an hour. The amounts obtained were as follows:

Am't of mixed oxides taken.		Amount of Nb_2O_5 found.
·6400		46·99
·6652	Nb_2O_5 present	47·45
·7455		46·70
·7663	47·36 per cent.	46·64
·8323		46·97

In the above determinations the reduction was evidently to Nb_2O_3. Marignac states that alkali fluorides impede the reduction, and this would explain why the reduction in the foregoing determinations was complete while in those where the potassium fluoride of niobium was used it was incomplete; as the amount of alkali fluoride was always in the same proportion to the amount of tantalum and niobium constant results might be expected, but it is remarkable that so small an amount of alkali fluoride should exert so great an influence on the reduction, and that the hydrofluoric acid added to dissolve the niobic acid did not also interfere with the reduction. Two analyses were made in order to throw light on this point by adding weighed amounts of potassium fluoride to a mixture of tantalic and niobic acids free from fluorides and containing 45·99 per cent of Nb_2O_5.

Weight of mixed acids.	Weight of KF.	Nb_2O_5 found.	Nb_2O_5 present.
5696	·0900	43·83	45·99
5405	·3728	43·87	

From this it would appear that the *amount* of alkali fluoride exerted no influence, but that its *presence* did. In this case, however, the reduction is proportionally greater than when $NbOF_3 2KF$ was used.

The influence of titanium was next studied. Titanic acid was treated exactly as the niobic acid and the following results were obtained.

TiO_2 found by reduction and titration.		Present.
20·38	20·36	20·48

In order to determine whether both niobium and titanium would reduce together as well as separately, the oxides were mixed together and the niobium determined by deducting from the amount of permanganate used the amount necessary to oxidize all the reduced titanium and calculating the niobium corresponding to the balance. In this way I obtained the amounts given below.

Mixture of three acids.	Titanic acid present.	Nb_2O_5 present.	Nb_2O_5 found.
1·1798 gr.	·0859 gr.	·4928 gr.	·4907
·8145	·0687	·3357	·3314

For the determination of titanium in the presence of niobium the method proposed by A. Weller (Berichte, xv, 1882, 2, p. 2592), appeared most suitable. It depends upon a comparison of the color produced by hydrogen dioxide in an acid solution of titanic acid, with the color of a solution containing a known amount of titanium. A. Weller, in describing his method, calls attention to its applicability to the determination of titanium in the presence of tantalum and niobium, but says he has not tried it in this case. I therefore undertook to determine the best manner of applying it. I found that it was necessary to use hydrofluoric or hydrochloric acid in order to keep the tantalum and niobium in solution when sufficiently diluted. I found that the first of these acids even in small amounts entirely prevented the formation of the color of titanium with hydrogen dioxide and that hydrochloric acid deepened it very greatly in proportion to the amount added. It is necessary, therefore, to have the solution free from fluorides, and to have the same amount of acid in the standard solution as in the solution to be analyzed. It is very difficult to obtain these conditions and I was entirely unsuccessful in my attempts to determine the amount of titanium in a solution containing much free acid. I succeeded in obtaining quite satisfactory results, however, by precipitating the strongly acid solution with a small excess of ammonia and then redissolving the precipitate with as little sulphuric acid as possible, the freshly precipitated niobic and tantalic acids being readily soluble in dilute acid.

In order to test the accuracy of this method a mixture of tantalic, niobic and titanic acids containing a known amount of each acid was dissolved in hydrofluoric acid in a platinum dish and the excess of acid evaporated off on the water bath. The fluorides thus obtained were dissolved in concentrated hydrochloric acid and poured into a glass flask of about 100 c. c. capacity, the platinum dish being rinsed out with concentrated hydrochloric acid. The solution and rinsings amounted to 50 c. c. Amalgamated zinc was then added and a piece of platinum and the solution allowed to reduce in a stream of carbonic acid for three-quarters of an hour at a temperature of about 80° C. During this process the greater part of the hydrofluoric acid was removed from the solution, going off as silicon fluoride. After cooling thoroughly the reduced solution was poured into a beaker and diluted to 350 c. c. with distilled water freshly boiled and perfectly cold. A standard solution of potassium permanganate was added till the solution, at first nearly black, became perfectly clear and a distinct pink color was produced by the addition of a single drop. To the solution containing the tantalum, niobium and titanium, ammonia was added in slight excess and then sulphuric acid till the precipitate produced by the ammonia dissolved entirely. In this way a solution was obtained containing a very small amount of free acid and practically free from fluorides. This solution was then diluted to 500 c. c., and 50 c. c. brought into a nesslerizing tube and 2 c. c. of hydrogen dioxide added. About 40 c. c. of water was added to another tube of the same dimensions and then 2 c. c. of hydrogen dioxide. A solution of titanic acid, prepared by evaporating potassium titanium fluoride with excess of sulphuric acid and diluting with water until 1 c. c. equaled one milligram of titanium oxide, was then run in until the color in the two tubes was nearly alike. They were then brought to the same volume and the standard solution of titanium added drop by drop till there was no longer any difference in color discernible. The number of cubic centimeters of the standard solution multiplied by ten gave the number of milligrams of TiO_2 in the solution analyzed. By calculating the amount of permanganate solution necessary to oxidize the Ti_2O_3 formed by reduction of this amount of TiO_2 the amount of permanganate employed to oxidize the niobium was found and from this the amount of niobium calculated. The tantalum was found by subtracting the sum of the niobic and titanic oxides from the sum of the three oxides. In this way the following amounts of each were found.

	Nb_2O_5	Ta_2O_5	TiO_2
Taken	·3357 gr.	·2246 gr.	·0687 gr.
Found	·3314 "	·2289 "	·0667 "

It is necessary that during the reduction the heat should not be too great, as the tantalic acid is liable to precipitate and to carry with it both niobium and titanium and the reduction consequently will be incomplete. If tantalum is not present the solution can be boiled without danger. In order to analyze a mineral containing these three elements the mixed oxides can be obtained in the usual manner by fusing the finely ground mineral with bisulphate of potassium, digesting with water, filtering, heating the residue with ammonium sulphide, washing to remove tin and tungsten if present, treating the residue with dilute sulphuric acid to remove iron, washing and igniting with ammonium carbonate. After obtaining the joint weights of the oxides, fuse with potassium bisulphate, digest with water, and the residue, washed free from sulphates, dissolve in hot hydrofluoric acid, evaporate nearly dry on the water bath and proceed with the reduction and determination as just described. An analysis of Branchville columbite by this method, gave the following results:

Ta_2O_5	18·95	19·44	Specific gravity 5·73
Nb_2O_5	60·95	60·46	
FeO	12·86	12·95	
MnO	7·07	7·00	Oxygen ratio 1: 1·03
	99·83	99·85	

Marignac constructed a table of specific gravities of columbite which showed that in general the specific gravity increases with the amount of tantalum. The relations which he found hold good for this analysis.

ART. XLIV.—*Notes on the Surface Geology of the country bordering the Northern Pacific Railroad;* by J. S. NEWBERRY.

FROM Chicago through Wisconsin and Minnesota the Northern Pacific Railroad passes over an almost unbroken sheet of drift, which, though of great interest, has been so fully described in the able reports of Messrs. Chamberlin, Winchell and Upham, that nothing further need be said here in regard to it.

Going west from Duluth to Brainerd the line of the road for the most part lies in what is evidently the old deserted bed of a westward extension of Lake Superior. The ground is still low and swampy and much of the surface is formed of what is unmistakably lake sand.

At various points farther west true till is seen with its striated pebbles; and one such exposure at Audubon is within reach of every traveler. Beyond this, bowlders scattered over the surface and pebbles in the ditches continue as evidence of the transport of material from the eastern highlands. About Bismarck the bowlders though fewer are still not rare and are gathered in groups and trails, as elsewhere along the margin of the drift area, suggesting transport by ice floats. The last of these bowlders is seen at Sims, about twenty miles from Bismarck. From this point to the crossing of the Little Missouri one can hardly find a stone to throw at a bird or a shrub big enough for a tooth pick. This is an extension northward of that broad prairie area which I have crossed in many places farther south. Here, between the eastern drift and that from the Rocky Mountains, the soil is formed entirely by the decomposition of the underlying rocks, and wherever these are shales and calcareous sandstones, as they are throughout most of the Cretaceous formation, there are no outcropping ledges of rock; the country is smooth and stones of all kinds are scarce. This belt, which runs from the Mexican to the Canadian line, is prairie because of the dryness of the climate and not on account of the soil or the geological substructure; for between the "Cross timbers" and the Raton Mountains with a considerable variety of geology and topography, there are no trees except along the water courses, which, fed by the melting of the snows on the Rocky Mountains, are perennial and supply constantly the amount of moisture that is a necessity for tree growth.

The peculiar fineness of the soil of the northern portion of this belt has been supposed to have something to do with the prevalence of grass and the absence of trees, since in Illinois and Wisconsin, along the border line between the forest and the prairie, the levels where the soil is fine are grass-covered, while the swells and ridges, rocky or gravelly, carry trees; but as I have shown elsewhere, these local peculiarities of the soil, favoring, the first grass and the second trees, have to some extent caused the observed interlocking of prairie and forest. Farther west, with every kind of soil and geological structure, there are no trees, but everywhere grass, while east of the Mississippi and beyond the battle ground between the two forms of vegetation, all kinds of topography, soil and substructure are covered with forest. No one who has traversed the continent along several parallels of latitude and has studied the relations of vegetation to soil and geological structure will fail to find conclusive evidence that the influence which has determined the kind and quantity of vegetation in the varied topographic and climatic districts of the west is the rainfall.

The valley of the Little Missouri is deeply cut in a table land composed of the Laramie coal-measures, of which 200 or 300 feet with several seams of coal are exposed in the cliffs. Thousands of silicified tree-trunks are scattered over the surface and innumerable stumps are apparently standing where they grew, but no foreign material is anywhere visible. A few miles below the railroad crossing the valley expands and opens into the famous *Mauvaises Terres*, or "Bad lands of the Missouri." The course of that stream being here nearly east and west and the valleys of the tributaries north and south, these coalesce and form in the old lake beds picturesque but dangerous labyrinths.

As soon as one enters the valley of the Yellowstone he finds himself surrounded by transported material. Gravel and bowlders of crystalline, sedimentary and volcanic rocks form the bed and bars of the river, increasing in coarseness and quantity to Livingston, but in all this material I was unable to find anything that was to me even presumably of eastern origin.

Dr. C. A. White (this Journal, vol. xxv, 1883, p. 206) reports finding what he considers eastern glacial drift along the valley of the Missouri and that of the Yellowstone, but my search for such material was vain.*

The geology of the Yellowstone Park has been well described by Dr. Hayden and his assistants, Mr. W. H. Holmes and Mr. A. C. Peale, but I was surprised to find the traces of glacial action so widespread and unmistakable. It is probably not too

* As will be seen farther on, I found in the valley of the Missouri about the falls great quantities of drift with bowlders of fossiliferous limestone, quartzite, gneiss and granite, all remarkably like the eastern drift, but which I subsequently traced to their place of origin in the Belt mountains.

much to say that every valley of the Park was once filled with ice; for moraines, bowlders, glacial lakes, and more rarely glacial striæ give testimony on this question that cannot be disputed. Ice-borne blocks are seen on the sides of the Yellowstone valley below the mouth of Gardner's River, and south of Mammoth Hot Springs every depression has once held a glacier. Swan Lake is of glacial origin and is bounded on the south by a terminal moraine, while lateral moraines and striated rock surfaces mark the old ice level high up on the sides of the valley. Near Marshall's the road leads over a succession of great moraines of clay and bowlders which continue to and around the Fire Hole basin, and prove that this also was once largely filled with ice. From all I could learn the evidences of glacial action which are found here in the lowest portion of the Park may be traced through all parts of it.

DRIFT OF THE UPPER MISSOURI.

The Missouri River, formed at Gallatin City by the union of the Madison, the Gallatin and the Jefferson, traverses with a northwest and then northerly course the valley between the Rocky and Belt Mountains, and finds its way out to the plains by a long circuit around the northern bases of the Belt and Crazy Mountains, eastern outliers of the Rocky Mountain system. Cutting through barriers formed by interlocking spurs at the "Gate of the Mountains," the river enters an undulating prairie country which extends from the north side of the Belt Mountains to and beyond the Canadian line. All this region is occupied by a sheet of drift that in thickness and extent rivals that of the plains surrounding the Canadian highlands; but, as far as my observation extended, I found this of local origin.

At the Great Falls of the Missouri the underlying rock is exposed, but the drift-sheet comes up to the edge of the gorge and forms the low hills which stretch away to the east and north like the long swells of the ocean. In the valleys of the streams which come down to the Missouri from the Belt Mountains, the rock substratum is generally visible; but the intervening plateaus are covered with a sheet of drift that varies greatly in thickness as it is spread over a rock surface that was once deeply and irregularly eroded. For example, near the Upper Falls of the Missouri, where the banks of the river are solid rock and perhaps a hundred feet high, a tributary coming in from the south cuts across an old valley filled with drift, which extends almost to the present river channel. At its mouth this tributary has high rocky banks, but a few hundred yards above they are altogether composed of drift. This is a true till, thickly set with bowlders, some of which are two feet or more in diameter.

The bowlders are usually rounded, sometimes subangular, and are composed of gray or red granite, quartzite, Paleozoic limestone and a variety of eruptive rocks. The resemblance to the drift from the Canadian highlands is so great that I was only convinced of its local origin when I found all of its constituents in place in the Belt and Rocky Mountains. The granites were to my eye indistinguishable from those of the eastern Laurentian series. As I subsequently learned, they are of Archæan age, and nothing but careful microscopic examination will show them to be distinguishable, if they are so.

These facts lead me to suspect that the very careful and experienced observers who have reported the finding of eastern Laurentian bowlders on the flanks of the Rocky Mountains, 4000 feet above the sea may have been misled by this striking resemblance.

On the undulating surface of the table lands between the tributaries of the Missouri, large bowlders are occasionally seen, as in the States bordering the Great Lakes, and we passed one of these somewhat angular in form which had served so long as a rubbing-post for the buffaloes, recently abundant in this region, that its sides are all polished, and a deep furrow is worn around it by their feet.

THE GORGE OF THE COLUMBIA.

The gorge of the Columbia is one of the most impressive and interesting topographical features in all the picturesque West. It is cut with a nearly straight westerly course across the whole breadth of the Cascade Mountains, fifty miles, and its banks rise from 2,000 to 4,000 feet directly from the river side. Most of the material of which the walls are composed is basalt. This can be seen to form distinct layers, the products of different overflows from the great volcanic vents north and south of it. Cape Horn, a bold headland, shows a vertical face of trap nearly 500 feet in height.

No one who examines the gorge of the Columbia will fail to be convinced that it has been cut by the river. The general altitude of the mountains in which there are no other passes lower than about 5,000 feet, as well as the altitude of the lake deposits on the eastern side indicate that the work of cutting this channel began at a height not less than 3,000 feet above the sea. At this time the river must have had a fall of at least this number of feet into the valley of the Willamette; and to realize the conditions then existing, we must picture to ourselves a series of cascades of greater magnitude and more picturesque than any now known. This water-power was, however, busily engaged in cutting down the barrier, and in process of time it was so completely removed that a navigable canal was opened from the Dalles to the ocean. The

Western entrance to the gorge is now at tide-level and the lower part of the river is, like the Hudson, an arm of the sea. It is true that at present the "Cascades of the Columbia," form a serious interruption to the navigation of the river, for they are produced by a dam sixty-three feet high, which fills the channel for three miles. But this dam is of recent date, as we know, and has been caused by an avalanche from the sides of the gorge. Above it the river is simply a long lake, and in low water a series of stumps are seen coming up from below the water-level which belonged to trees that could never have grown in the places they occupy if the barrier of the Cascades had existed.

Steamboats navigate the Columbia from the Dalles down, with a transfer at the Cascades, and this is much the better route to take for those who would get a good view of the gorge with its imposing walls, its hanging forests and its picturesque waterfalls which leap 1,000 feet from the cliffs, to say nothing of the old Indian burial grove, and the multitude of silicified tree trunks at the Cascades.

The railroad is built along the face of the southern cliff, high above the water, and although it gives only a one-sided view of the gorge, it is generally chosen by travelers who prefer rapid transit to beauty of scenery.

ANCIENT GLACIERS OF THE CASCADE MOUNTAINS.

As is well known, the Rocky Mountains from New Mexico to British Columbia abound in evidences of ancient glaciation. The same is true of the Uinta Mountains, the Wasatch, the Sierra Nevada and Cascade Mountains. In the group of five snowy peaks called in Oregon the Three Sisters—because only three are visible from the Willamette valley, miniature glaciers were found by our party in 1855 at the heads of McKenzie's Fork and one of the tributaries of the Des Chutes, and on Mt. Shasta and Mt. Rainier are many true glaciers, of which some are several miles in length. But all the glaciers and snow-fields now existing on the Cascade Mountains are insignificant compared with those of the Glacial period. Then every gorge was filled with snow and ice, the broader and more irregular summits were covered with glaciers and these descended far below the present line of perpetual snow. Now in many localities and over many square miles the rock surfaces are planed smooth or grooved like a plowed field, and every projecting crest of volcanic rock, rough and ragged as it was, is rounded over and worn into a *roche moutonnée*. From the Three Sisters glaciers descend into the valley of the Willamette on the west and that of the Des Chutes on the east, and I traced glacial markings from the snow line to a point 2,500 feet lower, where they pass under the alluvium of McKenzie's Fork.*

It has been claimed by Lecoq (*Les Glaciers et les Climats*) and following him by Professor Whitney and others (*Later Climatic Changes*), that the great development of glaciers during the Ice Period, such as those of the Canadian highlands, the Rocky Mountains, the Cascades and Sierra Nevada, was not the effect of a cold but a warm period, which increased the precipitation and consequently the snow-fall at all places where the temperature was low enough to cause it to take the form of snow. If this was all, however, the most extensive glaciers should be in the Alpine districts of the tropics or temperate zones wherever the precipitation is most abundant and the temperature low enough to produce perpetual snow. But the great glaciers of the present time are not on the Andes, the Himalayas or the Alps, but on Greenland and the Antarctic Continent where the climate is very cold and the amount of precipitation small.

We also find on the summits of the Cascades a demonstration of the fallacy of this view; since here some of the mountains rise 14,000 feet above the sea and the line of perpetual snow is not over 7,000 feet, while the annual precipitation is greater than in almost any other portion of our country. In fact the snow accumulates in such quantity that even in mid-summer it reaches down to where it is met and opposed by a vigorous forest growth—the product of a high temperature It is evident that no elevation of temperature, though it should increase the evaporation on the Pacific and the rain-fall on the coast, would cause the renewal of the ancient glaciers; but with a depression of temperature which should continue the present winter conditions through the year, the precipitation remaining the same, the accumulation would soon cover the mountain summits with snow and ice and bring the glaciers down to their old limits.

THE LOWER COLUMBIA.

The country bordering the Lower Columbia is too well known to require description. I am impelled, however, to refer to one or two points in its physical structure which are of special interest when brought into connection with facts of similar import observed in the region about Puget's Sound. I have said that the Lower Columbia is an arm of the sea. It is in fact a deep river valley which has been flooded by an influx of the sea caused by subsidence. This brings tide-water to the foot of the falls of the Willamette at Oregon City, and to the Cascades.

It requires no argument to prove that such a channel could not have been cut unless by a rapid stream flowing into the

*Pacific Railroad Report, vol. vi, Part II, Geology, p. 42.

ocean when it stood at a lower level. Whether the change in the relative level of land and sea here remarked was part of a general movement which produced the influx of the sea into the fiords which fringe the northwest coast; and whether this is not a part of a still grander movement that flooded the old excavated valleys of the James River, the Potomac, the Schuylkill, the Hudson, the St. Lawrence and the Saguenay and at the same time filled the fiords of the northeastern coast, are questions which cannot now be fully answered but are worth considering.

It will be noticed that the general plan of the topography of this part of the coast is altogether similar to that of California; namely, the great wall of the Cascades bordered on the east by the Willamette and Cowlitz valleys, and the Coast Mountains along the sea shore, are reproduced farther south by the Sierra Nevada, the great California valley and the Coast Ranges. And these features are not only physically similar, but geologically identical; the Cascades being the northern continuation of the Sierra Nevada, the more modern Coast Mountains being continuous, the great trough between them essentially one, but filled at its center by a mass of mountains.

SURFACE GEOLOGY OF THE PUGET'S SOUND BASIN.

The name Puget's Sound is made in popular use to cover all the peculiar group of inlets and tideways which lie immediately east of Vancouver's Island,—Puget's Sound proper, Admiralty Inlet, Hood's Canal, etc. These occupy the northern extension of the great Columbian valley, which, like its counterpart in California, lies between the Coast ranges and the Cordilleras. Farther north still this depression is deflected toward the northwest by a change in the trend of the Cascade Mountains and the representatives of the coast ranges on Vancouver's Island.

In Washington Territory the Coast Mountains are higher than in Oregon and have received the local name of the Olympian range, of which the highest summit is called Mt. Olympus. This range terminates somewhat abruptly but is apparently continued in the mountains of Vancouver's Island. Through the gap between these and the Olympian range a deep channel is cut, now an arm of the sea, called the Strait of Juan de Fuca. In former times, when this portion of the continent, and probably the whole northwest coast, stood higher above the sea, this Strait was the valley of a great river which drained most of the western slope of the Cascades in Washington Territory, and had as branches the Skagit, Snoqualme, Dwamish, Puyallup, Nisqually and various minor streams. During the Ice period this hydrographic basin was filled with a great glacier made up of contributions from all the surrounding mountains. It flowed out to sea by the Strait of Fuca, but this channel was far too narrow for it and it spread over all the southern portion of Vancouver's Island, planing off, rounding over or deeply scoring the rocks in its passage. As the glaciers retreated they left behind a sheet of drift several hundred feet in thickness, partly water-worn and stratified, partly unstratified bowlder clay with striated pebbles, of which the surface was nearly level. In process of time the draining streams had cut in this plain a series of valleys all tributary to one which led out through the Strait of Fuca to the ocean. After perhaps some thousands of years, during which the excavation of these valleys progressed, a subsidence of the land or rise of the ocean caused the water to flow in and occupy the main valley and all its tributaries up to the base of the mountain slopes.

Such in few words is the history of the formation of this remarkable system of inlets. They are simply the flooded valleys of a great river and of the branches that formerly joined it but now empty into the extremities of the finger-like inlets that have partially replaced them.

There are but few localities in Puget's Sound basin where the rocky substratum rises so as to be visible above the water level. Along the northern and western margin on Vancouver's, Sucia, Orcas and Whidby Islands, and at Chuckernut's and Sohome the rock appears, but at Tacoma, Steilacoom, Seattle, Port Madison, Port Townsend, and it may be said generally about the Sound, the shores are steep bluffs, 100 to 150 feet in height composed of drift alone. From the cliffs at Port Townsend and Tacoma, I took sub-angular scratched and ice-worn pebbles as characteristic and convincing as any to be found in the bowlder clays of the eastern States.

The subsidence which caused the sea water to flow into the subaerially excavated valleys of Puget's Sound also filled the channel of the Columbia, the Cascades and the system of fiords, of which these are representatives, that fringe the northwest coast. We have evidence, too, that the area occupied by the sea was at one time much more extensive than now, for all the country immediately about Puget's Sound is marked with a series of marine terraces which Mr. Bailey Willis, who studied them carefully when connected with the Transcontinental Survey under Professor Pumpelly, tells me can be traced to a height of 1600 feet above the present ocean level. These terraces are conspicuous on the low divide which separates the valley of the Cowlitz from the basin of Puget's Sound; and here, as over much of this region, the ground is covered with pebbles and water-worn bowlders, the product of the long continued dash of the shore waves on a slope composed of drift materials. In the advance and recession of the shore line, the finer materials have been mostly washed away, and the stony surface has little agricultural value. Fortunately it is well adapted to the growth of trees, and the splendid forest which covers it is perhaps an equivalent for all it has lost. The facts here given show why the cultivation of the soil in Washington Territory is limited to the narrow belt of modern alluvium along the streams, and indicate that coal mining, the fisheries and the lumber industry must be in the future as they now are, the most important sources of wealth.

MODERN GLACIERS OF THE SIERRA.

From the Willamette Valley and Puget's Sound grand views are obtained of the great snow peaks of the Cascade Mountains; the Three Sisters, Mt. Jefferson, Mt. Hood, Mt. Adams, Mt. St. Helens, Mt. Tacoma and Mt. Baker. Of these, Mt. Hood has an altitude of 11,225 feet, Mt. Adams 12,250, and Mt. Tacoma 14,400. In Colorado and California there are a number of summits of equal absolute altitude, but they have nothing like the relief above their surroundings that these have; carry far less perpetual snow, and in every way are less impressive. In Washington Territory the line of perpetual snow on the west side of the mountains is about 6500 feet; on the east side, several hundred feet higher. Mt. Tacoma carries therefore about 8000 feet of snow. Below this it is covered with a dense forest. As none of its foothills rise to the height of 2000 feet above the sea and are invisible at a distance, from many places about the Sound practically the whole of the mountain is seen at one view; a gigantic cone, 14,000 feet in height, apparently rising directly from the sea level. Mt. Shasta has the same altitude, and as seen from Scott's valley is wonderfully impressive, but it is situated farther inland and farther south, its base is higher and it has less snow, and is therefore somewhat less imposing. It is not too much to say then, that no other mountain on this continent and none in Europe rivals Mt. Tacoma in grandeur and beauty, and it is doubtful whether in the world there is any that produces a profounder impression upon the beholder. Mt. Hood, as seen under favorable circumstances from Fort Vancouver, especially when reflected from the lake-like surface of the Columbia, is as beautiful but far less grand.

Though appearing in the distance so smooth and symmetrical, Mt. Tacoma has been found to be a ragged and compound mass consisting of three conspicuous summits and many subordinate peaks, with precipices 2000 to 3000 feet in height and deep gorges which make the ascent difficult and even dangerous.

It has been ascended, however, several times, and its labyrinths sufficiently explored to prove that it carries from eight to twelve glaciers, some of which are many miles in length and will bear comparison with those of the Alps.

Every traveler who enters Puget's Sound region from the south is sure to be struck by the turbid milky appearance of the water of the Cowlitz River along which the railroad runs for miles. This character it shares with all streams that drain glaciers, and which has caused the Swiss mountaineers to give to the waters of such streams the name of *Gletscher Milch*. Its turbidity is due to the sediment produced by the constant grinding action of these enormous masses of moving ice set with stones upon their beds, and attests the sometimes disputed efficiency of glaciers as eroding agencies. The Puyallup, White River, and other streams, which come down from Mt. Tacoma, are alike milky, and each shows that one or more glaciers are continually grinding away at its head. On the contrary, the streams which do not come from glaciers and are supplied by rain only, and that filter through the decaying vegetation of the dense forests, carry very little sediment and that chiefly carbonaceous matter. These are clear but brown, and the contrast which the water of such streams presents to that of the rivers which drain the glaciers is very striking and justifies the names borne by two such of Black and White Rivers.

It has been contended by some writers, as has been mentioned, that the extension of glaciers in former times was due simply to an increase in the amount of precipitated moisture, but it is easy to see that the heavy rain-fall of Washington Territory might be increased indefinitely with no considerable elongation of the glaciers. But even with the rain-fall remaining as it is, if a depression of temperature should take place carrying the present conditions of winter through the year, the glaciers would soon creep down into their old beds, fill all the valleys of their draining streams and finally coalesce to form one grand glacier which would flow out through the Strait of Fuca to the ocean.

Following the coast northward from Puget's Sound we find the glaciers coming down lower and lower until in Alaska they reach the sea level. No one can claim that this is because the precipitation is greater there, since observations show that it is not, but every candid man will acknowledge that it is because at the north the temperature is lower. He must also accept these facts as a demonstration *that a prime factor in the production of the phenomena of the Ice Period was a secular depression of the temperature.*

Art. XLV.—*Rainband Spectroscopy*; by Louis Bell.

It is now more than ten years since Professor Piazzi Smyth pointed out that the absorption bands of aqueous vapor in the solar spectrum were likely to be of considerable service in meteorology; but although his observations were evidently successful and have been often repeated with equally good results, little progress has been made in the practical application of the principle. This lack of results appears to be due to two causes. First, as the method seemed to offer an easy way of predicting the weather, it at once fell into the hands of the would-be weatherwise who understood it very imperfectly and were thoroughly incompetent to use it. The successful often made the wildest claims for it, and those less skillful or fortunate were active in denunciations, even deriding it as "an optical illusion strengthened by long practice." Secondly, there were real difficulties in the way of its application. Nearly all observations of the rainband have depended upon eye estimations of its intensity, unsatisfactory at their best and doubly so when complicated by long intervals, clouds, and widely varying conditions of illumination. The rainband is a small object in the instruments generally used for the purpose, and it is no easy matter to compare it with such a vague and variable thing as a mental scale of blackness. The reference to the Fraunhofer lines is open to nearly as much objection, since their apparent intensity is liable to vary with the light, width of slit and general condition of the spectroscope to an extent which renders them very uncertain standards for scientific purposes.

The first mentioned cause is an unfailing concomitant of every discovery that smacks of popular science; the second is due only to the nature of the observation itself and can be removed with comparative ease. The object of this paper is to call attention to a device by which something like quantitative accuracy can be secured in the study of the rainband, and to the general methods which appear to give the best results in this investigation, which, it is to be hoped, will prove of permanent service to meteorology.

The desideratum is evidently a definite and trustworthy scale of variable blackness, extending over quite a wide range, and easily comparable to the rainband. A direct photometric measure of the intensity of the absorption band would of course be valuable, but is an observation too complicated and delicate for everyday use. The measurement could be made however by using a standard of light as one of the sources of illumination in the spectro-photometer of Vierordt ("Die Anwendung des Spectral-apparates") or some modification, like that described by the writer (American Chemical Journal, vii, No. 1). The ideal scale therefore should be constructed so that the absolute intensity of its various readings could be found by the above or some other method.

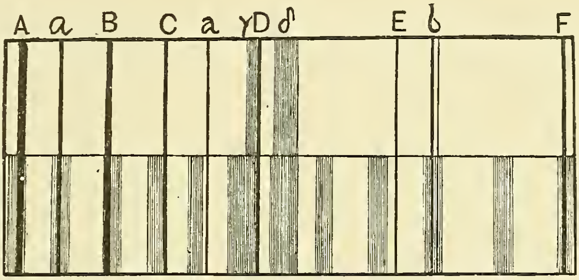
Janssen and others have tried various rude methods of estimation, but the only scientific instrument for measuring absorption lines yet devised is that due to Mr. C. S. Cook, and described by him in "Science," ii, 488. His plan consists, in brief, of forming diffraction fringes in the field by a silk fiber placed a trifle beyond the focus, and varying their intensity by moving the fiber to or from the focus by means of a screw collar. The fringes thus produced resemble the lines of the rainband quite closely, and form a very reliable and delicate scale. Its successful use, however, requires a skilled observer and much care, and its indications cannot be reduced to absolute measure except by a comparison with some absorption line which can in turn be evaluated with a spectro-photometer, itself by no means an easy matter. While this method is very effective in measuring the individual lines of the rainband some plan for measuring the band as a whole seemed desirable, and, after some casting about, the writer determined to give a careful trial to the interference bands produced in the spectrum by a crystal in polarized light. So far as known, this method has not hitherto been used.

The instrument used was a direct-vision spectroscope by Schmidt of Berlin, which gave a very bright spectrum with moderate dispersion and beautiful definition. Its five prisms polarized well enough to make it a very efficient analyzer. A bit of selenite having a well defined natural edge was then split down till the interference bands produced by it were about as wide as the maximum rainband as seen in the above instrument. The plate thus prepared was placed immediately in front of the slit, covering, however, only half the field. The usual cap of the spectroscope was removed, and replaced by a tube graduated to five degrees and carrying a 9^{mm} Nicol prism. Thus equipped, a long series of preliminary experiments were made to settle upon the best method of observation and adjustment of instrument.

Finally the following plan was adopted. The thickness of the selenite plate was so adjusted that the bands appear as in fig. 1, one of them being situated about the average width of the rainband on the less refrangible side of D, i. e. in the red. Then the Nicol prism was turned till the interference band was of the same intensity as the rainband, when the field appeared of the same brightness from the red side of the interference band to D. In this method of observation it is quite as well to have the selenite plate cover the entire slit, as the field is then uniform and the obtrusive black line produced by the edge of the plate is absent.

In passing it must be remarked that the nomenclature of the atmosphere lines is in a state of confusion thrice confounded. Each investigator seems to have numbered or lettered them to suit his taste. Only two telluric groups of lines have designations which are generally received, the band at w. l. 628 (a) and that at w. l. 578 (δ). In this state of affairs the writer would suggest that for sake of brevity it would be well to apply the letter γ to the well known rainband near D, reserving

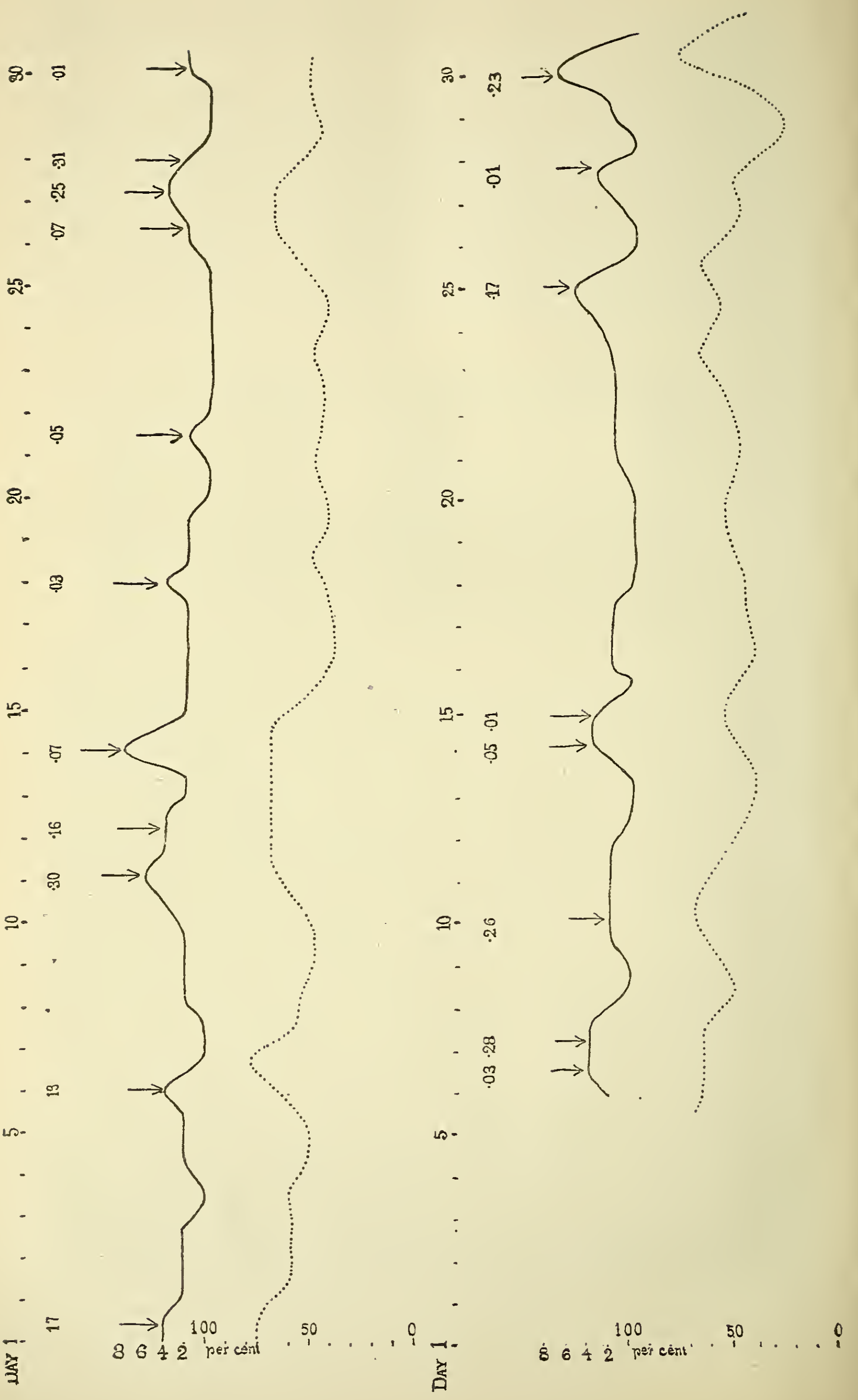
1.



β for the faint band given in Ångström's chart between a and D, and using Brewster's lettering for the more refrangible part of the spectrum. Then in referring to the individual lines that go to make up γ or any other of the bands it would be well to simply number them in order of their wave length —$\gamma 1$, $\gamma 2$, etc. And a careful map of γ is badly needed.

After being arranged as above, the spectroscope was used constantly for six months with complete success. The place of observation was Baltimore, Md., a location well calculated to give rainband observations a severe test, by reason of a climate more than usually variable. Having arranged a satisfactory scale, the point to be determined was, how far rainband observations alone are to be trusted as prophetic of the weather, and how they should be taken with a small instrument like the one used in order to secure maximum efficiency.

As to the first point the annexed chart answers the question. An observation was taken at 8 A. M. and another at 2 P. M., usually in the western sky. No other instrument was used to confirm the indications of the spectroscope. From day to day the instrument was now and then readjusted as regards slit, focus, etc., and there was no difficulty in securing identical scale readings after such readjustment. The instrument polarized too well, so that the interference band could be made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much blacker than was ever necessary. Consequently a small rotation of the Nicol would duplicate the strongest rainband and the scale was somewhat limited in extent. The tube carrying the Nicol was graduated to give a scale of about eighteen grades of intensity, but for the above reason only eight could be used, which however proved sufficient to give very good results. The rainfall and relative humidity for the period covered by the chart were obtained from the Signal Service station at Baltimore. In the chart the upper curve gives the variations in γ from day to day, the arrows being placed to indicate the time when a rain began, and the amount of rainfall being annexed. The dotted curve below shows the per cents of relative humidity for the same period. During the time represented there were nineteen rains, of which number fourteen (73 per cent) were distinctly predicted from two to thirty-six hours before their occurrence by an increase in the intensity of the rainband. On grouping the rainfalls in the case of a continued storm, like that of the last week of March, there were fourteen rains, of which thirteen (93 per cent) were predicted. In only one case did a rain come entirely without warning—the shower of March 18, which drifted up suddenly between observations. In one or two other cases rain fell shortly after the morning observation; but on a careful review of the observations, it appears that during the entire six months about eighty per cent of the rains gave fair warning through the spectroscope. This is a very satisfactory degree of efficiency for a small instrument. It will be noticed that in the chart there appears no very obvious connection between the strength of the rainband and the amount of rain. Now and then there is a rough correspondence, but that is all. A good reason for this is found in the fact that now and then an observation would be taken during a previously predicted storm and thus γ would be abnormally strong, as it is generally during a rain. The curve of relative humidity corresponds quite closely with the rainband curve, though in general its maxima and minima are some hours later. The occasional discrepancies are undoubtedly due to the fact that while the hygrometer indicates only the lowest layer of aqueous vapor, the spectroscope sums up everything along the line of sight and thus indicates vapor in the upper strata of the atmosphere entirely out of reach of the hygrometer, while on the contrary an earth stratum quite dense enough to affect the latter instrument would be frequently too slight to make itself apparent as a rainband, particularly in a small instrument.

In this integrating power of the spectroscope lies its strength, and unfortunately also its weakness as a meteorological instrument. While it shows with considerable accuracy the total amount of aqueous vapor in the entire thickness of air in the direction of observation, it tells nothing of its distribution along that direction. Thus without any increase in the rainband there may be a precipitation of rain due to a descending mass of vapor. So also clouds may or may not affect the rainband, though as a rule they do. The amount of vapor present is often quite independent of the amount and kind of visible clouds. Now and then the spectroscope will show dense masses of vapor where the eye can detect at most only the lightest cirri. Again, toward the end of a storm the sky may frequently be overcast with threatening clouds when the rainband clearly shows that the storm is over. Indeed the close of a storm is indicated quite as certainly as its beginning, and in this fact lies one of the great advantages of the spectroscopic method. A typical case is shown in the storm of May 1st, when the hygrometer reached its maximum some twelve hours after the rainband had fallen far enough to indicate the cessation of rain.

It is difficult, if not impossible, to designate the intensity of rainband that may be regarded as a sure precursor of a storm. Certainly no general rule can be laid down, for at present no two scales are alike and each locality probably has its own peculiarities. For the instrument used by the writer in Baltimore, a rainband corresponding to 4 of the scale generally meant rain within twenty-four hours. With the limited scale possessed by the instrument it was best to register the faint traces of a rainband as 0— too small to measure easily, although in a larger spectroscope one might venture on an estimate. It is very desirable that the readings of intensity should be reduced to an absolute scale, in which case observations taken at all points would be comparable and might lead to the discovery of the laws which govern the distribution of aqueous vapor. A knowledge of these could hardly fail to be of great value to the science of meteorology.

With the polarization scale herein described it is quite possible to determine the value of each scale division in per cent of light absorbed, by a careful comparison by means of the spectro-photometer, since the interference bands are wide enough to be easily measured. The coefficients of absorption once determined it would be possible to reduce them to terms of standard column of aqueous vapor, and then the problem would be well in hand and rainband observations would mean something more than mere qualitative estimation. Until these quantitative measurements are generally made, the spectroscope can be of comparatively small service except as an aid to the prevision of local storms.

For this latter purpose it is certainly effective and when used in conjunction with the usual meteorological observations, can hardly fail to give indications of real value. Alone it appears to be of about the same order of usefulness as the barometer alone and unaided, but in connection with other instruments it certainly deserves to be generally used, and ought to be added to the equipment of meteorological stations. Its cost is quite small—an instrument like the writer's, fitted with a similar scale, costing not more than $20. In practice a pile of glass plates is cheaper and better than a Nicol prism, since it polarizes less perfectly and thus allows the use of a more extended scale.

If an untrained observer begins rainband observations he is almost sure to be, at first, disappointed with the results. He procures a small pocket instrument of insufficient dispersion and goes to work under the impression that a daily observation will enable him to predict the weather for the next twenty-four hours or more with almost unerring certainty. Naturally he fails, and the more thoroughly he has believed the claims made in various popular articles the more bitterly he abuses the method. And very absurd claims have been made for it. There may be some gifted persons to whose eyes the rainband spectroscope becomes an oracle to reveal the meteorological future, discriminate between hail, snow and rain, designate the amount of each, predict the electric potential of an approaching cloud, indicate the per cent of ozone (!) in the atmosphere, and in general answer the scientific prayers of the astute suppliants. But the writer is not one of this fortunate number. If there is any method of predicting the kind of precipitation which is to occur, or of discovering the aforementioned potential, he would like to see it demonstrated in black and white, so that it will not require the eye of faith to comprehend it.

The rainband spectroscope, like most other scientific instruments, is capable of giving good results in competent hands, but there is no jugglery about it, neither is it the universal solvent of meteorological difficulties, and success in its use can be obtained only by frequent and careful observations, accompanied if possible by observations with other instruments.

For practical work one of the larger pocket spectroscopes should be chosen, giving a bright well defined spectrum with a moderate amount of dispersion. For use with the scale described the dispersion should not be great enough to resolve the rainband into distinct lines, but should show it simply as a dull band, so that it will be comparable to the interference bands. If the individual lines are to be studied a much larger dispersion is needed—enough to divide D without difficulty. The instrument should be used with a rather narrow slit and should be focused for sharp definition of D; γ will then appear as a narrow shading along the red edge of D if the weather is fine, or a dark stripe if it is rainy, and can be very easily compared with the scale.

The observations should be taken not less than twice a day, one of them being as late in the afternoon as practicable, since changes are so apt to appear about sunset. If convenient, observations should be taken at various altitudes. For a single altitude 20° is perhaps as good as any. As to the question of direction, it is a matter to be guided by local conditions. The illumination should be as good as can be attained without looking directly at or near the sun, and a quarter of the sky should be chosen where this condition is fulfilled. Observations in all directions are to be desired; but if only one is chosen it should preferably be the one from which storms usually come. An additional observation in the direction of the wind is likely to be useful. Observe systematically the same directions and altitudes. Avoid cumulus clouds when practicable, as they are likely to increase the rainband without a corresponding probability of rain. Employ clear skylight, if possible without much variation of direction or altitude.

More trust is to be put in a sharp rise or fall of rainband intensity than upon absolute intensity, since the latter is likely to average differently in various locations and be subject to various modifying factors, while the former in nearly every case means a definite change. With these and other precautions which will suggest themselves to individual observers, the rainband spectroscope is capable of giving very good results, and the field of investigation is a decidedly promising one.

For the convenience of those interested in the subject a brief bibliography is appended comprising the more important publications which have appeared upon the subject. A few of them refer to telluric lines in general.

Nature, xii, 231, 252; xiv, 9; xvi, 389; xxii, 194. Piazzi Smyth.
Edinburgh Astronomical Observations, xiv. Smyth.
Scottish Meteorological Soc. Jour., N. S., Nos. 51, 52. Smyth.
Madeira Meteorologic. Knowledge, ii, No. 48. Smyth.
Proc. Roy. Soc. Edinburgh, xii. Mill.
The Rainband (Hilger, 1883). Mill.
The Use of the Spectroscope in Meteorology U. S.
Signal Service Notes, No. iv, 1883. Upton.
Comptes Rendus, lxiii, 293, xcv, 885. Janssen.
A Plea for the Rainband (Browning, 1881). Capron.
Science, ii, 488. Cook.
Journal de Physique (2^{me} ser.), ii, 58, iii, 109. Cornu. iii. 5. Thollon.
How to Foretell the Weather with the Pocket Spectroscope. (Chatto and Windus, 1884.) Cory.

