THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

EDITORS AND PROPRIETORS,

JAMES D. DANA, B. SILLIMAN, AND E. S. DANA.

ASSOCIATE EDITORS,

PROFESSORS ASA GRAY AND WOLCOTT GIBBS,
OF CAMBRIDGE,

PROFESSORS H. A. NEWTON, S. W. JOHNSON,
GEO. J. BRUSH AND A. E. VERRILL,
OF NEW HAVEN,

PROFESSOR EDWARD C. PICKERING, OF BOSTON.

THIRD SERIES.

VOL. XI.—[WHOLE NUMBER, CXI.]

JANUARY TO JUNE, 1876.

WITH THIRTEEN PLATES.

NEW HAVEN: EDITORS.

1876

ERRATA.

Page 195, 4th line from top, for southwest read *northwest.*
Page 401, lines 16, 22 and 24 from top, for glycogen acid read *glycogenic* acid.
Page 482, line 5 from top, for Powell read *Howell.*

TUTTLE, MOREHOUSE & TAYLOR, PRINTERS.

CONTENTS OF VOLUME XI.

NUMBER LXI.

NUMBER LXII.

SCIENTIFIC INTELLIGENCE.

NUMBER LXIII.

SCIENTIFIC INTELLIGENCE.

NUMBER LXIV.

SCIENTIFIC INTELLIGENCE.

NUMBER LXV.

SCIENTIFIC INTELLIGENCE.

NUMBER LXVI.

SCIENTIFIC INTELLIGENCE.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

Art. I.—*Contributions to Meteorology, being Results derived from an examination of the United States Weather Maps and from other sources*; by Elias Loomis, Professor of Natural Philosophy in Yale College. Fourth paper. With Plate.

[Read before the National Academy of Sciences, Philadelphia, Nov. 2, 1875.]

Movement of Areas of high barometer.

Having determined the average direction and velocity of movement of areas of low barometer within the limits of the United States, I desired to make a similar determination respecting areas of high barometer. As I have in my possession only one Weather Map for each day, I have frequently found it difficult to follow the course of areas of high barometer from day to day, and have therefore confined my comparison to the monthly charts published by the U. S. chief signal officer. Among these I found three charts which gave the tracks of areas of high barometer for a month each, viz: Aug., 1873, Dec., 1874, and Jan., 1875. The following are the average courses and velocities of the areas of high barometer for these months:

Months.	Course.	Velocity. Miles per hour.	High minus Low.	
			Course.	Velocity.
January, ----------	E. 2° N.	21·0	7° S.	−5·7
August, -----------	E. 41 S.	18·9	46 S.	+0·5
December, --------	E. 18 S.	28·5	25 S.	−0·8
Mean ----------	E. 12 S.	24·8	21 S.	−1·2

The lower line shows the averages for the three months, regard being had to the number of cases in each month. Comparing these results with those given in my last article for areas of low barometer (this Jour., vol. x, p. 5), we find that for each month the course of high barometer is more southerly than low barometer. These differences are shown in column fourth. Column fifth shows the differences in velocity for each month.

These observations indicate that while the average track of storms east of the Rocky Mountains, across the United States, is nine degrees to the north of east, areas of high barometer advance toward a point several degrees south of east, and with a velocity somewhat less than the former.

Monthly minima of temperature.

In a former article (this Jour., vol. ix, p. 7) I gave a table showing the lowest temperature observed at New Haven for each month during a period of three years, together with the height of the barometer, direction of the wind, and degree of cloudiness for the corresponding dates, and I expressed the opinion that these low temperatures are due in part to the descent of cold air within an area of high barometer. Inasmuch as some persons ascribe these low temperatures to a flow of air from a higher latitude, it has appeared to me that it would be instructive to study the same phenomenon at a locality where a current of air from a colder latitude is impossible; and such a locality must be found at the point of minimum temperature for the northern hemisphere. Now according to Dove's charts; during the winter months Jakutsk, in Siberia, lat. 62° 2′ N., is situated very near the center of greatest cold for the northern hemisphere. I have, therefore, sought for a complete meteorological journal at this station, and have found it in Middendorff's Sibirische Reise, Band I, pp. 28–49, extending from Sept., 1844, to June, 1846. The following table shows the results obtained for each month of this period.

Column first shows the date of the lowest temperature for each month; column second shows the lowest temperature for each month in degrees of Reaumur; column third shows the direction of the wind, and column fourth shows the force of the wind; column fifth shows whether the sky was clear or overcast; column sixth shows the height of the barometer expressed in Russian half lines; column seventh shows the mean height of the barometer for each month; column eighth shows how much the observed height of the barometer differed from the mean height expressed in English inches (one-half line = 0·05 inch English).

From this table it will be seen that the monthly minima of temperature are almost entirely independent of the direction of the wind. A northerly direction occurs 10 times; southerly, 6 times; easterly 7 times; and westerly, 5 times. In every instance (except one) these winds were faint, and generally *very faint.* In two-thirds of the cases the sky was entirely clear, and in only two cases was the sky entirely overcast. In three-fourths of the cases the barometer stood above its mean height.

Lowest temperature for each month at Jakutsk, Siberia, for the years 1844, 5 *and* 6.

Date of minimum.	Minimum temp'e.	Wind direction.	Wind force.	Face of sky.	Russian Half Lines. bar.	m'n bar	Differ. Eng. Inches.
1844, Sept. 30	−3°·1	N. W.	very faint.	overcast.	597·9	594·2	+0·19
Oct. 31	−26·4	Calm.		clear.	599·2	593·4	+0·29
Nov. 29	−40·1	N. N. W.	very faint.	clear.	601·5	596·5	+0·25
Dec. 18	−44·8	Calm.		partly clear.	595·1	601·2	−0·30
1845, Jan. 16	−44·4	S. S. W.	very faint.	clear.	596·6	597·9	−0·06
Feb. 6	−40·7	Calm.		clear.	601·3	599·1	+0·11
March 7	−31·9	N.	very faint.	clear.	601·2	594·9	+0·31
April 5	−20·5	S.	very faint.	clear.	601·9	592·4	+0·48
May 13	− 7·8	W.	strong.	overcast.	591·8	589·6	+0·11
June 9	− 1·3	E.	faint.	clear.	584·4	587·3	−0·14
July 30	+ 4·9	N. E.	very faint.	clear	593·5	587·6	+0·30
Aug. 21	− 0·7	S.	very faint.	clear.	589·6	588·7	+0·05
Sept. 26	− 5·3	Calm.		partly clear.	596·2	594·2	+0·10
Oct. 31	−25·5	N. E.	faint.	partly clear.	597·4	593·4	+0·20
Nov. 23	−39·8	N.	very faint.	clear.	603·9	596·5	+0·37
Dec. 25	−41·1	N. E.	very faint.	clear.	604·6	601·2	+0·17
1846, Jan. 10	−41·9	N. N. E.	faint.	partly clear.	595·9	597·9	−0·10
Feb. 2	−43·6	N. N. E.	faint.	partly clear.	596·1	599·1	−0·15
Mar. 14	−28·0	S. E.	faint.	clear.	599·5	594·9	+0·23
April 10	−16·8	S.	very faint.	clear.	596·0	592·4	+0·18
May 3	−17·4	N.	faint.	clear.	596·3	589·6	+0·33
June 9	+ 2·4	W. S. W.	faint.	mostly overcast.	582·4	587·3	−0·24

In all these particulars, except the direction of the wind, the phenomena at Jakutsk are quite similar to those observed at New Haven. So far as I have yet learned it is true universally that periods of unusual cold are generally accompanied by a barometer above the mean. Now it has been shown (this Jour., vol. ix, p. 2) that within areas of high barometer the motion of the air is *outward* from the center of this area, and therefore there must be a downward motion to supply the air flowing outward. In other words, it must be regarded as an observed fact that periods of unusual cold are generally accompanied by *a descent of air from the upper regions of the atmosphere.*

Influence of Winds on the temperature, moisture and pressure of the atmosphere.

In order to determine the influence of winds upon the temperature, etc., of the atmosphere, I selected the Meteorological observations made at Girard College, Philadelphia, from 1840 to 1845. A large sheet of paper was ruled with 16 vertical columns, and at the head of these columns were placed the letters N.; N.N.E.; N.E., etc., for the directions of the wind. I first took the observations for the three winter months, and beginning with Dec. 1, 1840, found that at the first observation the wind was N.W. The temperature at this observation was compared with the mean temperature of that month for the same hour, and the difference (with its algebraic sign) was placed in the column headed N.W. I proceeded in like manner with each succeeding observation during the winter months for the whole period of five years. The average of all the numbers in each column was taken and the results are shown in column second of the following table. These numbers clearly indicate that the coldest winds are from the neighborhood of the N.W., and the warmest winds from the neighborhood of the S.E. As, however, the progress of the numbers is somewhat irregular, I have taken the average of each successive three numbers in this column and placed the results in column third. These numbers show a remarkable regularity, indicating a minimum with a N.W. wind, and increasing thence uninterruptedly to a maximum with a S.E. wind, and thence decreasing uninterruptedly to the minimum. The entire range of the numbers in column third is 8·°46 Fahr., while the extreme range in column second is 10·°18.

Influence of winds on the temperature, moisture and pressure of the atmosphere.

Direction of wind.	Thermometer at Philadelphia. Fahrenheit.				Force of vapor at Philadelphia. Inches of mercury.				Barometer at Philadelphia. 29 inches plus.			
	Winter means.	Winter aver'd.	Sum'er means.	Sum'er aver'd.	Winter means.	Winter aver'd.	Sum'er means.	Sum'er aver'd.	Winter means.	Winter aver'd.	Sum'er means.	Sum'er aver'd
	°	°	°	°								
N.	−1·57	−1·53	−3·69	−3·23	−·007	−·005	−·096	−·085	1·039	1·005	·950	·933
N. N. E.	−0·45	−0·25	−2·78	−2·85	+·005	+·003	−·072	−·072	·986	1·013	·937	·956
N. E.	+1·28	+0·40	−2·07	−2·51	+·012	+·005	−·047	−·055	1·014	1·038	·980	·975
E. N. E.	+0·36	+1·00	−2·68	−2·21	−·003	+·011	−·046	−·040	1·113	1·033	1·008	1·001
E.	+1·37	+2·66	−1·87	−1·82	+·023	+·025	−·026	−·029	·973	1·018	1·014	1·018
E. S. E.	+6·26	+4·25	−0·90	−1·15	+·055	+·043	−·016	−·004	·967	·953	1·033	1·014
S. E.	+5·11	+5·94	−0·66	−0·24	+·051	+·060	+·031	+·023	·918	·913	·994	·978
S. S. E.	+6·46	+5·69	+0·83	+0·55	+·075	+·059	+·054	+·046	·857	·898	·906	·934
S.	+5·50	+5·00	+1·48	+1·68	+·052	+·054	+·052	+·057	·920	·889	·901	·917
S. S. W.	+3·05	+3·88	+2·72	+2·48	+·036	+·029	+·066	+·061	·891	·900	·943	·912
S. W.	+3·09	+2·47	+3·24	+2·51	+·029	+·029	+·066	+·056	·890	·885	·893	·906
W. S. W.	+1·26	+1·08	+1·57	+2·42	+·023	+·007	+·036	+·038	·874	·911	·882	·896
W.	−1·12	−1·19	+2·44	+1·60	−·032	−·016	+·012	+·005	·968	·923	·914	·881
W. N. W.	−3·72	−2·03	+0·80	+0·10	−·039	−·032	−·034	−·034	·926	·961	·846	·882
N. W.	−1·26	−2·52	−2·94	−1·79	−·025	−·025	−·081	−·067	·989	·968	·887	·882
N. N. W.	−2·57	−1·80	−3·22	−3·28	−·012	−·015	−·086	−·088	·989	1·006	·912	·916

I proceeded in a similar manner with the observations for the three summer months and the results are shown in column fourth. The average of each successive three numbers in this column was taken and the results are shown in column fifth.

In order to determine the influence of the winds on the moisture of the air I took the observations of the force of vapor (expressed in inches of mercury) and compared each observation with the mean force for that month, and the given hour. The results for the three winter months are shown in column sixth of the preceding table, and the averaged numbers are shown in column seventh. The results for the three summer months, obtained in like manner, are shown in columns eighth and ninth.

In order to determine the influence of the wind on the pressure of the air, I placed each observation of the barometer in the column having the observed wind at the top, and took the averages of the numbers in each column. The result for the three winter months (subtracting 29 inches) are shown in column tenth and the averaged numbers in column eleventh. Similar results for the three summer months are shown in columns twelve and thirteen.

On comparing the numbers in this table we see that in winter at Philadelphia the lowest temperature occurs with a wind from the N.W., and in summer with a wind from a point about 15° west of north.

Now if we suppose a mass of air to be transferred from a higher latitude to a lower, we should expect that its relative temperature would be the lowest when it moved in a direction perpendicular to the isothermal lines. Observing this rule we should conclude that in winter the coldest wind at Philadelphia must come from a quarter about 15° west of north, provided it commenced its motion from any place within 600 miles from Philadelphia. But if it came from a distance of over 1000 miles from Philadelphia then the coldest wind would come from a point 30° west of north. But the coldest wind actually comes from a point 45° west of north ; that is, at Philadelphia in winter the coldest wind blows from a point 15° more westerly than the coldest region about Philadelphia.

In summer, if we extend the comparison to a distance of 1000 miles from Philadelphia, we shall find the coldest region to lie in a N.E. direction ; but if we confine ourselves to a radius not exceeding 600 miles, we shall find nearly the same temperature prevailing in all directions between the limits of N.E. and N. 25° W. But the coldest wind is observed to blow from a point N. 15° W., which lies within the limits above determined. On the whole, we conclude that at Philadelphia the coldest wind comes very nearly from the coldest region within a distance of from 500 to 1000 miles from Philadelphia, with a suspicion, however, that the former is a few degrees more westerly than the latter.

In winter the warmest wind at Philadelphia is found to blow from a direction S. 40° E., while in summer it blows from the S.W. The former direction takes us to the Gulf Stream at about its nearest point, and at a distance of 250 miles. In summer the warmest region within 400 or 500 miles of Philadelphia lies in a direction S. 30° W., while the warmest wind blows from a point 15° more westerly. On the whole, the observations indicate that both the warmest and coldest winds at Philadelphia blow pretty nearly from the regions of greatest heat and cold, but there is reason to suspect that these directions are not quite coincident.

From the table of monthly minima of temperature at New Haven given in my former article, (this Jour., vol. ix, p. 7) it will be seen that the average monthly minimum is 25° below the mean temperature of the corresponding month. The table last given shows that a small portion of this effect (viz. 5°) may be ascribed to the influence of the direction of the wind, but there remains unexplained *four-fifths* of the whole effect which is to be ascribed to the influence of other causes.

The preceding table shows that both in summer and winter the force of vapor at Philadelphia is greatest with the same wind which brings the highest temperature; and it is lowest with the wind which brings the lowest temperature. The deviations from this rule are so small as to render it probable that the discrepancies would entirely disappear in the means of a long series of observations.

Since cold air has a greater density than warm air, and dry air has a greater density than vapor of water, it might be expected that the wind which brings the lowest temperature and the least vapor, would bring the highest pressure. We see. however, from the preceding table that such is not the case. In winter the highest pressure comes with a wind from the N.E., or perhaps N. 55° E.; while in summer the highest pressure comes with an east wind, which directions are distant more than 90° from the coldest quarter of the horizon. So, also, in winter, the lowest pressure comes with a S.W. wind, and in summer with a west wind, both of which directions are quite distant from the warmest quarter of the horizon. It seems probable that the excess of pressure which accompanies an easterly or N.E, wind is but the result of the high barometer which usually precedes a N.E. storm.

Diurnal inequality in the rain-fall.

In my former article (this Jour., vol. x, p. 3) I noticed a diurnal inequality in the progress of storms and was hence led to infer that there must be a diurnal inequality in the fall of rain. In searching for observations to test this conclusion, I found a decided diurnal inequality in the rain-fall at Philadelphia showing a maximum about 6 P. M. and a minimum at 3 A. M. The observations made by the United States Signal Service did not show any decided diurnal inequality, owing, perhaps, to their including only three daily observations. In a series of hourly observations made at seven stations in Great Britain I found evidence of two daily maxima and two daily minima.

Since the publication of my former article I have found in Kreil's Klimatologie von Böhmen the results of ten years' observations at Prag, lat. 50° 5′, which show a decided diurnal inequality, having a maximum about 4 P. M. and a minimum about 7 A. M., with a second maximum which is less distinctly marked. The following table shows the average annual rain-fall at Prag as deduced from observations from 1850 to 1859 expressed in Paris lines:

Rainfall at Prag, Austria.

Hour.	Rain-fall.	Hour.	Rain-fall.
Midnight.	6·864	Noon.	7·063
1 A. M.	6·507	1 P. M.	8·901
2	5·986	2	8·837
3	6·514	3	10·207
4	6·313	4	10·778
5	6·771	5	10·845
6	5·637	6	8·835
7	5·049	7	8·211
8	6·949	8	6·927
9	5·722	9	6·374
10	5·798	10	6·953
11	6·072	11	6·542

These numbers follow a law bearing a close resemblance to that of the Philadelphia observations, and lead us to presume that a similar law must prevail in the fall of rain over a considerable portion of the United States.

I have received the hourly observations of rain-fall at seven stations of Great Britain complete for the year 1874. At most of the stations there are evidently two periods of maximum rain-fall, but the time of maximum appears to depend very much upon local circumstances.

Comparison of storm paths in America and Europe.

In comparing the tracks followed by storms in America and Europe I have depended chiefly upon the following materials:

1. The daily United States Signal Service maps from 1871 to 1875, and especially the monthly maps showing the tracks of storm centers.

2. Atlas des mouvements generaux de l'atmosphere, redigé par l'observatoire Imperial de Paris, embracing 18 months, from June, 1864, to Dec. 1865.

3. Cartes synoptiques journalieres construites par N. Hoffmeyer, Copenhagen, embracing 9 months, from Dec. 1873 to Aug. 1874.

In order to determine what may be called the average track of storm centers in the United States, I ruled a large sheet of paper with several vertical columns headed 122°, 117°, 107°, etc., these numbers denoting degrees of longitude from Greenwich. I then took one of the monthly maps showing the tracks of storm centers, and following each of the tracks in succession determined in what latitude it crossed the meridians indicated at the top of the table, and the results were set down in the appropriate column. I proceeded in the same manner with each of the monthly maps and then took the average of all the numbers in each column. The results are shown in the first two columns of the following table; where column first shows the meridians of longitude from Greenwich, and column second shows the average latitude in which each of these meridians is intersected by the storm paths. The curve thus determined is traced on the accompanying chart and passes over the center of Lake Erie. It will be seen that the average direction of storm paths is not the same on all meridians. The directions given in my former article (see this Jour., vol. x, p. 1) must be understood to be the average direction of storm paths for the region covered by the United States observations; and this represents pretty nearly the mean direction for a place whose latitude is 42½° and longitude 83½° W., which is nearly the position of Detroit, Michigan.

Average direction of storm-paths.

U. S. Weather Maps.		Paris Maps.		Danish Maps.		Mean of Par. and Dan.	
Long. from Greenwi'h.	Latitude	Longitude from Paris.	Latitude.	Longitude from Paris.	Latitude.	Longitude from Paris.	Latitude.
122° W.	45·8°	60°–45° W.	46·2°	60°–50° W.	53·7°	55° W.	50·0°
117	46·9	45 –30	46·5	50 –40	59·5	45	52·9
107	44·7	30 –15	47·5	40 –30	61·8	35	54·2
97	42·2	15 – 0	53·0	30 –20	62·6	25	54·9
87	42·5	0 –15 E.	54·5	20 –10	61·5	15	55·6
77	42·6	15 –30	56·9	10 – 0	63·1	5 W.	58·3
67	45·2	30 –45	51·4	0 –10 E.	58·9	5 E.	56·7
62	45·9			10 –20	57·8	15	56·9
				20 –30	61·6	25	59·0
				30 –40	61·2	35	56·8
				40 –50	58·5	45	55·0
				50 –60 E.	55·3	55 E.	53·3

I proceeded in a somewhat similar manner with the Paris maps, but since these maps do not generally exhibit lines drawn so as to show the tracks of storms from day to day, I placed in one column the latitudes of all the storm centers between the meridians of 60° and 45° W. from Paris; in a second column I placed those between the meridians of 45° and 30° W., and so on for each 15° of longitude. I then took the average of all the latitudes in each of the columns. The result is shown in columns third and fourth of the preceding table, and the path thus determined is traced on the accompanying chart. This path passes over Dublin, and will be seen to form a natural continuation of the track deduced from the American observations. This result is doubtless accidental and is due to the fact that near the American coast the observations from which the Paris maps were constructed were derived from a region extending but little north of the stations of the United States Signal Service. If the Paris maps had included observations from Labrador and Greenland the average track of the storms represented would have been more northerly than it now is.

I proceeded in a similar manner with Hoffmeyer's charts except that the meridians were selected at intervals of 10°, and the results are shown in columns fifth and sixth. The path thus determined lies several degrees north of that previously determined, and this arises from the fact that the maps exhibit the results of observations made in Greenland and Iceland as well as from more southern latitudes. In order to deduce an average result from the French and Danish maps I have combined them in a single series, and the result is shown in columns seventh and eighth of the preceding table. The path thus determined is traced on the accompanying chart and passes through the northern extremity of Scotland.

In order to show the connection between storm paths and the mean height of the barometer, I have drawn upon the same chart two other barometric lines. The mean height of the barometer at the level of the sea varies with the latitude of the place. On the Atlantic ocean at the equator the mean height of the barometer is about thirty inches. If from this point we travel northward the pressure increases, and in latitude 30° becomes about 30·2 inches. Thence the pressure diminishes to 29·6 inches near latitude 70°, from which point the pressure slightly increases as we advance northward. A somewhat similar result takes place in going from the equator to the North Pole upon any meridian, but the maximum pressure is not the same under all meridians, and the same is true of the minimum pressure. The undulating line near the bottom of the accompanying chart shows the line of the greatest mean pressure varying on different meridians from about 30 inches to 30·2 inches. The undulating line near the top of the chart shows the line of the least mean pressure, being about 29·6 inches on the meridian of Greenwich and increasing somewhat as we proceed either east or west from that meridian. These lines are drawn chiefly from data collected by Alexander Buchan. (See Edinburgh Phil. Trans., vol. xxv.)

We perceive then that at all places near the southern margin of the chart the mean pressure of the atmosphere is greater than it is further northward, and this is generally sufficient to cause an average surface wind from south to north although the wind advances from a warmer to a colder region. On the other hand, at places near the northern margin of the chart the mean pressure of the atmosphere is somewhat greater than it is further south, and this force combined with a lower mean temperature causes a surface wind from north to south. Here then are permanent causes producing winds from opposite directions near the upper and lower portions of the chart, and these must be a permanent source of storms independent of those inequalities of pressure which arise from causes of a more local nature.

The average path of storms in their progress from America to Europe is apparently modified by the line of greatest mean pressure. This line has a more northerly position in Europe than it has in America, and this may be the reason why storm tracks generally bend northward in advancing from America to Europe. There are some minor particulars in which storm paths are apparently modified by the line of greatest mean pressure; but instead of attaching importance to coincidences which may prove to be accidental, it is more prudent to wait and see if these peculiarities are confirmed by further observations.

Oscillations of the barometer in different latitudes.

For the purpose of determining in what region of the globe the oscillations of the barometer are the greatest, I have prepared a table showing the mean monthly oscillation of the barometer at as many stations as possible in high northern latitudes. A few of the numbers in the following table are derived from Kaemtz Meteorology, edited by C. V. Walker, p. 297. The other numbers have been collected by myself from various sources which are indicated in the last column, and some of the results have required a careful discussion of many years' observations. Column fourth shows the average monthly range of the barometer for the three winter months, and column fifth shows the same for the three summer months expressed in English inches.

As some of these numbers depend upon observations of only one year, and therefore do not represent mean values very accurately, I have endeavored to combine them so as to obtain a few normal values. I combined all the observations north of latitude 70° in one general average, and all the observations between latitudes 60° and 70° in a second average. I then divided the observations of Kaemtz (Met., p. 298) into similar groups, each embracing ten degrees of latitude, viz., 60°—50°; 50°—40°, etc., and thus obtained the normal values shown in the table at the bottom of this page.

Mean monthly oscillation of the barometer for winter and summer.

Place.	Latitude.	Longitude.	Range of bar. Win'r	Range of bar. Sum'r	Authority.
	° ′	° ′			
Van Rensselaer Harbor,	78 37	70 53 W.	1·483	0·727	Kane's obs. reduced by Schott.
Northumberland Sound,	76 52	97 0 W.	1·116	0·728	Belcher Expedition, 1852–4.
Wellington Channel,	75 31	92 10 W.	1·130	0·587	" "
Melville Island,	74 47	110 48 W.	1·220	0·693	Parry's first voyage.
Upernivik, Greenland,	72 48	55 53 W.	1·451	0·697	Collectanea Met., 5 years obs.
Vardoe, Norway,	70 22	31 7 E.	1·776	0·937	Met. Iag. i Norge, 2 years obs.
Boothia Felix,	70 3	91 52 W.	1·231	0·899	Ross 2d Arctic Expedition, 2½ years.
Alten, Finmark,	69 58	23 2 E.	1·424	0·907	Br. Ass. Rep., 1848–50, 3 years.
Kaafiord, Finmark,	69 56	23 5 E.	1·496	0·901	Gaimard Met., v. 2, p. 451, 3 years.
Tromsoe, Norway,	69 39	18 58 E.	1·721	0·791	Met. Iag. i Norge, 1 year obs.
Jacobshaven, Greenland,	69 12	51 0 W.	1·496	0·906	Collectanea Met., 10 years obs.
Bodoe, Norway,	67 17	14 24 E.	1·913	0·921	Met. Iag. i Norge, 2 years obs.
Ft. Confidence, Br. N. A.,	66 54	118 49 W.	1·315	-- --	Athabasca obs., p. 355, 7 months.
Torneo, Finland,	65 51	24 14 E.	1·512	0·851	Kaemtz Met., p. 298.
Haparanda, Sweden,	65 51	24 11 E.	1·675	0·892	Met. Iakttagelser, 1859–69.
Godthaab, Greenland,	64 10	51 53 W.	1·409	0·685	Collectanea Met., 3 years obs.
Naes, Iceland,	64 9	22 0 W.	1·639	0·971	Observationes Met., 15 years obs.
Umea, Sweden,	63 50	20 18 E.	1·555	0·868	Kaemtz Met., p. 298.
Christiansund, Norway,	63 7	7 45 E.	1·642	0·929	Met. Iagttagelser i Norge, 9 years obs.
Hernosand, Sweden,	62 38	17 57 E.	1·644	0·896	Met. Iakttagelser, 1859–69.
Aalesund, Norway,	62 29	6 9 E.	1·658	0·949	Met. Iag. i Norge, 9 years obs.
Dovre, Norway,	62 5	9 7 E.	1·559	0·870	Met. Iag. i Norge, 5 years obs.
Jakoutsk, Siberia,	62 2	129 42 E.	1·012	0·803	Kaemtz Met., p. 298.
Fahlun, Sweden,	60 36	15 37 E.	1·640	0·835	Met. Iakttagelser, 1859–69.
Abo, Russia,	60 27	22 19 E.	1·465	0·778	Kaemtz Met., p. 298.
Bergen, Norway,	60 24	5 18 E.	1·654	0·904	Kaemtz Met., p. 298 & Met. Iag. i Norge.

Monthly oscillation of the barometer (normal values).

Latitude.	Winter inch.	Summer inch.	Winter comp.	Winter O – C.	Summer comp.	Summer O – C.
° ′						
6 12	0·110	0·106	0·125	–0·015	0·115	–0·009
15 40	0·198	0·164	0·234	– ·036	·169	– ·005
24 10	0·424	0·236	0·404	+ ·020	·255	– ·019
34 57	0·664	0·362	0·690	– ·026	·398	– ·036
46 2	1·068	0·556	1·029	+ ·039	·568	– ·012
54 13	1·315	0·744	1·280	+ ·035	·694	+ ·050
64 46	1·549	0·869	1·566	– ·017	·837	+ ·032
74 9	1·344	0·753	1·757	– ·413	·933	– ·180

The winter oscillations up to lat. 65° are pretty well represented by the formula:

$$W = 1{\cdot}890 \sin^2 l + 0{\cdot}104 \cos^2 l,$$

and the summer oscillations by the formula:

$$S = 1{\cdot}000 \sin^2 l + 0.104 \cos^2 l.$$

The differences between the observed and computed values are shown in columns fifth and seventh in the above table. These results indicate a steady increase in the mean monthly oscillation up to about lat. 65°, and from that point the oscillation diminishes as we proceed northward. The term $0{\cdot}104 \cos^2 l$ represents approximately the diurnal oscillation of the barometer, and the

Storms on the Atlantic Ocean by Maury's Charts.

Lat.	80°–75°	75°–70°	70°–65°	65°–60°	60°–55°	55°–50°	50°–45°	45°–40°	40°–35°	35°–30°	30°–25°	25°–20°	20°–15°	15°–10°	10°–5° W.	5°–0°
60°–55											60 16 27	102 38 37	123 35 28	117 31 27	78 12 15	30 3 10
55–50								150 57 38	420 111 26	510 140 27	694 169 24	850 159 19	932 117 12	1260 152 12	1393 133 10	583 46 8
50–45					126 11 9	288 28 10	919 121 13	1242 209 17	1570 369 24	1740 277 16	1627 242 15	1539 165 11	1478 160 11	1312 156 12	920 85 9	313 13 4
45–40		1820 126 7	3249 260 8	2544 266 11	2679 269 10	2419 241 10	1863 280 15	1581 234 15	1119 127 11	732 66 9	396 58 15	269 44 16	168 16 9	128 9 8	67 1 1	0
40–35	243 8 4	4193 607 14	2974 475 16	1797 393 22	1393 177 13	1100 115 10	773 62 8	480 28 6	349 27 8	242 23 9	341 35 10	268 5 2	302 24 8	340 7 2	334 9 3	225 14 6
35–30	1534 126 8	2265 231 10	1645 137 8	766 65 9	723 72 10	860 71 8	986 60 6	893 48 5	747 27 3	392 25 6	175 4 2	129 0 0	223 9 4	77 0 0	3 0 0	
30–25	1945 81 4	1393 30 2	1137 57 5	948 17 2	394 17 4	351 6 2	564 5 1	726 9 1	958 61 6	663 6 1	209 8 4	153 0 0	87 6 7			
25–20	316 12 4	380 9 2	262 4 2	650 7 1	637 6 1	267 6 2	262 9 3	452 18 4	806 4 0	664 25 4	338 0 0	136 0 0	15 0 0			
20–15	243 4 2	320 2 1	152 0 0	183 0 0	459 1 0	541 4 1	326 1 0	302 10 3	449 6 1	711 13 2	638 8 1	159 0 0	6 0 0			
15–10	65 0 0	0 0 0	53 1 2	53 0 0	96 1 1	387 0 0	594 1 0	508 0 0	415 3 1	667 9 1	835 21 3	622 0 0	225 0 0			
10–5					23 0 0	0 0 0	289 0 0	668 0 0	632 0 0	739 3 0	1667 5 0	1109 0 0	483 0 0	716 0 0	80 0 0	70 0 0
5–0						0 0 0	95 0 0	421 0 0	613 0 0	1004 0 0	2004 3 0	1262 0 0	335 0 0	107 0 0	362 1 0	233 0 0

other term of the formulas varies as the square of the sine of the latitude, and this law of increase holds pretty closely up to about latitude 65°. That part of the barometric oscillation represented by the first term of the formula is the effect of storms, and the oscillation diminishes within the Arctic circle.

These results seem to indicate that in the Northern Hemisphere, storms increase in frequency as we proceed northward as far as latitude 60° and perhaps somewhat farther. The same result is shown by Maury's storm chart of the North Atlantic. The preceding table presents a summary of the results of this chart. The ocean is divided into squares by parallels of latitude drawn at intervals of five degrees from each other, and meridians of longitude at intervals of five degrees Each square of the preceding table contains three numbers. The first shows the number of observations within the given square, each observation representing a period of eight hours. The second shows the number of gales reported, and the third is the average number of gales occurring in a hundred observations. Thus in the square included between the parallels of 40° and 45° of north latitude, and between the meridians of 45° and 50° west longitude from Greenwich, the first number is 1863, which shows the number of observations obtained in that square. The second number is 280, which denotes the number of gales reported; the third number is 15, which denotes that the number of gales was 15 per cent of the whole number of observations. An inspection of this table will show that on each meridian the frequency of gales increases with the latitude up to the highest latitude from which observations are reported.

Storms traced across the Atlantic Ocean.

When storms from the American continent enter upon the Atlantic Ocean they generally undergo important changes in a few days and are frequently merged in other storms which appear to originate over the ocean, so that we can seldom identify a storm in its course entirely across the Atlantic. The following are the only cases I have found on the French and Danish charts (embracing a period of 27 months) in which storms can be pretty distinctly traced across the Atlantic.

1. Nov. 30—Dec. 11, 1864. A storm traced from Newfoundland to Ireland.
2. April 20—May 3, 1865, traced from Labrador to Ireland.
3. May 26—29, 1865, from Gulf St. Lawrence to Ireland.
4. Oct. 2—10, 1865, from Cape Cod to Ireland.
5. Oct. 11–17, 1865, from Newfoundland to Ireland.
6. March 1—5, 1874, from Hudson Bay to North Cape.
7. April 14—17, 1874, from Hudson Bay to Norway.

8. April 16—23, 1874, from Gulf St. Lawrence to Norway.
9. May 23—30, 1874, from Gulf St. Lawrence to Norway.
10. Aug. 1—4, 1874, from Gulf of St. Lawrence to North Cape.
11. Aug. 12—17, 1874, from Hudson Bay to Norway.

If the observations each day were sufficiently numerous to show the isobaric curves for every part of the Atlantic Ocean, doubtless many more storms might be traced from America to Europe, but it is presumed that such cases do not occur on an average more than once or twice a month. The storms of Europe generally have their origin considerably east of the American Continent and soon become so violent as to draw within their influence any small barometric depression which started from America.

Velocity of Ocean Storms.

The average velocity of storms upon the Atlantic Ocean as deduced from 134 cases on the French maps is 19·3 miles per hour; the velocity deduced from 49 cases on the Danish maps is 20·3 miles per hour; giving an average of 19·6 miles per hour from both series of maps. The average velocity for the storms of the United States as deduced from 485 cases is 26 miles per hour. From a considerable number of cases in Europe, Prof. Mohn has deduced an average velocity of 26 7 miles per hour. These numbers indicate that storms travel with less velocity over the Atlantic Ocean than they do over the Continents of America and Europe; and it seems to follow that the progressive movement of a storm is not the result of a *simple drifting* of the atmosphere; for it seems probable that the average progress of the atmosphere in an easterly direction is as rapid over the Atlantic Ocean as it is over North America.

Storms of Jan. 29—*Feb.* 8, 1870, *on the Atlantic Ocean.*

A succession of storms of unusual severity passed over the Atlantic, between Jan. 29 and Feb. 8, 1870, an account of which has been published by Capt. Henry Toynbee, of the London Meteorological office. On the 30th of January an area of low barometer prevailed near Nova Scotia; on the 31st it was east of Newfoundland; and on the 1st of February it was merged in another storm which had prevailed for several days on its eastern side. On the 2d of February a second storm center appeared near Newfoundland; on the 3rd it had advanced east about 700 miles; and on the 4th it became merged in another storm off the Irish coast.

On the morning of the 5th a third storm appeared near the center of the Atlantic, which must have developed with unusual rapidity, since on the preceding day, observations had indicated no great atmospheric disturbance in that neighborhood.

The isobar of 29 inches is shown on the accompanying chart. On the afternoon of the same day, this storm blended with another storm on its eastern side, and there resulted one of the most violent hurricanes ever experienced on the Atlantic Ocean. At 6 P. M. the barometer fell to 27·33 inches, which Capt. Toynbee pronounces the lowest ever observed on this part of the Atlantic. The accompanying chart represents the isobar of 29 inches on the morning of the 6th, when the diameter of this curve was over 1000 miles, and the diameter of the isobar of 30 inches was over 2000 miles. During the next two days the storm advanced slowly towards the southeast, and its severity was much diminished. The accompanying chart shows the isobar 29·5 inches on the morning of Feb. 8th. During this interval of three days the center of the storm had moved only about 900 miles, showing an average velocity of about 12 miles per hour.

Application of Ferrel's formula.

In vol. viii of this Journal, p. 343, Prof. Ferrel has given a formula which enables us to compute the depression of the barometer resulting from a violent storm. If we divide the denominator of this formula by the number of inches in a mile, and suppose the wind to move in a circle, the formula becomes

$$G = \frac{v \sin l}{250} + \frac{v^2}{131r},$$

where G is expressed in inches, but v and r are expressed in miles. I have applied this formula to the storm of Feb. 5th, 1870, and the results are shown in the following table. Column first shows the isobars which have been selected as the basis of comparison; column second shows the radius of each isobar as nearly as can be determined from the observations of Capt. Toynbee's memoir; and column third shows the velocity of the wind in miles on each of these isobars. These velocities were obtained by taking the mean of the various observations corresponding to the barometric heights given in column first. These velocities were recorded in the numbers of Beaufort's scale (0–12) and were reduced to miles by the table in Scott's Met. Instruments, p. 58. Column fourth shows the gradient to 100 miles computed by the above formula, for points midway between the several isobars selected. If this gradient be supposed to be maintained for a distance equal to the distance between the isobars, it will show a change of barometric pressure about the same as that actually observed. For the inner circle, the computed gradient will represent the observed depression of the barometer if we suppose that near the center of the storm there was a considerable mass of air revolving with a diminished velocity.

Ex. 1.—*Storm of Feb.* 5, 1870, *Atlantic Ocean, lat.* 51° 3′ *N.*

Barometer. Inches.	Radius of Isobar. Miles.	Velocity, miles.	Gradient to 100 miles.
27·33	0	90	
28·00	60	66	1·79
28·50	200	59	·42
29·00	400	52	·25
29·50	680	45	·18
30.00	1020	38	·15

I have made a similar application of the formula to two violent cyclones of recent occurrence on the coast of the United States, and the results are shown below. In the Punta Rassa cyclone the assumed velocities 90 and 70 miles agree pretty well with the velocities actually observed; the velocities 50 and 35 miles are somewhat greater than the observations at the surface of the earth, but may be presumed to have been the velocities at a little elevation above the earth's surface. The velocities assumed for the Indianola cyclone are the velocities actually observed or estimated at Indianola.

Ex. 2.—*Storm of Oct.* 6, 1873, *Punta Rassa, lat.* 27° 0′.

Barometer.	Radius of Isobar.	Velocity.	Gradient to 100 miles.
28·40	0	90	
29·00	50	70	2·10
29·50	200	50	·33
30·00	650	35	·11

Ex. 3.—*Storm of Sept.* 16, 1875, *Indianola, lat.* 28° 31′.

Barometer.	Radius of Isobar.	Velocity.	Gradient to 100 miles.
28·90	0	90	
29·50	100	60	1·00
30.00	500	35	·15

The following is an example of a great inland storm of unusual severity. Column third shows the *greatest* velocity of the wind observed at any station near the corresponding isobars in column first, and column fourth shows the velocity assumed in computing the gradients in column fifth.

Ex. 4.—*Storm of Nov.* 18, 1873, *New England, lat.* 41°

Barometer inches.	Radius of Isobar. Miles.	Velocity of wind.		Gradient to 100 miles.
		Observed.	Assumed.	
28·60	100	50	56	
29·00	300	57	48	·24
29·50	700	27	40	·15
30·00	1200	28	30	·10

Stationary Storms.

When a storm center has crossed the United States and passed to Nova Scotia or Newfoundland, we often find on the United States Weather Maps for two or three subsequent days the word *low* on the northeast corner of the maps, seeming to indicate that the center of the storm remained during that period nearly stationary. The Danish maps (from Dec., 1873, to Aug., 1874,) show us that storms do sometimes remain nearly stationary for several days.

Case I. From the 5th to the 8th of March, 1874, a violent storm moved from New Mexico to the St. Lawrence valley. On the 9th the center of this storm was a little north of Halifax; on the 10th it was still near the same place; on the 11th it had moved northeast nearly 400 miles; on the 12th it had moved south about 200 miles; on the 13th it had moved north about 200 miles; on the 14th it had moved south about 200 miles; and on the 15th it moved northeast about 700 miles. Thus during five days (March 9–14) the center of the storm had advanced less than 350 miles, being an average motion of less than three miles an hour, and during the first four days the barometric depression was greater than it was on the 8th.

Case II. From April 26th to 30th, 1874, a storm moved across the United States from Colorado to the St. Lawrence valley. During the next day (May 1st) the storm was stationary; on the 2d it moved a little to the southeast; on the 3d it moved a little to the east; and on the 4th it reached St. Johns, Newfoundland. Thus in four days the center moved 775 miles, being an average rate of about eight miles an hour; and during the first half of this time the average movement scarcely exceeded four miles an hour.

In preparing the materials for this article, I have been assisted by Mr. Edward S. Cowles, a graduate of Yale College of the class of 1873.

ART. II.—*Studies on Magnetic Distribution*; by HENRY A. ROWLAND, of the Johns Hopkins University, Baltimore.

(Continued from page 458 of the last volume.)

V.

LET us now consider the case of that portion of the bar which is covered by the helix. First of all, when the helix is symmetrically placed on the rod, equations (5) and (6) will apply. As Q''_ε is the quantity which is usually taken to represent the distribution of magnetism, being nearly proportional to the "surface-density" of magnetism, I shall principally discuss it.

In the first place, then, this equation shows that the distribution of magnetism in a very elongated electro-magnet, and indeed of a steel magnet, does not change when pieces of soft iron bars of the same diameter as the magnet are placed against the poles, *provided that equal pieces are applied to both ends;* otherwise there is a change. This result would be modified by taking into account the variation of the permeability, &c.

Let us first consider the case where the rod projects out of the end of the helix, as in Tables V, VI and VII. By giving proper values to the constants we obtain the results given in the last columns of the table. The agreement with observation is in most cases very perfect. We also see the same variation of r that we before noticed in the rest of the curves, and we see that it is in just the direction theory would indicate from the change of μ.

In these tables we come to a very important subject, and one to which I called attention some years back, namely, the *change in the distribution when the magnetizing-force varies, and which is due to change of permeability.* The following tables and figures show this extremely well, and are from very long rods with a helix a foot long at their center, as in the last three tables. The bar in both these tables was ·19 inch in diameter and 5 feet long. The zero-point was at the center of the bar and of the helix. The tables give values of Q'_ε for the magnetizing-forces which appear at the head of each column, and which are the tangents of the angles of deflection of the needles of a tangent-galvanometer. Table VIII. only gives the part covered by the helix. Both tables are from the mean of both ends of the bar.

TABLE VIII.

x.	Strength of magnetizing current.			
	·108.	·194.	·378.	·600.
0–1	2·7 (0–2)	3·2 (0–2)	·7	·6
1–2			·9	·6
2–3	2·4	2·7	·9	·8
3–4	3·3	3·9	1·7	·8
4–5	4·0	6·0	4·0	3·2
5–6	5·7	8·7	9·3	14·7

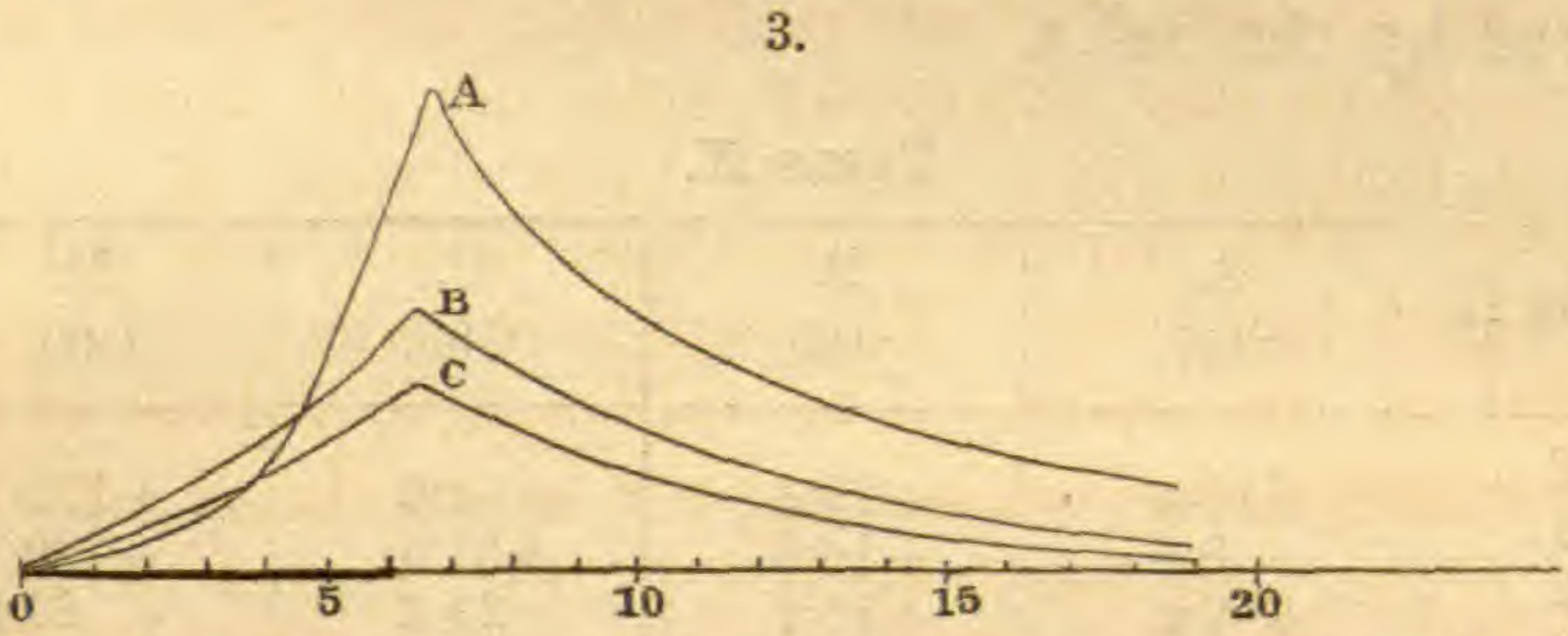
These experiments show in the most positive manner the effect we are considering, and we are impressed by them with the great complication introduced into magnetic distribution by the variable character of magnetic permeability.

TABLE IX.

x.	C. ·257.	B. ·363.	A. 1·303.
0			
1			1·1
0–2 }	2·5	3·1	
2			1·3
3		4·1	2·1
2–4 }	7·2		
4		5·9	4·0
5	6·1	8·2	9·6
6	7·7	10·9	18·6
7	7·9	11·5	21·3
8	6·5	9·0	16·8
10	10·0	15·0	27·4
12	6·2	10·9	20·9
15	5·0	9·8	21·5
18	2·0	4·7	14·8
30	2·0	3·6	16·5

In fig. 3 I have represented the distribution on half the bar as given in Table IX, the other half being of course similar. Here the greatest change is observed in the part covered by the helix, though there is also a great change in the other part.

3.



Plot of Table IX, showing surface-density for different values of the magnetizing-force.

These tables show that, as the magnetization of the bar increases, *at least beyond a certain point*, the curves on the part covered by the helix increase in steepness; and the figure even shows that near the middle of the helix an *increase of magnetizing-force may cause the surface-density to decrease;* and Table VIII. shows this even better. Should we calculate Q″, however, we should always find it to increase with the magnetizing-force in all cases. These effects can be shown also in the case where the bar does not extend beyond the helix, but not nearly so well as in this case, seeing that here Q″ can obtain a greater value.

Assuming that μ is variable, the formula indicates the same change that we observe; for as Q″ increases from zero upward, μ will first increase and then decrease; so that as we increase the magnetizing-force from zero upward, the curve should first decrease in steepness and then increase indefinitely in steepness.

In these tables the decrease of steepness is not very apparent, because the magnetization is always too great, and indeed on this account it is difficult to show it; but in Tables V, VI and VII. this action is shown to some extent by the values of r in the formulæ. The change of distribution with the helix arranged in this way at the center of the bar is greater than in almost every other case, because the magnetism of the bar Q″ can change greatly throughout the whole length of the helix, and thus the value of r be changed, and so the distribution become different.

The next case of distribution which I shall consider is that of a very long rod having a helix wound closely around it for some distance at one end.

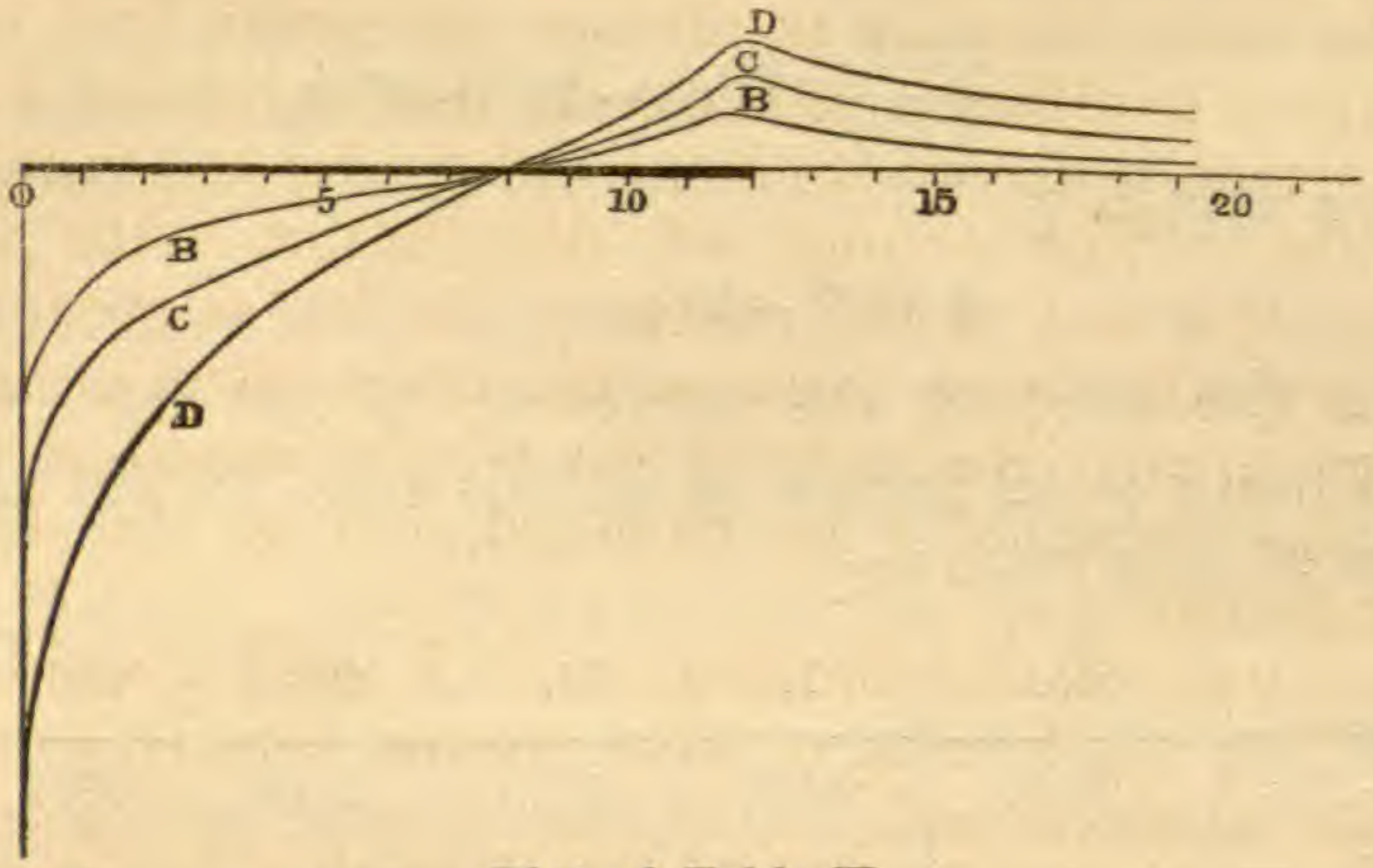
Table X. is from a bar 9 feet long with a helix wound for one foot along one end. The bar was ·25 inch in diameter. All except the first column is the sum of two results with the current in opposite directions, and after letting the bar stand for some time, as indeed was done in nearly every case. The first column contains twice the quantities observed, so as to compare with the others. The zero-point was at the end of the bar covered by the helix.

Table X.

x and L.	A. ·245.	B. ·360.	C. ·600.	D. 1·09.
0	+17·6	+29·4	+52·0	+108·7
1	+ 9·6	+16·8	+31·5	+ 60·1
2	+ 7·4	+13·1	+24·3	+ 45·8
3	+ 5·4	+ 9·8	+19·1	+ 34·1
4	+ 3·4	+ 7·2	+14·7	+ 22·8
5	+ 2·0	+ 4·6	+ 9·9	+ 16·0
6	+ 0·6	+ 2·4	+ 5·4	+ 9·6
7	− ·8	+ ·3	+ 1·2	+ ·6
8	− 1·8	− 1·6	− 2·1	− ·3
9	− 3·0	− 3·6	− 6·6	− 8·8
10	− 5·0	− 6·3	− 8·6	− 15·6
11	− 7·4	−10·0	−16·4	− 27·1
12	− 8·4	−10·0	−16·9	− 26·5
13	− 6·0	− 7·9	−14·5	− 22·6
14	− 5·2	− 7·0	−12·5	− 21·0
15	-----	− 5·3	−11·9	− 19·0
16	-----	− 9·4	−19·1	− 31·2
18	-----	− 5·3	−15·2	
20	-----	− 6·5	−19·3	
24	-----	− 5·6	− 6·0	
36	-----	− ·7	− 1·2	
48				

The value of Q''_{ε} between 0 and 1 includes the lines of force passing out at the end of the bar, and is therefore too large.

4.



Plot of Table X.

In fig. 4 we have a plot of the results found for this bar. The curves are such as we should expect from our theory except for the variations introduced by the causes which we have hitherto considered. Thus the sharp rise in the curve when near the end of the bar has already been explained in connection with Table III. A small portion of it, however, is due to those lines of induction which pass out through the end section of the bar, and in future experiments these should be estimated and allowed for. When considering surface-density we should also allow for the direct action of the helix, though this is always found too small except in very accurate experiments.

To estimate the shape of the curve theoretically in this case, let us take equation (4) once more, and in it make $s'=\infty$ and $s''=\sqrt{RR'}$ which will make it apply to this. We shall then have $A'=-1$, and $A''=\infty$.

Whence for the positive part of Q''_ε we have

$$Q''_\varepsilon=-\frac{\mathfrak{H}\Delta L}{2R'r}\{-1+2\varepsilon^{r(x-b)}-\varepsilon^{2r(x-b)}\}=\frac{\mathfrak{H}\Delta L}{2R'r}\{1-\varepsilon^{r(x-b)}\}^2;$$

and for the negative part

$$Q''_\varepsilon=-\frac{\mathfrak{H}\Delta L}{2R'r}(1+\varepsilon^{2r(x-b)})(1-\varepsilon^{-rx});$$

therefore the real value is

$$Q''_\varepsilon=-\frac{\mathfrak{H}\Delta L}{2R'r}(\varepsilon^{r(x-b)}(\varepsilon^{-rb}-2)+\varepsilon^{-rx}).$$

And if x is reckoned from the end of the rod, we have

$$Q''_\varepsilon=\frac{\mathfrak{H}\Delta L}{2R'r}\varepsilon^{-r(b+x)}\{1-2\varepsilon^{rb}+\varepsilon^{2rx}\}. \quad . \quad . \quad . \quad . \qquad (10)$$

When $x=0$, this becomes

$$\frac{\mathfrak{H}\Delta L}{2R'r}\varepsilon^{-rb}(2-2\varepsilon^{rb});$$

and when $x=b$, it becomes

$$\frac{\mathfrak{H}\Delta L}{2R'r}\varepsilon^{-2rb}(1-2\varepsilon^{rb}+\varepsilon^{2rb}),$$

the ratio of which is

$$\tfrac{1}{2}(\varepsilon^{-rb}-1);$$

and this is the ratio of the values of Q''_ε at the ends of the helix. When b is 12 inches, as in this case, we get the following values of this ratio:—

$r=$	·05.	·1.	·15.	·20.	·30.	∞.
$-\frac{1}{2}(\varepsilon^{-rb}-1)=$	·2256	·3494	·4173	·4546	·4863	·500
$\frac{-2}{\varepsilon^{-rb}-1}=$	4·43	2·86	2·40	2·20	2·06	2·00

To compare this with our experiments, let us plot Table X. once more, rejecting, however, the end observations and completing the curve by the eye, thus getting rid of the error introduced at this point. We then find for this ratio, according to the different curves,

B.	C.	D.
2·1	2·3	3·2

It is seen that these are all above the limit 2, as they should be, though it is possible that it may fall below in some cases owing to the variation of the permeability. As the magnetization increases, the values of the above ratio show that r decreases, as we should expect it to do from the variation of μ.

To find the neutral point in this case, we must have in formula (10)

$$\varepsilon^{2rx}=2\varepsilon^{rb}-1,$$

where x is the distance of the neutral point from the end. Making $b=12$, we have from this

$r=$	·05.	·10.	·15.	·20.	·30.	∞.
$x=$	10·1	8·96	8·31	7·89	7·39	6·00

By experiment we find that the neutral point is, in all the cases we have given in Table X, between 7·5 and 8·1 inches, which are quite near the points indicated by theory for the proper values of r, though we might expect curve D to pass through the point $x=9$, except for the disturbing causes we have all along considered.

Our formulæ, then, express the general facts of the distribution in this case with considerable accuracy.

These experiments and calculations show the change in distribution in an electro-magnet when we place a piece of iron against *one pole only.* In an ordinary straight electro-magnet the neutral point is at the center. When a paramagnetic substance is placed against or near one end, the neutral point moves toward it; but if the substance is diamagnetic it moves from it.

The same thing will happen, though in a less degree, in the case of a steel magnet, so that its neutral point depends on external conditions as well as on internal.

We now come to practically the most interesting case of distribution, namely, that of a straight bar magnetized longitudinally either by a helix around it, or by placing it in a magnetic field parallel to the lines of force; we shall also see that this is the case of a steel magnet magnetized permanently. This case is the one considered by Biot (Traité de Phys., tome iii, p. 77) and Green (Mathematical Papers of the late George Green, p. 111, or Maxwell's "Treatise," art. 439), though they apply their formulæ more particularly to the case of steel magnets. Biot obtained his formula from the analogy of the magnet to a Zamboni pile or a tourmaline electrified by heat. Green obtained his for the case of a very long rod placed in a magnetic field parallel to the lines of force, and, in obtaining it, used a series of mathematical approximations whose physical meaning it is almost impossible to follow. Prof. Maxwell has criticised his method in the following terms ("Treatise," art. 439):—"Though some of the steps of this investigation are not rigorous, it is probable that the result represents roughly the actual magnetization in this most important case." From the theory which I have given in the first part of this paper we can deduce the physical meaning of Green's approximation, and these are included in the hypotheses there given, seeing that when my formula is applied to the special case considered by Green, it agrees with it where the permeability of the material is great. My formula is, however, far more general than Green's.

It is to Green that we owe the important remark that the distribution in a steel magnet may be nearly represented by the same formula that applies to electro-magnets.

As Green uses what is known as the surface-density of magnetization, let us first see how this quantity compares with those I have used.

Suppose that a long thin steel wire is so magnetized in the direction of its length that when broken up the pieces will have the same magnetic moment. While the rod is together, if we calculate its effect on exterior bodies, we shall see that the ends are the only portions which seem to act. Hence we may mathematically consider the whole action of the rod to be due to the distribution of an *imaginary* magnetic fluid over the ends of the rod. As any case of magnetism can be represented by a proper combination of these rods, we see that all cases of this sort can be calculated on the supposition of there being two magnetic fluids distributed over the surfaces of the bodies, a unit quantity of which will repel another unit of like nature at a unit's distance with a unit of force. The surface-density at any point will then be the quantity of this fluid on a unit-surface at the given point, and the linear density along a rod will be the quantity along a unit of length, supposing the density the same as at the given point.

Where we use induced currents to measure magnetism we measure the number of lines of force, or rather induction, cut by the wire, and the natural unit used is the number of lines of a unit-field which will pass through a unit-surface placed perpendicular to the lines of force. The unit-pole produces a unit-field at a unit's distance; hence the number of lines of force coming from the unit-pole is 4π, and the linear density is

$$\lambda = \frac{Q_\varepsilon}{4\pi \Delta L}, \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

and the surface-density

$$\delta = \frac{Q_\varepsilon}{4\pi^2 d \Delta L}. \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

These really apply only to steel magnets; but as in the case of electro-magnets the action of the helix is very small compared with that of the iron, especially when it is very long and the iron soft,* we can apply these to the cases we consider.

Transforming Green's into my notation, it gives

$$\lambda = \left(\frac{\pi d^2}{4}\right) \mathfrak{H} \varkappa r \frac{\varepsilon^{r(b-x)} - \varepsilon^{rx}}{1+\varepsilon^{rb}}, \quad . \quad . \quad . \qquad (13)$$

in which $\varkappa$ is Neumann's coefficient of magnetization by induction, and is equal to

$$\frac{\mu - 1}{4\pi}$$

This equation then gives

$$Q''_\varepsilon = \Delta L \left(\frac{\pi d^2}{4}\right) \mathfrak{H} r (\mu - 1) \frac{\varepsilon^{rx} - \varepsilon^{r(b-x)}}{1+\varepsilon^{rb}}. \quad . \quad . \quad . \qquad (14)$$

Equation (5) can be approximately adapted to this case by making $s' = \infty$, which is equivalent to neglecting those lines of force which pass out of the end section of the bar. This gives $A' = -1$, hence

* I take this occasion to correct an error in Jenkin's "Textbook of Electricity," where it is stated that, by the introduction of the iron bar into the helix, the number of lines of force is increased 32 times. The number should have been from a quite small number for a short thick bar and hard iron to nearly 6000 for a long thin bar and softest iron.

$$Q''_\varepsilon = \Delta L \frac{\mathfrak{H}}{\sqrt{RR'}} \frac{\varepsilon^{r(b-x)} - \varepsilon^{rx}}{1+\varepsilon^{rb}}. \quad . \quad . \quad . \qquad (15)$$

Now we have found (equation 7) that $r=\frac{2}{d}\sqrt{\frac{1}{\pi\mu R'}}$ nearly, and this in Green's formula (equation 14) gives

$$Q''_\varepsilon = \Delta L \frac{\mathfrak{H}}{\sqrt{RR'}} \frac{\mu-1}{\mu} \frac{\varepsilon^{rx} - \varepsilon^{r(b-x)}}{1+\varepsilon^{rb}}, \quad . \quad . \qquad (16)$$

which is identical with my own when μ is large, as it always is in the case of iron, nickel, or cobalt at ordinary temperatures.

When x is measured from the center of the bar, my equation becomes

$$\lambda = \frac{\mathfrak{H}}{4\pi\sqrt{RR'}} \frac{\varepsilon^{rx} - \varepsilon^{-rx}}{\varepsilon^{\frac{rb}{2}} + \varepsilon^{-\frac{rb}{2}}}. \quad . \quad . \quad . \qquad (17)$$

The constant part of Biot's formula is not the same as this; but for any given case it will give the same distribution.

Both Biot and Green have compared their formulæ with Coulomb's experiments, and found them to represent the distribution quite well. Hence it will not be necessary to consider the case of steel magnets very extensively, though I will give a few results for these farther on.

At present let us take the case of electro-magnets.

For observing the effect of the permeability, I took two wires 12·8 inches long and ·19 inch in diameter, one being of ordinary iron and the other of Stub's steel of the same temper as when purchased. These were wound uniformly from end to end with one layer of quite fine wire, making 600 turns in that distance.

In finding λ from Q''_ε, the latter was divided by $4\pi\Delta L$, except at the end, where the end section was included with ΔL in the proper manner. x was measured from the end of the bar in inches.

TABLE XI. Iron Electro-magnet.

x=distance from end.	Q_ε. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
	22·5	41·1	33·9	−7·2
½				
	12·6	25·1	26·9	+1·8
1				
	19·3	19·3	18·9	−0·4
2				
	12·0	12·0	11·7	− ·3
3				
	6·6	6·6	7·1	+ ·5
4				
	3·9	3·9	4·0	+ ·1
5				
	2·9	2·9	1·7	−1·2
6				

$4\pi\lambda=42.$

The observations in Table XI. are the mean of four observations made on both ends of the bar and with the current in both directions.

The agreement with the formula in this table is quite good; but we still observe the excess of observation over the formula at the end, as we have done all along. Here, for the first time, we see the error introduced by the method of experiment which I have before referred to in the *apparently* small value of $4\pi\lambda$ at $x=\cdot75$.

On trying the steel bar, I came across a curious fact which, however, I have since found has been noticed by others. It is that when an iron or steel bar has been magnetized for a long time in one direction and is then demagnetized, it is easier to magnetize it again in the same direction than in the opposite direction. The rod which I used in this experiment had been used as a permanent magnet for about a month, but was demagnetized before use. From this rod five cases of distribution were observed: first, when the bar was used as an electro-magnet with the magnetization in same direction as the original magnetism; second, ditto with magnetization contrary to original magnetism; third, when used as a permanent magnet with magnetism the same as the original magnetism; fourth, ditto with magnetism opposite; and fifth, same as third but curve taken after several days. The permanent magnetism was given by the current.

TABLE XII. Stub's Steel.

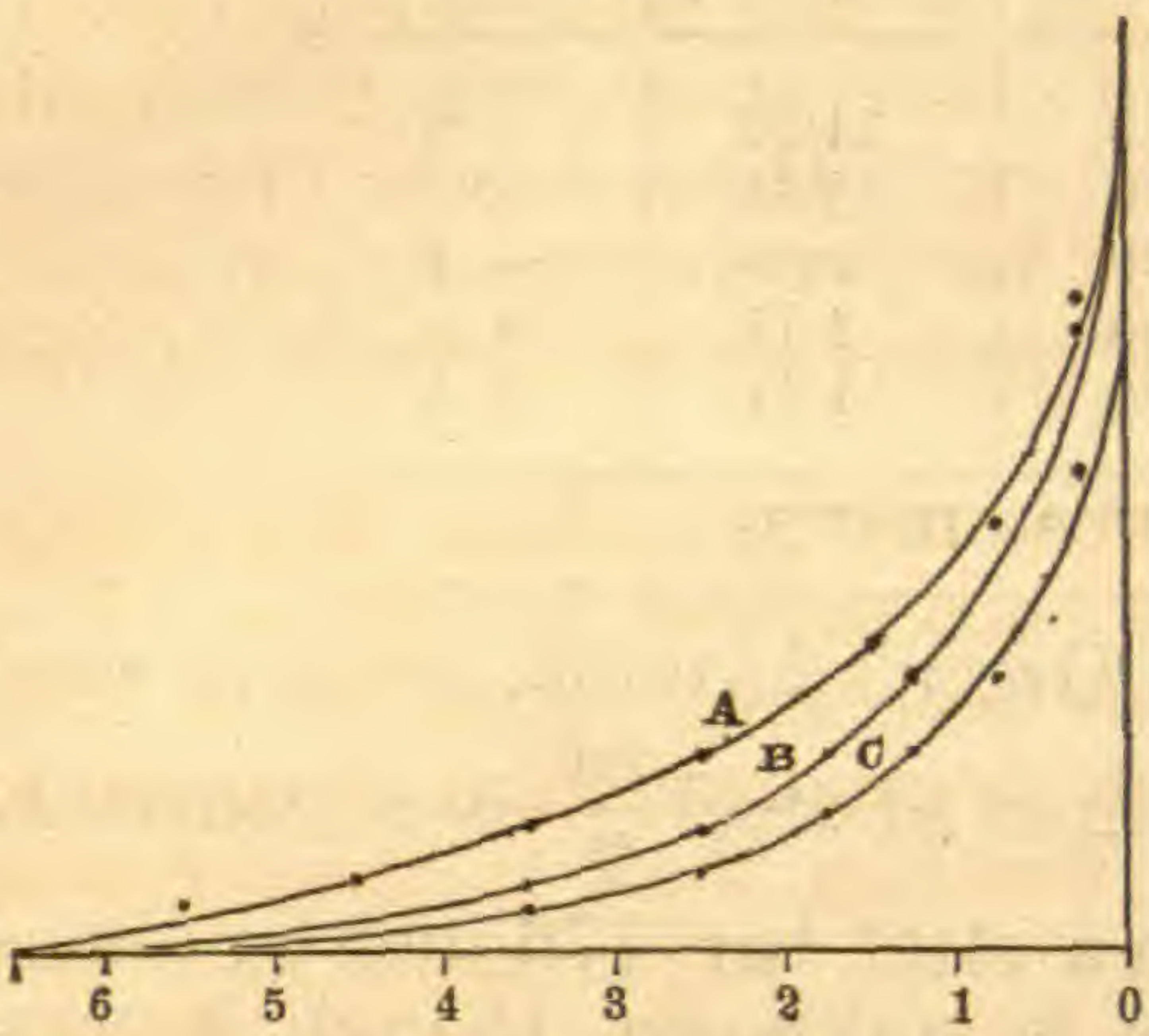
x.	Electro-magnet.				Permanent magnet.					
	Magnetism same as original.		Magnetism opposite to original.		Magnetism same as original.		Magnetism. opposite to original.		Ditto third After three or four days.	
	Q_ε.	$44\pi\lambda$.	Q_ε.	$4\pi\lambda$.	Q_ε.	$\pi\lambda$.	Q_ε.	$4\pi\lambda$.	Q_ε.	$4\pi\lambda$.
0										
	23·3	42·5	15·9	29·0	14·4	13·7	4·8	4·6	12·8	12·2
½										
	11·5	23·0	7·7	15·4						
1										
	8·2	16·4	5·9	11·8	8·2	8·2	4·0	4·0	7·3	7·3
1½										
	6·1	12·2	4·3	8·6						
2										
	7·4	7·4	5·5	5·5	5·3	5·3	2·9	2·9	4·8	4·8
3										
	3·6	3·6	2·7	2·5	3·0	3·0	1·6	1·6	2·9	2·9
4										
	1·7	·8	1·0	·5	2·2	1·1	·9	·4	2·0	1·0
6										

The observations in Tables XI and XII. can be compared together, the quantities being expressed in the same unknown arbitrary unit. It is to be noted that the bars in Tables XI. and XII. were subjected to the same magnetizing-force.

First of all, from these tables and figures we notice the change in distribution due to the quality of the substance;

thus in fig. 5 we see that the curves for steel are much more steep than that of iron, and would thus give greater values to r in the formula, a result to be expected. We also observe in both figures the great change in distribution due to the direction of magnetization. In the case of the electro-magnet this amounts to little more than a change in scale; but in the permanent magnet there is a real change of form in the curve. It seems probable that this change of form would be done away with by using a sufficient magnetizing-power or magnetizing by application of permanent magnets; for it is probable that the fall in the curve E is due to the magnetizing-force having been sufficient to change the polarity completely at the center, but only partially at the ends.

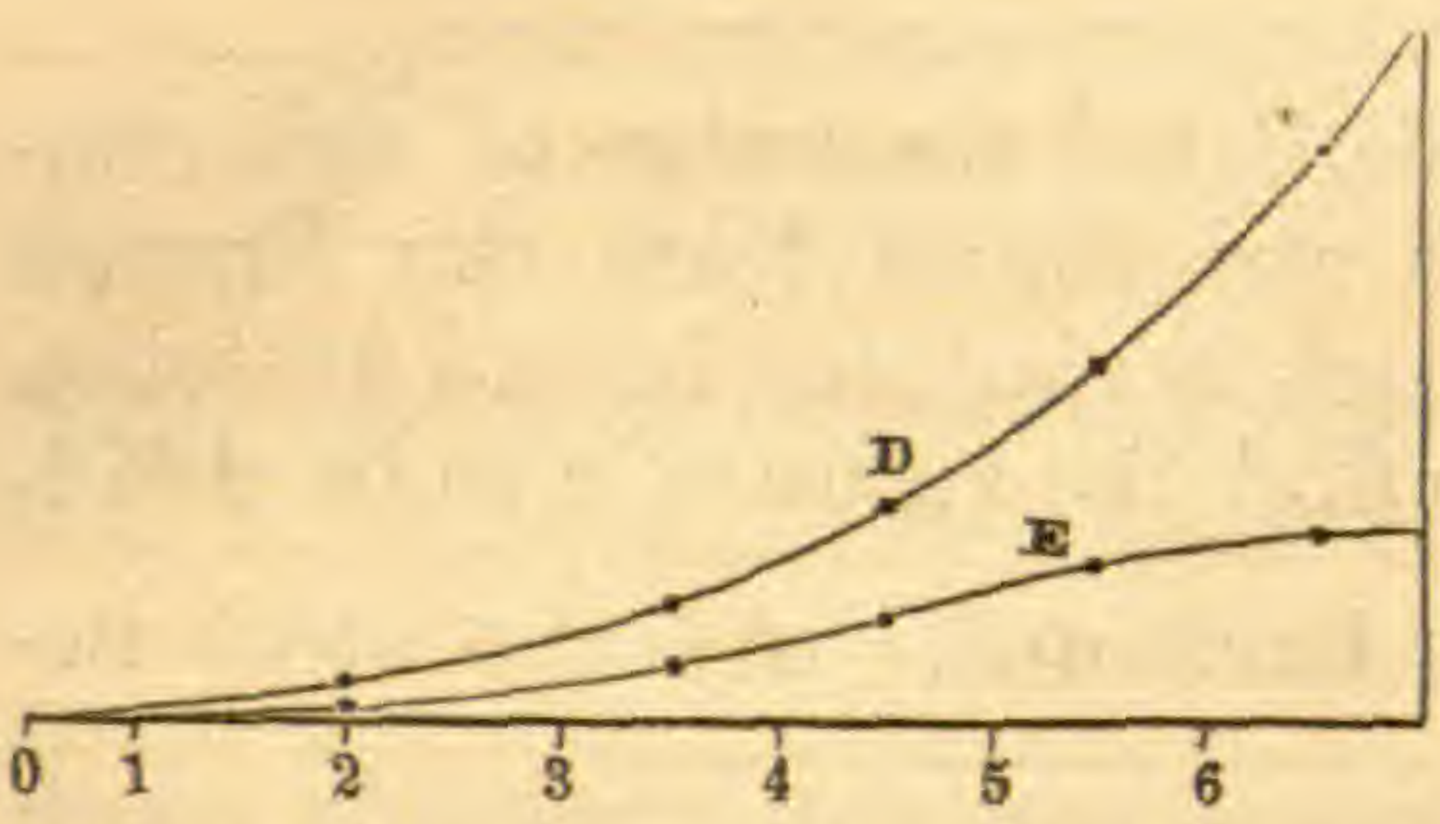
5.



Results from electro-magnets:—
A. Iron, from Table XI.
B. Steel, from Table XII, magnetized same as originally.
C. Steel, from Table XII, magnetized opposite to its original magnetism.

On comparing the distribution on electro-magnets with that on permanent magnets, we perceive that the curve is steeper toward the end in electro-magnets than in permanent magnets. At first I thought it might be due to the direct action of the helix, but on trial found that the latter was almost inappreciable. I do not at present know the explanation of it.

6.



Results from steel permanent magnets:
D. Magnetized in its original direction, Table XII.
E. Magnetized opposite to its original direction, Table XII.
Scale four times that of fig. 5.

As before mentioned, Coulomb has made many experiments on the distribution of magnetism on permanent magnets, and so I shall only consider this subject briefly. I have already given one or two results in Table XII.

The following tables were taken from two exactly similar Stub's steel rods not hardened, one of which was subsequently used in the experiments of Table XII. They were 12·8 inches long and ·19 inch in diameter.

The coincidence of these observations with the formula is very remarkable, but still we see a little tendency in the end observation to rise above the value given by the formula.

TABLE XIII.

x.	Q_ε. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
	46·6	34·9	34·26	+·6
1·28				
	23·8	18·6	18·60	0
2·56				
	12·6	9·8	9·88	−·1
3·84				
	7·2	5·6	4·77	+·8
5·12				
	2·3	1·8	1·41	+·4
6·40				

$4\pi\lambda = \cdot117(10^{\cdot203(b-x)} - 10^{\cdot203x})$.

In equation (7), and also from Green's formula, we have seen that for a given quality and temper of steel $\frac{rd}{2}$ is a constant. From Coulomb's experiments on a steel bar ·176 inch in diameter whose quality and temper is unknown, though it was

TABLE XIV.

x.	Q_ε. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
	42·6	31·9	30·74	+1·2
1·28				
	21·4	16·7	16·72	0
2·56				
	10·9	8·5	8·86	−·4
3·84				
	5·4	4·2	4·28	−·1
5·12				
	1·7	1·33	1·27	+·1
6·40				

$4\pi\lambda = \cdot105(10^{\cdot203(b-x)} - ^{\cdot203x})$.

probably hardened, Green has calculated the value of this constant and obtained ·05482, which was found from the French inch as the unit of length, but which is constant for all systems. From Tables XIII. and XIV. we find the value of r to be ·4674, whence $\frac{rd}{2} = \cdot04440$ for steel not hardened. As the steel becomes harder, this quantity increases and can probably reach about twice this for *very* hard steel.

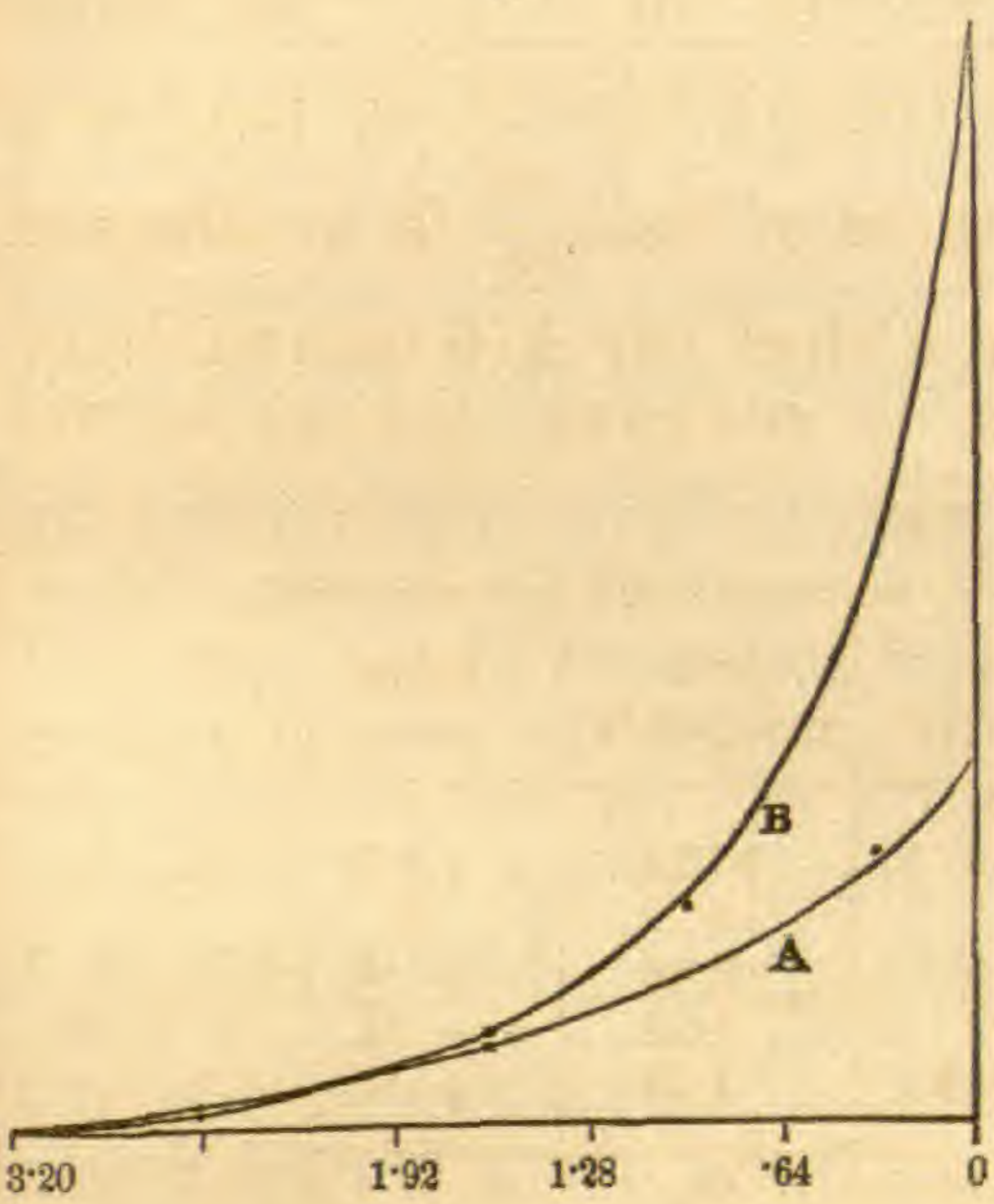
TABLE XV.

x.	Soft steel, A.		Hard steel, B.	
	Q_ε.	$4\pi\lambda$.	Q_ε.	$4\pi\lambda$.
0				
	20·4	29·1	47·7	68·1
·64				
	9·8	15·3	13·9	21·7
1·28				
	6·0	9·4	7·0	11·0
1·92				
	3·8	3·0	2·6	2·0
3·20				

To show the effect of hardening, I broke the bar used in Table XIV. at the center, thus producing two bars 6·4 inches long. One of these halves was hardened till it could scarcely be scratched by a file, but the other half was left unaltered. The following table gives the distribution, using the same unit as that of Tables XIII. and XIV. The bars were so short that the results can hardly be relied on; but they will at least suffice to show the change.

In fig. 7 I have attempted to give the curve of distribution from Table XV, and have made the curves coincide with observation as nearly as possible, making a small allowance, however, for the errors introduced by the shortness of the bar. It is seen that the effect of hardening in a *bar of these dimensions* is to increase the quantity of magnetism, but especially that near the end. *Had the bar been very long, no increase in the total quantity of magnetism would have taken place, but the distribution would have been changed.* Hence from this we deduce the important fact *that hardening is most useful for short magnets. And it would seem that almost the only use in hardening magnets at all is to concentrate the magnetism and to reduce the weight.* And indeed I have made magnets from iron wire whose magnetization at the central section was just as intense as in a steel wire of the same size; but to all appearances it was less strongly magnetized than the steel because the magnetism was more diffused; and as the magnetism was not distributed so nearly at the end as in the steel, its magnetic moment and time of vibration was less.

7.



Results from permanent magnets.
A. Soft steel.
B. Hard steel.

It is for these reasons that many makers of surveyors' compasses find it unnecessary to harden the needles, seeing that these are long and thin.

We might deduce all these facts from the formulæ on the assumption that r is greater, the harder the iron or steel.

Having thus considered briefly the distribution on electro-magnets and steel magnets, and found that the formulæ represent it in a general way, we may now use them for solving a few questions that we desire to know, though only in an approximate manner.

[To be continued.]

ART. III.—*On Recent Researches in Sound;* by WM. B. TAYLOR.

THAT two so eminent physicists as Professor Tyndall in England, and Professor Henry in our own country, should have been for some time past (and almost simultaneously) engaged in investigating the aberrant actions of Sound, with especial reference to securing increased efficiency to the national systems of Fog-signaling, is a noteworthy circumstance, and one of no slight practical importance. In view of the many disastrous marine accidents resulting from fogs on either coast, every thoughtful mind must regard with profound interest a series of researches requiring so much patient labor for the attainment of new and accurate information on the subject, and so high a degree of scientific sagacity and skill for its right interpretation.

As somewhat different explanations have been offered by these two distinguished observers to account for certain abnormal phenomena of sound, a concise statement of the facts and views respectively announced, will interest the general reader. The records of these investigations are, on the one side, the Philosophical Transactions of the Royal Society of London for the year 1874, vol. clxiv, page 183, "On the Atmosphere as a Vehicle of Sound," by John Tyndall, LL.D., F.R.S., a communication read February 12, 1874; and on the other side, the Annual Report of the Light House Board of the United States for the year 1874; the Appendix to which is an account of the operations of the Board relative to Fog-Signals, by Joseph Henry, Chairman of the Light House Board. In addition to these principal sources of information, reference will be made to an interesting communication read before the Royal Society, April 23, 1874, "On the Refraction of Sound," by Professor Osborne Reynolds, and published in the Proceedings of the Royal Society for 1874. The salient points of the observations are selected, and are here arbitrarily designated by bracketed numbers, to facilitate comparisons.

I.

Ten years ago, or in 1865, Professor Henry commenced his investigations on the subject of Sound in connection with fog-signals, at the Light House station near New Haven, Connecticut. Omitting here his careful experiments in regard to the character of the various instruments employed, the principal results then obtained, were the following:

[1.] The reflection of sound was observed to be very imperfect and inexact. A large concave reflector with a smoothly plastered surface of 64 square feet, produced a sensible increase of effect in the sound, within a distance of 500 yards in front of the signal: beyond this distance, the difference became imperceptible. It appeared that "while feeble sounds at small distances are reflected as rays of light are, waves of powerful sound spread laterally, and even when projected from the mouth of a trumpet, at a great distance tend to embrace the whole circle of the horizon." (L. H. Rep., p. 88.) A trumpet, however, which could be heard six miles in front (in the direction of the axis) was heard only three miles in the rear. (p. 92.)

[2.] "For determining the relative power of the instruments, the use of two vessels had been obtained." The instruments at the light-house station were a large bell, a steam-whistle 6 inches in diameter, a double whistle, "improperly called a steam gong," 12 inches in diameter, the cups being 20 and 14 inches deep, producing the harmonic interval of a tone and its fifth, and a Daboll trumpet operated by a hot air engine. The blow-off sound from the "exhaust" of the air engine was also noted. "The penetrating power of the trumpet was nearly double that of the whistle." (Rep., p. 90.) The order of audible range on the first day was found to be 1st, trumpet, 2nd, exhaust, 3rd, bell, the whistle not being sounded. On the second day, 1st, trumpet and "gong," 2nd, whistle, 3rd, exhaust. In the rear the trumpet was heard no farther than the whistle. On the third day, the order was similar,—1st, trumpet, 2nd, whistle, 3rd, exhaust, 4th, bell. (p. 91.) The opportunity was unfavorable to the observation of these sounds when they were moving directly with the wind.

[3.] Simultaneous observations from two vessels sailing in nearly opposite directions, showed that the sound did not extend against the wind so far as in the direction of the wind; and on subsequent days, results obtained from sounds moving nearly against the wind, and at right-angles to it, indicated that an opposing wind, when light, obstructed sound less than when stronger, and that wind at right-angles to the sound, permitted it to be heard farther. (Rep., p. 92.)

[4.] "During this series of investigations an interesting fact was discovered, namely, a sound moving against the wind, inaudible to the ear on the deck of the schooner, was heard by ascending to the mast-head." (p. 92.) These results were obtained in 1865.

[5.] An experiment subsequently made at Washington during a fog, with a small clock-work alarm bell, indicated that the fog did not absorb sound; though want of the opportunity of a comparative observation prevented the result from being entirely satisfactory. (p. 93.)

In 1867, the principal object of investigation was a comparison of different instruments, the character and value of the improvements made in them and especially an examination of a new fog-signal made under the direction of the Board by Mr. Brown, of New York,—the steam siren (p. 194), an inctrument which has since played an important part in fog-signaling. Employing 1st a large Daboll trumpet, 17 feet long, (its steel tongue being 10 inches long), and operated by a hot air engine, 2nd, a siren operated by a tubular steam boiler, and 3rd, a steam whistle, 8 inches in diameter,—an elaborate series of experiments was made as to their penetrating power, as to the most efficient pitch or tone, (p. 95), the effect of varying steam pressure from 20 pounds per square inch to 100 pounds per square inch, (p. 97), the material and shape of the trumpets, &c. (p. 98.)

[6.] During this series of experiments in 1867, attention was called by General Poe, of the Light House Board, to the circumstance that the sound of the paddle-wheels of a steamer some four and a half miles distant from the shore could be distinctly heard by bringing the ears near to the surface of the beach. This fact had previously been noticed on the northern lakes. The desirability of experimenting with large hearing trumpets placed near the surface of the water is suggested by Professor Henry. (p. 98.)