ART. XLVI.—*A new Genus of Chazy Sponges, Strephochetus;* by Professor HENRY M. SEELY.

THE Chazy rocks of the lake towns of Addison County, Vermont, afford a fossil form which, so far as is known to the writer, has not been heretofore described. That it should have been so long neglected is the more remarkable since it is an excellent guide to some layers of the Chazy when other well known fossils are obscure or wanting.

The nearest approach to an illustration of this form is found in the Geology of Vermont, vol. i, p. 277, fig. 175, where *Phytopsis tubulosum* is figured. In the text, attention is called to a horizontal section of the stem; and the concretionary arrangement of the calcareous matter is pointed out. No perpendicular section is given.

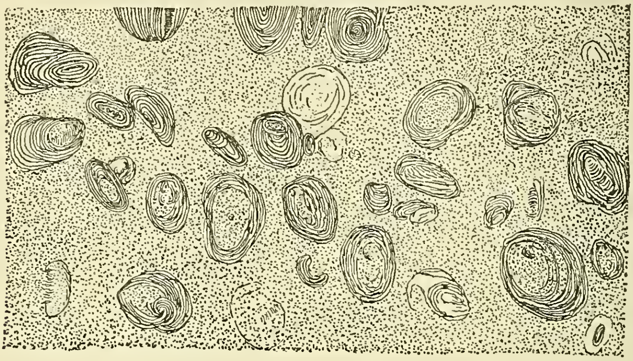
It was assumed on a recent examination that the form was of animal origin and a provisional name indicating that it was a Stromatopora was assigned it. A more extended examination however does not confirm the first impression. The size of the object favors its complete inspection, it being so small that a section of the entire fossil cut in any direction can be placed on a slide and put upon the stage of the microscope.

The mass is seen to be mostly made up of concentric layers of material, these layers being apparently composed of fine wavy twining threads about $\frac{1}{2000}$ of an inch in diameter; but in section these threads are seen to be essentially circular tubes. The more exact measurement of these tubules is, according to Dr. M. C. White, outside measure $\frac{1}{1450}$ inch, the inner diameter, clear space, $\frac{1}{2200}$ inch. The outer portions of the layers seem more compact, and on weathering the fossil assumes a Stromatoporoid appearance. The form is accompanied by grains having in many cases a concentric structure, and these are apparently oölitic; yet with these are mingled minute fragments having the structure of the larger nematoid mass. These grains, oölitic, fragmental, and possibly in many cases the young of the animal, have in some instances penetrated the larger mass and partake with it the concentric arrangement.

In seeking the relationship of this fossil, its general character shows it to be allied to the sponges. The scanty materials at hand with which to make comparison do not permit me to pronounce with entire confidence as to its very nearest allies.

Sections of a fossil form, described by S. A. Miller, as *Stromatocerium Richmondense*, Journal of the Cincinnati Society of Natural History, vol. v, April, 1882, show here and there, but rarely, a structure which is probably identical. The newly named Calciferous fossil, *Cryptozoön proliferum*, figured and described by Professor James Hall, Thirty-sixth Report of the Regents of the New York State University, helps to determine the relationship. A portion of that description reads: "The substance between the concentric lines, in well-preserved specimens, is traversed by numerous minute irregular canaliculi which branch and anastomose without regularity."

1



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3

DESCRIPTION OF FIGURES.

1. *Strephochetus ocellatus* in rock, natural size.
2. The canaliculated structure of the fossil, enlarged one hundred diameters.
3. The same—view from near the center of the field, enlarged about three hundred and fifty diameters.

The sections made, and those viewed from the cuttings of others from what is supposed to be this Cryptozoön, do not show the described structure plainly enough to permit me to make satisfactory comparison with the Chazy form. The structure is possibly identical. The little canals which so largely make up this little eye-like Chazy fossil go twining over and around each other, and up to the present time they have been rarely observed branching and never anastomozing.

This fossil does not seem near enough any already recognized form to be included in it generically, and until other relationships are found it may properly stand by itself a separate genus.

Strephochetus (στρέφω I twine, ὀχετός canal), nov. gen.

A free calcareous sponge showing in structure concentric layers composed of minute twining canals.

Strephochetus ocellatus, n. sp.

Type of genus. A compact calcareous sponge, spherical or slightly flattened, distinctly concentric in character, usually less than half an inch in diameter, forming when in masses, a tough limestone. When weathered the concentric character is very evident, the fossil then looking like little eyes peering from the stone.

These forms are often gathered in crowded masses, the intermediate spaces being filled with fragments of the fossil mingled with oölitic grains. More rarely they appear here and there in a mass of oölite.

This fossil occurs in connection with well recognized Chazy forms and especially with *Maclurea magna*. It is found in place in the towns of Addison, Bridport, etc., in Addison County, Vermont, and in bowlders in those towns as well as at Crown Point, N. Y.

Middlebury College.

ART. XLVII.—*William B. Rogers's Geology of the Virginias.* A Review;* by J. L. and H. D. CAMPBELL.

MRS. WM. B. ROGERS could not have commemorated the life and labors of her distinguished husband in a more fitting way than she has done, by collecting and editing his reports and other valuable papers on the Geology of the Virginias, which form a neat and compact volume of about 800 pages. The geological survey of Virginia (at that time including West Virginia), for which Prof. Rogers, with a competent corps of assistants, was employed, was never fully completed; and yet enough had been done to justify his beginning work on his final report, while his assistants were closing up some remnants

* A reprint of Annual Reports and other Papers, on the Geology of the Virginias. By the late William Barton Rogers, LL.D., etc., Director of the Geological Survey of Virginia from 1835 to 1841; President of the National Academy of Sciences. New York: D. Appleton & Co.

of field work still to be done. In the last annual report which he was allowed to make (December, 1841), he says: "Having brought the active labors of the survey thus near their completion, I am now entering upon the task of preparing the final report. Those who, like the Board, have been able to follow me and my assistants in the diversified and laborious researches in which we have been actively engaged for nearly six years, and who reflect upon the immense extent of ground over which our explorations have been carried, the largest area ever submitted to systematic geological investigation in any part of the world, will be at once prepared to form a just idea of the number and extent of our results, and of the magnitude of the task of producing a full report on the geology of the State." (p. 543) The Hon. John Rutherford, at that time president of the Board of Public Works, in a communication to the Virginia Legislature, says: "To enable him, therefore, to present such a report, to arrange the numerous mineral specimens, and to prepare the necessary drawings and maps, Prof. Rogers recommends that the services of his corps, as at present organized, be continued until the close of the present geological year in April next, and that an additional appropriation of $2,500 be made for the purposes above mentioned."

Notwithstanding the cogent reasons given for making the needful appropriation, the Legislature of a great and then wealthy State, failed to grant the pitiful sum of $2,500 asked for in order to complete one of the most interesting and important geological surveys ever undertaken on the continent. Thus it happens that the fruits of nearly seven years of laborious research have been allowed to lie hidden from the public for more than 40 years—except in the form of a few obscure pamphlets—until within the last few months, when Mrs. Rogers has given them to the scientific world in a permanent form. If Prof. Rogers had been allowed to complete his work and reduce it to a systematic form, it would have been more satisfactory to the general reader than it is as now presented; and yet the volume before us is among the most valuable contributions made to our geological literature in a long time. It brings before the public for the first time a comprehensive view of a region, to which we wish to invite the attention of our fellow-laborers, as a *typical field* for geological studies.

With this end in view we propose to give a brief synopsis of what the field geologist will find here. We wish to show that the two Virginias embrace within their extensive area illustrations of every geological feature of the American continent, with the exceptions of the great "lava-flows" and the volcanoes of the far West, and the modern coral reefs of tropical seas. The excellent map and sections which accompany the volume, illustrate our geology in a comprehensive way, and will be a most valuable help to a clear comprehension of the subject before us. But the younger field-workers at least, some of whom doubtless desire to see for themselves what we have in this wide and varied field of 61,400 square miles, will be aided in their explorations by brief sketches of some characteristic features of our several geological formations and a statement of points on leading routes of travel at which observations can be conveniently made. We therefore offer a general view of each of the geological formations and groups found in the Virginias, taken in their *chronological order*, which is not done in the volume; except so far as relates to a very concise view given of the several subdivisions of the Appalachian range in the second and third annual reports.

The notation by numbers from I to XVI, given by Prof. Rogers, will be retained in our present discussion as a part of the history of the survey, and at the same time we shall give the more complete and generally applicable notation of Prof. Dana's table, which has been practically adopted by Prof. Rogers in the last chapter of the volume, page 717.

It may be well to state in this connection that a brief summary of many of the most important facts given below will appear in our notes on Virginia prepared for a forthcoming new edition of Macfarlane's Geological Railway Guide.

The reader of Prof. Rogers's Reports will find some few mistakes and inconsistencies which no doubt would have been corrected had he lived to see his work pass through the press. But we must remember that he had to begin on a very wide and unexplored field, that he was laying an important part of the foundation of our grand system of American Geology, the superstructure of which has been in process of erection for half a century and more, and is not yet completed. In this preliminary work it is astonishing to find so few errors in the published results of his labors, and to find what a solid basis his successors have to build upon.

Archæan Group.—This group constitutes the country rocks over an extensive area in Virginia, but is found nowhere in West Virginia except in the N.E. corner near Harper's Ferry. To these rocks Prof. Rogers in his State Reports applies the old name, "Primary;" but in his notes for Macfarlane he adopts the term, "Archæan." His map shows very clearly the outline of this great geological belt, extending from the Potomac to the line of Tennessee. Its eastern boundary is a somewhat waving line extending from the city of Alexandria in a southern direction through the cities of Fredericksburg, Richmond and Petersburg, and cutting the line of North Carolina a little way north of Weldon. From that point its south border is the Carolina line to its western limit. The "backbone" of the Blue Ridge marks out, with a few limited variations, its N.W. border from the Tennessee line to Harper's Ferry on the Potomac, a distance of about 340 miles. The narrowest part of the area thus defined borders on the Potomac between Alexandria and Harper's Ferry—about 50 miles by a right line. About one-third of this distance, however, is occupied by overlying beds of Mesozoic age, to be noticed hereafter in connection with a number of other large patches of the same age similarly situated.

In his State Reports, Prof. R. makes no subdivisions of his "Primary" formation, except of a very general lithological character; but in his notes for Macfarlane (note 6, p. 722), he indicates four subdivisions, and singularly puts their notation in the order, A, D, B, C. His D, which he calls, "Norian or Upper Laurentian," seems to appear on the railroads, according to his notes, at only two stations, one at the junction of the Chesapeake & Ohio with the Virginia Midland Railway, about one mile southwest of Charlottesville; the other seven miles farther in the same direction, at Ivy station on the Chesapeake & Ohio Railway. It is true that there are "four rather distinct groups of Archæan rocks found in Virginia, viewed lithologically," but we regard the question, as to whether or not they belong to so many distinct horizons, not sufficiently settled to justify our recognizing more than the two general divisions, Laurentian and Huronian, 1*a*, and 1*b*.

One of the mistakes alluded to above, and which the reader will encounter early in his perusal of the volume, is found in two places in the Report on the "Geological Reconnoissance" of the State. On page 27 he says: "Observations render it doubtful whether in the Blue Ridge any truly primary rocks occur." Again on pp. 83, 84, in discussing "the region west of the limestones (below Lynchburg), as far as the western flank of the Blue Ridge"—that along his profile section No. I, which crosses the southern portion of Albemarle and northern corner of Nelson county—he comes to the "conclusion, that by far the largest portion of its surface is occupied by rocks which *do not* belong to the primary system, while they have, at the same time, served to display the modifying effects of igneous agents, as manifested in the changed structure of many of these rocks, on a scale of wonderful variety and extent. Early in the present report, allusion was made to the prevailing errors on the subject of the true geological character of this region, inclusive of the Blue Ridge; and enough has already been stated, in regard to the structure of the region, to satisfy the enlightened geologist of the entire impropriety of the designation, primary, which it has heretofore uniformly received."

In subsequent Reports, and in his notes on the geology of the Chesapeake & Ohio and Virginia Midland Railways he recognizes the rocks of this same region as primary or Archæan. We do not call attention to this change of view for the purpose of throwing any discredit upon the author's conclusions, but rather to indicate his honesty of opinion in changing where subsequent observations pointed out his error. In the subsequent Reports, made from more careful and detailed explorations, there is left but little room for such changes, while the general accuracy of his conclusions is remarkable for that early period.

Prof. Rogers's classification of the primary rocks is about as distinct as any that has yet been made, but the territorial limits given to these general classes, or rather their outcroppings, fall far short of covering the whole area occupied by them. Still he is correct as far as he goes in the following paragraphs; if his definitions are accepted.

"*Granite* (1*a*.)—An unstratified or igneous rock generally found inferior to or associated with the oldest stratified and metamorphic rocks, and sometimes penetrating them in the form of *veins*, and of *dykes* or *walls* rising in the midst of them. . . . A true unstratified granite, though extensively displayed in some parts of our southern district, is by no means of common occurrence. As an example of it I would cite the belt of whitish, hard, solid rock, extending from a little distance east of Little Falling River in Campbell county (S.E. corner), with a breadth of between one and two miles across Staunton River in the neighborhood of Brookneal," (pp. 287–88). Referring to exposures of gneiss and gneissoid granite near Richmond and Petersburg, he says: "We observe at many exposures, veins of granite and syenite penetrating the gneiss, and in some instances in such extent as to present large protruding masses, or broad surfaces of the granitic and syenitic rocks above, as may be well seen at several points along the James River canal (now the Richmond & Allegany railway), and perhaps still more readily at the rugged exposures adjoining the mills on both sides of the Appomattox at Petersburg," (p. 457.)

"*Gneiss* (1*a*.)—A stratified rock composed of the same material as granite, having a laminated texture. . . . Gneiss rocks are in extensive use as building material in nearly all of the Atlantic states. In the southern districts they have been quarried below Columbia and between that point and Lynchburg. . . . A wide belt of micaceous and feldspathic gneiss traverses Albemarle, Nelson, Amherst, Bedford, Franklin and Patrick counties, presenting frequent beds of granite and syenite, the latter more largely developed in proceeding toward the southwest." (pp. 288–290). "Rocks of this description occupy a large portion of the district (northern). Besides being met with in intermediate lines, they are largely developed in two extensive tracts, one of which stretches in the form of a belt from the eastern flank of the Blue Ridge, to the neighborhood of Southwest Mountain in Albemarle county, gradually contracting in width in its prolongation toward the northeast, and finally near the Potomac, compressed between the flank of Short Hill and a line some distance west of the Kittoctin Mountains; the other forming an irregular triangular area, having its apex a little west of Fredericksburg, its eastern side in nearly a meridional direction, coinciding with the eastern limit of primary rocks along the head of tide, and its western side parallel to the general course of Southwest Mountain, and terminating a few miles west of Columbia. . . . Along the eastern margin and for some miles westward, the variety chiefly met with is a gray rock, consisting of quartz, feldspar and black mica with occasional spangles of white mica, and scattered grains of hornblende. It is of an even texture, readily separating into large slabs and blocks, suitable for columns and other purposes. . . . Of this variety innumerable exposures are presented on both sides of the James river in and above Richmond, and on the Appomattox in and above Petersburg, and a similar rock, though in much less extent, is seen on the Rappahannock above Fredericksburg." (pp. 455–456).

"*Micaceous, Talcose and Argillaceous Slates* (1*b*).—Westward of the triangular district of gneiss rocks just alluded to we find a broad belt of more slaty rocks, which, according to the peculiar ingredient predominant for the time, are either of the micaceous, talcose or argillaceous character. This belt reaches nearly to the eastern flank of Southwest Mountain, in Fluvanna, Albemarle and Orange counties; and farther north, though in part covered by the middle secondary rocks (Mesozoic), there widely expanded over the corresponding region, is still traceable in considerable width east of that formation through Fauquier, Prince William and Fairfax counties, as well as in narrow and interrupted tracts along the base of Bull Run and Kittoctin Mountains. . . .

"Referring however merely to general features, the micaceous composition, thus most strikingly exhibited toward the eastern margin of the slaty belt, is seen changing as we proceed west into one in which talc is more or less blended with the mica, and even in some places entirely displaces it. . . .

"Throughout the whole of this belt of slaty rocks, veins and beds of quartz are of very common occurrence, and, in the talcose and micaceous slates especially, are frequently more or less *auriferous*. In fact this belt includes nearly every locality in the northern primary district in which mines have been opened for extracting gold, or in which the metal has been found; and it may be added, that its prolongation south of the James River embraces the gold district of that part of the State." (p. 459).

The gold belt of Virginia is of varying width, but nowhere probably less than several miles wide. It extends through portions of the counties of Spotsylvania, Orange, Louisa, Fluvanna, Goochland, Buckingham and Appomattox, in all of which counties gold has been mined to a greater or less extent for many years—in some places very profitably.

Associated with the slaty formations of this region is a remarkable belt of metamorphic limestone. It is comparatively narrow, but in length it has been traced almost entirely across the State in a northeast and southwest direction. It is crossed by the Chesapeake and Ohio Railway near Gordonsville, while the Richmond and Alleghany Railway follows it longitudinally with the valley of the James from near Howardsville in Albemarle county to the great bend below Lynchburg. Our present view is that this belt and the slates described above are *Huronian* (1*b*).

To those geologists who may wish to examine in person the Archæan group so widely developed in Virginia, we would suggest some convenient points for explorations near stations on the principal railway lines in this portion of the State—beginning in the northeastern part of the Archæan belt.

(1.) On the Washington and Ohio Railroad, near Falls Church, Vienna and Hunters, and between these stations, may be found exposures of some prominent members of the group in cuts and along the margins of streams.

(2.) Near the Richmond, Fredericksburg and Potomac Railroad, fine exposures of the gneisses and gneissoid granites are readily found in the quarries along the Rappahannoc above Fredericksburg; along the banks of the James at and above Richmond and along the Appomattox at Petersburg.

(3.) At Fairfax on the Virginia Midland Railway; near Charlottesville on the Rivanna River, and along the river cliffs at Lynchburg, are points favorable for inspection.

(4.) Of all the leading routes of travel in Virginia no one affords so many favorable localities as the Richmond and Alleghany Railway for the study of the different formations from the lowest beds of the Archæan up to the top of the Hamilton group of Devonian age. From Richmond to Scottsville this road, following the valley of the James, cuts the ledges nearly at right angles to their strike. For several miles above Richmond the gneisses and granites are exposed to view in quarries and cuts. Then, after crossing the Richmond coal-fields, good exposures are found near Goochland C. H. of beds of gneiss and slates; and at Columbia quarries of gneissoid granites are worked, and several gold mines are not far north of the same point. Bremo Bluff, a few miles farther up the river, presents some good points in the great gold-bearing slate belt. This is the best point of departure for visiting Buckingham, one of the most interesting mineral counties in the State. A few miles west of Scottsville in Albemarle county, the line enters the limestone belt described above, and changes its course toward the southwest. The river has cut a serpentine channel of considerable depth in the general direction of the strike of this belt and exposed the limestones and their associated rocks to view at numerous points for a distance of about fifty miles. Among these points we may mention the vicinities of New Market, Gladstone, Walker's Ford and Stapleton.

The Richmond and Alleghany Railway from Lynchburg to Rope Ferry bridge cuts the Archæan beds nearly at right angles to their strike, and affords well-defined exposures at or near every station. About a mile and a half below the bridge on the north side of the river, the contact of the Archæan rocks (1*a*) with a trough of Cambrian age overlying them may be readily observed.

The Norfolk and Western and the Richmond and Danville railroads both afford some favorable points for observation in this ancient group; especially near Burkesville at the intersection of these lines, and at Lynchburg and Liberty on the former, and near Amelia C. H., Boston and Danville on the latter line.

Cambrian, *Primordial* (No. I, Rogers), or Potsdam, 2*a*, *b*.—Virginia affords rare facilities for studying this group, except the Acadian epoch, 2*a*, the existence of which along this portion of the Blue Ridge range has not been ascertained with any certainty. If it be here it is not characterized by fossil remains. Professor Rogers thus describes the rocks of this group: "The rock, or group of rocks, which is frequently exhibited in extensive exposures along the western side and base of the Blue Ridge, more especially in the middle counties of the valley, is usually a compact, rather fine-grained, white or yellowish gray sandstone, where resting on the declivity of the ridge, it presents a gentle inclination to the northwest—while the subjacent and more ancient strata of the ridge, in almost every instance, dip steeply to the southeast. In Page, Rockingham, Augusta, Rockbridge (and Botetourt) counties this group forms the irregular and broken range of hills lying immediately at the foot of the main Blue Ridge, and sometimes attaining an altitude little inferior to that of the main mountain. A level region, sometimes of considerable breadth, and strewed profusely with fragments of this rock, in general intervenes between the rugged hills and the first exposures of the valley limestones. . . . . In many instances two, sometimes three, ranges of hills are interposed between the limestones and what may be considered the termination of the rocks of the Blue Ridge." Of the higher beds of these sandstones—the more purely siliceous—he says: "In nearly all the exposures from Balcony Falls (in Rockbridge county) to Thornton's Gap (in Page county) as well as at various other places it exhibits vague fucoidal and zoöphytic impressions on the surfaces of bedding, together with innumerable markings at right-angles to the stratification, penetrating in straight lines to great depths in the rock, and from their frequency and parallelism determining its cleavage in nearly vertical planes. These markings are of a flattened cylindrical form, from $\frac{1}{8}$th to $\frac{1}{10}$th of an inch broad, giving the surface of the fractured rock a ribbed appearance, and resembling perforations made in sand which have been subsequently filled up, without destroying the distinctness of the original impressions" (pp. 167, 168). This is his account of the *Scolithus linearis.*

"At Harper's Ferry and for some distance south, where the altered character of the sandstones and slates of this formation renders it occasionally difficult to recognize the true dip, and even to identify the rock itself, an eastern declination is almost uniformly observed, the slates forming a portion of the bed of the Shenandoah, dipping beneath the sandstone which rises in bold cliffs along its eastern margin, while the limestone of formation II (3*a*, *b*), dips in the same direction beneath the slate, thus pointing to an inversion extending entirely through formation I, and even affecting to a great distance the rocks of formation II . . . . At Balcony Falls, where the western dips are preserved throughout a large part of the thickness of this formation, the most favorable opportunities are presented for studying the composition and marking the arrangement of these rocks. The formation here rests upon igneous rocks, chiefly of the syenitic character, which in this place form the main axis of the Blue Ridge. These are well seen in traveling along the tow path of the canal (now the R. & A. Railway) which follows the course of the river through the wild and beautiful gorge by which it makes its way from the valley eastward. As we approach the western termination of the pass, we mark the commencement of the rocks of formation I, which are seen on the side of the canal (railroad) lying on the syenitic mass with a northwest dip. The lowest stratum, or that in contact with the syenite, is a brownish decomposing slate, evidently much altered by its proximity to the igneous rock beneath; next is grayish and reddish sandstone; then a slate similar to the former, then a repetition of the sandstone, again a slate, and at the termination of the gap heavy beds of massive white sandstone, such as constitute the type of this formation. The average dip of the latter, as presented in the imposing cliffs which guard the entrance to the pass, is 55° N.W." (p. 205). The above account given by Prof. Rogers does not coincide entirely with the geology of this same mountain gorge, as presented in a paper published in this Journal, vol. xxix, June, 1885. The difference however, so far as regards the portion west of the axis of the Blue Ridge, is not in the general features but simply in the more *detailed* account given in the Journal. But as to what is east of the axis the case is quite different. We have fully ascertained as a settled fact that the beds of sandstones, conglomerates and slates for fully two miles east of the main mountain are of Cambrian age, forming a comparatively shallow syncline resting on Archæan rocks which are exposed along both of its margins, as shown in the paper referred to above. We have recently ascertained that this area cut off from the principal mass of the Cambrian beds, and thrown over to the eastern base of the mountain, extends at least ten miles northeast of James river towards the head waters of Pedlar river in Amherst county.

Some months before this volume of Rogers's Reports came into our hands, we examined another area on the east side of Blue Ridge at Tye River Gap in Nelson county, which is covered with alternating beds of conglomerates, slates and sandstones, considerably metamorphosed but bearing a striking resemblance to the *lower* Cambrian beds east of the mountain at Balcony Falls, which we suspected were of the same age. The only circumstance militating against that conclusion, was the absence of Scolithus borings in the sandstones. But this may be accounted for by the fact that these remains are rarely if ever found in the lowest beds in this region, and the additional fact that higher beds seem to have been swept away. More extended and minute research in this region may, however, yet prove the presence of Scolithus borings. In describing a section of rocks cropping out at Tye river Gap, Prof. Rogers says: "There are also noticed two bands of sandstone and slate. That on the western side occurs about one mile below the summit, is about 200 yards in width, and consists of gray and reddish slates, sometimes micaceous, with associated layers of sandstone, conglomerate, greenstone and greenstone slate. The greenstone is sometimes amygdaloidal.

These layers are horizontal, and with a moderate dip to the S.E. The other band occurs about ½ mile below the summit on the *east side*, and presents repeated alternations of red and gray slates, white sandstones and conglomerates. They are nearly vertical with a high dip to the S.E., and in width extend half a mile" (p. 313).

The foregoing would serve well as an accurate description of the lower beds of Cambrian age at several points we have observed along the Blue Ridge, with perhaps the exception of the amygdaloidal greenstone; but these beds, at least those on the east side of the mountain, are nowhere in the Reports referred to his formation I. Though on section No. 9, accompanying the published volume, we are pleased to find our suspicions verified, so far as the coloring of the section indicates Prof. Rogers's conclusions—arrived at no doubt from his latest revision of his earlier work. There is at least a probability of other portions of the eastern margin of the Cambrian rocks being cut off from their main mass by the elevation of the core of the Blue Ridge. At any rate, enough has been definitely determined to prove that the axis of this mountain does not everywhere mark out the eastern coast line of the great Paleozoic ocean, and that it will be worth our while to search for shore marks farther east than has been our custom heretofore.

Points convenient for studying the Cambrian group in Virginia have been incidentally mentioned already, and nothing more is called for under this head than a summing up of such localities as are readily accessible from railway stations. At Harper's Ferry, Prof. Rogers's section No. I appears to make the main mountain and the Short Hill some distance east of it, both Cambrian, with an uplift of Archæan beds between them. In his notes in Macfarlane's Guide he calls the rocks here, "Altered Cambrian 2*b*, or Archæan B, followed west by Cambrian 2*b*, 3*a*." Prof. Fontaine, in his notes for Macfarlane on the Baltimore & Ohio railroad, says: "The gorge at Harper's Ferry is cut through metamorphic rocks, of probably Huronian age. One-and-a-half miles west of the station, a fault brings down the Potsdam and Calciferous rocks against the Azoic." This is an interesting point, and worthy of further investigation. It seems to present another example of a belt of Cambrian rocks thrown to the eastern side of the Blue Ridge axis. We have not studied this portion of the formation with sufficient care to form a definite opinion of its peculiarities of structure.

The Manassas Gap railroad cuts the beds of this age between Happy Creek and Front Royal. The same formation is again well exposed at Thornton's Gap, a few miles east of Luray on the Shenandoah Valley railroad. There are numerous points on the same line from which good exposures of 2*a*, *b*, may be conveniently reached, as the road skirts the margin of the formation from Front Royal to Roanoke, a distance of 177 miles. Such points as Milnes, Crimora (near Turk's Gap), Waynesboro, Vesuvius, Loch Laird and Buchanan, are worthy of note. Then Balcony Falls, already mentioned, and Buford's on the Norfolk & Western railway in Bedford county.

VALLEY LIMESTONES—NO. II (*Canadian*, 3*a*, *b*, *c*).—Under this division Prof. Rogers in his State Reports embraces what is now recognized as the Canadian group (3*a*, *b*, *c*), and the Trenton Limestone (4*a*). But in his notes, prepared for Macfarlane's Guide, he gives the latter its true place. We know of no region in this country which offers a more extensive or better defined display of this important geological group than the "Great Valley of Virginia." This valley occupies a belt extending from the Potomac on the N.E. to the line of Tennessee on the S.W., a distance of about 340 miles, with a breadth varying from 15 to 25 miles, giving an area not much if any less than 6,000 square miles. We here quote Prof. Rogers. "The extensive zone comprehended under this title embraces all that portion of the State having for its eastern boundary the western slope of the Blue Ridge and its inflected continuation the Poplar Camp and Iron Mountains, and for its western, the Little North, and a portion of Big North Mountain, with the southern portion of the former, Caldwell's and Brushy Mountains; and, near its southwestern termination, . . . the extension of Walker's Mountain" (p. 203). . . . "From the Potomac southward nearly to the Rockbridge line, it presents the forms of two belts, separated by an intervening tract of variable width, formed of the next or several of the next superior formations. As far south as the northern termination of Massanutten range, the dividing band is composed exclusively of the group of slaty rocks constituting formation III, (4*b*, *c*). Farther south these mountains bear aloft still higher formations, resting upon a basis of the slates, and broadly skirted by them on either side; and again, after the termination of these ridges in the peak near Keezletown, the same zone of slate continues its course toward the south, gradually diminishing in breadth until it eventually disappears. Similar interruptions in the continuity of the limestone rocks are produced by the Short's Hill, Purgatory and other mountains toward the S.W. portions of the valley. . . . . The prevailing dip of the limestone along its eastern margin, where it joins the shales of formation I, is to the southeast. Indeed but few instances occur in the State in which this rock is seen reposing in its natural position upon these older strata. As might be inferred from the general prevalence of this inversion in the vicinity of the Blue Ridge, the natural causes by which it was brought about exerted a powerful influence upon the position of all the rocks of the valley, and in some instances would seem to have propagated a like inversion entirely across this zone and even into the rocks composing the mountains bounding it on the west." (pp. 209, 210).

Prof. Rogers nowhere in his State Reports undertakes to subdivide these limestones by the several horizons which he gives them in his notes for Macfarlane's Guide (see pp. 717, 720). Here he recognizes the three subdivisions, *3a*, *3b* and *3c*, which are tolerably well defined in the Great Valley.

We are aware that Prof. Dana in his "Table of Formations," as revised for Macfarlane's Geological Railway Guide, has reduced the sub-divisions of the Canadian group to two, Calciferous and Chazy, for which we have no doubt he can present very cogent reasons. In the Virginias this whole group is more remarkable for the paucity than for the abundance of its fossils; but still enough have been found to mark its place in geological history. Under the guidance, however, of organic remains alone, we have not been able to trace very definite horizons for separate epochs in this Canadian period, but when we call in the aid of lithological features as an additional basis of division, we find *three* tolerably well defined formations, blending into one another to some extent it is true, but not more so than is very common in contiguous and conformable limestone beds.

It is a fact worthy of note that what we call three formations of this period carry with them, throughout their whole extent of 340 miles, such well defined lithological peculiarities as to indicate clearly that they were severally deposited under quite different, though not suddenly changed, circumstances.

The lowest beds, which we regard as corresponding with the Calciferous (*3a*) of New York—though containing here far less of calciferous sandstone than of calciferous shale—is well characterized, as exposed in numerous ravines along the western base of the Blue Ridge, by heavy beds of purplish shales, with interstratified beds of dolomitic and siliceous limestones, like that employed at Balcony Falls factory for making hydraulic cement. The shales are both calcareous and ferruginous, but become more calcareous and less ferruginous as they are found to rise higher in the series.

In our notes on the Virginia railroads, prepared for a new edition of Macfarlane's Geological Railway Guide, we have retained the Quebec (Levis) (*3b*) as a distinct division because of the comparatively abrupt transition from predominating ferruginous shales on the one hand, to predominating ferruginous and dolomitic limestones on the other, on the surfaces of which we find in some localities numerous fucoidal impressions quite different from any found in the lower series. Here the limestone beds increase in thickness, in the regularity of their bedding, in their purity as regards silica and alumina, but they carry iron enough to produce by their disintegration dark, fertile soils. At many points, as may be seen between Charlestown and Luray on the Shenandoah Valley Railway, the interbedded shales become very much thickened at the expense of the limestones, but the general lithological peculiarities are well preserved at nearly all exposures of the beds along the Great Valley. Many of the limestones of this division are hydraulic, as at Shepherdstown on the Potomac, and on North River, six miles south of Lexington, at both of which points cement has been manufactured.

The transition from 3*b* to the upper division, (Chazy 3*c*), is not very sharply defined, yet we feel assured, in passing from the outcroppings of the lower beds to those of the higher, that we have come upon strata deposited under a decided change of circumstances. The beds consist of a larger percentage of carbonate of lime, but are less regularly stratified, and have fewer interstratified shales; a comparatively thin bed of brown sandstone full of impressions of fossil shells imperfectly preserved, appears among the lower beds, and is very continuous; then higher up and not far below the base of the Trenton limestone beds of chert of variable thickness are found—sometimes a single bed but often two or three. "In some of these slaty bands, and in the cherty beds so largely interstratified with the limestones of the valley, Goniatites, Ammonites and other remains are by no means infrequent, and when found are generally in a beautiful state of preservation." (p. 172).

THE TRENTON GROUP—NO. III of *Rogers*, (4*a*, *b*, *c*).—[*Trenton Limestone* was included among the "Valley Limestones" in the State Reports, but on p. 717 it is put in its true place]. Both the upper and the lower horizons of this group seem to be well defined throughout its whole extent in Virginia. The lower division, Trenton limestone (4*a*), in many places abounds in corals (Chætetes, Columnaria, etc.), while all the classes of Mollusca (Brachiopods, Lamellibranchs, Gasteropods and Cephalopods), have numerous representatives—often in great abundance. In the counties of Augusta, Rockbridge and Botetourt, the lowest beds of Trenton are evidently composed of the debris of an ancient coral reef with some admixture of mollusks and crinoids all cemented by infiltration of carbonate of lime into a solid limestone from 100 to 200 feet thick, of a fine gray color and highly valued for architectural purposes.

Between the Trenton limestones and the Utica shales (4*b*), there is no well defined plane of separation. The transition is manifested by a rapid diminution in the number and thickness of the fossiliferous limestones, and a corresponding increase of shales, until shales alone constitute the upper portion of the formation. The transition to the highest division, 4*c*, in the middle counties of the State is marked by changes in both lithological and paleontological features. The shales become more ferruginous as well as more siliceous. In fact some of the highest beds become really argillaceous sandstone of a purplish color with thin layers of similarly colored shales intervening. This part of the formation is in many portions of its range the repository of valuable ores of iron.

Prof. Rogers gives a very good view of some of the leading exposures of his No. III (4*b*, *c*.) "This member of the series consists of slates and slaty sandstones, of various shades of bluish black, lead color and yellowish brown, the dark varieties in general predominating. Their structure is laminated and fissile, not unfrequently evincing the presence of small quantities of mica. When weathered they in most cases assume a yellowish or dingy brown appearance. Usually this slate is devoid of carbonate of lime, though bands are occasionally met with containing organic impressions, and of a composition more or less calcareous. Iron pyrites is of very common occurrence, giving origin to the sulphurous impregnation of numerous medicinal springs taking their rise in these rocks. . . .

"Resting immediately upon the upper boundary of valley limestone, No. II (3*c*) this rock or group of strata is exhibited on a very extensive scale along the base and flanks of the Peaked Mountains and the Massanutten, and other parallel ranges in Shenandoah, Rockingham, Page and other counties. From the synclinal structure of most of these ridges, the slate is exposed on both sides of the mountain, dipping inward, that is, to the N.W. on the eastern side and S.E. on the western side. The striking symmetry of contour exhibited by the Peaked Mountains [southwest terminus of the Massanuttens], when viewed endwise from a point south of the termination of the range, illustrates the basin-shaped arrangement of the strata of slate resting in a trough of subjacent limestone, and surmounted by the sandstone which forms No. IV (5*a*) of our series. There are few, perhaps no other, exposures in the slate in which the structure and relations of these beds can be so satisfactorily observed, as in the group of mountains here referred to. . . . " (p. 174).

"It has been seen that this formation, constituting a broad belt extending from the Potomac far into the middle counties of the valley, constitutes a great synclinal trough, the northern portion of which stretches far into Maryland, sustaining for a considerable part of its length in Virginia, the group of long parallel ranges called the Massanutten Mountains. . . . Under like circumstances this formation is found constituting the trough-shaped basis on which the higher formations repose in the House Mountains, Short's Hill and Purgatory Mountains. In the House Mountains, the slate forming the base and extending about two-thirds of the height presents but gentle dips, so that the two successive formations III and IV (4*b*, *c* and 5*a*), of which they are composed, are piled upon the subjacent limestones in nearly horizontal planes." (p. 218).

"On the western margin of the valley a narrow belt of formation III extends from the Potomac along the base of Little North Mountain, to its termination in Rockbridge county. It then bends westward a little south of the gap through which flows the North River, and again resuming its course parallel to the Blue Ridge, spreads along the flanks of the main North Mountain and its continuations; . . . keeping this direction it skirts Caldwell's Mountain, and continues low down on the North or Brushy Mountain, to its termination in Washington county" [on the Tennessee line]. (p. 219).

In the Appalachians of the Virginias are some very interesting, and some of them quite extensive and fertile anticlinal valleys, of which the Canadian and Trenton limestones form the bottoms, while the Utica and Hudson slates constitute the lower slopes and spurs of the ridges by which the valleys are bounded. Such are the Crab Bottom in Pendleton and Highland counties, Warm Springs Valley in Bath county, Rich Patch in Alleghany, Sweet Springs Valley in Alleghany and Monroe, and Sinking Creek Valley in Craig and Giles counties.

The formations of the Canadian and Trenton periods are so frequently accessible to traveling geologists from the same points on railway lines and places of common resort, that we have concluded to note such localities under one general head. In the northeast portion of the Great Valley, the main stem of the Baltimore & Ohio Railway cuts the broad Silurian belt nearly at right angles. About one or two miles west of Harper's Ferry station the road passes rather abruptly from older beds to the Canadian limestones and shales, which are exposed in several cuts between that point and Kearneysville. Still farther west the more fossil limestones and shales of 4*a*, *b*, *c*, come into view as far as Martinsburg and beyond.

The Valley Branch of the Baltimore & Ohio railway runs for a short distance from Harper's Ferry on the west border of the Cambrian, but soon passes to the Canadian, group which may be conveniently observed at Charlestown and Cameron stations. At Wadesville, and along the banks of Opequon Creek above and below, the exposed rocks are of Trenton age—a part of the irregular synclinal belt which 25 miles farther towards the S.W. passes beneath the Massanutten range. Winchester, Woodstock, Harrisonburg, and Staunton are all near the western margin of the Trenton belt of the Massanutten range, and afford conveniently accessible outcroppings of 3*b*, *c*, and 4*a*, *b*. After passing Staunton this line cuts 3*b* and *c*, for five miles, then runs for a few miles on 4*a*; then before reaching Greenville it again runs upon 3*b*, *c*, which are well exposed in cuts and along streams to a point within six miles of Lexington, where 4*a* becomes the country rock and so continues to be until we reach the town, which is built upon Trenton limestone. The river cliffs for five miles above and below the railway bridge at this place, together with the hills surrounding the town, and the flanks of the neighboring House Mountains, afford unsurpassed facilities for studying both the Canadian and Trenton groups, and especially the latter.

The Manassas Gap railway, between Front Royal in Warren county and Strasburg in Shenandoah county, crosses the trough in which the Massanuttens rest at the extreme northeast terminus of that range of mountains, affording not only views of the Trenton foundation, but also of the superstructure of Medina and Clinton sandstones which form the crest of the historic Fort Mountain.

The Shenandoah Valley railway starts upon 4*a*, at Hagerstown, Md., crosses the Potomac on a bridge, the abutments of which are 3*b*. From this point to its terminus at Roanoke City, in Roanoke county, Va., its bed is composed almost entirely of Canadian, with a few patches of Cambrian and considerable areas of local drift. The road at some points affords opportunities for interesting observations. The cliffs of the Potomac at Shepherdstown, the cuts near Charlestown and the margins of both branches of the Shenandoah near Front Royal, bring into view well-defined beds of 3*b*, *c*. At Luray, the station and the greater part of the village appear to rest upon 3*b*, while the entrance to the caverns is, as far as we have been able to examine the locality, most probably in a hill composed of 3*c* (Chazy). Weyer's Cave, in the eastern corner of Augusta county, has about the same geological features and surroundings as the Luray caverns. Waynesboro is close to the line of connection between the Cambrian and the Canadian groups, and is a favorable point at which to study the structures of both, while the neighboring tunnel through the Blue Ridge shows the relation of the Archæan and Cambrian rocks with their actual contact at the western extremity of the tunnel, where apparent conformity exists between the two series. The Natural Bridge and the cliffs about Buchanan are both favorable localities for examining beds of 3*a*, 3*b*, and 3*c*—all being well exposed in the vicinity.

From Balcony Falls to Eagle Rock, in Botetourt county, the Richmond and Alleghany railway crosses the strike of the valley limestones in such a way as to open some well-defined sections.

But along the Lexington branch of the same road, the beds from the Cambrian to the Trenton are cut by the road and the river (the channel of which it follows) in such a way as to expose in a striking manner the numerous faultings and foldings to which this portion of the Great Valley has been subjected at some remote period of its history. A profile section from the crest of the Blue Ridge through Lexington and cutting the House Mountains, may be seen in this Journal, vol. xviii, 1879, p. 19. It is designed to show in a graphic way a variety of structures not often found within the same compass.

Our sketch has now brought us to the top of what is generally regarded as the Lower Silurian Age. We have dwelt upon this portion of the geology of Virginia at some length; first, because the formations of which we have been treating are remarkably well and widely developed in this State; and secondly, because we desire to give our fellow-students the benefit of a concise, and, as far as space and time will permit, a systematic view of the field which is still open for additional investigations. The higher formations we shall discuss hereafter.

Washington and Lee University, Va., September, 1885.

[To be continued.]

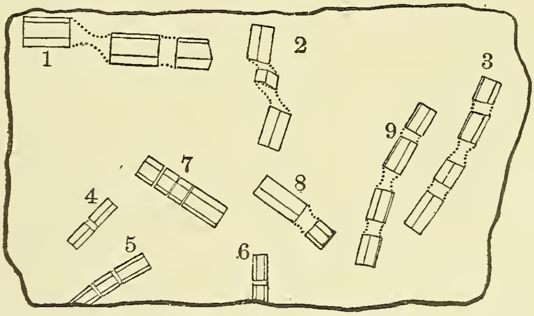
Art. XLVIII.—*On displacement through intrusion;* by James D. Dana.

The wedging action of intruding material may act either in fissures and crevices, or among the grains or constituent particles at or near the surface of a rock; and the displacement that results may consist in the opening and widening of fissures, in the separation of masses, in the faulting of the separated fragments, and in rock disintegration. The methods are either organic, molar or molecular. The *first* includes, (1) the intrusion of vegetable growths from microscopic plants to the roots and stems of trees; the *second* (2) the intrusion of melted rock into fissures or between layers; (3) the intrusion of gas under pressure, or when suddenly developed; and the *third* comprises (4) the freezing of intruded water, (5) chemical deposition from a solution of outside origin; (6) chemical deposition or composition, arising from changes within the rock. The minutest grain of a new mineral made or deposited within the rock has a displacing and disruptive action or tendency.

The production of a pseudo-breccia from quartzyte, described by me in this Journal for last November and January (xxviii, 451, xxix, 57), is an example of displacement by one of the last two methods here mentioned; that is, through deposition from either an introduced solution or from oxidation of inside pyrite (or other iron-bearing mineral). A specimen of this pseudo-breccia, from Canaan, Conn., nearly three inches each way, recently shown me by Mr. R. E. Cornish, of Norfolk, Ct., consisted on one side of small angular blocks of the quartzyte, irregularly displaced, and having the large interstices lined and partly filled with limonite; while on the opposite side, the quartzyte was solid excepting a few fine rifts occupied by limonite. The limonite in the specimen was so abundant that it must have been derived from an outside source, and the mass of quartzyte probably came from the gravel of a limonite deposit or some place where limonite-making was going forward. Under such a condition, the deposition of a coating of crystallized quartz over the limonite might come from the silica which is set free (and abundantly so) in the decomposition of the schist adjoining the limonite-making locality.

The interstices between the pieces of quartzyte are, as stated above, only *partly* filled; and hence open cavities are left. This condition results because the separation, even though wide, requires that the deposited mineral should cross the interstitial space only at one or two points; and when the limonite deposit in the cavities is a smooth coating, or botryoidal, or covered with drusy quartz, the cavity has become a small geode.

The specimen figured below exemplifies a somewhat different case of displacement by intrusion, coming under the 5th of the above mentioned methods, which is essentially the same with that of the pseudo-breccia in the method of applying the



divellent force. It is a piece of well-characterized mica schist of the muscovite variety, containing broken and displaced crystals of black tourmaline. It was obtained on one of my Berkshire excursions of last summer from a quartzyte ridge up Roaring Brook, on the eastern border of the town of Lenox.

The broken prisms of tourmaline are various in amount of displacement, the largest interval being about a third of an inch. Some of the displaced pieces are partially revolved, and some are tipped out of the plane of the rest of the crystal. The intruded mineral is quartz, a milk-white variety; and its form between the tourmaline fragments is shown by the dotted lines.

The displacement is evidently not wholly due to any compression of the rock, or to movement within it in any direction as a result of pressure or tension—the usual explanation in similar cases (and probable causes of fracturing); for the prisms of tourmaline lie, and are displaced, in all directions, and those that are side by side differ greatly in amount and kind of displacement as well as in its direction. The displacing agent was silica from an intruding siliceous solution; and deposition of quartz from the solution must have been continued at the plane of junction with the tourmaline until the displacements were completed. The change of direction and partial rotation of some of the tourmaline sections are evidence of irregular resistance to the displacement in the material of the rock.