[7.] Experiments on the divergence of acoustic beams, while indicating a considerable reduction of sound toward the rear of the trumpet, showed also very strikingly, the increasing tendency of sound to spread on either side of the axis of the trumpet. (p. 98.) This corresponds with the observations [1] on the employment of sound reflectors.

An important suggestion is made, requiring experimental determination, namely, that condensed air would probably give more efficient results to both the fog-whistle and the siren, than steam. "From hypothetical considerations this would appear to be the case, since the intensity of sound depends on the density of the medium in which it is produced; and as the steam is considerably lighter than air, and as the cavities of all these instruments are largely filled with steam, the intensity of sound would on this account seem to be less." (Rep., p. 99.)

In the absence of Professor Henry in England in 1870, experiments were continued by General Duane, one of the Light House District engineers. These will presently be noticed.

[8.] In 1872 Professor Henry observed from a steamer in the harbor of Portland, Maine, that while approaching an island from which a fog-signal was audible,—at the distance of two or three miles, the sound was lost for nearly a mile, and then slightly regained at nearer approach. This was partly in the rear of the signal; and from its position on the farther side of the island from the steamer, with a large house and rising ground interposed, Professor Henry infers that the region of inaudibility was covered by an acoustic shadow, encroached upon at a greater distance by the divergence of the rays of sound, which, bending, reached ultimately the surface of the water. (p. 107.) A similar phenomenon was observed in the same year on approaching Whitehead station near the coast of Maine. The fog-signal was heard from the distance of six miles to about three miles, and then lost until within a quarter of a mile. (p. 107.) Again, at little Gull Island, in a vessel receding from the siren signal in the direction of its trumpet axis, the sound was lost at a distance of two miles, and then regained at a distance of four and a half miles. (p. 111.) These last cases are referred by Professor Henry to a flexure of the rays of sound resulting from differences of wind velocity in the upper and lower strata of air.

[9.] In 1872, it was observed that a fog-signal was heard from one station to another, while a simultaneous signal from the latter was inaudible in the opposite direction. On board a steamer approaching Whitehead station (a mile and a half from the coast of Maine), the signal, a steam-whistle, failed to be heard from the distance of about three miles to about a quarter of a mile from the station; while a smaller whistle on the steamer was distinctly heard by the keeper at the station during that time. The wind was slightly transverse to the direction from the steamer to the station, but approximately in that direction. The steamer after stopping at the station, on passing from it almost directly against a light wind, continued to hear the signal with variable distinctness for about fifteen miles. (p. 108.) In September, 1874, the keeper at Block Island, on the coast of Rhode Island, observed according to instructions, the times when the fog-signal from Point Judith at a distance of seventeen miles was audible, and in comparing the times when the Block Island signal (a powerful steam siren) was heard at Point Judith, it appeared that the two sounds had not been heard simultaneously by the two keepers. (p. 112.)

[10.] In August, 1873, at Cape Elizabeth station in Maine, the phenomenon of ocean-echoes was distinctly noticed on board a steamer as it was passing directly outward from the signal; the sound after each whistle being returned from the unobstructed space beyond. (p. 109.) In September, 1874, at Black Rock Island also, shortly after each blast of the trumpet, a prolonged echo from the open ocean was distinctly heard. The echo was observed not to be loudest at the siren-house, but at a point several hundred yards to one side; the wind being in the direction of the primitive sound, and nearly opposite to the direction of the reflected echo. (p. 112.) This was supposed by Professor

Henry to be caused by a reflection of the sound from the crests and slopes of the waves.

[11.] On September 23rd, 1874, three observations were made on board steamers moving in opposite directions about one and a half miles from Sandy Hook, New Jersey. First, before noon with the wind from the west, second, at noon with the wind lulled to a calm, and third, an hour and a half later, with the wind blowing from the east. These observations gave the unexpected result of the sound being heard in each case uniformly farthest from the west, irrespective of the wind. (p. 114.) On the next day, September 24th, the observations were repeated farther out at sea, or six miles from the nearest land. Small balloons, sent off with each observation on the sound, showed that notwithstanding the change of surface wind as before, from morning to afternoon, the upper current of wind was steadily and continuously from the west. (p. 115.) Professor Henry supposes that in the first case "the motion of the air being in the same direction both below and above, but probably more rapid above than below on account of resistance, the upper part of the sound-wave would move more rapidly than the lower, and the wave would be deflected downward, and therefore the sound as usual heard farther with the wind than against it." In the third case with a local sea-breeze in the opposite direction, and the upper current remaining unchanged, "the sound should be heard still farther in the same direction or against the wind at the surface, since in this case the sound-wave being more retarded near the surface, would be tipped over more above, and the sound thus thrown down." (p. 115.) This explanation derived from a communication of Professor Stokes, at the Dublin Meeting of the British Association in 1857, (Rep. of B. A., 1856, p. 22 of Abstracts) would appear to be a very satisfactory solution of the apparent anomaly.

II.

In 1870, General Duane, the engineer in charge of the Light-house District embracing the coast of Maine, New Hampshire, and Massachusetts, was assigned by the Light House Board, (as one "who from his established reputation for ingenuity and practical skill in mechanism, was well qualified for the work,") to make experiments and observations on fog-signals. Accordingly during the year 1871, extensive investigations were made by him at Portland, Maine. Passing over his valuable remarks on the qualities of fog-signals, the following are the principal facts observed by him:

[A.] The extremely variable range of sound. The steam fog-whistles on the coast of Maine could frequently be heard at a distance of twenty miles, and as frequently could not be heard two miles, with apparently the same state of the atmosphere. (L. H. Rep., p. 100.)

[B.] The signal was often heard at a great distance in one direction, while scarcely audible at a mile in another direction, and this quite irrespective of the wind. (p. 100.)

[C.] Falling snow was observed not to obstruct sound sensibly, as the steam-whistle on Cape Elizabeth can be "distinctly heard in Portland, a distance of nine miles, during a heavy northeast snow-storm, the wind blowing a gale directly from Portland toward the whistle." (p. 100.)

[D.] The signal station frequently "appears to be surrounded by a belt varying in radius from one to one and a half miles, from which the sound appears to be entirely absent." Receding from the signal, its sound may be audible for the distance of a mile, then lost for the distance of a second mile, and then audible again for a much farther distance. This abnormal phenomenon has been observed at various stations, and at one where the signal is on a bare rock in mid-ocean, twenty miles away from land, and with no surrounding objects to affect the sound. (p. 100.)

No observations have been made to show that this occasional sound-chasm is really a "belt" entirely surrounding the signal; a supposition which appears to be antecedently improbable, and one which would require a large number of radiating observations made simultaneously, to establish it. The curious and exceptional fact, however, is confirmed by the observations of Henry [8] made subsequently.

[E.] Confirmatory of Henry [1], General Duane found that a whistle in the focus of a large parabolic reflector, though giving a notably louder sound in front near the reflector, yet at the distance of a few hundred yards, had its beam of sound so spread that the acoustic shadow behind the mirror vanished, and no perceptible difference appeared. A wooden trumpet or square pyramidal box 20 feet long, in a horizontal position with the whistle in the smaller end, gave, however, more successful results, the increase of sound in the open axis being perceptible at the distance of a mile. (Rep., p. 103.) This corresponds also with Henry's observation [7].

[F.] In repetition and explanation of observation [A] General Duane remarks: "It frequently occurs that a signal which under ordinary circumstances would be audible at the distance of fifteen miles, cannot be heard from a vessel at the distance of a single mile. This is probably due to the reflection mentioned by Humboldt." (p. 104.) This great traveller and scientific observer, in his graphic narrative of exploration in the northern part of South America published at the beginning of the century, ascribes the diminished audibility during the day, of the noise from the cataracts of the Orinoco, at a place on the Atures, to the unequal heating of the air and the reflection and dispersion of the sound from the surfaces of the striæ of differing density.

[G.] It was further noticed by General Duane that "when the sound is thus impeded in the direction of the sea, it has been observed to be much stronger inland;" tending to confirm his idea that the sound in passing from a warmer to a cooler region of air "undergoes reflection at their surface of contact." (p. 104.)

Professor Henry dissents from this opinion that the extinction of powerful sounds is due to unequal density of the atmosphere. Admitting that "a slight degree of obstruction of sounds may be observed" from such a condition, he thinks it "entirely too minute to produce the results noted." (p. 104.) He believes that the "true and sufficient cause" is the difference between the upper and lower currents of air, which tends to bend the sound rays either upward or downward, as suggested by Professor Stokes in 1857. He adds, "In the comments we have made on the Report of General Duane the intention was not in the least to disparage the value of his results which can scarcely be too highly appreciated." (Rep., p. 106.)

[H.] A difficulty occasionally observed with vessels in a fog, is an apparently false direction of the audible signal; which General Duane regards as "due to the *refraction* of sound in passing through media of different density." (p. 104.)

[I.] While thus adopting "the conclusion that these anomalies in the penetration and direction of sound from fog-signals, are to be attributed mainly to the want of uniformity in the surrounding atmosphere," General Duane was also led from observation and experiments to believe "that snow, rain, fog. and the force and direction of the wind, have much less influence than has generally been supposed." (p. 104.) This is in confirmation of his previous observation [C].

III.

Professor Tyndall commenced his investigations on fog-signals on the 19th of May, 1873, "at the instance of and in conjunction with the elder brethren of the Trinity House," as the scientific adviser of the Corporation.

[1.] On May 20, 1873, observations showed the relative penetrating power of different instruments to be variable. At six miles the fog-horn was inaudible, while an eighteen pound gun with three pound charge was heard for ten miles. On many subsequent occasions the horn was found to be superior to the gun. (*Trans. R. S.*, p. 188.) Occasionally the whistles were superior to the trumpet, though not generally so. (p. 189.)

Later experiments in October showed that the pitch of the sound had variable penetration on different days and even at different times on the same day. The siren (an American instrument lent by the United States Lighthouse Board, and put in use October 8, 1873) was generally decidedly triumphant, but not always so. (*Trans.*, pp. 220, 221.)

[2.] The defect of sound in the acoustic shadow of an intervening obstacle (a chalk cliff) was very strikingly manifested. In June the same sharpness of shadow line was observed; and even with the instruments in view, at the distance of a mile, their sound entirely failed near the shadow line at one side. (*Trans.*, p. 190.)

[3.] Although "the wind exerts an acknowledged power over sound" yet, on the 25th of June, "when the range was only six and a half miles, the wind was favorable; on the 26th when the range exceeded nine and a quarter miles, it was opposed to the sound." (p. 194.) On October 11, the sound was observed to be much affected by an adverse wind. It was also noticed on this as well as on subsequent occasions, that "an opposing wind affects the gun-sound far more seriously than that of the siren." With a favoring wind, sounds were heard twice as far as with an adverse wind, even at a point "more deeply immersed in the sound-shadow." (p. 224.)

[4.] July 1, at a distance of five and a quarter miles from a rotating horn it was observed that the sound was sensibly stronger in front than at the rear of the trumpet, the reduction being estimated as seven to ten. (p. 192.)

[5.] July 1, "In a thick haze, the sound reached a distance of twelve and three-quarter miles, while on May 20, in a calm and hazeless atmosphere, the maximum range was only from five to six miles." (p. 193.) And subsequent observations made in London, December 10 and 11, showed that a thick fog offered no sensible obstruction to the passage of sound. (p. 209, 210.)

[6.] On July 3, at 2.15 P. M. "with a calm clear air and smooth sea," at three miles from the signal station "neither horn nor whistle was heard. The guns were again signaled for; five of them were fired in succession, but not one of them was heard." (p. 194, 195.) As a hot sun was pouring its beams on the sea, Professor Tyndall supposed that the copious evaporation resulting, would most probably act very irregularly, producing streams or wreaths of vapor, and thus render the air *flocculent* with these invisible cloudlets, whose surfaces would occasion a large amount of repeated reflection and dispersion of the sound waves. As the sun afterward became clouded at 3.15 P. M., the sounds of the signal were heard at three miles, and very faintly at four and a quarter miles; and later at six miles, and seven and three-quarter miles. Toward the close of the day the signals were heard at twelve and three-quarter miles. (p. 196, 197.)

[7.] On the same day at one o'clock, the echoes from the direction of the open sea were very distinct at the signal station. "The instruments hidden from view, were on the summit of a cliff 235 feet above us, the sea was smooth and clear of ships, the atmosphere was without a cloud, and there was no object in sight which could possibly produce the observed effect. From the perfectly transparent air, the echoes came, at first with a strength apparently but little less than that of the direct sound, and then dying gradually and continuously away." (p. 198.) These remarkable echoes are supposed by Professor Tyndall to be returned from the invisible surfaces of the vaporous striæ, which thus render the air opaque to the sonorous waves. Subsequently, on the 8th of October, the American siren being just received and set up, its loud echoes were observed to be "far more powerful than those of the horn," and to last eleven seconds, while those of the horn had eight seconds duration. (p. 199.) On the 15th of October, the direction of the echoes was found to correspond with the principal axis of the direct or primitive sound; the direction of the return sound changing with the rotation of the horn. (p. 200.)

[8.] On October 8th rain and hail were found not to obstruct sound. While in the morning (after a thunder storm) from Dover and the South Foreland across the English channel "for a time the optical clearness of the atmosphere was extraordinary, the coast of France, the Grisnez lighthouse, and the Monument and Cathedral of Boulogne being clearly visible in positions from which they were generally quite hidden; the atmosphere at the same time was acoustically opaque;" and the horn was feebly heard at six miles. (p. 205.) But in the afternoon a storm arose, and although the rain was falling heavily all the way between the signal station at Foreland and the point of observation on the steamer, "the sound instead of being deadened, rose perceptibly in power. Hail was now added to the rain, and the shower reached a tropical violence." "In the midst of this furious squall both the horns and the siren were distinctly heard," and as the shower lightened, diminishing the local pattering on the deck, they were heard "at a distance of seven and a half miles distinctly louder than they had been heard through the rainless atmosphere at five miles." (p. 206.) On the 23d of October, a similar experience was noticed on land, and, contrary to the usual impression, snow was also observed to offer no serious obstacle to sound. (p. 207.)

It must be borne in mind that the investigations by Professor Tyndall were concluded before the publication of the United States Lighthouse Report. And it is noticeable that these two series of original observations thus independently made on the opposite sides of the Atlantic, in the main quite strikingly confirm each other.

Tyndall's notice [1] of the inconstant relative range of different instruments corresponds with Henry (2), though indicating a much more marked variability.

Tyndall's notice [2] of the sound shadow, corresponds generally with Henry [7], and Duane [E], but assigns a sharper definition to its limit; probably in consequence of the intervention of a larger obstacle (a cliff), and an observation within a shorter distance.

Tyndall [3] confirms Henry [3] and [11].

Tyndall [4] corresponds with Henry [7] and Duane [E].

Tyndall [5] confirms by a series of careful observations, the opinion of Henry [5] and Duane [I].

Tyndall [6] confirms Duane [A and F], and in like manner adopts and extends the suggestion of Humboldt as to the cause of acoustic opacity. Professor Tyndall's admirable skill in experimental physics enabled him to illustrate and fortify his hypothesis by exhibiting in a popular lecture an apparatus for producing in an elongated box or tunnel, aerial laminæ of unequal density, through which the sound from a small alarm box failed to excite a sensitive flame. That this mottled condition of the air is therefore a true cause of acoustic obstruction is no longer doubtful. To what extent a similar condition of the atmosphere actually prevails, in view of the law of the diffusion of gases, and how far such usual or unusual inequalities of density in the air are capable of entirely dispersing the powerful sound of a steam trumpet or siren, at the distance of a quarter of a mile, are not so positively determined. With a continuous wind any such condition of aerial "flocculence" might be expected to be very speedily dissipated.

This theory, however, fails entirely to explain the interesting observations of Henry [4, 8, and 9]. It is scarcely credible that a local screen of aerial flocculence could obliterate on the deck of a schooner, a fog-signal audible at the mast-head. Atmospheric refraction on the other hand, completely satisfies the observed condition; an opposing wind blowing at the time. Still less successful is the theory, in dealing with the abnormal phenomenon of simultaneous audibility at long range, with the intermediate "belt" of acoustic opacity, first observed by Duane [D]. And lastly, the assumption of simultaneous transmission of sound *through* a flocculent air-screen in one direction and its absorption or dissipation by the screen in the opposite direction, (acoustic "non-reversibility,") is obviously inadmissible. Nor is the supposition of acoustic "diffraction" around the defined edge of a vapor cloud, more available.

Professor Tyndall in his recent Preface to the last edition of "Sound" remarks upon this observation of Henry [9]—"a sufficient reason for the observed non-reciprocity is to be found in the recorded fact that the wind was blowing against the shore-signal, and in favor of the ship-signal." (*Preface*, p. xxi.) But he offers no suggestion how this "sufficient reason" is supposed to apply. As it is well-known that an ordinary wind cannot increase the range of sound more than two or three per cent (an amount quite inappreciable), this circumstance alone is wholly inadequate to account for the complete suppression of the shore-signal (a ten-inch steam-whistle) from the distance of three miles to *a quarter of a mile*, while the feebler sound of the ship-signal (a six-inch steam-whistle) was making itself distinctly heard throughout the three miles. Something more therefore than the direct or convective action of the wind must be invoked to explain the facts.

Tyndall's observation [7] on the aerial or ocean echoes, corresponds with Henry [10] excepting as to the direction of the principal echo. This difference is doubtless due to the special arrangement of the surfaces or points of reflection in the respective cases observed. Professor Tyndall connects this phenomenon with that of acoustic opacity [6]; and here again his fine experimental skill is brought into requisition to demonstrate the reality of artificial "aerial echoes." By so simple a device as the employment of the flat side of a "bat-wing" gas-jet, the sound beam from a reed instrument was shown to be entirely deflected from one sensitive flame, and reflected back toward another.

This view of a relation between the acoustic opacity outward or seaward, and the reinforcement or reflection of sound inward, is in striking accord with Duane [G], who however in referring to the "reflection" of sound, does not specifically allude to the ocean "echo." On the refraction theory also, a necessary result is that a deflection of the sound-beam upward in one direction, must be attended with a downward deflection and consequent increase of sound in the opposite direction.

Professor Henry had referred these mystic echoes to the crests and slopes of distant waves; (in conjunction probably with a curvature of the sound-beams, constituting a kind of acoustic "mirage.") To this suggestion, Professor Tyndall opposes the observation that "the echoes have often manifested an astonishing strength, when the sea was of glassy smoothness." (*Sound*, *Pref.*, p. xxiii.)

That this very interesting subject presents features requiring still further and more refined investigation is sufficiently obvious from the single consideration that aerial opacity and echo have not been shown to bear that direct relation which the vapor theory requires. Professor Tyndall has recorded that, on the 17th of October (1873), "It is worth remarking that this was our day of longest echoes, and it was also our day of greatest acoustic transparency, the association suggesting that the duration of the echo is a measure of the atmospheric *depths* from which it comes. On no day, it is to be remembered, was the atmosphere free from invisible acoustic clouds; and on this day when their presence did not prevent the direct sound from reaching to a distance of 15 or 16 nautical miles, they were able to send us echoes of 15 seconds duration." (*Trans.*, p. 202.) If these echoes were not "folded," this would represent an extreme limit of about a mile and a half. Our most powerful sounds cannot afford to waste much of their energy on echoes, if under the inexorable law of increasing attenuation as the square of the distance they are to be audible through a range of 16 miles: less than the 400th of the intensity at one nautical mile, that is heard at the distance of 100 yards from the source; and one 256th of this at the distance of 16 nautical miles, or less than the hundred thousandth of the intensity at 100 yards. And the inference is strong that in such a case accompanying echoes must be derived from sound beams in a somewhat different direction.

Further observations are needed also to ascertain whether these aerial screens of unequal density and acoustic opacity are capable of returning echoes on opposite sides, as is to be expected if we may accept the analogy of catoptrics: and whether the echoes are as frequently heard from steamers in mid-ocean, or whether they mainly attach themselves to coast lines. As Professor Henry has well stated: "Much farther investigation is required to enable us to fully understand the effects of winds on the obstruction of sound, and to determine the measure of the effect of variations of density in the air due to inequality of heat and moisture." (*L. H. Rep.*, p. 117.)

As the last of the series here selected, Tyndall's observation [8] agrees well with the observation of Duane [1].

[To be concluded.]

ART. IV.—*Effect of Temperature on the Power of Solutions of Quinine to rotate Polarized Light. The corrections to be applied for the same. Suggestions regarding the preparation to be used when Quinine is employed as a Medicine;* by JOHN C. DRAPER, Professor of Natural History, College of the City of New York.

IN an admirable article on "The Action of the Solution of certain Substances on Polarized Light," by O. Hesse, in the Annalen der Chemie for 1875, the writer after dealing at length with the varying action of the alkaloids on a beam of polarized light says: "If we now take into consideration the fact that transparent bodies, as water and alcohol, are able, under the influence of electro-magnetism to deflect the plane of polarized light, although this property does not otherwise belong to them; and that the optical powers of a substance can be influenced by mere mechanical means, as Scheibler has proved in certain kinds of glass; we must admit, that '*There is no real relation between the rotating power of a substance and its molecules.*'" He then adds, "The rotating power of a substance is simply the result of the variable action of its factors, viz: the arrangement of the molecules as regards the volume, the solvent, the temperature, the concentration, the chemical combination, the dissociation and other things."

The importance of utilizing the rotation power of quinine for the practical purposes of analysis has induced me to endeavor to determine, as far as possible, the corrections to be applied for the variations in question, and especially for those dependent on temperature. Concerning this, A. Bouchardat says, "variation in temperature causes variation in the rotation power of quinine." In the paper mentioned above, O. Hesse says, "in the case of Thebaine and Quinine the rotation diminishes under an increase of temperature;" but he afterward adds, "I found that the variation between 15° C. and 25° C. was insignificant."

In my experiments the polariscope employed belonged to my friend, Dr. R. A. Witthaus. It was made by Laurent, of Paris, and read by verniers to two minutes. The tube was of glass 220 millimeters in length, with a lateral aperture near the center, through which a thermometer was introduced for the determination of temperature. Around this tube I placed a water jacket, the temperature of which was easily raised to and kept at any required degree, by the injection of steam through a pipe which passed to the bottom of the jacket. Having satisfied myself by a series of experiments that extreme variations of temperature in the water of the jacket, or bath, did not produce any appreciable effect upon the indications of the instrument itself, I proceeded to the determination of the rotation power of the purest sample of quinine I could procure.

Bearing in mind the statement quoted above, that the concentration, solvent and chemical combination have their influence on the amount of rotation, I assumed specific conditions for the preparation of the experimental solutions which might be easily reproduced. They were, 1st, the use of the uncombined alkaloid quinine, carefully dried over strong sulphuric acid, 2d, ninety-seven per cent alcohol as the solvent and a concentration proportion of one gram of quinine, to fifty cubic centimeters of the alcoholic solution. For the sake of convenience the factors required in calculating the results are presented in the following tabular arrangement, viz:

v. volume of 97 p. c. alcoholic solution $=$ 50 cubic centimeters.
p. weight of quinine $=$ 1 gram.
λ. length of tube $=$ 220 millimeters.
$\alpha =$ angle of rotation observed with sodium flame.

The formula being $[\alpha]j = \frac{\alpha \times v}{\lambda \times p} \times 100$ and the average of 200 observations on four solutions at a temperature of 25° C. being $\alpha = -6{\cdot}789°$ we have

$$(1) \quad [\alpha]j = \frac{-6{\cdot}789° \times 50}{220 \times 1} \times 100 \text{ or}$$
$$[\alpha]j = -154{\cdot}30° \text{ at } 25° \text{ C.}$$

Raising the temperature to 47° C. the average of 200 observations on the same solutions as before was $\alpha = -6{\cdot}245°$ from which by the formula we have

$$(2) \quad [\alpha]j = \frac{-6{\cdot}245° \times 50}{220 \times 1} \times 100 \text{ or}$$
$$[\alpha]j = -141{\cdot}93° \text{ at } 47° \text{ C.}$$

The difference of temperature in (1) and (2) being 22° C. and the difference in the angle of rotation 12·37°, it follows that 1° C.= ·562° difference.

That is, in a solution of quinine of the strength in question, viz: 20 milligrams of alkaloid to one cubic centimeter of alcoholic solution, for each additional degree Centigrade of temperature the angle of rotation diminishes ·562 of a degree.

To ensure the correctness of these figures I caused my assistant, Mr. Ivan Sickels, also to carry out a series of experiments, and the result of seven hundred observations at temperatures between 25° C. and 47° C. gave figures which only differed in the third decimal place. We are therefore justified in employing the correction in question for values in the vicinity of 25°

C. which closely approaches the temperature at which such observations are made in actual practical work.

Effect of variation in the Proportion of Alcohol in the Solution.—Hesse having shown that the strength of the alcohol has a marked effect on the rotating power of quinine, it followed that perhaps variation in the proportion of quinine dissolved in a given specimen of alcohol would also give variation in the power of rotation. In the examination of this problem I employed a freshly prepared solution of one gram of undried quinine in 50 cubic centimeters of 97 per cent alcohol. The average of 100 readings of the angle of rotation at various temperatures from 20° C. to 50° C. was

$$(3) \quad \alpha = -6{\cdot}05° \text{ at } 35° \text{ C.}$$

To the above 50 cubic centimeter solution 50 cubic centimeters of the same alcohol were added, forming a solution of half the strength of the first solution. The average of 100 readings at similar temperatures was

$$(4) \quad \alpha = -2{\cdot}61° \text{ at } 36° \text{ C.}$$

To this second solution an equal volume of alcohol viz: 100 c. c. was added, giving a solution of one quarter the strength of the first. The average of 100 similar readings was

$$(5) \quad \alpha = -1{\cdot}27° \text{ at } 36° \text{ C.}$$

In the first solution (3) $p=1$ and $v=50$
In the second solution (4) $p=1$ and $v=100$
In the third solution (5) $p=1$ and $v=200$

By the formula $[\alpha]j = \frac{\alpha \times v}{\lambda \times p} \times 100$ we have

$$\text{For (3)} \quad [\alpha]j = \frac{-6{\cdot}05° \times 50}{220 \times 1} \times 100 = -137{\cdot}50° \text{ at } 35° \text{ C.}$$

$$\text{For (4)} \quad [\alpha]j = \frac{-2{\cdot}61° \times 100}{220 \times 1} \times 100 = -118{\cdot}64° \text{ at } 36° \text{ C.}$$

$$\text{For (5)} \quad [\alpha]j = \frac{-1{\cdot}27° \times 200}{220 \times 1} \times 100 = -115{\cdot}45° \text{ at } 36° \text{ C.}$$

From the above experiments we perceive that the effect of a dilution by alcohol of the solution of quinine is to lessen its power of rotation, and as far as the experiments have been conducted this effect is more marked in the first degree of dilution than in the second.

The repetition of these experiments by Dr. R. A. Witthaus and Mr. Sickels, on a similar series of solutions made with the same alcohol and an undried specimen of quinine, gave the following averages of many hundred readings.

In the 1st solution (6) $p=1 \cdot\cdot v=50 \cdot\cdot \lambda=220 \cdot\cdot \alpha=-5{\cdot}58°$ at 29° C.
In the 2d solution (7) $p=1 \cdot\cdot v=100 \cdot\cdot \lambda=220 \cdot\cdot \alpha=-2{\cdot}40°$ at 31° C.
In the 3d solution (8) $p=1 \cdot\cdot v=200 \cdot\cdot \lambda=220 \cdot\cdot \alpha=-1{\cdot}17°$ at 35° C.

By the formula $[\alpha]j=\frac{\alpha\times v}{\lambda\times p}\times 100$ we have

For (6) $[\alpha]j=\frac{-5{\cdot}58°\times 50}{220\times 1}\times 100=-126{\cdot}82°$ at 29° C.

For (7) $[\alpha]j=\frac{-2{\cdot}40°\times 100}{120\times 1}\times 100=-109{\cdot}09°$ at 31° C.

For (8) $[\alpha]j=\frac{-1{\cdot}17°\times 200}{220\times 1}\times 100=-106{\cdot}36°$ at 35° C.

Here again we perceive that the effect of dilution is to diminish the power of rotation, and to about the same extent and in the same manner as in my series of observations. It is therefore evident, that to secure results suitable for a reliable comparison, the solutions of quinine employed should be as nearly as possible of the same strength. The proportion which according to my experience it is most desirable to use is that of about one gram of alkaloid to 50 cubic centimeters of alcoholic solution. While a greater strength than this does not present any advantage in a tube of 220 millimeters, it is objectionable on account of its obstructing the passage of the light.

Quinine combined with Sulphuric Acid.—For the examination of this compound of quinine I prepared a solution which held the same proportion of quinine alkaloid in a given portion of the solution as that contained in the alcoholic solution. The solution was made by taking one gram of dried quinine, dropping it into about 30 cubic centimeters of distilled water, and adding just sufficient sulphuric acid to dissolve it.* The quantity was then made up to 50 centimeters with distilled water, and the 220 millimeter tube filled therewith.

At a temperature of 21° C. the average rotation as determined by 100 observations was $-11{\cdot}36°$. By the formula

$$[\alpha]j=\frac{\alpha\times v}{\lambda\times p}\times 100 \text{ we have}$$

(9) $[\alpha]j=\frac{-11{\cdot}36°\times 50}{220\times 1}\times 100=-258{\cdot}18°$ at 21° C.

The temperature of the solution in the tube was then raised by means of the water jacket, and the average of 100 observations was $\alpha=-10{\cdot}73°$ at 43° C. By the formula

(10) $[\alpha]j=\frac{-10{\cdot}73°\times 50}{220\times 1}\times 100=-243{\cdot}86°$ at 43° C.

* This solution was employed as being similar to that used by physicians.

The difference in temperature being 22° C. and the difference in rotation 14·32°, we have

1° C. = ·650° difference in rotation.

That is, for every rise of one degree Centigrade the rotation diminishes ·650 or nearly two thirds of a degree in a solution of sulphate of quinine in which there is one gram of alkaloid to 50 cubic centimeters of solution.

Effect of variation in the proportion of water.—A solution of sulphate in water prepared as before and containing one gram of alkaloid to 50 c. c. of solution when examined under a variety of temperatures, gave as the average result

(11) $\alpha = -11 \cdot 03°$ at $31\frac{1}{2}°$ C.

This solution diluted by an addition of 50 c. c. of distilled water by which v was raised from 50 to 100 gave under a similar variety of temperatures the average

(12) $\alpha = -5 \cdot 18°$ at 32° C.

Adding 100 c. c. of water to the last solution and thereby raising v to 200, gave under the same conditions

(13) $\alpha = -2 \cdot 58°$ at $31\frac{1}{2}°$ C.

Arranging these in a tabular form we have

For (11) $p = 1 \cdot\cdot v = 50 \cdot\cdot \alpha = -11 \cdot 03°$
For (12) $p = 1 \cdot\cdot v = 100 \cdot\cdot \alpha = -5 \cdot 18°$
For (13) $p = 1 \cdot\cdot v = 200 \cdot\cdot \alpha = -2 \cdot 58°$

From these by the formula $[\alpha]j = \frac{\alpha \times v}{\lambda \times p} \times 100$ we have

For (11) $[\alpha]j = \frac{-11 \cdot 03° \times 50}{220 \times 1} \times 100 = -250 \cdot 70°$ at $31\frac{1}{2}°$ C.

For (12) $[\alpha]j = \frac{-5 \cdot 18° \times 100}{220 \times 1} \times 100 = -235 \cdot 45°$ at 32° C.

For (13) $[\alpha]j = \frac{-2 \cdot 58° \times 200}{220 \times 1} \times 100 = -234 \cdot 54°$ at $31\frac{1}{2}°$ C.

Conclusions.

(*a.*) In the case of the sulphate, as has also been shown by Hesse and others, there is a greatly increased rotation power imparted to the alkaloid by its union with the acid. In the experiments presented the values are: for one gram of alkaloid to 50 cubic centimeters of solution $[\alpha]j = -154 \cdot 30°$ at 25° C. for the alkaloid: for one gram of alkaloid + sulphuric acid to 50 c. c. of solution in water $[\alpha]j = -258 \cdot 18°$ at 21° C., which applying the correction of ·650° for each degree Centigrade becomes $[\alpha]j = -255 \cdot 48°$ at 25° C. for the sulphate.

(*b.*) The aqueous solution of sulphate shows the same changes under the influence of temperature as the alcoholic solution of the alkaloid, the difference being in the case of the alkaloid 1° C. = −562° and in the case of the sulphate 1° C. = −650°.

(*c.*) In both the sulphate aqueous solution and the alcoholic alkaloid solution, there is the same diminished rotation under dilution, and this occurs chiefly in the first dilution as is shown in the following table:

	Alkaloid Solution.	Sulphate Solution.
1st dilution	$[\alpha]j = -137{\cdot}50°$ at 35° C.	$[\alpha]j = -250{\cdot}70°$ at 31½° C.
2d dilution	$[\alpha]j = -118{\cdot}64°$ at 36° C.	$[\alpha]j = -235{\cdot}45°$ at 32° C.
3d dilution	$[\alpha]j = -115{\cdot}45°$ at 36° C.	$[\alpha]j = -234{\cdot}54°$ at 31½° C.

In closing, I would direct attention to the results indicated in conclusion (*a*), wherein we find that the presence of sulphuric acid has changed the rotation power of a given weight of the alkaloid from −154·30° to −255·48°; and I ask, is it not possible, nay, even probable, that the physiological action of the drug may undergo a similar or perhaps even greater increase? In past times it was the custom to administer quinine in the form of a sulphuric acid solution, and the results were certain and prompt even with minute doses. In recent times, on the contrary, the fancy of patients demands that quinine should be given in pill or some allied form; and though greatly increased doses are used, the practitioner finds it is less certain in its effect. The cause of the difference is doubtless the change in molecular arrangement that produces the marked difference in the action of the alkaloid and sulphate solutions on polarized light; and since the action of the sulphate solution is so much greater than that of the alkaloid solution it is evidently the proper form for the administration of Quinine as a Medicine.

College of the City of New York, Oct. 29, 1875.

ART. V.—*Description of some remains of an Extinct Species of Wolf and an Extinct Species of Deer from the Lead Region of the Upper Mississippi;* by J. A. ALLEN.

THE remains described in the present paper form part of the collection of mammalian fossils made many years since by Professor J. D. Whitney, from the lead-crevices and superficial strata of the lead region of Wisconsin, Iowa, and Illinois, being a part of those enumerated by the late Professor Jeffries Wyman in Whitney's Geological Report of the Lead Region of the Upper Mississippi (pp. 421–423), published in 1862.

The collection originally contained, besides those now described, other remains belonging to the genera *Mastodon*, *Megalonyx* and *Platygonus,* and an extinct species of *Bison.* In addition to these I find an imperfect radius that seems not to differ at all from that of a young male *Cervus Canadensis*, and a part of another radius that does not differ appreciably from the corresponding part of a radius of *Antilocapra Americana.*

The remains of the fossil deer now described are those mentioned by Professor Wyman, namely a left metatarsus, a humerus and a radius, all more or less imperfect.* Professor Wyman described the humerus as "closely resembling that of the red deer, and of intermediate size between this and the humerus of the caribou." As these cervine remains evidently belonged to a species different from any hitherto described, either extinct or living, I propose for it the name *Cervus Whitneyi*, in honor of their discoverer, Professor J. D. Whitney.

The remains of *Canis* consist of a femur, two tibiæ and a humerus (the latter and one of the tibiæ in perfect condition), and may not have been those mentioned by Professor Wyman, although he enumerates parts corresponding to these; since it seems impossible that he could have described them as not differing in size from corresponding parts of the "gray wolf (*Canis occidentalis* Dekay,—*C. griseus* Sabine)," and as being not distinguishable from them; they in reality indicating a species of nearly twice the size of that animal. The rami and "fragment of a right upper jaw" mentioned by Professor Wyman as belonging to the same species are not now in the collection. This species seems to correspond in size quite nearly with the *Canis dirus* which Leidy described (first under the preoccupied name of *primævus*, and still later under the name of *Indianensis*)† from a portion of an upper jaw found with the remains of *Megalonyx*, *Tapirus*, *Equus* and *Cervus Virginianus* in the banks of the Ohio River near Evansville, Indiana, and also with the *Canis Haydeni* Leidy, described later from the Pliocene sands of the Niobrara River from a fragment of a right ramus. Since of the present species we have only a few of the bones of the limbs, it may be better to give it a provisional name than to refer it to either of the species already described, and await the reception of additional material to show their relationship. I accordingly propose for this species the name *Canis Mississippiensis*. As previously noticed, the remains associated with those now described nearly all belonged to extinct species, and to the fauna immediately preceding the

* Another specimen referred to under the head of *Cervus* by Professor Wyman as "an imperfect humerus of a much smaller animal than the preceding" belongs to the extinct peccary, (*Platygonus compressus*).

† *Canis primævus* Leidy, Proc. Acad. Nat. Sci. Phila., vii, 200, 1854. Journ., Acad. Nat. Sci. Phila., iii, 167, pl. xvii, figs. 11, 12, 1856. (Name preoccupied).
Canis dirus Leidy, Proc. Acad. Nat. Sci. Phila., 1858, 21. (Same specimen.)
Canis Indianensis Leidy, Journ. Acad. Nat. Sci. Phila., vii, 368, 1867. (Same specimen.)

present. The bones, though light and somewhat soft, are still white and in an excellent state of preservation, and, though some are broken, have not suffered much abrasion. The humerus of the wolf shows the marks of the teeth of some small rodent.

CANIS MISSISSIPPIENSIS, sp. nov.

The remains of this species, consisting of a perfect right humerus, the distal two-thirds of a right femur, an entire left tibia and the greater portion of a right tibia, indicate a species of nearly if not quite twice the bulk of the existing large wolf of the northern hemisphere (*Canis lupus*), and which had a stature fully one-fifth greater, the difference between them being nearly as great as that between *Canis lupus* and *Canis latrans.* The bones do not differ appreciably in respect to form from those of *Canis lupus.* Their measurements (given in millimeters), in comparison with those of the corresponding bones of a specimen of *Canis lupus* (number 268 of the Museum of Comparative Zoology) from Kansas are as follows:—

Comparative Measurements of Bones of *Canis Mississippiensis* and *Canis lupus.*

	C. Mississippiensis.	C. lupus.
Humerus.—Total length,	223	176
Greatest diameter of proximal end,	55	44
Antero-posterior diameter of head,	41	34
Greatest transverse diameter of distal end,	46	37
Greatest antero-posterior diameter of inner condyle,	36	28
Least circumference of shaft,	62	50
Femur.—Total length,	---	193
Transverse diameter of axis and great trochanter,	---	45
Transverse diameter of condyles,	43	35
Antero-posterior diameter of condyles (inner side),	53	39
Least circumference,	56	44
Length of corresponding parts (distal two-thirds),	155	123
Tibia.—Total length,	244	200
Transverse diameter of head,	47	38
Antero-posterior diameter at most elevated point of the tuberosity,	43	35
Transverse diameter of distal end,	31	24
Least circumference of shaft,	52	43

CERVUS WHITNEYI, sp. nov.

The remains of this species, consisting of a left humerus, entire except lacking the proximal epiphysis, a left radius, also lacking the distal end, and a right metatarsal, which has also lost the distal termination, indicate a species of about the same proportions as *Cervus Virginianus*, but much larger, considerably exceeding in size *Cervus macrotis*. The measurements given below indicate the fossil species to have been at least one-seventh larger than *C. macrotis*, and apparently more than one-fifth larger than *C. Virginianus*. A comparison of the bones themselves give a stronger impression of the greatly larger size of the fossil species than do the tabulated measurements. In respect to form, the humeri of the three species do not materially differ, although the condyles in *C. macrotis* have a rather greater relative breadth than in either of the other species. The radius also differs but little in form in the three, but in the fossil species the ulna (it has now been broken away and is lost) was solidly anchylosed with the radius nearly throughout its length, being free only near its distal extremity, whereas in *C. macrotis* it is anchylosed for only its middle portion, being not only free proximally as well as distally, but for quite a space near the proximal end does not even touch the radius, there being an interval of fully two millimeters between them. In *C. Virginianus* the radius and ulna are nearly as fully anchylosed as in the fossil species. The metatarsal bone is similar in form to that of *C. macrotis*, except that it is relatively more compressed laterally in its distal portion, and seems to have been (the distal end is lacking) relatively narrower at its lower articulation. In this respect it corresponds more nearly with the distal portion of the metatarsus of *C. Virginianus*, which is much rounder and relatively more slender than that of *C. macrotis*. The metatarsal of the fossil species differs from that of *C. Virginianus*, however, in having the groove of the posterior surface continued much further distally than in that species. In the following table of comparative measurements the specimens taken are middle-aged males, the *Cervus macrotis* (No. 1781 of the Mus. Comp. Zool.), being from the Medicine Bow Mountains, Wyoming Territory, and the *C. Virginianus* (No. 1733 of the Mus. Comp. Zool.) from Maine.

Comparative Measurements of Bones of *Cervus Whitneyi*, *Cervus macrotis*, and *Cervus Virginianus*.

	C. Whitneyi.	C. macrotis.	C. Virginianus.
Humerus.—Total length,	---	227	220
Length from most prox. part of head to most dist. part of inner condyle,	---	203	200
Breadth of condylar surface,	48	42	38
Antero-posterior breadth of inner condyle,	51	42	42
Least circumference of shaft,	85	76	73

	C. Whitneyi.	C. macrotis.	C. Virginianus.
Radius.—Total length,	---	242	230
Transverse breadth of proximal end,	---	39	37
Transverse breadth of distal end,	41	38	36
Least transverse diameter of shaft,	29	25	24
Least circumference,	80	68	65
Metatarsus.—Total length,	---	273	255
Transverse breadth of proximal end,	33	29	28
Antero-posterior breadth of proximal end,	36	32	30
Transverse breadth of distal end,	---	35	33
Least transverse diameter of shaft,	22	21	18
Least circumference of shaft,	67	66	58
Length of corresponding portions (proximal five-sixths),	273	232	220

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Action of Nitric Acid on Silver and Copper, alone and in presence of Nitrates.*—Acworth has examined at length, in the laboratory of Dr. Frankland, the gases which are evolved by the action of nitric acid on metals, both with and without the presence of nitrates in the solution. The following are his conclusions: (1) cold dilute nitric acid acting on copper evolves nearly pure nitrogen dioxide; (2) in presence of a strong solution of cupric nitrate, this same action gives rise to nearly pure nitrogen monoxide; (3) potassium nitrate has no effect; (4) ammonium nitrate causes the evolution of nitrogen and nitrogen monoxide, mixed with some dioxide; (5) nitric acid, acting on zinc or iron in presence of ammonium nitrate, evolves nearly pure nitrogen; (6) mercury under the same circumstances acts similarly; (7) on silver, the gaseous products are nitrogen and nitrogen dioxide, with scarcely a trace of the monoxide; (8) in presence of ammonium nitrate, silver produces nitrogen chiefly, mixed with a little nitrogen dioxide.—*J. Chem. Soc.*, II, xiii, 828, September, 1875. G. F. B.

2. *On the Condensability of the Gaseous Products of the vistillation of Carbonaceous Shales.*—Distillation of carbonaceous shales at a low temperature, is extensively resorted to, as is well-known, for the production of liquid hydro-carbons for illuminating purposes. The large amount of gas simultaneously produced, and its high illuminating power, suggested to Coleman a series of experiments upon the condensability of these gaseous products. For this purpose a compression-pump was provided, by which the gas was condensed into an iron tube. This tube was

slightly inclined, and at about three-fourths of its length, a reservoir was placed. Beyond this the gas passed through copper tubes which were immersed in a freezing mixture. Upon the main tube was a safety valve which allowed the pressure to be regulated at pleasure; this was maintained at about 140 pounds to the square inch. In the first experiment, 538 liters of gas were passed through the apparatus, in the second 467 liters, and in the third 1274 liters. In both reservoirs, 84 c. c. of liquid was obtained in the first experiment, 77 c. c. in the second, and 195 c. c. in the third. Of the 77 c. c., 54 c. c. of sp. gr. ·690 condensed in the first reservoir (i. e., by pressure alone without cold) and 23 c. c. in the second, of sp. gr. ·650. Of the 195 c. c., 114 c. c. of sp. gr. ·691 condensed at + 16°, and 81 c. c., of sp. gr. ·658, condensed at —18°. As a mean therefore each liter of gas yielded about ·158 c. c. of liquid of sp. gr. ·680; which is equivalent to one gallon for each 1000 feet of gas. After this treatment the gas was found to have lost its illuminating power, giving no more light when burned from a bat wing jet than does a Bunsen burner. From this and other facts, the author concluded that ethylene is absent from shale gas. Common coal gas when subjected to this treatment gave no appreciable quantity of liquid. The shale products, by weight, therefore, which are obtained on distillation, are:—non-luminous combustible gas 20·9 per cent; volatile liquids, sp. gr. ·680 dissolved as vapors in the gas 4·9 per cent; commercial paraffins, sp. gr. ·700–·800, 52·3 per cent; tarry acid or basic bodies 21·9 per cent. The author proposes a method for commercially preparing these light oils from the gas.—*J. Chem. Soc.*, II, xiii, 856, Sept., 1875. G. F. B.

3. *On the Medico-legal determination of Arsenic.*—Having occasion to revise, for purposes of physiological investigation, the methods ordinarily employed for the detection of arsenic in the tissues,* GAUTIER ascertained that they were seriously deficient in quantitative exactness. He thereupon devised an improved method of separating the arsenic from the organic matter, based upon those of Orfila and Filhol, and a modification of the method of Marsh, by which the arsenic is obtained in a weighable form. The former is as follows:—100 grams of the finely divided animal matter is placed in a porcelain capsule with 30 grams pure nitric acid, and moderately heated. At first the mass liquefies, then it thickens and becomes orange-colored. The capsule is taken from the fire and 5 grams pure sulphuric acid are added. Heat is again applied till white fumes appear. Then 10 or 12 grams of nitric acid is allowed to flow drop by drop on the residue, and it is heated to carbonization. An easily pulverizable mass is thus obtained, which is exhausted with boiling water, filtered, the filtrate reduced with a few drops of hydro-sodium sulphite, and precipitated as usual, by a current of hydrogen sulphide. The arsenous sulphide, transformed into arsenic oxide by nitric acid, is ready for the Marsh apparatus. This consists of a

* See this Journal for December, 1875, page 474.

flask of 180 to 200 c. c. capacity, having two tubulures, and placed in a vessel of cold water. In it are placed 25 grams of pure zinc, on which is poured sulphuric acid diluted with five parts of water. The disengaged gas passes through cotton and then through a tared glass tube heated to redness by charcoal for a length of 20 to 25 cm. The air being expelled, the arsenic, mixed with more dilute sulphuric acid is poured into the apparatus in small portions, an hour being required for the introduction of 5 milligrams of arsenous oxide. The action is kept up for two hours longer, by which time all the arsenic has been carried over. Copper sulphate hinders, platinum chloride facilitates the separation of the arsenic from the solution. After the evolution of gas ceases, the tube containing the annulus of arsenic is weighed again and the amount of arsenic determined. The results are very accurate. In two experiments, in which 5 milligrams arsenous oxide were mixed with 100 grams muscular tissue, the rings weighed 3·72 and 3·67 milligrams respectively; the theoretical quantity being 3·79 milligrams. In a third, 2½ milligrams arsenous oxide were mixed with 100 grams blood; the annulus weighed 1·78 milligrams, the calculated weight being 1·88 milligrams. In 2·1 grams of the brain of a rabbit, fed for 15 days with doses of arsenous oxide gradually increasing from 5 to 50 milligrams, the arsenic recovered was sufficient to give a brilliant ring nearly a centimeter long. A vigorous dog was fed with gradually increasing doses of arsenous oxide, from 4 to 80 milligrams, for a month. 100 grams of the liver yielded 5·3 milligrams, and 100 grams of muscle yielded 0·27 milligram of metallic arsenic.—*Bull. Soc. Ch.*, II, xxiv, 250, Oct., 1875. G. F. B.

4. *Formation of Nitrites by Bacteria.*—The presence of nitrites in spring waters, which has usually been ascribed to the oxidation of ammonia therein, is now stated by MEUSEL to be produced by the reduction of nitrates by the agency of bacteria. In proof of this, he shows: that such water which contained bacteria and nitrates, but neither ammonia nor nitrites, gave, after standing four days, the reactions of nitrous acid ; that antiseptics such as salicylic acid, phenol, benzoic acid, alum, and much salt even, prevent or hinder the production of nitrites; that aqueduct-water containing pure nitrates, which alone does not show the production of nitrites even in presence of bacteria, has this change effected upon the addition of glucose, gum, dextrin, cellulose, starch, etc., in the course of from 2 to 14 days; that freshly distilled water, boiled with glucose and niter, shows no nitrites even after standing for weeks, because bacteria are absent: and that putrefying albuminates reduce nitrates to nitrites. The decomposition of cellulose by bacteria in presence of nitrates proves that niter is not only direct food for plants, but that it also performs by its oxygen an important function in the soil. The author believes that these facts have important bearings in agriculture and in medicine.–*Ber. Berl. Chem. Ges.*, viii, 1214, Oct., 1875.

G. F. B.

5. *On the supposed new Hydrocarbon, C_3H_2.*—A short time ago PINNER described* a new hydrocarbon obtained by the action of sodium upon dichlorallylene, to which he assigned the formula C_3H_2. Further investigation has shown him that the formula is more probably C_3H_4 and that the body in question is either allylene itself or an isomer of it. Assuming dichlorallylene to be $C_3H_2Cl_2$, the action of a molecule of sodium upon one molecule of it would be $C_3H_2Cl_2+Na_2=(NaCl)_2+C_3H_2$; but if C_3H_4 is produced, two stages of the reaction are required; $C_3H_2Cl_2+(Na)_4=(NaCl)_2+C_3H_2Na_2$ and $C_3H_2Na_2+(H_2O)_2=(NaOH)_2+C_3H_4$. In the former case the resulting aqueous solution must contain chlorine and sodium in atomic proportions; in the latter, the sodium is double the chlorine. While more alkali than chlorine was always found, it was far from being twice the quantity. To solve the problem, therefore, the author analyzed carefully the tribromide. While C_3HBr_3 requires 13·0 per cent C and 0·4 per cent H, $C_3H_3Br_3$ requires 12·9 per cent C and 1·1 per cent H. In two analyses the carbon was 13·02 and 12·91, and the hydrogen 1·15 and 1·17 per cent. These results, which contradict the former ones, led the author to examine more carefully the composition of dichlorallylene. He finds that instead of its being $C_3H_2Cl_2$ as assumed, it is really $C_3H_4Cl_2$, having 3·6 per cent hydrogen instead of 1·83 which the first formula requires. This fact harmonizes both the above observations and settles the new hydrocarbon as C_3H_4. Hence the product of the action of chlorine upon aldehyde is not crotonyl chloral, but butyl chloral; though from it, however, crotonic acid has been obtained by Sarnow. This problem, Pinner is now occupied in solving.—*Ber. Berl. Chem. Ges.*, viii, 1282, Nov., 1875. G. F. B.

6. *On Aromatic Compounds containing Arsenic.*—MICHAELIS has published a preliminary note upon phenyl-arsenous chloride, $AsCl_2C_6H_5$, which he obtained by the action of arsenous chloride upon mercury-diphenyl, in a sealed tube at 170°. The reaction is given as follows:

$$Hg(C_6H_5)_2+(AsCl_3)_2=HgCl_2+(AsCl_2C_6H_5)_2.$$

It is a heavy, colorless, strongly refractive liquid, slowly decomposed by water. Investigations upon this and other analogous metallic derivatives are in progress.—*Ber. Berl. Chem. Ges.*, viii, 1316, Nov., 1875. G. F. B.

7. *On Diacetone-alcohol.*—HEINTZ, who has been recently investigating the amines derived from acetone by the action of ammonia, has examined diacetonamine, to see whether the reaction with its nitrite would result, as is general with other amines, in the production of an alcohol; the nitrogen of both being eliminated as gas, and hydroxyl taking the place of amidogen. Diacetonamine oxalate was dissolved in three times its weight of hot water, and cooled to 5°. To the liquid kept constantly stirred, potassium nitrite was gradually added, in amount equal to

* Abstract in this Journal, III, x, 293, October, 1875.

nearly twice the weight of the oxalate. After standing several days, the temperature being allowed slowly to reach that of the atmosphere, the liquid was distilled, whereby some mesityl oxide passed over. The residue, freed from an oily layer by a separating funnel, was neutralized with potassium carbonate and agitated with ether. The etherial solution, dried by calcium chloride, left on distilling off the ether, a liquid boiling between 163·5° and 164·5°, having the formula, $C_6H_{12}O_2$. Its vapor density was 4·19. This is diacetone-alcohol, and it has the rational formula

$$CH_3 . CO . CH_2—\overset{CH_3}{\underset{CH_3}{C}}—OH.$$

—*Liebig's Annalen*, clxxviii, 342, Oct., 1875. G. F. B.

8. *A new relation between Electricity and Light.*—Mr. JOHN KERR has succeeded in showing that dielectrified media are birefringent. Two holes were drilled in a block of plate glass so as to leave a space of only about a tenth of an inch between their ends. Copper wires covered with rubber and shellac were inserted into these holes, and the current from an induction coil capable of giving a spark of 20 to 25 cms. passed through them. A second passage is opened to the current of variable length through the air, so that when the spark passes the glass is subjected to an electric strain. The light of a lamp is passed through a Nicol prism, then through the glass, and finally through a second Nicol at right angles to the first. Since every plate of glass exerts a depolarizing action a certain amount of light is transmitted. This is cut off by interposing a second piece of the same plate of glass and slightly turning one of the Nicols. If the plane of polarization is inclined 45° to the line along which the electric action is exerted through the glass on closing the primary circuit, light will be visible in about two seconds. It brightens continuously for nearly half a minute, and if the current is broken, will gradually fade away. The light thus restored cannot be extinguished by any rotation of the analyzer. If the plane of polarization is parallel or perpendicular to the lines of electric force no action is obtained. There is as great an effect with a rapid succession of contrary electrizations as with a continued electrization in one direction.

A small square of glass held edgewise in a vice was then introduced in the path of the beam and compressed feebly horizontally. A second plate like a microscope slide was also inserted and bent by the hands until the light introduced by the depolarization of the first plate was extinguished. From this arrangement of the apparatus, it appears that the dielectrization of the plate glass is equivalent optically to a compression of the glass along the lines of electric force. Dielectrified glass acts upon transmitted light as a negative uniaxial crystal with its axis parallel to the lines of force.

Half a dozen other solids were tried, but only two, resin and quartz, gave results worth mentioning. The great difficulty was to get a sufficiently strong superficial insulation, the masses being too small. A plate of resin was employed of nearly the same size as the glass. Small squares of thin plate glass were placed in optical contact with its two faces and parallel to each other. It gave evidence of irregular strain near the terminals, separated the red and blue rays by a small angle, and was imperfectly transparent. But its chief defect was that it allowed a spark discharge over its surface a length of 7 inches before the distance of the spark terminals much exceeds 2·5 inches. With all these deficiencies, however, it gave a regular and definite effect, and the action was contrary to that of glass.

A plate of quartz cut perpendicular to the axis, 3 mms. thick and 20 long, was employed with a result similar to that of glass.—*Phil. Mag.*, l, 337. E. C. P.

9. *Waves on Mercury.*—M. C. DECHARME states that by blowing through a tube touching the surface of mercury, we may produce a sound and circular waves forming a symmetrical network upon the liquid. The smaller the interior diameter of the tube, the higher and weaker is the sound and the shorter the waves. As long as the diameter is less than about half a millimeter, the sound resembles the buzzing of an insect. When the diameter amounts to ·7 or ·8 mms. the sound assumes a clear and distinct musical character. With larger tubes the note becomes loud. There is a great tendency of the sounds to pass to their harmonics, with slight changes of the pressure of the air or of the length of the immersed portion of the tube, especially when the latter has a diameter of 2 to 5 mms. For this reason it is difficult to obtain single sounds, but they are almost always high and unstable harmonies. The best results are attained with tubes 0·8 to 5 mms. in diameter, held vertically so as just to touch the surface, and cut off perpendicular to their axes. The tube is then connected with a large rubber vessel full of air and compressed by a weight which should be greater the smaller the tube.

In general the sounds thus produced depend, as regards their height, quality and intensity, on the diameter, length and nature of the tube, the thickness of its edges, the form of the edges of the orifice, and on the temperature, pressure and nature of the gas; finally and above all, on the capacity of the reservoir of air, or rather on the harmonic ratio of its volume to that of the sounding tube and of the connecting tube. This last condition is so essential that a tube giving good results with one reservoir will not work satisfactorily with another. An important application of this device is to the production and projection of interference waves in an elliptical vessel. The nodes and loops are in this case clearly marked, fixed and symmetrical. The concord of the third, fifth and octave may be similarly projected on a screen with great clearness by employing two sounding tubes.—*Journ. de Phys.*, iv, 207. E. C. P.

10. *Spectrum of the light of the blue Grotto of Capri.*—Dr. H. W. VOGEL, on a recent visit to Capri, tested the light of the blue grotto with a spectroscope. As the entrance is only about four

feet in breadth and height, a large part of the light which enters passes through the water. The spectroscope shows that the red is wholly absorbed by the water, and the yellow is so far enfeebled that the D line was scarcely visible. The green, blue and indigo on the other hand shone out brightly, with the F and *b* lines united in a well-marked absorption line.—*Pogg. Ann.*, clvi, 326.
E. C. P.

11. *Action of Magnetism on an Electric Spark.*—M. H. Becquerel has shown that the action of a powerful electromagnet when its current is broken between its poles, is purely mechanical. The spark in this case is accompanied by an explosion, and takes the form of a small flame, which seems to be projected by the action of the magnet as it would be by a current of air. The same effects may be produced by a bellows or even by the mouth. A little explosion is produced, increasing in strength with the force of the air current. It is generally admitted that the sound accompanying the induction spark is due to a sudden expansion of the air and of the volatilized portions of the electrode, followed by a sudden return of the particles to their former position. According to this view the shorter the time of discharge the louder should be the noise.

The substitution of the purely mechanical action of a current of air seems to show that under the influence of the air current as with the magnet, the effect is due to a sudden rupture of the chain of molecules which transmit the electric current of only short duration, forming the induction current. The time of discharge being thus notably diminished the sound accompanying it assumes a remarkable intensity. The idea of making an air-current act in this way is due to M. de Moncel, who has employed it to separate the spark from the aureole.—*Journ. de Phys.*, iv, 206. E. C. P.

12. *Interference Fringes.*—M. Nodot suggests the substitution of rhombs of Iceland spar for Nicol's prisms in forming the interference fringes of Fizeau and Foucault. The sunlight is admitted through a narrow slit, and focussed on the slit of the spectroscope by a lens. Two rhombs of spar are then interposed, and between them is placed a plate of quartz, cut parallel to the axis, and inclined 45° to the planes of polarization of the spars. Three images of the rhomb are thus formed, the central one of double the brilliancy of the others, since it is formed by the superposition of two images. Allowing each image in turn to fall on the slit of the spectroscope, it will be seen that fringes are present in all three, those of the two outer images being complimentary to the fringes of the central image. Evidently the brilliancy of the effect will be double that usually attained, since a Nicol's prism necessarily cuts off at least half the light.

The experiment may be varied by forming the three images one above the other, and allowing all to fall on the slit of the spectroscope, when a spectrum is formed in which the upper and lower parts are fainter than the central portion, and the fringes of the former are complimentary to those of the latter. In other words the bright portions of one set correspond to the dark parts of the other.—*Journ. de Phys.*, iv, 209. E. C. P.

13. *The Wind Theory of Oceanic Circulation. Objections examined;* by JAMES CROLL, of H. M. Geological Survey of Scotland. (From the Philosophical Magazine for October, 1875).*—*The fundamental arguments of the advocates of the gravitation theory.*—1. The gravitation theorists base their argument on two principal assumptions which cannot be conceded. First, they maintain that the existence of polar water in the depths of the ocean is consistent with their theory only; and, secondly, they assume as a necessary condition of the wind theory that the understratum of the ocean should consist of warm water. It is a well recognized fact that the ocean beyond the reach of sun heat is occupied with water of a polar temperature; and they therefore point triumphantly to the fact as at once a proof of their position and a conclusive argument against the wind theory. But, on the other side, it will not be difficult to show that the existence of cold water throughout the ocean depths is as much a necessary result of the wind theory as of the gravitation theory, and that there is no relation whatever between the wind theory and warm water in the depths of the sea.

It is supposed that the return *under-currents* from the polar regions are by far too insignificant to be able to maintain at a polar temperature the great depths of the ocean.

Let us examine this objection. It is freely admitted, nay even strenuously maintained by the advocates of the gravitation theory themselves, that the heating-power of the sun does not extend to any great depth below the surface of the ocean; consequently there is nothing whatever to heat this mass of water underneath except the heat coming through the earth's crust; but the amount of heat derived from this source is so trifling that an under-current from the Arctic regions of no great magnitude would be sufficient to keep the mass at an ice-cold temperature.

On a former occasion† I showed that, taking the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, the total amount received per annum by the North Atlantic, between the equator and tropic of Cancer, including the Caribbean Sea, is equal to only $\frac{1}{894}$ of that conveyed by the Gulf-stream, on the supposition that each pound of water carries 19,300 foot-pounds of heat,—and that consequently an under-current from the polar regions of not more than $\frac{1}{55}$ the volume of the Gulf-stream would suffice to keep the entire mass of water of that area within 1° of what it would be were no heat derived from the crust of the earth, and an under-current of less than $\frac{1}{17}$ that of the Gulf-stream coming from the polar regions would keep the entire North Atlantic from the equator to the arctic circle filled with ice-cold water. A polar under-current half

* Received for this Journal from the author.

† Philosophical Magazine, June, 1874; "Nature," vol. x, p. 52.

the size of the Gulf-stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient under such circumstances to maintain the mass 1° Fahr. above the temperature it possessed when it left the polar regions.

In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions, either in the form of under-currents, or in the form of a general underflow as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water; for there is no source from which this influx of water can derive heat, save from the earth's crust, which amount is so trifling as to produce no sensible effect.

It is therefore evident that the great mass of cold water occupying the depths of the ocean cannot be urged as an objection to the wind theory.

2. But it is asserted that the impulse of the wind on the surface of the ocean cannot produce and maintain deep under-currents. This is an objection which has been urged by some eminent physicists; but it is based upon a misapprehension of the manner in which, according to the wind theory, under-currents are produced.

It is true, as the objectors maintain, that a wind simply impelling the water forward will not necessarily produce an under-current, since compensation will more readily take place by return surface-currents, as in this case the path of least resistance will generally be at the surface. But when the general surface of one half of an ocean basin is being constantly impelled forward by prevailing winds in a contrary direction to that in which it is being impelled in the other half, compensation cannot possibly take place by means of return surface-currents. For a full discussion of this point I must refer the reader to my work, "Climate and Time," Chap. XIII.

It is, however, needless to advance arguments *à priori* against the possibiliy of such under-currents; for we have actually several well-known examples of such currents, the particulars of which will also be found in the work to which I refer.

3. But supposing it could be shown that the winds cannot directly produce under-currents, it can nevertheless be demonstrated that they can do so indirectly. A vertical circulation filling the deep recesses of the ocean under the equator with polar-cold water, follows as readily and truly from the wind theory as it does from the gravitation theory. It has been shown that the general tendency of the system of the winds is to impel the surface-water of the equatorial regions into the temperate and polar regions as rapidly as it is heated. But such a transference of surface-water must tend to destroy static equilibrium by making the equatorial too light and the temperate and polar columns too heavy, as truly as though the transference had taken place by

means of difference of temperature. The effect must be to produce a constant ascent of the equatorial column and an *inflow* of cold water below equal to the *outflow* above. In short, the wind must produce a system of circulation precisely the same as that supposed to take place by difference of temperature.

By both theories the cause of the vertical motion is the transference of water from the top of the one column to the top of the other. This vertical motion is therefore as much a necessary consequence of the wind theory as it is of the gravitation theory.

II. Geology and Mineralogy.

1. *On the Gravel and Cobble-stone deposits of Virginia and the Middle States;* by Wm. B. Rogers. (Proc. Boston Soc. Nat. Hist., vol. xviii, 1875, 101.—The deposits here described as occurring in the great river valleys and on the adjoining slopes, at Richmond, Va., Washington, D. C., and elsewhere, consist chiefly of layers of water-worn gravel and stones, with ferruginous sands and clays. In most localities the larger pebbles are found in the upper part of the deposit; but in others, as at Alexandria and Richmond, the cobble-stone deposit is overlaid by bedded sands and gravel. Casts of Scolithus occur in some of the pebbles collected at Washington and Richmond. The deposit at Washington covers the entire plain on which the city is built; it averages 75 feet in height above mean tide, but rises on the north to about 100 feet. Thence it spreads over the slopes, covering the grounds of Columbian College, and the higher hill of the Soldiers' Home, over 200 feet above sea level. In the vicinity of the Capitol the stones are often a foot in diameter, and near Georgetown in a recent excavation some are much larger. The facts point to transportation along the valleys, but by streams of much greater width than those now there. The distance transported may be learned from the fact that the nearest Potsdam or Scolithus sandstone to Richmond is 80 miles, and along James River 160 miles; and that from Washington to the western side of the Blue Ridge is 40 miles, and along the Potomac 50 to 60 miles. Prof. Rogers remarks on the origin of these deposits as follows:

"Speculating on the causes by which these deposits have been formed, it may, on the one hand, be imagined that during the Glacial period the icy covering of the north and west prolonged itself in the valleys of the great rivers, as far south as the James, and even the Roanoke River, bringing down to the belt of land now marking the limit of tide water debris from the Appalachian rocks, mingled with materials derived from the intervening region, and that the grinding and sorting action of the waters subsequently obliterated glacial marking, and gave to the whole deposit the distribution and stratification which it now presents; or, on the other hand, it may be conceived that the transporting force of the rivers themselves, swollen and rapid as they must have been in the closing ages of the Glacial period, brought about the same results. But even in this case, it is highly probable that glacial action had much to do with the original accumulation of the rocky debris on the flanks of the Blue Ridge, and in the Appalachian valleys beyond."

Prof. Rogers further observes that there is a siliceous and argillaceous formation in Virginia, easily confounded with the finer drift beds, which underlies the Tertiary, and is placed by him at the base of the Cretaceous formation or top of the Jurassic; that sections are exposed in a deep railroad cut between Washington and Baltimore, and on the way to Wilmington; that it is seen beneath the Cretaceous green-sand in Maryland, Delaware, and New Jersey, and near Baltimore was found by Prof. Tyson to contain stumps of Cycads. When the Cretaceous and Tertiary are absent these beds are easily confounded with the stratified drift. Its contact with the superficial deposits was well presented in April, 1875, near Washington, at a cut at the extremity of 16th street, at the base of the Columbian College Hill, and on 14th street where it ascends the same hill.

2. *Report of the Geological Survey of North Carolina*, Volume I. *Physical Geography, Resumé, Economical Geology;* by W. C. Kerr. By authority of the General Assembly. 326, 120 pp. 8vo. Raleigh. 1875.—The Geological survey of North Carolina under Prof. Kerr was commenced in 1866. The report now issued was presented to the Legislature in 1870, but not then ordered for publication; and now it appears in 1875 "under a permissive resolution, out of the working fund of the Survey." A second volume, the preface states, will go to press during the year.

The report commences with an introductory chapter on the Physical Geography of the State. In the course of it the fact is brought out that on the *south* side of the rivers in eastern North Carolina there are usually bluffs and high banks, and on the *north*, swamps and low flats; and that this is a feature also of eastern South Carolina. Further, the Miocene shell-beds were found only on the *south* side of these large rivers. "The cause," according to the author, "is doubtless the rotation of the earth co-acting with the river current;" and he cites the law of motion, wrought out with mathematical demonstrations by Prof. Ferrel, that, "in whatever direction a body moves on the surface of the earth, there is a force arising from the earth's rotation which deflects it to the right in the Northern hemisphere, but to the left in the Southern." The chapter treats of the topographical features and climate of the State and contains a long table of altitudes.

On its Geology the author gives only the general outlines. The series of rocks includes those of the Archæan, which are made to cover (1) a large area running northeastward across the State immediately west of its center and extending westward to include the Blue Ridge; also (2) an area in central North Carolina running north-northeast from Raleigh, and some small areas in that vicinity, one of them to the south on the Cape Fear River; and (3) an area west of the Blue Ridge. The first consists largely of syenyte and related hornblendic rocks, which rarely contain mica and are not schistose; the *second*, of light and gray fine-grained gneisses with some ledges of coarse syenyte and masses of titaniferous magnetite or hematite; one bed of hematite on the Cape Fear River being 40 feet thick; the third of gneiss, hornblendic and mica schists, with some syenyte and coarse granite, and a belt of chrysolyte ledges. The coarse granite of the third area affords much mica in large plates, some 20 inches across; and the mining operations for the mica date back to the mound-builders, the granite veins being "honeycombed with the ancient tunnels and shafts, which were located and excavated with more skill and success than the modern workers have yet attained."

No Silurian rocks are recognized, unless on the extreme western border, within what the map colors as Archæan, where, according to Prof. Bradley, the Lower Silurian exists as a continuation of rocks of that era in Tennessee.

The remaining formations described are the Triassic, the Cretaceous, and the Tertiary. The two Triassic areas, one on Deep River and the other on Dan River, at a distance 75 to 100 miles from one another, have the rocks dipping in opposite directions, those of Deep River, or the more eastern area, dipping southeastward about 15° to 35°, and those of the other northwestward 35°. Prof. Kerr makes the supposition—improbable as it appears to the writer—that the two are the margins of a single anticlinal that once spanned the broad interval between them. The Connecticut River Triassic has in general a similar southeastward dip; but there is no where the opposite side of the anticlinal unless we look to the New Jersey area for it, which is quite too far south to answer.

A large part of the volume is occupied with a chapter on Economical Products of North Carolina. It embraces a large amount of information on soils; marls and fertilizers; peat and muck; ores and mines (among which those of iron are very extensive, and those of gold and copper, also, have been profitably worked); coal (Triassic); graphite, and kaolin; fire-clay, pyrophyllite, corundum, mica, and mineral waters.

The coal field of Deep River has an area of about 300 square miles. Dr. Genth obtained in an analysis of two samples, fixed carbon 63·28, 70·48, volatile matter 25·74, 21·90, ash 10·14, 6·46, moisture 0·84, 1·16=100. It contained sulphur 1·35, 1·02. The Dan River coal afforded the same chemist 75·96 and 76·56 p. c. of fixed carbon, 11·44, 13·56 of ash, the volatile matter in each about 12 per cent.

The report contains also a well-colored geological map of North Carolina and several plates of fossils.

3. *Second Geological Survey of Pennsylvania.*—The following Reports for 1874 have recently been published by the State Board of Commissioners at Harrisburg.

Report of Progress on the Brown Hematite (Limonite) Ore Ranges of Lehigh County, with a description of the mines lying between Emaus, Alburtis and Fogelsville; by FREDERICK PRIME, Jr., Assist. Geol. 74 pp. 8vo, with cuts and maps.

Report of Progress in the Venango Co. District; by JOHN F. CARLL. *Observations on the Geology around Warren;* by F. A. RANDALL. *Note on the Comparative Geology of Northwestern Ohio and Pennsylvania and Western New York;* by J. P. LESLEY. 128 pp. 8vo, with wood-cuts and maps.

Report of Progress in the Laboratory of the Survey, at Harrisburg; by ANDREW S. M'CREATH. 106 pp. 8vo.

Prof. Prime gives in his report a brief account of the topography and geology of the district under examination, and of the rocks with which the limonite ores are associated. These rocks are stated to be of the magnesian limestone series—that is, Lower Silurian beds of the age of the Calciferous sandrock, and the Chazy, Birdseye and Black River limestones. Mr. Prime says: "The great mass of this formation is dolomite; but there occur one and possibly more beds of hydro-mica (or damourite) slate intercalated in it. The limonite ore always accompanies the hydro-mica slate, and with it there is often clay from the decomposition of the slate. The following are three out of five analyses of this slate from Lehigh Co.:

	Si	Al	Fe	Mg	Ca	Na	K	H	
1. Fogelsville	49·92	34·06	0·91	1·77	0·11	0·74	6·94	6·52	= 100·97
2. Hensingerville	45·40	24·69	5·06	13·56	tr.	0·27	5·85	4·80	= 99·63
3. Near Allentown	59·30	30·30		trace	tr.	1·51	6·24	4·70	= 102·05

From the amount of the potash in the first analysis (by Dr. Genth), Mr. Prime calculates the amount of damourite present (with free silica, etc.) to be 55·40 p. c.; in the second (by Mr. S. Castle), 49·70 p. c.; in the third (by Mr. P. G. Salom), 53·02 p. c. This hydro-mica slate, with often associated limonite beds (as a result of alteration), extends, as Mr. Prime observes, from Vermont to Alabama, showing thus a long range of Lower Silurian rocks in the eastern mountain region of North America.*

The report describes further the mines and ores, and gives many analyses. It is followed by a note from the State Geologist, making some explanations with regard to the equivalency of the Pennsylvania formations, and remarking on some topographical changes the country has undergone. Mr. Lesley goes outside of his field in his closing remarks, and states—what is sustained as yet by no adequate stratigraphical evidence—that the "Green Mountain system of Vermont" and "the White Mountain system of New Hampshire," are, like "the Laurentian Mountains of Canada," older than the Potsdam; and that the Green Mountain system, one of these "three great mountain systems of the north," is Huronian. The observations by Mr. Prime in Pennsylvania, above-mentioned, and the parallel facts in the Green Mountain system to which he draws attention, all point as regards the Green Mountains in the opposite direction. The writer has studied stratigraphically the Green Mountain region from Con-

* The first determination of the fact that the so-called talcose slates are mostly hydro-mica slates is accredited to Prof. Dana. The latter gives the credit to others in this Journal (III, iv, 366, 1872), where he named the rock *hydro-mica slate.*

necticut to Vermont, and has found that the hydro-mica and chloritic hydro-mica slates associated with the limonite beds of Berkshire are of the same formation with the hydro-mica, chloritic, and micaceous slates of Graylock and the Taconic range; and with the hydro-mica slates of the ridge lying northeast of Rutland in Vermont, and of others west and north of Rutland; and with the staurolitic schists of the limonite region of Salisbury, Connecticut. Since the limestones associated with the slates of West Rutland abound in distinct Lower Silurian fossils, referred to the Chazy by Billings,* part of the Green Mountain slates and schists are unquestionably Lower Silurian. What is the age of the rest is not yet positively known.

Mr. Carll's report contains the results of his observations in the oil district of Venango County—giving in detail the geological and topographical distribution of the oil, and its distribution also in depth.

Mr. Lesley's notes on the comparative geology of the adjoining parts of the States of Ohio, New York and Pennsylvania, are of great interest. The equivalency and distribution of the formations are discussed, and the age of the oil-bearing beds, and some new views and facts are brought out. Mr. Lesley states that the Catskill sandstone does not thin out westward in New York, as heretofore described, but that it continues into Ohio, and that it includes the "Rock-City" conglomerate of Chatauqua Co., N. Y.; also, that the conglomerate under the Coal measures, to which the Rock-City conglomerate has been referred, "seems nowhere to reach the New York State line, even in outlying patches."

The Chemical Report of Mr. M'Creath contains descriptions and proximate analyses of bituminous coals of many localities, and analyses of various iron ores, limestones and fire clays. It states that the average amount of water in the bituminous coals analyzed is only 1·03 per cent; the average of ash about 5·38 per cent; of phosphorus, ·014 per cent; of volatile combustible matter from bituminous coals of Clearfield Co., 23·64 per cent, of Centre Co., 23·81, of Jefferson Co., 32·60, of Armstrong Co., 34·99 per cent; of fixed carbon in coals of Clearfield and Centre Cos., 68·97; of sulphur in 34 coals from Clearfield Co., 1·36 per cent.

4. *The Vertebrata of the Cretaceous Formations of the West;* by E. D. COPE. 304 pp. 4to, with 57 plates. Report of the U. S. Geological Survey of the Territories by F. V. HAYDEN, U. S. Geologist in charge, and under the authority of the Department of the Interior. Vol. II. Washington, 1875.—Professor Cope's Report is another of the great works on science due to researches

* These West Rutland fossils are not rare and inconspicuous. Guided by the Rev. A. Wing to the localities he had discovered, I found the rock over the *central* part of the valley in some places full of them, and other specimens less distinct occur in the western margin of the valley; Mr. Wing has observed them also on the eastern margin. The famous marble quarries of the valley are in the intermediate portion of the limestone formation where the metamorphism was more complete. J. D. D.

connected with the Geological Survey of the Territories under the charge of Dr. Hayden. The volume takes up only the Vertebrates of the Cretaceous, leaving those of the Tertiary for a later Report. Each subject, owing to the great number of species discovered in the beds, is a very large one; and the latter will make, as the Preface states, two such volumes. These two are in addition to the quarto volume, in the same series, by Dr. Leidy. The work aims to mention all the species thus far discovered in the Cretaceous west of the Mississippi, and to describe in full those made known by the author. Professor Cope, in an introductory chapter presents his views "on the significance of paleontological science;" then treats in Part I of the classification and distribution of the Cretaceous deposits of the West; in Part II, gives "Descriptions of the Cretaceous Vertebrates of the West;" and in Part III, introduces a Synopsis of the known Cretaceous Vertebrates of North America. The 57 plates of illustrations are full of figures, and well engraved.

The whole number of species of Reptiles in the American Cretaceous beds is stated as follows: Dinosaurs, 18; Pterosaurs, 4; Crocodilians, 14; Sauropterygia (Plesiosaurs, etc.), 13; Testudinates, 48; Pythonomorphs (the Mosasaur tribe), 50 = 147. Of the last tribe, 15 species occur in the Greensand of New Jersey, 7 in the Rotten limestone of Alabama, 26 in Kansas, and one from each, North Carolina, Mississippi and Nebraska. Only four are known from Europe.

Professor Cope, besides bringing out his own large contributions to the subject, mentions also those of Dr. Leidy and Professor Marsh, yet not always in a way to do them full justice. He appears to have forgotten one of his statements, when penning the note to page 124, on *Clidastes propython*. The note reads as follows:

"Professor Marsh (American Journal of Science and Arts, 1872, p. 454,) quotes me as assigning ten cervical vertebræ with articulated hypapophyses to this species. This I have not done, but state (Synopsis of the Extinct Batrachia and Reptiles of North America, p. 221,) that it possesses six such vertebræ. Professor Marsh's statement, and consequent supposition that he first determined the number of cervical vertebræ in the genus Clidastes, are the result of a misapprehension."

But Professor Marsh sustains his remark (this Jour., ii, p. 454, 1872) by reference to page 218 of Cope's Synopsis, where Professor Cope, in drawing out the characters of the genus "Clidastes" from "especially the nearly complete skeleton of *Cl. propython*," says: "Hypapophyses exist on the ten cervical vertebræ," thus recognizing *ten* as the number. And on page 221 of Cope's Synopsis (the page he refers to) there is nothing that sets this aside. Professor Cope, in view, as he says, of the fact that "a considerable number of the vertebræ [in *Cl. propython*] has been lost," gives on that page the following enumeration of the [*by him*] *known* vertebræ: Atlas and axis, 2; cervicals, 6; dorsals, 15;

and next adds, under the line *dorsals*, 15, "at least to be added to this series, 10," making the sum "33." No correction of the previous statement as to the total number of cervical vertebræ is made or suggested.

In the catalogue of works and memoirs on American Cretaceous Reptiles, headed LITERATURE OF THE SUBJECT (pp. 51, 52, 53), Professor Cope has omitted to mention several highly important contributions by others: for example, Dr. Leidy's memoir of 1865, *on Cretaceous Reptiles of the United States*, a quarto of 135 pages, illustrated by 20 plates; a paper by the same author, *on Elasmosaurus* (Proceedings of the Acad. Nat. Sci. Philad., 1870, p. 9); another *on Discosaurus and its allies* (ibid., p. 18), and another *on Hadrosaurus and its allies* (ibid., p. 67); also, the following of Professor Marsh's papers: *Notice of some New Mosasauroid Reptiles* (this Journ., II, xlviii, 392, 1869); *on a New Species of Hadrosaurus* (ibid., III, iii, 301, 1872), and *Note on Rhinosaurus* (ibid., III, iv, 147, 1872). Again, in his *Synopsis of the known Cretaceous Vertebrata of North America*, constituting Part III of the volume, several Cretaceous Reptiles described by others are omitted, and also the following four species of Cretaceous birds described by Professor Marsh: *Graculavus velox*, of New Jersey (this Journal, III, iii, p. 353, 1872); *G. pumilus*, of New Jersey (ibid., p. 364); *G. agilis*, of Kansas (ibid., v, 1873), and *Palæotringa vagans*, of New Jersey (ibid., iii, 366). J. D. D.

5. *Description of new Species of Fossil Plants from Alleghany Co., Virginia; with remarks on the rocks seen along the Chesapeake and Ohio Railroad, near the White Sulphur Springs, Greenbrier Co., West Virginia;* by F. B. MEEK. 19 pp. 8vo. Proceedings of the Washington Philosophical Society. Read before the Society, June 15, 1872. (Received Dec. 8, 1875).—The fossil plants described by Mr. Meek are from Lewis's Tunnell, and occur in the lower part of the Subcarboniferous, near its junction with the upper Devonian. The species are *Lepidodendron scobiniforme* M., *Cyclopteris Lescuriana* M., *C. Virginiana* M., *C. Alleghaniensis* M., besides an undetermined *Stigmaria* and some doubtful *Carpolithes.*

6. *Coal plants of Tinkiako in Southern Shensi in China.*—AD. BRONGNIART has determined the following plants from the southern part of Shensi, one of the western provinces of China (Bull. Geol. Soc. France, 408, 1874): *Pecopteris Whitbyensis*, two species of *Sphenopters*, a leaf of a *Zamia* near *Zamites distans*, fragments of *Lycopodites Williamsoni*, a species of *Palissya*, and also *Bayera dichotoma* Fr. Braun. The species are nearest to the Jurassic plants of Whitby, and not Carboniferous. Subcarboniferous fossils are described from the same region by MM. Paul Fischer and Bayan (ibid., p. 409 and pl. 16), who report, from shales, the following: *Spirifer lineatus* Mart sp., *Athyris ambigua*, *Meekella Garnieri* (n. sp.), *Productus Davidi* (n. sp.), *P. costatus* Sow., var *cælestis*, and *Bellerophon tangentialis* Phill.

In this connection, it is to be noted that the coal plants of Chaitung, west of Pekin, obtained by R. Pumpelly, and described by Dr. Newberry (Geol. Res. in China, etc., Smithson. Contrib., No. 202, 1867,) are referred by the latter to the Trias; they embraced the species *Pterozamites Sinensis* Newb., *Podozamites lanceolatus* Lindl., *Pod. Emmonsii* Newb., (*P. lanceolatus* of N. Carolina, Emmons), *Sphenopteris orientalis* Newb., *Pecopteris Whitbyensis* Brongn., *Hymenophyllites tenellus* Newb., *Taxites spatulatus* Newb. Von Richthofen, on the contrary, concluded, from the conformability of the coal-bearing beds to others below containing Paleozoic fossils, that the coal of China (this Journal, II, i, 410, 1870), was for the most part Carboniferous.

7. *The Dawn of Life, being the History of the oldest known Fossil Remains, and their relations to Geological Time and to the development of the Animal Kingdom;* by J. W. Dawson, LL.D., F.R.S., etc. 240 pp. 12mo, with plates and wood-cuts. London, 1875. (Hodder and Stoughton.)—This volume contains a complete account of the history of the discovery of the Eozoon, and of its structure and nature as developed by Dr. Dawson, Prof. Carpenter, and others. The facts described are illustrated by excellent figures; and one of them, facing page 35, representing an Eozoon mass, looks exceedingly like a form of coral—the Stromatoporæ—to which group it was referred by Logan before its interior structure was studied. The subject discussed is of profound geological importance, since it bears on the question as to the first expression of the animal idea in an organism, and the volume is therefore one of great interest. The Eozoon is referred by the author to the section of the Rhizopods containing the Foraminifers, and to the division of the Foraminifers called *Perforata*, to which the *Nummulinidæ*, *Globigerinidæ* and *Lagenidæ* belong, which have calcareous skeletons penetrated by pores. An inferior division, called the *Imperforata*, have calcareous membranous or arenaceous skeletons without pores.

8. *Geographical and Geological Surveys*; by J. D. Whitney. 96 pp. 8vo. From the North American Review for July and October, 1875. Cambridge, 1875. (Welch, Bigelow & Co.)—Professor Whitney in these papers brings to bear the results of his wide experience as a geographical and geological explorer, in a discussion of the objects, methods, and purposes of such surveys, and gives some account of their history in this and other countries. Much information is presented on the topographical maps issued by foreign governments, and on those in progress and needed at home. The history of geological exploration in the United States is treated with considerable detail and with discrimination. The volume is one to which all may go for information and judicious advice as to the ends accomplished by State surveys, and the means required to secure from them the greatest good to the people.

9. *Descriptive Catalogue of the specimens in the Museum of Melbourne, illustrating the rock system of Victoria;* by G. H. F. Ulrich, M.E., F.G.S. 108 pp. 8vo. Melbourne, 1875.—Besides

the detailed description of the individual specimens in the museum, this little book contains some remarks upon the different kinds of rocks of Victoria and their relations, which give it something more than a local interest. A considerable number of analyses are given, especially for the igneous rocks.

10. *Geology of Illinois.*—The last volume of the series of reports connected with the Geological survey of Illinois under Mr. Worthen is now in the binder's hands.

11. *Geological map of the United States and Canada.*—In a few days, a Geological chart of the United States (east of the 104th meridian) and Canada, by Prof. F. H. BRADLEY, will be on sale by Messrs. Ivison, Blakeman, Taylor & Co., 138 and 140 Grand street, New York, the publishers of Professor Dana's works on Geology. Its size is 24 inches by 16. It contains all the detail and accuracy possible on a chart of this size in the present state of the science. The geological areas are well distinguished by a judicious system of lining, instead of by colors, and hence the chart will be afforded, as we understand, at the low rate of one dollar each. It should be in the hands of all students in geology, and is absolutely indispensable to every teacher.

12. *Einleitung in die Krystallberechnung;* von Prof. CARL KLEIN, Erste Abtheilung. 208 pp. 8vo. Stuttgart, 1875.—Prof. Klein develops the subject of Crystallography from the standpoint of Quenstedt, according to whom the relations of the planes of a crystal are shown by the lines in which they are projected upon a given plane of projection. He does not confine himself to the somewhat unwieldy formulas of Quenstedt, however, but makes all calculations of axial relations and parameters by means of spherical triangles in the manner employed by von Kobell. The author has worked out with much care and completeness the solutions of the various problems which arise, and accompanies each with an example performed in full, so that the student cannot fail to comprehend the method. E. S. D.

13. *On Troilite;* by Dr. J. LAWRENCE SMITH.—A paper on troilite by Dr. Smith was read before the French Academy of Sciences on the 22d of November last. The author shows by new and careful analyses that troilite, or the sulphide of iron of meteorites, has the composition he has before obtained for it, represented by the formula FeS, and not that of pyrrhotite (Fe^7S^8) to which species it is referred by M. Saint Meunier in a communication to the Academy of March, 1874. He observes that the specific gravity of troilite, 4·813, also separates it from pyrrhotite, a selected specimen of the latter affording him only 4·642. As this meteoric sulphide is found imbedded in a mass of iron, "the natural supposition is that the sulphur would be saturated with the iron." Hence, he adds, "troilite like schreibersite, is exclusively a celestial mineral.

Rectification of the Geological map of Michigan, embracing observations on the Drift of the State; by Alexander Winchell. 17 pp. 8vo. Salem, 1875. From Proc. Amer. Assoc. for 1875.