In my note, on the "Decay of Quartzyte," in the number of this Journal for last January (xxix, 57), I refer to the "split and enlarged heads and stems of crinoids" as examples of the separation process. Being aware that crinoidal specimens of this kind had received much attention from Professor A. H. Worthen, Director of the Illinois Geological Survey, I wrote him recently on the subject, and here introduce his valuable letter of October 8th, received in reply.

"In reply to yours of the 5th inst., I will say that I know of no general report on the geodized fossils of the Keokuk limestone. At the St. Louis meeting of the Amer. Assoc., in 1878, I read a short paper on this subject, and exhibited specimens both of crinoids and shells that had been subjected to the geodizing process. No copy however was furnished for publication. I have seen one specimen of a geodized stem of Barycrinus nearly or quite a foot in diameter and have specimens in my own possession from four to six inches in diameter. Where shells have been geodized their size is not usually increased so much, and I do not now call to mind any specimen where the diameter has been increased more than half its original size. The stems of Barycrinus have a pentangular central perforation which seems to render them peculiarly susceptible to this process. The stems of large specimens of Barycrinus vary from one to two inches in diameter, and increase by the geodizing process from four to six diameters."

The process here described is of wide geological importance. As the cases show, any intruding mineral may act like the quartz and limonite, in the disrupting, and displacing work. The limit of the width of separation that may thus be occasioned remains to be ascertained from further observations.

ART. XLIX.—*An Endoparasite of Noteus;* by SARA GWENDOLEN FOULKE.

IN classifying the *Ciliata-Holotricha*, W. Saville Kent has created a special division for those members of the order which possess no distinct oral aperture, distinguishing them as the *Holotricha-Astomata.* This division includes but one family group—the Opalinidæ—comprising four genera: *Opalina*, *Anoplophrya*, *Haptophrya*, and *Hoplitophrya.* The Opalinidæ are, without exception, endoparasitic in habit.

Of these four genera the characteristics may be summed up as follows: *Opalina* and *Anoplophrya* are both free swimming, without means of attachment, and differ chiefly in the form of their endoplast; *Haptophrya* and *Hoplitophrya* are both furnished with means of attachment, the difference in form of which furnishes the generic distinction, the former possessing a sucking disc, the latter a corneous keel-like band, or one or more hooks. *Opalina* proper is further separated by restricting its habitat to "the intestinal viscera of various tailed or tailless Amphibia."

A *Noteus*, species unknown, having been crushed in the live-box, there were expelled from the animal's body, with its fluids, ciliated bodies exhibiting decided movements. Scarcely more tangible in their colorless transparency than air-bubbles, these bodies, varying in shape from globose to ovate, were more or less uniformly clothed with long, delicate cilia, whose rhythmical undulations produced but slight onward motion. No endoplasm was visible, and no opportunity for the use of reagents was afforded, as, in about ten minutes, the bodies became quiescent, and then rapidly disintegrated, the cilia disappearing first. Dr. Jos. Leidy recommends as a successful medium of preservation for such forms a little white of egg introduced into the water, which is not of itself sufficiently dense to support so delicate cell-walls.

One of the forms was gourd-shaped, the constriction being slightly above the middle, the whole appearance strongly suggesting lateral fission. Another, perfectly globose individual contained a number of the refractive germ-like bodies characteristic of the Protozoa, which, on being liberated by the dissolution of the parent cell, dispersed through the water, probably to seek a new host and complete the cycle of development. On the globular form the cilia appeared to be placed in bands or clusters, while those on the ovate form were more evenly distributed. It is possible that one may be merely an immature form of the other.

Simultaneously with these parasites a sac of protoplasm, measuring only $\frac{1}{1000}''$, and containing ten minute, scarlet to dark red bodies, was expelled. It seemed to come from near the center of the forward part of the body, but was not connected with the "eyes," as these remained intact. This sac remained motionless near the Rotifer for an hour, the scarlet bodies continuing in incessant motion during that time, but no change of any kind taking place. I have been unable to determine the nature of this sac or the contained bodies, and should be glad of any information as to its probable character.

The parasites measured about $\frac{1}{600}''$, exclusive of the cilia, whose length more than equalled the diameter of the body. I believe them to have come from some one of those cavities of the Rotifer's body which are filled with clear, rather thin fluid, perhaps from the stomach, but think it unlikely they can have come from the intestinal canal, because of their extreme fragility, and of the very long investing cilia, making the total size too great for such confined quarters.

The characteristics above noted bring this form within the genus *Anoplophrya*, if we except the inconspicuousness of the endoplasm, supposing it present, but prevent its identification with any specific form there included, that to which it most nearly approaches being *A. socialis*, described by Dr. Leidy, under the name of *Leucophrys socialis*, as present in the fresh-water Polyzoon, *Urnatella gracilis*. From *A. socialis* it differs, however, in point of size, being but one-sixth that of the latter; in not having the cuticle striate, and in the superior length of its cilia.

I propose to name this new species *Anoplophrya Notei.*

Briefly stated, the specific characteristics of this form are as follows: body globose or ovate, variably clothed with cilia more than equalling its length; endoplast undetected; contractile vesicle small; length, $\frac{1}{600}''$. Hab., endoparasitic of *Noteus.*

Art. L.—*The Spectrum of Nova Andromedæ;* by O. T. Sherman.

When the telegram announcing the outburst in the nebula of Andromeda arrived, the equatorial of the Yale Observatory was in use following the spectra of γ Cassiopeiæ and β Lyræ. For some time past the astronomer had been engaged upon them and was beginning to feel sufficient confidence to commence the record. This practice is of more than apparent importance as it underlies the whole work with the nebula. As precaution in observing, the eye was used only after some hours sleep, the spectrum was gazed at intently for some moments and subjected to a slight apparent to-and-fro motion. No light more brilliant than the first glare of a match was used. The spectroscope was a direct vision by Duboscq, provided with two series of prisms. Using the single series, b_3 and b_4 are just separated, using the double series the nickel line between the D_1 and D_2 and the fine lines between b_1 and b_2 are visible. The single series was usually employed for work, the double as a check.

On the earliest opportunity after receiving the information, namely the early morning of September 5th, the telescope was directed to the Nova. On the whitish blue stripe forming the continuous spectrum were discerned one bright line crossing the spectrum and farther toward the red two bright knots of light. They were not very much brighter than the surrounding glare but were evident. They remained when the larger dispersion was employed.

Settings were made by bringing the cross wire appearing as a dark line upon the bright background over the apparent condensation. The scale was read by the glare of a match and the observation recorded by a pencil dot upon a prepared form. The instrument was then pointed to the comparison star, say γ Cassiopeiæ and a similar observation made. This process was repeated independently upon Sept. 5, 7, 9, 11. On Sept. 16 and 21 the lines were seen but no setting attempted. Since then no observations upon the Nova have been obtained. The mean of the settings upon the three stars gives the following series:

	γ Cass.	β Lyræ.	Nova Androm.
Hα	20°·2	20°·32	
D_3	19·35	19·44	
?	19·10	19·03	18·98
?	18·5	18·43	18·51
Hβ	17·56	17·51	17·55
Hδ	15·93	15·90	
Hγ?	14·20	14·25	

The lines Hα D3 Hβ Hδ have been identified by other observers. On their foundation the curve connecting the scale readings and wave lengths was drawn. The equivalents of the scale readings read off are:

	From γ Cass.	From β Lyræ.	Mean.
18·51 =	536·0	527·0	531·5
18·98 =	564·0	551·0	557·5

The remaining line, Hβ seems from its image to be due to the light of the whole nebula. The two 18·51 and 18·98 seem to be due to the light of the Nova.

It is of interest to compare the wave lengths obtained above with the wave lengths of the coronal lines 1474 and 1250 ± 30, lines which also appear in the spectrum of the solar protuberances. The wave length of 1474 is 531·59, that of 1250±30 is 556·05±3·44.

While it may be too much to claim that these measures afford an absolute proof of identity, yet certainly the strong suspicion is not without interest, importance and suggestiveness.

Although it has no immediate connection with the subject of the present article, it may not be out of place to note the presence of these 18·5 and 19·0 lines in the spectra of γ Cassiopeiæ and β Lyræ. Concerning these spectra we hope to speak later.

Observatory of Yale College, Oct. 1, 1885.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the determination of the Specific Density of liquids at high temperatures.*—R. Schiff has described a convenient form of apparatus for determining the specific density of liquids at high temperatures, based on the principle of the weight thermometer. The pyknometer itself consists of a cylindrical glass vessel of 8 to 10 c. c. capacity, with a conical neck terminating in a fine opening. Upon this conical neck a recurved glass cap or helmet is ground air tight. The instrument is placed in a funnel-shaped vessel, the tube of which passes through the cork of a flask placed beneath it, containing the liquid by means of whose vapor the apparatus is to be heated. At top the funnel has three openings; through one the excess of vapor passes to an inverted condenser, through the second, a thermometer is inserted; and through the third, which is central and larger than the others, passes (1) a wire holder to support the pyknometer, and (2) a glass stem to which the cap or helmet is attached and by means of which this may be made to close the opening in the specific density flask. To make an observation, the flask is filled with the liquid to be examined, a small glass plug being used to close it during the weighings, and is supported within the funnel. As the temperature rises, the liquid expands and overflows into the recurved portion of the helmet. When the temperature has become uniform, the instrument is removed from the vapor, allowed to cool and weighed. The difference between the first and second weighings gives the overflow. This divided by the weight of the substance remaining gives the expansion of the substance per weight-unit. And, knowing the volume of the flask at 0° and its expansion-coefficient (determined by making two experiments with mercury, one in ether vapor at 34° and the other in steam at 100°), the specific density is readily calculated. —*Ber. Berl. Chem. Ges.*, xviii, 1538, June, 1885. G. F. B.

2. *On the direct union of Bromine with Chlorides, forming a new class of Perbromides.*—In a series of experiments upon the reaction which takes place between bromine and certain chlorides, BERTHELOT has observed the formation of a new group of secondary compounds produced by the direct addition of bromine to these chlorides, in strict analogy to the formation of the alkali perbromides. When for example fuming hydrogen chloride, of specific density 1·153, is mixed with bromine, 100 c. c. dissolve within a few minutes 36·4 grams of the bromine; increasing finally to 40·1 grams. The ratio between the two is 36·5 to 40·1; which corresponds to the formula $(HCl)_2Br$. This fact, as well as the calorimetrical one that the heat evolved in the reaction is double that evolved when bromine is dissolved in water, proves, in the author's opinion, the formation by direct union of a bromide of hydrogen chloride, comparable to hydrogen perchloride. Analysis showed that at most only one three-hundredth of one equivalent of the chlorine had been replaced. This fact as well as the ready dissociation of the compound shows that the bromine has acted by direct union and not by substitution. Hydrogen chloride dissolves iodine also in much larger proportion than water, one liter dissolving 6·04 grams; thus indicating the formation of a similar periodide. A solution of barium chloride containing 450 grams of the crystallized salt to the liter dissolves 115 grams bromine, nearly four times the quantity dissolved by the same quantity of pure water. The heat evolved is 0·5 calory for Br liquid = 80 grams. The solution may be diluted with its own volume of water without throwing down the bromine. Only one hundredth of an equivalent of the chlorine suffered replacement in this experiment. A solution of strontium chloride containing 400 grams of the crystallized salt to the liter, dissolves 92 grams of bromine, three times as much as pure water. The same kind of absorption has been observed with silver chloride.—*Bull. Soc. Ch.*, II, xliii, 545, June, 1885. G. F. B.

3. *On the Reduction of Carbon dioxide to monoxide by Carbon.* NAUMANN and PISTOR have studied the conditions, especially of temperature, under which carbon dioxide is reduced to monoxide through the action of carbon upon it. In the first five experiments (which were conducted at comparatively low temperatures in order to determine the lower limit at which the reduction took place) a combustion tube 82 cm. long and 15 mm. in diameter, was used, filled with fragments of charcoal previously washed with acid and then strongly ignited. Through this tube the carbon dioxide, prepared from marble and hydrogen chloride and carefully dried, was passed at the rate of 2 or 3 bubbles per second; the tube being heated in a gas furnace, and being surrounded with two concentric sheet iron tubes enclosing wire spirals, for the purpose of rendering the temperature more uniform. In order to measure the temperature, a number of capsules of glass or platinum, containing salts of different melting points and for

the higher points, metallic spirals, were distributed at intervals within the tube. If at a given time one of these salts is observed to be melted and another not, the temperature of the tube obviously lies between the two melting points. In the first experiment, in which the temperature never reached 530° (since silver iodide, fusing point 530°, was never melted and even lead chloride, fusing point 501°, was softened only in the middle of the tube) the layer of charcoal being 66 cm., the evolved gas during the entire hour was completely absorbed by potassium hydrate; showing that no reduction had taken place. In the second, in which the temperature remained below 585° (the fusing point of silver pyro-phosphate) a minute quantity of non-absorbable gas, burning with a blue flame, was obtained. This temperature 530° –586° marks the limit of the reduction. In the third, silver iodide was completely fused, silver pyrophosphate sintered together, but lithium chloride (fusing point 602°) remained unaltered. The evolved gas contained 12·6 per cent carbon monoxide. In the 4th experiment the layer of charcoal was reduced to 10 cm. and it was found that no action took place even at 602°; so that in the fifth experiment, carbon monoxide gas was observed only when the temperature lay between the melting point of potassium iodide (634°) and that of potassium bromide (703°). The glass tube was then replaced by one of porcelain 8 mm. in diameter, which was heated in a Fletcher gas furnace, using an air blast. In the sixth the carbon layer being 30 cm. long, the temperature did not reach 814° (the fusing point of Na_2CO_3) 90 c. c. of gas were collected in 16 minutes and the CO present in the evolved gas was 12·7 per cent. In the 7th, with a layer of charcoal 24 cm. long, and the temperature about 861° (the fusing point of Na_2SO_4) the gas evolved at the rate of 1·7 bubbles per second consisted of 58·7 CO and 41·3 CO_2. In the 8th, the carbon layer was only 10 cm. long, silver was melted, (954°) but copper was not (1054°) and the gas (1·2 bubbles per second) consisted of 94·2 CO and 5·8 CO_2. In the 9th, the carbon layer was 57 cm. long, the temperature above 861° but below 954°, the gas passed at the rate of 3 bubbles per second and contained 90·7 per cent of CO. In the 10th experiment a platinum tube 5 mm. in diameter was used, the carbon layer being only 2 mm. long. In 4·25 minutes 70 c. c. of gas were collected (about 3 bubbles per second) of which 18·1 per cent was CO. In the 10th experiment, the tube was filled with alternate layers of charcoal and of copper turnings, the former amounting in all to 10 cm., the latter to 34 cm. The temperature in the greater part of the tube was between 861° and 954°. The gas passed at a rate of 2 bubbles per second and contained 36·5 per cent of CO. From these experiments the authors conclude: 1st, that the reduction of carbon dioxide to carbon monoxide by carbon has its lower temperature-limit at about 550°. 2d, that if according to theory, the reduction at a given temperature is proportional to the frequence of the contacts between the carbon dioxide and the carbon, then an increase in the length of the carbon layer or a decrease in the velocity of the gas-current, should increase the amount of monoxide produced, as is shown in the 7th and 9th experiments. 3d, that the reduction increases with the temperature. The paper concludes with a discussion of the thermochemical questions involved in the results obtained.—*Ber. Berl. Chem. Ges.*, xviii, 1647, July, 1885. G. F. B.

4. *The Decomposition of Carbon dioxide by the Electric spark.*—In consequence of an observation showing that no explosion is propagated by a spark in a mixture of carbon monoxide and oxygen which has been dried over phosphoric oxide, made by Dixon, he has undertaken, assisted by Lowe, the study of the action of the electric spark upon carbon dioxide when similarly dried. The gas carefully dried over phosphoric oxide, was placed in a eudiometer and, by means of a chain composed of short pieces of platinum fused into small glass beads, was submitted to the action of a series of induction sparks. The amount of decomposition varied from time to time, but approached no fixed limit; the maximum decomposition being about 45 per cent under 100 mm. pressure. Placing a condenser in the secondary circuit, diminished the effect. To test the effect of varying the length of the spark, a eudiometer was next employed which had two pairs of platinum wires, the ends being 1·5 mm. and 6 mm. apart respectively. On passing the spark through 52·10 c. c. of the perfectly dried gas, between the former terminals, the volume began soon to increase, attaining finally 62·34 c. c.; showing a decomposition of 39·08 per cent of the CO_2 into CO and O. In a second experiment in which the sparks passed continuously for six hours, the decomposition was 33·97 per cent. When the spark passed through the wider space for five hours, the decomposition was 30·20 per cent. Returning to the shorter spark the coil was left running over night with one cell of battery. In the morning it had stopped and the decomposition was 43·33 per cent. In these experiments the pressure was about 500 mm. and the temperature varied from 10° to 15°. It is therefore clear that the shorter spark produced the greater decomposition. Since the variations of volume due to changes in the nature of the electric discharge, mask the effect of varying pressures, a differential process was resorted to. Two similar eudiometers were prepared and fitted with wires made of platinum-iridium alloy, each terminating in a 2 mm. ball, the distance between them being the same in the two tubes. When a series of sparks was sent simultaneously through both tubes containing dry CO_2, the effect was the same in both, the volumes varying together so long as the pressure was preserved constant. The greater decomposition was produced by the feebler spark and under the less pressure. When 100 volumes of dry CO_2 were placed in one tube and 150 volumes of a dry mixture of carbon monoxide and oxygen were placed in the other, and a series of sparks from the same coil was passed through both, the volume of CO_2 increased and that of the CO

and O diminished until after some hours they became equal under the same pressure. On continuing the spark the volumes in the two tubes altered together, increasing or diminishing as the nature of the spark varied. Since a white-hot coil of platinum wire produces no permanent alteration of volume in dry CO_2; and since Deville has shown the dissociation of CO_2 in contact with white hot platinum; it follows that the dissociated CO and O must at once reunite under these conditions. Hence the author predicted that a white hot platinum wire would produce complete combination of dry CO and O, even without explosion. On heating the platinum coil to redness in the carefully dried mixture, it at once glowed intensely and in a few minutes complete combination was found to have taken place. From this result the authors conclude that while hydrogen unites with chlorine and iodine directly, carbon monoxide and oxygen require an intermediary to effect their combination.—*J. Chem. Soc.*, xlvii, 571, August, 1885.

G. F. B.

5. *On the direct synthesis of Benzene derivatives by the action of Potassium on Carbon monoxide.*—Nietzki and Benckiser have repeated Lerch's experiments on the explosive compound of potassium and carbon monoxide which is formed in preparing this metal, and have proved that the derivatives he obtained from it, trihydrocarboxylic, dihydrocarboxylic and carboxylic acids are identical with the hexaoxybenzene, tetraoxyquinone and dioxydiquinoylbenzene respectively, already described by them. The carbonyl-potassium was obtained by passing a current of CO, completely dried and freed from oxygen, over potassium heated to melting in a combustion tube. At the close of the experiment the potassium had increased in weight about 70 per cent; confirming Brodie's view that one molecule of CO was absorbed for each atom of K. The product was a solid grayish mass with here and there patches of a red-brown, green or black color. After cooling, the tube may be filled with strong alcohol without danger of explosion. On treating the crude product with hydrogen chloride, hexaoxybenzene $C_6(OH)_6$ is produced, which proves that the carbonyl-potassium is most probably $C_6(OK)_6$ or potassium-hexaoxybenzene. The former is accompanied however by tetraoxyquinone, its first oxidation product from which it may perhaps be formed during the solution in HCl, by the reducing action of the K still present upon the corresponding potassium compound directly produced: $(CO)_6+K_4=C_6(OK)_4O_2$. To ascertain this the freshly-prepared carbonyl-potassium was dissolved in acetic oxide and precipitated by water. The brown precipitate crystallized from glacial acetic acid gave the characteristic hexaacetylhexaoxybenzene. The residue after treating the crude product with alcohol is a dark green powder, which turns red in the air, and which boiled with HCl dissolves with a brownish red color. On cooling the solution deposits stellate groups of steel-blue needles of tetraoxyquinone $C_6(OH)_4O_2$. If the crude product be washed with dilute alcohol it turns red and finally

produces an ochre-red powder, the so-called rhodizonic acid. This the authors find to be identical with their dioxydichinoylbenzene $C_6(OH)_2O_4$.—*Ber. Berl. Chem. Ges.*, xviii, 1833, July, 1885.

G. F. B.

6. *Composition of Ocean water.*—MR. W. DITTMAR has given (Proc. Phil. Soc. Glasgow, vol. xvi, Dec. 3, 1884) a summary of his results obtained in the investigation of the specimens of ocean water brought home by the Challenger expedition. We cite briefly from this abstract. Mr. Dittmar remarks in the outset on the small change in the constitution of the solids in solution in the ocean produced by the contributions of rivers, stating that the estimated annual amount from the thirteen principal rivers of the earth, $1{\cdot}337 \times 10^9$ tons (of 1000 kilos each), when compared with the amount in the ocean water of dissolved solids (46,280 billion tons in the 1·3 trillion tons of water), is so small that it would take 30,000 times as much to tell distinctly on the most exact sea-water analysis which could be made by our present methods.

The water was found to contain an appreciable amount of base over and above that which would neutralize the two principal acids, and this is in the state of *carbonate;* and part of this carbonate must be carbonate of lime. From the mean results of 77 analyses, this surplus base per 1,000 grams of ocean water is equivalent to 0·25 grams of carbonate of lime; but this number may need a correction, considered beyond. The following table gives the results of Mr. Dittmar's 77 analyses:

Average composition of Ocean-water Salts.

	Per 100 parts of Total Salts.	Per 100 of Halogen calculated as Chlorine.	
	Dittmar.	Dittmar.	Forchhammer.
Chlorine	55·292 } *	99·848	Not determined.
Bromine	0·1884 }	0·3402	Not determined.
Sulphuric acid, SO_3	6·410	11·576	11·88
Carbonic acid, CO_2	0·152	0·2742	Not determined.
Lime, CaO	1·676	3·026	2·93
Magnesia, MgO	6·209	11·212	11·03
Potash, K_2O	1·332	2·405	1·93
Soda, Na_2O	41·234	74·462	Not determined.
(Basic oxygen equivalent to the halogens)	(−12·493)	....	....
Total salts	100·000	180·584	181·1

Combining acids and bases, we have (Dittmar)—

Chloride of sodium	77·758
Chloride of magnesium	10·878
Sulphate of magnesium	4·737
Sulphate of lime	3·600
Sulphate of potash	2·465
Bromide of magnesium	0·217
Carbonate of lime	0·345
Total salts	100·000

* Equal conjointly to 55·376 parts of chlorine, which accordingly is the percentage of "halogen reckoned as chlorine" in the *real* total solids.

Reducing to the absolute mass of the ocean as given above, we arrive at the following numbers :

Absolute composition of the Salts of the Ocean.

UNIT=1 BILLION=10^{12} TONS.

Chloride of sodium	35990
Chloride of magnesium	5034
Sulphate of magnesium	2192
Sulphate of lime	1666
Sulphate of potash	1140
Bromide of magnesium	100
Carbonate of lime	160
	46283

Total bromine	87·2 (W. D.)
Total iodine	0·03 (Köttstorfer.)
Total chloride of rubidium	25·0 (C. Schmidt.)

Mr. Dittmar adopted Tornoe's expression numerically for the *alkalinity* of the water by stating the number of milligrams of CO_2 which would convert the surplus base into normal carbonate, and referring it to one litre of sea-water analyzed, or—to get rid of degree of salinity—referring it to 100 parts by weight of total salts in the water, or to 55·42 parts of halogen counted as chlorine. The statement that the alkalinity = ·154 signifies that per 100 parts of total salts the water contains 0·154 parts of CO_2 as normal carbonate, or rather in the $R''O,CO_2$ part of the carbonate as formulated. The alkalinity ranged from 0·140 to 0·164, with a tendency to the highest results in the bottom waters. One cause of more lime carbonate in the bottom waters is found in the shells of dead crustaceans, etc., over the bottom. Some free CO_2 also may be present.

The *salinity* of the waters is oceanographically a function of the geographic position, depth, and time. Mr. Dittmar determined, from comparisons of ascertained specific gravities with chlorine determinations, that the per-millage of chlorine in an ocean water at a given temperature is proportional to the excess of its specific gravity above that of pure water at the same temperature. This is expressed in the formula,

$$ {}_4S_t - {}_4W_t = \chi(a + bt + ct^2), $$

where ${}_4S_t$ denotes the specific gravity of sea-water, and ${}_4W_t$ that of pure water at t°, both referred to pure water at $+4^\circ$ C. as =1000, while a, b, and c are constants having the values

$$ a = 1{\cdot}45993,\ b = -0{\cdot}0055922,\ c = 0{\cdot}0000649. $$

The results agree closely with the determinations of specific gravities of Challenger waters by Mr. Buchanan, of the Challenger expedition.

The *atmospheric constituents* of the waters were also studied. Buchanan followed Jacobsen's method in collecting the gases from the waters. Mr. Dittmar found that in the surface waters the oxygen and nitrogen vary in amount from temperature, but the waters nowhere contain more than 15·6 c.c. of nitrogen or more than 8·18 c.c. of oxygen per liter; and that the nitrogen never falls below 8·55 c.c. The oxygen is diminished below its theoretical minimum 4·30 c.c. by the processes of life and organic decomposition and other oxidation changes. In waters at depths the variation in amount of oxygen is greater, because of the feeble source of supply and the losses from oxidation. No deep-sea water was found, in the analyses, wholly without free oxygen, and this "confirms the conviction that absolute stagnation nowhere exists in the ocean, not even at its greatest depths." But in one sample the amount of cubic centimeters per liter was found to be 15·08 N_2 and 0·6 O; in another 13·38 N_2 and 2·04 O; "indicating approximate rest at these two places at any rate." At the ocean's surface the equilibrium in the absorbed nitrogen and oxygen is maintained by the atmosphere.

As to *carbonic acid in sea-water*, all the waters were found to contain it, and those from the deep sea were particularly rich in it. The analyses prove that "if sea-water anywhere contains free CO_2 at all, it amounts to little in comparison even with the combined CO_2 of the carbonates." But sea-water may give off CO_2 even when only enough is present to make all the lime bicarbonate. Experiments showed that the loss may continue until there remains only what is necessary to make the lime a sesquicarbonate. "In surface waters the proportion of carbonic acid increases when the temperature falls, and vice versa;" and "within equal ranges of temperature it seems to be lower in the surface water of the Pacific than it is in that of the Atlantic Ocean." Free carbonic acid is, however, occasionally in rather large proportion, 22 out of 195 waters analyzed by Mr. Buchanan having their surplus base at least fully saturated, and one, although a surface water at 25·1° C., contained as much as 41 milligrams of free CO_2 per liter."

The total weight of potential carbonate of lime in the ocean amounts to 160 billion tons, equal to 70 such units of CO_2; but as the CO_2 is present as partially saturated bicarbonate, 105 billion tons may be accepted as a rough approximation. The source of this CO_2 Mr. Dittmar supposes to be from submarine volcanic action. As volcanoes get the CO_2 by decomposing limestone about the conduit, it would in that case come from the calcareous material of the ocean's depths, or from the earth's crust beneath, set free by volcanic heat.

The total weight of loose CO_2 in the ocean is at least 15 times greater than that of the free CO_2 in the air. If the amount of lime carbonate contributed by all the rivers of the world is assumed to equal the total solids which the thirteen rivers before named contribute, $1·3375 \times 10^9$ tons, it would take them 1194 years to increase the amount in the ocean one per cent of its present value, the sum total of the carbonate of lime of the ocean being 160 billion of tons.

II. Geology and Mineralogy.

1. *On the development of Crystallization in the Igneous rocks of Washoe, Nevada, with notes on the Geology of the District;* by Arnold Hague and J. P. Iddings. Bulletin No. 17 of the U. S. Geol. Survey. 44 pp. 8vo. Washington, 1885.—This memoir is one of the most important and wide-bearing that has come from the U. S. Geological Survey. It is "the result of an investigation of the extensive and well-selected lithological material collected by Mr. G. F. Becker during his examination of the geological lode;" and illustrates the fact—doubted and denied by many geologists—that the rock of an igneous eruption may vary in crystallization from true lavas of glass-containing igneous rocks in an upper or outer region, to completely crystalline kinds, granite-like in texture, in a lower or inner region.

The grand section through igneous rocks—over four miles long—afforded by the Sutro tunnel, and the vertical sections in shafts 2,000 to 3,000 feet deep, make the Washoe region especially favorable for such an investigation. The great differences between the rocks of the territory led Mr. Becker, in his report, to recognize, in accordance with German teaching, two groups among them, a Tertiary and a pre-Tertiary; and the same distinction was made out in his study of the rocks met with on descending in the mine, the lower being of the older division. The specimens collected in his careful investigation of the region number more than 2,000, 600 of them from the surface rocks, and over 1,400 from underground. The rocks made out by him were granular diorite, porphyritic diorite, micaceous diorite-porphyry, quartz-porphyry, earlier-diabase, later-diabase, earlier-hornblende-andesite, augite-andesite, later-hornblende-andesite, basalt.

Messrs. Hague and Iddings, after a microscopic study of thin sections of the various rocks (over 500 in number), have obtained the following results.

The study of the *diabase* and *augite-andesite* (of which there were 140 thin sections) shows, *first*, "the absolute identity," in nature and occurrence, of the mineral constituents of the two rocks, so that many kinds under each are not distinguishable; *secondly*, a gradual transition from a microlitic glassy groundmass to a holocrystalline; and from microscopically fine-grained to coarse-grained and porphyritic varieties. These two points are established by finding imperceptible gradations, in all respects, between the diabase and augite-andesite. A study of the successive rocks along the Sutro tunnel sustains the conclusions that the glass-bearing rocks are the exterior; that the degree of crystallization increases inward; that the two kinds of rocks here considered are for the most part indistinguishable.

The *granular-diorite*, at the head of the Sutro tunnel, was proved to belong with the pyroxene rocks. The same rock from Mt. Davidson and Ophir Ravine was found to be identical with the coarse-grained diabase.

Porphyritic diorite and *Earlier-Hornblende-andesite.* All shades of gradations between these two rocks occur. Transitions from glassy varieties to the most crystalline are well seen about Mt. Davidson and elsewhere. A comparison of sixty thin sections failed to make out any distinguishing features. Some of these *Earlier* Hornblende-andesites are glass-bearing.

Mica-diorite and *Later Hornblende-andesite.* For the latter rock the name hornblende-mica-andesite is stated to be better, as the mica is an essential ingredient, and distinguishes it from the earlier-hornblende-andesite. But from the varieties of mica-diorite it is indistinguishable; there is great variation in macroscopic aspect and this makes more forcible "the absolute identity of the two rocks." The coarsest variety of the mica-bearing diorite is "*thoroughly granite-like in all its mineral constituents*," while at the other end of the scale of gradations there are kinds having a glassy ground-mass crowded with microlites, and these are the surface kinds.

Quartz-porphyry, *Dacite* and *Rhyolite.* These rocks were made pre-Tertiary by von Richtofen and Becker, and Tertiary by Mr. Clarence King; and the latter conclusion is that of Messrs. Hague and Iddings. They are for the most part rich in crystals of quartz, feldspar and mica, with some hornblende, but the dacite contains less quartz and less hornblende. The examination leads to the conclusion, that while there are great variations in texture, color and density, there exist no petrographic distinctions between them.

Younger-Diabase, Black Dike, Basalt. The younger-diabase, a pre-Tertiary rock, occurs as a narrow dike for more than a mile along the lower levels of the Washoe mines; its upper part makes the Black Dike. A comparison of thin sections of the surface basalt and the diabase shows that there are no petrographic distinctions between these olivine-bearing rocks.

The comparisons of these various rocks as regards chemical composition—using analyses in Becker's work, with three new ones—sustains the close similarity of the augite-andesites, hornblende-andesites and hornblende-mica-andesites. The silica in augite-andesites and hornblende-andesites varies most in percentage, but only from 56·40 to 60·82. The hornblende-mica-andesites and mica-andesites have 63·30 to 65-68 per cent of silica. Silica determinations of five pyroxene-andesites and hornblende-andesites from other localities (near the base of Mt. Davidson), give for the silica 55·66 to 57·06 per cent, showing remarkable uniformity.

The authors conclude:

That the degree of crystallization developed in igneous rocks is mainly dependent on the conditions of heat and pressure under which the mass has cooled, and is independent of geological time. The same magma has given origin to the most glassy and the most granite-like kinds, the most modern-looking and the most ancient-looking, the glassy kinds occurring at or near the surface, while those at considerable depths cooled slowly and became wholly crystalline in texture.

That all the Washoe regions are of Tertiary age.

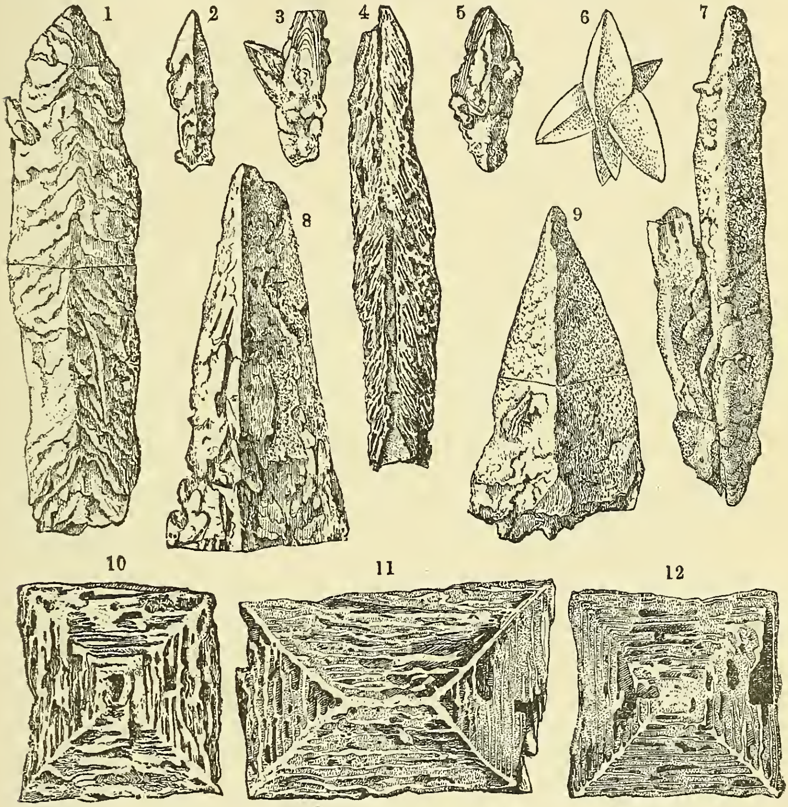
That the so-called granular diorite, diabase, and augite-andesite are identical and belong to the same geological body; that the porphyritic diorite and hornblende-andesite are identical, and should bear the latter name; that the so-called mica-diorite is identical with the later-hornblende-andesite; that the quartz-porphyry resolves itself into both dacite and rhyolite; that the later-diabase and the basalt are one in rock, the former known as a dike and the latter in flat-topped masses.

That the Comstock lode occupies a fissue along a line of faulting in a rock of Tertiary age, and cannot be considered as a contact vein between two different rock-masses.

The facts from the great sections of Washoe rocks so carefully investigated are of fundamental importance to petrography. As far as igneous rocks are concerned, they seem to demolish the system of rock-classification now most popular; for, taking out the distinctions of *earlier* and *later;* and that of granitoid, micro-crystalline and glass-bearing, among rocks and groups of rocks, the system goes to pieces, and the definition of *species* or *kinds* among rocks will have to fall back, with some exceptions, on the reasonable ground of mineral constitution irrespective of age and texture. J. D. D.

2. *A crystallographic Study of the Thinolite from Lake Lahontan;* by EDWARD S. DANA (Bulletin of the U. S. Geol. Survey, No. 12).—The calcareous tufas of Lake Lahontan, the great Quaternary lake of northwestern Nevada, were called thinolite by King in his Report of the Survey of the 40th Parallel, in allusion to the fact that they were a shore deposit (θίς, shore). Of these tufas a certain limited portion is crystalline in character, and a study of its forms, as remarked more particularly later, led Mr. King to the conclusion that it was a pseudomorph after the mineral gay-lussite; and he bases his explanation of the history of the lake upon this hypothesis. The relations of the tufas of Lake Lahontan have been since then thoroughly studied by Mr. I. C. Russell. He shows that there are three distinct varieties, the *lithoid* or stone-like, the *thinolithic*, or crystalline and the *dendritic;* these were deposited at different stages in the Lake's history in the order named. The chief interest attaches to the crystalline variety, or thinolite proper, and it is to the description of this that the Bulletin No. 12 is devoted. It is found well developed at a number of different points in the Lahontan basin, conspicuously about Pyramid Lake, and outside of it in that of Mono Lake; where best exposed it forms a layer of interlacing crystals six or eight feet in thickness. Chemically the thinolite consists of almost pure calcium carbonate. As regards general aspect, many varieties are found, from the open, delicately porous form, skeleton-like in character, to that which is firmly compact and has a smooth exterior. The form of the

individual crystal is roughly either that of a rectangular prism, often with projecting edges and generally tapering off toward the extremities (see figs. 1, 4, 7, 8), or that of an acute square or rhombic pyramid (fig. 9). Crystals of the former kind are usually grouped in a more or less closely parallel position with but little interlacing, the individual crystals having a length up to 8 or 10 inches; when the crystals are smaller, however, they interpenetrate each other and give the mass a reticulated aspect.



The distinctly pyramidal crystals are generally small and project from the mass in all directions.

Of the true crystalline structure of the thinolite but little can be definitely concluded from the external examination of the specimens. It is only when sections have been cut, transverse and longitudinal, that the form is revealed. The transverse sections show uniformly a system of square or rectangular ribs with others in a diagonal position, intersecting at an angle of about 90°. In the porous kinds the section cut shows these ribs alone, the intervening spaces being open; this is illustrated in fig. 10, which is a section cut across the crystal shown in fig. 1 at the point indicated. In the more compact varieties the spaces between the skeleton ribs, which uniformly consist of granular calcite, are more or less completely filled with amorphous calcite, often in concentric layers; this is shown in fig. 11 cut from the base of the crystal shown in fig. 8, and still more in fig. 12 cut at the point indicated from the crystal shown in fig. 9. A longitudinal section of a crystal resembling fig. 1 is shown in fig. 4 and reveals a system of plates converging toward one extremity at an approximate angle of 35°.

The arrangement of the system of plates of which these pseudomorphous crystals are made up, as shown in the figures, proves that the crystals conformed approximately to the type of an acute square pyramid. The structure of numerous dissected crystals, as shown in figs. 2, 3 and 5, which are not uncommon in some of the specimens, conforms to this type; as does also the external form whenever distinct. Provisionally then it is concluded that the original mineral, after which the thinolite is a pseudomorph, crystallized in the tetragonal system in acute pyramidal forms. The crystals might equally well be referred to an orthorhombic or monoclinic form approximating to the tetragonal, but the simpler hypothesis seems the more probable.

Only a negative conclusion as to the nature of the original mineral is definitely reached, viz: that it could not have been gay-lussite, nor gypsum, nor anhydrite, nor in fact any known mineral. It is suggested, however, in view of the fact that the crystals resemble in form the pseudomorphs of lead carbonate after phosgenite ($PbCO_3+PbCl_2$), that the original mineral in this case may have been an isomorphous salt with the composition $CaCO_3+CaCl_2$, or perhaps $CaCO_3+2NaCl$. The value of such a hypothesis will have to be tested by synthetic methods.

In conclusion the writer calls attention to the resemblance of many of the thinolite forms (cf. fig. 6) to the well known barley-corn pseudomorphs of Sangerhausen, long referred to gay-lussite, although that explanation was shown by DesCloizeaux to be erroneous. It was this resemblance which chiefly led Mr. King to refer the thinolite also to gay-lussite. Pseudomorphs of calcium carbonate apparently identical with those from Sangerhausen have been described from half a dozen other localities, and it is suggested here that probably all of them, and the thinolite also, were derived from the same original mineral. The ultimate solution is therefore much to be desired on this account, as well as because it is so important in explaining the geological history of Lake Lahontan.

3. *Address before the Geological Section at the recent meeting of the British Association at Aberdeen*, by the President of the Section, Prof. J. W. Judd.—In his able address at Aberdeen, Prof. Judd reviews historically, *first*, the problem of the Scottish Highlands; *secondly*, that of the Elgin sandstone; *thirdly*, the relations between Scottish and Scandinavian geology; *fourthly*,

the extent, effects and geological history of faultings and denudation, particularly in the Scottish Mountain region.

With regard to the Highland controversy he points out that Macculloch, nearly seventy years ago, took the first right steps in the investigation of the region, proving the existence of a great formation of red sandstones and quartzyte, distinct from the "Old Red," and finding some fossils in the quartzytes that have since been proved to be Paleozoic; and recognizing its unconformability to underlying crystalline rocks and its being overlaid with apparent conformability by other crystalline schists and gneisses. In 1854, Mr. Charles Peach discovered other fossils in the formation which settled the question as to Paleozoic age. In 1856, Prof. Nicol showed that Macculloch's "Primary Red Sandstone" formation, included two series, an upper fossiliferous, and a lower unfossiliferous which was unconformable to the rest; and to the latter he gave the name of the Torridon sandstone. In 1859 he reached, in opposition to other investigators, the conclusions which are now admitted facts, even to the latest—that the apparently conformable succession of this early Paleozoic series into overlying schist and gneiss, was "due to the thrusting of the crystalline rocks over the sedimentary by great overthrow faults" comparable with those found in the Alps. These results were published by Professor Nicol in the Quarterly Journal of the Geological Society for 1861. The conclusion was afterward supported by some other observers, but it took more than twenty-five years for his results to gain general acceptance.

The review of the close relations between Scottish and Scandinavian geology as regards geography, strata, mountains and mountain-making, faulting and denudation, is of special interest. The Scottish Highlands, with the Hebrides and Donegal, Orkney and Shetland, are, Prof. Judd observes, "mere outliers of the Scandinavian peninsula;" and geographical separation took place as late as the Glacial era.

He recognizes fully the light with regard to the mountain formations which the Highlands received from the work of Prof. Rogers on the Appalachians. But he does not see fit to give credit for the principle that, among crystalline rocks, lithology may serve as a substitute for paleontological evidence; and he even throws discredit on the term "fundamental gneiss" of Murchison. His views on this point we fully endorse; and one sentence is almost in the words of an article by the writer read in August last before the geological section of the American Association. We cite a paragraph:

"I confess that, speaking for myself, I am not sanguine as to the success of such endeavors. The miserable failures which we have seen to have attended similar attempts, in the case even of far less altered rocks, where identifications have been based on mineralogical resemblances only (and in connection with which definite paleontological or stratigraphical evidence has been subsequently obtained) ought surely to teach us caution in generalizing from such uncertain data. It has been argued that, where paleontological evidence is wholly wanting, and stratigraphical relations are doubtful or obscure, then we may be allowed to avail ourselves of the only data remaining to us—those derived from mineralogical resemblances. But surely, in such cases, it is wiser to admit the insufficiency of the evidence, and to say 'We do not know!' rather than to construct for ourselves a 'fool's paradise,' with a tree of pseudo-knowledge bearing the Dead-Sea fruit of a barren terminology! The impatient student may learn with the blind poet that

They also serve, who only stand and wait.

It is thought by some that the application of the microscope to the study of rock-masses may reveal peculiarities of structure that will serve as a substitute for paleontological evidence concerning the age of a rock when the latter is wanting. Greatly as I value the insight afforded to us by the microscope when it is applied to the study of the rocks, and highly as I esteem the opinions of some of those who hold these views, yet I fail to see that any such connection between the minute structure and the geological age of a rock has as yet been established."

He later states, with regard to certain Cambrian beds in Scandinavia, that Kjerulf believes there is evidence of their passing horizontally into true gneiss; and adds that "similar appearances are not wanting in the case of our Torridon sandstone," which he is disposed to refer to the same age, though leaving the question open as it has afforded yet no fossils.

The fossiliferous beds overlying the Torridon Sandstone containing "Primordial" forms of trilobites are either true Cambrian or the equivalent of the American Calciferous and Chazy. The Triassic rocks of Scandinavia are described as undistinguishable in all their features from those of the Highlands. On both sides of the North Sea they pass up insensibly into Rhætic and Infra-lias beds, of marine and estuary origin having vast thickness, though thin in England, and having coal seams, and in Scania many plants of several distinct floras. Over these in each country are Jurassic beds, 3000 to 4000 feet thick, with other coal beds and abundant Ferns, Cycads and Conifers; and the upper Jurassic beds in Sutherland bear evidence of forests of such trees, and at the same time of transportation on large rivers, at certain seasons, of bowlders of vast dimensions by floating ice. The following epoch of the Neocomian was apparently a time of emergence and great denudation; but the beds of the Cretaceous once covered large areas in the Scottish Highlands and Scandinavia. The Tertiary period left few stratified deposits in either land, but was an era of great sub-aerial denudation, stripping the country largely of Mesozoic and older strata "except where buried deeply by gigantic earth-throes or sealed up under lava streams." In the west of Scotland a chain of volcanic mountains with summits towering to the height of 10,000 to 15,000 feet have been reduced by this same denudation to heights of little more than 3000 feet. Dislocations of one, two and perhaps three miles in amount, parallel in general to the Highlands (N.E. and S.W.) have occurred in Paleozoic, Mesozoic, Cretaceous and Tertiary times. "Great strips of Triassic and Old Red Sandstone strata, like those of Elgin and Turriff and Tomintoul, and of the line of the Caledonian Canal, are found let down among the crystalline rocks by the gigantic faults," and in the Western Highlands displacements of several thousands of feet affect the older Tertiary rocks. The great central valley of Scotland consists of Newer Paleozoic strata faulted down between the Archæan and Older Paleozoic of the Highlands on one side and the Borderland on the other. Fissures, injected by lavas from the great Tertiary volcanic foci of the Western Isles and Antrim, cross the Highlands, the central valley, the Borderlands of Scotland and the region of secondary rocks in Northern England.

The address of Prof. Judd is a very valuable contribution to the great subjects of mountain disturbances and denudation, and also to other topics connected with the geology of the higher latitudes. J. D. D.

4. *Results of the fusion of pyroxene and hornblende minerals.*—The Geological Zeitschrift, volume xxxvii (p. 10) contains an article, by Mr. Arthur Becker, entitled "Fusion experiments with pyroxene and amphibole minerals, and observations on olivine grains." The object of the experiments was to ascertain "whether the crystalline system of these minerals was determined absolutely by their chemical composition." For this purpose the author fused a number of specimens of pyroxene and hornblende in a furnace and kept them as near the melting point as possible for from. 8 to 36 hours, and then allowed them to cool slowly. He succeeded in obtaining in most cases an almost completely crystalline mass, the characters of whose crystals he carefully studied with the microscope. Of the orthorhombic system, he treated hypersthene, bronzite, enstatite and anthophyllite; of the monoclinic, augite and hornblende; and of the triclinic, rhodonite, fowlerite, babingtonite and bustamite.

The following are the results of his experiments. Although not deciding with certainty whether the fused triclinic minerals cooled in the triclinic or the monoclinic system, yet, in view of the very great resemblance to the mineral in the unchanged state, he infers that the original form is retained. As to the orthorhombic and monoclinic pyroxene and amphibole minerals, the experiments proved that, when melted and cooled under the conditions stated, they "crystallize again in the same systems to which the original minerals belonged; but that the members of the hornblende series cool as augite."

The hornblende from Wolfsberg in Bohemia yielded results of especial interest. In the first experiments the fused mineral exhibited in the glassy ground-mass numerous large brown-violet, sharply defined augites, and a very large amount of small bright augite-microlites, for the most part rounded. The other experiment, "in which the temperature of the fused mass during the formation of the crystals was, at least at the beginning, somewhat higher," gave a product consisting of a brown glassy substance out of which some whole and many fragmentary crystals of olivine projected, which contained numerous inclusions of the ground-mass." The olivine, he says, according to his experiments and those of others, easily forms from liquid fusion (provided, of course, the chemical composition of the magma allows) when it is kept for a long time at a pretty high temperature. Besides a considerable number of brown irregularly defined weakly-polarizing and somewhat dichroic folia separate out, such as Mr. Becker had obtained in a former experiment. In some of these an extinction angle of 2°–5° in the longer direction was observed. They are, according to this experiment, new formations and not incompletely dissolved mineral particles. He observes that similar brown scales exist sometimes in natural basalts, and questions whether this is so because the basaltic hornblende, which melts more easily than augite, was again made liquid by additional heat and then, in cooling, as in this experiment, separated into olivine glass and this compound. A. G. D.

5. *An effect near Merak, on western Java, of the Krakatoa Eruption.* (From a paper by the Rev. Philip Neale, late British Chaplain at Batavia, in Leisure Hour.)—One of the most remarkable facts concerning the inundation remains to be told. As we walked or scrambled along, we were much surprised to find great masses of white coral lying at the side of our path in every direction. Some of these were of immense size, and had been cast up more than two or three miles from the seashore. It was evident, as they were of coral formation, that these immense blocks of solid rock had been torn up from their ocean bed in the midst of the Sunda Straits, borne inland by the gigantic wave, and finally left on the land several miles from the shore. Any one who had not seen the sight would scarcely credit the story. The feat seems almost an impossible one. How these great masses could have been carried so far into the interior is a mystery, and bears out what I have said in previous papers as to the height of this terrible wave. Many of these rocks were from twenty to thirty tons in weight, and some of the largest must have been nearly double. Lloyd's agent, who was with me, agreed in thinking that we could not be mistaken if we put down the largest block of coral rock that we passed, as weighing not less than fifty tons.

6. *Geological and Natural History Survey of Minnesota for* 1884. N. H. WINCHELL, State Geologist. 196 pp. 8vo.—Prof. Winchell describes in this report and gives figures of the Primordial fossils from the red quartzyte of the Pipestone or Catlinite region of Missouri, noticed on page 316 of this volume. The species described are named *Lingula calumet*, the shells of which are distributed in great numbers through portions of the rock, and *Paradoxides Barberi.* The specimen of the latter has

a length of five inches, and may be three-fourths of the whole animal. It is distorted and somewhat obscure in its markings, but appears to be a true fossil. The pipestone beds have been regarded as Huronian. They now are placed with the earlier Primordial. Prof. Winchell observes that, in several deep wells that have been drilled in central and southeastern Minnesota, a great thickness of red and green shales have been reached which may be of the same formation. He adds that red gneisses, felsytes and porphyritic felsytes in Wisconsin *overlie* the red quartzytes and are therefore brought within the Primordial zone.

The report contains also a paper by Prof. N. H. Winchell on the crystalline rocks of the Northwest, the same paper that he presented to the Philadelphia meeting of the American Association. The author becomes quite earnest in his defense of Emmons's Taconic System. He says, after arguing in its favor, that "there may be reasons why the current literature of American geology is almost silent respecting the great work of Emmons, and why the Taconic is not known among the recognized geological formations." And, in view of the future triumph of the system, it is added, "No amount of error, though heaped to the sky and supported by the highest authority can long subsist." The writer of this notice, though apparently among those aimed at in these remarks, does not feel hit by them; for he has for fifteen years endeavored by hard work in the field and various published papers to put the Taconic system into current geological discussion, and give it its right place in the geological series; and he has still more to say on the subject from more field work. Should the Taconic system, in the process, lose its identity, the time will then have come for "characterizing in deserved terms the attempt to bury the Taconic in the Quebec coffin;"* and we shall hope to have from Prof. N. H. Winchell the *obit* discourse.

Among the other contents of the Report are geological notes on portions of Minnesota by Mr. Warren Upham; on the Foraminifera and other organisms of the bowlder clay, by A. Woodward and B. W. Thomas (with a plate), and by Dr. George M. Dawson; Notes on Blue Earth County, by Prof. A. F. Bechdolt, in which an interglacial peat bed is described. J. D. D.

7. *Underground Temperatures.*—The Proceedings of the Royal Society for Feb. 12, 1885, contain an abstract of a paper on underground temperatures by Professor J. PRESTWICH. The author reviews the published facts, and the conditions in the several cases, in order to eliminate the more doubtful ones and reach the probable normal rate of decrease in temperature or thermic gradient. The various determinations give a range in the rate from

* In this expression, Professor Winchell alludes to the fact that Sir William Logan referred the Taconic series (after a summer's study of the region in Massachusetts, and the determination of Vermont fossils by Mr. Billings) to the Quebec Group, regarded as the middle division of the Lower Silurian. But why the work of this honest and able geologist, which *deserves* only praise, should call out rebuke from any geologist and from one who has never studied the region is to us inexplicable.

less than 30 to more than 120 feet per degree; and 30 meters for 1° C. (=54·7 feet for 1° F.) is the commonly adopted rate in Europe, while in England 50 feet is adopted by some and 60 or more by others. He considers the conditions in (1) coal mines, (2) other mines, and (3) artesian wells and bore-holes; and the action in modifying temperature taking place in mines: through (1) ventilation, (2) underground flows of water, (3) chemical reactions, and (4) the working operations; and, in artesian wells, from (1) pressure of water on the thermometers, and (2) convection currents in the column of water.

In the ventilation in coal pits the amount of air passing through varies from 5000 to 150,000 cubic feet per minute, and has a large cooling effect. It is generally greatest in the deepest mines. The escape of gas from a blow-hole is also cooling, usually lowering the temperature 2° or 3°; and in one case the temperature of 74° F. existed in the coal at a depth of 1588 feet, and 62° F. in a hole with a blower of gas at a depth of 1588 feet. The discharge of water causes a loss of heat. On the other hand, a crushing of the coal raises the temperature. There is also variation dependent on the form of the surface above, it rising under hills and falling under valleys. For correct determinations from coal mines, therefore, the height of the pit above the sea-level should be known; the exact mean temperature of the place; the depth, at each station, beneath the surface; the temperature of the air in circulation; the length of exposure of face; whether there is discharge of gas or not; the dip of the strata, and the quantity of water discharged.

Eliminating the more doubtful observations, the seven best—at Boldon, North Seaton, South Hetton, Rosebridge, Wakefield, Liège and Mons,—give a mean gradient of 49½ feet for 1° F. The bore-holes at Blythswood, South Balgray and Creuzot, give a mean of 50·8 feet.

In other mines, the loss of heat by ventilation is much less than in coal mines and that from pumped water far more. In the Gwennap district, where 550 acres were combined for drainage purposes, above 20,000,000 gallons have been discharged in twenty-four hours from a depth of 1200 feet; the water issues at temperatures from 60° to 68° F., or more than 12° above the mean of the climate, showing a large abstraction of heat from the rocks through which the waters percolate. Again, surface waters may enter and lower the temperature. The underground currents sometimes raise and sometimes lower the temperature of the rocks. Mr. Were Fox, in his many careful observations on underground temperatures gave preference to the rocks, and Mr. Henwood, an equally experienced observer, considered the springs as giving surer results. The ten best of Mr. Henwood's observations, at depths from 800 to 2000 feet, give a mean of 42·4 feet per 1° F.; and the observations in eight mines, 1100 to 2100 feet deep, by Mr. Fox, give 43·6 feet per 1° F. The observations in European mines Professor Prestwich regards as too uncertain for use.

From artesian wells and borings, when the connection waters are shut off by tubing, and when the pressure of the waters on the thermometer was guarded against, as those made at Kentish Town, Richmond, Sperenberg, Pregny and Ostend, give 51·9 feet per 1° F. In these wells the waters do not overflow. In others which do overflow, and which should therefore give the best results, as at Grenelle, Tours, Rochefort, Mondorff, Münden and others, the mean afforded is 50·2 feet per 1° F. In the Sahara Desert, the mean from 11 overflowing wells at depths of 200 to 400 feet gave 36 feet per 1° F. The author also considers the variations resulting from difference of conductivity in rocks, but makes no application of the subject.

The mean thermic gradient deduced from all the observations is 48 feet per 1° F.; but this is considered only an approximation. The question of change of rate downward also is considered but the facts reviewed gave as regards this point no satisfactory result. Professor Prestwich inquires in closing whether a gradient of 45 feet per degree may not be nearer the true normal than 48 feet.

8. *Notes on the Stratigraphy of California*, by G. F. BECKER. U. S. Geol. Survey, No. 19. Washington, 1885.—In this paper, Mr. Becker treats of the metamorphic rocks of the Coast Ranges and their age; their identity with the Mariposa and Knoxville beds, but non-conformity with the Chico beds; on the relations of the Coast Ranges and the Sierra Nevada; and also on the California Paleozoic rocks, and other points in California Geology.

The age of the metamorphic rocks of the Coast Ranges is shown to be near the limits of the Jurassic and Cretaceous. They contain species of *Aucella* and other fossils near Knoxville, identified by Dr. C. A. White, and the beds are hence named the Knoxville group. Fossils occur also on Sulphur Creek in Colusa Co., and at Mount Diablo, and according to White, near the New Almaden Mine. The beds consist in part of serpentine which is one of the alteration products, as described by Whitney and others. The non-conformity with the Chico beds being established, the period of Coast Range metamorphism must have been "before the first of the Wallala beds were deposited and still longer before the opening of the Chico period"—or "neither long before nor long after the Neocomian."

The Mariposa region is one of three along the western side of the Sierra Nevada which affords fossils; it is a narrow strip of country lying along the foothills from Mariposa to Nevada, which is often known as the gold-belt proper—and its fossils, *Aucella* (*A. Piochii*), *Belemnites*, etc., are regarded as showing identity with the "Knoxville group." The rocks resemble those of the latter group in kinds and in the prevalent silicification and serpentinization. Dr. Becker observes that neither Dr. White nor himself could find any reason for maintaining that the fossiliferous rocks were not of the auriferous series; or for dissenting from Prof. Whitney's opinion that they form an integral portion of the modern

Sierra Nevada Range. From the facts it follows that the Sierra and Coast Ranges experienced upheaval long before the era of the Chico group; and that the same disturbance which determined the existence of the Coast Ranges added the gold belt proper to the Sierra Nevada; and that probably "a portion at least of the Cascade Range was elevated and metamorphosed at the same time. Consequently, before the Chico era there was a sinking," admitting the ocean over a great part of the Coast Ranges and over considerable areas at the base of the Sierra for the later depositions. During the Pliocene very little of either range was below water.

The following paragraph is from pages 22, 23 of the paper.

"I think it may be asserted, as a result of all the geological work done from the Rocky Mountains to the Pacific, that there has been, throughout geological time, a definite tendency in the structural development of this area. The geologist of the fortieth parallel exploration showed that a fault began upon the west flank of the Wahsatch in the Archæan, the same fault which Mr. Gilbert has traced as still in progress. The last-named geologist has also detected a similar fracture on the east side of the lower portion of the Sierra. The eastern portion of the Great Basin was lifted above the surface of the ocean after the close of the Carboniferous, the western portion of the same area followed before the Cretaceous, and at one or both of these epochs the country was laterally compressed, an action no doubt closely connected with the progress of the great faults. About the time of the Neocomian, California experienced an east and west compression, and again, following the Miocene, was an uplift throwing the horizontal strata of the coast into north and south folds. From the Wahsatch to the Pacific Coast there thus appears to have been a recurrent, if not a constant tendency to lateral compression, in substantially one and the same direction. Now Dr. White points out that an extraordinary difference has existed between the marine fauna of the Pacific Coast and that of the waters east of the Sierra from a time prior to the Cretaceous onward, and hence that a land barrier must throughout have occupied substantially the position of the Sierra Nevada, which must therefore have experienced repeated upheavals to compensate for constant erosion. There are also said to be some paleontological grounds for supposing at least a partial separation of these areas during the Carboniferous. This supposition is in entire accord not only with the structural analogies of the region but with the detailed observations of Mr. Clarence King* and his colleagues, who were led to infer the existence of a continental area during the Paleozoic, west of long. 117°30′, lat. 40°. Such a range as the Sierra, though partaking in the general compression and movement of the whole country, must offer a tremendous resistance; and, at any one of the active periods during which the physical conditions permitted contortion of strata along the western flank of the

* Systematic Geology, p. 534.

Sierra, these must have been driven against the barrier until they could yield no more. Thus if a pile of cloths were compressed from their edges (as in Hall's famous experiment) with enormous energy, they would be forced into plications so sharp that the dip at any point would be nearly vertical. It seems to follow that at different upheavals, some of them perhaps as yet untraced, strata to the west of the great Sierra may have been driven into the nearly vertical position of the gold slates and their original stratigraphical relations completely obscured. I do not consider it certain, therefore, or even probable that the Carboniferous slates near Pence's Ranch first assumed their present position subsequently to the Knoxville period. It may be that they have stood nearly as now ever since the Carboniferous of Utah was raised above water, while the slates of Horsetown, of the age of which nothing is known, so far as I can see, may possibly owe their vertical dip to still earlier convulsions."

9. *New American Limuloid species from the Carboniferous.*—Prof. A. S. PACKARD, in a note in the American Naturalist of March, 1885, mentions the discovery at Mazon Creek, Morris, Illinois, of a new species of *Belinurus*, and one of *Cyclus*, genera hitherto unobserved on this continent; and from the Carboniferous beds of Pennsylvania, a new species of *Euproöps*. Moreover the Cyclus, in its four or five pairs of limbs, "apparently of the same nature as those of the larval Limuli," shows that it is really a tail-less Limuloid. The species described are named *Belinurus Lacoei*, *Euproops longispina*, *Cyclus Americana* and *Dipeltis diplodiscus*; the last is Cyclus-like.

10. *Embryology of Limulus.*—Prof. PACKARD closes a note on this subject (Proc. Philad. Amer. Phil. Soc., Jan., 1885) with the following conclusions. The fact that the embryo of Limulus has at first no abdominal appendages, and only cephalic, shows divergence from the Tracheata (Arachnida, etc.) and allies it to the Crustacea. The absence of a serous membrane, of an amnion, of procephalic lobes, of protozonites (which occur in the early embryo of the scorpion and spider) show further divergence from the Tracheata. The embryology is scarcely more like Tracheata than the Crustacea; it is a primitive type more related to the branchiate arthropods than the tracheate and "should be regarded as a generalized or composite form, which with its fossil allies, the Eurypterida and Trilobita, constitute a distinct class.

11. *Town Geology: the Lesson of the Philadelphia Rocks*; by ANGELO HEILPRIN. 134 pp. 12mo, with 7 plates, and several wood-cut illustrations.—Mr. Heilprin's work is a popular illustration of some of the principles of geology by means of facts from the vicinity of Philadelphia. The figures are good, and the explanations of the subject simple and clear. The work will be found an easy introduction to the science for the young geologist. Two of the plates contain representations of Mesozoic fossils. The last of the chapters is on the Drift deposits and the era of ice, under the title of "Philadelphia Brick and Cobble Stone; a vision of Arctic Climates."

12. *Einführung in die Gesteinslehre: Ein Leitfaden für den akademischen Unterricht und zum Selbststudium von* A. von Lasaulx. 215 pp. 8vo. Breslau, (Edward Trewendt).—This little work, like another by Hussak, recently published, is especially adapted for the instruction of those who are commencing their petrographical studies. The author assumes that they have made themselves familiar with the now fully developed methods of modern petrography, mechanical, microscopic, microchemical, and goes on to describe the kinds of structure in rocks, the most important rock-forming minerals, and the classification and description of the different kinds of rocks. The author's idea of leading students to go direct to original papers for information is a good one, and with this end in view the literature of the subject is given very fully in an appendix.

13. *Pyrargyrite and Proustite.*—Dr. Ernst Rethwisch has published, as an inaugural dissertation at the University of Göttingen, an admirably thorough discussion of the crystallographic and chemical characters of the two ruby silver ores. Such a review, especially on the crystallographic side, has long been a desideratum in mineralogical literature. The complexity of the subject may be gathered from the fact that the total number of planes identified in the two species amounts to one hundred and eight.

14. *The Marble Border of Western New England: its geology and marble development in the present century.* (Middlebury Historical Society, vol. i, part II, Middlebury, Vt., 1885).—This pamphlet contains a short paper on the geological features of the Marble belt, by Professor Ezra Brainerd, and another on the Marble Fields and Marble Industry, by Professor H. M. Seely of Middlebury, Vermont. The marble belt is for the most part the belt of the Stockbridge limestone, the great limestone of Emmons's "Taconic System." The subject is treated from an economical and historical point of view, as indicated in the title.

15. *Die Meteoriten-Sammlung des k. k. mineralogischen Hof-Kabinetes in Wien am* 1. *Mai* 1885, *von* Dr. Aristides Brezina. From the Jahrb. der k. k. geol. Reichsanstalt, 1885, pp. 151–276; with four plates.—The collection of meteorites of the Vienna Museum has long been recognized as ranking among the most important in the world. Of late years the increase has been very rapid and the present catalogue includes 358 numbers. Dr. Brezina, however, has done much more than merely give a history of the collection and a list of localities with dates, weights and so on. He discusses also at some length the origin of the peculiar structure of meteoric stones, and the systems of classification proposed. The work is therefore an important contribution to a most interesting subject.

16. *Botany of the Challenger Expedition.* Vol. I, 1885.—This bulky and well-illustrated quarto volume is devoted to the deeply interesting subject of Insular Floras, namely, of the Bermudas, of the Southern Atlantic Islands, with St. Helena, etc., of Juan

Fernandez, the Southeastern Moluccas, the Admiralty Islands, etc. And the work has been done, with admirable promptness, by Wm. B. Hemsley. To the proper systematic part, he has prefixed a general discussion of the present state of our knowledge of the principal insular floras, adding a copious bibliography; and finished with an appendix, on the dispersion of plants by ocean currents and birds. So here is much matter for consideration. We can at this moment only announce the reception of this volume and indicate the general character of its contents. A. G.

17. *Methods of Research in Microscopical Anatomy and Embryology;* by Charles Otis Whitman, M.A., Ph.D. Boston: S. E. Cassino & Co. 1885. 8vo, pp. viii, 255.—To those who have used Dr. Whitman's notes under the head of Microscopy in the American Naturalist for several years past, this work, to a large extent based on them, will certainly be welcome. The book well accomplishes its purpose of supplying the need created by the recent rapid development of the methods of research in microscopical anatomy and embryology, for it judiciously brings together all the more important new processes used in these departments. The author says that no effort has been made to give the treatise an encyclopædic character, and perhaps for this very reason, the work appears to be much more satisfactory than Mr. A. B. Lee's Microtomist's Vade-Mecum (Philadelphia, P. Blakiston, Son & Co., 1885), which covers much of the same ground. S. I. S.

III. Astronomy.

1. *The Star System* 40, o^2 *Eridani.*—Professor A. Hall has published, in the *Astronomische Nachrichten*, No. 2682, the results of thirty sets of measurements, made with the 26-inch refractor to determine the parallax of the principal star of this remarkable system. This star is of the fifth magnitude, having the very large proper motion of 4″ a year. At a distance of 81″ from it is a binary whose distance is now about 3″ and whose components are of the 9th and 11th magnitudes, and this binary has the same extraordinary proper motion as the principal star, the three stars being presumably physically related.

Professor Hall made observations in March and September, 1884, and March, 1885, comparing with a 10th mag. star $32^s \cdot 5$ following and a little south of 40 Eridani. His result is

$$\pi = 0'' \cdot 223 \pm \cdot 0208.$$

Although the observations were made with difficulty, owing to the small field of view of the telescope, yet Prof. Hall expresses much confidence in his result.

Since the commencement of Prof. Hall's observations in March, 1884, Dr. Gill has published the measurements made by himself and Dr. Elkin upon several Southern stars, and among these is a determination by Dr. Gill of the parallax of 40 o^2 Eridani, viz:

$$\pi = 0'' \cdot 167 \pm 0'' \cdot 018.$$

These two results agree in showing that this system is more distant than its large proper motion would lead us to expect.

Dr. Gill compared the star with two stars of the 6th and 6·7th magnitudes, over a degree distant in opposite directions, thus eliminating the temperature corrections. The star passed much nearer the zenith of Cape Town than of Washington.

The observations upon the close binary indicate a period of revolution between 125 and 140 years, of which 102 have elapsed since Herschel's first observation. If we take the mean of the two determinations of parallax, the mean distance of the close binary is about 25 radii of the earth's orbit, and the sum of the masses of these components is not largely different from the sun's mass.

The distance of the principal star from the binary, measured perpendicularly to the line of sight, is a little over 400 radii of the earth's orbit. Distinct evidence of rotation of the binary about the principal star is yet wanting.

Measured in units of annual velocity of light, Prof. Hall's parallax implies a distance from us of about 15, while Dr. Gill's parallax gives a distance of about 20.

2. *Report No.* 8 *of the Cincinnati Observatory. Observations of the Comets of* 1883, by H. C. Wilson, Astronomer pro tem.; published under the direction of J. G. Porter, A.M., Astronomer. Cincinnati, 1885.—The Report No. 7 gave the results of the observations at the observatory on the comets of 1881 and 1882. No. 8 contains the results from comets I and II of 1883 (the Brooks-Swift and Pons-Brooks comets), made by Mr. H. C. Wilson, assistant astronomer during the time that he was in temporary charge of the observatory. The report is illustrated by 13 fine plates, 11 of which are devoted to views of the comet at different times during its passage.

IV. Miscellaneous Scientific Intelligence.

1. *The Washington Co., Penn. Meteorite;* by G. F. Kunz.—At 4 o'clock on Saturday evening, September 26th, 1885, under a remarkably clear sky, a meteor passed over several townships of Washington County, in southwestern Pennsylvania. Its general direction was southeast over Hanover township, Burgettstown in Smith township, Jefferson township, Cross Creek village in Cross Creek township, the village of Hickory in Mt. Pleasant township, Canonsburg in Chartiers township, and Cecil township. The falling body was also distinctly seen and heard in many towns in southwestern Alleghany County, as Bridgeville, Mansfield, Middletown and McKeesport. The great noise and disturbance which it created were attributed to various causes, usually to the explosion of some boiler in the neighborhood or blasting in the quarries. The meteorite undoubtedly exploded and fell somewhere in this section.

Mr. John Connor of Canonsburg writes that the meteor passed over his house, accompanied by a loud roar like a peal of thunder.

He himself was in the barn at the time, and on account of the brightness of the day, thought that a boiler had exploded on his farm at a spot where a company were prospecting for oil. The only person on the farm who witnessed the sight was a lad of 17, named Richardson, who was visiting there and happened to be out in the fields. He reports that he heard a queer hissing noise quickly followed by a roar, as of thunder. Young Richardson, probably deceived by the excessive glare of the meteor, thought it fell on the Connor farm, but a thorough search failed to discover any fragments there. Professor Tingley of Alleghany College, Meadville, Penn., kindly volunteered his assistance and devoted several days to inquiry, visiting the Burgettstown County fair then in progress, and searching far and wide for a trace of any meteoric fragments, but he was unable to hear any rumors of the finding of any. It is probable that the mass fell in some unfrequented woods, where it may not be discovered for some time, as meteorites appear to the startled observer to be much nearer than they really are. Numerous exaggerated and sensational accounts, purporting to give accurate details of the fall of enormous meteoric masses, have appeared in the newspapers, yet in all cases these have proved to be myths, unsubstantiated by the finding of any fragments as yet.—*Read at the New York Academy of Sciences*, Oct. 12th.

2. *British Association at Aberdeen.* — The meeting of the British Association opened at Aberdeen on the 12th of September. From *Nature* of Sept. 10th, we learn that up to the preceding Saturday, the local committee had disposed of tickets to the value of 1,000*l.* to "local people alone; and that 2,500 people were expected to take out tickets for the meeting. The actual number attending the meeting was 2,203. The grants of money made for research amounted to 1,195*l.*"

The President of the meeting, Sir Lyon Playfair, discussed, in his inaugural address, the relations of Science to the State and the claims of Science from the State, with special reference to its place in education. Germany, as is usual, was appealed to for incentive; and the fact was stated in this connection that:

"Strassburg has had her university and its library rebuilt at a cost of 711,000*l.*, and receives an annual subscription of 43,000*l.* In the rebuilding of the university eight laboratories have been provided so as to equip it fully with the modern requirements for teaching and research." "The cost of these laboratories has been as follows: Chemical Institute, 35,000*l.*; Physical, 28,000*l.*; Botanical, 26,000*l.*; Physiological, 13,900*l.*; Physiological Chemistry, 16,000*l.*; Observatory, 25,000*l.*; Anatomy, 42,000*l.*; Clinical Surgery, 26,000*l.*"

The reports of the meeting show that it was fully as successful as any that have preceded it. Professor G. Chrystal's address before the Physical Section, like Sir Lyon Playfair's, was on the Diffusion of Scientific knowledge; and Professor Armstrong's, before the Chemical, inclined in the same direction, the first half treating of the importance of encouraging chemical research and education, the latter half, of chemical action. An abstract of Professor Judd's address before the Geological Section is given on page 392. Francis Galton, F.R.S., before the section of Anthropology, gave a "lecture," as he termed it, on "types" and their "inheritance"—"gathered from family records entrusted to him by persons living in all parts of the country." Professor W. C. McIntosh's address before the Biological Section, reviewed the subject of the phosphorescence of marine animals.

The work of the association is reported quite fully in *Nature*, commencing with the number for September 10th (No. 828). The several addresses of the Vice-Presidents are given in full. Birmingham is to be the next place of meeting for 1886, and Sir William Dawson the President of the meeting.

3. *Louis Agassiz, his Life and Character;* edited by Elizabeth Carey Agassiz. 2 vols., 794 pp. 12mo. Boston, 1885. Houghton, Mifflin & Co.).—The very general admiration for Professor Agassiz will be sustained and enhanced by the story of his life, prepared with excellent taste and judgment by Mrs. Agassiz. It sets forth by a skillful interweaving of letters and narratives, in language as vivid and simple as Agassiz's own style, the early surroundings of the young naturalist, his development under an ardent devotion to Nature as his chief teacher, and his accomplished work by which he became a lasting power in the world. The work is hence of interest to the philosopher for its illustration of the type of man, under one of its phases, to which science owes its recent progress. At the same time the biologist, paleontologist, and geologist, here learn of the successive stages in the establishment of the new views, which were the outcome of his study of nature; for example, how in 1837, the idea of a northern ice-period was struck out in Agassiz's intercourse with the Alps and Juras; the Alps giving the actual glacial phenomena to his mind, the Juras as well as Alps affording the same kind of glacial records on rocks and heights miles outside of and thousands of feet above modern ice-limits—a view which later he corroborated in the Scottish Highlands and in America.

Agassiz's part in the progress of science was so important that the volumes have great value for their contributions to the history of science. But, after all, the attractiveness of the man at his work and among men gives the pages their chief charm.

Americans have reason for holding Agassiz in high honor, in view of his devotion, almost from the day of his arrival in this country, to the interests and exaltation of American science. He sent none of his various memoirs to foreign journals or academies for publication, because, as he told the writer, he was now an American. J. D. D.

4. *Hawaian or Sandwich Island Survey.*—The survey of the Hawaian Islands has been in progress for some years, under Professor W. D. Alexander as Surveyor General. In 1881, a detailed map of Oahu was published, measuring nearly five feet by

three-and-a-half, showing well its remarkable precipices and the system in its heights and valleys. Recently a survey has been made of the small western island of the Hawaian Chain called Nihoa or Bird Island. The island is 5200 feet in extreme length and 2000 in mean width. It is the remains of the upper unsubmerged portion of a volcanic mountain. The highest point is near the northwest angle and is 903 feet above the sea level. The northeast is but little lower, 869 feet. The north, east and west sides are precipitous, and from the top of the bluffs, there are gradual inward slopes, like the slopes of a crater, leading down to a large, partly enclosed bay, which occupies nearly the whole south side. Great numbers of dikes intersect the high precipices on the northwest side which were found to traverse the whole island. The survey indicates that the island was once the site of a great volcano, and the dikes show the courses of fissures through which the lavas flowed at various eruptions.

OBITUARY.

James Macfarlane, of Towanda, Pa., died suddenly on the eleventh of October. He was born at Gettysburg on the second of September, 1819, was graduated at Pennsylvania College, of the same place, in 1837, studied law at Carlisle, and was admitted to the bar in 1845. He made himself well acquainted geologically with the coal-measures and coal regions of Pennsylvania, and published a work of great value on the Coal Fields of America. His "Geologists' Traveling Hand-book,"[1] in which the formations along all railroad routes in the country, as far as known, are given, proved to be a great convenience to travelers, and of much value to the science; and during the two or three years past he has been engaged in its revision for a new edition—and a printers' proof arrived on the morning of his decease. His occasional papers have reference mostly to the Coal-measures.

Thomas Bland was born at Newark, Nottinghamshire, England, Oct. 4, 1809. His father, Dr. Thomas Bland, was a physician. His mother was a Shepard, and a niece of Richard Shepard, who was a conchologist, from whom she acquired a love of natural history which led her to make collections of plants, minerals and shells, and this love of nature was inherited by her son Thomas. He was educated at Charter House School in London, where he was a classmate of Thackeray. He subsequently studied law and entered upon its practice in London. In May, 1836, he became a Fellow of the Royal Geological Society of London. In 1842 he removed to Barbadoes and thence to Jamaica, where he resided until about 1850, collecting largely in various departments of natural history, especially in chonchology. While at Jamaica he made the acquaintance of Prof. C. B. Adams, then of Middlebury College, Vermont. A close friendship ensued which ended only with the untimely death of the latter in 1853. In 1850 Mr. Bland returned to England, and after a stay of a few months, accepted the appointment of superintendent of a gold mine at

Marmato, New Granada. In 1852 he removed to New York, where the remainder of his life was spent in various agencies connected with mining and other enterprises. Of late years he had suffered much from impaired health, becoming for the last few months incapacitated for mental labor, and on the 20th August, 1885, he passed peacefully away.

Though Mr. Bland was always interested in general science, he seems to have received the special direction toward the study of terrestrial mollusks from his intimate friendship in Jamaica with Prof. Adams. He devoted himself to the wonderfully rich fauna of the West India islands, and continuing the labors which Prof. Adams had begun, he soon became a leading authority on that branch. His general knowledge of science led him to devote particular attention to the subject of geographical distribution, so philosophically treated in his published papers. When in South America, he collected largely and corresponded on the subject with all the prominent European conchologists. Again, on removing to New York, he became, through Prof. Adams, acquainted with the American conchologists, and formed a strong friendship especially with Mr. Wheatley and Mr. Redfield. Through the latter he became a member of the New York Lyceum of Natural History, and was for many years a most useful member of its publication committee. He was also a member of the Natural History Societies of Boston, Philadelphia, and other American cities, and the intimate personal friend and correspondent of all the American conchologists.

In 1855 he became acquainted with Mr. W. G. Binney, who was just commencing the continuation of his father's work on the Terrestrial Mollusks of North America. An intimate friendship was established which was only broken by the death of Mr. Bland. The association of these two in the study of our land shells resulted in a series of publications which has thoroughly elucidated the subject. If these publications have any excellence, it is owing to the happy combination of untiring zeal and inherited love of the subject, with all the traditions of the collections and collectors on one side, and on the other the absence of prejudice, the extended experience, the general scientific training and especially the philosophic mind of Mr. Bland.

A detailed catalogue of Mr. Bland's scientific writings, seventy-two in number, has been prepared and published by Mr. A. F. Gray.

Finally, it must be said that Mr. Bland was a genial acquaintance and a most self-sacrificing friend, ever ready with assistance, advice, encouragement or consolation, as these qualities might be required. W. G. B.

Edward Henri von Baumhauer, Editor of the Archives Néerlandaises des Sciences exactes et naturelles published by the Société Hollandaise des Sciences at Harlem, and secretary of the Society, died on the 18th of January last, in his 66th year. The success and European character of the Archives is largely due to his enterprise and science.

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[THIRD SERIES.]

Art. LI.—*On the effect upon the earth's velocity produced by small bodies passing near the earth;* by H. A. Newton.

1. The space through which the earth travels is traversed also by small bodies, or meteoroids. These give meteors of all degrees of brilliancy, and it is reasonable to assume that bodies too small to furnish visible meteors also lie along our path. The impact of these bodies upon the earth, and the consequent increase of the earth's mass, have their effect upon the earth's motions both of rotation and revolution, and hence upon the lengths of the year and the day. The moon's orbit and the length of the month likewise suffer change.

Professor Oppolzer in a paper in the Astronomische Nachrichten (No. 2573) has considered the amount of these two actions and has computed the density which the meteoroid matter must have in the space which the earth is traversing in order to produce upon the relative lengths of the month and the day the observed and unexplained acceleration of the moon's mean motion. This computed density is 59×10^{-18}, the mean density of the earth being unity, and is at least a myriad times, perhaps a million times as great as can be reasonably assigned to observed meteors.* But a body that passes near to the earth has also an action of like character by reason of the attraction of gravitation alone, and I propose to show its amount, for it seems well worth showing that the bodies which pass near us do not have an effect at all comparable with that produced by those which actually come into the earth's atmosphere.

* A proper amount of cosmic dust, if we can reasonably assume that so large an amount of this questionable substance exists, might explain that acceleration.

2. Assume in the first place that the earth is at rest and that a group of small bodies each weighing m' pounds are evenly distributed through space; that they are all moving parallel to each other so as to have a common velocity v relative to the earth on entering the sphere of the earth's sensible attraction; and that there are at first n of these bodies in each cubic unit of space. Let the space considered be a cylinder whose axis has the direction of the motion of the bodies and passes through the earth. Each of these bodies will describe a hyperbolic orbit about the earth, and leave the sphere of the earth's action with the same velocity v with which it entered. By reason of the smallness of the bodies, and their even distribution, the action of the bodies on each other may be disregarded.

3. Because the bodies are assumed to be evenly distributed they will have no resultant action upon the earth at right angles to the original direction of motion. But in that direction of motion the momentum of the whole system of earth and meteoroids will be the same before as after the passage of the bodies. What the meteoroids lose the earth gains. Let the asymptotes of the hyperbolic orbit of one of these bodies make an angle a with its conjugate axis. The momentum of the body on entering the earth's sphere of action will be $m'v$ in the direction of motion. Its momentum in the same direction upon leaving the sphere of action will $m'v \cos 2a$, since the direction of motion has changed $2a$. The loss of momentum will be $m'v(1-\cos 2a)$, and the earth gains by reason of the transit of the body an equal momentum in the same direction.

4. Let the perpendicular distance from the earth to the original line of motion of a meteoroid be p. The number of bodies that pass the earth in a unit of time and that have such distance greater than p and less than $p+dp$ will be $2\pi pnvdp$. The momentum communicated to the earth by the whole group in a unit of time will be the same as that communicated through their whole orbits by those which pass the earth in a unit of time, and this will be the integral

$$\int 2\pi pnv^2m'(1-\cos 2a)\, dp$$

taken between the proper limits.

5. The factor $1-\cos 2a$ is a function of p and v. To find its value let p_0 and v_0 be the distance and velocity of the meteoroid at perigee, let r be the earth's radius, and $g=32\frac{1}{6}$ feet. Using feet-second units we have

By conservation of energy $v_0^2-v^2=\dfrac{2gr^2}{p_0}$, (1)

By conservation of areas $v_0p_0=vp$, (2)

And by nature of the hyperbola $p=p_0(\tan a+\sec a)$ (3)

These equations give

$$1-\cos 2a=\frac{2g^2r^4}{g^2r^4+p^2v^4},$$

and substituting in the integral of the preceding article we get

$$\rho m=\int_{p'}^{p''} 2\pi pnv^2m'\frac{2g^2r^4}{g^2r^4+p^2v^4}\,dp=\frac{2\pi m'ng^2r^4}{v^2}\log\frac{g^2r^4+p''^2v^4}{g^2r^4+p'^2v^4}, \quad (4)$$

where p'' and p' are suitable limits of the integral, m is the earth's mass in pounds, and ρ is the velocity in feet per second communicated to the earth per second.

6. The value of p' is that value of p which permits the meteoroid to just touch the e·rth's atmosphere without entering it. Assuming that the radius of the earth including the atmosphere is r' (which may be assumed $=r+100$ miles), putting p' for p and r' for p_0 in equations (1) and (2), we have

$$v_0{}^2=2g\frac{r^2}{r'}+v^2,$$

$$v_0r'=vp'.$$

Hence $$p'^2v^2=2gr^2r'+r'^2v^2, \quad (5)$$

and $$g^2r^4+p'^2v^4=(gr^2+r'v^2)^2$$

Putting also δ for the density of the meteoroid matter if distributed through the space occupied by the group, the earth's density being regarded as unity, and observing that $m\delta=\frac{4}{3}\pi r^3nm'$, we obtain from (4)

$$\rho=\frac{3\delta g^2r}{2v^2}\log\frac{g^2r^4+p''^2v^4}{(gr^2+r'v^2)^2},$$

Or, $$\rho=\frac{3\delta gr'\beta^2}{2rx^2}\log\frac{\mathrm{P}^2\left(1+\frac{\beta^4}{\mathrm{P}^2x^4}\right)}{\left(1+\frac{\beta^2}{x^2}\right)^2}, \quad (6)$$

by putting u for the earth's velocity in its orbit, $v=xu$, $p''=\mathrm{P}r'$, and $\beta^2=gr^2/r'u^2=\cdot069$. The term β^4/P^2x^4 may be dropped as of no account unless x is very small. Substituting numbers we get

$$\rho=\frac{6{\cdot}83\delta}{x^2}\log\left\{\frac{\mathrm{P}}{1+\frac{\cdot069}{x^2}}\right\}. \quad (7)$$

If the velocity of the meteoroids be that of a comet in a parabolic orbit $x^2=2$ and

$$\rho=3{\cdot}41\delta\,(\log\mathrm{P}-\cdot034).$$

If their velocity be that of the earth in its orbit

$$x^2=1,\text{ and }\rho=6{\cdot}83\delta\,(\log\mathrm{P}-\cdot068).$$

In formulas (6) and (7) ρ expresses the acceleration given to the earth per second in feet per second; δ is the density of the matter of the group if distributed through the whole space considered, the mean density of the earth being unity; x is the velocity, the earth's velocity in its orbit being unity; and P is the radius of a cylindrical stream of meteoroids, the unit being 4056 miles. If the unit for P be taken equal to 4056 $\left(1+\frac{\cdot 069}{x^2}\right)$ miles, and we use common logarithms, the value of ρ becomes

$$\rho = \frac{15\cdot 7\delta}{x^2} \log_{10} \text{P}.$$

7. Equation (7) may be thus expressed as a theorem.

If a cylindrical stream of small bodies evenly distributed and all moving in the same direction with common velocity shall move past the earth supposed to be in the axis of the cylinder, the small bodies by the law of universal gravitation shall communicate to the earth a velocity along the axis in each unit of time;

(*a*) that shall be proportional to the density of the group;

(*b*) that shall decrease as the velocity increases varying very nearly inversely as the square of the velocity;

(*c*) that shall increase with the radius of the cylinder, and shall be proportional to the logarithm of that radius, measured by a unit that differs from the earth's radius by a small quantity which is a function of the velocity.

Strictly taken, if the cylinder have an infinite radius the velocity communicated is also infinite; but if the radius be comparable with measurable stellar distances, the velocity communicated is not large, since $\log_{10}$ P would then not exceed 12.

8. If we assume the bodies at rest, and the earth in motion through them with a velocity v, ρ expresses the resistance of the system to the earth's motion. Hence, *if infinite space were filled evenly with discrete material bodies at rest, and the law of universal gravitation holds true ad infinitum, there would then be exerted an infinite resistance to the motion of a planet in a continuous right line through the system.* Moreover, a finite system of such discrete bodies at rest constitutes a truly though feebly resisting medium by reason of gravity alone and independently of their impact with the moving planet.

If the small bodies however should constitute an elastic medium the above reasoning does not apply. For equation (6) depends upon the property that the small body enters the sphere of the earth's action in one direction, describes a hyperbolic orbit, and leaves it in another. This may not be asserted of particles of an elastic fluid filling space.

Again, if the planet moves in its orbit about the sun, instead of in the assumed right line, the integration may not be extended *ad infinitum.* For the earth's motion to and fro would for the very distant meteoroids develop resistances in opposite directions which would cancel each other, the action of the planet upon the more remote bodies not being instantaneous but requiring long periods of time for its development.

9. The action of one of these bodies on the earth may also be looked upon as like an impact. For we may consider the hyperbolic orbit to be replaced by its asymptotes and the whole action of the earth in changing the body's motion to be concentrated at one point, namely, the center of the hyperbola. The reaction of the body upon the earth will be of the nature of an impact in the line drawn to the center of the hyperbola. The combined impacts of all the bodies would have a resultant in the direction of the motion of the bodies.

Again the action may be regarded as though the earth was in motion and the bodies at rest, and that the earth drew the small bodies around as it passed them into its own wake where they exert a greater attraction than they did in front of the earth. This concentration would not take place if the bodies formed an elastic medium.

10. Thus far the small bodies have been assumed to be at rest or moving in one direction with one velocity. Let us now extend our hypothesis and assume that the bodies have all the same absolute speed cu, but that their absolute velocities are directed to points evenly distributed over the celestial sphere, that the bodies are as before evenly distributed in space, and that the earth moves with a velocity u through the system. The velocities relative to the earth will not be uniform nor their directions evenly distributed.

To represent these velocities draw $AB=u$, and about B as a center with the radius cu describe a spherical surface CD. AB will represent in amount and direction the earth's velocity, CB the meteoroid's absolute velocity and AC the meteoroid's relative velocity. There will be two cases, according as A is within or without the sphere: in other words according as c is less or greater than unity. The distinction between these will be considered further on. The meteoroids may be supposed to come from points evenly distributed over the spherical surface CD.

Let the angle $ABC=\theta$, $BAC=\varphi$ and $AC=xu$. Then

$$x^2=1+c^2-2c\cos\theta,$$
$$xdx=c\sin\theta d\theta,$$
$$\cos\varphi=\frac{1-c^2+x^2}{2x}.$$

11. The bodies which move in directions which make angles

greater than θ and less than $\theta+d\theta$ with the earth's motion constitute a fraction of the whole group expressed by $\frac{1}{2}\sin\theta d\theta$. Their united action upon the earth at right angles to the earth's motion is zero because of symmetry. Each meteor's total action multiplied by $\cos\varphi$ gives the effective action along the line of the earth's motion. If we represent as before the density of the whole system by δ, and the earth's retardation per second in feet per second by ρ_1, we have instead of (6) the formula,

$$\rho_1=\frac{3\delta g r_1\beta^2}{4r}\int_0^\pi \frac{\sin\theta\cos\varphi}{x^2}\log\frac{P^2\left(1+\frac{\beta^4}{P^2x^4}\right)}{\left(1+\frac{\beta^2}{x^2}\right)^2}d\theta,$$

whence $$\rho_1=\frac{3\delta g r'\beta^2}{8rc}\int_{\pm(c-1)}^{c+1}\frac{1-c^2+x^2}{x^2}\log\frac{P^2\left(1+\frac{\beta^4}{P^2x^4}\right)}{\left(1+\frac{\beta^2}{x^2}\right)^2}dx. \qquad (8)$$

Since x does not vary through zero the lower limit must be taken arithmetically positive, whatever be the value of c. The value of the definite integral will be different according as c is greater than unity, or less than unity. The factor $1+\frac{\beta^4}{P^2x^4}$ may be considered equal to unity except in the cases where $c-1$ and θ are both very small and P not very large. The indefinite integral then becomes,

$$\frac{2(x^2-1+c^2)}{x}\log\frac{Px^2}{x^2+\beta^2}+\frac{4(c^2-1)}{x}-\frac{4(\beta^2+1-c^2)}{\beta}\tan^{-1}\frac{x}{\beta}.$$

Hence when $c<1$

$$\rho_1=\frac{3\delta g r'\beta^2}{r}\Big\{\log P+\log(1-c^2)-\tfrac{1}{2}\log((1+c)^2+\beta^2)((1-c)^2+\beta^2)+1$$
$$-\frac{\beta^2+1-c^2}{2\beta c}\tan^{-1}\frac{2\beta c}{\beta^2+1-c^2}\Big\}. \qquad (9)$$

or $$\rho_1=\delta(k\log P-A),$$

where k is a constant ($=6{\cdot}83$), and A is a function of c. But if $c>1$ we get

$$\rho_1=\frac{3\delta g r'\beta^2}{r}\Big\{\log\frac{c+1}{c-1}+\tfrac{1}{2}\log\frac{(c-1)^2+\beta^2}{(c+1)^2+\beta^2}-\frac{1}{c}$$
$$+\frac{c^2-1-\beta^2}{2\beta c}\tan^{-1}\frac{2\beta}{c^2-1+\beta^2}\Big\}. \qquad (10)$$

Hence $\rho_1=\delta A'$ where A′ is a function of c but independent of P′.