III. Botany and Zoology.

1. Karl Koch, *Vorlesungen über Dendrologie. Lectures on Dendrology*, delivered in Berlin in the winter semester of 1874–75. Stuttgart, F. Enke, 1875, pp. 408, 8vo.—These are the notes of a course of popular lectures by Prof. Koch, on a subject in which he is thoroughly at home. They must have been delightful to hear, as they are pleasant to read, and are full of interesting matter. It is only the first course of a series which is to be continued this winter. The first division, of seven lectures, is a history of landscape gardens and gardening. The second division, of as many more, treats of the structure, growth, and life of trees, of the influence of woods upon mankind, and as regulators of temperature and atmospherical changes. The third division, in four lectures, treats of Coniferous trees,—all in a popular way. Prof. Koch insists that the two willows confounded as forms of the Weeping Willow, are neither of them Persian or Assyrian, except by immigration, but natives of a farther east, i. e., of China and Japan. One of them may have reached Western Asia, however, early enough to have been collected by Tournefort, and so to excuse the error fixed by Linnæus by his name of *S. Babylonica.* But even the last volume of DeCandolle's Prodromus does not rectify it. Notwithstanding Prof. Koch's correction and elucidation, it is likely that popular books and the popular belief will continue to associate the Weeping Willow with the River of Babylon and the hanging harps of the weeping Israelites, although the tree of the Psalm most likely was a Poplar. We believe it was Ker Porter who remarked that willows were to be found along the river, but only as low shrubs: upon these nothing larger than a comparatively modern musical instrument associated with the name and nation could well be hung. When the course is completed we shall look for an English edition of these lectures upon tree-lore. A. G.

2. *Insectivorous Plants;* by Charles Darwin. With illustrations. London: Murray. New York: D. Appleton & Co.—This long expected work appeared last autumn, was immediately reprinted by the American publishers, and before this time has been so widely read that no detailed account of it is at all necessary. Its main topic is *Drosera* or Sundew, upon which the vast number and diversity of the observations and experiments—at once simple, sagacious, and telling—which it records, are about as wonderful as the results. As to the latter, it is established beyond question that the common Sundews are efficient fly-catchers; that the stalked glands, or tentacles, as Mr. Darwin terms them, are sensitive and turn inward or even in other required directions in response to irritation; that they equally respond and move in obedience to a stimulus propagated from a distance through other tentacles and across the whole width of the leaf; that the sensitiveness belongs only to the glands and tips of the tentacles, but

is propagated thence down their stalks and across the blade of the leaf through the cellular tissues; that they accurately and delicately discriminate animal or other nitrogenous matter from anything else; that the glands absorb such matter; that when excited by contact, or by the absorption of nitrogenous matter by the viscid enveloping liquid, an acid secretion is poured out and a ferment analogous to pepsin, the two together dissolving animal matter; so that the office and action of these glands are truly analogous to those of the glands of the stomach of animals. Finally that animal or nitrogenous matter, thus absorbed and digested in the glands, is taken in, and conveyed from cell to cell through the tentacles into the body of the leaf, was made evident by ocular inspection of the singular changes in the protoplasm they contain. So particularly have the investigations been made and so conscientiously recorded, that the account of those relating to one species of Sundew, *Drosera rotundifolia*, fills 277 pages of the English edition, or more than half of the book. After all it ends with the remark: "and we see how little has been made out in comparison with what remains unexplained and unknown." The briefer examination of six other Sundews follows, some of them equally and others less efficient fly-catchers and feeders.

Dionæa is next treated, but with less detail. Indeed, except as to the particular nature of the secreted digesting fluid, there is little in this chapter that had not been made out or already become familiar here. That the secretion has digestive powers, and that it is re-absorbed, along with whatever has been digested, is now proved beyond reasonable doubt. That the motor impulse is conveyed through the cellular parenchyma, and not through the vascular bundles, or spiral vessels, and that the latter do not originate the secretion, as Rees and Wills in a recent paper seem to suppose they must, appears to be shown by the facts, and was antecedently probable. "The wonderful discovery made by Dr. Burdon Sanderson is now universally known: namely, that there exists a normal electrical current in the blade and footstalk, and that when the leaves are irritated the current is disturbed in the same manner as takes place during the contraction of the muscle of an animal." The conclusion here needs to be checked by parallel experiments, to see whether the same reversion of current does not take place whenever a part of any leaf or green shoot is forcibly bent upon itself.

Aldrovanda vesiculosa, of the *Drosera* family, "may be called a miniature aquatic *Dionæa*;" for, as discovered by Stein in 1873, "the bilobed leaves open under a sufficiently high temperature, and when touched suddenly close." Being submerged, their prey is confined to minute aquatic animals. For want of proper material and opportunity, Mr. Darwin was able to follow up only for a little way the observations of Stein and Cohn,—enough, however, to show that it also captures and consumes animals, but perhaps avails itself of the nitrogenous matter only when passing into decay.

Drosophyllum, a rare representative of the order, confined to Portugal and Morocco, grows on the sides of dry hills near Oporto; so that, as to station, it is the very counterpart of *Aldrovanda.* Its leaves are long and slender, in the manner of our *Drosera filiformis*, and are covered with much larger glands. To these flies adhere in vast numbers. "The latter fact is well known to the villagers, who call the plant the 'fly catcher,' and hang it up in their cottages for this purpose." Mr. Darwin found the glands incapable of movement, and their behavior in some other respects differs from that of *Drosera*; but they equally secrete a digestive juice. Insects usually drag off this secretion instead of being fixed on the glands by it; but their fate is no better; for as the poor animal crawls on and these viscid drops bedaub it on all sides, it sinks down at length exhausted or dead, and rests on a still more numerous set of small sessile glands which thickly cover the whole surface of the leaf. These were till then dry and inert, but as soon as animal matter thus comes in contact with them, they also secrete a digestive juice, which, as Mr. Darwin demonstrated, has the power of dissolving bits of coagulated albumen, cartilage, or meat, with even greater readiness than that of *Drosera.*

Mr. Darwin next records various observations and experiments upon more ordinary glandular hairs of several plants. To certain Saxifrages his attention was naturally called, on account of the presumed relationship of *Droseraceæ* to this genus. He declares that "their glands absorb matter from an infusion of raw meat, from solutions of nitrate and carbonate of ammonia, and apparently from decayed insects. To such plants the vast number of little insects caught may not be useless, as they may be to many other plants (tobacco, for instance) with sticky glands, in which Mr. Darwin could detect no power of absorption. The prevalent idea, that glandular hairs in general serve merely as secreting or excreting organs, and are of small or no account to the plant, must now be reconsidered. Those of the common Chinese Primrose (*Primula Sinensis*) although indifferent to animal infusions, were found to absorb quickly both the solution and vapor of carbonate of ammonia. Now, as rain-water contains a small percentage of ammonia, and the atmosphere a minute quantity of the carbonate or nitrate, and as a moderate-sized plant of this primrose was ascertained, (by estimate from a count on small measured surfaces by Mr. Francis Darwin) to bear between 2½ to 3 millions of these glands, it begins to dawn upon us that these multitudinous organs are neither mere excrescences nor outlets, nor in any just sense insignificant.

Mr. Darwin next investigates the densely crowded short glandular hairs, with their secretions, which form the buttery surface of the face of the leaves of *Pinguicula*, the Butterwort. He finds that the leaves of the common Butterwort have great numbers of small insects adhering to them, as also grains of pollen, small seeds, &c.; that most substances so lodged or placed, if yielding soluble matter to the glands, excite them to increased secretion; but that if non-nitrogenous the viscid fluid poured out is not at all acid, while if nitrogenous it invariably has an acid reaction and is more copious; that in this state it will quickly dissolve the muscles of insects, meat, cartillage, fibrin, curds of milk, &c.; that when the surface of a plane leaf is fed, by placing upon it a row of flies along one margin, this margin, but not the other, folds over within twenty hours to envelope them; and when placed on a medial line, a little below the apex, both margins incurve. He concludes "that *Pinguicula vulgaris*, with its small roots, is not only supported to a large extent by the extraordinary number of insects which it habitually captures, but likewise draws some nourishment from the pollen, leaves, and seeds of other plants, which often adhere to its leaves. It is therefore partly a vegetable as well as an animal feeder." The leaves in one or two other species were found capable of greater and more enduring inflection, and the glands excitable to increased secretion even by bodies not yielding soluble nitrogenous matter.

The aquatic type of this family is *Utricularia;* and the bladder-bearing species of this genus are to *Pinguicula* nearly what *Aldrovanda* is to *Dionæa* and *Drósera*—the bladders imprisoning minute aquatic animals, by a mechanism almost as ingenious as that of *Dionæa* itself. Observations of the same kind were made in this country by Mrs. Treat, of Vineland, New Jersey, before Mr. Darwin's investigations were made known. These submerged aquatic stomachs, ever deluged with water, apparently do not really digest their captures, but merely absorb the products of their decay.

The same must in all probability be said of such Pitcher-plants as *Sarracenia* and *Darlingtonia*, which Mr. Darwin merely alludes to at the close of his volume but does not treat of. *Nepenthes*, however, according to Dr. Hooker's investigations, has attained a higher dignity, and converted its pitcher into a stomach. This parallelism, and this higher and lower mode of appropriating organic products by each of the three well-marked carnivorous families of plants, are highly suggestive.

In concluding this notice of a book for which we have no room to do justice—but which is sure to be in the hands of many interested readers—there is somewhat to be said in regard to the discovery of the lure in some of our Sarracenias. We have by degrees to discover our discoverers. In this Journal, only so far back as the number for August, 1873, is a notice of the discovery of a sweet secretion at the orifice of the pitcher of *Sarracenia flava*, by Mr. B. F. Grady, of Clinton, North Carolina (in the article by an oversight called "Mr. Hill"), which effectively lures flies to their destruction. This statement, made in a letter, had been for several months in our hands, awaiting the opportunity of confirmation, when an allusion to the same thing appeared in the English edition of LeMaout and Decaisne's System of Botany, without reference to any source, and on inquiry we learned that the authority for the statement was forgotten. But early in the following year, when the monograph of the order appeared in the last volume of DeCandolle's Prodromus, a reference was found to a paper by Dr. Macbride in the Transactions of the Linnæan Society. His observations (made upon *S. variolaris*), it appears, were communicated to Sir J. E. Smith, read before the Linnæan Society in 1815, and published soon after. They are referred to by his surviving friend and associate, Eliott, in his well-known work, and therefore need not have gone to oblivion, or needed rediscovery here in our days by Mr. Grady and Dr. Mellichamp, the latter greatly extending our knowledge of the subject. Probably the main facts were all along popularly known in the regions these species affect, and where their use as fly-traps is almost immemorial. But the *gist* of these remarks is, that a colleague has just called our attention to an earlier publication than that of Dr. Macbride, viz., an article on "Certain Vegetable Muscicapæ," by Benjamin Smith Barton (one of our botanical fathers), published in Tilloch's Philosophical Magazine, for June, 1812. Among other matters not bearing directly upon this point, he says of *Sarracenia*, without reference to any particular species: "A honeyed fluid is secreted or deposited on the inner surface of the hollow leaves, near their *faux* or opening; and this fluid allures great numbers of the insects which they are found to contain into the ascidia."

Here is earlier publication by three years. Yet we suspect that Dr. Barton knew little about it at first hand, and we find clear evidence that he had not anticipated Dr. Macbride. All his references have an indefiniteness quite in contrast with Dr. Macbride's narrative; he says that "some if not all the species of the genus appear to possess a kind of glandular function," without mentioning those that have it, or the absence of it in the only species growing around him at the north; and he adds that he "was entirely unacquainted with this curious economy . . . when I published the first edition of my Elements of Botany, and even when I printed the appendix (in vol. i) to the second edition of this work." Now his paper is dated September 11, 1811; and the volume referred to, as just printed, is dated 1812. But Macbride states that his observations were chiefly made 1810 and 1811; he corresponded intimately with Eliott, through whom, if not directly, his observations would probably find their way at once to the Philadelphia naturalists. A. G.

3. *The Movements and Habits of Climbing Plants;* by Charles Darwin. Second edition, revised, with illustrations. London: Murray. 1875. pp. 208.—This most interesting treatise was read to the Linnæan Society over ten years ago and published in the ninth volume of its Journal, in 1865. There was a separate issue, which has long been exhausted. It is now carefully re-edited, considerably added to, and reproduced as an independent volume. It will no doubt be much sought after, as the topic and treatment of it are peculiarly fascinating and instructive, and the

book is throughout readable. Mr. Darwin's gift for making things clear without technicalities, is as great as that of many writers for enveloping them in technical obscurity. Having given an account of this essay upon its original appearance, we need only mention the republication, which will be within the reach of all, as an edition is about to be issued by Appleton & Co. A. G

4. *Hæckel's Ziele und Wege der heutigen Entwickelungsgeschichte.*—The controversy carried on by Hæckel in defence of some of his pet theories has gradually assumed a more and more personal character. The criticisms in his *Generale Morphologie* were sharp, but justifiable from his standpoint. In the *Schöpfungsgeschichte*, they had already become sensational. In the *Anthropogenie* his sketches of contemporaries and his analysis of their work assumed a still more unpleasant emphasis; and this has now culminated in a pamphlet entitled "Ziele und Wege der heutigen Entwickelungsgeschichte."

It is difficult to characterize this production without indulging in the same style of epithets which Hæckel uses so freely. From the title we expected one of those brilliant chapters, which, however untrustworthy, are full of suggestions; we were sadly disappointed to find it filled simply with abuse of His, Gœtte, Ludwig, Reichert, Michelis, Agassiz and others.

We shall not fill the pages of this Journal with countercharges or explanation; a man so skilled in coarse invective, who has risen to such a height of intolerance, is proof against anything so tame as fact or argument. This is not the place to refute his absurd claims to omniscience, and his assumptions of immunity for the very offences he so mercilessly condemns. According to Hæckel it is an unpardonable sin for His or Gœtte to give a false interpretation of what they have seen, or for Ludwig and Reichert to differ from him in his explanation of protoplasm; but when he himself, to suit a purpose, deliberately falsifies facts, when he manufactures with names and figures an archetype which never existed, we are called upon to be grateful that a corner of the veil shrouding creation is lifted, and that we are fortunate enough to live at a time when so infallible an interpreter of its mysteries, has taken up his abode at Jena.

In the concluding pages, devoted to Agassiz and Michelis, all the bitterness of his bigotry and dogmatism are poured forth against the latter, while he stoops so low in his attacks on the former as to pick up all the baseless slanders ever circulated by his enemies during his life. With scientific productions like these we have no concern. A few more such criticisms, and Hæckel's claim to be recognized as a true and devoted student of nature will be forgotten. In its place, he will gain, what he seems to seek, the front rank among scientific demagogues. A. AG.

5. *Memoirs of the American Association for the Advancement of Science.* I. *Fossil Butterflies;* by S. H. SCUDDER. 99 pp. 4to, with three plates. Salem, 1875.—The sum of one thousand dollars was given in Aug., 1873, by Mrs. Elizabeth Thompson, of

New York City, to the American Association, to be used, according to the directions of the Standing Committee, for the promotion and publication of original investigations by members of the Association.

The memoir by Mr. Scudder is the first paper published by the Thompson Fund, and is one which well deserves so prominent a place. Mr. Scudder has had especial advantages in this work, having with one or two trifling exceptions, as he states, "either personally inspected all the fossils described within recent times as butterflies, or having procured new and excellent original drawings of them." He has brought together in this volume all that has been published on this group of fossils whether of text or illustration, presenting thus a complete account of our knowledge of these insects. After the detailed descriptions of the genera and species of fossil butterflies, the author discusses various related topics; their comparative age, the probable food plants of Tertiary caterpillars; the present distribution of butterflies most nearly allied to fossil species, and so on. The plates were executed in Paris, and are beautiful examples of the best lithographic work.

IV. Astronomy.

1. *Small Planets recently discovered.*—In the number of this Journal for August last (p. 158), a table of the planets so far as No. 146 was given. Nine planets have been since discovered, making fifteen during the year.

No. 147	was discovered by	Schulhof, at Vienna, July 10th.
148	" "	Prosper Henry, at Paris, Aug. 8th.
149	" "	Perrotin, at Toulouse, Sept. 21st.
150	" "	Watson, at Ann Arbor, Oct. 19th.
151	" "	Palisa, at Pola, Nov. 1st.
152	" "	Paul Henry, at Paris, Nov. 2d.
153	" "	Palisa, at Pola, Nov. 2d.
154	" "	Prosper Henry, at Paris, Nov. 4th.
155	" "	Palisa, at Pola, Nov. 8th.
156	" "	Palisa, at Pola, Nov. 22d.
157	" "	Borelly, Marseilles, Dec. 1.

It has been suggested by Tietjen that No. 152 may prove to be Dike (99). If so the later numbers will need to be changed to correspond. H. A. N.

2. *The Cape Catalogue of* 1,159 *Stars, deduced from observations at the Royal Observatory at the Cape of Good Hope, under the superintendence of* E. J. Stone.—The Royal Observatory at the Cape of Good Hope was established in 1820. The leading idea was to found a first class observatory in the southern hemisphere for work of a character similar to that of the Greenwich Observatory in the northern hemisphere. The observations were to be made with instruments of the same class, and the result to be drawn up in the same form, in order that the whole might constitute two corresponding series, capable of comparison in all their parts. No opportunity of making observations capable of improving our knowledge of the refraction of the atmosphere, was to be

neglected. Under the successive superintendency of Fallows, Henderson, and Sir Thomas Maclear, the latter of whom arrived at the Cape in 1834, and of E. J. Stone, who arrived in 1870, the work of the Observatory has been carried on under very adverse circumstances, and the results thus far accomplished have somewhat disappointed the earlier expectation, and compare unfavorably with those achieved by the energies of Gilliss and Gould. Mr. Fallows was able to publish a small catalogue of star places; Mr. Henderson was able to detect the parallax of *Alpha Centauri*, and to produce a very valuable catalogue of very accurate places of a number of stars. Sir Thomas Maclear seems to have concentrated his energies during many years upon the measurement of an arc of the meridian, of the value of which work there can be but one opinion; but this was allowed to disorganize the other work of the observatory to such an extent that, as Mr. Stone states, he in 1870, found himself with a very limited staff, unexpectedly confronted with the results of 36 years of miscellaneous observations in all stages of reduction, nothing completed, and nothing available for publication and use, without a considerable expenditure of time and labor. Under these circumstances, he has judged it best to pay especial attention to the later years of observation, and has compiled a catalogue of places of 1,159 stars observed in the years 1856 to 1861; all of them made with transit circle, an instrument similar in all respects to the Greenwich instrument, which has been in use since 1851. The Cape Catalogue of Mr. Stone, is accompanied by a comparison of the right ascensions of the clock stars as observed at Greenwich and the Cape of Good Hope, by means of which comparison some systematic errors are brought to light, which are, however, very small in extent, and may be themselves attributed to the effect on the clock of rapid changes of temperature in the evenings during December, January, and February. The latitude of the observatory must, he thinks, still be considered as uncertain.

The printing of the work, which was done at Cape Town, does not suffer by comparison with similar work in England.

C. ABBE.

3. *Observatory in the Pyrenees.*—An observatory has been established on the Pic de Midi, similar to that on the Puy de Dome, and chiefly through the efforts of General Nansouty.—*L'Institut*, Dec. 1.

IV. Miscellaneous Scientific Intelligence.

1. *Reports on the Meteorological, Magnetic, and other Observations of the Dominion of Canada for the calendar year ending December* 31, 1874.—In this volume the Minister of Marine and Fisheries, Honorable A. J. Smith, has given in full a reprint of the tri-daily simultaneous observations made at a large number of stations throughout the Dominion of Canada, together with tables of monthly and annual means, resultant direction, and velocity of the wind, etc. In consideration of the exceedingly small annual

appropriation at the disposal of Professor Kingston, Superintendent of the Meteorological office of the Dominion, it would seem that the contributions to meteorology by that State are highly creditable. Prof. Kingston states that regular weather reports are telegraphed from fifteen Canadian stations to the Weather Bureau at Washington, in exchange for which a few reports are sent from the United States by telegraph, and a large number by mail. The only station in Canada at present furnished with self-recording apparatus is that at St. Johns College, at Winnipeg, where, by private munificence, an anemograph has been set in operation by the Bishop of Ruperts Land. With reference to Montreal, which ranks as one of the chief stations, it is stated that the position of McGill College, on account of its proximity to the mountains, is singularly ill adapted for anemometric observations, on which account an anemometer was, in August, 1874, erected on a pole on the summit of the mountain, and connected by telegraph wires with the recording apparatus in the Observatory which is 550 feet lower down on the mountain. There are in Canada thirty-five stations to which storm warnings are occasionally forwarded from Toronto. It would appear that these storm warnings do not give so much satisfaction in the Canadian ports as do the corresponding ones in the United States; this deficiency is explained by Professor Kingston as due in an important degree to the errors of incompetent observers, or to the failure in the prompt delivery of reports to the central office, but perhaps especially to the neglect of the agents at the drum stations to report the results of all storm warnings; and he makes a suggestion, which it would be highly desirable to carry into effect in the United States, and, indeed, in all countries, to the effect that all light-house keepers, and all other government officials at inland places, as well as on the coast, be required, as a part of their regular duty, to report promptly by mail, in a very brief manner, the circumstances attending any gale that may occur in their neighborhood. Among the appendices to Professor Kingston's report, is the annual report of the Director of the Observatory at Quebec, who states that the new observatory and house were finished early in May, and the instrument, etc., were removed thither. This observatory contains rooms for the equatorial, the transit and computing room and photographic, and is directly adjacent to the dwelling of the Director, Commander E. D. Ashe. The principal work of the equatorial consists in taking photographs of the sun's surface from which to determine the time of rotation, inclination of its axis, etc. The principal routine work of the observatory, in a commercial point of view, is to give the correct time to the shipping. The time ball is now dropped by electricity, and the method has been brought to a considerable state of perfection. Commander Ashe has also entered heartily into the work of determining latitudes and longitudes of points in the Dominion. C. A.

2. *Mt. St. Elias.*—The recent measurements of Mr. W. H. Dall, Acting Assistant U. S. Coast Survey, of the height of Mt. St. Elias, make it 19,464 feet. The memoir,—a part of the Coast Survey

Between Yokohama and Honolulu the depth is remarkably uniform, averaging 2,858 fathoms, and the material of the bottom is Report for 1875—is illustrated by a map and also a plate containing two views of the Mountain. The views, taken at distances of 53 and 24 miles, evidently have the vertical scales very greatly increased, as compared with the horizontal, but how much is not stated.

3. *Sea-bottom and Zoology of the deep sea: the Challenger's Observations;* by WYVILLE THOMSON.—A gigantic Hydroid was obtained June 17th, in the North Pacific, 34° 37′ N., 140° 32′ E., at a depth of 1,875 fathoms, where the temperature was 1°·7 C. and the bottom gray mud. The species seemed to belong to *Monocaulon* of Sars, a Corymorpha-like solitary polyp; it measured from tip to tip of the expanded tentacles 9 inches, and the height of the hydroid was 7 feet 4 inches. Another was taken July 5, in 37° 41′ N., 177° 4′ W., at 2,900 fathoms, the bottom red clay, but with manganese nodules, the weight of which tore the trawl. The hydroid is too delicate in texture to bear the rough change from the bottom to the surface. The tentacles of the proximal range are about 100 in number and 4 inches long. The sporosacs are in close tufts at the base of the tentacles. This gigantic *Corymorphoid* was associated on June 17th, with Ophidoids, Macrurids, Scopellids, several Gasteropods, Crustaceans related to *Dorippe*, *Galathea*, *Caridids*, and a fine *Scalpellum*, a few Annelids, many Echinoderms (*Brisinga*, *Phormosoma*, *Ophiurids*, *Holothurids*), and on July 15th, there were some *Aphroditids*, a sea-urchin related to *Diadema*, Holothurids, sponges.

The clayey material of the bottom, brought up June 17, was in a peculiar concretionary state, and bored by an Annelid of the Aphrodite group, some of which were still in the burrows.

In a sounding of June 28th, of 2,800 fathoms, a Rhizopod-like form was obtained, between the Radiolarians and the Foraminifers, its test siliceous as in the former, but the shape as in the latter; their tests were extremely abundant in the "red-clay." There were also obtained a *Scalpellum*, a number of Annelids, Echinoderms of the genera *Pourtalesia*, *Archaster*, *Brisinga*, *Antedon*, a *Cornularia*, specimens of *Fungia symmetrica*, some *Actiniæ*. On July 2d, in 2,050 fathoms, the bottom was a light brownish ooze, with many *Globigerina* shells; several specimens of an undescribed *Hyalonema* were brought up.

The cold water which fills up the trough of the Pacific is regarded by Professor Thomson "an indraught from the Southern Sea," as in the Atlantic; and in both oceans the bottom water is constantly moving northward. The temperature of the water for the first thousand fathoms in the Pacific, in the corresponding latitude of 35° N., is much lower than in the Atlantic. Further, in the Atlantic the temperature sinks gradually, though very slightly, through the last thousand fathoms to the bottom, while in the Pacific, the minimum temperature of 1·7 C. is reached at a depth not greater than 1,400 fathoms, and from that depth to the bottom the temperature is the same.

"red clay," somewhat grayer than the typical "red clay," containing some pumice, numerous siliceous shells, the proportion of which increases with the depth, and scarcely a trace of carbonate of lime (although the water swarms with "ooze-forming" Foraminifers). The pumice was often penetrated with peroxide of manganese, and concretions of the same oxide were abundant in the "red clay." These concretions are rounded or mammillated, fibrous-concentric in structure, and often have a nucleus of some foreign body, as pumice, a shark's tooth, or some other organic relic; and in one case a fragment of a Hexactinellid sponge was preserved as a beautiful fossil at the middle. "The singular point is the amount of this manganese formation and the vast area which it covers." Life was found to be, "although not very abundant in species by no means meagre," in the North Pacific at depths between 2.000 and 3,000 fathoms, all the larger invertebrate groups being represented. In one dredging, at a depth of 3,125 fathoms, a small sponge was obtained, a species of *Cornularia*, an *Actinia*, an Annelid in a tube and a Bryozoon. "We were again struck with the wonderful uniformity of the fauna at these depths—if not exactly the same species, very similar representatives of the same genera existing in all parts of the world." —*Extracts from articles in Nature of Oct.* 28 *and Nov.* 25.

The Challenger arrived at Valparaiso November 19th, on her way home.

4. *Report of an Expedition up the Yellowstone River, made in* 1875; by Lt. Col. J. W. Forsyth and Lt. Col. F. D. Grant, under the orders of Gen. P. H. Sheriden. 17 pp. 8vo, with a map. Washington, 1875.—This expedition succeeded in navigating the Yellowstone River to a distance of 483 miles above its mouth, the only obstacles to farther progress being the excessively rapid current. It was found that the water of the Yellowstone is deeper than that of the Missouri, above the point where the two rivers join. Some interesting views accompany the report, and also a large map of the river, by Lieut. R. E. Thompson.

5. *Preliminary Report of Explorations in Nebraska and Dakota in the years* 1855, '56, '57*;* by Gen. G. K. Warren, U. S. A. 125 pp. 8vo. Washington, 1875.—This is a reprint of the report of Gen. Warren, originally published in 1858, and noticed in this Journal II, xxvii, 378. The present volume is issued in view of the general interest now felt in the Black Hills country, the original report being practically inaccessible.

6. *Atti della Societa Toscana di Scienze Naturali Residente in Pisa.* Vol. I. Parts 1 and 2. 146 pp. roy. 8vo. Pisa. 1875.—These first publications of the Tuscan Society of Science in Pisa, contain papers on the mammalian fauna of the Pliocene of Tuscany, by C. I. F. Major; on the fishes of the same by R. Lawley; on Eocene corals of Friule by D'Achiardi; on the natrolite (savite) and analcite of Pomaja, by D'Achiardi, and other papers geological and zoological, by Meneghini, De Stefani, Baraldi, Richiardi, with one botanical, Sulla teoria Algolichenica, by G. Arcangeli.

OBITUARY.

Emile Kopp, Professor of Chemistry in the Polytechnic School of Zurich, died on the 30th of November at the age of fifty-nine years. He was an Alsatian by birth, and held a chair in the University of Strasbourg previous to 1848. He took an active part in the revolution of that year, and was one of the Deputies who escaped to Switzerland at the time of Louis Napoleon's *coup d'etat.* While residing in Switzerland he was appointed Professor of Chemistry at Lausanne, but he left the country voluntarily, with the other French exiles, when their rendition was demanded by the French government. Passing into England, Kopp supported himself for several years as a private tutor at Manchester, and at the same time familiarized himself with the great chemical industries of that vicinity. The influence of his sojourn in England was strikingly manifest throughout his subsequent career. After the lapse of several years he was permitted to return to France on the parol of one of the Senators of that period (probably M. Dumas) who pledged himself that the returned exile should in no way interfere with the imperial government. On reaching Paris, Kopp opened a private laboratory for instruction in applied chemistry, which was maintained for several years, and was always filled with students. From this laboratory he was called to the charge of extensive works for the manufacture of steel at Saverne, in the east of France, which place he left some years later to assume the chair of applied chemistry in the University of Turin, whence he was soon called to Zurich.

For many years Kopp exhibited great literary activity, and he is probably best known to the generality of chemists from his remarkable compilations relating to the history and progress of the coal-tar colors and of the madder colors. He was largely instrumental in writing Hofmann's famous report on the Chemical Products and Processes of the International Exhibition of 1862, as was duly acknowledged by Prof. Hofmann. This report, as is well known, has served as a model upon which most subsequent reports upon chemical matters have been based. But, in spite of much writing, he accomplished a great deal of work in the way of research, notably in respect to the coloring matters just mentioned, and in other departments of calico-printing. He devised novel processes for making soda from salt, and for the recovery of sulphur from soda-waste, and published numerous observations upon a great variety of subjects.

His familiarity with the methods and processes of technical chemistry, as applied in different countries, was very great, and his judgment of them was singularly sound and impartial. He labored untiringly to inform himself of all improvements and discoveries in the domain of chemical technology, and was doubtless at the time of his death one of the best teachers of applied chemistry that has ever lived.

Wheatstone.—Sir Charles Wheatstone died at Paris, on the 19th of October, at the age of seventy-three years.

STORM OF FEB. 5-8 1870.

PLATE I

Punderson & Crisand N. Haven, Conn

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

Art. VI.—*Sir William Edmond Logan.**

On the 22nd of June, at Castle Malgwyn, Llechyrd, South Wales, Canada's veteran geologist passed from his labors. For several years his health had been failing, and he felt more and more the need of rest and change of climate. Accordingly, in August, 1874, he crossed to the mother country, intending to pass the winter there, and then to return to his work in the spring. But rest and a more genial clime were unavailing, and now—kindest of friends, most indefatigable of workers for science and for his country—he is no more!

William Edmond Logan was born at Montreal, in 1798. He was of Scottish parentage, and his father, after a residence of many years in Canada, returned to Scotland, and purchased an estate near Stirling, known as Clarkstone. His education was begun at Mr. Skakel's school, in this city, and completed at the High School and University of Edingburgh.

On leaving college he betook himself to mercantile pursuits, and we find that in 1818 he entered the counting-house of his uncle, Mr. Hart Logan, of London. Here he remained for about ten years, and here, it is said, he first became fond of geology, making geological excursions into the country whenever opportunity offered.

In 1829 he paid a visit to Canada; but, returning the same year, took up his residence at Swansea, in South Wales, where he was appointed manager of a copper-smelting establishment, and of coal mines, in which an uncle of his was interested. In

* Obituary notice read before the Natural History Society of Montreal, October 25th, 1875.

1834, he made a tour through France and Spain, visiting many of the mines in the latter country, and making many observations on the geology of the regions through which he passed. In 1838, his uncle dying, Mr. Logan resigned his position at Swansea. But the nine years he spent here were well-spent years; for not only had he gained a practical knowledge of mining and metallurgy, which afterwards proved of the greatest value to him, but had done a large amount of very excellent geological work—work which caused Dr. Buckland, of Oxford, to say of him, "He is the most skillful geological surveyor of a coal-field I have ever known." During his stay at Swansea, he was an active worker for the interests of the Royal Institution of South Wales. He was Honorary Secretary and Curator of the geological department, and the Institution is indebted to him for valuable collections of minerals and metallurgical products, besides books, drawings and laboratory apparatus. The whole of his geological work in South Wales he placed gratuitously at the disposal of the Ordnance Geological Survey of Great Britain, and it was not only gladly accepted, but published "without alteration," and made the basis of future work in that region. Concerning it, Sir H. T. De la Beche afterwards wrote as follows:

"Prior to the appearance of the Geological Survey in that part of the country, Mr. W. E. Logan had carefully investigated it, and at the meeting of the British Association for the Advancement of Science, held at Liverpool in 1837, he exhibited a beautifully executed map of it.

"The work on this District being of an order so greatly superior to that usual with geologists, and corresponding, in the minuteness and accuracy of its detail, with the maps and sections executed by the Ordnance Geological Survey, we felt desirous of availing ourselves of it, when Mr. Logan most handsomely placed it at our disposal. Having verified this work with great care, we find it so excellent that we shall adopt it for that part of the country to which it relates, considering it but fair and proper that Mr. Logan should obtain that credit to which his labors so justly entitle him.

"His sections are all levelled and measured carefully with proper instruments, and his maps are executed with a precision only as yet employed, except in his case, on the Ordnance Geological Survey; it being considered essential on that survey, for the right progress of geology, and the applications to the useful purposes of life, that this accuracy and precision should be attained."

In 1840, Logan read a paper before the Geological Society of London, in which he explained, for the first time, the true relation of the *Stigmaria* underclays to the overlying beds of coal, showing that the underclay was the soil in which the plants grew which were afterwards converted into coal. Of the 100 thick and thin coal-seams in the South Wales coal-field, he found that not a single one was without an underclay, and the inference appeared to be that there was some essential connection between the production of the one and the existence of the other. "To account," said he, "for the unfailing combination by drift, seems an unsatisfactory hypothesis; but whatever may be the mutual dependence of the phenomena, they give us reasonable grounds to suppose that in the *Stigmaria ficoides* we have the plant to which the earth is mainly indebted for those vast stores of fossil fuel which are now so indispensable to the comfort and prosperity of its inhabitants."

So much did he become interested in this subject that in the following year (1841) he crossed to America, and visited the coal-fields of Pennsylvania and Nova Scotia, in order to ascertain whether the same conditions existed there. Such he found to be the case; and in the following spring he read an interesting paper before the Geological Society, the object of which, to use his own words, "was to state the occurrence immediately below the coal-seams of America of the same *Stigmaria* beds as had been observed below those of South Wales, and to show the importance of this prevailing fact." Shortly after his return from America, he also visited coal-seams in the neighborhood of Falkirk, Scotland, there too, finding the *Stigmaria* clays beneath the coal.

It was during his visit to Nova Scotia, in 1841, that he discovered in the Lower Coal-measures of Horton Bluff the footprints of a reptilian animal—a discovery which, perhaps, failed to attract as much attention as it deserved, although it was the first instance in which any trace of reptiles had been detected as low down in the geological scale as the Carboniferous. The winter of 1841–42 was also spent in Canada, and the facts were obtained for a paper on the packing of ice in the St. Lawrence, which was subsequently read before the Geological Society of London.

Such, briefly, was the career of Logan previous to his appointment as Director of the Geological Survey of Canada. Already he had acquired a reputation in Britain as a geologist, and had given himself the best of trainings for the work upon which he was about to enter on this side of the Atlantic. But what was meantime passing in Canada? * * * *

"In July, 1841, in the first United Parliament, a petition from the Natural History Society of Montreal, praying for aid to carry out a systematic geological survey of the Province, was presented by Mr. B. Holmes. It was referred to a select committee consisting of Messrs. Holmes, Neilson, Quesnel, Merrit, and the Hon. Mr. Killaly, but it was not reported on. A similar petition was presented by Mr. Black, from the Literary and Historical Society of Quebec, which was read. The government took up the matter, and on the motion of the Hon. B. Harrison, the sum of £1,500 sterling for the purpose of a survey was introduced into the estimates."*

Lord Sydenham dying in 1841, it fell to his successor, Sir Charles Bagot, to appoint a Provincial Geologist. Sir Charles referred the matter to Lord Stanley, Secretary of State for the Colonies, and His Lordship, on recommendation of Murchison, De la Beche, Sedgwick, and Buckland, offered the position to Mr. Logan in the spring of 1842.

Logan was now thoroughly in love with geology, and seeing in Canada the grandest of fields for original research, at once accepted. Still he well understood the difficulties which lay before him, and shortly afterwards addressed the following words to De la Beche: "You are aware that I have been appointed by the Provincial Government of Canada to make a Geological Survey of that Colony. The extent and nature of the territory will render the task a most laborious one; but I am fully prepared to spare no exertion of which I am capable to render the work, when it is completed, satisfactory to those who have instituted the examination and creditable to myself. * * No one knows better than yourself how difficult it would be for one person to work with effect in all the branches of so extensive a subject. To carry out the field-work with vigor, to reduce all the sections with the requisite degree of accuracy, and map the geographical distribution of the rocks, to collect minerals and fossils, and to analyze the one, and by laborious and extensive comparisons, to determine the geological age of the other, is quite impossible without a proper division of labor. * * In Canada, all the expensive means of palæontological comparison have yet to be brought together. There is no arranged collection of fossils, and no such thing as a geological library to refer to."

Arriving in Canada late in August, 1842, Logan devoted several months to making a preliminary examination of the country, and to collecting information with regard to the topographical work which had been accomplished. This was done entirely at his own expense. In December, he returned to England to fulfill engagements there, but came out again in the following spring. During his visit to the old country, he was so fortunate as to secure the services of Mr. Alexander Murray, a gentleman who afterwards proved himself an invaluable assistant and friend, and who has contributed largely to our knowledge of the geology of Canada, and, more recently, to that of Newfoundland.

*From Scobie's Canadian Almanac for 1851.

Reaching Halifax on the 20th of May, Logan spent several weeks in examining portions of the coal-fields of Nova Scotia and New Brunswick, and it was at this time that he made his section of the Coal Measures at the South Joggins, which, as has been truly said, is "a remarkable monument of his industry and powers of observation." It gives details of nearly the whole thickness of the coal formation of Nova Scotia, or 14,570 feet, including 76 beds of coal and 90 distinct *Stigmaria* underclays. Shortly after his visit to the Joggins, he wrote to a friend as follows: "I never before saw such a magnificent section as is there displayed. The rocks along the coast are laid bare for thirty miles, and every stratum can be touched and examined in nearly the whole distance. A considerable portion has a high angle of inclination, and the geological thickness thus brought to view is very great. I measured and registered every bed occurring in a horizontal distance of ten miles, taking the angle of dip all the way along." And again, in a letter to De la Beche written in the spring of 1844, referring to the Joggins section, he says: "Since my return from field-work, I have reduced all the measurements and made out a vertical column. It occupies fifty-four pages of foolscap, closely written, and you will be astonished at the details in it."

Reaching Gaspé early in July, the summer and autumn were spent in making an examination of the coast, while Mr. Murray was at work in the Upper Province, examining the country between Lakes Huron and Erie. The Gaspé peninsula had been selected by Mr. Logan as the field for his first operations, as it was thought that outlying patches of the Carboniferons might be found to exist there, and the government was especially anxious to ascertain whether there was any truth in the reported occurrence of coal.

The following season, the work in Gaspé was continued, the Director being this time accompanied by Mr. Murray, who, in 1845, again carried on the work, while Mr. Logan was engaged in explorations on the Upper Ottawa and Mattawan. Altogether, during the three seasons, 800 miles of the Gaspé coast were examined, and several sections made across the peninsula, from the St. Lawrence to Bay Chaleur. No coal was found, but many geological facts of importance were accumulated, and a large amount of topographical work accomplished in what was previously almost a *terra incognita.*

"Living the life of a savage, sleeping on the beach in a blanket sack with my feet to the fire, seldom taking my clothes off, eating salt pork and ship's biscuit, occasionally tormented by mosquitoes,"—such is the record which Logan has left us of his Gaspé life, the foretaste of what was to be endured for many years. From early dawn till dusk he paced or paddled, and yet his work was not finished, for while his Indians—often his sole companions—smoked their pipes round the evening fire, he wrote his notes and plotted the day's measurements.

To give details of his work during the many remaining years of his life would be to write a book; and all that we can do here is to trace briefly what his movements were, at the same time calling special attention to those of his labors which have given him a world-wide fame.

The summer of 1846 found him studying the copper-bearing rocks of Lake Superior. These he showed to consist of two groups of strata, the "upper" and the "lower," the latter of which was seen at Thunder Bay to rest unconformably upon chloritic slates belonging to an older series, to which the name of Huronian was subsequently given. This older set of rocks, which he had already observed, in 1845, on Lake Temiscamang, he had ample opportunity of studying in 1848, when he devoted several months to an examination of the Canadian coast and islands of Lake Huron, where the formation attains—as shown by Murray—a thickness of 18,000 feet.

The seasons of 1847 and 1849, and a portion of that of 1848, were employed in studying the rocks of the Eastern Townships. Part of these were shown to be a prolongation of the Green Mountains of Vermont, and to consist of altered Silurian strata instead of "Primary strata," as was previously supposed by American geologists. In 1849 also, a short time was spent in an examination of the rocks about Bay St. Paul and Murray Bay, where coal had been reported to exist. The member for Saguenay County had previously made application to the Legislature for means to carry on boring operations in the vicinity of Bay St. Paul, but before his request was granted it was deemed advisable to obtain the opinion of the Provincial Geologist. By this means the Government was saved a large and useless expenditure of money.

In 1850 an examination was made of the gold-bearing drift of the Chaudière, and the auriferous district found to extend over an area of between 3,000 and 4,000 square miles. Most of the year, however, was devoted to the collection of specimens for the London Exhibition of 1851, at which Mr. Logan acted as Juror. His visit to England at this time must have been for him an agreeable change. After a lapse of eight years to meet again with men like De la Beche, Murchison and Lyell, to hear from their own lips of the strides which science had been making, and in turn to tell of all that he had himself seen and done; surely this was a treat that none but the scientific man can understand who has long been well-nigh deprived of the society of brother scientists. For him, however, there was little relaxation from labor, for he toiled early and late in order that the

Canadian minerals might be displayed to the best advantage. And every one knows the result—the collection elicited universal admiration, and Mr. Logan received a highly complimentary letter of thanks from the Prince Consort, and was elected a Fellow of the Royal Society, his name having been proposed by Sir Roderick Murchison.

Returning to Canada in August, before the close of the Exhibition, his explorations were renewed with undiminished vigor, and the remainder of the season devoted to an examination of the rocks in the county of Beauharnois, where the Potsdam sandstones had afforded those curious tracks of crustaceans to which Owen gave the name of Protichnites, and to a further study of the Chaudière gold region. During the winter he again visited England to attend to the distribution of a portion of the Exhibition collection which was to be left there, and to see to the return of the remainder.

In 1852 an examination was made of a strip of country on the north side of the St. Lawrence, extending from Montreal to Cape Tourmente below Quebec. The distribution of the fossiliferous rocks was accurately determined, and several excursions were made into the hilly "metamorphic country" to the north. In his report on this season's operations, published in 1854, Logan for the first time designated the rocks comprising these hills as the "Laurentian series," substituting this for "metamorphic series," the name which he had previously employed, but which, as he says, is applicable to any series of rocks in an altered condition.

The following season was spent among the Laurentian hills of Grenville and the adjoining townships, a field which proved so attractive that he afterward returned to it in 1856 and 1858. Nearly the whole of 1854 was occupied in making preparations for the Exhibition which was to take place at Paris in the following year, and to which Mr. Logan was to go as one of the Canadian Commissioners. It was in the autumn of 1854 also, that a select committee was appointed by the Canadian Government to inquire into the best method of making the information acquired by the Geological Survey more readily accessible to the public. A lengthy report on the subject—indeed on the entire working of the Survey—was published, and the evidence which it contains is of a most flattering character, both as regards the Director and those associated with him.

Then came the Paris Exhibition of 1855, at which the representation of the economic minerals of Canada was so complete and the arrangement so admirable that the collection attracted universal attention. This in itself Logan would have regarded as amply repaying him for his trouble; but greater honor was in store for him. The Imperial Commission presented him with the grand gold medal of honor, and the Emperor of the French made him a Chevalier of the Legion of Honor. Early in the following year (1856) he was knighted by Queen Victoria, and received from the Geological Society of London the Wollaston Palladium Medal in recognition of his distinguished labors in geology. Long previous he had won the confidence and esteem of his fellow-countrymen in Canada, but this seemed to be a fitting time to testify to him their appreciation of his worth. Accordingly, on his return to Montreal, the citizens presented him with a testimonial on which were engraved the words:

"In commemoration of his long and useful services as Provincial Geologist in Canada, and especially his valuable services in connection with the Exhibition of all Nations in London in 1851, and in Paris in 1855, by which he not only obtained for himself higher honor and more extended reputation, but largely contributed in making known the natural resources of his native country."