12. The results deducible from (9) and (10) may be thus stated as a theorem.

If a widely extended group of small bodies evenly distributed through space have absolute velocities all equal to each other but directed towards points evenly distributed over the surface of the celestial sphere, and if the earth moves *in a right line* through them;

(a) A portion of the bodies will come into the earth's atmosphere and affect the earth's motion in the manner of a direct collision;

(b) The rest will pass by and exert an effect by reason of gravitation.

If the earth's velocity be *less than that of the bodies* the total effect of the action of the bodies of this second class (b) upon the earth's motion will be an exceedingly minute retardation of the earth's motion, even though the extent of the group is infinite. The outer limit of the group, when large, disappears from the expression.

If, however, the earth's velocity be *greater than that of the bodies* the total effect of the action of the bodies of this second class upon the earth's motion will consist of two parts:

First, a very minute acceleration of the earth's motion, depending for its amount upon the absolute velocity of the bodies;

Second, a retardation of the earth dependent for its amount upon the assumed extent of the group. If the action of those bodies which would pass nearer to the earth than a distance P be considered this second part of the action is proportional to the logarithm of P.

That the quantities δA and $\delta A'$ are very small is later to be shown.

13. The change in the form of the expression for ρ_1 as c increases through unity, and its likeness to the abrupt change of the potential of a point relative to a thin spherical shell as the point passes inside the shell is worthy of notice.

The values of A and A′ being independent of P, are dependent upon the inferior limit of the integral (4), which should manifestly be a function of c and θ.

14. In the two forms for ρ_1, viz.,

$$c<1 \qquad \rho_1=\delta(k\log P-A)$$
$$c>1 \qquad \rho_1=\delta A'$$

the values of A, A′, and $k\log P$, can be computed for assumed values of c and P. Thus we get for A and A′

c	*A*	*c*	*A*	*c*	*A′*	*c*	*A′*
0·4	0·6	0·7	1·3	1·1	2·6	1·4	0·2
0·5	0·7	0·8	2·1	1·2	0·9	1·5	0·1
0·6	0·9	0·9	4·3	1·3	0·4	1·6	0·1

For a distance P equal to the radius of the moon's orbit

$k \log P=27{\cdot}8$: for P equal to the radius of the earth's orbit $k \log P=68{\cdot}5$: and for P equal to the distance of a star having a parallax as small as can be measured $k \log P=168$ approximately.

15. If instead of regarding c as constant we consider a series of groups, some having $c<1$ and some having $c>1$, the total value of ρ_1 made up by adding the several values for the groups, will have negative values of A partly or wholly cancelled by positive values of A′.

If c is a continuous variable in a group, integration takes the place of summation. In particular, if c varies through unity, and there is no abrupt change in the number of meteoroids for which $c>1$ and $c<1$, the values of $\log(c-1)$ and $\log(1-c)$ by which the values of A and A′ become infinite when $c=1$ will near the limit cancel each other.

16. Turning now to the meteoroids that come into collision with the earth, we may regard, firstly, the earth as at rest and without attraction and the meteoroids moving past with a uniform velocity v. In a unit of time the earth would encounter the bodies which would fill a cylinder whose radius is r' and length is v. Their mass $=\delta\pi r'^2 v$, and the momentum communicated to the earth is $\delta\pi r'^2 v^2$.

If the earth's attraction be now considered, the value of r'^2 should become p'^2. But by (5)

$$p'^2v^2=2gr^2r'+r'^2v^2=r'^2u^2(x^2+2\beta^2),$$

$$\text{and } \rho=\frac{3\delta}{4r^3}r'^2u^2(x^2+2\beta^2)$$

where ρ includes the effect of the earth's attraction in changing the course of the meteoroids before impact.

17. If we proceed as in sections 10 and 11 to consider a system of meteoroids equally distributed both in locus and in direction of absolute motion, we find for its effect ρ_2 on the earth per second in feet per second,

$$\rho_2=\frac{3\delta}{8r^3}\int_0^\pi \sin\theta\cos\varphi v^2p'^2d\theta=\frac{3\delta r'^2u^2}{16cr^3}\int_{\pm(c-1)}^{c+1}(1-x^2-c^2)(2\beta^2+x^2)dx.$$

This gives for

$$c<1 \qquad \rho_2=\frac{3\delta r'^2u^2}{4r^3}\left(1+2\rho^2+\frac{2c^2}{3}(1-\beta^2)-\frac{c^2}{15}\right)=\mathfrak{A}\delta$$

$$c>1 \qquad \rho_2=\frac{\delta r'^2u^2}{r^3c}\left(\beta^2+c^2+\frac{1}{5}\right) \qquad =\mathfrak{A}'\delta$$

where $\mathfrak{A}$ and $\mathfrak{A}'$ are functions of c.*

* In his treatment of this question (*Astr. Nach.* No. 2573) Oppolzer seems to have overlooked altogether the absolute motions of the meteoroids.

The computed values of $\mathfrak{A}$ and $\mathfrak{A}'$ for different values of c are

c	$\mathfrak{A}$	c	$\mathfrak{A}$	c	$\mathfrak{A}'$	c	$\mathfrak{A}'$
0·4	445	0·7	513	1·0	609	1·3	723
0·5	465	0·8	543	1·1	645	1·4	764
0·6	487	0·9	575	1·2	684	1·5	8.06

18. The absolute velocity of the meteoroids that come into the earth's atmosphere must be in general greater than the earth's velocity. For otherwise the number of shooting stars seen in morning hours would be much greater relatively to those seen in evening hours than observations show. Comets and meteors have like orbits, and no known comet would have at a distance unity a velocity as small as the earth's velocity. Therefore the term $k \log \mathrm{P}$ that enters into the expression for ρ_1 applies to only a small part of the meteoroids. Remembering this, and also the considerations stated in section 15, we may safely say that in the solar system $\rho_2 > 100\rho$, that is, that the *effect upon the earth's motion of the meteors that come into the earth's atmosphere exceeds at least one hundred fold that of the meteors that pass by without impact.*

Art. LII.—*Sources of Trend and Crustal Surplusage in Mountain Structures*;* by Alexander Winchell.

Two facts in mountain structure have baffled, hitherto, the attempts made to arrive at a comprehension of the mechanics of mountain formation. The north-and-south trend of the profounder physiographic features of the earth has no light thrown upon it by any of the orogenic theories commonly entertained. It appears also, from calculations made by Captain C. E. Dutton, Rev. O. Fisher and others, that the shortening of the earth's circumference in cooling from the incrustive stage to the existing temperature would be insufficient to supply the folds and plications wrought into the structure of the mountains. Professor E. W. Claypole has reached a similar result from approximate measurements across a portion of the Appalachian chain in Pennsylvania. These determinations, so far as they are valid, reveal an insufficiency in the contractional theory. Still the mechanical principles of the theory cannot be successfully assailed; and it becomes necessary to seek for some coöperative cause which has hitherto been overlooked. I venture to contribute one suggestion toward the explanation of meridionality, and another suited to aid in the explanation of

* Substance of a communication presented to the Geological Section of the American Association, August 27, 1885.

meridionality and supply also an adequate supplement of crustal surplusage to meet the demands of orogenic phenomena.

1. I assume that meridionality in the earth's surface features, will be granted. There are, indeed many transmeridional features; but they have arisen from geological actions comparatively late. The oldest mountain chains and continental lines tend distinctly toward north-and-south trends; and this predisposition has given direction to many trends of later geological appearance. This is the fundamental impress received by the earth's crust. Evidently, it belongs to a primitive formative stage. We must seek for the cause in the early periods of incrustation.

Now, let us consider lunar-tidal action during those periods. This is a cause to which I appealed in a work published as long ago as 1870, and in periodical literature as early as 1858. Were the moon's tidal efficiency no greater then than at present, its deformative influence must have been experienced by the earth. If ever our planet was a molten sphere, a tidal prolateness stretched its axis in the direction of our satellite; and the axial revolution of the planet changed constantly the portion of matter tidally elevated. As the matter of the molten earth possessed some degree of viscosity, there was then, as always, a *lagging* of the tide, and the moon exerted that action now so well understood, which antagonizes the planet's rotation. After incrustation had begun, this action was not materially diminished. The greater viscosity (or partial rigidity) of the crust would, indeed, tend to shorten the prolate tidal axis; but, in proportion to increase of the index of viscosity; the lagging of the tide would also increase and thus augment the moon's retral action on the tidal protuberance. But the tide continued to rise and fall; and would have continued if the earth had become solid granite. Tidal movements of the crust must result in fractures, friction and displacements.

By as much as the moon's tangential pull on the tidal mass was capable of antagonizing the earth's rotation, by the same it tended to displace the tidal mass toward the west. There must have been some *retral slipping*. The power which could deform a planet could move a raft of frozen matter floating on a molten liquid, especially if floating in the midst of a hemisphere of fragments more or less discontinuous. This westward impulse was continually repeated on each meridian as it came in succession into the position of the tidal crest. The effect was such as could result from a westward push of the forming crust, applied successively over the whole surface within the parallels limiting the tidal disturbance. The slight backward slipping of every part in succession of the tidally moved zone must have developed, in the growing crust, internal structures meridionally disposed. These must have consisted of incipient meridional ridges and accumulations, meridional belts of greater and less strength, and belts of differentiated internal elements —either in form, position or material.

Now, while refrigerative contraction would necessarily develop wrinkles in the crust, it would not determine for them a meridional trend. This was pointed out by Captain Dutton. The tidal action instanced would, however, produce this result. Nor can it be pronounced insignificant in amount, since evidently, a force which could move the earth on its axis could move a floating patch of the earth's shattered crust.

It will be readily understood that the retral slipping of the tidal mass would be greatest at the crest, as first shown by Professor G. H. Darwin, and would diminish according to a certain law, toward the north and south. As the mean declination of the moon may be regarded as zero, the greatest mean slipping would be on the equator. The sub-meridional pre-dispositions instituted would therefore trend from the equator eastward of north and south.

If, as is probable, the moon's distance were much less during the incrustive periods, the tidal results cited would become more conspicuous. If, as is equally probable, the earth's rotation were correspondingly more rapid, the tidal results would be correspondingly further augmented.

The actions here considered pertain necessarily to the early forming stages of the crust, and have impressed its profounder features. Later, with increased rigidity of crust, tidally formed predispositions were less controlling; and with the growth of oceans, crustal pressures were experienced from other directions.*

2. Meridional trends would be further promoted by the secular subsidence of the earth's equatorial protuberance. That this accompaniment of the slow retardation of the earth's rotary motion must have exerted geological influences was first distinctly shown by Professor J. E. Todd; and I have elsewhere recorded the fact;† but its determinative influence on the trends

* These views, for the greater part, were first propounded by me in printed form in *World Life*, Nov., 1883, pp. 252–255, 350–355; but were taught in lectures several years previously.

† The ample résumé of W. B. Taylor, in the October number of this Journal will not be overlooked. An abstract of this paper was read before the Geological Section immediately after the presentation of my own communication.

I embrace this opportunity to remind the reader that the first conception of the now accepted cause of the moon's synchronistic motions must be credited to Kant rather than Ferrel, as Mr. Taylor thinks. In 1754, Kant presented to the *Royal Academy of Sciences*, Berlin, a memoir entitled: *Untersuchung der Frage ob die Erde in ihrer Umdrehung um die Achse wodurch sie die Abwechselung des Tages und der Nacht hervorbringt einige Veränderung seit den ersten Zeiten ihres Ursprunges erlitten habe.* After appealing to the action of the tides as a cause of diminution of the earth's rotational velocity, he says: "Dieses legt uns auf of the earth's wrinkles was first conceived by me in the early spring of 1885.* To say that the equatorial protuberance underwent a secular subsidence is to say that the equatorial circumference of the earth, as an effect of retarded rotation, has shortened more than the polar—that indeed the polar circumference has lengthened. That is, the greatest lateral pressure has been experienced from east and west around the equator. An excess of pressure in this direction must develope crustal changes having north and south continuity. Whether the results were foldings or crushings together, or over slippings, their axis-trends would be meridional. This cause then, conspired with early tidal action in predetermining the direction of the longitudinal dimension of the earth's structural features.

3. The same cause produced crustal surplusage around the equatorial zone. Aside from refrigerative contraction of the earth, the equatorial circumference diminished while the polar increased. This cause alone would, therefore, have developed meridional mountain plications over the protuberant belt. If the crustal surplusage resulting from refrigerative contraction was less than existing mountain plications demand, here is a cause which would supplement the supply from that source. Careful measurements may show that the supplementary surplusage needed is not greater than calculations may prove this cause capable of affording. If so, the contractional theory will experience the relief which every physical geologist must have desired, if not anticipated.

4. The meridional predisposition depending on subsidence of equatorial protuberance would be developed north and south of the equator as far as the parallels marking the limits of the protuberance. The meridional predisposition induced by lunar tidal action would be experienced north and south of the equator to the latitude marking the limits of the prolate tidal swell resulting from the moon when over the equator, *plus* the amount of the moon's lunar-monthly declinations. The extent of these actions, therefore, is as great as the actual trends demand; and embraces also, all the strongly plicated portions of the earth's surface.

einmal die Ursache deutlich dar, die den Mond genöthigt hat, in seinem Umlaufe um die Erde immer diesselbe Seite zuzukehren." He proceeds to say that this phenomenon is not due to overloading on the nearer side; that the influence began at the moment when the moon abandoned the earth; that the moon was at first in a fluid state, and that it then rotated with much greater velocity than at present. This historic fact deprives Mr. Ferrel of priority, but does not diminish the credit due him. Kant's memoir was dated at Königsberg, the identical spot where Helmholtz, one hundred years later, put forth the same thoughts as original with him.

* It is but just credit to a sagacious pupil, Mr. W. E. Bond, of Albion, N. Y., to say that he embodied a clear and original exposition of the principle in a thesis presented in June.

Art. LIII.—*The Genealogy and the Age of the Species in the Southern Old-tertiary;* by Otto Meyer, Ph.D.

[Parts of this article were read at the meeting of the National Academy of Sciences, Albany, N. Y., November 12th.]

Part III.

Reply to Criticisms.

In Part II of my essay I endeavored for several reasons to be as brief as possible. Such authors and sentences only were quoted as were considered absolutely necessary, saying: "Interesting as it would be, I cannot review here the whole literature regarding this subject," (p. 65). In a few cases it was ventured to condense the result of long researches into one statement. The criticisms* in the October number of this Journal have shown that I was wrong in doing so, and I now propose to add further explanations in consequence of the misunderstandings which have resulted.

Professor Hilgard contests my statements about his work, and says that the stratigraphy clearly shows the succession to be commencing below, Claiborne, Jackson and Vicksburg. In replying to his remarks I shall endeavor to show that I was right in affirming that Professor Hilgard has not proved this to be the true succession. I designate this statement of mine as Statement B. Mr. Hilgard also says that he differs from me "more fundamentally in the sweeping statement" that "only a competent and careful examination of the fossils could indicate the relations of the Old-tertiary strata of Mississippi;" I am therefore obliged also to explain this statement of mine (Statement A) in detail. Professor Hilgard's observations, as far as all-important points are concerned, are published in his "Geological Report of Mississippi;" Jackson, 1860 (beyond referred to as "I"), and "On the Tertiary formations of Mississippi and Alabama;" this Journal, Jan. 1867, pp. 29–41 (beyond abbreviated to "II").

Statement A.

To explain this statement I consider it necessary to proceed in a somewhat methodical manner. We ask at first: can we trace the typical Vicksburg or Claiborne bed over or under the

*1. The Old Tertiary of the Southwest; by E. W. Hilgard, pp. 266–269. 2. Remarks on a paper of Dr. Otto Meyer on "Species in the Southern Old-tertiary;" by Eugene A. Smith, pp. 270–275. 3. Observations upon the Tertiary of Alabama; by T. H. Aldrich, ibid., pp. 300–308.

typical Jackson bed? If two of the beds were connected directly, for instance, by a river, and we could follow the strata foot by foot, then stratigraphy alone could determine their relative age without an examination of fossils. As this is not possible there remains only to determine strata at a distance from a typical locality as belonging positively to a certain period, and then to study the stratigraphical relations to each other or to one of the typical localities. Can we use lithological characters in the determination, of a stratum, whether it was deposited in the Clairbornian, Jacksonian or Vicksburgian time? Professor Smith and Mr. Aldrich seem inclined to use lithological characters; but I cannot attribute to them any value, and Professor Hilgard seems to be of the same opinion, for he says (II, p. 30): "A great deal of the obscurity in which the relative age of the Southwestern Tertiary has been involved, is owing to too great a reliance placed by most observers on lithological characters, differences as well as resemblances," and (II, p. 31) "Nowhere has the geologist more need of divesting himself of reliance upon lithological characters, than in the study of the Mississippi Eocene." So we see that we are led to the examination of fossils, and I have only to show now that this examination must be "competent and careful."

Certain fossils may have lived in a single one of three periods only, as far at least as the region in question is concerned. Such fossils are then characteristic of this era. They may be designated here as α fossils. I am not able to name with certainty any example of such a fossil in Alabama and Mississippi.

Another class of fossils, here called β fossils, occur in two or more of these beds, so nearly alike that no differences are recognized and known. For instance, I am unable to discriminate between *Cytherea minima* Lea from Jackson and Claiborne, between *Alveinus minutus* Conr. from Vicksburg, Jackson and Claiborne, and nobody else has as yet published a recognized difference.

A third class of fossils, γ fossils, is numerous in the Southern Old-tertiary. It embraces such fossils as occur in two or three beds in forms, which, though similar, are in some respects more or less different from each other; these differences being in some cases so slight as to be apparent only on the closest examination; in other cases more apparent, and sometimes rather great. For instance, *Venericardia planicosta* in Jackson and Claiborne differ, but the difference is so slight, that I have not applied varietal names. In other cases I consider the differences sufficient to justify the use of a varietal or even a specific name. For examples, see Part I of this essay.

A fourth class, called δ fossils, are such as have been hitherto found in one of the three localities only, with no similar forms in the other beds, with which they might be confounded. For instance, *Arca Mississippiensis* Conr. in Vicksburg.

Having made this classification, we ask what is the value of the fossils in each of these groups for determining the age of a newly found bed in the region in question. The β fossils are evidently useless for this purpose. For instance, *Cytherea minima* Lea is of no use in distinguishing a Clairbornian from a Jacksonian bed, although this fossil, as long as it is not found in Vicksburg, may be considered a δ fossil in reference to the Vicksburgian.

This argument seems simple, and yet my emphasizing it appears to have been in vain. I had to remonstrate in Part II of this essay (p. 63) against Lyell's using, to demonstrate a Clairbornian age, fossils which he himself cites on the same page as occur ring in Jackson and Vicksburg. Nevertheless, Mr. Aldrich and Professor Smith cite (p. 306 and p. 274) fossils, as for instance, *Rostellaria velata* Conr., *Pecten Deshaysi* Lea, *Turbinolia Maclurii* Lea, *Dentalium thalloides* Conr., *Astarte sulcata* Lea, *Melongena alveata* Conr., *Infundibulum trochiformis*, *Turritella lineata*, found in Jackson as well as in Claiborne, to demonstrate the Clairbornian age of a bed. Hilgard uses on p. 269 the genus *Orbitoides* to distinguish from the Jacksonian a bed in Louisiana as Vicksburgian, while he himself has determined fourteen years ago both known species of Orbitoides, and a new one besides, as occurring in the Jacksonian strata of the same State (See Hopkins's Second Annual Report of the Geol. Surv. of Louisiana. New Orleans, 1871; pp. 11, 12, 13.)

A single α fossil, however, would characterize a bed at once. But what are α fossils? They may exist among the δ fossils, but they must not be confounded with them. δ fossils, though hitherto found in only one bed, are constantly being found on each new examination, in the other beds. I have myself found a large number of species at Jackson, which were known before only from Vicksburg or Claiborne. *Venericardia rotunda* Lea, and the allied form *V. diversidentata* Meyer have been known before only from Claiborne and Jackson. Neither Conrad, nor Hilgard, nor I have found this type in Vicksburg. So we might have been tempted to consider this form as a good δ fossil in reference to Vicksburg. Now Mr. Aldrich says (his article, p. 308) that he has it from Vicksburg. There is no end of this. In the most thoroughly explored Tertiary localities in Germany, almost every new examination extends the range of known species, beside bringing to light new ones. There is another reason why we cannot designate any fossil as α fossil. We cannot expect that all the species that lived during the Vicksburgian era should be represented at the locality at Vicksburg. During the same time very different species may have lived in Alabama, and among their remains may be α fossils. Since, therefore, we do not know α fossils, and since β fossils are useless, there remain only γ and δ fossils for the determination of a bed. A single δ fossil counts for little, but a number of them affords cumulative evidence. Of the greatest importance for our purpose are the γ fossils, since a great number belong in this class, while δ fossils are comparatively scarce. If we find, for instance, in a new bed nothing but *Astarte sulcata* Lea, *Venericardia rotunda* Lea and *Mitra pactilis* Conr., and all these in that form in which they occur in the typical Claibornian bed, we may be justified in referring such a bed to the Claibornian. For such a comparison, however, it is essential that we recognize and state the differences in the forms as they occur in the three beds. If a difference between two γ forms is not recognized, this γ fossil becomes useless for the identification of beds. For instance, if Mr. Aldrich sees no difference between *Venericardia rotunda* Lea and *Ven. diversidentata* Meyer, but says they are alike (his article, p. 307), it is of no use, that he cites *Ven. rotunda* among those forms, which demonstrate the Claibornian age of a bed (p. 306). If we observe the differences, however, these γ fossils (we may speak here of "recognized" γ fossils) are as valuable as δ fossils are.

As the result of these examinations, we can say now the following:

1. There are no α fossils. That is, there exists no single species from which alone we can characterize with certainty a bed as Claibornian, etc.
2. γ fossils which are not "recognized," and β fossils are useless in the determination of beds.
3. Beds can be determined only by δ fossils and "recognized" γ fossils.

Having now collected in Vicksburg, Jackson and Claiborne many species which were not known before, or known only from one of the localities at the time of Hilgard's observations, I have seen, that a number of such species which he, according to our classification, had to consider as δ fossils do not belong to this class and that others exist, which he did not know. Moreover, and this is more important, he did not use in a systematic way the method of "recognizing" the γ fossils. After having worked according to the above method, and with my present knowledge of the fossils, through all his determinations of beds, I come to the conclusion, that some of them are imaginary, many of them may be probable, but no one is fully established. As a positive determination of the beds, however, is the foundation of conclusions I made my statement A: "only a competent and careful examination of the fossils could indicate the relations of the Old-tertiary strata of Mississippi."

I select here three cases, where the determinations of Hilgard are imaginary. The first includes those based upon Zeuglodon. Wherever he finds Zeuglodon or hears that the bones of this animal have been found, he maps the locality as Jacksonian. He thus uses Zeuglodon practically as a Jacksonian α fossil, It is not necessary to repeat here what is said about α fossils in general, but we may be somewhat surprised to find that, according to our definition, Zeuglodon is even not a δ fossil, that is, it is not found among the marine shells of Jackson (see list of Jackson fossils, I, p. 132). What are the facts, then, cited in I, where Zeuglodon is found with other fossils?

1. From observations at Moody's Branch and in the McNutt Hills, Hilgard *deduces* that Zeuglodon bones have been found *above* the Jackson fossils, above but separated only by a few feet (pp. 130, 131).
2. Zeuglodon found with *Cypraea fenestralis* and *Conus tortilis* "and a very large *Pyrula* not seen elsewhere" (p. 134). The first species is a Jacksonian δ fossil, but *Conus tortilis* (= *Conus sauridens*) extends from Vicksburg to Claiborne (see part I of this essay, p. 466).
3. Zeuglodon found with *Ostrea*, *Pecten nuperus*, *Scutella*. *Pecten nuperus* (=*P. Deshaysi* Lea) is either a β or not "recognized" γ fossil.
4. Zeuglodon associated in prairies with "an oyster somewhat resembling *Gryphæa convexa* of the Cretaceous, the vertebræ and teeth of fish and a branching coral (*Eschara* sp.), (p. 128.)

On these facts, so far as I am aware, Hilgard bases the use of Zeuglodon as a Jacksonian α fossil in I. In a later article (this Journal, 1866, p. 68), he cites Zeuglodon with three Jacksonian δ fossils. I collected and have in my possession parts of the skull, etc. of Zeuglodon associated with the oyster and coral just mentioned, and *Pecten Deshaysi* Lea, between Barnett and Pachuta, Clarke Co., Miss., near the New Orleans R. R.,* I have seen a large vertebra of Zeuglodon in a little street in Enterprise, Miss., that is in a locality which I consider as Clairbornian; I do not know, however, whether it was found there, or brought from a distance. I have had sent me from Claiborne the vertebra of a large marine mammal, which I have presented to the Yale College Museum; I do not know, however, where it was found. The whole literature about Zeuglodon cannot be cited here, but altogether I must say that we do not know when and how long Zeuglodon was living in the waters of the South-

* The greatest part of the same specimen had been collected, before I came to this locality. I saw it afterwards in the exhibition in New Orleans (1884), exhibited by the State of Mississippi.

ern Old-tertiary. All localities which are mapped by Professor Hilgard as Jacksonian, based upon the presence of nothing but Zeuglodon, are in my opinion mapped without sufficient reason.

The second case concerns such strata as are determined as Vicksburgian by nothing except *Orbitoides.* Orbitoides has been found in Louisiana in a fauna determined by Hilgard as Jacksonian. I found moreover a specimen of Orbitoides in stratum *e* and one in stratum *b* in Claiborne.* Orbitoides is a β fossil from Vicksburg to Claiborne, and this Vicksburgian determination is therefore without any foundation.

The third case refers to the strata north of Enterprise, mapped by Hilgard as "siliceous Claiborne." From this whole large formation he cites altogether only two species, *Venericardia planicosta* and "*Cardium Nicolleti?*" (I, p. 124). The first species is known from Claiborne and Jackson, the second from Jackson. Therefore I am of the opinion that this formation north of Enterprise is determined as Claibornian without a shadow of reason.

Statement B.

Hilgard condenses his proof of the succession in the sentence printed in italics:† "Going down streams flowing to the southward, the strata successively sink below the water's edge as the observer progresses, in the order Claiborne, Jackson, Vicksburg, Grand Gulf, as identified by their leading fossils." This is based mainly upon two profiles in two different meridians, the first through Jackson along the Pearl river, the second along the Chickasawhay.

The proof rests on two criteria. The first is that all the strata are determined as Claibornian, Jacksonian and Vicksburgian in a reliable way; the second is, that they have the stratigraphical relation which Hilgard here claims, that is, that the dip in his two profiles is not only south, but nothing else than south. Now we might stop and say: the first criterion is not correct, we cannot rely upon his determination of the beds, consequently his deductions from their stratigraphical relation have no value. This is a fact; but I wish to show, besides, that the second criterion is even worse than unreliable, that it contradicts Hilgard's own observations.

In I, Hilgard states that the dip of the strata in Mississippi is south, with one exception. This exception, however, refers just to one of our profiles. In I, pp. 128, 129, he explains, that the dip from Jackson to Canton is north. He calls this

* Part II, of this essay, pp. 69, 70; see also the remark below.
† His article, p. 268.

"some irregularity" (p. 128). But on our second profile, along the Chickasawhay, he mentions only southern dip. In II, however, he says, p. 37, that going southward, he finds an anomalous reappearance of older strata in *two* different meridians. "One of the cases is noticed in my report (p. 128). From Jackson to Canton, etc." "*The other case occurs on the Chickasawhay, contrary to the statement in my report* (*l. c.*), *in making which I overlooked some specimens and notes of* 1855, *then mislaid.* Then he describes how Jackson strata "dip up" again at the most southern part of the belt of the marine Tertiary. Further he continues (p. 38): "*So far, I have been unable to observe the marine Tertiary in juxtaposition with the Grand Gulf group on the Chickasawhay and cannot positively assert that the former dips under the latter at all.*" These statements, so far as I am aware, Prof. Hilgard has not withdrawn in later publications and therefore I cannot imagine anything else, than, that in penning his last article against my theory he has entirely forgotten his former publications.

Before continuing, I give now a general profile across the marine Tertiary of Mississippi and Alabama, based on these observations of Hilgard and on my former researches.

General profile across the Southern Old-tertiary.

A′, A″, Lignitiferous strata with gypsum, without Tertiary shells; (A′, Eolignitic, A″, Grand Gulf); V, Vicksburgian; J, Jacksonian; C, Claibornian.

According to this profile the waters of the Tertiary formed a belt in the direction E.S.E. to W.N.W. (as far as Alabama and Mississippi are concerned). Their deposits *overlie* the A″, that is, the Grand Gulf group. If this profile is right, all existing maps are wrong, since they represent the marine Old-tertiary as underlying the Grand Gulf, which is usually colored as Miocene (see the maps of Heilprin and McGee); while I think that, with perhaps the exception of some local fresh-water formations, the Grand Gulf group is older than the marine Old-tertiary. With this profile I need not explain why in the northern part of the belt the dip is everywhere south, in the southern part, however, becomes north, and why older strata reappear when we go south.

This phenomenon, observed twice by Hilgard in Mississippi, was also observed by Tuomey in Alabama. I cite here Hilgard, II, pp. 36, 37: "In the general (north and south) section accompanying Tuomey's geological maps of Alabama, the Tertiary strata are represented as dipping southward, conformably with those of the Cretaceous. Nevertheless, in the section from Baker's Bluff to the lower salt works on the Tombigby, he finds the white limestone (=Jackson and Vicksburg groups) occupying 'a trough-like depression in the Buhrstone formation.' In conversations with me, a few months prior to his death, he expressed his belief that such was the general disposition of the Tertiary strata, and that on close examination it would turn out that the strata passed over in going southward from the border of the Cretaceous, would be again passed over in reversed order still farther south. My report of the existence in Mississippi of a lignitiferous formation (the Grand Gulf group) southward of the marine Tertiary seemed to confirm this view." We see that Tuomey before his death came to the same opinion about the stratigraphy of the Southern Old-tertiary that is represented by the above profile.

How does Hilgard now explain this "anomalous reappearance of older strata"? He finds the cause of it in local upheavals. In reference to Jackson, he says (I, p. 129): "It seems difficult to account for this condition of things unless by supposing a local upheaval of the underlying formation to have taken place before the deposition of the lowest of the Jackson stage." In II, p. 38, he explains the other two facts in a similar way: "Similarly, if between Baker's Bluff and the Salt Works on the Tombigby, or between Dr. Miller's and Red Bluff on the Chickasawhay, the sea bottom had a slight trough-shaped undulation (such as the ocean beds of our time frequently exhibit) the existing state of things would result." We see that Hilgard has to use the hypothesis of local undulation in each of the three sections which are studied, and each time at the southern part of the belt. This is not probable, but it is possible.

There are, however, other observations of Hilgard, which agree with my cross-section, but which force him to more hazardous hypotheses. As already cited, he has not observed the juxtaposition of the Grand Gulf group with the marine Tertiary on the Chickasawhay. He continues, however (II, p. 38), that, "in Hinds, Rankin and Smith counties their relative age is clearly exhibited." From these words we might expect that he has observed there the Grand Gulf beds overlapping the marine Tertiary, especially the Vicksburgian. I cannot find any place in Hilgard's publications where this is actually proved. On the contrary, in the neighborhood of Brandon he finds the lignito-gypseous beds of the Grand Gulf group *overlaid* by limestone with Orbitoides (I, p. 138, II, p. 40). He does not make now the simple conclusion, that the Vicksburgian overlies the Grand Gulf, but he argues: as the Vicksburgian overlies these strata, they cannot belong to the Grand Gulf, they must belong to the Vicksburgian. These strata have precisely the characters of the Grand Gulf group, and Hilgard says expressly: "*and so I referred them until I found them overlaid by a string of limestone nodules containing Orbitoides, about forty-five feet above the uppermost sands of the Vicksburg group.*" Limestone with Orbitoides is certainly marine Tertiary and is considered by Hilgard always as Vicksburgian. Therefore I fail to see not only why he does not conclude from this observation that the marine Tertiary overlies the Grand Gulf, but also why he does not mention it as a case where the Vicksburg group "dips up," or has a northern dip, since this follows from his statement of the heights.

If Vicksburg is at the top of the marine Old-tertiary we should expect to find below it the Jacksonian and Clabornian. We cannot expect to find exactly the same species as at Jackson and at Claiborne; for during the Jacksonian and Claibornian time a different fauna may have lived in Vicksburg, consisting perhaps of a few species only. But what we have to expect is to find at least marine Tertiary. What is, however, the base of the Vicksburg Tertiary at Vicksburg? I have not observed it, but according to Hilgard, I, p. 141, there are below my "Lower Vicksburgian" at first "25 feet of gray or black lignitic clays or sands with iron pyrites, exuding salts and sulphuretted hydrogen." Then "3 feet solid, lustrous lignite, with whitish cleavage planes," and then follows the top of a limestone without fossils. This is what we should expect according to my profile—lignitiferous strata without marine fossils; even the salt, which according to Hilgard (I, p. 148) characterizes the Grand Gulf, is not wanting.

What is under the Jacksonian at Jackson and neighborhood? We find (I, p. 123): "In the wells bored in South Madison by the Rev. Mr. Lambuth, the fossiliferous marine strata of the Jackson group were passed through at about 90 feet, after which 'blue dirt,' with selenite, several ledges of sandstone and a lignite bed of 40 feet thickness were struck, but no more marine strata were reached at a depth of 415 feet." We see here that the non-marine lignitiferous strata, which crop out at a few places in Vicksburg, appear here near Jackson, but north of it, in a depth of 90 feet, and then have a thickness of at least 325 feet. Hilgard continues: "At Jackson, however, at the Penitentiary well, after passing through 32 feet of surface material and fossiliferous strata of the Jackson age, lignitic clays were penetrated for 418 feet, after which a bed of shells

20 feet thick, extremely rich in greensand, was passed through into water-bearing sand. The friable shells brought up by the auger are too much comminuted to allow of determination. Whether this led to a continuation of the Shongalo deposit or an independent basin or estuary; there can be little doubt that it also is of the Claiborne age." So we see that the non-marine lignitiferous strata below the marine Tertiary have a thickness of 418 feet beneath Jackson. Hilgard's last sentence gives us his explanation of these phenomena, precisely expressed on p. 107: "The several marine stages are in most cases separated by intervening strata of dark colored, often lignitic clays, as above mentioned; moreover, both the base and the top of the Older Tertiary are formed by strata of this character of considerable thickness." Now it seems probable that, especially in the lower part of the marine Tertiary, lignitiferous beds of moderate thickness may appear between the other strata; but what Hilgard advances here looks to me extremely improbable. A Jacksonian of 30 feet, separated from a Claibornian, of which no shell is determined, by 418 feet non-marine lignitiferous strata appears to me a hazardous hypothesis. In the same way Hilgard is obliged to accept that the Vicksburgian is separated from the Jacksonian below it by the same kind of strata of unknown thickness. He might reply: the thickness of these lignitiferous strata in Vicksburg is only 28 feet and then follows a Jacksonian limestone (I, p. 141), but what fragment of a fossil has he for such a determination of this limestone?

The most inexplicable of all things for Mr. Hilgard, however, must be and is the fact, that in his Grand Gulf formation, which surpasses greatly in extent the whole marine Tertiary in Mississippi, not a fragment of a marine shell has been found (see I and II, pp. 40, 41). II, p. 40, says: "Neither Wailes, who resided among them . . . . nor myself, who have delved in scores of exposures, have ever found a trace of any fossil whatsoever." If the Grand Gulf group originated by a rising of the coast *after* the time of the Old-Tertiary, why has not a single Miocene shell been found in this enormous "Miocene" territory? In II, p. 41, Hilgard attempts an explanation. "Should the chain of the Antilles, after the close of the Eocene epoch, have for some time cut off the Gulf of Mexico from the Atlantic, it seems possible that the deposits of the former might have changed their character to the extent required by the facts observed. A strong influx of fresh water—perhaps that pertaining to the Great Lignite era—from the continent might for the time being have extinguished the Eocene marine fauna without replacing it by another sufficiently numerous to be readily detected in the deposits of the period, which might thus correspond to the Atlantic Miocene. Upon the subsequent irruption of the Gulf Stream through the Antilles chain, the formation of normal marine deposits along the margin of the Gulf would be resumed."

This and the other hypotheses, to which Hilgard is forced by his observations, appear to me so surprising, that I found no other explanation than that which was expressed by my statement, that he accepted the theory of his predecessors as a proved fact. This statement is also attacked with much warmth by Hilgard in his article, but I do not see any reason to withdraw either this or my other statements about his work. In one case, however, Hilgard protests with reason against one of my sentences (p. 65): "Therefore Hilgard made this belt make a sharp curve around Jackson." This may sound, as he understands it, as if he did not find the Vicksburgian south of Jackson, where he maps it. This I do not wish to say. I withdraw the sentence and replace it by the following: "Therefore Hilgard had here at least to look for a deviation from the regular straight line."

The only (marine?) shells, so far as I am aware, that are known in the Grand Gulf group, were observed by A. Bigelow * in Alabama. In the lower part of "a sandstone formation, which is quite extensive in the southern part of that State," he observes "very obscure impressions of shells, apparently all bivalves. There are evidently several genera; the outlines of some are quite regular, and in two or three a part of the hinge is discernible." This does not sound like Miocene. Bigelow continues on the next page: "It is questionable whether impressions of shells can be found in any other place than the one I have mentioned." He finishes his article: "The age of this sandstone I am unable to determine; I hope to have the opportunity soon of submitting my specimens to some one well acquainted with fossils." It is to be regretted that Bigelow seems to have found no such opportunity. Can any one give any information in regard to these fossils?

The Grand Gulf in Louisiana is described by Hopkins.† I fail to see in his report any *proof* of an overlying of this group over the Vicksburgian. On the contrary, *at the southern part* of the marine Tertiary belt, in Catahoula parish (p. 16), he states that *a bed* containing *Corbula alta overlies* lignitic beds, and gives an explanation of this fact similar to that of Hilgard. Hopkins's explanation of the absence of fossils in the Grand Gulf is the following (p. 20): "It is, that the water must have

* This Journal, II, 1846, ii, pp. 419–422.
† Second Annual Report Geol. Surv. of Louisiana, 1871, pp. 18–21

contained too much salt for the fresh-water species to live in it, and too little for the marine, and must have been subject to great fluctuations in freshness by the influx of water in wet seasons, and its evaporation in dry, to exclude species that can live in brackish water." From the *advanced state of erosion of the Grand Gulf group* under its cover of drift (pp. 20, 21) Hopkins concludes that at least during the Pliocene time this group must have been dry land. On p. 18 Professor Hopkins says: "A rather sharp series of escarpments marks the northern boundary of the Grand Gulf." These are perhaps old shore-lines.

Professor Hilgard determined a certain portion of Louisiana territory in Landry parish *within 70 miles of the Gulf coast at first as Grand Gulf, then as Cretaceous.** In another article† he says: "While the Vicksburg rocks show at all long exposures a distinct southward dip of some three to five degrees, the position of the Grand Gulf strata can rarely be shown to be otherwise than nearly or quite horizontal on the average; although in many cases faults or subsidences have caused them *to dip, sometimes quite steeply, in almost any direction.*" Why have these "Miocene or Post-miocene" disturbances left the Eocene strata undisturbed?

Interesting as it would be to discuss more fully the Grand Gulf and Eolignitic, brevity again obliges me to turn to Professor Smith and Mr. Aldrich. I myself described limestone in the profile of Claiborne above the proper Clabornian, stratum "*e*." Therefore Professor Smith might have omitted everything by which he attemps to show the same thing, that is, that this stratum is sometimes overlaid by limestone. His article and the corresponding part in that of Mr. Aldrich, however, attempt to show, moreover, that *Orbitoides* and *Spondylus dumosus* occur above "*e*." Professor Smith as well as Mr. Aldrich believe that they have proved this beyond doubt. While still questioning it, I will here not dispute it. It follows that no conclusion can be made either from *Orbitoides* or from *Spondylus dumosus*, as they occur above as well as below stratum "*e*." I fail, however, to find anything in Professor Smith's article that proves the fauna in Jackson or Vicksburg in Mississippi to be more recent than that in the stratum "*e*" in Claiborne in Alabama, and hence say nothing further on this point.

As to three charges made against me I add a few words: I indicated some words in a quotation from Lyell by . . . . , by dots. Professor Smith says that I thus carefully omitted the proof of Lyell. I carefully scrutinized these words before

* See this Journal, Nov., 1869, p. 343. † This Journal, July, 1881, p. 58.

using dots, and dotted them because if they had been cited, it would have been necessary to show the reader that they did not prove anything at all. I should have been obliged to show how Lyell fails to give the proof of this statement here or afterwards, how no author in the following literature proves this statement about this locality, and how Tuomey, a good observer, knowing the strata of Alabama better than anybody before him and accepting all conclusions of Lyell, flatly contradicts this statement by saying: "It must be recollected that the Claiborne fossiliferous bed is nowhere in absolute juxtaposition with the overlying *Orbitoides* limestone," which words I cited afterwards (p. 64).

On page 273, Professor Smith says that I did not quote an article of Hale; and Mr. Aldrich also refers (p. 306) to the same apparent neglect. As I mention Mr. Hale's name on page 69, in the fourth line, it was sufficiently manifest that I knew of his published paper, and since I say that I do not attempt to review the whole literature, they should have inferred that I found nothing of sufficient importance in Mr. Hale's article to require a further mention. As attention has thus been drawn to Mr. Hale's article, I will here state that, although it is not directly connected with our subject, one fact which he mentions is of the highest interest and has impressed me for a long time. On page 357 and 358 he speaks of a terrestrial vertebrate fauna, found as far as I can understand him, in the proper Claibornian stratum. A cranium from this fauna in his cabinet he compares with Glyptodon. I have not found anything about this in the rest of the literature. Can anyone give information about this very remarkable fauna and tell where the specimens are?

One of my main purposes in Claiborne was to keep the fossils of the different strata separate from each other. Therefore the suggestion of Professor Smith, that my specimen of Orbitoides in "*b*" (and in "*e*") and Conrad's *Spondylus dumosus* might have been washed down from the higher strata above, is not very flattering either to me or to Conrad. Conrad says, that he found his specimen attached to an oyster in the resp. stratum. So this oyster must have been washed down also. But I fail to see *à priori*, why Professor Smith suggests this possibility. As yet no observer has found a single specimen of these two species in the strata above "*e*" in the Claiborne profile. Therefore, as far as we know from observations, they do not occur there. How, then, could they be washed down? As for *Spondylus dumosus*, moreover, Mr. Aldrich states (p. 305) that he has "lately found it at Hatchitigbee bluff, 25 feet beneath the buhrstone," that is still lower beneath the proper Claibornian than Conrad found it.

Mr. Aldrich says, p. 303: "Now if Dr. Meyer's theory is correct we should find underneath the Claiborne beds the Jackson and the Vicksburg formations."

He then proceeds to show that several hundred feet of marine Tertiary occur beneath the Claibornian, while no one has demonstrated the existence of such strata below the Vicksburgian. Does now Mr. Aldrich expect that all those species which lived during the Vicksburgian era at Vicksburg and these alone lived also at every place in Alabama? I explained that the fauna of Wood's Bluff, a locality far below the Claibornian, contains entirely new forms or β fossils, which two classes do not indicate an age. It contains also a few Vicksburgian (one Jacksonian) δ fossils.* This is as much as we can expect according to my theory, and I gave this as my "reason 6." Mr. Aldrich seems now to have the following opinion about the range of, for instance, that variety of *Pleurotoma terebralis*, which Conrad calls *P. cristata:* this variety occurs in the lowest part of the whole Tertiary (Wood's Bluff and lower); then follow 700 feet without it, then the whole Claibornian and the whole Jacksonian without it and at the top we find the same variety again in the Vicksburgian. While according to my theory this form occurs probably only in the Vicksburgian. Mr. Aldrich misunderstands my reasons 3 to 6 so thoroughly that it is perhaps useless to explain them at length and to compare this explanation with what he says about them. Three points only may be mentioned.

There may be two different opinions about the relation of the two varieties of a γ fossil: 1. They have a common near ancestor. 2. They are connected by direct descent. As all agree that the Vicksburgian, Jacksonian and Claibornian was deposited in different times, I think that the second opinion is the best explanation. This—that this opinion is the most probable—is the theory advanced in the first part of this essay. Professor Smith calls this (p. 270): "inferences derived from what he thinks should have been the course of evolution." He as well as Mr. Aldrich think now that I base my reasons 3 to 5 upon this theory. Only reason 5 is based upon it. Mr. Aldrich says that the argument in reason 5 "passes his understanding." This is not my fault, it is not my duty to explain to him the "biogenetische Grundgesetz." Reasons 3 and 4 are general methods, independent of direct or indirect descent. In reason 7 I cite the observation of an author and say what conclusions I make from them. Mr. Aldrich need not accept my conclusions if he thinks they are wrong, but he has no

* Mr. Aldrich's mention of *Natica Mississippiensis* Conr. from Wood's Bluff (p 308 corroborates Professor Heilprin's determination of this Vicksburgian fossil.

right to say: "Here Dr. Meyer is guilty of assuming a parallelism which he has not seen." On p. 305 he says: "if they are below this then Dr. Meyer has to sandwich in between Claiborne and Jackson the whole of the Buhrstone formation." Mr. Aldrich misunderstands me again. I do not and I need not accept such a theory of "sandwiches" of several hundred feet thickness, as Hilgard is obliged to do and Mr. Aldrich must do with him, if he wishes to maintain the old theory. To avoid further misunderstandings I give here a table of the strata in Mississippi and Alabama. As far as the Red Bluff is concerned, I accept Hilgard's determination (I, pp. 135, 136), as this is apparently corroborated by my own observations on fossils.

	Miss.	Ala.
Claibornian.	Enterprise?	The Claiborne profile.
Jacksonian.	Jackson, (Moody's Branch).	Buhrstone, etc.
Vicksburgian.	Red Bluff. Higher Vicksburgian. Middle Vicksburgian. Lower Vicksburgian.	Wood's Bluff and strata below.

Grand Gulf = ? Eolignitic.

Art. LIV.—*The Condensing Hygrometer and the Psychrometer;* by Henry A. Hazen.

[Read before the Washington Philosophical Society, Oct. 24, 1885.]

Much has been written and said against the use of a condensing hygrometer for determining accurately the moisture contents of the air. The following is a recent example: Mr. R. Strachan of England, writes as follows to Symons's Meteorological Magazine for June, 1885: "A condensing hygrometer, whether Daniell's, Regnault's, Dines's or Alluard's, has the thermometer's bulb immersed in a cooling medium, and one surface of the dew plate is also in contact with the cooling medium, but the surface upon which the dew is formed is cooled by conduction and is exposed to the air which may be many degrees, 50 or 60, or more, warmer. In these circumstances, when dew appears, the thermometer must be colder than the outside of the place. When the dew disappears the thermometer cannot have received the same addition of heat as the outer surface of the plate." There seems to be a slight misconception by the writer of the above as to the amount of the effect under these conditions. It is an undoubted fact that in the hands of an unskilled observer the results will be exceedingly faulty, but if due attention be paid to the elimination of errors, a fairly constant determination of the dew-point may be made again and again and under widely differing conditions.

The psychrometer has received even greater condemnation than the instrument just alluded to. The following is quoted from Symons's Magazine for May, 1885. p. 56, and is an extract from a letter by Mr. Dines: "We have for many years past relied almost exclusively on the indications of dry and wet-bulb thermometers to give relative humidity; it is now admitted that 'all deductions from these are open to doubt.' I should go further than this and say that they ought not any longer to be used for that purpose. It is only too true to say that, out of the mass of observations which has been accumulating for the last few years, we have no data upon which we can rely to ascertain to what degree of humidity the air attains in this country, and which is of far more importance, to compare the dryness of the air at one place with that at another; even at times when both thermometers read alike it does not follow that the air is saturated with vapor, or in other words that its relative humidity is 100 per cent. Objections have been made to the form in which the figures composing a table of relative humidity are given, but this is as nothing when compared with the manner in which those figures are obtained. I should be sorry to say one word against the use of the dry and wet thermometers, but instances by the thousand show plainly that they are not fitted to give the dew-point temperature, and it is to the persistent use of them for this purpose, that Mason's Hygrometer, one of the most simple and useful of all meteorological instruments, has been brought into discredit." Here again there is great misunderstanding; it is granted that, as ordinarily observed, the psychrometer, in a very close shelter or on the north side of a building where there is no ventilation, will give exceedingly unsatisfactory results, but that is not the fault of the instrument. It is hoped to show that with proper care the indications of the psychrometer may be made very accurate, and, what is more important, may be made nearly invariable, i. e., so that constant results may be had under the same conditions. Unfavorable criticisms like the above might be quoted sufficiently numerous to fill a small volume.

It would seem that, if a nearly uniform law can be established between the indications of these two instruments, under all conditions of temperature, dryness, motion and other conditions of the air, certainly we can depend upon the indications of each by itself at all times. It is the object of this paper to indicate a practical method of using these instruments, to contribute toward the establishment of a law controlling them, and to show what accuracy may be attained by either of them. The importance of a practical discussion of this question will be readily recognized. In many instances weeks and even months have been spent in an endeavor to obtain satisfactory comparisons. A most significant sign of the present state of the problem is to be found in the fact that the International Meteorological Committee which has in charge the duty of computing meteorological tables have abandoned the project of preparing tables for the psychrometer. When we add to this the fact that large sums have been expended in preparing and printing the existing tables, which are so discordant among themselves, the necessity of a further investigation becomes very apparent.

The Condensing Hygrometer.

One instrument of this class, that of Mr. Dines, is very simple in construction and not very expensive. In this, cold water or melting ice is employed as a cooling agent, the dew being deposited on a plate of black glass, underneath which the cooling liquid passes, the temperature being indicated by a thermometer near the plate. There are serious objections to this instrument for accurate work. The presence of so much water in the neighborhood of the plate vitiates the result; with a properly arranged Alluard apparatus, shortly to be described, the harmful effects from an open bottle of water on the same table may be easily recognized. Moreover, this instrument cannot be used at temperatures of the dew point below 35°. There is little doubt but that a properly ventilated psychrometer will give much more accurate results for moisture contents of the air than can be hoped for with this instrument. Another instrument of this class is the Alluard form of Regnault's hygrometer. In this the usual cylindrical silver cup gives place to a gilded prismatic box. The usual tubes, one running to the bottom and the other from the top, are supplemented by a third running to the top and terminating on the outside in a little funnel by which the box may be conveniently filled with the cooling fluid. Of the latter, both ether and rhigolene were tried aud preference given to the rhigolene. This may be evaporated more readily, the residuum left after evaporating a large quantity is by no means as great or as harmful in stopping farther evaporation as is the case with ether; finally, it has no affinity for moisture. This latter point is important; in one instance, an operator with ether continually complained that his fluid congealed at low temperatures; in this case it was not the ether at all, but the moisture taken up by it, that produced the effect. In using rhigolene it is only necessary to empty the box after evaporating a half pint or so; no objectionable deposit is left in the box. The thermometer is held in position by a cork at the top; the cork should be so bored that the thermometer may be brought very near the plate. This plate on which the dew is deposited is brass, plated with gold, and has many advantages over the earlier cylinder of silver. Dew is much more easily detected, the plate is easier to clean, and fumes of the evaporating liquid do not affect the appearance of dew. The writer found the latter an important matter; it was impossible to keep the silver in a satisfactory condition for a large number of observations. To guard against fumes from the evaporated liquid, if in a room, the outlet pipe should be carried to the outside air, and in the open air, the same tube should be placed to the leeward of the instrument.

Before entering upon the use of this instrument it is essential to carefully investigate the errors which are likely to occur. The effect of accidental errors may be practically eliminated by multiplying the observations, but there is a class of constant errors which cannot be thus treated. These may be divided into two classes: 1st, those that may best be investigated by experiments upon a single instrument; and 2d, those which can be completely determined only by the use of two or more instruments. Under the first class the following questions need an answer.

Question 1. What is the effect of using different depths of liquid?

Answer. It is only necessary that the instrument be completely immersed. Experiments in which the instrument was first pushed to the bottom of the box and then pulled to the top of the liquid showed no appreciable difference.

Q. 2. What should be the position of the inlet tube as regards the thermometer and the plate when dew is observed?

A. The inlet tube should be carefully arranged at the back of the box, leaving as much space as possible between it and the thermometer.

Q. 3. What should be the position of the thermometer?

A. It should be as near the plate as possible.

Q. 4. Does the rapidity of flow of air into the liquid need attention?

To answer this question, which has also been regarded as an important one by others, a test tube was arranged with two thermometers placed on opposite sides and as far apart as possible, the inlet tube being passed down very near one of these.

On evaporating the liquid very slowly it was seen that the bubbles arose very near the thermometer that was close by, and there was at first a uniform indication by this of ·3° *lower* temperature than by the other. As the liquid lost its temperature and became much cooler than the air it was found necessary to force in the air much more rapidly in order to continue the cooling process; after a time the two thermometers indicated the same temperature and then the one nearer the inlet tube began to read higher than the other and so continued throughout the experiment, reading half a degree higher for some time. The explanation seems very simple; at first the slowly flowing air only agitated the liquid near its thermometer and hence it caused a lower reading, afterward the air relatively much warmer, flowing more rapidly through the liquid was not entirely cooled to its temperature and hence caused a too high reading of the near thermometer. We may conclude then, that, under both slow and rapid air motion, the thermometer near the plate will not be affected very differently from the plate itself.

Under the second division we seek for answers to the following questions. 1st. What effect does the thickness and material of the plate have? 2d. Are there any appreciable differences in the hygrometric properties of plates? 3d. Is there any difference in the ease of detection of the dew, etc.? I have not had access to apparatus by which to settle these questions, but they may be partly answered by using a single instrument. As quoted above from Mr. Strachan, the thickness of the plate would seem the most important source of error. The following partial answer to the objection is given. It does not seem probable that the *inside* of the plate would have precisely the temperature of the thermometer, but theory would indicate that there would be a gradual diminution of temperature from the outside of the plate to a point inside the liquid, and this would be greater the lower the temperature of the air, as compared with that of the dew point. For testing this question we have the following methods. 1st. We may compare results obtained with a small difference between the air and the dew-point, at which time there would be slight diminution of the temperature of the liquid below that of the front of the plate, with results obtained under just the opposite conditions and which ought to indicate the greater diminution. Such comparison shows an appreciable difference, but this is in precisely the opposite direction from that indicated by theory, i. e., with a small difference between the dew-point and the air, the former is lower, as compared with the air, than with a large difference.

2d. We may lower the temperature rapidly at one time and slowly at another; if we have the same result in both cases we can assume that the effect of the thickness of the plate is inappreciable. It is easy to see this, suppose the temperature of the liquid be lowered ·1° per second, and there is a large retardation in the diminution of the outside temperature, or, which is the same thing, the outside temperature is higher because of the air being 40° above the liquid ; in such case the thermometer, we will say, will have a temperature ·1° lower than the dew-point, or in other words it takes 1 second for the temperature of the liquid to reach the outside of the plate. Now if we lower the temperature at the rate of ·05° per second then the inside temperature will be ·05° lower than the outside approximately. If now we consider the dew-point constant during the experiment, we shall have with the rapid fall an indicated dew-point ·05° *lower* than with the other. A long series of trials have shown the difference in the two methods inappreciable. It should be borne in mind that whatever be the amount of the effect due to the thickness of the plate, the tendency will always be toward a too low dew-point. A method of testing this question which would seem to be free from many of the objections that may be raised against those already proposed is the following, lack of time and funds have prevented carrying out the plan. We may arrange a plate with two or even three different thicknesses of metal. Suppose such a plate have thicknesses for each third of its width of ·01″, ·02″, ·04″ and it be cooled down gradually ; we would expect dew first on the thinnest and last on the thickest, and the difference in temperature as the dew appeared on each could be readily noted.

A second source of error may be due to a difference in the hygrometric properties of plates. It is readily seen that if there be such a difference it would be rather a difficult matter to settle which should be taken as the standard, though it would seem that, so far as this one thing is considered, that plate which gave the lowest dew-point would be the best. No experiments have yet been made to settle this point. It would require observations, from two trained men, at the same time. It may be said, however, that this consideration is far outweighed by others relating to the cleaning of the plate, detection of dew, etc. It is a very difficult matter to clean the plate properly ; the best method would seem to be by using a mixture of dilute ammonia and rouge as a wash, applying with silk, thoroughly drying and wiping it off by rubbing ; this process gives a most beautiful surface for the dew. Quickness of detection of dew can only be obtained by practice. No fixed rules can be laid down for arranging the light, the position of the eye, etc. It is necessary to cut off all reflections from light-colored objects ; this may best be done by placing a black cloth in front of the apparatus. A light breeze is an advantage in the detection of dew, and in still air this should be raised by a fan. If there is a high wind it will be necessary to put the apparatus in a box, having one of its vertical sides removed, this open side should be placed to the leeward so that the wind will be cut off from the plate.

When in good condition this apparatus works very nicely and seems to follow fluctuations in the dew-point very closely. The following are taken from a number of records illustrating this point.

Fluctuations of the dew-point.

Deerfield, Mass.,				Oakland, Md.,	
Nov. 14, 1884.		Mar. 25, 1885.			Mar. 26.
21·0°	12·0°	11·1°	12·5°	22·6°	19·0°
20·0	11·6	11·6	12·2	22·7	19·4
19·4	11·4	12·3	10·4	23·0	20·0
19·6	10·0	11·4	10·4	23·6	20·7
19·2	10·2	11·2	10·5	23·3	21·1
18·3	10·4	10·9	11·0	23·3	21·6
19·0	9·4	9·0	11·4	24·7	22·2
19·0	10·0	8·7	11·5	25·1	21·2
19·6	9·8	9·7	11·6	23·6	20·2
19·7	9·1	9·7	11·8	23·6	
20·4	9·6	11·0		22·6	
20·7	10·3	10·9			
20·8	11·1				
	11·0				

These dew-points were determined one after the other as rapidly as possible, the interval being not more than 15 seconds between each reading. There seems to be an uniform change in these readings, sometimes the dew-point increases and then diminishes; as all the conditions were constant during each set, it would seem that we have represented an actual fluctuation in the moisture contents of the air.

Still another form of this apparatus is that devised by Crova. The essential characteristic of this is the deposition of dew on the inside of a hollow, horizontal cylinder, the outside of which is cooled in the usual way. The dew is detected by looking through an eye-piece at one end of the cylinder, and the appearance is said to be highly satisfactory. A most serious objection to this form is the extreme difficulty of properly cleaning and polishing the inside of the cylinder; another is the impossibility of attaining very accurately the temperature of the surface on which the deposition takes place. Some remarkable results have been obtained with this apparatus and have been published in the Zeitschrift für Meteorologie, 1884, p. 46. In the following table there are given: 1st, the result out of each set of three which showed the greatest difference between Regnault and Crova; 2d, the mean of each set of three, and the mean of all three sets. The value of A, in the psychrometric formula (soon to be described) $x=f-A(t-t')\frac{5}{9}h$, has been assumed as ·00080 for the observations with the Regnault apparatus and the corresponding value with the Crova instrument has been computed.

Comparison of Regnault and Crova.

No. of Obs.	Dry observed	Wet computed	Dew-point observed. Regn.	Dew-point observed. Crova.	Value of A. Regn.	Value of A. Crova.
	°	°	°	°		
1	72·5	58·0	44·1	46·9	·00080	·00067
1	67·5	56·8	46·9	49·6	·00080	·00067
1	77·9	65·4	56·7	59·2	·00080	·00059
6	70·7	56·7	42·7	44·2	·00080	·00073
4	67·0	57·0	48·0	49·2	·00080	·00073
6	78·2	65·5	57·0	59·1	·00080	·00063
16	72·7	60·0	49·4	51·2	·00080	·00069

These results are extraordinary, and are probably due in large measure to an improper condition of instruments rather than to an actual difference in their normal indications. Certainly differences greater than ·5° F. ought not to occur in a properly observed Regnault apparatus, while these comparisons show an extreme range in the differences between the two instruments of 2°·9. I have dwelt at length on these results as they show clearly some of the difficulties to be met with in comparisons of this kind. I add a remarkable comment by a prominent writer, on the original observations. "In the observations given by Crova we find, as was to be expected, that Regnault's apparatus gives dew-points too low by as much as 1°·6 C."

The Psychrometer.

As is well known, this consists of two thermometers; one of them, the wet, has a muslin over the bulb, which is kept continually moistened by a wick attached above the bulb and dipping in a cup of water, and measures the temperature of evaporation; the other is dry and indicates the air temperature. The amount of moisture makes but little difference; Regnault found that if water dripped slowly from the bulb, he still obtained accordant results; this, however, was in still air. Since the wet bulb may at times stand 20° below the dry, it might be supposed that the stem of the thermometer immediately above the bulb would tend to raise the temperature. This effect is very slight, amounting in the case of a spherical bulb to ·3° when the air is perfectly still. The muslin should be very thin and may be made to fit a cylindrical bulb very tightly, if a piece the right size is first immersed in water and then put on the bulb, a string being tied upon it just above and below the bulb. The water used should be rainwater, if possible. In the winter season, or when the wet bulb temperature is below freezing, great care must be taken in coating the bulb with a uniform layer of ice. To do this properly, it is necessary, whenever it is found that the air temperature is going below freezing, to take the wick out of the cup and hang the free end over the support behind; it should not be taken off the thermometer. If this wick becomes stiffened with ice before it has been taken out of the cup and cannot be bent, the whole, including bulb, cup and wick, must be immersed in luke-warm water, lifted up from beneath, till the wick is sufficiently pliable to permit properly adjusting as above. To wet the bulb, keep the cup of water as near 32° as possible and 10 to 25 minutes before the observation, if there is no artificial ventilation, lift the cup up from beneath and wholly immerse the bulb; in withdrawing the cup, wait a moment after the bulb is exposed and touch the drop that will be hanging from it, with the edge of the cup. If there is a high wind, two or three coats at once may be needed to obtain a good result. A mass of ice should not be allowed to accumulate on the stem or about the bulb, but it should be melted off and a uniform coat applied. No brass scale or protector should ever be allowed within 1·5 inches of the wet bulb. This scale has the temperature of the air, and if the muslin or wick touch the scale its temperature will be conducted to the bulb, thus giving too high a reading. If the muslin becomes brown or covered with dust it should be replaced by clean; this may need to be done once a month in some places. Frequently the muslin will be found rather firmly fastened to the bulb by the formation of a lime-like substance; in such case, the bulb may be cleaned by using a bath of dilute muriatic acid.

When the temperature reaches about 20° there seems to be a peculiar effect produced which causes the indicated temperature to be too high and which increases as the temperature falls. This may be due to a contraction of the ice-film or coating; at 0° the effect amounts to nearly 5°, but after that point is reached the ice seems to partly give way, as the contraction seldom goes beyond 5°. Experiments have been made by immersing dry and ice-covered bulb thermometers in a bath of mercury and also in disulphide of carbon with identical results. As long as the temperature was lowered or remained stationary, even down to −20°, there was still a marked effect observed, but the moment the temperature began rising, possibly because the ice expanded so much more rapidly than the glass, the two thermometers began to approach each other and quickly agreed. The above explanation seems extraordinary and it may be necessary to seek another. It may be that at low temperatures the ice refuses to conduct the very last part of the heat from the bulb; however, this explanation does not answer when the temperature begins rising, as in that case the ice-covered bulb ought to again lag and be slightly cooler than the liquid.

The writer's attention was first called to this matter by the astonishingly high relative humidities reported from the northwestern stations, whenever a high-pressure area with very low temperatures appeared. At such times the relative humidity is almost invariably reported at 100 per cent, and yet ordinarily these same high areas, with N.W. winds, frequently reduce the humidity 50 and even 60 per cent when they reach places toward the S.E. where the temperature is above 20°. Again, many observers have reported the ice-covered bulb reading higher than the dry on cold days, even when they had corrected thermometers and very high winds: it was thought that many times the trouble arose from a lack of ventilation and an improper coating of ice. Later experiments, however, with perfectly correct thermometers, most careful ice-coats, good ventilation and a nearly saturated air at temperatures down to 0° have repeatedly confirmed these abnormal results, showing in one instance the iced bulb ·5° higher than the dry. When we consider that at −20° a difference of ·5° between dry and wet amounts to 36 per cent in the deduced relative humidity, the great importance of a thorough investigation of this point becomes very apparent. We must know the amount of this contraction with different coatings of ice and at different temperatures; knowing these facts, we may apply a correction to the wet bulb-reading before using it for computing the moisture content of the air. The best method of testing the question would seem to be by a rigid series of comparisons at very low temperatures between the ice-covered bulb and a condensing hygrometer.

In the use of the psychrometer for making the best observations the most important consideration is that of a thorough ventilation, and this is especially to be attended to if readings are to be made in a room or very still air. Out of doors, if there is a good air circulation about the wet bulb, artificial ventilation is not needed in the summer, but with an ice-covered bulb the action is so sluggish that it is very convenient to have a movable fan for increasing the air circulation and expediting the observation. The most convenient method of ventilating the wet bulb is by swinging in a circle of some two feet radius. This method seems to have been first adopted by Saussure at the end of the last century; he employed a swung wet bulb for obtaining the temperature of evaporation but did not join with it a thermometer for obtaining the air temperature at the same time, the latter being an important consideration. Most careful experiments have been made looking to the determination of all the errors to which this sling psychrometer may be supposed to be subject. At the greatest velocities attainable by hand, no harmful effects have ever been noted from friction with the air or from expansion of the bulb through the centrifugal action of the mercury column. Any convenient velocity may be used; about four meters per second, ten miles per hour, was found satisfactory. The results were precisely similar at all velocities from two to ten meters per second, great care being taken in wetting the bulb uniformly during the entire time of swinging, and also continually changing the plane in which the bulb was swung. Repeated trials in a room have shown that an accuracy of a tenth of a degree may readily be attained with this device in one minute. In the open air it is necessary to swing out of sunshine and in a moderate shade only, if the air temperature is desired. It has been thought by some that at night with a clear sky, radiation from the bulbs into space is harmful, but experiment shows that the amount of this radiation from bulbs suspended in a still air is very slight, and the rapid motion entirely annuls any evil effect. For the best results it is essential that the sling wet bulb should be continuously moistened. To do this properly requires some skill and constant watchfulness. The muslin should be carried up the thermometer stem about an inch, and to the latter there should be fastened a tube of glass drawn out to a point. The point should be placed just at the top of the muslin, or at least one inch from the bulb, otherwise its heat imparted to the water will tend to raise the temperature. A string inserted in this tube may be pressed down or pulled out according as the air is moist or dry, the intention being to keep the muslin constantly and uniformly moistened.

Comparisons.

The investigations of others have established a formula for comparing these instruments, which may be taken as a working hypothesis, as follows, for temperatures above freezing:

$x=f-\frac{5}{9}(t-t')\,Ah$, in which
$x=$ the tension of vapor of saturated air at the dew-point temp.
$f=$ the tension of vapor of saturated air at the wet-bulb temp.
$t=$ air temperature.
$h=$ air pressure in inches of mercury.
$A=$ a constant to be determined.

For temperatures below 32°, it has been thought by some even to the present time, from theoretical considerations, that the evaporation from ice is different from that from water, and hence the value of A must change suddenly at 32°. If there be such a change allowed for, the distinction should be between water and ice and not above or below 32°, for frequently the wet bulb may be lowered to below 26° without the formation of ice. This is an important problem, and has been practically solved in various ways. In March, 1884, the writer, with the assistance of Mr. Fassig, performed some experiments in a room with the temperature just below freezing. It was found possible to have two wet bulbs in operation at the same time as sling thermometers, the one being covered with water and the other with ice. The difference between the two was practically inappreciable The results in detail may be found in the American Meteorological Journal for June, 1884. Later comparisons between a condensing hygrometer, water- and ice-covered bulbs confirmed the above conclusion. The results of these may be found in the journal just quoted, for February, 1885. Even before that time somewhat the same conclusion had been arrived at in Europe, though the experiments here were entirely independent of those.

Value of A.

Having given the dew-point, t, t' and h, we can obtain the constant of the formula. As t' enters the formula twice, it has by far the greatest influence and it is partly because it has been found impossible to obtain an invariable reading of t' that so many differing values of A, have been obtained; a few examples will suffice.

In still air Regnault found A= ·00128; in the open air ·00074: Doyere with the sling psychrometer in 1855 from a very few observations in hot weather found ·000687; Sworykin, with fair ventilation ·000725; Blanford ·000827; Angot from over 3,000 observations ·000851; Chistoni ·000851. The latter three results are probably without any uniform ventilation. The first comparisons by the writer were made in Feb. 1883, with a bellows for a ventilator of the dry and wet bulbs; these results were published in Science, June 8, 1883. A longer series of experiments were instituted in October, 1884. From 700 sets of observations, both indoors and out, with high and low temperatures, and with large and small differences between t and t', a value of A was obtained at ·00068 and upon this value was computed the table for relative humidity to be found at the end of this paper. The value of A that has been adopted may seem rather small, nevertheless, it agrees exactly with that adopted by Regnault for temperatures below freezing, so that no change is made in the table below that point. This value depends upon the separate determination of three quantities, each of which has usually had great uncertainties entering into it. It is believed that the errors entering into t and t' have been reduced for each individual observation to less than ·1°. It is surprising to see how swing after swing may be made with the sling psychrometer resulting in absolutely no change in the readings. The same cannot be said, however, regarding the determination of the dew-point; in this errors will creep in on different days of ·3°, and in consequence the value of A may be changed slightly. When there is a small difference between t and t' there is great difficulty in getting uniform results, but when $(t-t')$ is small a larger difference from the mean in A produces a much smaller effect upon the deduced moisture content than when $(t-t')$ is large. With $(t-t')$ equal to or greater than 15° there is no difficulty in getting A within two of the fifth place again and again and on different days. The vapor tensions employed in computing the table have been taken as recently computed by Broche from Regnault's observations.

One of the more interesting questions which arises in connection with the formula is the effect of hight above sea upon the relation between the $dp:t$, and t'. Glaisher, after bringing together many thousands of comparisons between a Daniel's hygrometer and the psychrometer, at various hights in India and elsewhere, decided that hight had no appreciable effect upon the deductions to be made with the psychrometer. Although these observations were made without any uniform ventilation for the psychrometer and cannot be regarded as giving absolute results at either sea level or on mountain tops, yet it would seem as though they were of sufficient accuracy to enable relative comparisons so far as the question of hight comes in, i. e., if the same instruments observed in precisely the same way at two hights give the same results, we may infer that there is no difference in the relations of the quantities measured, although large errors may occur at both hights. The question of a possibly better ventilation at the higher station need not be considered, as this would have tended to lower the wet-bulb reading and would have been in exactly the direction of theory at high stations. The exposures and amount of moisture at the two hights would have a slight influence, but in a long series of observations there would be an elimination of these effects. Dr. Pernter has published a series of observations with a ventilated psychrometer,* and Regnault's condensing hygrometer at a barometric pressure of about 600^{mm}. These observations gave a mean value of A as ·001042; while the value of A at sea level adopted by Dr. Pernter is ·000843. If we multiply

* This psychrometer was in a ventilated screen of Prof. Wild's form, but Prof. Wild thinks that the screen used was his earlier form with a clock-work ventilator, which did not give a very perfect ventilation.

the first of these by 600 and the second by 750, the approximate air pressure at the two hights, we obtain ·625 and ·632 respectively, showing that Ah was nearly constant and that the relations to each other of the other three quantities are practically the same at all altitudes. These observations, however, were not sufficiently numerous and were not made with a psychrometer well enough ventilated to give satisfactory results.

In order to test the question a week was spent, in March, 1885, at Oakland, Md., at a hight of 2,800 feet above sea level and where the pressure was about 27″. A series of comparisons at this point made with the identical instruments and under precisely the same conditions as those previously made at sea level gave a value for Ah of ·0197, while the sea level value was ·0204. If we assume h as 30″ in both these cases, we shall have A ·000657 and ·000680. The temperatures employed at Oakland ranged from 102° to 14°, and with the former the dewpoint was 48° lower than the air. These observations were not sufficiently extended to give an absolute value for Ah, but the relative comparisons may be regarded as approximately accurate. Any one acquainted with the use of these instruments will recognize the very small margin there is in the two values above. The difference in the computed humidity from either of the values will be nearly inappreciable. Moreover, whatever may be the absolute relation between the $dp : t$ and t' under different pressures, none but the most refined observations can by any possibility detect it, and so far as ordinary observations are concerned no account need be taken of this difference up to 3,000 feet. This is especially apparent when we consider the extreme difficulty under the most satisfactory conditions of obtaining even an approximate value of the wet and dry temperatures, and it should be borne in mind that these difficulties are not due, for the most part, to accidental errors which may be averaged out by multiplying the observations, but are caused by constant errors, such as imperfect ventilation, too high temperature through the day, and too low just after sunrise for an hour or two, etc. The question of applying a correction for elevation to the indications of a psychrometer depends largely upon the practical benefit to be derived. No such correction is needed in order to make intercomparisons at high stations, but it may be argued that it is needed in order to obtain, first, a comparison between a high station and a neighboring lower one, and second, to obtain the diminution in moisture contents at any time or during any season between sea level and the upper atmosphere that is passing a high station. To these arguments it may be answered, first, we do not know the variations of humidity in different strata so as to obtain a law of reduction to sea level and make a comparison at all possible, and second, the conditions of moisture are so variable on mountain tops, so influenced by local surroundings and by currents of air laden with moisture condensed by the mountain side, so utterly different from the conditions in the free air strata at the same hight, that no comparisons of any practical value can be made. These considerations tend to show that the use of a special table for stations up to 3,000 feet altitude is very questionable, and in the present stage of meteorology a refinement entirely unnecessary, especially for individual observations. The additional labor and watchfulness needed in examining all records to see whether the table has been properly applied, and above all, the extreme annoyance resulting from an attempt at interpolating for hight in a table, and the great danger of making errors in such interpolation, far outweighs any theoretical advantage to be gained, which, as has already been shown, is in itself very questionable. While the table accompanying this paper is applicable to the sling psychrometer, yet most careful comparisons between the sling and a psychrometer in a fairly open shelter having a good exposure, have shown little or no difference; this may easily be understood, since velocities of wind as low as two meters per second have given as good results as those five times as great. The table is applicable to all open shelters except those from windows and on walls; in such cases there should be used, if possible, some means for artificial ventilation. For all traveling parties and at all stations where the temperature is often below freezing, the sling psychrometer is on all accounts the best instrument for obtaining air humidity.

The experiments for this paper were completed last spring, but it has been found impossible to study the great mass of data and properly prepare it for publication before this time.

October 12, 1885.

H. A. Hazen—Table of Relative Humidity.

$x = f - \cdot 00068\,(t-t') \times \frac{5}{9} \times 30!\quad \text{R. H.} = \frac{x}{F}.$

x = vapor tension at dew-point temperature.
f = " " wet bulb " or t'.
F = " " air " or t.

t	0·0	1·0	2·0	3·0	4·0	5·0	6·0	7·0	8·0	9·0	10·0	11·0	12·0	13·0	14·0	15·0	16·0	17·0	18·0	19·0
										t−t′										
0	100	70	41	11																
1	100	71	43	15																
2	100	72	45	18																
3	100	73	47	21																
4	100	74	49	24																
5	100	75	51	27	3															
6	100	76	53	30	7															
7	100	77	55	33	10															
8	100	78	56	35	13															
9	100	79	58	37	17															
10	100	80	59	39	20															
11	100	80	61	41	22	3														
12	100	81	62	43	25	7														
13	100	82	63	45	28	10														
14	100	82	65	47	30	13														
15	100	83	66	49	33	16														
16	100	84	67	51	35	19	3													
17	100	84	68	53	37	22	6													
18	100	85	69	54	39	24	10													
19	100	85	70	56	41	27	13													
20	100	86	71	57	43	29	15	1												
21	100	86	72	58	45	31	18	4												
22	100	86	73	59	46	33	20	7												
23	100	87	74	61	48	35	23	10												
24	100	87	74	62	49	37	25	13	1											
25	100	88	75	63	51	39	27	16	4											
26	100	88	76	64	52	41	29	18	7											
27	100	88	77	65	54	43	31	20	10											
28	100	89	77	66	55	44	33	23	12	1										
29	100	89	78	67	56	46	35	25	15	4										
30	100	89	78	68	58	47	37	27	17	7										
31	100	89	79	69	59	49	39	29	19	10										
32	100	90	80	70	60	50	40	31	22	12	3									
33	100	90	80	70	61	51	42	33	24	15	6									
34	100	90	81	71	62	53	43	34	26	17	8									
35	100	90	81	72	63	54	45	36	28	19	11	2								
36	100	91	82	73	64	55	46	38	30	21	13	5								
37	100	91	82	73	65	56	48	39	31	23	15	7								
38	100	91	83	74	66	57	49	41	33	25	17	10	1							
39	100	91	83	75	66	58	50	42	35	27	19	12	4							
40	100	92	83	75	67	59	51	44	36	29	21	14	7							
41	100	92	84	76	68	60	52	45	38	30	23	16	9							
42	100	92	84	76	69	61	54	46	39	32	25	18	11	2						
43	100	92	85	77	69	62	55	47	40	34	27	20	13	5						
44	100	92	85	77	70	63	56	49	42	35	28	22	15	8						
45	100	93	85	78	71	64	57	50	43	36	30	24	17	11	5					
46	100	93	86	78	71	64	58	51	44	38	31	25	19	13	7	1				
47	100	93	86	79	72	65	59	52	45	39	33	27	21	15	9	3				
48	100	93	86	79	72	66	59	53	47	40	34	28	22	16	11	5				
49	100	93	86	79	73	66	60	54	48	42	36	30	24	18	12	6	1			
50	100	93	86	80	73	67	61	55	49	43	37	31	26	20	14	8	3			

H. A. Hazen—Table of Relative Humidity—continued.

t	*t—t′* 0·0	1·0	2·0	3·0	4·0	5·0	6·0	7·0	8·0	9·0	10·0	11·0	12·0	13·0	14·0	15·0	16·0	17·0	18·0	19·0
50	100	93	86	80	73	67	61	55	49	43	37	31	26	20	14	8	3			
51	100	93	87	80	74	68	62	56	50	44	38	32	27	22	16	10	5			
52	100	93	87	81	74	68	62	56	51	45	39	34	28	23	18	12	6	1		
53	100	94	87	81	75	69	63	57	52	46	41	35	30	25	19	13	8	3		
54	100	94	87	81	75	69	64	58	53	47	42	36	31	26	21	15	10	5		
55	100	94	88	82	76	70	64	59	53	48	43	38	32	27	22	17	11	7	2	
56	100	94	88	82	76	70	65	60	54	49	44	39	34	29	24	18	13	9	4	
57	100	94	88	82	77	71	66	61	55	50	45	40	35	30	25	20	15	10	6	1
58	100	94	88	83	77	71	66	61	56	51	46	41	36	31	27	22	17	12	8	3
59	100	94	88	83	78	72	67	62	57	52	47	42	37	33	28	23	18	14	10	6.
60	100	94	89	83	78	72	67	62	57	52	48	43	38	34	29	25	20	16	12	8
61	100	94	89	83	78	73	68	63	58	53	48	44	39	35	30	26	22	17	13	9
62	100	94	89	84	79	73	68	64	59	54	49	45	40	36	31	27	23	19	15	11
63	100	95	89	84	79	74	69	64	59	55	50	46	41	37	33	28	24	20	16	12
64	100	95	89	84	79	74	69	65	60	55	51	47	42	38	34	30	26	22	18	14
65	100	95	90	84	80	75	70	65	61	56	52	47	43	39	35	31	27	23	19	15
66	100	95	90	85	80	75	70	66	61	57	52	48	44	40	36	32	28	24	20	17
67	100	95	90	85	80	75	71	66	62	57	53	49	45	41	37	33	29	25	21	18
68	100	95	90	85	80	76	71	67	62	58	54	49	46	42	38	34	30	26	23	19
69	100	95	90	85	81	76	72	67	63	59	54	50	46	42	39	35	31	27	24	20
70	100	95	90	86	81	76	72	68	63	59	55	51	47	43	39	36	32	28	25	22
71	100	95	90	86	81	77	72	68	64	60	56	52	48	44	40	37	33	29	26	23
72	100	95	91	86	81	77	73	68	64	60	56	52	49	45	41	38	34	30	27	24
73	100	95	91	86	82	77	73	69	65	61	57	53	49	46	42	39	35	31	28	25
74	100	95	91	86	82	78	73	69	65	61	57	54	50	46	43	39	36	32	29	26
75	100	95	91	86	82	78	74	70	66	62	58	54	51	47	43	40	37	33	30	27
76	100	95	91	87	82	78	74	70	66	62	58	55	51	48	44	41	37	34	31	28
77	100	96	91	87	83	79	74	70	67	63	59	55	52	48	45	42	38	35	32	29
78	100	96	91	87	83	79	75	71	67	63	59	56	52	49	46	42	39	36	33	30
79	100	96	91	87	83	79	75	71	67	64	60	56	53	50	46	43	40	37	34	30
80	100	96	91	87	83	79	75	71	68	64	60	57	54	50	47	44	40	37	34	31
81	100	96	91	87	83	79	76	72	68	64	61	57	54	51	47	44	41	38	35	32
82	100	96	92	87	84	80	76	72	68	65	61	58	55	51	48	45	42	39	36	33
83	100	96	92	88	84	80	76	72	69	65	62	58	55	52	49	46	43	39	37	34
84	100	96	92	88	84	80	76	73	69	66	62	59	56	52	49	46	43	40	37	35
85	100	96	92	88	84	80	77	73	69	66	63	59	56	53	50	47	44	41	38	35
86	100	96	92	88	84	80	77	73	70	66	63	60	57	53	50	47	44	41	39	36
87	100	96	92	88	84	81	77	74	70	67	63	60	57	54	51	48	45	42	39	36
88	100	96	92	88	85	81	77	74	70	67	64	61	57	54	51	48	46	43	40	37
89	100	96	92	89	85	81	78	74	71	67	64	61	58	55	52	49	46	43	40	38
90	100	96	92	89	85	81	78	74	71	68	64	61	58	55	52	49	47	44	41	38
91	100	96	92	89	85	81	78	75	71	68	65	62	59	56	53	50	47	44	42	39
92	100	96	92	89	85	82	78	75	72	68	65	62	59	56	53	50	48	45	42	40
93	100	96	92	89	85	82	78	75	72	69	65	62	59	56	54	51	48	45	43	40
94	100	96	93	89	85	82	79	75	72	69	66	63	60	57	54	51	49	46	43	41
95	100	96	93	89	85	82	79	76	72	69	66	63	60	57	55	52	49	46	44	41
96	100	96	93	89	86	82	79	76	73	69	66	63	61	58	55	52	50	47	44	42
97	100	96	93	89	86	82	79	76	73	70	67	64	61	58	55	53	50	47	45	42
88	100	96	93	89	86	83	79	76	73	70	67	64	61	58	56	53	50	48	45	43
99	100	96	93	90	86	83	80	76	73	70	67	64	62	59	56	53	51	48	46	43
100	100	96	93	90	86	83	80	76	74	71	68	65	62	59	56	54	51	49	46	44

ART. LV.—*A new form of Absorption Cell*; by ARTHUR E. BOSTWICK.

THE writer has devised and used the cell described below for the purpose of obtaining the absorption spectra of liquids which have but little selective absorption, and which would therefore have to be used ordinarily in large quantities.

The cell is a rectangular box about six inches long by three broad and three in height. The bottom and the two ends are of pine wood, covered with shellac, and the two sides are of ordinary looking-glass, cemented to the wood, so that the box is water-tight. The reflecting surface of the looking-glass is turned inward and at each of two diagonally opposite corners the amalgam is scraped away so as to make a vertical slit about two millimeters in width. One of these is placed close to the spectroscope slit, and through the other a parallel beam of light is admitted. It is evident that the box may be so placed that the beam will be internally reflected in it a number of times, depending upon the angle between the two, and will finally pass through the second slit into the spectroscope. The length of its path through the cell may therefore be varied indefinitely by turning the latter, and is limited only by the decrease in intensity caused by general absorption—not only in the liquid, but also at each reflection.

A solution of bichromate of potash, so weak that a test-tube full of it was of a barely perceptible yellow color and showed no absorption at all when held before the spectroscope slit, when placed in this cell, absorbed the whole upper end of the spectrum, the F line being scarcely visible. In this case sunlight was used, the beam being reflected six times, and having a path whose length inside the cell was about two feet. With mirrors of polished metal the result might be even better, since the absorption in the glass would be eliminated. In this case however the number of liquids which could be used in the cell would be somewhat limited.

ART. LVI.—*Preliminary notice of Fossils in the Hudson River Slates of the Southern part of Orange Co., N. Y., and elsewhere*; by NELSON H. DARTON.

IN a very detailed study, now nearly completed, of the formations other than Archæan in central and eastern Orange Co., N. Y., fossils have been discovered in many new localities

* This Journal, Sept., 1880, pp. 197, 198.

in several formations which have thrown much light upon the complicated stratigraphical structure of the district. An account of the results of the investigation will soon be ready for publication.

The principal paleontological discoveries are in continuation of those of Professor Dwight and Mr. Dale nearer Newburg, and the one to which attention is now called is the finding of Trenton and Hudson River fossils in the Hudson River (?) slates at several widely distant points.

The first and most important is near Sugar Loaf village, twenty-one miles S.W. of Newburg, also at Walden on the banks of the Wallkill, eleven miles N.W. of Newburg, and at an intermediate point, Rock Tavern, ten miles west of Newburg. Two other probable localities are known, but have not as yet been sufficiently explored for description.

Mather, in his final Report on the First District, 1843, p. 369, states, in referring to the Testacea of the Hudson River Group, "a few were observed . . . . . near the villages of Walden and Sugar Loaf," but so far as I can determine this statement stands alone until now, when I can confirm and augment it.

Near Sugar Loaf village most of the fossiliferous beds lie to the west of the station, and at Bulmer's Quarry a bed was discovered that yielded all the species found. Its thickness was about 40 mm.; it was composed almost entirely of fossils and fragments mixed with a soft red earthy matter and not crossed by the very prominent cleavage planes of the enclosing slates; other thinner beds of this material were found and a few indeterminable Crinoidal stems were observed in the rock on the more or less obscure bedding planes; no fucoid remains were recognized. The fossils were more or less decomposed and often contorted, but many specimens were beautifully distinct. The following remains were identified and are given in the order of their abundance.

Orthis pectinella H.; *O. testudinaria; O. plicatella* H.; *Leptæna sericea* Sow.; *Camerella hemiplicata* H.; *Strophomena alternata* Cv.; *Streptorhynchus planimbona* or *S. felifexta?* H.; and a *Trinucleus concentrica* Eaton, was recognized by a portion of the border of a cephalic shield. Several fragments were found which showed no specific character, especially a *Chætetes*, a *Favosite*, and crinoidal columns.

Occasional beds bearing more or less distinct fossils were traced eastward from the above locality for several thousand feet, nearly to the Archæan rocks of Sugar Loaf hill and dipping under them with a steep S.E. dip.

At Rock Tavern, fossils were discovered in the shales exposed in the new railroad cut at points where alteration was less, and the bedding planes more distinct. The remains found were of *Leptæna sericea* and *Orthis testudinaria* in about equal proportions, generally greatly contorted and in various stages of distinctness. The slates dip N. 80° W.>40°, and grade insensibly above and below into highly altered strata contorted and seamed in every direction, especially the underlying beds. This disturbed area is a local one, however, and much progress has been made in this district in tracing out the foldings of the slates. This fossiliferous bed is not far from the base of the formation, and is not intercalated in the other members by folding in an overturned synclinal.

The fossils found near Walden were at the junction of beds of grauwacke and thin layers of fossil slates, exposed in the quarries below the bridge and elsewhere. *Leptæna sericea* was quite abundant at one point, and *Orthis testudinaria* was associated but in less numbers, also *O. pectinella* and a fragment of *Conularia* probably *Trentonensis*. The fossils were finely preserved and the subjacent rock contained numerous small nodules of soft, bituminous matter as noted by Horton in his Report to Professor Mather in 1839.* They are often 10 mm. in diameter. Several indistinct fucoid impressions appear on some of the layers but none were recognizable.

The rocks dip southwestward at a moderate angle, and stratigraphic studies in this district make it appear probable that the fossiliferous beds are at a low horizon in the formation. Further details of the stratigraphic structure will be given in the paper before noted.

In conclusion, I wish to acknowledge my indebtedness to Professor R. P. Whitfield for his kindness in determining the greater number of the species for me, and for his aid and advice at many times in the investigation.

ART. LVII.—*Report of the American Committee-delegates to the Berlin International Geological Congress, held Sept. 28th to Oct. 3d*, 1885; by PERSIFOR FRAZER, D.Sc., Secretary.

THE following report of the third session of the International Geological Congress in Berlin, was made from notes taken by the Secretary of the American committee for this session. These notes were afterwards written out in full, with the kind assistance of Professor Williams to whom the writer hereby expresses his sincere obligations. It will be remembered that the inception of this most important gathering was the action of a com-

* 3d Annual Report of Natural History Survey, p. 144.

mittee at the Buffalo meeting of the American Association for the Advancement of Science, in 1876, the year of the Centennial Exposition. This committee left the work of organizing the first Congress in the hands of a committee of geologists, who thereupon selected as the date of the first Congress the year 1878, or that of the French "Exposition Universelle." The first Congress was duly held, and was remarkable for nothing so much as the absence of any representatives of Germany. After settling some preliminary matters it was decided that the next sitting of the Congress should be held at Bologna, in 1881.

This second session of the Congress was also held; and by this time, the methods of accomplishing the ends of unification in nomenclature and coloring having become better understood, it was determined to undertake to make a map of Europe on a scale of $\frac{1}{1,500,000}$. A committee was appointed to take this in hand, and another to devise ways and means of making a consistent nomenclature for the science.