The Natural History Society of Montreal presented him with an address, and made him an honorary member, while the members of the Canadian Institute of Toronto, of which Sir William was the first President, had his protrait painted and hung up in their hall. They also presented him with an address expressive of their affectionate esteem and respect. Sir William's reply to this was so full of feeling, and so highly characteristic, that we give a portion of it: "Whatever distinctions," said he "may be bestowed on us at a distance, it is upon the respect, esteem, and confidence shown us at home, that our happiness and satisfaction must chiefly depend. I can assure you with sincerity that the honor conferred upon me, when you elected me the first President of the Institute, was one highly prized, although the circumstances of a distant domicile, and the intent pursuit of the investigations with which I am charged, rendered it extremely difficult for me to be of much use in your proceedings. . . . . It is a fortunate circumstance for me that my name should be connected with an act of grace on the part of Her Majesty, which serves to confirm your feeling in regard to the fact that as Canadians we enjoy a full share in the honors and privileges of British subjects. And I am proud to think that it was perhaps more because I was a Canadian, in whom the inhabitants of the Province had reposed some trust, that the honor which has been conferred upon me by Her Majesty was so easily obtained. That I am proud of the honors which have been bestowed upon me by the Emperor of France, in respect to my geological labors, and also by my brother geologists in England, there can be no doubt. But I have striven for these honors because I have considered they would tend to promote the confidence which the inhabitants of the Province have reposed in me, in my endeavors to develop the truth in regard to the mineral resources of the Province; and in this work none could have been more interested in my success than the members of this Institute."*

In August, 1857, the American Association for the Advancement of Science held its annual meeting in Montreal, and for several months previous Sir William was hard at work getting his museum in readiness to receive his brother geologists. Owing largely to his untiring exertions, the meeting was a most successful one. He himself read two interesting papers, one on the "Huronian and Laurentian Series of Canada," and another on the "Sub-division of the Laurentian Rocks of Canada" After the business of the Association was concluded, accompanied by Professor Ramsay, who had come over to represent the Geological Society of London, and Professor Hall, he made a Geological tour through New York State. Returning from this trip, he spent the autumn months among the Laurentian Rocks of Grenville. Here too, as already mentioned, he continued to work during the season of 1858.

For several years after this, his time was much taken up with the preparation and publication of the *Geology of Canada* and its accompanying Atlas, the former of which appeared in 1863, and the latter in 1865. Before these could be completed, however, many facts had to be added to the stock already obtained, and besides a large amount of geological work among the Laurentian rocks of Grenville and the rocks of the Eastern Townships, a personal examination of many parts of the country, as well as of portions of the New England States, was rendered necessary.

In 1862, Sir William was again present, in the capacity of Juror, at the London International Exhibition, and again displayed a large and interesting collection of economic minerals. Another opportunity of seeing his scientific friends in Britain was also afforded him in 1864, when he went to London to superintend the engraving of the Atlas already mentioned. In 1866, a geological collection was again prepared for the Paris Exhibition of 1867, and Sir William worked so closely in getting up a geological map to accompany it that he is said to have nearly ruined his eyesight. 1868 found him once more on this side of the Atlantic, hard at work in the Pictou coal-field, and the results of this season's work constitute the last of his reports. In 1869, he resigned his appointment to Mr. Selwyn, the present Director of the Survey.

The few remaining years of his life were occupied chiefly with a study of the rocks of the Eastern Townships and portions of New England: but, unfortunately, the conclusions at which he arrived concerning them were not published.

* Can. Journal, New Series, vol. i, p. 404.

No man has done as much as Sir William Logan to bring Canada before the notice of the outside world, and no man is more deserving of being held in remembrance by the people. Just as statesmen or generals have risen up at the moment of greatest need to frame laws or fight battles for their country, so Sir William appeared to reveal to us the hidden treasures of Nature, just at a time when Canada needed to know her wealth in order to appreciate her greatness For rising nations require to know what their resources are. He possessed rare qualities—qualities, which, combined, eminently fitted him for his work. He was strong in body, of active mind, industrious and doggedly persevering, painstaking, a lover of truth, generous, possessed of the keenest knowledge of human nature, sound in judgment, but always cautious in expressing an opinion.

He belonged to that school of geologists—unfortunately not so numerously represented as it ought to be—whose motto is, "Facts, then theories," and was wholly above rasping down facts to make them fit theories. As a consequence, he rarely had to un-say what was once said; and this is why he so thoroughly gained the public confidence. So long as he felt that he was in the right, he held to his own views as tenaciously as did ever any true Scot; but if shown to be in the wrong, he knew how to surrender gracefully.

Those who have clambered with him over our log-strewn Laurentian hills know well what were his powers of endurance. He never seemed to tire, never found the days long enough. His field-books are models of carefulness, replete with details, and serve as an example of the painstaking way in which he did all his work. They were written in pencil, but regularly inked in at night, when the camp fire was often his only light. In addition to his field-book proper, he frequently kept a diary, and delighted to jot down little every-day occurrences, or sketch objects of interest—for the hand that could so well wield a hammer, could also guide a pencil and produce drawings of no mean artistic skill. His descriptions of his backwoods experiences are often very amusing, and we cannot resist giving a specimen. He had been traveling through the forest for two months and had suddenly come upon the house of a settler called Barton, whose good wife was justly alarmed when Sir William and party entered her dwelling. Sir William describes his appearance, on this occasion, as follows:—"We are all pretty-looking figures. I fancy I cut the nearest resemblance to a scare-crow. What with hair matted with spruce gum, a beard three months old, red, with two patches of white on one side, a pair of cracked spectacles, a red flannel shirt, a waistcoat with patches on the left pocket,—where some sulphuric acid, which I carry in a small vial to try for the presence of lime in the rocks, had leaked through,—a jacket of moleskin, shining with grease, and trowsers patched on one knee in four places, and with a burnt hole in the other: with beef boots—Canada boots, as they are called—torn and roughened all over with scraping on the stumps and branches of trees, and patched on the legs with sundry pieces of leather of divers colors; a broad-brimmed and round-topped hat, once white, but now no color, and battered into all shapes. With all these adornments, I am not surprised that Mrs. Barton, speaking of her children, and saying that here was "a little fellow frightened of nothing on earth," should qualify the expression by saying, "but I think he's a little scared at *you*, Sir."

It was not alone in the field that Sir William was busy. His office work was often most arduous, and during the earlier years of his directorship, in addition to preparing his annual report, he even kept the accounts, entering every item of expenditure, so that he could at any time show exactly how every penny of the public money placed at his disposal had been spent. He also tells us that, with his own hands, he made, at that time, four manuscript copies of the Annual Report of Progress, often reaching more than one hundred printed pages—one copy for the Government, one for the House of Assembly, one for the Legislative Council, and one for the printer.

His manner of living was simple as it was solitary. Like his four brothers, he never married, nor does he seem to have formed many intimate friendships. Still every one who knew him loved him and respected him, and if you go the length and breadth of all the land, you will everywhere hear his praises, alike from rich and poor.

He peculiarly possessed the power of inspiring others with his own enthusiasm; not only those in his employ, but even uneducated farmers and backwoodsmen—men who, as a rule, are rather sceptical about the advantages to be derived from geology.

Though possessed of private means, he spent little upon himself; not that he was parsimonious, but he cared not for fashion or luxury. But with him Science never pleaded her needs in vain. The first grant of the Legislature, to make a geological survey of the Colonies, was £1,500—an amount which, Sir William quaintly remarked, was but a drop of what would be required to float him over twenty-five degrees of longitude and ten of latitude. This was, of course, very soon spent, and not only this, but at the end of the second year the Survey was £800 in his debt, and he had no guarantee whatever that his money would be returned to him. Since then the Survey has been constantly indebted to him for books, instruments, and other aids, and the building on St. James street, now used for office purposes, was built by him, two years ago, and rented to the

Government for about half the amount which he could have obtained from other tenants. To Logan also, McGill University owes much; for, in 1864, he founded and endowed the "Logan Gold Medal" for an honor course in geology and natural science, and, in 1871, gave $19,000, which, together with $1,000 given by his brother, the late Mr. Hart Logan, forms the endowment of the "Logan Chair of Geology."

Since resigning his position as Director of the Geological Survey, he has carried on explorations at his own expense, and, at the time of his death, arrangements had been nearly completed for putting down a bore-hole in the Eastern Townships, at a cost of $8,000; as he thought that this would enable him to prove the truth of his views with regard to the age of the metamorphic rocks there. . . . .

Sir William was the first to give us any definite information about those wondrous old Laurentian rocks which form the backbone of our continent. He showed us that they were older than the Huronian, and that they consisted of a great series of metamorphosed sedimentary rocks, which are divisible into two unconformable groups, with a combined thickness of not less than 30,000 feet. The great beds of limestone which he found in the lower series, the plumbago, the iron ores, the metallic sulphurets, all seem to point to the existence of life in the Laurentian days; but the discovery of *Eozoon Canadense* made conjecture give place to certainty. Now we *know* that the world of that far-off time was not a lifeless world. Life, whatever that may be, had been joined to matter.

The first specimens of *Eozoon* were found by Dr. James Wilson, of Perth; but at the time of their discovery were regarded merely as minerals. In 1858, however, Mr. J. McMullen, of the Geological Survey, discovered other specimens, the organic origin of which so struck Sir William that in the following year—four years before their true structure and affinities were determined by Dawson and Carpenter—he even exhibited them as fossils at the meeting of the American Association.

In widely extending our knowledge of the early geological history of the earth, Sir William has done a great work; indeed this may be regarded as his greatest work. Its importance has everywhere been recognized, and the name Laurentian, which he chose for the rocks at the bottom of the geological scale in America, has crossed the Atlantic, and is now applied to the homotaxial rocks of Europe. Sir Roderick Murchison, who dedicated the fourth edition of "Siluria" to Sir William Logan, even substituted Laurentian for "Fundamental Gneiss," the name which he had given to the rocks of the West Highlands of Scotland. "I at first," says Murchison, "termed them 'Fundamental Gneiss,' and soon after, following my distinguished friend

Sir William Logan, I applied to them his term, 'Laurentian,' and thus clearly distinguished them from the younger gneissic and micaceous crystalline rocks of the Central and Eastern Highlands, which were classed as metamorphosed Lower Silurian."

Logan was not a voluminous writer, and during the later years of his life writing was a great effort to him. Occasional papers from his pen have appeared in the *Transactions of the Geological Society* of London, in the *Canadian Naturalist* and the *Canadian Journal*, and some of these have already been referred to; but most of what he has written is to be found in the *Reports of Progress* annually submitted to the Government, and in that invaluable book, the *Geology of Canada*, which is, to a large extent, a digest of what is contained in the reports published previous to 1863. He sometimes expressed himself quaintly, but everything he wrote is clear and exceedingly concise.

In addition to being a Fellow of the Royal Society and of the Geological Societies of London and Paris, he was a member of numerous other learned societies both in Europe and America. At the time of his death, and for many years previous, he was one of our Vice-Presidents; but though frequently solicited to accept the office of President, he always declined,—not on account of any lack of interest in the Society, but he felt his time was too fully occupied to permit of his successfully discharging the Presidential duties. We have already alluded to some of the medals which were awarded to him; but it may be mentioned that altogether he was the recipient of more than twenty, including two from the Royal Society.

And now, in concluding, let me say to you, my friends, if you would do honor to the memory of that noble old man, who fought so long, so bravely, for his country, for science, for you, then honor the cause for which he fought: strive with all your might to advance the interests of that cause, and to raise up a superstructure befitting the solid foundation which Logan has laid. He himself even hoped to build the superstructure; but his anticipations were not realized, for life was not long enough, and we must take up the mantle which he has dropped.

B. J. HARRINGTON.

ART. VII.—*On Recent Researches in Sound;* by WM. B. TAYLOR.

[Continued from page 41.]

IV.

THE communication of Professor Reynolds "On the Refraction of Sound by the Atmosphere," is in two parts; the first of which considers "The effect of Wind upon Sound," and the second part "The effect of variations of Temperature." The experiments were all made in "a flat meadow of considerable extent;" and the apparatus employed "consisted of an electrical bell mounted on a case containing a battery. The bell was placed horizontally on the top of the case, so that it could be heard equally well in all directions; and when standing on the ground, the bell was one foot above the surface." An anemometer was also used to determine the velocity of the wind. (Proceedings of the Royal Society; republished in the L. E. D. Phil. Mag., for July, 1875, vol. l, p. 67.)

The experiments were made on four different days, the 6th, 9th, 10th, and 11th of March, 1874; and on the last two days the ground was covered with snow, which furnished an opportunity of comparing the effect of different surfaces on the range of Sound. Additional experiments were made on the 14th of March.

[1.] "On all occasions the effect of wind seems to be rather against distance than against distinctness. Sounds heard to windward [that is *against* the wind] are for the most part heard with their full distinctness; and there is only a comparatively small margin between that point at which the sound is perceptibly diminished, and that at which it ceases to be audible." (Phil. Mag., p. 63.)

[2.] The sound of the alarm-bell was always heard "farther with the wind than at right-angles to its direction; [contrary to the old observation of De La Roche in 1816,—which was obviously an exceptional one;] and when the wind was at all strong, the range with the wind was more than double that at right angles. . . . . *With* the wind, over the grass the sound could be heard 140 yards, and over the snow 360 yards, either with the head lifted or on the ground; whereas at right-angles to the wind, on all occasions the range was extended by raising either the observer or the bell." (p. 68.)

[3.] When the wind was light the sound beyond the distance of 20 yards, was much less audible at the ground than a few feet above it; and when inaudible in every direction at standing height, the sound could be distinctly recovered by mounting a tree. The same result was obtained by raising the alarm-bell upon a post 4 feet high; which while materially increasing the range of the sound—even in the direction of the slight wind, in all other directions doubled the range. This is explained by Professor Reynolds, by the continual waste and destruction of the sound waves which pass along the rough surface of the ground or grass, causing the waves immediately above to diverge continually downward, to be in like manner absorbed; the effect of which is to gradually weaken the sound more and more, as the waves proceed; so that even "when there is no wind, the distant sounds which pass above us are more intense than those we hear." (p. 68.)

[4.] Whatever therefore tends to gradually bend downward the sound rays will increase their sensible range. Professor Reynolds found by observations with the anemometer that the velocity of the wind increased from the ground upward; (pp. 63, 64) and hence it must give greater rapidity to the upper portion of the sound waves in the direction in which it is blowing and cause their impulses to continually tip downward. "This was observed to be the case on all occasions. In the direction of the wind when it was strong, the sound could be heard as well with the head on the ground as when raised, even when in a hollow with the bell hidden from view by the slope of the ground; and no advantage whatever was gained either by ascending to an elevation, or raising the bell." (p. 68.)

[5.] "Elevation was found to affect the range of sound against the wind in a much more marked manner than at right-angles. Over the grass no sound could be heard with the head on the ground at 20 yards from the bell, and at 30 yards it was lost with the head 3 feet from the ground, and its full intensity was lost when standing erect at 30 yards. At 70 yards when standing erect the sound was lost at long intervals, and was only faintly heard even then; but it became continuous again when the ear was raised 9 feet from the ground, and it reached its full intensity at an elevation of 12 feet." (p. 69.) The same results were obtained with snow on the ground, excepting that the sound was heard somewhat lower, being less dissipated or absorbed by the surface contact. At 160 yards the bell was inaudible—even at an elevation of 25 feet, and the sound was supposed to be hopelessly lost; but at a further elevation of 33 feet from the ground, it was again heard; while at 5 feet lower it was lost. At the proper elevation the sound appeared to be as well heard against the wind as with it, at the same distance. These last two observations very strikingly correspond with and confirm the observations of Henry [3], and [4].

[6.] "The least raising of the bell was followed by a considerable intensifying of the sound;" and while it could be heard only 70 yards when resting on the ground, (i. e., one foot high), when set on a post 5 feet high, it could be heard 160 yards, or more than twice the distance,—the sound-beams evidently rising faster at or near the ground, than they do higher up. (p. 69.) "The intensity of the sound invariably seemed to waver, and as one approached the bell from the windward side, the sound did not intensify uniformly or gradually, but by fits or jerks." This is supposed to be the result of the more or less curved sound rays crossing each other at a small angle and producing an "interference." (p. 70.)

A subsequent experiment was made on the 14th of March, during a strong west wind, its velocity at an elevation of 12 feet being 37 feet per second, at 8 feet, 33 per second, and at one foot from the ground (there being no snow on the grass) 17 feet per second. While the results as to varying range fully confirmed the previous experiments, the raising of the bell caused the sound to be heard even better against the wind than in the direction of the wind. (p. 71.) This curious circumstance is explained by Professor Reynolds as "due to the fact that the *variation* in the velocity of the air is much greater near the ground, than at a few feet above it;" and "when the bell is raised the rays of sound which proceed horizontally will be much less bent or turned up than those which go down to the ground; and consequently after proceeding some distance these rays will meet or cross, and if the head be at this point they will both fall on the ear together, causing a sound of double intensity. It is this crossing of the rays also which for the most part causes the interference" just mentioned. (p. 71.)

Professor Reynolds concludes that "these experiments establish three things with regard to the transmission of sound: 1. That when there is no wind, sound proceeding over a rough surface is more intense above than below. 2. That as long as the velocity of the wind is greater above than below, sound is lifted up to windward and is not destroyed. 3. That under the same circumstances it is brought down to leeward, and hence its range extended at the surface of the ground. These experiments also show that there is less variation in the velocity of the wind over a smooth surface than over a rough one. It seems to me that these facts fully confirm the hypothesis propounded by Prof. Stokes; that they place the action of wind beyond question; and that they afford explanations of many of the anomalous cases that have been observed." (p. 71.)

[7.] In regard to the second part of the communication, treating of the effect of Temperature differences in refracting sound, Professor Reynolds shows that as "every degree of temperature between 32° and 70° adds approximately one foot per second to the velocity of sound," there must necessarily be an upward flexure of the rays, whenever by reason of any considerable increase of temperature in the lower strata of the air, the lower portion of the sound waves is projected in advance of the upper portion. (p. 71.) Atmospheric vapor also, though exercising but little direct influence on the velocity of sound, "nevertheless plays an important part in the phenomena under consideration; for it gives to the air a much greater power of radiating and absorbing heat, and thus renders it much more susceptible of changes in the action of the sun. . . . . It is a well-known fact that the temperature of the air diminishes as we proceed upward, and that it also contains less vapor. Hence it follows that, as a rule, the waves of sound must travel faster below than they do above, and thus be refracted or turned upward." (p. 72)

The variation of temperature will be greatest in a quiet atmosphere when the sun is shining. The report of Mr. Glaisher "On eight Balloon Ascents in 1862" showed that "The decline of temperature [upward] near the earth with a partially clear sky is nearly double that with a cloudy sky."* "During the night the variations are less than during the day. This reasoning at once suggested an explanation of the well-known fact that sounds are less intense during the day than at night. This is a matter of common observation, and has been the subject of scientific enquiry." (p. 73.) The opinion must here be hazarded that this familiar phenomenon has first received its true and satisfactory explanation from Professor Reynolds.

Assuming that for a few hundred feet upward, the diminution of temperature on a clear summer day is 1° for each hundred feet, a horizontal sound-ray would be bent up in an arc having a radius of about 20 miles. From a cliff 235 feet high, a sound should be audible from $1\frac{1}{2}$ to 2 miles on the sea, and the ray should then begin to rise above the observer's head. This is shown to accord very closely with the observation of Tyndall [6]. Professor Reynolds after quoting the observation at length, remarks: "Here we see that the very conditions which actually diminished the range of the sound were precisely those which would cause the greatest lifting of the waves. And it may be noticed that these facts were observed and recorded by Professor Tyndall with his mind altogether unbiased with any thought of establishing this hypothesis. He was looking for an explanation in quite another direction. Had it not been so he would probably have ascended the mast and thus

* Mr. Glaisher remarks: "From these results we may conclude that in a cloudy state of the sky, the decline of temperature is nearly uniform up to the clouds; that with a clear sky the greatest change is near the earth, being a decline of 1° in less than 100 feet, gradually decreasing as in the general law indicated in the preceding section, till it requires 300 feet at the height of 5,000 feet, for a change of 1° of temperature." (Rep. Brit. Assoc, 1862, p. 462.)

found whether or not the sound was all the time passing over his head. On the worst day an ascent of 30 feet should have extended the range nearly one quarter of a mile." (Phil. Mag., p. 76.)

V.

The instructive result, brought into view by the foregoing summaries, is that the differences noticed are essentially those of interpretation, and not to any important extent, of observation: an illustration if any were needed, of the high and rare order of imaginative insight requisite to the successful investigation of the more recondite operations of natural law. The differing actions of acoustic reflection and acoustic refraction suggested by the ingenious hypotheses of Humboldt and of Stokes, and espoused respectively by Tyndall and Henry, are probably both operative but their relative importance has yet to be established. It is certain, as already indicated, that some of the phenomena observed lie quite beyond the reach of the acoustic cloud hypothesis.

A particularly interesting case which is claimed with equal confidence for either theory, is the remarkable observation of General Duane, that at Portland, Maine, the steam whistle on Cape Elizabeth, nine miles distant, "can always be distinctly heard" with "the wind blowing a gale directly *toward* the whistle" or against the sound. (L. H. Rep., p. 100.) At Portland Head, about midway between this fog-whistle and the point of observation is another signal,—a Daboll trumpet. While both these signals are better heard with an adverse wind ("a heavy northeast snow storm") than at other times, yet "as the wind increases in force, the sound of the nearer instrument—the trumpet—*diminishes*, but the whistle becomes *more distinct*." (Rep., p. 92.) The abnormal influence of the wind in reversing the order of these two signals is not the least surprising feature of the general phenomenon.

Professor Tyndall believes that this curious observation only "proves the snow-laden air from the northeast to be a highly homogeneous medium;" (Sound, Preface, p. 19,) the intervening air at other times being acoustically less transparent.

Professor Henry supposes "that during the continuance of the storm, while the wind was blowing from the northeast at the surface, there was a current of equal or greater intensity blowing in an opposite direction above, by which the sound was carried in direct opposition to the direction of the surface current;" (Rep., p. 92)—somewhat in the nature of a vertical cyclone. He adds: "The existence of such an upper current is in accordance with the hypothesis of the character of a northeast storm, which sometimes rages for several days at a given point on the coast without being felt more than a few miles in the interior, the air continuously flowing in below and going out above. Indeed in such cases a break in the lower clouds reveals the fact of the existence above of a rapid current in the opposite direction." (p. 92.)

Professor Henry's attention had been directed to this point as early as 1865, by discovering that a signal was audible against the wind at the mast-head of a vessel, after ceasing to be audible on deck: Obs. [4]. "This remarkable fact at first suggested the idea that sound was more readily conveyed by the upper current of air than the lower, and this appeared to be in accordance with the following statement of Captain Keeney, who is commander of one of the light-house vessels, and has been for a long time on the banks of Newfoundland in the occupation of fishing: 'When the fishermen in the morning hear the sound of the surf to leeward, or from a point toward which the wind is blowing, they take this as an infallible indication that in the course of from one to five hours the wind will change to the opposite direction from which it is blowing at the time.' The same statement was made to me by the intelligent keeper of the fog-signal at Block Island. In these cases it would appear that the wind had already changed direction above, and was thus transmitting the sound in an opposite direction to that of the wind at the surface of the earth." (Rep., p. 92.) The full significance of this idea however was not apprehended until the hypothesis of Professor Stokes (already alluded to) was taken up and considered. This appeared to furnish a satisfactory explanation of the observed effect of an upper current,—not on the actual range, but on the *direction* of the sound waves

Professor Tyndall thus comments on the rival hypothesis of Professor Henry: "In the higher regions of the atmosphere he places an ideal wind, blowing in a direction opposed to the real one, which *always* accompanies the latter, and which more than neutralizes its action. In speculating thus he bases himself on the reasoning of Professor Stokes, according to which a sound-wave moving against the wind is tilted upward. The upper and opposing wind is invented for the purpose of tilting again the already lifted sound-wave downward." (Pref. to Sound, pp. 19, 20.)

The word "invented" is scarcely the most appropriate term for an hypothesis derived from such patient research and careful induction. While in the case considered, the reversed upper wind of a local circulation is rendered so probable by the circumstances presented. it is proper to remark that this condition is not at all essential to the refraction doctrine. The hypothesis of Professor Stokes by no means assumes that "a sound-wave moving against the wind is tilted upward." (Rep. Brit. Assoc., 1857, pp. 22, 23, of Abstracts.) An opposing wind exercises no sensible influence on either the velocity or the range of sound, nor (if *uniform*) on the direction of sound. Ordinarily indeed, a wind (which may be likened to an aerial river) is retarded at the earth precisely as the current of a stream is, over its bed.* When, however, the mouth of the aerial chimney of ascent is low, it may very well happen that the lower current of air (excepting immediately at the surface of the earth) is considerably swifter than the successive layers of wind above it; and in such a case the effect of the opposing wind will be not to tilt upward the sound-beam, but to tilt it downward. In like manner a "favoring" wind, if more sluggish above, will tilt the sound-beam upward, and thus prove unfavorable to its audibility. In short, the postulate required for acoustic refraction is simply that there shall be a *difference* of amount between the upper and the lower currents of wind. And as this condition is certainly not an unusual one, we have here apparently a true and satisfactory account of the seeming anomalies of sound with reference to the influence of the wind.

But if the natural tendency of a mere diminution of velocity in the upper strata of an adverse wind is thus to bend an advancing sound downward, "a precisely similar effect" as Professor Henry has well remarked, "will be the result but perhaps in a considerably greater degree, in case an upper current is moving in an opposite direction to the lower, when the latter is adverse to the sound." (Rep., p. 107.) In September, 1874, when a signal near Sandy Hook, N. J., was observed to be audible at a greater distance against the afternoon sea-breeze than with it, Professor Henry ascertained by the employment of small toy balloons, that the upper current was opposed to the lower one, and in the direction of the maximum sound range: Obs. [11.] He was enabled thus to demonstrate experimentally the reality of the "ideal wind" which had been so confidently accepted before, from other conspiring intimations.

The critical commentary above cited, which postulates for this doctrine of acoustic refraction the super-position of "an ideal wind blowing in a direction opposite to the real one," as a condition "which more than *neutralizes* its action," quite fails to apprehend its true import. No action analogous to "neutralization" is assumed by the doctrine. There is no solution

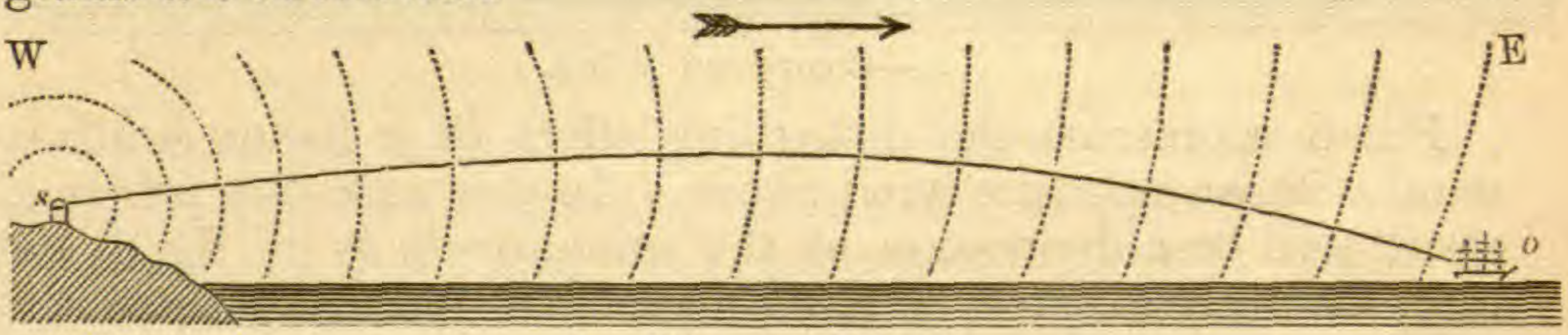
* Professor Henry determined by experiment in 1865, when the velocity of the wind was not more than six miles per hour, that the speed of the clouds as indicated by their moving shadows, was several times this rate. (L. H. Rep., p. 93.) And Professor Reynolds in 1874, by observations with the anemometer, ascertained that near the ground the retardation of the wind rapidly increased; so that the lower sound rays move more nearly in the arc of a parabola, than of a circle. (Phil. Mag., pp. 64 and 70.)

of continuity between opposing currents; but every gradation of movement in each successive intermediate stratum. And as it is wholly improbable that the sound-beam *which reaches the observer's ear*, ever passes high enough to approach the upper "ideal wind," nothing is neutralized. Obedient to the law of instantaneous resultants, the beam of acoustic impulse presses on ever at right angles to the wave-surface which is conditioned by compounded factors.

As wide of the mark is the supposition that the upper and opposing "ideal wind" is "for the purpose of tilting again the already lifted sound-wave, downward." As has been just contended, the one wind is as incapable of depressing the sound-wave, as the other is of lifting it.

The misconception culminates in the objection that "Professor Henry does not explain how the sound-wave *re-crosses* the hostile lower current, nor does he give any definite notion of the conditions under which it can be shown that it will reach the observer." (Loc. cit., p. 20.) There *is* no "hostile lower current," since as above pointed out, an opposite wind may be just as favorable to the propagation of sound, as a concurrent one.

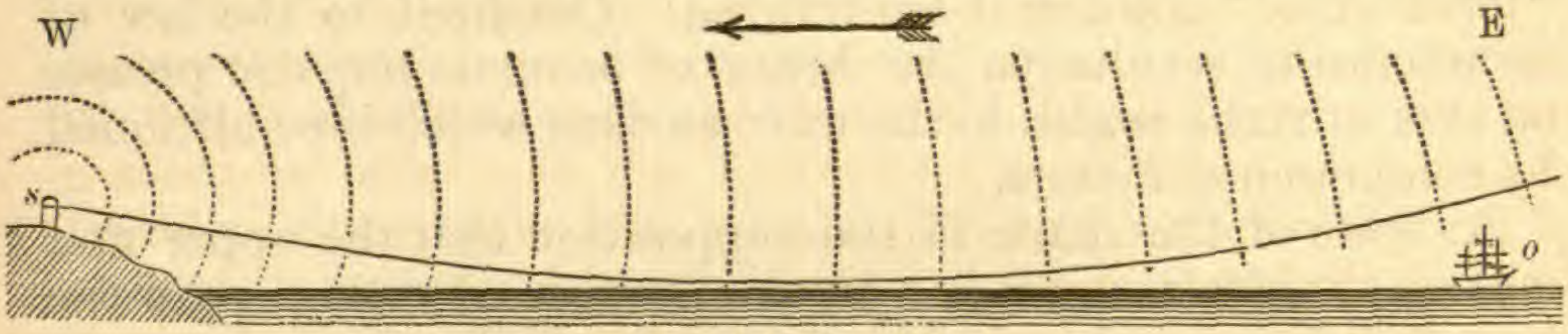
To give, however, a more definite notion of the conditions under which it can be shown that the sound-wave will reach the observer without crossing currents, the accompanying diagrams are submitted.



1.—Favoring Wind.

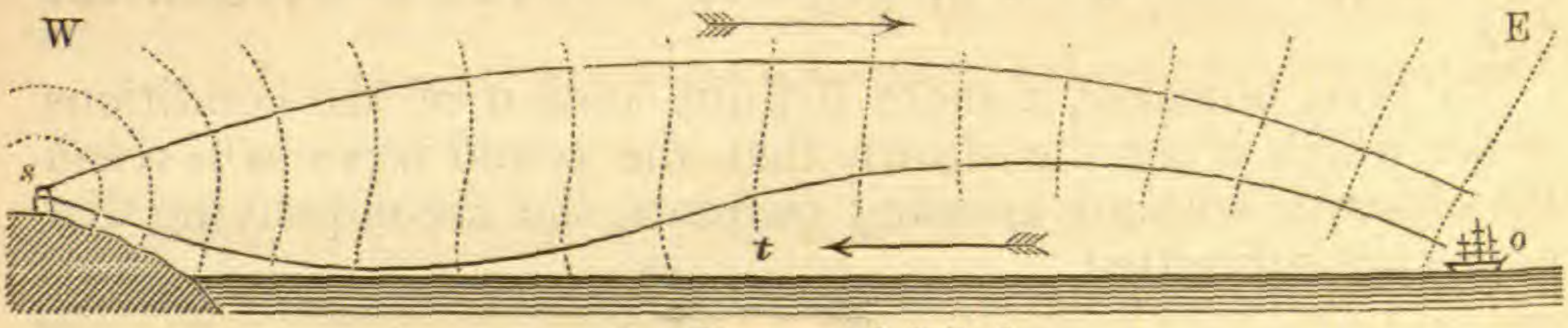
Fig. 1 exhibits the more ordinary effect of a *favorable* wind in depressing the beam of sound: *s* being the signal-station, and *o* the point of observation; the wind blowing from *W.* to *E.* As the spheroidal wave-faces become more pressed forward above by the freer wind (assuming it to be retarded at the surface by friction), and as the direction of the acoustic beam is constantly normal to the successive aerial surfaces of impact, it follows that very minute differences of concentricity in the successive waves, will by constant accumulation gradually bend the line of dynamic effect downward, as shown in the sketch on a very exaggerated scale. Of the sound rays below the line represented, some will by reflection from the sea, reach the observer's ear and thus increase the sound.

Fig. 2 represents the ordinary effect of an *opposing* wind here blowing from *E.* to *W.* The wave faces being more resisted above by the freer contrary wind (assuming as before a surface retardation), the sound-beams are curved upward, and the lowest ray that can reach the distance of the observer at *o*, is that which touching the surface of the sea is gradually so tilted up-



2.—Adverse Wind.

ward that it passes above the ear of the listener, leaving him practically in an acoustic shadow; very much as an observer on the deck of a vessel when losing the sight of the hull of another vessel ten miles off, by reason of the interposed convexity of the ocean, stands in the *optical* shadow of the earth. In both cases if the conditions favor, the boundary of the shadow may be re-crossed by ascending from the deck to the mast-head, and the sight or the sound-beam thus regained.



3.—Compound Wind.

Fig. 3 represents the disturbing effect of a lower contrary wind with an opposite wind above. In this case the principal result will be a depression of the sound-beam as in fig. 1, but more strongly marked, as the differences of motion as we ascend will be more rapid. Attending this action, however, there will probably be some lagging of the lower stratum of the adverse wind by reason of the surface friction; the tendency of which will be to slightly distort the lower sound radiations, by giving them a reverse or serpentine curvature. The upper rays of sound would probably have only a single declining curvature, similar to that shown in fig. 1.

One result of this condition of the *locus* of the *normals* (to use a mathematical phrase) would be to make the sound less audible (or possibly sometimes inaudible) at a point (as at *t*) midway between the two stations. This hypothetical case of compound refraction would appear to offer a plausible explanation, not only of the paradox of a nearer trumpet-sound being diminished in power by the wind which increased the effect of a more distant whistle, but also of the puzzling "belt" of inaudibility previously noticed. Duane [D], and Henry [8].

Numerous other cases might be represented by diagrams, as of a sound being hindered or tilted upward by a concurrent wind of unequal velocity, or downward by an opposing wind of similar character, and of the various permutations of differing currents in oblique directions; to which might be added various resultants of unequal motion producing *lateral* refraction, but this is unnecessary. Enough has been said, it is hoped, to clear from popular misapprehension, the admirable hypothesis of Professor Stokes, raised by the equally admirable investigations of Professor Henry, to the rank of a "theory;" and to show that it has a real and demonstrated basis, or in other words that it is a *vera causa*. The question of its sufficiency lies entirely within the grasp of mathematical discussion; but a long series of accurate and comprehensive observations will yet be required to discover its full compass of practical result, and to determine its precise limit of capacity in subjugating the "abnormal phenomena" of sound.

ART. VIII.—*Studies on Magnetic Distribution*; by HENRY A. ROWLAND, of the Johns Hopkins University, Baltimore.

(Continued from page 29.)

VI.

M. JAMIN, in his recent experiments on magnetic distribution, has obtained some very interesting results, although I have shown his method to be very defective. In his experiments on iron bars magnetized at one end, he finds the formula ε^{rL} to apply to long ones as I have done. Now it might be argued that as the two methods *apparently* give the same result, they must be equally correct. But let us assume that the attraction of his piece of soft iron F varied as some unknown power n of the surface-density δ. Then we find

$$F = C\varepsilon^{nrL},$$

which shows that the attractive force or any power of that force can be represented by a logarithmic curve, though not by the same one. Hence the error introduced by M. Jamin's method is insidious and not easily detected, though it is none the less hurtful and misleading, but rather the more so.

However, his results with respect to what he calls the normal magnet* are to some extent independent of these errors; and we may now consider them.

Thus, in explaining the effect of placing hardened steel

* "On the Theory of Normal magnets," *Comptes Rendus*, March 31, 1873, translated in Phil. Mag., June, 1873.

plates on one another, he says: "Quand on superpose deux lames aimantées pareilles, les courbes qui représentent les valeurs de F (the attractive force on the piece of soft iron) s'elévent, parce que le magnétisme quitte les faces que l'on met en contact pour réfugier sur les parties extérieures. En même temps, les deux courbes se rapprochent l'une de l'autre et du milieu de l'aimant. Cet effet augmente avec une troisième lame et avec une quatriéme. Finalement les deux courbes se joignent en milieu."

In applying the formula to this case of a compound magnet, we have only to remark that when the bars lie closely together, they are theoretically the same as a solid magnet of the same section, but are practically found to be stronger, because thin bars can be tempered more uniformly hard than thick ones. The addition of the bars to each other is similar to an increase in the area of the rod, and should produce nearly the same effect on a rod of rectangular section as the increase of diameter in a rod of circular section. Now the quantity $p=\frac{rd}{2}$ is nearly constant in these rods for the same quality of steel, whence r decreases as d increases; and this in equation (17) shows that as the diameter is increased, the length being constant, the curves become less and less steep until they finally become straight lines. This is exactly the meaning of M. Jamin's remark.

Where the ratio of the diameter to the length is small, the curves of distribution are *apparently* separated from each other, and are given by the equation

$$\lambda=\frac{\mathfrak{H}}{4\pi\sqrt{RR'}}\varepsilon^{-rx}, \quad . \quad . \quad . \quad . \quad (18)$$

which is not dependent on the length of the rod. This is exactly the result found by Coulomb (Biot's Physique, vol. iii, pp. 74, 75.) M. Jamin has also remarked this. As he increases the number of plates, he states that the curves approach each other and finally unite; this he calls the "normal magnet;" and he supposes it to be the magnet of greatest power in proportion to its weight. "From this moment," says he, "the combination is at its maximum." The normal magnet as thus defined is very indefinite, as M. Jamin himself admits.

By our equations we can find the condition for a maximum, and can give the greatest values to the following, supposing the weight of the bar to be a fixed quantity in the first three.

1st. The magnetic moment.

2d. The attractive force at the end.

3d. The total number of lines of magnetic force passing from the bar.

4th. The magnetic moment, the length being constant and the diameter variable.

Either of these may be considered as a measure of the power of the bar according to the view we take. The magnetic moment of a bar is easily found to be

$$M=\frac{\mathfrak{H}}{4\pi r^2 R'}\left\{\frac{b}{2}-\frac{1}{r}\frac{1-\varepsilon^{-rb}}{1+\varepsilon^{-rb}}\right\}; \quad . \quad . \quad (19)$$

and if γ is the weight of a unit of volume of the steel and W is the weight of the magnet, we have finally

$$M=\frac{\mathfrak{H}}{4\pi R'C^2}\left\{\frac{1}{2}-\frac{1}{Cb^{\frac{3}{2}}}\frac{\varepsilon^{Cb^{\frac{3}{2}}}-1}{\varepsilon^{Cb^{\frac{3}{2}}}+1}\right\}, \quad . \quad (20)$$

where $C=\frac{r}{\sqrt{b}}=p\sqrt{\frac{\pi\gamma}{W}}$.

This only attains a maximum when $\frac{b}{d}=\infty$, or the rod is infinitely long compared with its diameter.

The second case is rather indefinite, seeing that it will depend upon whether the body attracted is large or small. When it is small, we require to make the surface-density a maximum, the weight being constant. We find

$$\delta_0=\frac{\mathfrak{H}}{\gamma\pi^2 R'p}\frac{\varepsilon^{Cb^{\frac{3}{2}}}-1}{\varepsilon^{Cb^{\frac{3}{2}}}+1}, \quad . \quad . \quad . \quad (21)$$

which attains a maximum as before when $\frac{b}{d}=\infty$. When the attracted body is large, the attraction will depend more nearly upon the linear density

$$\lambda_0=\frac{\mathfrak{H}}{4C\pi R'\sqrt{b}}\frac{\varepsilon^{Cb^{\frac{3}{2}}}-1}{\varepsilon^{Cb^{\frac{3}{2}}}+1}, \quad . \quad . \quad . \quad (22)$$

which is a maximum when $\frac{b}{d}=\frac{1{\cdot}42}{p}$.

For the third case we have the value of Q″ at the center of the bar from equation (6),

$$Q''=\frac{\mathfrak{H}}{2C^2R'b}\frac{(\varepsilon^{\frac{1}{2}Cb^{\frac{3}{2}}}-1)^2}{\varepsilon^{Cb^{\frac{3}{2}}}+1}. \quad . \quad . \quad . \quad (23)$$

The condition for a maximum gives in this case

$$\frac{b}{d}=\frac{1{\cdot}65}{p}.$$

For the last case in which the magnetic moment for a given length is to be made a maximum, we find

$$\frac{b}{d}=\frac{\cdot 1}{p}.$$

This last result is useful in preparing magnets for determining the intensity of the earth's magnetism, and shows that the magnets should be made short, thick, and hard for the best effect.*

But for all ordinary purposes the results for the second and third cases seem most important, and lead to nearly the same result; and taking the mean we find for the maximum magnet

$$\frac{b}{d}=\frac{1\cdot5}{p}. \qquad . \quad . \quad . \quad . \qquad (24)$$

We see from all our results that the ratio of the length of a magnet to its diameter should vary inversely as the constant p. This constant increases with the hardness of the steel, and hence the harder the steel the shorter we can make our magnets. It would seem from this that the temper of a steel magnet should not be drawn at all, but the hardest steel used, or at least that in which p was greatest. The only disadvantage in using very hard steel seems to be the difficulty in imparting the magnetism at first, and this may have led to the practice of drawing the temper; but now when we have such powerful electromagnets, it seems as if magnets might be made shorter, thicker and harder, than is the custom. With the relative dimensions of magnets now used, however, hardening might be of little value.

We can also see from all these facts, that if we make a compound magnet of hardened steel plates there will be an advantage in placing more of them together, thus making a thicker magnet than when they are softer. We also observe that as we pile them up the distribution changes in just the way indicated by M. Jamin, the curve becoming less and less steep.

Substituting in the formula the value of p which we have found for Stub's steel not hardened, but still so hard as to rapidly dull a file, we find the best ratio of length to diameter to be 33·8, and for the same steel hardened about 17, though this last is only a rough approximation. This gives what M. Jamin has called the normal magnet. The ratio should be less for a U-magnet than for a straight one.

For all magnets of the same kind of steel in which the ratio of length to diameter is constant the relative distribution is the same; and this is not only true for our approximate formula, but would be found so for the exact one.

Thus for the "normal magnet" the distribution becomes

$$\lambda=C(\varepsilon^{3\frac{x}{b}}-\varepsilon^{-3\frac{x}{b}}),$$

where C is a constant, and x is measured from the center. The distribution will then be as follows:

* Weber recommends square bars eight times as long as they are broad, and tempered very hard. (Taylor's Scientific Memoirs, vol. ii, p. 86.)

$\frac{x}{b}=$	0	·1	·2	·3	·4	·5
$\frac{\lambda}{C}=$	0	·609	1·27	2·05	3·02	4·26

8.

Distribution on "normal magnet."