The two committees met at Foix and Zurich during the four years that intervened between the Congress of Bologna and that of Berlin, and the work of the present Congress has been mainly the adoption of the propositions made at these meetings. At the meeting of the American Association for the Advancement of Science at Ann Arbor last summer, Professor H. S. Williams and Professor Persifor Frazer were added to the original committee, constituting the American delegation and actually represented at Berlin by Professors James Hall and J. S. Newberry. Professor Brush, who was in Berlin at the time, was elected by the committee one of the members. The members of the American committee present at the time of the opening session of the Berlin Congress were, Prof. James Hall (President), Prof. J. S. Newberry, Prof. Brush, Prof. H. S. Williams and Prof. Persifor Frazer (Secretary).

The International Geological Congress met at its third session in Berlin, Monday, September 28. The members and delegates arriving before this date registered at the office of the Bureau in the Bergakademie. The council met at 11 o'clock Monday morning to determine upon the programme of the first session and nominate officers of the present meeting, and at 5 o'clock the members of the Congress assembled at the house of the Reichstag for mutual greetings. Only members of the congress were admitted, and those having registered and received the card of membership were presented with the badge of the Congress, which was in the form of a medal, with the well-known geological and mining symbol of crossed hammers in the center under which are the words "Geologorum conventus —mente et malleo;" and on the reverse—"Berlin, 1885."

The formal opening of the Congress took place Tuesday morning, September 20th, at 11 o'clock, at the House of Deputies.

At this meeting, Professor Capellini, of Italy, occupied the chair as President of the Congress at Bologna.

On his right was Dr. von Dechen and M. Hauchecorne, on the left Professor Beyrich and Professor Hall.

On the ministerial benches on the right were the diplomatic and government officers, and on the left the vice-presidents, representing various countries.

Professor Capellini introduced the "Cultus-Minister," Herr von Gossler, who welcomed the Congress in German.*

Herr von Gossler dwelt upon the fact that no science could procced in any direction without calling to its aid the assistance of the other sciences. He noted the advantage which had accrued to astromomy by this course. He reminded his hearers that Prussia had been the home of von Buch and von Humboldt, and in the name of the Prussian government he warmly appreciated the honor conferred upon Berlin by its choice as their place of meeting, and bade them welcome with the miner's greeting "Glück auf." He added humorously, that as the facts of geology rest upon the results of the action of water, he knew the present weather would not deter the true geologist from his work.

Dr. von Dechen then read his address in French, beginning with thanks to the members for having elected him honorary President. He called to mind the names of many men of European science of a past generation, specifying among them some of the greatest with whom he had been intimate in Paris, in London and in Germany. He stated that much had been done in Geology since the last Congress at Bologna, and much still remained to be done. After thanking the government for its kind reception of the guests, he concluded by expressing the high appreciation of the people of Berlin of the honor done them by the Congress in meeting in their midst.

Professor Capellini then addressed the Congress. His first words were that he owed the honor of occupying the chair to the fact of his having been chosen to preside over the Congress at Bologna. He sketched the origin and history of the Congress from the time of its inception by the committee of the American Association for the Advancement of Science in 1876, through the Paris sessions in 1878 and the Bologna Congress of 1881, and mentioned particularly the very friendly attitude which his Majesty, the king of Italy, had assumed toward its

* By the action of the Congress at Bologna the language of its debates is French.

work and deliberations. He continued: "I had the honor in the month of August last, to communicate to his Majesty, King Humbert, the project of holding the third session of the Congress in Berlin, and his Majesty specially charged me to convey to the officers and members his kindliest greeting, and the assurance of his sincere interest in the result of its deliberations, and desired me to be the interpreter of his wishes for its complete success. (Hearty applause.) In conclusion I have the honor to announce, that Dr. Beyrich has the floor." Hereupon Dr. Beyrich read from manuscript his address in French. It was an exhaustive history of the development and proceedings of the Congress up to the Bologna session, and also of the successive meetings of the committees on the chart of Europe at Foix and at Zurich. He also gave an account of the meeting of the German committee at Stuttgart in 1883. The meeting of the Congress at Berlin was determined upon for 1884, but was postponed on account of the cholera. The objects of the Congress—the discussion and determination of questions of geological classification, nomenclature and cartography were explained, and a general account of the results already attained was given. He closed by calling attention to the charts and collections in the Bergakademie which illustrated these results.

At the conclusion of the address of Dr. Beyrich, the following list of nominations for officers of the council for the Berlin session was read by Professor Capellini, and the nominations there made were elected by unanimous vote of the Congress, whereupon M. Capellini yielded the chair to Dr. Beyrich.

Members of the Bureau.

Honorary President: Dr. von Dechen.

President: Prof. Beyrich.

Vice-Presidents: Messrs. Credner, Fraas and von Gümbel, of Germany; Stur, of Austria; Dewalque, of Belgium; Johnstrup, of Denmark; Vilanova, of Spain; James Hall, of the United States; Jacquot, of France; Hughes, of Great Britain; Szabó, of Hungary; Blanford, of India; de Zigno, of Italy; Kjerulf, of Norway; van Calker, of Holland; Choffat, of Portugal; Stefanescu, of Roumania; Inostranzeff, of Russia; Torell, of Sweden; Renevier, of Switzerland.

General Secretary: M. Hauchecorne.

Secretaries: Messieurs Fontannes, Bornemann père, Fornasini, Wahnschaffe.

Treasurer: M. Berendt.

Members of the Council: Messrs. Benecke, Dupont, Böckh, Ewald, Frazer, Gaudry, Geikie, Giordano, von Hantken, de Lapparent, Lepsius, Mayer-Eymar, von Mojsisovics, Newberry, Pilar, Platz, Strüver, Topley, Williams, Zittel.

At the opening of the evening session, at 6 o'clock, M. Hauchecorne, the secretary, requested the members to inform the Bureau of any ladies they might have with them, in order that means might be provided for their comfort and entertainment. The first printed list of members was then presented.

The secretary further stated, that catalogues of the museums of science and of arts had been prepared and would be distributed at the close of the meeting. The Prussian minister had provided for the opening of the museums to all members of the Congress, from 9 o'clock till 3 P. M., and certain days were designated when the chiefs, or their representatives, would be present to show and explain their contents.

M. Renevier (of Switzerland), chairman of the committee on *the chart of Europe*, then commenced to read the report of his committee, explaining beforehand that it was not *his* report but the report of the committee which had met at Foix and at Zurich, and deliberated upon the matters referred to them. Although the place and times had been appointed for the discussion of all matters pertaining to the preparation of the geological map of Europe, unfortunately, the committee was not complete at any of its meetings.

The Committee of Direction has made a contract with the house of D. Reimer & Co., of Berlin, which engages to undertake the publication of the map under excellent economic and scientific conditions. The map will be divided into 49 sheets, each sheet of 48 centimeters by 53 centimeters. These 49 sheets united will form a rectangle $3\frac{36}{100}$ meters high, $3\frac{72}{100}$ wide. Professor Kiepert, of Berlin, has agreed to prepare the topographic base, which will be entirely remodeled according to the most recent data that can be obtained. The house of D. Reimer & Co. undertakes the publication at its own cost, on the single condition that the international committee guarantee to it the placing of 900 copies at 100 francs a copy, and furnish it sums on account in advance. The price of subscription is 100 francs, but 125 francs will be fixed at as the trade price of the work. This guarantee subscription has been divided as follows. Each of the great States of Europe: Great Britain, France, Spain, Italy, Austro-Hungary, Germany, Scandinavia and Russia agree to take 100 copies each. The six small States, Belgium, Holland, Denmark, Switzerland, Portugal and Roumania, will divide among themselves the last hundred copies.

A promise has been received from each of the above named countries that it will lend its assistance to the committee, conformably to the distribution above, with the single exception of Spain, whose answer has not yet been received. The commission will consider what can be done to obtain this answer.

As to the geological symbolization, it will be furnished naturally by the national committees, each one for its own country, and these contributions will be harmonized by the labor of the Committee of Direction, which, besides, will have the duty of completing the work, by all the data accessible to it, published or unpublished. The chromo-lithographic work will be done by the editors, Reimer & Co., conformably to the international scale fixed at Bologna and completed at this meeting.

The scale of the map was fixed by the unanimous consent of the Bologna Congress, September 29th and 30th, 1881, at $\frac{1}{1500000}$, at the same time that the map was decided upon, and its execution was confided to eight members composed of:

Messrs. Beyrich, Hauchecorne,	*Committee of Direction at Berlin.*	Germany.
Daubrée,		France.
Giordano,		Italy.
Dr. Möller,		Russia.
Mojsisovics,		Austro-Hungary.
Topley,		Great Britain.
Renevier (*General Secretary*),		Switzerland.

Specimens of the work done on the chart were exhibited to the Congress. The greatest progress had been made on those portions under the charge of Germany and Italy. The chart exhibited the wisdom of the decision of the Bologna Congress in expressing the successive subdivisions of the periods by graduated tints of the same color, the deepest tints representing the oldest stage.

At this point, Professor Capellini exhibited a roll that had been handed him as the first installment of the colored map of Italy, made on the scale agreed upon ($\frac{1}{1500000}$). It contained Central and Southern Italy.

M. Nikitin (Russia), reported that an installment of the map of Russia was *en route*, and that it would be exhibited the next day.

In conclusion, the report offered the following resolutions for the adoption of the Congress:

1st. Dr. Moeller, who has resigned, is hereby replaced in the commission by Mr. Karpinski.

2d. The "Carbonic" system (or Permo-carboniferous), will be represented by a gray color in three tints.

3d. The "Devonic" system will have three tints of brown.

4th. The color of the "Siluric" system is left to the choice of the committee on the chart.

5th. The eruptive rocks will be represented by seven tints of red, from bright to dark brownish.

6th. The determination of other questions in the report is left to the discretion of the committee on the map.

This was acted upon section by section. Section 1 was adopted without dissent. Section 2 was then read.

Professor Hughes (Cambridge), objected strongly. He said the discordance between the two formations in England was enormous and that the English geologists would never consent to this union.

Professor Dewalque (Belgium), defended the proposal of the committee.

M. Hauchecorne (Germany), urged that the gray chosen by the committee for the Permian was a greenish gray very different from that of the Carboniferous, and he believed that if Professor Hughes would look at the chart as made, he would find that all the distinction he desired was accomplished by this tint. His view was that a distinction of two entirely different tints of the same general color base would effect as complete a representation of the difference between the two series as could be effected by totally different colors.

M. Nikitin (Russia), thought the Carboniferous ought not and could not be joined to the Permian, and discussed the case of the so-called transition beds in Nebraska and elsewhere in illustration of the view.

Professor Renevier (Switzerland), thought the Culm, Carboniferous and Permian really constitute one system, but in order not to prejudge the case he had invented the term "Carbonic." Section 2 was then adopted. It was voted that the Carboniferous and Permian be colored in different tints of gray.

M. Dewalque (Belgium), objected to the use of the term Silurian in the following (4th) section, on the ground that the question of the limitation of the Silurian was to be brought up hereafter.

M. Renevier said he had used the term "Silurique" in order not to bring up the Silurian question, and moreover, he had said "Silurique, Cambrian included." He called the attention of M. Dewalque to the fact that it was impossible for him to discuss things without applying to them names, but that he did so in a manner that he thought would commit the committee and Congress in the least possible degree.

Professor Hughes energetically protested against the use of the word "Silurique." He had not found the Cambrian in the region of the Silures.

M. Jacquot (France) allied himself warmly with Professor Hughes in protesting against the use of the term Silurique, at least for the measures in France. One can recognize distinctly the difference between the Silurian and Cambrian in every part of the extended contact in his country, in the Pyrenees and in various other places they are never to be confounded.

Professor Renevier said, it is not a question of confounding them, but it is simply a question of using one general color-base for a column of measures which have certain points of analogy and are usually found together. They could be easily distinguished from each other by differences of tint or other means.

M. Jacquot replied that he could not see any reason for uniting two things that are distinct.

M. Hauchecorne (Secretary), said: Gentlemen, we must get on, and I ask as a personal favor on behalf of the committee on the chart of Europe, that the members repose a certain amount of confidence in it. It is not intended to prejudge any question or force upon the delegates any views other than those they desire to support. He suggested that the fourth article might be so altered as to allow the committee to adopt provisionally according to their choice, a scheme of colors for convenience, and that this choice should not decide the scientific question connected therewith at all.

M. Jacquot accepted the suggestion of the Secretary, and thereupon section 4 was adopted.

5th. The eruptive rocks shall be represented by seven tints ranging from dark to light red. Carried.

6th. The solution of other questions that might arise were referred to the committee on the chart for decision. Carried.

M. Choffat (Portugal) said that in joining the Dogger and the Malm, a junction is made which is opposed by all paleontological and petrographical evidences.

M. Hauchecorne stated that in his opinion the objection was too much a matter of detail to be brought before the Congress at this time, and he appealed to M. Choffat to withdraw his objection.

M. Choffat replied that, in doing his work in Portugal, it was impossible for him to take this view of the two series.

M. Hauchecorne again appealed in the name of the committee to M. Choffat, to withdraw his objection, stating that the committees on the maps of Europe and of Portugal would have ample time to consider and adjust all these points of difference. No definite action was taken.

M. Hauchecorne then announced that the council would meet at 11 A. M. and the Congress at 2 P. M. on Wednesday. The first two hours of the session would be devoted to business and the last two to purely scientific discourses.

2D SESSION, 2:30 P. M., 30th SEPT.

At the request of Dr. Beyrich, the Honorary President, Dr. von Dechen, took the chair. The Secretary then made announcements in regard to excursions, and stated that the Congress until 4 P. M., would discuss the report of the committee upon the chart of Europe. Afterwards they would listen to lectures upon special subjects: M. Gaudry upon certain reptiles, and Dr. Newberry (of New York), on a new fish from the Devonian.

The Secretary further announced the gifts which had been presented to the Congress.

M. Dewalque began the reading of the report of the committee upon uniformity of nomenclature at p. 13: A. Archæan System, Nos. 1, 2 and 3. "The first question to settle is, whether it should be included under the Paleozoic. The negative of this does not seem doubtful. Consequently and in conformity with the proposition of the French report, we propose to the Congress to decide that this system shall form a group to be known as the *Primitive* group. The termination of the word primitive will recall the characters which distinguish it from the groups '*primary, secondary,*'" &c.

M. Blanford proposed that we postpone the question of forming such a group till a later occasion.

Professor Hughes did not think that we had found the bottom of this group, and therefore we should wait for the determination of the term to be used, whether group or system. He called attention to an error in the report by which it would seem that the English committee prefers the term *Pre-Cambrian.* The English prefer the term *Archæan* to *Pre-Cambrian*, and they have used the former term.

M. Dewalque said if this group be not accepted, it must belong to the Paleozoic. [Loud objections.] Mr. Dewalque replied there was no way of avoiding the dilemma.

Professor Hughes thought we might represent it as a part of an unfinished system, but not as a system or a group.

M. de Lapparent (France) said if the Congress is willing to decide that there are no fossils in the Archæan, it should be set apart; if it contain fossils it must be joined to the Paleozoic.

M. Renevier proposed the term *Terrain* to avoid pre-judging the question of the rank in the classification of these rocks. He objected to the use of this term in any systematic sense, but believed it might be employed in a general sense.

Dr. von Dechen said, we want the terms "group" and "system" used for the chart, and do not want any vague terms. He believed it was necessary to maintain the usage of terms adopted by the Congress at Bologna.

Professor Hughes suggested that the use of the term *group*, for the Archæan be adopted, without settling its subdivision into systems, or attempting any correlation between subdivisions in different countries.

M. Renevier replied that we do not apply to eruptive rocks, the words "group" or "system," but simply "rocks." If eruptive rocks require no classification further than this, the words and coloration are sufficient for the Archæan.

Dr. Beyrich said that all that was necessary at present was the acceptance of the Archæan as anterior to Paleozoic time.

M. Stefanescu proposed the term "group" for all the rocks preceding the Paleozoic. His question was, "Shall we say Archæan 'system' or 'group?'"

M. Firket said there were two questions involved:

1st. Archæan or Primitive? 2d. Group or system?

M. Dewalque spoke to the same effect.

Dr. Hauchecorne asked for a vote on the terms "group" or system." "Group" was chosen.

The vote was then taken on "Archæan" or "Primitive." "Archæan" was adopted, after M. Renevier (chairman of the committee on the chart of Europe), had explained his views upon the question.

M. Dewalque proposed that some member should make a motion to divide the Archæan into three parts.

Dr. Hauchecorne asked M. Dewalque to make some proposition in order to bring the question before the Congress. No action was taken.

He stated that it was proposed to subdivide the Archæan into Azoic schists, Crystalline schists, and Protogine schists.

Professor Hughes suggested that it would be better to express the petrographic character and not divide the group chronologically. To this M. Renevier agreed.

M. Jacquot stated that no division of the Archæan in France was possible at present. The work of M. Lory in France and in the Alps results in establishing, as the best procedure, the coloration of mica-schists and gneisses in the same manner. He also supported M. Hughes's proposition.

M. Firket agreed to the petrographic divisions, but objected to the term "Azoic." It begs the question of the existence of life.

M. Stefanescu stated that the Archæan is well represented in Roumania, but there was extreme difficulty in making subdivisions.

M. Lapparent respectfully requested that the term "Protogine" be suppressed once for all, and gave his reasons. A vote was taken and the term was suppressed.

The proposition of Professor Hughes was then adopted, viz: to accept the Archæan as a group, leaving the petrographic divisions to each geologist and not assigning to them any chronological value.

The question then taken up was B 4, 5, and 6 (p. 14 of the committee's report), as follows:

The conference at Zurich has admitted provisionally the union into one system (for which the name is yet to be determined), of the different beds, corresponding to the Cambrian and Silurian of the British Isles. The French, Portuguese and Roumanian committees propose the name of Silurian System. Before voting on the proposition, the Congress will have first to pronounce upon the names to be given to the three groups, and then on their union into one or two systems. The Hungarian Committee propose a Cambrian and a Silurian system; the latter comprising the groups 5 and 6 united. The Belgian Committee would have proposed an analogous grouping, but preferred to conform to the decision taken by a large majority at Zurich. The French Committee does not propose any name for the three groups. The Roumanian Committee gives them inadmissible names (these should be each in one word), "lower, middle and upper.' The Belgian Committee proposes the names Cambrian, Ordovician and Silurian. The Portuguese substitute 'Bohemian' for the last term. We have already said the English Committee has not been called to decide upon the questions of the report which have been submitted to it."

Since the receipt of the reports of the national committees, the questions to be decided have been complicated. M. Jules Marcou, in an important work published by the American Academy of Science and Arts, and entitled "The Taconic System and its position in stratigraphic geology," has vindicated the priority of the term *Taconic* of which the Cambrian alone (or Primordial fauna) would be the equivalent. We think the question is demonstrated. In such a case the term Cambrian would be employed to replace the Ordovician, the name Silurian would come back by right to group 6. If we be not in error this solution would avoid many difficulties. We propose to the Congress to determine first, the names that the groups 4, 5 and 6 should bear. It will have to decide afterward whether they constitute one or two systems, and finally the name or names to be employed.

Professor A. Geikie proposed that the Congress postpone the subject of subdividing the Cambrian and Silurian until the meeting in England; on the ground that the Silurian question was mainly an English question. (Loud murmurs). Professor Hughes agreed with Professor Geikie as to the propriety of postponing the discussion of these questions, and said that Professor Hall had also expressed his approval of this course.

The chairman, Dr. von Dechen, put the question to divide the Silurian, but leave the names till the meeting in England. M. Capellini regretted such action, if it would postpone the completion of the European map. M. Hauchecorne said it would not, as the map could be completed without waiting for the determination of the names.

The motion was then put and carried.

The Congress then took up the Devonian. M. Dewalque continued reading that part of the report in regard to the Devonian (pp. 15, 16), numbers 7, 8 and 9. (*a.*) Conformably with the only propositions that have been made, the Congress is requested to decide that the three series of this system shall bear the names respectively of the Rhenian, the Eifelian, and the Fammenian.

(*b.*) We propose that the Calceola beds should form a part of the Eifelian.

(*c.*) Finally we propose to the Congress to decide that the upper limit of the Devonian system is to be placed at the base of the Carboniferous limestone; that is to say, that the system comprises the psammites of Condroz, the lower Carboniferous, (Kiltorkan, Marwook, Pilton) the upper 'Old Red' or the Calciferous sandstone, etc.

M. Renevier asked why the Coblentzian was called Rhenian. M. Lapparent explained that Coblentzian was used in a more restricted sense.

M. Dupont demanded that the upper Devonian begin with the zone of *Cyrtia Murchisoniana*. Dr. Beyrich remarked that few in Germany would agree to this classification. M. Renevier desired to say that M. Gosselet, whom he had expected to see here, regarded the junction between the Calceola beds and the Stringocephalus beds as forming the division between the lower and middle Devonian. M. Dupont remarked that such was the classification of M. Gosselet some time ago. Much had been done since. Dr. von Dechen said the Calceola beds should be in the middle Devonian.

The third section of the Devonian (in regard to its upper limit) was then read.

M. Geikie said that an error had crept into this clause and he proposed to strike out all after the word 'Condroz' except the words "the Upper Old Red." M. Renevier objected to sharp lines. We should not go into such details and he asked for the striking out of the clause on principle. M. Capellini said if it was necessary for the coloration of the map he would retain it, but it did not seem to him to be necessary. M. Dewalque thought it was necessary to make sharp distinctions in the map as to the beginning and ending of series, otherwise how was it possible to compare corresponding series in different countries? The limits must be at the same horizon for all regions recorded on the map.

M. Capellini proposed to adjourn the decision of fixed limits, because it was not necessary to the coloring of the chart. M. Hauchecorne was of M. Dewalque's opinion. Dr. Beyrich thought that we could not separate the Devonian from the Carboniferous at an absolute horizon.

M. Renevier said this was necessary in important cases but was not important here. M. Lapparent remarked that if the geologists of England are content to sit still and make no objection to the proposed limitation, the Congress would save much trouble by permitting the proposed limitation to be accepted, because the English are most interested in it.

At the close of the discussion the clause was stricken out.

Several scientific papers were then read, among them one by M. Gaudry on some fossil reptiles, and another by Dr. Newberry on a new Devonian Fish.

3D SESSION, OCT. 1, 2:30 P. M.

Session opened with Dr. von Dechen in the chair. M. Fontannes, secretary, read the minutes of the last two meetings, after which M. Dewalque continued the reading of the report of the committee on unifying the nomenclature. Professor Capellini read a telegram from the Syndic of Bologna as follows: "Bologna, proud of having been the seat of the second session of the International Geological Congress, sends an affectionate greeting to the illustrious savants assembled at Berlin, and hopes that their works will aid the progress of civilization."

M. Hauchecorne then announced the scientific memoirs which would be presented at 4 P. M.

Szabo: On the new map of Schemnitz.

Mayer-Eymar: The perihelions of the Globe and the sedimentary rocks.

Reusch (Norway:) 1. Exhibition of a meteorite which fell in Norway in 1884, with some observations on meteorites in general.

Reusch (Norway:) 2. Exhibition of specimens and charts illustrating the phenomena of pressure and tension in metamorphic rocks.

Taramelli: On chemical deposits, and two or three other papers.

This was followed by a list of the donations given to the members of the Congress and to individuals.

The announcement of the trips to Thale, Leipzig and Stassfurt were so modified as to enable those members who desired to proceed directly on Wednesday to Dresden instead of remaining to make the geological excursions with Professor Credner. These would go directly to Dresden under the auspices of Geheimrath Professor Geinitz, and on Thursday visit the Natural History Museum of the Zwinger, and afterward the collections of the Royal Gallery. Thursday evening they were to reassemble on the Brühlische Terrasse, and the next day to spend the time in observing the collections of Dresden. They will reassemble on the Belvidere on Friday evening. (This programme was carried out with some modifications.)

The continuation of the Report of the committee on nomenclature was then proceeded with by M. Dewalque at p. 15. D. the Carboniferous System.

M. de Lapparent took the floor and supported the proposition of the committee to unite the Permian with the Carboniferous. His ground was that every classification should base its horizons upon established fauna. Most happily for the geologists, in the earlier formations there is the most valuable evolution of the Cephalopods; but it was not thus with the Permian, as he appealed to the paleontologists who had occupied themselves with these beds to declare. Among other arguments presented, he remarked that in Asia, there was no Pelagic fauna, by means of which one could distinguish the Carboniferous and the Permian, and the same was true in other countries of which there were representatives present. He concluded, "I believe that in establishing the Permian as a unit we construct something which has nothing in common with the characters adopted for other sub-divisions; which has no distinctive characters of its own; and which in fact does not exist. Whereas united to the Carboniferous we have two distinct horizons of faunas, each of which is susceptible of further subdivision by pronounced differences in character. Dr. Beyrich made some observations. Mr. Jacquot thought that Mr. Dewalque should read to the Congress the opinions that had been expressed by the different national committees. This would have, in his view, the most capital importance in deciding the question. Mr. Dewalque conformably to the request of the last speaker, called first upon the French committee.

M. Lapparent did not think that his opinion should be brought into conflict with that of the French committee, to which as a member his name was attached.

M. Renevier spoke on this question.

M. Choffat, in the course of his remarks, insisted that the question of the thickness of measures was an entirely insignificant one.

M. Capellini read the report of the French Committee and observed that M. Lapparent may very well present his own views in the Congress, even though they be different from those of the committee.

Prof. Hughes exhibited a chart of a section made by himself: there was a large gap between the Permian and the Carboniferous; still the amount of time to be ascribed to that gap is different in different places, and no doubt if the contact line could be every where examined, places would be found where the two systems would approach each other very nearly. As at the base of the Carboniferous also, there is an enormous break of at least 27,000 feet of measures that had been eroded before the present discordant contact was effected. That between the Permian and

Carboniferous represents also an enormous lapse of time. In reply to the argument from the percentage of fossils common to the Carboniferous and Permian, he observed that the number of fossils, which are found in a given neighborhood depends both upon the excellence of the geologists looking for them and the assiduity of their search. The percentage of fossils common to the Paleozoic and Mesozoic is increasing every day in proportion to the hammering done.

M. de Lapparent was of the opinion that the arguments for establishing these stages should be pelagic traces rather than geographic situations. He continued, that if we could restore the geographical divisions of the world as they were at the time when these various groups were laid down and the Carboniferous and Permian did not present analogies which could be made out, he (Lapparent) would acknowledge himself in error, but the same argument could not be drawn from the present geographic conditions of the earth. He would cite, however, another argument, namely that from petrographic studies. There was not to be found in the Permian a trace of certain rocks so peculiar throughout the Carboniferous. All the outflows characterizing the Carboniferous on the one hand and the Triassic on the other were wanting in the Permian measures, where another order of things from that preceding seemed to have supervened.

Dr. Beyrich made some remarks. Another member of the Congress, stated that the Rothliegendes must be separated from the Carbonic and also from the Triassic.

The Hon. President, von Dechen, said that the Rothliegendes was a very remarkable group. It has the thickness in some places of 1,600 meters, and even at this depth the bottom is not found. Rothliegendes and Zechstein occur over vast extents of country. In Russia there are outcrops of it larger than the whole of some countries existing in Europe.

M. Blanford said: "In taking up this question we take up one that concerns many parts of the world. Outside of Europe there is no Permian—I mean no European Permian. It is impossible to separate the upper from the middle and lower Carboniferous. I believe that the fauna of the Zechstein is a local fauna and therefore I give my adhesion to the views of M. de Lapparent as to uniting the Permian and the Carboniferous."

M. Capellini, rising with the report of the committee at Zurich in his hands, remarked that there must be some mistake in the printed report inasmuch as it was there stated that M. Blanford was of the opinion that there was an evident division between the Carboniferous and Permian.

M. Blanford stated that the report was entirely correct and that he would explain how the misunderstanding arose. He was under the impression, during the discussion at Zurich, that the question was simply of European geology—and in the vote that was taken he had no part.

M. Stur made some observations on the course to be pursued in treating these two formations. He believed in uniting the Permian and Carboniferous in one system.

M. Nikitin: "We have two regions in Russia where we have studied these groups. They are divided into two stages. In central Russia, in the Volga valley, we can distinguish them, but at the foot of the mountains we cannot. We cannot at the present time, therefore, define accurately the limits between these different systems, but no doubt in the future we shall be able to do so.

M. Renevier was glad to hear from M. Stur's remarks the confirmation of views which he had always held and often expressed, namely: that the classification based on gaps is false and artificial. I agree with M. Nikitin, that our groups are all artificial. (Dissenting murmurs). Oswald Heer calls the Permian Upper Carboniferous by its flora. And as to the fauna he has shown a great number of species that are similar. M. Gaudry has done the same for the reptiles; M. Fritsch's views tend in the same direction. The divisions ought to be made on paleontological evidence.

Professor Newberry remarked that he knew it was a question here of the European map, and perhaps it would be an impertinence on the part of an American to express any opinion; "but I am asked," he continued "to express the opinion of my honored colleague Professor Hall, that there is no Permian in America. From my own studies also I know, that there is an insensible transition from the Carboniferous beds, to those which correspond in position to the Permian, and there is no strict line of demarcation between the Trias and the Permian. Therefore, for America (and only for America I speak), the Permian as a separate division does not exist."

M. Capellini: "The president asked me to see what can be done to advance the map, and although it appears to me that a majority of those present is in favor of joining the Permian and Carboniferous, still there is a respectable number of those who are opposed to it. And therefore the commission on the map would propose to adjourn the discussion and definite settlement of this question until a future time.

M. Topley said: "M. Blanford speaks only in general terms and not for England in the matter of these groups. It is highly important, as well for the classification as for the economic geology of England, to preserve the identity of each system. He agreed with Professor Hughes in drawing a strong line of demarcation between the Permian and the Carboniferous.

M. de Lapparent said: I ask the Congress to give the statements made by Professor Newberry and M. Blanford, in regard to the absence of the Permian in various parts of the world, the attention that they deserve. It seems to me that the object of this Congress was to establish a system applicable to all the world and not to Europe alone, or it should not have invited geologists from other countries than the European to participate.

M. Capellini remarked that these matters were to be settled as broad and grand questions in Science—without paying too much attention to individual matters of detail in which different countries might differ.

M. Neumayr thought that just because the questions were grand and broad they should be left to the free and unrestricted discussion of scientific men in the journals and societies of the world, and not be settled by a majority which changes with every country, and after the address of an eloquent orator.

M. Capellini made some further observations. Professor Hughes said that Professors Newberry and Blanford had stated that there was no Permian in India and America, but that they had simply found fossils having a Permian facies in the Carboniferous. He concluded by expressing the belief that it was better to leave the question open.

M. Hauchecorne: I agree entirely with the views of Professor Hughes as to the scientific aspect of the question, which we propose to leave to the future. But in the map we will arrange the order of the beds provisionally as it is in the proposed chart of colors without uniting the two systems in the legend of the chart by a bracket.

The Hon. President von Dechen agreed with the views expressed by Mr. Neumayr and desired the map to go on to its completion at the earliest moment.

M. Dewalque: "I propose the following as expressing the opinion of the Congress on this subject:"

"The Congress not wishing to pronounce any view on the scientific question of the proper division of the Permian and Carboniferous, preserves the classification as it now is." (Adopted with about fifteen dissenting votes).

4th Session, Oct. 2d, 2:30 p. m.

The Congress assembled in the Reichstags chamber and M. Capellini occupied the chair as chairman *pro tem.*

The report of the Council was read and the nominations proposed by it for the committee on the chart of Europe were voted upon and unanimously elected. They were as follows in alphabetical order (in French) by countries:

Germany,	Römer.	India,	Blanford.
Austria,	Neumayr.	Italy,	Capellini.
Belgium,	Dewalque.	Japan,	Neumann.

Canada,	T. Sterry Hunt.	Norway,	Kjerulf.
Denmark,	Johnstrup.	Holland,	van Calker.
Spain,	Vilanova.	Portugal,	Choffat.
United States,	James Hall.	Roumania,	Stefanescu.
France,	de Lapparent.	Russia,	Inostranzeff.
Great Britain,	Hughes.	Sweden,	Torell.
Hungary,	Szabó.	Switzerland,	Renevier.

The members of this committee were requested to vote for a president for the next meeting. M. Fontannes read the journal of the preceding session, which was approved.

M. Capellini, in the chair, then took up the question on which the Congress was engaged at the close of the last session, and asked if any one wished to speak further upon giving three divisions to the Trias. After a pause, M. Renevier remarked that he did not wish to take up the time of the Congress, but he wanted to know how it is intended to color the Trias. Is it intended only provisionally to accept the divisions for the chart or not?

M. Stefanescu said the proposition to accept the divisions of the chart prejudges the whole question.

M. Dewalque, M. Blanford, and M. Capellini further discussed the question, and finally the three-fold divisions of the Trias proposed at the Zurich meeting was agreed to.

The question as to the proper place of the Hettangian beds (whether with the Trias or with the Lias) was discussed but no decision was reached.

It was decided to divide the Jurassic into three parts.

The question of the union of the Rhætic, not including the Hettangian, with the Lias or Trias was again discussed.

M. Hauchecorne observed that the scale of colors and symbols were so arranged that the Rhætic could be classed with the Trias or Lias to suit the observer. The question as to the superior limitation of the Lias with the zone of *Ammonites opalinus* was discussed.

M. Choffat thought that so little of this series is known in Europe that the limit should be left for each geologist to place it at his own discretion. Agreed to.

Next the Tertiary was taken up. An animated debate ensued in which M. Meyer of Zurich, Dr. Beyrich, M. Stefanescu and M. Neumayer took part.

Finally the chairman, M. Capellini, proposed that, in view of the fact that no progress seemed possible owing to the divergence of views maintained, a vote of confidence in the committee on the chart be taken; assuring the members of the congress that the committee would exhaust every means to satisfy the views of the different members. (This vote of confidence was carried unanimously.)

The proposition in regard to the Eruptive Rocks was then taken up.

Professor von Dechen declared that there should be no distinction made between the rocks of extinct and of active volcanoes, or between ancient and modern eruptive rocks, but there should be a strong distinction drawn between ancient tuff and ancient eruptive, and between modern tuff and modern eruptive rocks proper.

Dr. Beyrich agreed with his Excellency, Dr. von Dechen, on this point.

M. Blanford said it should be taken into consideration that in parts of England, in the Hebrides, in parts of America and elsewhere, there were eruptive rocks, and lavas which resembled stratified rocks very closely. He objected to the petrographic division of the eruptive rocks, while the sedimentary rocks are divided chronologically; the more so, as many of the eruptive rocks, like those he has instanced, strongly resemble the stratified rocks.

The whole matter was finally left to the committee on the chart.

The president pro tem. then passed to the second order of business, and gave the floor to Dr. Neumayr, who read the report upon the proposed plan for the preparation of his Nomenclator Paleontologicus.

5TH SESSION, OCT. 3D, 10 A. M.

At the morning session several scientific papers were read. Among them was a report upon the system of coloration in use in the United States Geological survey. Mr. McGee, who had prepared this report, did not arrive till late in the progress of the Congress. The paper was presented to the council in English, but the rule requiring all the communications to the Congress to be presented in French, necessitated the preparation of an abstract in that language. This abstract, at the request of Mr. McGee, was presented to the Congress by Dr. Frazer, together with prefatory and explanatory observations by himself regarding the map; exhibiting the principal features of the system which was displayed.

At 2.30 P. M. the sixth and closing session of the Congress was called to order. The journal of the last sitting was read and approved. M. Hauchecorne made several amendments.

Three sheets of the map of Galicia were presented, with a letter from their author, Professor Szajnocha. A letter was received from M. Abich, stating that he had returned to St. Petersburg and had resumed his labors.

M. Capellini (pres. pro tem.) called attention to the Nomenclator Paleontologicus, of which M. Neumayr had given description yesterday, and recommended that it be published under the auspices of the Congress and under the editorial direction of a committee, consisting of Messrs. Gaudry, Neumayr, Zittel and Etheridge, with power to add to their number. (Carried.)

M. Vilanova then mounted the tribune and asked assistance for his polyglot dictionary of geology, a Spanish-French specimen of which he exhibited.

The committee on the formation of an international geological society, and of an international geological journal reported, and a letter was read from M. Gregorio of the committee favoring the plan; whereupon the president pro tem., M. Capellini, stated that upon consideration of the report and the facts, the council had decided against the advisibility of both plans.

Baron Levi asked an explanation. M. Capellini stated that no reports could be made to the Congress, unless previously recommended by the council, and explained that it was not intended to slight the proposition of his countryman, whose acts and motives were warmly approved and appreciated, but simply to adjourn the question till the meeting of the next Congress. Upon this a vote was taken upon the action recommended by the council, viz: favoring the scheme of an international geological journal, provided it were supported by joint private enterprise, which was approved. The president pro tem. then announced that the second part of the programme would be proceeded with and gave the floor to M. Nikitin, who explained the work he had done on the portion of work in Russia committed to his care, viz: Central and South East Russia, including the basin of the Volga.

On the conclusion of M. Nikitin's remarks, M. Vasseur took the floor and exhibited thirteen sheets of the geological map of France, prepared according to the principles adopted at Bologna. M. Hauchecorne, the general secretary, stated that it was a pity that the legend of the Russian maps should be printed in characters which people of other nationalities could not understand, and he asked that a copy of each map should be furnished with the names in French characters. M. Nikitin replied that every sheet that he had exhibited contained the names of all the important places and all the rivers and streams in French characters, and demonstrated that this was the case.

M. Posepny read a treatise on the fluid condition of the interior of the earth. M. Ochsenius presented his views on the origin of salt deposits and gave diagrams and explanations, claiming analogies between certain chemical and physical conditions in the Caspian and the German oceans and the results of explorations to be seen in the mines at Stassfurt and elsewhere.

M. Capellini (president pro tem.) then announced that the hour had come to draw the session to a close.

It was time for the Congress to determine the place of meeting of the 4th Congress of 1888. The council had to propose that the next meeting be held in the year 1888, between the fifteenth day of August and the fifteenth day of September; that London be the place of meeting and that Messrs. Geikie, Blanford, Hughes, and Topley be the committee to prepare for the proper reception of this Congress. Professor Hughes thought it had been very appropriate to cede to Germany the place of meeting of the present Congress, and its success had justified his opinion. He repeated his statement made to the council, that he had a petition signed by one hundred and thirty-seven English geologists requesting the Congress to meet in London. This petition included the names of the Duke of Argyle, the Earl of Enniskillen, and some of the most eminent geologists of England; and he hoped that England would be chosen as the next place of meeting.

M. Geikie expressed the same views and said that English geologists follow the action of this Congress with the greatest interest, and would unite in giving it a warm reception.

The recommendation of the council was approved.

The acting president, M. Capellini, yielded the chair to the president, Dr. Beyrich. M. Capellini then took the floor and said: "Before parting, thanks were due to certain august personages and societies and individuals, naming His Majesty, the Emperor of Germany; the Prussian Government, and especially the Minister of Public Works, and the Cultus Minister who opened the Congress with an able address, the Academy of Mines, his Excellency Dr. von Dechen, Dr. Beyrich, and Dr. Hauchecorne." (Applause).

Dr. Beyrich observed that in the last words he had to address to the Congress, he begged to be permitted to speak in the language in which he thought. He thanked the Congress for its kind assistance and support, and introduced his Excellency, Dr. von Dechen.

The honorary president remembered well the first scientific Congress held in Berlin in 1858, under the auspices of the Baron Alexander von Humboldt. Berlin was then a small town but had grown enormously since. He concluded by hoping that all the members would return to their homes with an agreeable souvenir of their sojourn in Berlin.

Dr. Hauchecorne, the general secretary, spoke of the eminent service of M. Capellini, and concluded with the hope that the friendships made here would endure and be the more closely knit at the future session to be held in London.

M. de Lapparent mounted the tribune and expressed, on behalf of the members of the Congress, their sense of obligation to the German committee of arrangements. Geological questions, he said, were of a kind to be settled on the spot, and geological brethren mutually dug in the earth and divided the debris in a christian spirit. While here in Berlin, our intellects, our artistic tastes, and our capacities for pleasure have all been considered. Honor to the noble science of geology, which can induce intelligent men such as our hosts, to provide for the dead fossils from the earth's crust mansions as superb as the residences of kings. (Applause.)

The Congress was thereupon declared adjourned.

ART. LVIII.—*Bright Lines in Stellar Spectra;* by O. T. SHERMAN.

UP to date, as far as my knowledge goes, bright lines have been admitted to form part of the spectra of but six stars, β Lyræ, γ Cassiopeiæ, and four small stars in Cygnus. The claims of four others in Orion have been advanced and denied.

In the recent volumes of the Nachrichten, Konkoly and Gothard have called attention to the first and second of the stars enumerated above. The result of the former work may be summed up in the identification of the bright lines D_3, $H\alpha$, $H\beta$, $H\gamma$, $H\delta$, of the dark lines b, D, and a broad band in the violet, and the recognition of a seven day period for the variation of the spectrum of β Lyræ.* Konkoly also says:† "Ich glaube auch noch manchmal im Grün und Blau einige sehr zarte Linien gesehen zu haben, was aber auch eine durch das Flattern des Spectrums verursachte Täuschung sein kann." With a view to following these stars and learning whatever a conscientious study of their spectra might show, the equatorial (8 in.) of the Yale College Observatory was devoted thereto.

The spectroscope employed is a direct vision by Duboscq; the distance from the slit to the collimating lens being about $14{\cdot}8^{cm}$. The train is broken into two series of three prisms each. Using the single series the lines b_3 and b_4 are barely separated. Using the double series the nickel line between D_1 and D_2 is seen, and b_3 is separated from b_4 by about the width of the latter. The power of the eyepiece of the observing telescope is about 130. A cylindrical lens behind the eyepiece is usually employed. Previous to each night's work the instrument was adjusted upon the sun; a solar spot, when possible, being brought sharply in focus upon the jaws of the slit. For stellar observation the slit was opened wide, 5^{mm} or more.

* Astronomische Nachrichten, 2539, 2548, 2651, 2581.

† Astron. Nachr., 2548, p. 62.

When the series of observations was first commenced there were recognized in the spectra but a few bright lines, so situated as to be probably the hydrogen lines, D_3, and in addition 1474(K) and 1250(K). As the observer became accustomed to the spectrum of β Lyræ it became apparent that there were also other bright lines. With the single series of prisms ten such were counted.

Recalling now the course of reasoning which led to the daylight observation of the solar prominence, and also that many more bright lines than those already recognized were seen in the spectrum of the solar atmosphere, I employed the highest dispersion obtainable. The number of bright lines was increased to seventeen. It seems extremely probable that an increased dispersion will bring out many more. Arrangements for so improving the apparatus are in progress. The story for γ Cassiopeiæ is similar.

The instrument has been turned upon numerous other stars and in each case many or few bright lines have been seen, lines, so far as I know, formerly unsuspected. The careful description awaits the completed apparatus. At present it would seem that the lines are most easily seen in the red stars. This may be a mistake. The word lines is here used only by analogy to signify bright stellar images. At the red end under a sharp focus they stand out the full breadth of the spectrum, bearing somewhat the same relation to the background as the prominence to the solar spectrum. In the brighter portion of the spectrum they are cut down to fine star points. At the blue end they become more distinct but not so sharp as at the red. At times they shine with almost a metallic brilliancy; at other times they are faint, faded, and easily passed over. Certain sets appear to be prominent at times, others at other times.

The difficulties of the observation, and the roughness of the recording apparatus have hindered a completely satisfactory identification of the lines. Assuming the position of the hydrogen lines and of D_3, and on their basis drawing the curve connecting scale reading and wave length, the mean of nine observations upon γ Cassiopeiæ affords the following approximate wave lengths. The positions of the dark lines are underlined.

$H\alpha$, 635·6, $\underline{628}$, 616, D_3, 584·0 ? $\underline{576}$, 555·75, 542·2, 530·98, 516·75, $\underline{502}$, 499·0, $\underline{492}$, $H\beta$, $\underline{467·35}$, 462·3, $H\gamma$ 418 ? $H\delta$, $\underline{399·3}$.

It is of interest to compare these with the following wave lengths taken from Prof. Young's catalogue of lines observed at Sherman:*

$H\alpha$, 634·6, 614·06 D_3, 585·27, 553·4, 544·59, 531·59, 516·83 and 516·67, 501·76, $H\beta$, 457, $H\gamma$, 421·5, $H\delta$.

* This Journal, Nov., 1872.

While the identification is not complete the number of the approximate coincidences renders it extremely probable that the lines observed are those of the solar atmosphere.

To quote the pioneer of American spectroscopic observations, it would seem that there are many stars in the same condition as the sun, but with the corona more pronounced.

Yale College Observatory, November, 1885.

Art. LIX. — *Note on the Optical Properties of Rock-salt;* by S. P. Langley.

Since the first experiments of Melloni the optical properties of rock-salt have received comparatively little attention, although this substance is every day coming more into use, as the importance of the study of radiant heat is recognized. It was asserted by Melloni that rock-salt is almost perfectly diathermanous to all kinds of heat radiations, and that it transmits a little over 92 per cent. of the incident heat of whatever kind. This statement was disputed by Provostaye and Desains,* who maintained that there was considerable difference in its absorptive action toward heat radiated from sources differing widely in character; still, even admitting the claims of these physicists, the great difference between the action of this substance and others, such as glass, is very striking, and we are dependent chiefly on it for our analysis of the action of obscure heat.