This distribution is not the same as that given by M. Jamin; but as his method is so defective, and his "normal magnet" so indefinite, the agreement is sufficiently near.

The surface-density at any point of a magnet is

$$\delta = \frac{\mathfrak{H}}{8\pi^2 p \mathrm{R}'} \frac{\varepsilon^{2p\frac{x}{d}} - \varepsilon^{-2p\frac{x}{d}}}{\varepsilon^{p\frac{b}{d}} - \varepsilon^{-p\frac{b}{d}}}, \quad . \quad . \quad . \quad (25)$$

which, for the same kind of steel, is dependent only on $\frac{x}{d}$ and $\frac{b}{d}$. Hence in two similar magnets the surface-density is the same at similar points, the linear density is proportional to the linear dimensions, the surface integral of magnetic induction over half the magnet or across the section is proportional to the surface dimensions of the magnets, and the magnetic moments to the volumes of the magnets. The forces at similar points with regard to the two magnets will then be the same. All these remarks apply to soft iron under induction providing the inducing force is the same, and hence include Sir William Thomson's well-known law with regard to similar electromagnets; and *they are accurately true notwithstanding the approximate nature of the formula from which they have here been deduced.*

Our theory gives us the means of determining what effect the boring of a hole through the center of a magnet would have. In this case R′ is not much affected, but R is increased. Where the magnet is used merely to affect a compass-needle, we should then see that the hole through the center has little effect where the magnet is short and thick; but where *it is long, the attraction on the compass-needle is much diminished.* Where the magnet is of the U-form, and is to be used for sustaining weights, the *practice is detrimental, and the sustaining-power is diminished* *in the same proportion as the sectional area of the magnet.* The only case that I know of where the hole through the center is an advantage, is that of the deflecting magnets for determining the intensity of the earth's magnetism, which may be thus made lighter without much diminishing their magnetic moment.

In conclusion, let me express my regret at the imperfection of the theory given in this paper; for although the equations are more general than any yet given, yet still they rest upon two quite incorrect hypotheses; and so, although we have found these formulas of great use in pursuing our studies on magnetic distribution, yet much remains to be done. A nearer approximation to the true distribution could readily be obtained, but the results would, without doubt, be very complicated and would not repay us for the trouble.

In this paper, as well as in all the others which I have published on magnetic subjects, my object has been not only to bring forth new results, but also to illustrate Faraday's method of lines of magnetic force and to show how readily calculations may be made on this system. For this reason many points have been developed at greater length than would otherwise be desirable.

ART. IX.—*On rifts of Ice in the rocks near the summit of Mt. McClellan, Colorado, and on the different Limits of Vegetation on adjoining summits in the Territory;* by EDWARD L. BERTHOUD.

THE silver mines of Argentine District, a mining center about eight miles southwest from Georgetown, are located on the north slope of a high peak named McClellan Mountain, which forms a very prominent point of the main central range, and immediately facing a precipice fully 1500 feet high, the majestic mass of Gray's Peak; while 1½ miles south is Argentine Pass, 13,100 feet in height.

This mountain, 13,430 feet* above the sea, is intersected in a northeast and southwest direction by a system of mineral veins, containing silver in large quantity with a little gold. The veins seem generally to be nearly vertical, and occur at elevations varying from 12,300 feet to 13,400 feet. Three of them have been extensively mined, and two, the International and Belmont, have been developed and worked since 1867–68 with success, and with fair paying results; but with probably at a greater average cost per ton of mineral mined than any other similar mines in Northern Colorado. The Centennial Lode, the third mine examined, is now being well developed by its owners, who

* Vide Gardner, in Hayden's Report, 1873–74.

are working into the vein horizontally by excavating a drift. The ores found in these mines are galena rich in silver, decomposed quartz and honey-combed quartz, with sulphurets of silver, and some decomposed iron pyrites and a little carbonate of lead, with occasional small patches of sphalenite.

I have been thus particular in the description of these mines, merely to give a good general idea of their value and location. In a personal and critical examination of them, during a recent visit to the region, a peculiar feature was observed which excited much surprise.

The discovery-drift of the Centennial Lode runs into McClellan Mountain at an altitude above 13,100 feet, on a course southwest, at about 30 feet from the entrance of the tunnel. Intercalated in the vein, I found three or four well defined veins of solid ice, parallel with the bedding of the rock, and filling all its thinner side cracks and fissures; in fact, after further examination I found that the frozen stratum, and the congealed, hard earth, rock and gravel, began only a few feet below the accumulated rock and debris of the mountain slope, and continued as far as the excavation reached, some forty feet in depth.

From the Centennial Lode I went westward about 300 feet, and examined the drift that has been excavated into the mountain some 500 feet, upon the vein of the International Lode. Here there is repeated the same frozen substratum and the same rift or veins of ice in the country rock and in the vein. I went into the tunnel about 100 feet and found this glacial condition still existed; and the owner of the mine assured me that the ice and frozen rock continued all the way to the end of the tunnel and caused a good deal of extra expense in mining the ore.

The course of the "International Lode" is southwest, and its drift is about 50 feet in vertical elevation above the drift of the Centennial Lode.

The next "Lode" examined was the Belmont Lode, west and nearly parallel to the International. This mine is exploited by a system of horizontal galleries one above the other to the summit of the mountain, at 13,400 feet elevation. In the lower galleries the same frozen icy condition prevails as at the first two veins. But the summit drift, which was at the date of my visit about 60 feet long, does not show veins of ice in the wall-rock of the veins; this is probably due not only to the greater narrowness of the summit, here scarcely 200 feet where pierced by the tunnel, but also to the influence of wind and sun upon its western seamed and riven surface, and to its more perfect drainage and exposure.

This is certainly a singular phenomenon, when we consider that across the narrow valley north of McClellan Mt., not over three-fourths of a mile distant and upon another high peak, the limit of tree growth exceeds 12,400 feet elevation on the south slope of that peak. Here can be seen *Pinus aristata*, some of the trees two feet in diameter and thirty feet high that retain their hold, and slowly increase in size, thus maintaining themselves in respectable numbers in spite of furious gales of snow and wind, and an extreme Arctic cold.

In Miscellaneous Publications, No. 1, U. S. Geological Survey of the Territories, which was published last year, under the direction of Prof. F. V. Hayden, the line of tree growth is given by Mr. J. T. Gardner in his report, as from 11,000 feet to 11,900 feet, between latitudes 39° and 40°. We believe this to be correct, and a fair general average. In Argentine District, which comprises McClellan Mountain, we have a very notable departure from this limit of from 500 to 1400 feet in elevation, and also about 1300 feet above timber line on Gray's Peak, three to four miles southwest, as given by Mr. Gardner. At the Equator and in the Torrid zone the limit of the growth of Pines is generally placed at 12,800 feet above the sea; how is it that, in lat. 39° 33′ N., the limit of the growth of Pines has receded only 400 feet?

In McClellan Mountain and in Argentine District there are two antagonistic phenomena in immediate proximity; on one side of the valley, a mountain slope facing northeast, well grassed, totally devoid of shrubs and trees, where soil and rocky debris are underlain by a perpetual icy coat of hundreds of feet in depth, supporting on its surface a growth of plants strictly Alpine and Arctic, and abounding with Ptarmigan, *Lagopus leucurus*, and the tailless, earless marmot; and where on the 2d October, 1875, I found the following plants yet in bloom; *Sedum stenopetalum*, *Potentilla norvegica*, *P. fruticosa*, *Sibbaldia procumbens*, *Astragalus alpinus*, *Silene acaulis*, *Draba aurea*, *Phleum alpinum*, *Primula Parryi*, *Gentiana*, *Heuchera*, *Castilleia pallida*, *Ranunculus nivalis*, *Pedicularis*, *Cardamine and Crepis*, while less than half mile distant, on the opposite slope of the vale, *Pinus aristata* of large size and a profuse growth of birches, willows, grasses and Arbutus, with flowing springs and small ponds, diversify its southwestern slope.

It has been suggested* that the frozen soil and rock of some mines examined by him, northwest from McClellan Mountain, on the west slope, have been thus left ice bound since the Glacial period; and that they thus retain their former ice-bound condition, from the excessive altitude of the mines there explored.

This may be the case, but it seems doubtful. There are in Colorado many mines at altitudes very nearly as high as the highest on McClellan Mountain, yet none have been exploited to the

* R. Weiser, in this Journal, III, viii, 477, 1874.

depth of from 100 to 500 feet in solid frozen soil and ice ribs. I am inclined to believe that the glacial condition of McClellan Mountain is due to local causes. Prominent among these would be the loose nature of the soil and deep rocky debris of the mountain, and the slow percolation of water exposed to excessive evaporation that is promoted and quickened by continued gales from the north and northwest that strike against the precipitous face of the mountain range in that direction. The opposite slope, on the contrary, which shows the abnormally high timber line, faces a Pass (Argentine Pass) 13,100 feet in height, which gives a way perfectly unobstructed for south-southwest winds. These prevail frequently in winter and spring, and are invariably temperate or even warm, and thus to their influence may be due the milder and more propitious character of this locality. In Colorado Territory it has been remarked that in our mountains, even in January, a southwest wind is invariably genial and warm; in two hours I have known a southwest wind to raise the thermometer from 13° below zero to 47° above. This abrupt change, however, is disastrous to tree growth, and destroys the quaking Asp, Cedar, and even Pines in more exposed localities; while the Cherry, Box Elder and the bitter Cottonwood (*Populus angulata*) have perished in the ensuing spring in our lower valleys and on the foot-hills.

I have presented this subject in order to secure for it further elucidation and discussion. The facts are of no little interest, since they conflict with accepted views as to the limits of growth of plant, and the influence of altitude on climate.

ART. X.—*On a New Form of Lantern Galvanometer;* by FRANCIS E. NIPHER, Professor of Physics in Washington University.*

IN the September number of this Journal, Prof. Barker has described a lantern galvanometer, which appears to possess many advantages over any heretofore described, and which is evidently a valuable addition to the apparatus of the public lecturer.

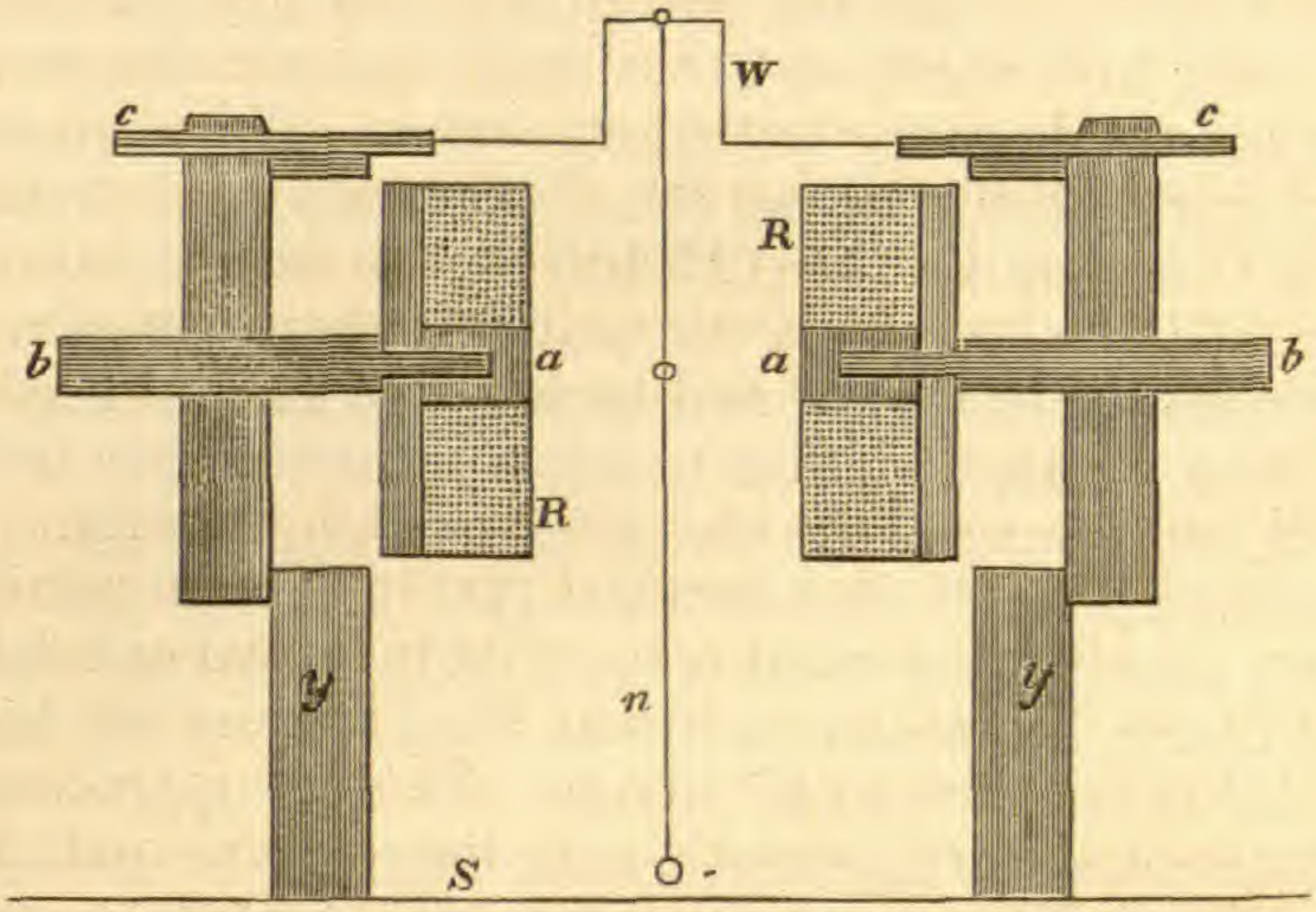
While meditating the construction of this instrument, the galvanometer now to be described was devised. A vertical section is shown in Fig. 1.

A square box (Y, Y), open at the top and bottom, is pierced on opposite sides to admit the wooden rods (*b*). To the inner extremities of these rods are attached coils (R), of covered cop-

* Read before the St. Louis Academy of Science, Oct. 18, 1875.

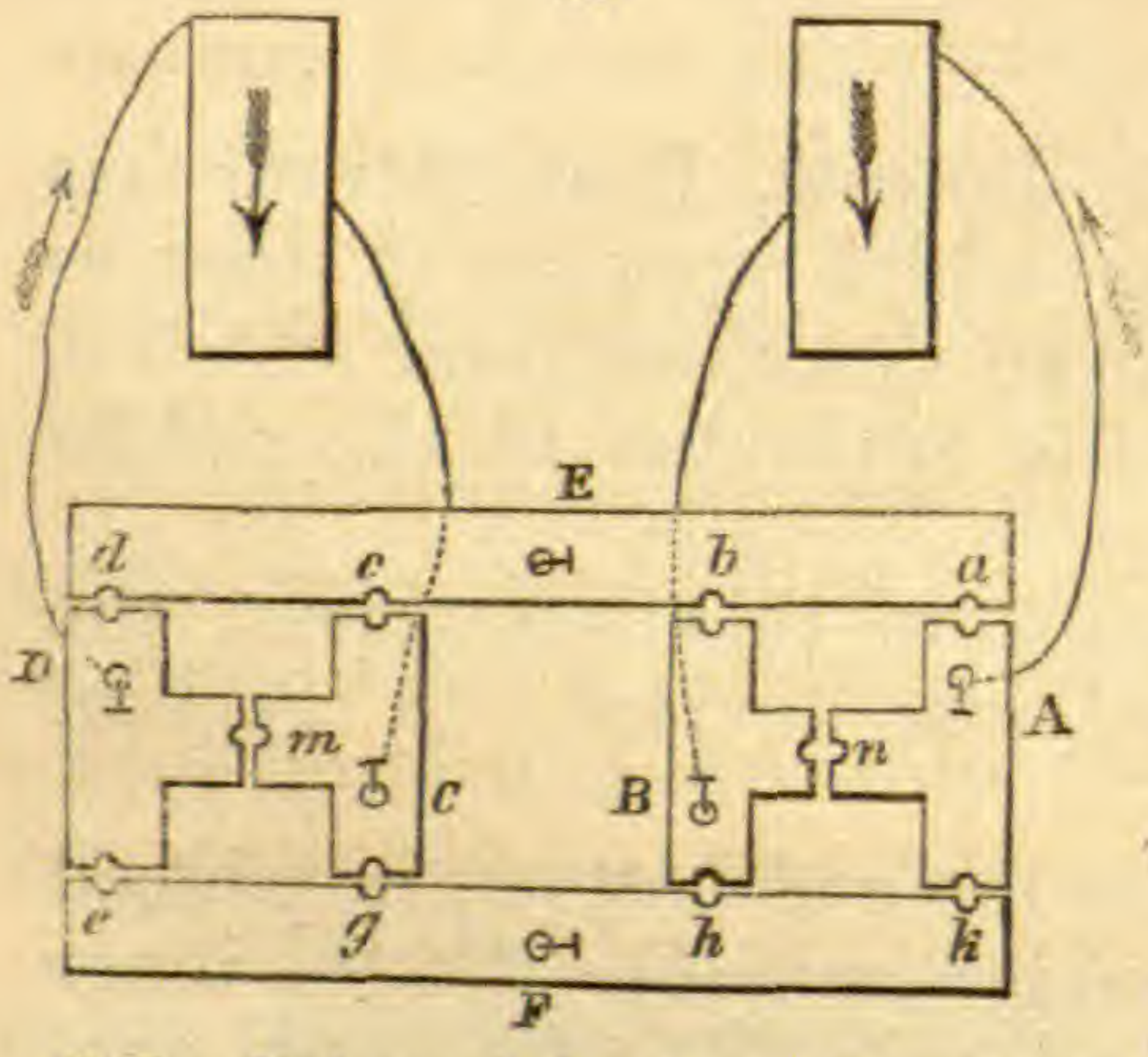
per wire, No. 14, wound upon cylinders of wood (*a*).* Wooden rods (*c*), clamped so as to move with gentle friction, bear a wire (*w*), from which an astatic system of needles is suspended by means of a silk fiber. The upper needle is midway between

1.



the centers of the two coils. The lower needle plays over a scale (*s*) photographed on glass, beneath which is the horizontal condensing lens of the vertical lantern. The needles are ordinary sewing needles, and are each 1·5 inches in length.† Each coil is composed of 34 7 meters of wire, the resistance of which is 0·444 ohms. Each coil should have the same number of windings, and the same resistance. This is easily effected by care in winding. By sliding the rods (*b*) in or out, the distance between the coils may be varied from 2 cm. to 10 cm., the image of the lower needle being in all cases perfectly distinct. In this way the instrument is adjusted to currents of any strength. Scales cut in the rods (*b*) serve to regulate the distances.

2.



On the outside of the box are six plates of brass, whose form and arrangement are shown in fig. 2. The extremities of the coils are connected with the four plates A, B, C, D. This connection may be made by means of binding-screws on the inside of the box, in which case the coils may be replaced with ease by others of greater or less resistance. The plates are put in metallic contact by means of

* For Duboscq's lantern, the coils must be placed lower than here represented.

† The lower needle may be replaced by a bristle from a painter's brush, or some other light pointer, the upper one being damped by magnets as recommended by Mayer.

brass plugs, inserted at *a*, *b*, *c*, *d*, *e*, *g*, *h*, *k*. Putting plugs at *h* and *e*, and connecting the poles of a galvanic cup at the binding-screws A and C, and the current runs successively through the two coils R, each causing deflection in the same direction. Let R represent the resistance of one coil of the galvanometer, then the resistance of the galvanometer will be 2R. This arrangement is used in working with ordinary galvanic currents.

If instead of the former connections, plugs be put at *a*, *d*, *g*, and *h*, the wires from the source of electricity being connected at E and F, then the galvanometer resistance becomes $\frac{1}{2}$R. This arrangement is to be used with circuits of small resistance, such as thermo-currents. For this kind of work the instrument is thoroughly adapted.

This instrument can also be used as a differential galvanometer. To do this, put the positive pole of the battery at E. Plug *a* and *c*. Divide the negative wire into two equal branches which are to be connected at B and D. The circuit being thus closed, the needle evidently remains at zero. Introducing any wire the resistance of which is to be determined, into one branch, bring the needle to zero again by introducing known resistances into the other, and the unknown resistance is readily determined. In measuring fractions of an ohm, a rheochord is, all things considered, the best. The contacts are good, and an audience obtains a better idea of what is meant by electrical resistance than when a resistance box alone is used. Using platinum wire weighing 7·37 grams per meter, the resistance of which is one ohm to 192·9 cm. of wire (which is 96·45 cm. on the instrument scale), and thousandths of an ohm can be measured direct.

If ground connections are made the negative pole of the battery is sent to ground direct, and the branches of the current from B and D are sent to ground through the unknown resistance and the resistance box respectively.

Shunts may be introduced into either of the half circuits. This may be done by introducing coils of resistance $\frac{1}{9}$R or $\frac{1}{99}$R, between the binding screws A, B or C, D. These wires may also be wound upon metallic plugs, which have been split lengthwise, the parts being insulated and each being connected with one extremity of the wire. Permanent shunts may be introduced by connecting one extremity with plates A or D, the other extremity being attached to an insulated plate, to be put in contact with B or C by means of a solid metallic plug. These shunts are used in Latimer Clark's differential galvanometer, and their use in measuring resistance is too well known to need further explanation.

The advantages possessed by this galvanometer are:

1. It is easily adjusted to any vertical lantern, from which it can be removed in a moment if desired.

2. The distance between the deflecting coils being readily varied, it can be adjusted to currents of various intensity.

3 The resistance of the galvanometer is quickly varied from one-half, to twice the resistance of one of the galvanometer coils.

4. The coils may be replaced by others when desired.

5. It can instantly be converted into a differential galvanometer and used in measuring resistance.

6. It can be constructed in any work-shop at a very small expense.

St. Louis, Oct. 25, 1875.

ART. XI.—*On a new occurrence of Tartronic Acid, with some remarks on the Molecular Structure of Glyceric Acid;* by SAMUEL P. SADTLER.

(Read before the American Philosophical Society, September 17, 1875.)

IN the Propyl series, nine normally formed acids are possible, besides several isomeric unsymmetrically formed ones. They are:—

I.	IV.	VII.
$C_3H_6O_2$	$C_3H_6O_3$	$C_3H_4O_4$
CH_3	$CH_2.OH$	$CO.OH$
CH_2	CH_2	CH_2
$CO.OH$	$CO.OH$	$CO.OH$
II.	V.	VIII.
$C_3H_6O_3$	$C_3H_6O_4$	$C_3H_4O_5$
CH_3	$CH_2.OH$	$CO.OH$
$CH.OH$	$CH.OH$	$CH.OH$
$CO.OH$	$CO.OH$	$CO.OH$
III.	VI.	IX.
$C_3H_4O_3$	$C_3H_4O_4$	$C_3H_2O_5$
CH_3	$CH_2.OH$	$CO.OH$
CO	CO	CO
$CO.OH$	$CO.OH$	$CO.OH$,

and the following are the acids considered as having the molecular structure just given:—

I, propionic acid; II, lactic acid (of fermentation); III, pyruvic or pyro-racemic acid; IV, ethylene lactic acid; V, glyceric acid; VI, carbacetoxylic acid; VII, malonic acid; VIII, tartronic acid; IX, mesoxalic acid.

In one or two of these cases, however, there is still a difference of opinion as to whether the acid named is the one possessing the normal molecular structure given above, or is only an isomer of it, having its carbon atoms differently united. Notably with glyceric acid is this yet an open question. Some results lately obtained in the course of a study of this acid appear to me to be of value for the solution of this question.

The other view of the molecular structure of glyceric acid makes it unsymmetrical, two of the carbon atoms being doubly united. The formula given is

$$\begin{array}{l} \quad CH_2.OH \\ \quad | \\ \ /C.OH \\ O\ \ | \\ \ \backslash CH.OH. \end{array}$$

As will be seen, this formula does not contain the carboxyl group, hitherto supposed to be the inevitable characteristic of an organic acid. The author of this theory is Prof. Wislicenus, of Würzburg, and the following are the reasons given in support of it. If lactic acid be acted upon with hydrogen iodide, α iodo-propionic acid is formed, according to the following reaction:

$$\begin{array}{llll} CH_3 & & CH_3 & \\ | & & | & \\ CH.OH & +HI= & CH.I & +H.OH \\ | & & | & \\ C.OOH & & CO.OH. & \end{array}$$

This when heated to 150° with strong HI is changed into propionic acid. If, on the other hand, glyceric acid be acted upon with hydrogen iodide, β iodo-propionic acid is formed. If this had the formula

$$\begin{array}{l} CH_2I \\ | \\ CH_2 \\ | \\ CO.OH, \end{array}$$

on treatment with moist silver oxide, it would pass into ethylene lactic acid. It does not, however, do this, but a new acid isomeric with ethylene lactic acid is formed—hydracrylic—

$$\begin{array}{l} \quad CH_2.OH \\ \quad | \\ \ /CH \\ O\ \ | \\ \ \backslash CH.OH. \end{array}$$

That the molecular structure of this acid is essentially different from that of ethylene lactic acid is proved by the oxydation products of the two. Ethylene lactic acid yields malonic acid, while hydracrylic does not yield a trace of this, breaking up into glycolic and oxalic acids and carbonic dioxide. Moreover, hydracrylic acid on heating yields acrylic acid, a derivative of allyl alcohol, instead of the lactid yielded by the lactic acids.

Prof. Wislicenus, however, frankly gives one experiment made by himself, the result of which tends the other way. He reduced the β iodo-propionic acid by sodium amalgam and obtained what appeared to be the normal propionic acid, showing the regular molecular structure.

In favor, moreover, of the normal structure for the molecule of glyceric acid is the formation of pyruvic or pyroracemic acid.

$$\begin{array}{l} CH_3 \\ | \\ CO \\ | \\ CO.OH \end{array}$$

from glyceric acid upon heating this to 140°, explained by the following reaction:

$$\begin{array}{lcl} CH_2.OH & & CH_3 \\ | & & | \\ CH.OH - H.OH = & & CO \\ | & & | \\ CO.OH & & CO.OH. \end{array}$$

The structure of this pyruvic acid is known from the fact that acted upon by nascent hydrogen it gives normal lactic acid.

A strong additional argument would be had, if we could show a connection between glyceric acid,

$$\begin{array}{l} CH_2.OH \\ | \\ CH.OH \\ | \\ CO.OH \end{array}$$

and tartronic acid,

$$\begin{array}{l} CO.OH \\ | \\ CH.OH \\ | \\ CO.OH. \end{array}$$

Hitherto tartronic acid had not been formed from glyceric acid, but only in an indirect way, by the spontaneous decomposition of nitro-tartaric acid, according to the following reaction:

$$\begin{array}{ll} CO.OH & CO.OH \\ | & | \\ CH.O(NO_2) & CH.OH \\ | & | \\ CH.O(NO_2) = & CO.OH + N_2O_3 + CO_2. \\ | & \\ CO.OH & \end{array}$$

However this mode of formation was interesting as tending to show its symmetry of structure. For that matter a dibasic, triatomic acid could hardly exist, except by the assumption of two carboxyl groups.

I have been fortunate enough to find tartronic acid associated with glyceric acid in the oxydation products of glycerine. The preparation of the two acids was as follows: One part by weight of glycerine is mixed with one part of water, and to the mixture is added, by means of a long funnel tube reaching to the bottom of the cylinder, about one and a quarter parts of red fuming nitric acid. After allowing them to rest until all gas evolution has ceased (which usually takes some six days), the solution is evaporated down at a gentle heat until the fumes of nitric acid are no longer perceptible. It is then very thick and syrupy. It is now diluted with water, and plumbic carbonate is added in excess. The oxalate and undissolved carbonate are filtered off, and the solution slightly concentrated and allowed to crystallize. The glycerate of lead deposits in thick crystalline crusts. These are separated from the mother-liquor, dissolved, and the lead precipitated out from the solution by sulphuretted hydrogen.

The colorless or light straw-colored filtrate is somewhat concentrated, and calcic carbonate is added to neutralization. The solution is filtered, if necessary, and to the filtrate is added 95 per cent alcohol. The calcium salts present are all precipitated, in greater part at once, and completely on standing twelve hours.

If the solution had been very concentrated the calcium salt is precipitated in a granular condition. If, on the other hand, it was more dilute, the salt only separates gradually, and has a beautiful micaceous and scaly appearance.

I had at first considered this precipitate to be pure calcium glycerate, but found on dissolving it in water, in order to free it from the lime and obtain the glyceric acid, that while the greater portion dissolved readily in warm water, a considerable portion, although not more than one-tenth of the whole amount, remained and dissolved only on continued boiling. This, when filtered off and washed in cold water, appeared as a dull white, almost impalpable powder, contrasting in appearance with the crystalline glycerate.

It was dried carefully at 100° until constant weight was obtained.

Calcium determinations were first made. Weighed portions were ignited in a platinum crucible once or twice with excess of concentrated sulphuric acid until the weight remained constant.

·5755 grms. salt yielded ·4925 grms. $CaSO_4$ equal to 25·22 per cent Ca.

·1759 grms. salt yielded ·1505 grms. $CaSO_4$ equal to 25·16 per cent Ca.

The theoretical per cent of calcium in calcium tartronate is 25·32, while in calcium glycerate, allowing for two molecules of water of crystallization, it is 13·99.

I had analyzed the micaceous preparation of calcium glycerate about the same time, and had gotten in two determinations, 14·03, 14·07 per cent of calcium respectively. The difference was so great that I could not understand it. On reckoning up the molecular weight, however, assuming one atom of calcium to be present, I got 159. The molecular weight of calcium tartronate is 158. Being dibasic, the molecular weight of the calcium compound is of course much less than the weight of the calcium compound of glyceric acid, a monobasic acid.

I endeavored twice to make a combustion of the salt in order to get the per cent of hydrogen and carbon. Each time calcium carbonate remained undecomposed at the heat of the combustion. I therefore gave them up.

I then took the remainder of my salt, grown rather small, to my great regret, and neutralizing the lime with oxalic acid, obtained the free acid. This, on concentration, deposited out crystals. On examination with a lens they were seen to be of tabular form, well agreeing with the appearance of tartronic acid obtained from nitro-tartaric acid. A combustion was made of these, and here, unfortunately, an accident to the potash bulbs lost me the carbon determination. The hydrogen determination, however, is given.

·4348 grms. salt yielded 13·23 grms. H_2O equal to 3·38 per cent hydrogen.

The theoretical per cent of hydrogen in $C_3H_4O_5$ is 3·33.

An important test that I wished to make but was compelled to forego for the time, was to act upon this tartronic acid with hydrogen iodide. Were its structure symmetrical, it should yield α iodo-malonic acid, which by further treatment with HI or with reducing agents would yield malonic acid.

Wishing to obtain larger quantities of the tartronic acid for further examination, I have since oxidized another portion of glycerine and treated the products in the same way. This time I got no tartronic acid whatever, at least only a trace of calcium salt remained undissolved on heating with water. Evidently here the oxidation had proceeded somewhat differently as no tartronic acid formed. This result is not surprising on reflection, as the oxidation by nitric acid is not capable of much control, and a product once formed is liable to be still further oxidized. Thus glyceric and tartronic acids are both liable to be oxidized into oxalic acid, which always forms in considerable though varying quantity. Indeed the oxidation of glycerine by nitric acid is now known to yield a variety of products, of which, however, no doubt some are secondary ones.

Thus Heintz* has proved that racemic, formic, glycolic and glyoxalic acids are all found associated with the glyceric and oxalic acids in this product.

The tartronic acid just found, therefore, is only one of sev-

* Ann. der Ch. und Ph., clii, p. 325.

eral smaller side-products. The known symmetry of structure of the molecules of all these side products, however, certainly argues in favor of a similar symmetry in the glyceric acid molecule.

There is one way of reconciling these two views of the structure of glyceric acid, and that is the assumption of the existence of two isomeric acids, of which one is normal and the other an unsymmetrical acid.

Some results that I have just obtained in purifying the calcium glycerate seem, indeed, to point this way. Should the unsymmetrical glyceric acid preponderate in this mixture, Wislicenus' reactions with hydrogen iodide are readily understood. Another fact, which should not be lost sight of, is that in the decomposition of β iodo-propionic acid by moist silver oxide, Wislicenus* obtained not hydracrylic acid alone, but three other products accompanying it, so that the decomposition was not so simple

I am now engaged upon a study of this question, and hope to be able to give more information upon it in a short time.

ART. XII.—*Note on the "Chloritic formation" on the western border of the New Haven Region*; by JAMES D. DANA.

THE rocks of the hilly region west of the New Haven plain are, for nine miles westward, metamorphic slates, and beyond this distance mostly gneiss. Immediately adjoining the region there is what Percival has called a "chloritic formation," the area trending about north-northeast; then on the west of this, with the same trend, (2) a hydromica slate, but little removed from argillite, becoming slightly garnetiferous toward the western limit; next (3) a glossy garnetiferous mica slate, containing some beds of gray semi-crystalline limestone; next (4) at Derby, common gneiss and coarse porphyritic gneiss. These rocks are involved in one system of folds, and are throughout conformable in bedding.

The rock of the "chloritic formation" varies much in texture and composition in passing from the Sound northward. Near Savin Rock, on the Sound, it is a chloritic hydromica slate. The gray and slightly silvery surface is more or less blotched and lined with the olive-green of chlorite, and the rock has in the mass in general a greenish tint. The slaty structure is usually perfect, and yet some layers fail of it. Grains of magnetite are common, and, less so, those of pyrite.

* Ann. der Ch. und Ph., clxvii, p. 41.

This slaty variety of the rock continues with little change for a mile and a half north. Beyond, the massive layers increase in extent. At the deep Derby railroad cut, two miles north of Savin Rock, the massive variety constitutes more than half of the rock exposed in the sections; and it is not all in separate beds; for thick beds that are slaty in one part are in others for many rods massive, and it is impossible to separate the massive from the slaty by any stratigraphical planes.

This massive rock is commonly without a trace of bedding; at the same time, it is variously and extensively jointed, so that it affords only deceptive indications of strike or dip. It varies in color from greenish gray to dark olive-green and blackish gray. Some of it is almost cryptocrystalline; but in general the texture is fine granular. Part of it is porphyritic with small crystals of a whitish feldspar.

Between this Derby cut and "Maltby Park," a mile and three quarters west of north, this massive rock constitutes nearly all the outcrops; and in some places the porphyritic variety is pale greenish gray, from the thickly crowded feldspar crystals.

Over Maltby Park the rock is again slaty and silvery, often with blotches of chlorite—a chloritic hydromica slate—as at Savin Rock; yet with enough of both the ordinary and porphyritic *massive* kinds among the slaty layers to exhibit its close relation to the rocks farther south. The slate occasionally has the chlorite in large lenticular concretions, and now and then is light gray and contains crystals of pyroxene. In some places, especially along seams, it is epidotic. Veins and seams of quartz are numerous. In the slate there are interrupted beds of limestone. Part of the limestone contains serpentine and is a handsome verd-antique marble; and with the serpentine there are often grayish green cleavable pyroxene (sahlite), asbestus and chromic iron.

A mile farther north, or five miles from Savin Rock, (west of Westville), the rock is almost wholly a dark green chlorite slate—the micaceous part absent.

The rocks in the course of the five to six miles are—in recapitulation—commencing at the Sound :

For $1\frac{1}{2}$ miles, chloritic hydromica slate, little of it massive.
$1\frac{1}{2}$ to $2\frac{1}{4}$ miles, chloritic hydromica slate, much of it massive.
$2\frac{1}{4}$ to 4 miles, massive chloritic rock with little of it slaty.
4 to $4\frac{3}{4}$ miles, chloritic hydromica slate, very little of the rock massive.
$4\frac{3}{4}$ to 6 miles, mostly dark green chlorite slate.

It is to be noted that throughout the formation the slaty and massive portions are so associated, sometimes as alternating beds, sometimes as parts of the same beds, that *their common metamorphic origin* cannot be questioned—a point that I have studied for years.

The resemblances of the massive rock to trap was long since noticed by Professor Silliman, who, in a paper on the geology of New Haven and its vicinity, published in 1811 in Bruce's Mineralogical Journal, called it "primitive greenstone." In fact, the similarity in external aspect is so close that hand specimens from some portions of it would without question be pronounced trap—that is, doleryte, diabase, or melaphyre—by the most experienced lithologists.

In 1872, an incomplete analysis of the feldspar in the pale grayish green porphyritic rock, outcropping just south of Maltby Park, was made, by Mr. Edward S. Dana. The amount of silica afforded by the feldspar having been found to be but 45 per cent, the conclusion suggested was that the rock consisted largely of labradorite, and that it was probably essentially identical with part of the trap of the Connecticut valley dikes. In view of the presence of chlorite, I hence regarded the compact rock of the region as a metamorphic diabase; and it is the rock specially referred to under that name in the last edition of my Manual of Geology.

Still, the analysis, besides being incomplete, was not satisfactory because the feldspar crystals, although of the normal hardness, were granular in texture, without good cleavage, suggesting that they might possibly have undergone a partial alteration. On account of Mr. Dana's departure for Europe, he was compelled to leave the investigation he had begun unfinished; and so it has remained until this summer, when it was taken up, at my request, by the skillful analyst connected with the mineralogical department of the Sheffield Scientific School of Yale College, Mr. George W. Hawes. His results prove that the rocks are in fact *metamorphic doleryte*, *metamorphic diabase*, and *metamorphic melaphyre;* the first two, labradorite rocks, and the last an oligoclase variety. To distinguish these metamorphic rocks from the igneous of the same composition, they are named, on my suggestion, *metadoleryte*, *metadiabase*, and *metamelaphyre.* The examples are part of a long series of rock species which have representatives both among igneous (or intrusive) and metamorphic rocks. Other kinds are *dioryte* and *metadioryte*. *syenyte* and *metasyenyte*, *felsyte* and *metafelsyte*, etc.

We have here the important geological fact that labradorite is a prominent constituent of certain metamorphic rocks which have the aspect of much dioryte, and which are probably of Lower Silurian origin.* The labradorite—a lime-and-soda feld-

* On the question of their age I have collected many facts and propose before long to publish.

spar—must have been a result of the metamorphic process. And its formation was probably favored by two conditions in the original mud-beds so changed; (1) the presence of a comparatively small percentage of silica, or not over 50 per cent; and (2) the presence of much disseminated carbonate of lime probably derived mainly from pulverized fossils. The fact that the chloritic formation contains two or more beds of limestone is reason for supposing that the mud elsewhere may have been more or less calcareous. The oligoclase of the metamelaphyre required for its formation only that the mud should contain a little more silica and soda and less lime.

The terms *doleryte* and *diabase* are here retained for the *igneous* rocks which have been so called—*diabase* being applied to the chloritic variety of doleryte.* Some German works on Lithology restrict the term doleryte to dolerytes not older than the Tertiary, and call the other kinds, whether chloritic or not, diabase. But this is giving different names to the same compound; and it is making geological age—a criterion fortunately never considered in the naming of other rocks—override difference of mineral composition.

ART. XIII.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXVII.—*The Rocks of the "Chloritic formation" on the Western Border of the New Haven region;* by GEORGE W. HAWES.

THE rocks which compose the ridge fronting the New Haven plain on the west, in the town of Woodbridge and Orange, and which have been described in the foregoing article by Professor Dana, bear, as he states, a close resemblance to the trap rocks of the Connecticut Valley. It hence becomes interesting to ascertain whether the similarity is sustained by their chemical composition and mineral constituents.

As in the case of the trap, these rocks are of different kinds. *First*, dark-colored crystalline rocks very similar in color, texture, fracture, and specific gravity, to the undecomposed dolerytes of this region; and *second*, rocks which are more or less green and appear to be chloritic, very closely resembling the diabase. The latter kind has its porphyritic varieties. Besides these there is a *third* kind which contains a higher percentage of silica, and has the composition of melaphyre.

I. *Metadoleryte.*—A specimen collected from an outcrop about a mile south of Maltby Park (on what was formerly Mr.

* On the origin of this chloritic condition of part of the trap of the Connecticut valley—the part distinguished here as diabase—see this Journal III, vi, 104, 1873; also Mr. G. W. Hawes, ibid., ix, 191, 1875.

Stœckel's farm) was selected for analysis. It was crystalline-granular in texture; and it would be hard to detect by the eye any difference between it and many kinds of doleryte which are found in this region. The analysis shows that in chemical composition also it is very nearly the same. An analysis of a specimen of true igneous doleryte, from the trap ridge called West Rock, in New Haven, is placed beside it for comparison.

METADOLERYTE, FROM STŒCKEL'S FARM.

	I.	II.	Mean.		Doleryte, from West Rock.
Silica	50·40	50·32	50·36		51·78
Alumina	14·43	14·71	14·57		14·20
Ferric oxide	2·48	2·47	2·48		3·59
Ferrous oxide	8·28	8·35	8·31		8·25
Manganous oxide	·43	.49	·46		·44
Lime	11·15	11·11	11·13		10·70
Magnesia	7·65	7·59	7·62		7·63
Soda	3·01	3·08	3·04		2·14
Potash	·43	·44	·44		·39
Titanic acid	1·65	1·74	1·70		
Chromic oxide	*tr.*	*tr.*	*tr.*	P_2O_5	·14
Ignition	·74	·83	·78		·63
	100·65	101·13	100·89		99·89
Specific gravity			3·04		3·03

The close resemblance between the igneous and the metamorphic rock will be noticed; they differ from one another less than do the different varieties of doleryte. Moreover, observations made upon thin sections indicate that the rock is composed of pyroxene, a triclinic feldspar, and a black opaque mineral which the analysis shows to be titanic iron. The pyroxene is a dark-green variety, but clear and undecomposed. If we assume that the pyroxene of this rock is of the same composition as that of the New Haven dolerytes,* the magnesia indicates that it contains 55 per cent of this ingredient, which being subtracted along with 3 per cent of titanic iron, leaves 41 per cent of a mineral, the oxygen ratio of which is very near to 1 : 3 : 6—proof that the feldspar is labradorite. Hence, the physical appearance, the chemical composition, and the proportion between the mineral constituents all show a very close resemblance to doleryte. The name of *metadoleryte* seems therefore to be particularly appropriate for this rock.

2. *Metadiabase*.—The chloritic variety, which has been referred to, resembles diabase in appearance as closely as the preceding kind does doleryte. There are, however, wider limits of variation in texture and in the proportion between the mineral constituents than is noticed in diabase; for the rock is sometimes uniformly crystalline, and sometimes coarsely

* See this Journal, III, ix, page 187.

porphyritic. As would be supposed, there are no amygdaloidal cavities or geodes either in the mass or in microscopic sections. The analysis was made upon a specimen collected at the Derby railroad cut where there is a fine display of these rocks. The specimen was uniform in texture and of a light-green color.

METADIABASE, FROM THE DERBY RAILROAD CUT.

	I.	I.	Mean.
Silica	48·25	48·15	48·20
Alumina	14·22	14·01	14·12
Ferric oxide	1·95	2·05	2·00
Ferrous oxide	7·39	7·43	7·41
Manganous oxide	1·30	1·19	1·24
Lime	11·53	11·47	11·50
Magnesia	8·26	8·11	8·19
Soda	2·63	2·56	2·60
Potash	·24	·23	·23
Titanic acid	1·61	1·55	1·58
Water	2·11	2·29	2·20
	99·49	99·04	99·27
Specific gravity			3·02

The analysis, taken with the observations made upon thin sections, shows that the rock is a mixture of pyroxene, chlorite, labradorite, and titanic iron, which are the constituents of diabase; and hence this metamorphic rock is appropriately distinguished by the name *metadiabase.* The absence of carbonate of lime is noticeable, showing that in this case the chlorite was formed simultaneously with the pyroxene, and not at the expense of the pyroxene, as in the case of the diabase of the trap dikes of the Connecticut valley, which always contains carbonate of lime as one result of the change. This rock in places contains pyrite, which is also frequent in trap.

PORPHYRITIC METADIABASE; SOUTH OF MALTBY PARK.