In certain researches which have been made of late at the Allegheny Observatory on the distribution of heat in the spectrum of the moon, and of terrestrial sources of radiation at very low temperatures, the use of an exceptionally perfect rock-salt train has been sought in order that heat deviations measured with a precision comparable with that of optical work might be secured, at the same time that the extremely feeble radiations at command should suffer the least possible amount of absorption in the apparatus. After long searching, blocks of rock-salt were finally obtained through the great kindness of Professor C. S. Hastings, of Yale College, from which two 60° prisms were cut of about 64 millimeters on a side, and lenses of nearly 75^{mm} aperture. The most perfect prisms the writer could obtain in Europe, did not distinctly show a single Fraunhofer line, and he was assured by opticians there that no rock-salt prism ever did or could do more. He is happy to say that the skill of our American opticians has produced what was pronounced impossible,—a rock-salt prism which shows the Fraunhofer lines with all the sharpness of flint glass. Such prisms

* Comptes Rendus de l'Académie des Sciences, xxxvi, p. 84.

have been made for us by Messrs. Alvan Clark & Sons, of Cambridge, and Mr. J. A. Brashear, of Pittsburgh, who has worked that here described and which shows the nickel line between the D's.

For the reduction and arrangement of these observations a more complete knowledge of the properties of rock-salt than had hitherto been obtained was essential, particularly as regards its indices of refraction for rays of determinate wave-length, and its diathermancy for dark heat rays of different degrees of refrangibility.

Advantage has been taken of the use of the train above mentioned to determine, not only the indices, which will presently be given, but also the the apparent transmission of rock-salt plates in different parts of the spectrum, but the latter part of the work is not represented here. We shall only observe that we have had occasion to form "heat"-spectra from radiating sources below the temperature of melting ice, and that while most ofthe rays, even from these sources, passed freely through the prism; with the smallest deviations corresponding to wave lengths, probably exceeding 100,000 of Ångström's scale, a slight absorption began to be noticed. We hope to shortly give more full determinations of this, in connection with a statement of the deviations and wave-lengths of heat from sources at all degrees between the temperature of melting platinum and that of melting ice, with which it will appear in a more proper connection.

Although in working with such heat radiations even as those forming a part of the solar infra-red spectrum, more error is to be expected than in the optical observations, these errors are, with our present apparatus, of an order not wholly incomparable with the optical ones.

It is however to be understood that the best heat spectrum work can only be accomplished with *brightly polished* rock-salt. The surfaces of the rock-salt prism and lenses undergo a deterioration when exposed to the air, which is more or less rapid according to the greater or smaller relative humidity of the atmosphere at the time. In ordinary dry weather they may be used several times before they become spoiled, while in damp or rainy weather, three or four seconds is a sufficiently long time to cover them with condensed moisture, and work under these circumstances is of course impossible. After the surfaces have in this way become unfit for use they are repolished, and the refracting angle of the prism is thereby unavoidably altered. The change is usually small, generally not exceeding 1′, so that for most of our heat measures it may be neglected altogether. The changes have however tended on the whole to reduce the refracting angle, so that it is now about 4′ smaller than when the prism was first used.

To make all observations strictly comparable, they are reduced to one value of the refracting angle, for which the deviations of the Fraunhofer lines and the wave-lengths corresponding to given deviations in the infra-red, have been determined with the greatest possible accuracy. This standard value of the refracting angle is 59° 57′ 54″. A series of observations for fixing the positions of the Fraunhofer lines was made by Mr. J. E. Keeler of this observatory, on September 14th, 1885. One arm of the spectrometer, which was firmly clamped, carried a glass collimating lens of 25 feet focus, and the other an achromatic observing telescope of nearly four feet focus, with a mycrometer eyepiece. The double deviations of the C, D_{1}, b_{1}, and F lines were observed, and also the differences of deviation between these and the other lines whose positions were determined. For observing the M and N lines a Soret fluorescent eyepiece was used, and in the infra-red a bolometer, having a single strip $\frac{1}{10}$ millimeter in width. In the two last cases the prism was automatically kept in the position for minimum deviation. The spectrometer circle reads by two opposite verniers to 10″, but on account of the construction of the instrument, (for whose principal purpose arms whose length is inconsistent with absolute rigidity had to be used) care is necessary to measure an angle with this degree of precision, as the arms are liable to spring slightly on the application of lateral pressure. The deviations given in our table were obtained by Mr. Keeler by setting on the line, with the micrometer eyepiece, after the telescope had been directed upon it and freed from strain by a light tap, and applying the micrometer correction to the circle reading. It was found by careful comparison of the solar spectrum given by the rock-salt prism with that by a fine prism of flint glass, that in spite of the greater dispersion of the latter, no lines could be seen in its spectrum which the rock-salt prism would not also show. The probable error of one setting of the micrometer was less than 1″. From the agreement of the different measurements made in this way, it is believed that the deviations throughout the visible spectrum are correct to within 5″. Those in the ultra-violet and infra-red, cannot of course pretend to this degree of accuracy. The positions of $\rho\sigma\tau$ (invisible) given by two independent series with the bolometer differed by 30″; those of φ (invisible) by 1′, those of ψ (invisible) by 30″ and those of Ω (invisible) agreed exactly.

We have thus obtained incidentally the data for constructing a table of refractive indices of rock-salt throughout the entire range of the solar spectrum, with an accuracy which we believe to be greater than has heretofore been attained, and which we deem of sufficient interest to give in full below, for the convenience of others having occasion to work with this material, and for testing theories of dispersion.

Refracting Angle of Prism=59° 57′ 54″.

Line.	Wave-length.	Deviation.	Δ	Refractive Index.
M	0·3727	43° 50′ 57″	1·21	1·57486
L	0·3820	43 35 27	1.20	1·57207
H_2	0·3933	43 19 32	1·19	1·56920
H_1	0·3968	43 14 44	1·19	1·56833
G	0·4303	42 36 7	1·16	1·56133
F	0·4861	41 51 47	1·13	1·55323
b_4	0·5167	41 33 43	1·12	1·54991
b_1	0·5183	41 32 52	1·12	1·54975
D_1	0·5789	41 2 41	1·10	1·54418
D_2	0·5895	41 2 29	1·10	1·54414
C	0·6562	40 42 56	1·09	1·54051
B	0·6867	40 35 49	1·09	1·53919
A	0·7601	40 22 25	1·08	1·53670
$\rho\sigma\tau$	0·94	40 1 26	1·07	1·5328
φ	1·13	39 49 11	1·06	1·5305
ψ	1.39	39 39 56	1·05	1·5287
Ω	1·32	39 29 21	1·05	1·5268

Temperature=24° C. Barometer 731·1mm.

The wave-lengths of the M and L lines are from Cornu, those of the lines between H and A inclusive, from Ångström, and those of the infra-red bands from the Allegheny observations. The column headed Δ was prepared at the suggestion of Mr. Keeler, and is for the purpose of facilitating the reduction of observations made with a different prism angle from that for which the table is computed, and for which our wave-length curves are drawn. If we differentiate the ordinary formula for a prism

$$n=\frac{\sin\frac{1}{2}(A+d)}{\sin\frac{1}{2}A}$$

with respect to A, which we now regard as a variable, we have

$$\frac{dd}{dA}=\frac{n\cos\frac{1}{2}A}{\cos\frac{1}{2}(A+d)}-1=\Delta$$

or $dd=\Delta\, d\,A$.

The values of Δ for the different lines of the spectrum are readily computed from the table of deviations and refractive indices. To find, then, the deviation of a line after any repolishing of the prism, we have merely to multiply the change of the angle by the approximate value of Δ taken from the table, and we obtain the change in the deviation of the line, and hence also the deviation required. Thus, if the new angle is found on measurement to be 59° 57′ 44″, $d\,A=-10''$, and the deviation of the F line, (say), will have been changed by $-10'' \times 1\cdot13 = -11\cdot3''$. That is, the deviation of the F line is now 41° 51′ 36″. The reduction from the new to the standard angle is of course the reverse of this, and that the use of the table saves much labor in redetermining the constants of the prism will be understood when it is added that ours has been entirely refigured and repolished by the maker as many as ten times during the present year.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Reaction of Barium sulphate on Sodium carbonate, under pressure.*—Spring has succeeded in effecting by means of pressure alone, a reaction between barium sulphate and sodium carbonate. An intimate mixture was made of one part of pure precipitated barium sulphate, thoroughly dried, with three parts of pure and dry sodium carbonate. About a gram of the mixture was submitted to the compression, the cylinder produced pulverized and extracted with water; and the insoluble residue analyzed to determine the amount of barium carbonate produced. After compressing the mixture under a pressure of 6,000 atmospheres for a few instants only, nearly one per cent of the barium sulphate had been transformed into carbonate. The uncompressed mixture gave only traces of barium carbonate. The cylinder from the first compression was pulverized and compressed anew. After four successive compressions the amount of carbonate produced rose to 4·78 per cent, and after six to 8·99 per cent, thus showing very clearly the value of renewing the surfaces of contact. If these cylinders after pressure be left to themselves for some time, the chemical action continues up to a period of fourteen days; the quantity of barium carbonate, produced in the cylinder submitted to six compressions, rising during this time to 10·89 per cent, thus throwing some light on the interesting question of diffusion in solids. Again, if these cylinders, after compression, be divided in halves and one half heated for three hours to 120°, it is found on analyzing these two halves that the barium carbonate formed has actually diminished during the heating; the percentage falling between one and two per cent. The author proposes now to study the reaction of sodium sulphate and barium carbonate under the same conditions.—*Bull. Soc. Ch.*, II, xliv, 166, Sept., 1885. G. F. B.

2. *On Sulphocyanuric acid.*—The ready conversion of cyanic into cyanuric ether, led Hofmann to attempt a similar transformation with sulphocyanic ether. And successfully, for on heating methyl sulphocyanate to 180°–185° for several hours it was converted into methyl sulphocyanurate, proved to be the normal

not the iso-acid. On attempting to repeat this process subsequently with carefully purified materials, no trace of the polymeric body could be found. The result was, however, readily effected, after a few drops of sulphuric or hydrochloric acid were added to the liquid before digestion. The ether is obtained in beautiful crystals which may be heated to 180° with water without change; though in presence of concentrated hydrogen chloride it splits at 100° into methyl mercaptan and cyanuric acid. Similar ethyl and amyl compounds were obtained. To obtain the sulphocyanuric acid, the methyl ether was mixed with sodium sulphide and heated in closed tubes to a temperature of 250° for three or four hours. The solution, after filtration, was treated with hydrogen chloride in excess, whereby the sulphocyanuric acid was precipitated as a yellow granular powder. It may be obtained pure by conversion into the sodium salt and reprecipitation. It is scarcely soluble in water, even boiling, and is also insoluble in alcohol, ether, benzene and nitrobenzene. It has the formula $(N{\equiv}C{-}SH)_3$. The sodium, barium, silver, lead, copper, potassium, lithium, calcium, magnesium and other salts are described insoluble.—*Ber. Berl. Chem. Ges.*, xviii, 2196, Sept., 1885. G. F. B.

3. *On the Synthesis of Cocaine.*—Some months ago MERCK announced the production of a derivative of cocaine, benzoyl-ecgonine. He has now succeeded in re-introducing the methyl group into this derivative and in reproducing cocaine. For this purpose the benzoyl-ecgonine was heated with the theoretical quantity of methyl iodide and potassium hydrate in methyl alcohol, in sealed tubes to 100°. The product obtained was identical with the natural cocaine in all its physical and chemical properties.—*Ber. Berl. Chem. Ges.*, xviii, 2264, Sept., 1885. G. F. B.

4. *On Hydrogen Persulphide.*—SABATIER has examined with care the substance known as hydrogen persulphide with a view to fixing definitely its composition. As ordinarily obtained it is a reddish-yellow oily liquid, varying in composition from H_2S_6 to H_2S_{10}. This uncertainty of composition is due to the free sulphur dissolved in the persulphide, the liquid saturated at 18° having the composition H_2S_{10}. It contains also H_2S dissolved, from which it may easily be freed by placing it in vacuo. The persulphide was prepared by Thenard's method by allowing a fine stream of calcium polysulphide to flow into concentrated hydrogen chloride, both cooled to 10°. The yellowish liquid, well dried, was then distilled in vacuo, and afforded a clear brilliant, very limpid, mobile yellow liquid, having an extremely irritating odor. While retained in the bulbs which acted as the receivers, it was permanent; but it decomposed on decantation. On analysis, three samples gave as the sulphur in excess of H_2S 57·9, 59·2 and 58·9 per cent; or 58·7 as a mean. This corresponds nearly to the formula H_4S_5, which requires 58·5 per cent. From these results the author concludes that the true formula of hydrogen persulphide is H_2S_2 analogous to H_2O_2; the excess of sulphur being due to a partial decomposition in the process of distillation, the sul-

phur vapor being carried over with that of the persulphide.—*Bul. Soc. Ch.*, II, xliv, 169, Sept., 1885. G. F. B.

5. *On the Valence of Phosphorus.*—The valence of the element phosphorus has long been an open question. On the one side the compound PCl_5 has been regarded as proof of its pentad character; while on the other, this pentachloride has been considered a molecular compound consisting of a molecule of the terchloride united to a molecule of chlorine, $PCl_3 . Cl_2$. Even the oxychloride $POCl_3$, may be written either $O=P\equiv Cl$ or $Cl_2=P-O-Cl$ to accommodate the former or the latter view. MICHAELIS and LA COSTE have now thrown some light upon this question; and this for the first time from the purely chemical side. In 1882 the former of these chemists, in connection with Gleichmann, discovered a body of the composition $PO(C_6H_5)_3$ which he called triphenyl-phosphine oxide. It was prepared by warming the hydrate, which itself was obtained either by the action of sodium hydrate upon triphenyl-phosphine dibromide, or by acting with potassium chlorate and hydrogen chloride upon triphenylphosphine; processes analogous to those by which phosphorus oxychloride is formed either from the pentachloride or the trichloride. The substance is a solid body, of specific gravity 1·2124 at 22·6°, having a vapor density of 9·9. The authors have now succeeded in preparing another body having the empirical formula $(C_6H_5)_3PO$, by the action of phenol upon diphenyl phosphorous chloride. This therefore must have the constitution $(C_6H_5)_2PO(C_6H_5)$. Hence if phosphorus is trivalent and its oxychloride is $Cl_2=P-O-Cl$, this compound must be identical with triphenyl-phosphine oxide above described. But these bodies, whose molecular magnitudes are both expressed by the formula $C_{18}H_{15}PO$, are radically different in physical as in chemical properties. Triphenyl-phosphine oxide as already stated, is a solid body fusing at 153·5, and completely indifferent to bromine, oxygen, sulphur, selenium, benzyl chloride and methyl iodide; while the isomeric phenoxyl-diphenyl-phosphine is a thick oily liquid, which readily combines not only with the elements above mentioned, but with the alkyl-halogens to form crystallizable addition products; a property characteristic of the compounds of trivalent phosphorus. The constitution of this phenoxyl-diphenyl-phosphine therefore can be expressed in a formula only by considering it as a derivative of an isomeric phosphorus oxychloride at present unknown, which contains trivalent phosphorus, thus:

Phenoxyldiphenyl-phosphine	*Triphenyl-phosphine oxide*
$(C_6H_5)_2\overset{III}{P}-O-C_6H_5$	$(C_6H_5)_3\overset{V}{P}=O$
Isophosphorus oxychloride	*Phosphorus oxychloride*
$Cl_2\overset{III}{P}.OCl$ (unknown.)	$Cl_3\overset{V}{P}=O$

Hence phosphorus and the other elements of that group must be considered quinquivalent, and the compounds into which these elements enter with a less valence, as unsaturated.—*Ber. Berl. Chem Ges.*, xviii, 2118, Sept., 1885. G. F. B.

6. *On the Measurement of the Resistance of Liquids.*—Two methods for measuring the resistance of liquids have been proposed, in both of which polarization is a minimum; one, the electrometer method suggested by Lippmann,* the other the alternating current method brought into use by Kohlrausch. Bouty and Foussereau have made comparative tests of these two methods for the purpose of determining their relative value. In the former, as modified by Foussereau,† the column of liquid is included in a closed circuit, containing also a battery and a known and adjustable metallic resistance. By means of a commutator, the terminals of a condenser may be alternately connected with two points in the liquid column or two in the metallic resistance, the difference of potential of these terminals—corresponding to the difference of potential of the points with which they are in contact—being determined by means of a Lippmann electrometer in the condenser circuit. By adjustment two points in the metallic circuit are found such that their difference of potential is exactly the same as that between the two points in the liquid. The resistance between the latter is then equal to that between the former, which is of course known. Bouty in employing this method uses a compensating battery in the condenser circuit, and has obtained very satisfactory results with it.‡ The experiments on the alternating current method, were made by means of a small Deprez generator revolving about 100 times per second. This current passed through a Wheatstone bridge, a sensitive telephone being used in place of the galvanometer. Large electrodes, 0·01 square meter each, were employed and the generator and resistances were carefully insulated. But it was found next to impossible to adjust the telephone to silence when metallic resistances formed the sides of the bridge; although the resistance coils used were by approved makers. With low values, a minimum sound could be distinguished; but as the resistance increased, this minimum sound became louder and less distinguishable. But, what is of more importance, when the bridge was balanced for sound, it was entirely unbalanced for the resistances; the error rising to 20 per cent even. This result is manifestly due to the fact that these resistance coils were not free from induction and the coefficient of self-induction with alternating currents became a serious matter. The sides of the bridge were then formed of liquid resistances. Three of these consisted of pairs of glass jars containing zinc sulphate solution and amalgamated zinc plates; a syphon between them regulating the resistance of each pair. The fourth consisted of a specially constructed liquid rheostat, made of two glass cylinders, one above the other, containing each a copper electrode of large surface immersed in copper sulphate solution. By means of a glass tube passing through the bottom of the upper jar, nearly to the bottom of the lower, communication is established between them. A glass rod passing through the tube serves to vary the liquid in the tube and so the resistance of the apparatus.

* C. R., lxxxiii, 192, 1876. † J. Phys., II, iv, 189, May, 1885.
‡ Ibid., II, i, 346, Aug., 1882, II, iii, 433, Aug., 1884.

At its upper end this rod carries an index moving over a graduated scale, calibrated by an electrometer. The resistance of this rheostat varies from 1200 to 5000 ohms. With these resistances in the bridge, the extinction of sound in the telephone was absolute. The liquids to be measured were placed on one side of the bridge, using platinum electrodes. With strong or only moderately weak solutions, an excellent balance could be obtained; but when these became more dilute, the error was considerable. With one thousandth solutions of magnesium chloride and potassium chloride, the difference between two consecutive measurements was 2½ per cent by this method; while with the electrometer method, the error was only one-third of one per cent. The authors believe, therefore, that for very dilute solutions, the electrometer method is preferable.—*J. Phys.*, II, iv, 419, Sept., 1885.

G. F. B.

7. *A method of precisely measuring the vibratory periods of tuning-forks.*—The third volume of the Memoirs of the National Academy of Sciences contains a paper by Professor A. M. Mayer embodying the results of a research recently carried on by him with funds from the Bache endowment. This research had as its object the elaboration of a method for measuring accurately the times of vibration of tuning-forks, and the determination of the laws of their vibrations with reference to the use of the tuning-fork as a chronoscope. The method employed was briefly to make a clock flash, at each second, a spark of induced electricity on a trace made by a style attached to the prong of the vibrating fork. To accomplish this the pendulum of the clock was armed with a triangular piece of platinum foil which each second cut through a globule of mercury contained in a small iron cup. To insure the best results, fresh mercury was taken with each experiment and the height of the mercury was adjusted by a screw collar in such a way as to make the globule as nearly as possible rigid and free from vibrations with each touch of the platinum point. The clock through this mercury connection was placed in the circuit of the primary coil of an inductorium, the current of which was given by a single voltaic cell. The tuning-fork, with one of its prongs armed with a light style of thin elastic copper foil, was screwed to a board with a hinge which with a screw-stop suitably placed allowed of its being inclined so that the style was just in contact with a smoked surface of paper wound on a rotating cylinder. The secondary circuit of the induction coil included the fork and cylinder. In the experiment the fork was raised on the hinge, set vibrating by a bow, and then depressed again, so that the style should write out its vibration on the smoked surface; at each second, as the platinum-pointed pendulum left the mercury, the primary circuit was completed and an induced current caused a spark from the point of the style, which made a single minute circular white spot on the blackened surface. The determination of the vibration-period of the fork is obviously given by counting the number of waves in the trace

and measuring the fraction of a wave with a microscope-micrometer. It was found to be essential to accuracy that the induction discharge should give a single spark only and that the spot made by it should bisect the trace of the fork. A series of experiments with discharges, obtained on a rapidly rotating surface of blackened paper with currents of various strengths, showed that the discharge is ordinarily complex, and consists of a shower of sparks producing a large number of spark-holes on the paper. The proper conditions to be fulfilled to give the single spark-hole with a given induction-coil can only be obtained by a series of experiments varying the strength of the primary current and the area of the condenser in the secondary. In the experiments described the primary coil was 150 feet in length, the secondary 8 miles, and a condenser of plates of glass with tin foil with 50 square inches of area were employed.

With the instrument which has been described a number of separate investigations were made. The first had to do with the question of the influence of varying amplitude on the time of vibration. With amplitudes varying in one case from 1·19^{mm} to 0·59, in another from 2·39 to 0·61, and a third from 2·07 to 0·78, no variation in vibration-period greater than ·05 of a vibration was noted. In a second series of experiments the effect of temperature was considered, and the result established with six Kœnig forks with Ut_2 and Ut_5 as extremes, that for all forks of the same steel and shape the effect of change of temperature was the same. A change of 1° F. produced a change of vibration-period of $\frac{1}{21561}$ part. In another series of experiments the law of the running down in the amplitude of a fork's vibration; and in another the numbers of vibrations per second of some European forks of various standards of pitch were determined. In the latter determinations the probable errors in one of the mean cases was estimated to be ±·0053 of a vibration; in another ±·004 of a vibration.

Professor Mayer discusses further the use of the apparatus described as a chronoscope, and gives the results of some experiments with it on the velocity of fowling-piece shot of various sizes with various charges of powder. The degree of uniformity of rate of rotation of the cylinder is shown to be immaterial, and further it is shown that no correction is needed for the weight of the tracing style nor for its scrape on the paper. With an A fork with 440 vibrations per second, it is stated that the number of vibrations can be determined by this method to at least $\frac{1}{100}$ of a vibration, and the time record consequently to $\frac{1}{44,000}$ of a second.

II. Geology and Natural History.

1. *Fourth Annual Report of the U. S. Geological Survey.* 1882–'83; by J. W. Powell, Director. 474 pp., royal octavo, Washington, 1884.—This fourth report of the U. S. Geological Survey, recently distributed, contains, like its predecessors, large

and important contributions to the science. After a review of the work in progress through the year, there are Reports on the Hawaiian Volcanoes, by Captain C. E. Dutton (pp. 81–212); abstract of a Report on the Mining Geology of the Eureka District, Nevada, by Joseph S. Curtis (pp. 225–251); Popular fallacies regarding the precious metal ore deposits, by Albert Williams, Jr. (pp. 253–271); A Review of the fossil Ostreidæ of North America, by Dr. C. A. White (pp. 281–316); a sketch of the Life History of the Oyster, by J. A. Ryder (pp. 315–334); A Geological reconnaissance in Southern Oregon, by Israel C. Russell (pp. 438–464). Captain Dutton's report is an elaborate memoir on the Geology of the Hawaiian Islands, and a very valuable contribution to the subject of volcanoes, prepared after a careful study of the region. To give any adequate abstract of the memoir would require the space of an article. All the reports are well illustrated by maps, sections, cuts, or plates.

2. *Geological Sketches of the Precious Metal deposits of the Western United States,* by S. F. Emmons and G. F. Becker. *With notes on Lead Smelting at Leadville.* 296 pp. 4to. From the 10th U. S. Census Report, vol. xiii. Washington, 1885.—This report is a popular, but not less a scientific, account of the great Western mining regions; and it is well adapted to its place in the Census report. It reviews the geological structure, mining deposits and mines for each of the States and territories in the west, giving sections of deposits, and tables mentioning the rocks and minerals of the various mines. The geologist and those interested in mines and the associations of ores, and in the sources of mining wealth in the country will find the report full of interest. The authors know well their subject, and describe the regions largely from personal study.

3. *Malesia: Plantæ Ospitatrici.*—In a former number (p. 245) we gave an abstract of Beccari's investigations and illustrations of those singular plants which, by an organic change of structure supply food and lodging to certain species of ants. The third part of the second volume of *Malesia* has now come to hand. It is wholly occupied with this subject, and illustrates it by 29 more plates of *Myrmecodia* and *Hydnophytum*, several of them showing the formicaries. Thirty species of *Hydnophytum* are here characterized, along with two more species of *Myrmecodia*. A general review is made of this group of *Rubiaceæ*, of the structure of the tubers, the formation of the galleries, their internal conformation, and of the ants that live in them, thus completing a monograph of the subject so far as these hospitable *Rubiaceæ* are concerned. A. G.

4. *Illustrationes Floræ Atlanticæ.*—The Flora of the French possessions in Northern Africa is making good progress, at least in the illustrations. The energetic Dr. Cosson brought out the first fasciculus of these fine imperial quarto plates, with their letter-press, in 1882. We have now received the second fasciculus, with the date of 1884. The plates (tab. 26 to 50) are all of

Cruciferæ, and the majority of the Brassicaceous tribe, of which the Mediterranean region is the special home. We have admirable illustrations of *Henophyton*, Coss. and Durieu (which would be *Oudneya*, so long obscure, except for a slip on the part of Robert Brown in citing a very different plant as a representative of it), *Reboudia* of the same authors, *Hemicrambe* of Webb, and *Cossonia* of Durieu, in three species, two of them here figured, all Brassicaceous types, along with curious species of *Sinapis*, *Erucaria*, *Enarthrocarpus*, etc. The remainder are siliculose genera, among them *Savignya* of the lamented Boisser. The figures were drawn by Cuisin, and are excellent. A. G.

5. *Physiological Botany.* I. *The Outline of the Histology of Phænogamous Plants.* II. *Vegetable Physiology;* by GEORGE LINCOLN GOODALE, A.M., M.D., Professor of Botany in Harvard University, being vol. II of *Gray's Botanical Text-Book.* New York and Chicago: Ivison, Blakeman, Taylor & Co. 1885. pp. 499.—Teachers of Botany especially will be glad to know that this long expected volume is at length provided to meet—as we trust it truly will—the demands of the higher instruction in their department. In making this announcement, it is not for us, at this time, to say more than this: that the volume, although moderate in size, is encyclopedic for the subject, is methodic and well-proportioned, is admirably illustrated, and will be thought to do credit to the series of which it forms a part. Thirty-five pages of Practical Exercises are bound up with the volume. This, and the Introduction of nearly as many pages on Histological Appliances, may give some idea of the pains that have been taken to make this book a *vade mecum* for the botanical laboratory. A. G.

6. *Rabenhorst's Kryptogamen-Flora von Deutschland, Oesterreich und der Schweiz. Vierter Band: Die Laubmoose*, von K. GUSTAV LIMPRICHT. Leipzig, E. Kummer. 1885.—This new edition, under the name of the late Dr. Rabenhorst's well known work, is truly a new one, of distinct volumes, by different authors, the *Pteridophyta* by Luerssen, the *Fungi* by DeBary, Rehm, and Winter, the Marine *Algæ* by Hauck. The latter is complete and has already been noticed here. Dr. Limpricht of Breslau now undertakes the Bryology; and two fascicles of his volume, of 128 pages, 8vo, are before us. More than half of these pages are devoted to structure and other general matters; but the second fascicle begins the Peat-Mosses, and carries them on to the twentieth species. The illustrations are in the letter-press and are excellent. A. G.

7. *On the Structure and Dehiscence of Anthers;* by LECLERC DU SABLON, (Ann. des sc. nat. bot., ser. 7, t. I, p. 97).—The author shows that the walls of the loculi of anthers differ as much in their histological characters as in their form. The cells of which the loculi are composed are very variously lignified. In some there is no appreciable deposit of lignin, but in others the amount is considerable and the distribution of the cells is characteristic.

It is known that under the influence of dryness, the lignified walls of cells contract less than do those which are unlignified. The unequal contraction of these different cells, as the anther approaches maturity, brings about a definite shrinking of the membrane in such a way as to break the loculus open in a determinate line of dehiscence. Some of the instances of adaptation adduced by the author are almost as striking as those which have long been known to occur in our dry fruits. G. L. G.

8. *Influence of strong sunlight on the vitality of Micrococcus.* M. Duclaux (Comptes Rendus, ci, p. 395).—Six forms of *Micrococcus* were placed under conditions most favorable to their rapid development in culture fluids, etc. In most cases exposure to sunlight for a few hours completely arrested all activity, and after fifteen to twenty hours all vitality was destroyed. G. L. G.

9. *On the Histology of Ascidia.*—Heckel and Jules Chareyre have reëxamined the internal face of the pitchers of some of the species of Sarracenia, Nepenthes, etc. They recognize the existence of four regions of peculiar structure, all of which, as previously shown by Hooker, are concerned in the entrapping and probable utilization of insects. In the last paper by these investigators they state that the bottom of the pitcher of *Cephalotus fascicularis* is lined with a membrane which has multitudes of *aquiferous stomata.* From these exudes the liquid which serves to dispose of the captured insects.—*Comptes Rendus*, Sept. 8, and 21. G. L. G.

10. *Reserve carbohydrates in Fungi.*—Leo Errara (Comptes Rendus, ci, p. 391), shows that the food-reservoirs known as sclerotia possessed by certain fungi, present a remarkable similarity in their general behavior during storing and use, to the food reservoirs of the higher plants. It has been found by him possible to detect in these reservoirs nearly all the forms of stored-food of the higher plants;—for instance, oil (in *Claviceps purpurea*), glycogen (in *Peziza sclerotiorum*), and cellulose-thickening (in *Pachyma cocos*). During the so-called germination of the resting parts, these reservoirs exhibit chemical changes strictly comparable to those already recognized in Phanerogams. G. L. G.

III. Miscellaneous Scientific Intelligence.

1. *Elements of Projective Geometry;* by Luigi Cremona. Translated by Charles Leudesdorf. Clarendon Press, Oxford, 1885.—Professor Sylvester and Professor Price have conferred a new favor upon mathematicians in inducing Professor Cremona, in connection with Mr. Leudesdorf, to issue an English edition of his Projective Geometry. The first edition of the original work was issued in 1872 and translations of it were very soon made in French and German. The present English translation is considerably enlarged and amended.

The work is not a descriptive geometry nor a treatise on perspective, but is an introduction to the methods of investigation now known under various names of Higher Geometry, Modern Geometry, Geometry of Position, etc. The knowledge required in a student of this work is that of elementary plane geometry, and the simplest elements of algebra. How little of the latter is needed is seen in the fact that ideal or imaginary points and lines, as well as the logical difficulties connected with them, are kept as much as possible in the background. The ideas of lines and points at infinity in the plane, of homology, of duality, of anharmonic ratios, of involution, of poles and polars, of polar reciprocal figures, and of foci, are developed in a manner to give to the student a clear apprehension of the several methods involved in these ideas and a power of using them. It is only a geometer of the highest rank who is capable of producing a first rate work of this character.

2. *National Academy of Sciences.*—A meeting of the National Academy was held at Albany, beginning with November 10, 1885. The following is a list of the papers entered to be read at the meeting. Those marked with an asterisk were read by invitation.

S. P. Langley: On obscure heat.

J. S. Billings: On a new form of craniaphore, for taking composite photographs.

A. S. Packard: On the Carboniferous Merostomatous fauna of America.

E. C. Pickering: On stellar photography.

E. D. Cope: On two new forms of Polyodont and Gonorhynchid fishes from the Eocene of the Rocky Mountains.

C. H. F. Peters: On certain stars observed by Flamsteed, and supposed to have disappeared.

James Hall: Remarks upon the International Geological Congress, with a brief historical notice of the origin of the congress.

James Hall: Notes on some points in the geology of the Mohawk Valley.

Simon Newcomb: When shall the astronomical day begin?

J. W. Powell: Remarks on the stone ruins of the Colorado and the Rio Grande.

A. Graham Bell: Preliminary report on the investigation relating to hereditary deafness.

C. A. Young: On the new star in the nebula of Andromeda.

C. H. F. Peters: On the errors of star catalogues.

T. H. Stafford: On the formation of a Polar catalogue of stars.

James Hall: Remarks upon the Lamellibranchiata fauna of the Devonian rocks of the State of New York, and the results of investigations made for the paleontology of the State.

*O. T. Sherman: On new lines in the spectra of certain stars.

*W. B. Dwight: Primordial rocks among the Wappinger Valley limestones, near Poughkeepsie, N. Y.

*J. A. Lintner: On recent progress in economic entomology.

*C. H. Peck: The New York State herbarium.

*Otto Meyer: On a Section through Southern Tertiaries.

OBITUARY.

Dr. William B. Carpenter, the English Physiologist, died on the 10th of November last at the age of seventy-two years.

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* This Index contains the general heads BOTANY, GEOLOGY, MINERALS, OBITUARY, ZOOLOGY, and under each the titles of Articles referring thereto are mentioned.

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OF

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NOTE.—The names of Minerals are inserted under the head of MINERALS; all Obituary notices are referred to only under OBITUARY. Under the heads BOTANY, GEOLOGY, ZOOLOGY the references to the topics in these departments are grouped together: in general, however, the same references appear elsewhere, at least under the author's name.

☞ In the references to volumes xxi to xxix, only the numerals i to ix are here given.

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*The paging 466, 467 is duplicated in vol. xxvii.

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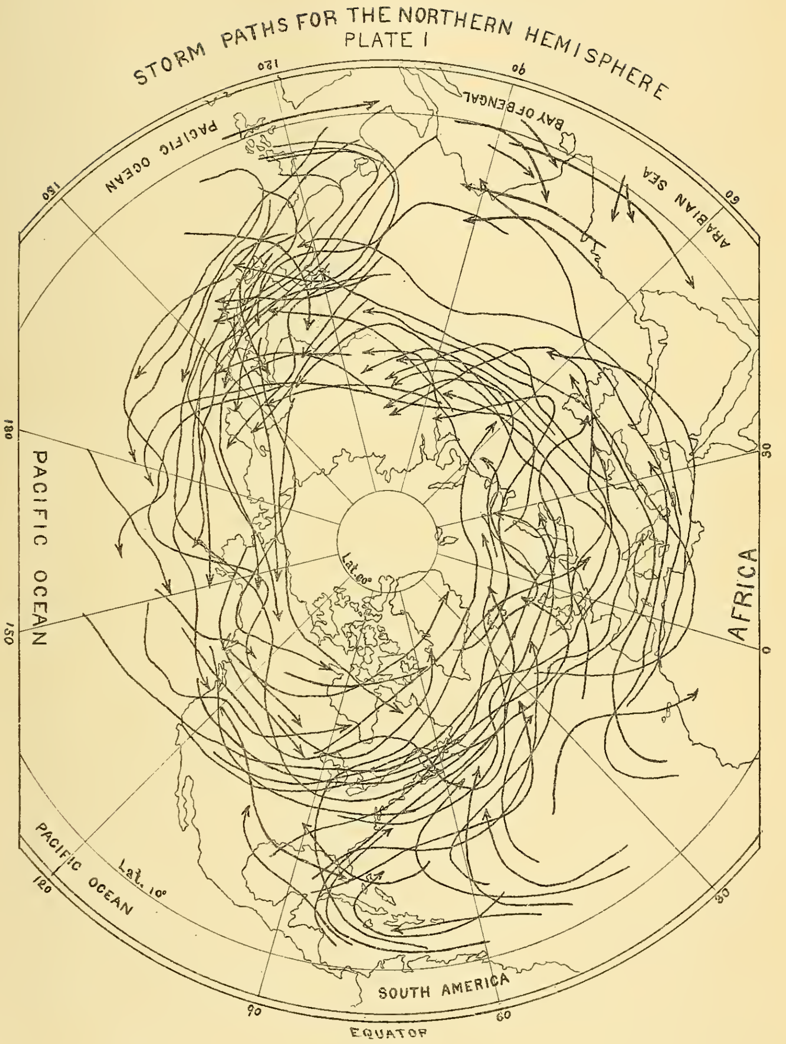

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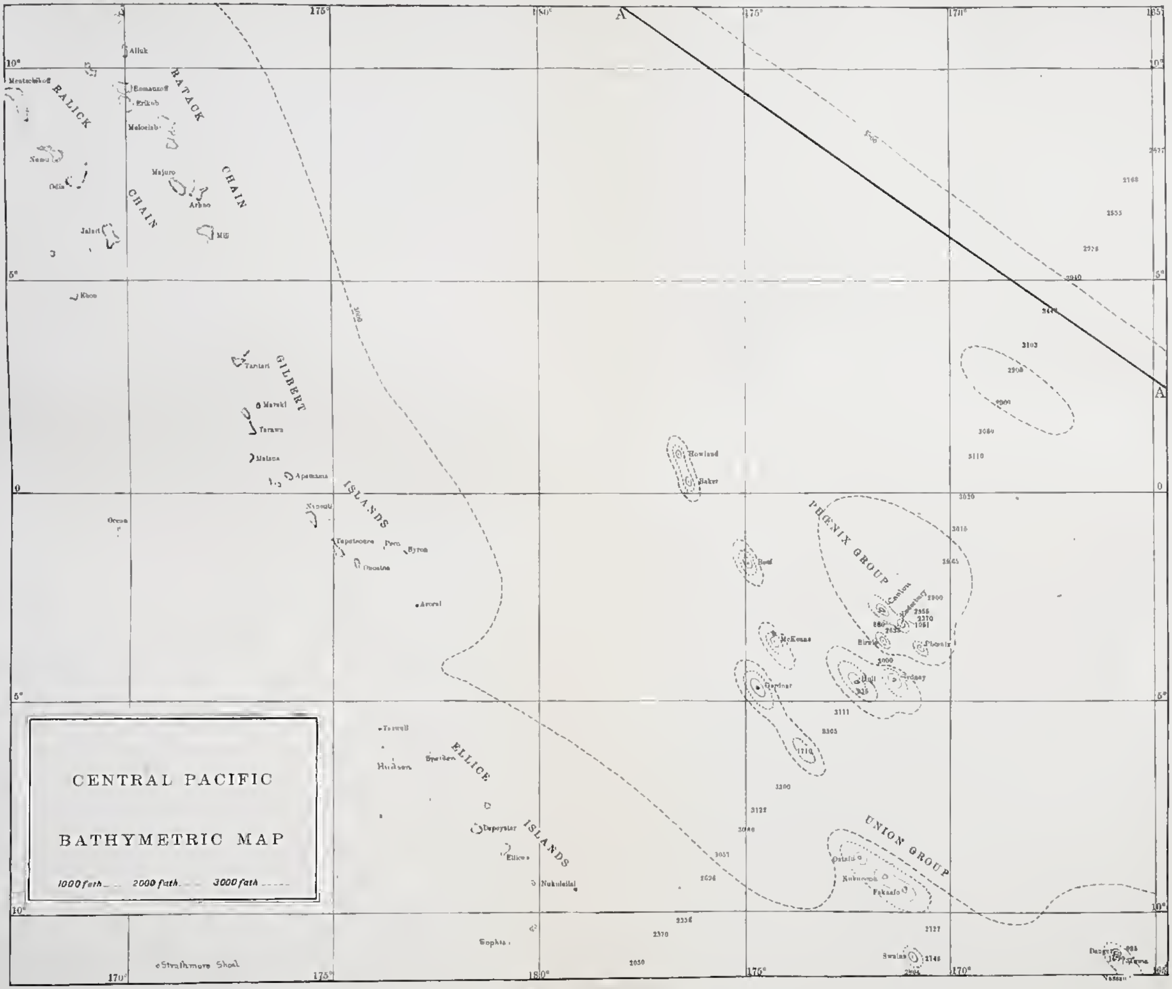
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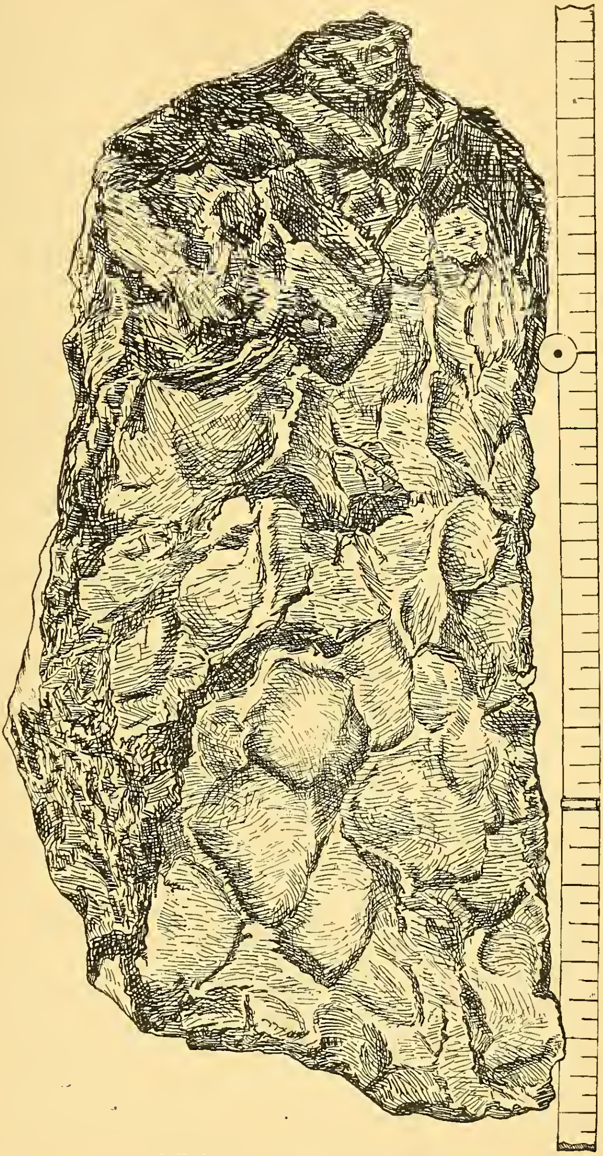

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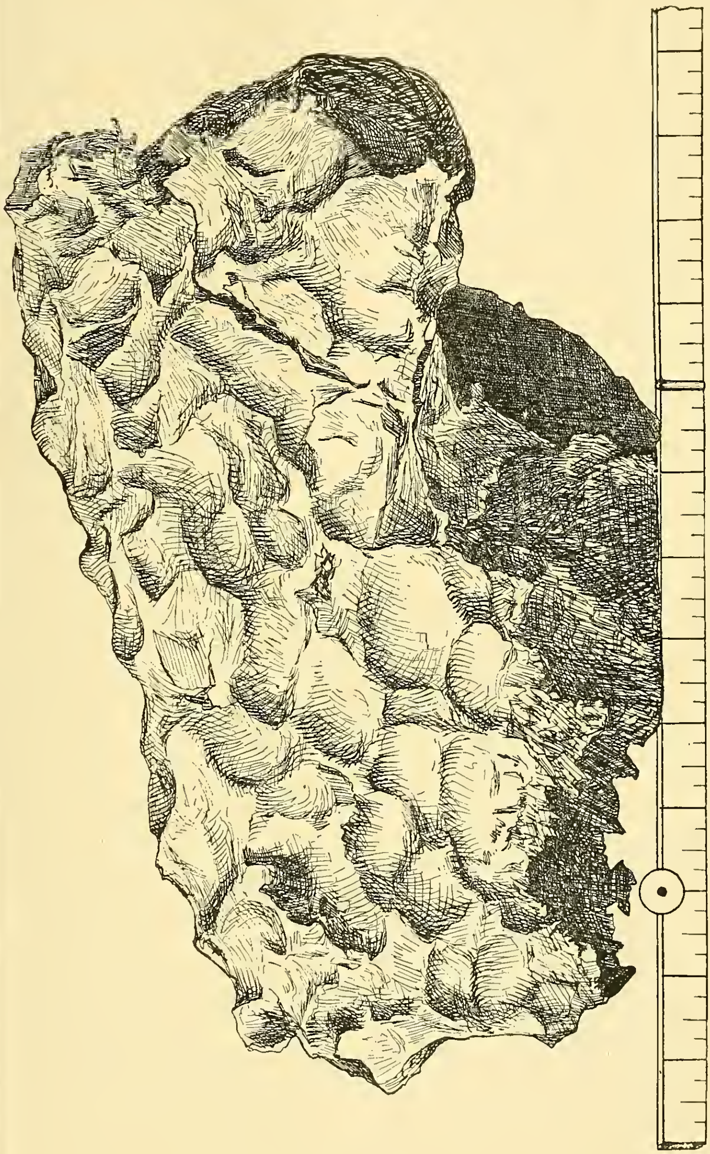

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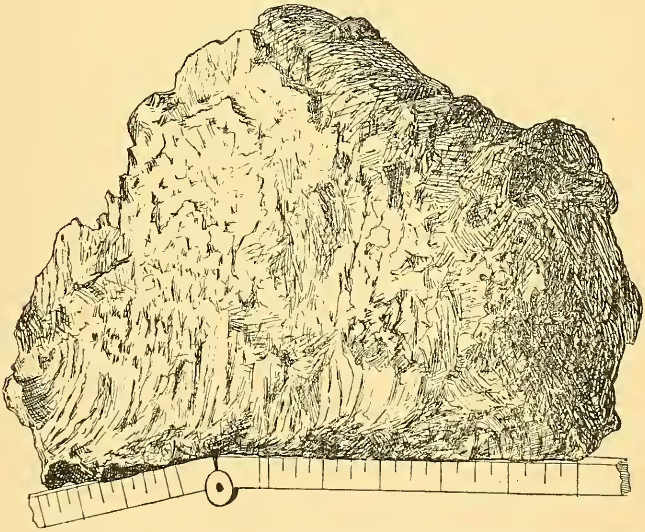

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TEN-VOLUME INDEX.

An *extra number* of this Journal, containing an index to volumes xxi to xxx, of sixty pages or more, will be ready in January. The publication of this number involves a large extra expense to the editors, and it will be sent, therefore, to those only who specially order it. The price is seventy-five cents per copy. Orders are solicited, as the edition will be a small one.