	I.	II.	Mean.		Diabase of Saltonstall Ridge.
Silica	48·57	48·65	48·61		49·28
Alumina	17·78	17·85	17·81		15·92
Ferric oxide	·35	·16	·25		1·91
Ferrous oxide	8·44	8·48	8·46		10·20
Manganous oxide	·20	·20	·20		·37
Lime	11·17	11·14	11·16		7·44
Magnesia	7·78	7·74	7·76		5·99
Soda	2·73	2·82	2·77		3·40
Potash	·47	·47	·47		·72
Titanic acid	1·35	1·35	1·35	CO_2	1·14
Water	1·60	1·65	1·63		3·90
	100·44	100·51	100·47		100·27
Specific gravity			3·01		2·86

There are varieties of this rock intermediate between these two, some specimens of which are beautifully porphyritic. In some kinds the feldspar is free from impurities; but in those varieties which are very feldspathic, and the feldspar crystals largest, these crystals are quite impure from the envelopment of chlorite. The porphyritic rock, from an outcrop near the Orange road, just south of Maltby Park, containing clear crystals of feldspar, was analyzed, and the result is given on the preceding page. An analysis of the diabase of Saltonstall Lake, from my former paper, is added for comparison.

This porphyritic rock is composed of the same minerals as the more compact varieties, for all of the ingredients can be easily recognized under the microscope. The possible presence of anorthite in the rock is suggested by the following analysis of some large grains of feldspar taken from an adjoining rock: SiO_2 45·52, Al_2O_3 29·84, MgO 2·35, CaO 15·99, NaO 1·61, KO ·37, ignition 2·38 = 98·06. This analysis was made by Mr. E. S. Dana some years since, but he states that the microscopic examination, and the analysis itself, show that the grains were very impure crystals of a triclinic feldspar, and as all the calculations upon the analyses point to the presence of labradorite, we cannot assume that any of the rocks which have been analyzed contain anorthite, though it is very likely to exist in the rocks of the series, since a constant composition in the feldspar could not be expected in the different layers of a rock made up of shifting sediments.

3. *Metamelaphyre*—a specimen taken from an outcrop on Stœckel's farm is so fine grained as to appear nearly cryptocrystalline; it is broken into angular fragments like some of our trap rocks, and in fact resembles some compact trap so closely as to make it impossible to distinguish it by the eye alone. Its analysis afforded the following results:

METAMELAPHYRE, FROM STŒCKEL'S FARM.

	I.	II.	Mean.
Silica	55·03	55·10	55·07
Alumina	14·38	13·98	14·18
Ferric oxide	7·15	7·25	7·20
Ferrous oxide	1·85	1·99	1·92
Manganous oxide	·30	·30	·30
Lime	9·05	9·01	9·03
Magnesia	6·02	5·94	5·98
Soda	4·08	4·14	4·11
Potash	·38	·37	·37
Titanic acid	1·56	1·56	1·56
Water	·68	·75	·72
	100·48	100·39	100·44
Specific gravity			2·99

If we assume that the pyroxene of this rock has the same composition as that of No. 1, we calculate, from the magnesia that it contains, 44 per cent of this ingredient; then, deducting three per cent of titanic iron, we have left a remainder of 53 per cent, which has very exactly the ratio and composition of oligoclase. This mineral constitution appears to be justified by the microscopic examination, since no free quartz or other mineral can be detected. If we restrict the use of the term melaphyre, as it is done in some recent works on lithology, to a mixture of oligoclase and pyroxene, with some titanic iron, the rock here analyzed is melaphyre in composition as well as appearance; and being a metamorphic rock, it is *metamelaphyre.*

We thus have representatives of the larger part of the pyroxenic igneous rocks, in positions which show conclusively that they are of metamorphic origin. The fact that metamorphic action can produce rocks exactly like the igneous in external aspect and chemical constituents is of great interest in the study of rocks.

ART. XIV.—*On a new Tertiary Lake Basin;* by GEORGE B. GRINNELL *and* EDWARD S. DANA.

SEVERAL Lake Basins of Tertiary age have already been discovered in the Rocky Mountain region, and the more important of them have been carefully explored. Those of Eocene age have only been known since 1870, but the Miocene deposits of the White River have long been noted for their wonderful scenery, as well as for the number and variety of the mammalian remains found in them. Another Miocene basin is known in Oregon, and both the lake beds of this period are overlaid by deposits of Pliocene age.*

During the explorations carried on last summer under the direction of Col. Wm. Ludlow, Corps of Engineers, a series of Tertiary deposits were identified by the writers near Camp Baker, Montana. These deposits indicate the existence in this region of a Miocene lake basin, which was succeeded by another lake basin in Pliocene time. As these basins are quite distinct from those heretofore known, it is considered important to put the fact of their discovery on record.

Camp Baker is situated on Deep Creek, a stream which flows into the Missouri River above Sun River. It lies about fifty miles nearly due east of Helena. It is surrounded on all sides by mountains, of which the Big Belt Range, lying immediately to the south or southwest, is the highest and most conspicuous.

* This Journal, III, vol. ix, p. 49, Jan., 1875.

The Little Belt Mountains lie to the north, and the Crazy Woman Mountains to the southeast, though at a greater distance.

The Tertiary beds found here consist for the most part of homogeneous cream-colored clays so hard as to be with difficulty cut with a knife. The beds are horizontal and rest unconformably upon the upturned yellow and red slates below. The clays of which they are formed resemble closely those found in the Miocene beds at Scott's Bluffs near the North Platte River in Wyoming. The deposits at Camp Baker have been extensively denuded and nowhere reach any very great thickness. At a point about three miles southeast of the Post, some bluffs were noticed where the Miocene beds attained a thickness of 200 feet, and these were capped by fifty feet of Pliocene clays, both beds containing characteristic fossils. In the underlying Miocene beds were found a species of *Rhinoceros*, several species of *Oreodon* Leidy and *Eporeodon* Marsh, a canine tooth apparently of *Elotherium* Pomel, and remains of Turtles. In the Pliocene beds the principal fossils were a species apparently of *Merychyus* Leidy, remains of an equine smaller than the modern horse, and Pliocene Turtles. These fossils have not yet been carefully studied, and for this reason their relations to the remains found in the other lake basins of similar age cannot here be stated.

We saw the first exposures of these beds a few miles west of the Sulphur Springs, just after crossing a rather high ridge of trachyte through which Deep Creek flows in a narrow and picturesque cañon. This point is about six miles southeast of Camp Baker. From here the lake bed was traced continuously along Deep Creek for a distance of fifteen miles, extending quite up to the mountains on the eastern side at least. Beds of the same character, containing similar fossils, were found on White Tailed Deer Creek, a branch of Deep Creek, about seven miles to the north of Camp Baker, as well as on Camas Creek to the southwest of the Post. Traces of this deposit, containing what appear to be remains of *Rhinoceros*, were also found two miles or more south of Moss Agate Springs, and at a considerable elevation above the creek bed. With more time than we had at command they could no doubt have been traced much farther, although in many places the beds have been washed out, or have been covered by the later local drift.

These Tertiary beds were all laid down after the elevation of the mountains and the igneous eruptions. They are, as has been said, perfectly horizontal, and are often seen covering over ridges of trachyte. The line of separation between the Miocene and Pliocene beds is in some places well marked. It consists of about six feet of hard sands, interstratified with layers of very small water-worn pebbles soldered together into a hard mass, but easily picked out with a knife. Each of these layers is about six inches in thickness. Immediately above these strata the Pliocene fossils were found. In several places fragments of trachyte were noticed in the Pliocene beds.

Near Camp Baker are a series of upturned ridges of Potsdam sandstones and limestones at a level very little above that of the Tertiary beds, and doubtless in this region the lake was divided into many arms, which bent around, and extended among, these ridges.

It is known that in the neighborhood of Fort Shaw, and near Helena, Pliocene deposits exist, and at Fort Ellis and in the valley of the Yellowstone we saw, but were unable to examine, gray sands and marls, which Dr. Hayden refers to the same age. No Miocene beds, however, have been identified at any of these localities. It seems probable that in Pliocene time at least, the Baker Lake may have extended north to the Missouri River, and perhaps up that stream to the Three Forks, thus connecting with the lake which existed near Fort Ellis. Indeed it would seem that we just touched upon the southern edge of this basin, which may have extended far to the north and west.

An interesting point in connection with these deposits, is the fact that they are at a much greater elevation than any other beds of the same age now known on the continent. The elevation of the White River and Colorado beds is about 3,000 feet, and that of the Oregon basin somewhat less, while that of the deposits near Camp Baker is over 5,000 feet.

In reference to the relations which this lake basin bears to the Oregon basin and to the White River deposits, nothing can be certainly known without a careful exploration of the whole region and a thorough study of its vertebrate remains. It is by no means impossible that the Baker Lake may have flowed into that at White River by some old river channel, but so little is known of the intervening country that no definite opinion can be pronounced on the subject.

ART. XV.—*Communications from the Laboratory of Williams College.* No. IV.—*On the Product of the action of Potassium on Ethyl Succinate;* by IRA REMSEN.

IN a notice published a short time ago in this Journal,* I described a few preliminary experiments, undertaken with the object of discovering the structure of a peculiar substance which is produced when potassium is allowed to act upon ethyl succinate. Since the time of the first publication, I have been engaged in prosecuting this investigation, the results of which are herewith communicated. The communication is hastened by the fact that quite recently a similar investigation has been undertaken in the laboratory of Wislicenus,† and in the publication of the experiments no reference is made to my work.

1. *Preparation and Properties.*

The substance under consideration was first obtained by v. Fehling‡ in the course of an exhaustive examination of the compounds of succinic acid. I give his description of the method of obtaining the substance: "If ethyl succinate, which has been thoroughly dried by means of calcic chloride, is brought in contact with potassium or sodium, the metal becomes oxidized, and the ether is decomposed. At the ordinary temperature the decomposition takes place more readily with potassium than with sodium. The action begins instantaneously; an inflammable gas is evolved which conducts itself like hydrogen. By gently heating the action is hastened; the mass becomes heated spontaneously, and care must hence be taken not to heat higher than 30–40° at first. In connection with the reaction a peculiar penetrating odor is perceived. If the action is too violent, the mass may easily be thrown out of the vessel in which it is contained."

"If sufficient potassium has been added the mass becomes thick and viscid, and the color of the mass is brown. This color appears to arise from secondary decomposition-products."—"If water is now added to the mass, and it be heated rapidly to boiling, a clear, yellow liquid is obtained, upon which an oily, yellowish layer floats; but it seems to be important not to heat for too long a time. The liquid congeals on cooling, forming a soft, pasty mass. By means of a filter the liquid is separated from a yellow crystalline mass, and the residue washed out with water."—"The yellowish residue upon the filter is purified by repeated recrystallizations from alcohol.

* Vol. ix, p. 120. † Berliner Berichte, viii, Jahrgang, 1039.
‡ Annalen der Ch. Pharm., xlix, 192.

The crystalline mass is now white with a slight tinge of yellow, possessing a beautiful satin-luster, and is very voluminous."

The analyses made agreed closely with each other and led to the formula $C_6H_8O_3$.

"The compound does not dissolve in water. Alcohol dissolves it readily, particularly with the aid of heat; cold ether dissolves it in every proportion. By heating with alkalies this product is decomposed, alcohol is given off which can easily be recognized by the odor; and a yellow solution is obtained, similar to that which was obtained at first by treating with water the mass which was produced by the action of potassium upon the ether. This solution contains potassic succinate." "The crystalline body fuses at 133° and sublimes completely at 206°. With ammonia this product forms a bright yellow body crystallizing in needles."

Since the time of the publication of the investigation of v. Fehling, this substance does not appear to have been reëxamined. Only Geuther* has indulged in some speculations in regard to its structure, though his speculations are not based upon new experiments. He proposed to double the formula of v. Fehling making it $C_{12}H_{16}O_6$, and then suggested that the compound was either disuccinic ether or diethyldisuccinic acid.

In view of the peculiar method of its formation, it seemed desirable to learn something more definite in regard to the chemical conduct of the body, and accordingly I prepared a considerable quantity of it and subjected it to examination. The statements of v. Fehling in regard to its preparation were found to be in the main correct. It is not a simple matter to tell when the reaction between the metal and the ether is at an end, as the mass becomes very thick, even while warm, and, the metal becoming covered with a layer of the fully decomposed mass is kept from further action. It is very important too, not to have an excess of the metal, for, as we shall see, the new subtance forms with potassic hydroxide, a compound which is easily soluble in water, and is also easily decomposed by the hydroxide, if the temperature is raised. I found it sufficient to recrystallize the product but once from alcohol, obtaining it thus almost pure, either in the form of laminæ with a strong luster, or of needles of considerable length. The alcoholic solution exhibits the property of fluorescence to a marked degree, but I have noticed that this property grows less marked the purer the compound becomes. The fusing point of the compound is given at 133° by v. Fehling, whereas I found it to be at 128°.

* Zeitschrift für Chemie, 1866, 5.

2. *Metallic Compounds.*

When sodium-amalgam is allowed to act upon the alcoholic solution of the compound, there is produced a voluminous red precipitate, which is very easily soluble in water. From the aqueous solution, chlorhydric acid precipitates a white substance which is insoluble in water and difficultly soluble in alcohol. This substance proved to be the original compound. The same red precipitate is produced when alcoholic solutions of the compound and potassic hydroxide are brought together, and a similar precipitate when sodic hydroxide is used instead of potassic hydroxide. The latter precipitate was first prepared for examination.

Sodium-Compound, $C_{12}H_{14}Na_2O_6 + 4H_2O$.

This compound was prepared by bringing together alcoholic solutions of the original body and sodic hydroxide. It is thrown down immediately, as a beautiful red precipitate. This precipitate consists of microscopic needles. It was filtered off and washed out with alcohol. In drying, the color changed from red to yellow, but it appears as though this change of color is not accompanied by a chemical change. The analyses gave the following results:

I. 0·201 grams of the substance gave 0·08 grams $Na_2SO_4 =$ 0·0259 grams Na.

II. 0·1202 grams of the substance gave 0·048 grams $Na_2SO_4 =$ 0·0155472 grams Na.

		Calculated.	Found.	
$C_{12}H_{14}O_6$	254	68·28	----	----
Na_2	46	12·37	12·89	12·93
$4H_2O$	72	19·35	----	----
	372	100·00		

According to this, the substance has the formula $C_{12}H_{14}Na_2O_6 + 4H_2O$. It is very easily soluble in water, and the body, $C_{12}H_{16}O_6$, is precipitated from this solution on the addition of an acid. By boiling with a little sodic hydroxide succinic acid is formed.

The corresponding potassium compound is mentioned by Wislicenus (loc. cit.) Another potassium compound of the formula $C_{12}H_{15}KO_6$ is also mentioned, the existence of which speaks clearly for the formula $C_{12}H_{16}O_6$ for the original substance, instead of the simple formula $C_6H_8O_3$.

Barium-Compound, $C_{12}H_{14}BaO_6 + H_2O$.

If an alcoholic solution of the substance $C_{12}H_{16}O_6$ is added to baryta water, a beautiful rose-colored precipitate is produced, similar to that produced with sodic hydroxide. This was filtered off from the solution of baryta-water and rapidly washed out with hot water. During the process of filtration the surface of the liquid on the filter was carefully protected from the influence of the air by a perforated cover which was connected with a tube containing potassic hydroxide. After all baryta had been washed away, the residue was boiled with alcohol, and again rapidly filtered in order to remove any uncombined $C_{12}H_{16}O_6$. It was then dried and analyzed with the following results:

0·1904 grams of the substance gave 0·1085 grams $BaSO_4$= 0·065798 grams Ba.

		Calculated.	Found.
$C_{12}H_{14}O_6$	254	62·10	----
Ba	137	33·50	33·51
H_2O	18	4·40	----
	409	100·00	

The formula is therefore $C_{12}H_{14}BaO_6+H_2O$. The compound is very stable. As was seen above it can be boiled with alcohol or water without undergoing decomposition. It is, however, decomposed by acids just as the sodium-compound is, the substance $C_{12}H_{16}O_6$ being precipitated.

Calcium-Compound, $C_{12}H_{14}CaO_6+H_2O$.

If lime-water is used instead of baryta-water, a precipitate is produced, which is, however, of a beautiful lemon-yellow color. This was purified in the same manner as the barium-compound. The analysis gave the following numbers:

0·2172 grams of the substance gave 0·0937 grams $CaSO_4$=0·0275 grams Ca.

		Calculated.	Found.
$C_{12}H_{14}O_6$	254	81·41	----
Ca	40	12·82	12·67
H_2O	18	5·77	----
	312	100·00	

Magnesium-Compound.—When the body $C_{12}H_{16}O_6$ is boiled in water with magnesia, a purple compound is produced, which resembles the above described compounds in many respects. This is probably the same compound that is mentioned by Wislicenus as having been prepared in another way.

We have here then a series of peculiar metallic compounds, which are not salts in the usual acceptation of that term. The substance $C_{12}H_{16}O_6$ is not an acid; at least it does not contain the group COOH, for it is entirely unaffected by the alkaline carbonates, and, its metallic compounds are themselves decomposed by carbon dioxide. Wislicenus proposes the formula

$$\begin{array}{l} CH_2.CO.CH.CO.O.C_2H_5 \\ | \quad\quad\;\; | \\ CH_2.CO.CH.CO.O.C_2H_5 \end{array}$$

for the original compound, according to which it is ethyl succinylsuccinate, a derivative of succinylsuccinic acid. The metallic compounds are explained by supposing the hydrogen atoms of the groups CH to be replaced by the metals, the hydrogen in these groups having a somewhat acid character. It seems to me that the great stability of the ether which we have above recognized speaks against this formula. Most acids, which consist of atoms grouped in the manner indicated by the above formula, are decomposed by boiling with baryta-water, whereas we have seen that this substance may be boiled with baryta-water without undergoing decomposition. It is true that we know very little concerning bibasic acids of this structure, and it is possible that what is true of the monobasic acids is only partially true of the bibasic. Be this as it may, it is certain that the experiments thus far published will not permit the positive conclusion that the above formula is the true one, and further investigations would be called for whether the formula is correct or not.

3. *Action of phosphoric Chloride upon the body* $C_{12}H_{16}O_6$.

With the hope of learning something more definite concerning the nature of the oxygen-atoms contained in the substance under examination, I next undertook the study of the action of phosphoric chloride upon it. At first I employed two molecules of the chloride to one molecule of the substance. When the two are brought together in a dry vessel, no action ensues until heat is applied. If the mass is very gently heated, the substance $C_{12}H_{16}O_6$ melts, and immediately reaction commences, and continues then without the further aid of heat, until the contents of the retort form a clear, homogeneous liquid. The reaction is accompanied by an evolution of chlorhydric acid, the amount of which, however, was such as to leave me in doubt whether this was a necessary product of the reaction, or was formed from the secondary decomposition of the phosphorus compound which distilled over. The oxichloride of phosphorus was distilled off by gentle heat, and the oil in the retort then treated with water. Decomposition soon began and there resulted a solid, insoluble product. On examination this proved to be the original substance $C_{12}H_{16}O_6$. As the amount of this product was comparatively considerable, I at once concluded that a chloride had been formed by the first reaction which by its decomposition with water yielded the mother-substance. This conclusion proved subsequently to be erroneous, in such a way as to show that the first error in judgment was partially excusable. In a second series of experiments, I employed four molecules of the chloride to one molecule of the substance, and thus reached new results. The same phenomena accompanied the reaction, that were noticed in the case already described. The direct product was a clear, yellow oil. This was treated with a little cold water. At first the oil simply fell to the bottom of the vessel, but in a short time decomposition commenced, and gradually the oil disappeared, a solid product remaining in its stead. On being filtered off and examined, the solid product proved to be a new acid, comparatively easily soluble in water. In the filtrate there was also contained a considerable quantity of the new substance, which was extracted by shaking with ether. The new acid crystallizes out of the concentrated aqueous solution in laminæ which are colored yellow. In alcohol it is exceedingly easily soluble. It dissolves in a little potassic carbonate, and is precipitated from the solution on the addition of a few drops of chlorhydric acid.

As this product is easily soluble in water, it is plain that it escaped me in the first experiment, by remaining dissolved in the water which served for the decomposition of the chloride, a sufficient quantity of water having been employed to dissolve the whole of the product, if the conversion of the substance $C_{12}H_{16}O_6$ into the chloride had been complete. That which was really found in the first experiment was simply a part of the original substance, which had not been acted upon by the chloride of phosphorus.

If we attempt to distil the chloride for the purpose of purification the mass is completely carbonized. A few drops of a colorless liquid boiling at a high temperature pass over, but the quantity of this liquid is too small to admit of an examination. It is decomposed by water, and the product is solid. It was impossible to determine the nature of the solid, owing to the small quantity obtained. It is not probable that it was succinic acid, for, in that case, the chloride from which it was obtained would have become solid at a low temperature, whereas it remained liquid even when cooled down to 0°.

If the product of the action of phosphoric chloride on the substance $C_{12}H_{16}O_6$ is heated for some time, it gradually becomes solid, or nearly so, and then has the appearance of a translucent resin. I analyzed this compound, but the numbers obtained did not agree in different analyses. It appears, thus, that condensation and decomposition of the chloride are caused by heat.

As the most important result of these experiments with phosphoric chloride, then, we see that, if four molecules of the chloride are caused to act upon one molecule of the substance $C_{12}H_{16}O_6$, a liquid chloride is formed which is decomposed by water yielding a new acid. I have not yet studied this new acid, and can, therefore, not state in what manner it is derived from the original substance. As it can apparently be prepared in any desirable quantity with comparatively little trouble, its examination will probably give interesting and positive results.

In addition to the results already recounted, I will mention the following:

1. *Acetyl chloride* exerts no influence upon the substance $C_{12}H_{16}O_6$. It simply dissolves it when gentle heat is applied, but, on cooling, the unchanged substance crystallizes out. This result could be anticipated with considerable certainty, as the presence of alcoholic hydroxyl in the substance was not at all probable. Still the experiment was necessary to prove the fact, no matter how probable it might appear.

2. *Ammonia* does not act upon the substance either in aqueous or alcoholic solution. v. Fehling (loc. cit.) states that with ammonia the body yielded a bright yellow compound crystallizing in needles. I endeavored in vain to obtain such a compound. I first boiled the substance with very strong aqueous ammonia; it remained unchanged. I then conducted dried ammonia gas into an alcoholic solution of the substance. The solution turned deep yellow in color, but I was unable to extract from it anything save the original substance. This indeed, sometimes crystallizes in needles—a fact which may have misled v. Fehling. By analogy we should expect the formation of a compound with ammonia corresponding to the metallic compound described above. It is possible that some change in the conditions may lead to its formation.

3. *Hydrogen* in the nascent state (from tin and chlorhydric acid) does not act upon the substance. If the group CO is present, it is difficult to see why this should not be converted into the secondary alcohol group CH.OH by the action of hydrogen.

4. A solution of *potassic permanganate*, as well as dilute *nitric acid*, oxidize the substance very slowly. The products of the oxidation I have not yet examined. In connection with the oxidation by means of potassic permanganate a peculiar phenomenon was noticed which deserves mention. I have stated that the oxidation took place slowly; the product was not an acid, so that the manganic oxide formed was precipitated; but, further, the substance oxidized was insoluble in water, so that the manganic oxide, being produced in contact with the

faces of the insoluble crystals, was deposited in even layers upon them, forming thus a complete envelope, and giving a genuine pseudomorph. I was at first deceived by this strange pseudomorph, believing it to be the product of the oxidation. It was insoluble in water, and appeared to be insoluble in alcohol. I found, however, afterward, that the alcohol dissolved the central portions of the pseudomorphs leaving the envelopes unchanged in form.

5. The substance was *heated with water* at 150° in a sealed tube. At this temperature decomposition took place, but not at a lower temperature. The products of the reaction were alcohol, and a solid, white crystalline substance which conducted itself in some respects like succinic acid. The alcohol was detected by placing the whole product in a flask and distilling with water. The distillate was tested by Lieben's reaction* for the formation of iodoform.

The experiments which have thus been described do not suffice to enable us to judge positively in regard to the structure of the substance under investigation. I have stated above the view held by Wislicenus, and also my objections to this view. It remains yet to be decided whether my objections are well founded, and this can be done only by the aid of new experiments.

No. V. *On the action of Ozone on Carbon Monoxide;* by Ira Remsen and Mase S. Southworth.

One of the most remarkable examples of so-called non-saturated compounds is carbon monoxide. If we accept the hypothesis of constant valence, the compound CO must possess free affinities, or, as some chemists believe, the two affinities of the carbon-atom, which are not saturated by the oxygen atom, must exercise an influence upon each other. We can not explain this case by assuming that two carbon-atoms are joined together by two affinities each, for we know that the formula of carbon monoxide is CO, and not C_2O_2 or a higher multiple, and, accepting this formula, it is plain that we cannot assume a double union of carbon atoms in the compound.

If, on the other hand, we accept the hypothesis of variable valence, believing that the valence of an element depends upon circumstances, we shall look in vain for circumstances which, in the one case, can cause the bivalence, in the other the quadrivalence, of the carbon-atom. A difference in temperature certainly does not cause the difference in valence. The atom

* Annalen der Chemie, Suppl. VII, 218.

of carbon is quadrivalent toward oxygen at the ordinary temperature and under ordinary conditions. How otherwise shall we explain the formation of carbon dioxide in the processes of decay, fermentation, etc.? But the atom of carbon is just as positively quadrivalent at high temperatures.

The comparative ease with which carbon monoxide takes up chlorine appears to prove that it possesses free affinities. But if we accept this as a proof of the existence of free affinities in carbon monoxide, we have still better grounds for believing that free affinities are present in ethylene, for this gas combines with chlorine much more readily than carbon monoxide does. Still the view is commonly held that in ethylene the two carbon-atoms of the molecule are united by the mutual action of two affinities of each atom.

These considerations show that the nature of carbon monoxide is, as yet, but very unsatisfactorily understood. The first question which suggests itself is this: How far are we justified in considering carbon monoxide as a body possessing free affinities?

If we attempt to answer this question entirely without prejudice, we see that the principal experiment which is supposed to prove the existence of free affinities in carbon monoxide is the above mentioned experiment with chlorine. Oxygen does not combine with carbon monoxide at the ordinary temperature. This is readily understood, for, in order that the carbon monoxide and oxygen may combine by direct contact of the two substances, the oxygen-molecule must first be decomposed into its constituent atoms. An interesting experiment in this connection has been described by E. Ludwig,* who shows that carbon monoxide is oxidized by chromic acid at the ordinary temperature forming carbon dioxide. In this case carbon monoxide is active enough to separate one atom of oxygen from chromic acid and to employ it for the formation of carbon dioxide.

We have occupied ourselves with an experiment similar to that described by Ludwig, and have obtained a different and unexpected result. It appeared to us to be of interest to know whether, at the ordinary temperature, ozone has the power to transform carbon monoxide into the higher oxide. According to the views which are commonly held concerning the nature of the substances experimented upon, the transformation mentioned could be predicted with a tolerable degree of certainty. Particularly is this the case, if we consider the result of Ludwig's experiment, for usually ozone gives up its extra atom of oxygen with still greater readiness than chromic acid does. There is indeed no substance in the whole field of chemistry which furnishes us with a better means for obtaining

* **Annalen der Ch. u. Pharm., clxii, 47.**

a free atom of oxygen than ozone. If then we bring in contact with ozone a substance, which in turn is capable of taking up an atom of oxygen without itself undergoing change; which, indeed, possesses an attraction for oxygen, we are certainly justified in expecting to see the two substances act upon each other. But the experiment gave the unexpected result that ozone does not act upon carbon monoxide.

Two very careful experiments were performed. Pure carbon monoxide free of dioxide was first collected in a gasometer. This was then conducted from one side through three cylinders containing potassic hydroxide and lime-water into a flask. From the other side a current of oxygen was conducted through potassic hydroxide and lime-water, and then through a tube, in which the oxygen was converted into ozone, into the same flask. This flask was provided with a stopper having three holes. From the third hole a tube led to a cylinder containing lime-water; and this cylinder was connected with a final cylinder containing potassic hydroxide. Let us see what purposes the different parts of the somewhat complicated apparatus served. In the first place, the carbon monoxide was caused to pass through potassic hydroxide and lime-water in order to absorb every trace of carbon dioxide which might be present. The oxygen was treated similarly for a similar purpose. The ozone generator employed was that described by Wright* for use with the Holtz electrical machine, the best conditions being retained throughout the experiment for the working of the apparatus. The pure carbon monoxide and the ozonized oxygen were then caused to meet in the final flask, the inside of which was moist, as, for some unknown reason, ozone does not exhibit its oxidizing properties as well when dry as when moist. The mixture of the two gases, and any carbon dioxide which might have been formed, were then passed together into lime-water, contained in a cylinder, the lime-water being protected from the influence of the carbon dioxide of the air by the potassic hydroxide contained in the last cylinder.

Slow currents of carbon monoxide and oxygen were now passed through the apparatus, and, although the action was continued for a long time, not a trace of a precipitate could be detected in the last cylinder, containing lime-water. The strength of the gas-currents was frequently changed, but nothing brought about the expected result.

In view of the importance of the experiment we were not satisfied with this one form of it. As direct sun-light greatly facilitates the combination of carbon monoxide with chlorine, it seemed probable that it would be of service in causing the combination of the two gases under examination; and, accord-

* This Journal, vol. iv, July, 1872.

ingly, we repeated the described experiment with the following modifications: The final flask, above mentioned, in which the carbon monoxide and the ozone were brought together, was replaced by two large glass balloons, and these were placed in the direct light of the sun. Again slow currents of carbon monoxide and ozone were passed through the apparatus for hours, the rapidity of the currents being varied at different times.

In this case also we obtained only a negative result. We hence are in a position to assert positively that carbon monoxide is not oxidized by ozone.

If we now bear in mind that ozone acts destructively upon a great many saturated stable compounds, that one of the atoms of the ozone molecule has a great tendency to unite with other bodies, then the result of the above described experiments remains inexplicable. It shows at all events that carbon-monoxide itself, at the ordinary temperature, has no very great tendency to unite with oxygen, for, if our ideas in regard to the nature of ozone are correct, the conditions for such union were very favorable in our experiment.

We hope gradually to be able to experiment more fully upon this interesting subject with the object of collecting material which may enable us better to understand the nature of the so-called non-saturated compounds. We propose next to study the action of hydrogen peroxide upon carbon monoxide.

December, 1875.

Art. XVI.—*Mineralogical Notes*; by Edward S. Dana.—No. I. *On the Optical Character of the Chondrodite of the Tilly Foster Mine, Brewster, New York.*

In a memoir on the Brewster chondrodite, published in the third volume of the Transactions of the Connecticut Academy, I have given the results of an optical examination of chondrodite crystals of the *second* type.* It was there shown that the optic axes lie *not* in the basal plane, but in a plane making an angle of about 154° 10′ with the base; and, in consequence, that the crystals of this type, at least from that locality, belong optically not to the orthorhombic system, but to the *monoclinic*, while the various measurements proved that the deviation in angle from the orthorhombic type could not be greater than 2 or 3 minutes. A recent repetition of the measurements with the stauroscope on the same crystals, and also on another not examined before, confirm the results ob-

* See also this Journal, III, ix, 63, for extracts from the paper.

tained, and leave no room for doubt on the subject. The following is the evidence on this point thus far obtained.

Measurements on four independent crystals gave for the supplement angle made by the plane of the axes:

I.	With e^1 ($\frac{2}{3}$-$\breve{i}$=203), 18° 9′;	hence with basal plane,	25° 50′.
	With e^2 (2-$\breve{i}$=201), 45° 9′;	" " " "	25° 46′.
II.	With e^a ($\frac{2}{5}$-$\breve{i}$=205), 40° 55;	" " " "	25° 59′.
III.	With B (i-$\breve{i}$=100), 65°–70°;	" " " "	20°–25°.
IV.	With the basal plane, direct measurement,		25°.

I have since made an optical examination of a crystal of the third type. One single crystal of this type allowed of a stauroscopic examination. Only a small portion of it was transparent enough for use, but the circumstances allowed of a very exact adjustment according to the method of Groth, and the probable error cannot exceed one degree. The measurement gave for the supplement angle between the base and the plane of two of the axes of elasticity 7½°, a result which, like the corresponding one obtained for the second type, is at variance with the supposed orthorhombic character of the species. The series of measurements were made at different times with independent adjustments, but no considerable variation was found in the result, so that it may be considered as being above question. It is remarkable that the correspondence between the two types is not greater. In crystalline form the third type is between the first and second. I have to regret that no satisfactory material is at hand for the extension of these investigations to the Vesuvian humite.

It may not be out of place to state here that, through the kindness of Mr. Cosgriff, the Yale College Cabinet has recently received some exceptionally large crystals of chondrodite from the Tilly-Foster Iron Mine. The crystals were quite perfect, and four inches or more in length. Like all the large crystals they are partially altered, and have therefore little luster. They are penetrated with serpentine and brucite derived from their alteration.

ART. XVII.—*On Hermannolite, a new species of the Columbium group;* by CHARLES UPHAM SHEPARD, Sr., Mass. Professor of Natural History in Amherst College.

IN vol. l, p. 90, of this Journal (1870), I described as probably new, a Columbium mineral from Haddam, Connecticut, to which in June last* I gave the name of Hermannolite, in honor

* See Popular Guide to the Museums of Amherst College, p. 71.

of Dr. R. Hermann of Moscow, to whom chemistry has been so much indebted for the elucidation of this difficult group of minerals. By reference to my description of the mineral it will be seen that I went no further than to determine the proportions of the bases, and of the metallic acids with which they were united, without attempting to ascertain the order in which the latter were present. I thus found:

Metallic acids,	78·30
Protoxide of iron,	13·86
Protoxide of manganese,	7·72
	100·28

Desirous of learning the exact proportions of the different acids, I availed myself of an opportunity during the past summer of sending specimens to Mr. Hermann for this purpose. He has had the goodness to perform the analysis, and to communicate to me his results in the following letter.

"Your opinion that the mineral from Haddam, which you most kindly named for me, was not columbite has been fully corroborated: for it contains no hyponiobous acid (Nb^2O^3), as the columbite does; but niobous acid (NbO^2); and, in addition, hypoilmenic acid (I^2O^5), and also, a small quantity of hypotantalic acid (Ta^2O^5). The chemical formala is therefore quite different from that of the Columbite: i. e., *not* RO, Me^2O^3, but

$$2(2RO, 3NbO^2)+(RO, Me^2O^5)Il^2O^5=(\tfrac{1}{4}Ta^2O^5+\tfrac{3}{4}Il^2O^5).$$

The result of the analysis was:

		Oxygen.		Calculated.
Hypotantalic acid,	7·029	1·301	5·427	5·0
Hypoilmenic acid,	14·917	4·126		
Niobous acid,	56·154	12·290		12·0
Protoxide of iron,	12·560	2·79	4·89	5·0
Protoxide of manganese,	9·340	2·10		
	100·000			

The lower specific gravity of the mineral observed by you as well as the easy solubility in sulphuric acid of the metallic acid present, are readily explained from their small content of tantalic acid, and from the greater proportion of oxygen in the niobous acid as compared with that of the hyponiobous acid in Columbite."

Moscow, Nov. 9, 1875.

The physical characters of the mineral are given in the volume of this Journal above referred to.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Didymium absorption-spectrum and the Atomic weight of Cerium.*—Bührig has examined the methods for the preparation of pure cerium and has made an elaborate determination of its atomic weight. As the absorption spectrum of didymium is so characteristic, the author made a careful study of it, with a view to use it to prove the freedom of the cerium from this metal. The pure sulphate in a tube 22 cm. long (1·0454 grams in 50 c. c. of water) gave 11 bands. Upon dilution, it was found that in this tube $\frac{1}{7174}$ gram of the sulphate in 100 c.c. water—corresponding to $\frac{1}{17942}$ gram Di,—could be detected by its bands. In a tube 52 cm. long, $\frac{1}{21739}$ gram of sulphate, corresponding to $\frac{1}{54545}$ gram of didymium, could be thus detected. Using a Duboscq, in place of the Hoffmann spectroscope, three additional absorption lines were observed, making in all 14. The spectrum given by a crystal of sulphate 0·9 mm. thick, contained 22 lines, and was considerably different from the others. Plates of these spectra are given. The cerium was obtained pure from the mixed oxalates of the cerite earths, by igniting these without the addition of magnesia, by solution in nitric acid, and precipitation of the cerium as ceroso-ceric sulphate. This precipitate, after washing, was obtained free from didymium by Gibbs' method. The atomic weight of cerium was determined from the combustion of the oxalate and was found to be, as a mean of ten closely concordant results, to be 94·1782. The author adds some analytical data concerning the salts of cerium.—*J. pr. Ch.*, II, xii, 209, Nov., 1875.
G. F. B.

2. *On the Density of Platinum, of Iridium and of their Alloys.*—Sainte-Claire Deville and Debray have prepared with great care both platinum and iridium in a state of purity and have determined the density of these metals as well as that of several of their alloys. The methods which they made use of to purify these metals are given at length in their memoir. The platinum ingots weighed from 200 to 250 grams, and gave a density of 21·5. The iridium, after breaking under the rolls, had a density of 22·42104; in the ingot, as melted, of 22·239. An alloy of 90 per cent of platinum and 10 of iridium had a density of 21·615; of platinum 85 and iridium 15, of 21·618; of 66·67 platinum and 33·33 iridium, 21·874; of platinum 5 and iridium 95, 22·384; thus increasing quite regularly.—*C. R.*, lxxxi, 829, Nov., 1875.
G. F. B.

3. *New method of Chlorinating Hydrocarbons.*—In the course of experiments made to discover a solvent for the comparatively unstable molybdenum pentachloride, Aronheim observed the energy with which it transferred its chlorine, even benzol when heated with it evolving torrents of hydrochloric acid gas. Further

examination showed it to be a far more energetic chlorine carrier than iodine, since (1) it acts more readily and quickly, (2) it carries the process more uniformly from one stage to the next, and (3) it can be more readily removed from the products. If to 500 grams anhydrous benzol, 5 grams $MoCl_5$ be added, and a stream of chlorine be passed through it, heat being applied by a water-bath and a return-cooler being used, after three days the liquid solidifies on cooling to an intermixed crystal mass, consisting of nearly pure para-dichlor-benzol, which after purification is equal in weight to the benzol taken. By acting on toluol in this way, the author in conjunction with DIETRICH, has obtained several new chlorine derivatives of this hydrocarbon.—*Ber. Berl. Chem. Ges.*, viii, 1400, Nov., 1875. G. F. B.

4. *On the Effect of Mass on the Chemical action of Water.*—OSTWALD has made, in the laboratory of the Dorpat University, a research upon the action of water in mass upon chemical action. A concentrated solution of bismuth chloride in hydrochloric acid was divided into 25 equal portions. To the first, water was added till a permanent turbidity appeared, and quantities of water gradually increasing from this were added to the other portions, the last receiving enough to precipitate all the bismuth. After standing six weeks, the various liquids were separately analyzed, the chlorine, bismuth, hydrogen and water being determined. These results are given in a table. To compare them a second table is given in which the chlorine and bismuth are calculated for 100 parts of water. If from these figures a curve be constructed with those for bismuth as abscissas and chlorine as ordinates, the form of the curve for two-thirds of its length is a hyperbola. The first third is nearly a straight line, differing from this no more than is allowed by experimental errors. Hence either Berthollet's law is true and the action is exactly proportional to the mass, the curve being due to foreign influences, or the law of the action of mass is a function of a higher order, and Berthollet's law only a special case of it, where the higher powers are neglected. The author inclines to the former view; since he has detected one such disturbing cause in the fact that considerably more of the bismuthyl chloride remains suspended in the diluter than in the more concentrated liquids.—*J. pr. Ch.*, II, xii, 264, Nov., 1875. G. F. B.

5. *Formation of Alizarin by Reduction of Rufigallic Acid.*—WIDMAN has observed that when rufigallic acid is reduced by sodium amalgam, a violet solution is obtained. On precipitating this by hydrochloric acid and dissolving the precipitate in potassa, barium chloride throws down a second precipitate, which when treated with HCl leaves a residue. This dissolved in methyl alcohol or acetic acid, is left on evaporation. Heated to 250°, it sublimes in brilliant orange-red needles, having all the reactions of alizarin. Hence rufigallic acid is hexa-oxyanthraquinone $C_{14}H_2\left\{\begin{matrix}(C)''_2\\(HO)_6\end{matrix}\right.$, and the production of alizarin in the vegetable kingdom, is explained.—*Bull. Soc. Ch.*, II, xxiv, 359, Nov. 1875. G. F. B.

6. *On the Separation of Mixed Liquids.* — DUCLAUX has made a careful study of the conditions under which a homogeneous mixture of two liquids will separate into two entirely distinct layers, and has arrived at some very curious results. He finds, for instance, that a mixture of 15 cubic centimeters of amyl alcohol, 20 cubic centimeters of ordinary alcohol and 32·9 cubic centimeters of water, gives at the temperature of 20° C., a molecularly unstable grouping, so that the least diminution of temperature causes it to separate into two nearly equal layers: He states that under these conditions the composition of the two layers is invariably the same whatever the composition of the initial liquid, the layers varying only in amount. The same fact is also true of three as of two liquids; though in this case the third liquid takes no part in the separation, and remains the same in each of the two layers as in the original liquid. Hence it is always possible to start with a given liquid such that by depression of the temperature, two layers of the same volume are produced. The range of variation of temperature necessary to effect this separation is extremely minute, being much less than a tenth of one degree Centigrade! Moreover, the introduction of mere traces of certain substances, as sodium and calcium chlorides and other soluble salts, and the vapor of chloroform produce the same effect as a lowering of temperature. So also a drop of water or one of amyl alcohol will cause the separation. The author has applied this phenomenon to the construction of an ingenious minimum thermometer. By varying the amount of water present in the above mixture for example, the temperature at which separation ensues may be varied. The solutions may be readily prepared by taking the necessary quantities of amyl and ethyl alcohol, maintaining them at the exact temperature required and adding water drop by drop, until a slight turbidity appears, which should dissolve upon the slightest heating. The mixture is then placed in a tube and this is hermetically sealed. Ordinarily the liquid is clear but it becomes turbid as soon as the temperature falls below that at which it was prepared. A few drops of carmine in ammonia makes the separation more distinct, since the lower layer only is colored. If ten parts of ether be mixed with six of commercial methyl alcohol, and water be carefully added as above, a liquid will be obtained acting as a maximum thermometer, since it becomes turbid and separates when the temperature rises above that at which it was made. This is colored with a little blue ink. Several tubes of each kind would evidently be exceedingly useful in maintaining a given temperature constant for any purpose, since they could be graduated to any interval.— *C. R.*, lxxxi, 815, Nov., 1875. G. F. B.

7. *Stationary Liquid Waves.* Professor GUTHRIE has recently communicated to the London Physical Society the results of his observations on wave motion. If water in a cylindrical vessel, not less than nine inches in diameter, be agitated by depressing and elevating a flat circular-disk on its surface at the center, a form of

oscillation is set up which the author terms binodal. He finds that these fundamental undulations in an infinitely deep circular vessel are isochronous with those of a pendulum whose length is equal to the radius of the vessel, and that the pendulum and water keep together throughout their entire paths. This was shown experimentally by a short pendulum with a heavy adjustable bob, having a card-board sector attached to its upper end. A silk thread attached to the edge of this sector carries a small paraffin disk, which rests at the center of the surface of the water contained in the cylindrical vessel. The length of the pendulum is altered until the motion of the disk is isochronous with that of the water. Two other forms of motion may also be produced by alternately compressing and extending opposite diameters, as in a bell, and by rocking the vessel. Each has its own period, the last being the slowest. They may be superposed and a rotation of the water, however great, does not interfere with their formation.

In rectangular troughs binodal and mononodal waves may be formed, the former by raising and lowering a wooden lath at the middle of the surface, and the latter by tilting the vessel. Experiments of binodal motion show that they are isochronous with a pendulum whose length is 2 divided by π times that of the trough. The principal questions still to be considered are: (1) Why are the motions pendular? (2) How is it that in circular binodal motion the times are identical with that of a pendulum of given length? and (3) What is the mathematical connection between the individual motion of each particle and that of the mass?—*Nature*, xiii, 99. E. C. P.

8. *Waves in Elastic Tubes;* M. MAREY has studied the laws of the circulation of the blood by a mechanical representation in which a liquid wave is made to traverse an elastic tube. The changes in the shape of the tube are measured at six points by small elastic reservoirs connected with a chronograph so that the form of the wave and its time of transit past each point, are represented graphically. The wave is generated by forcing water by a pump into the tube. Positive waves are thus formed which follow the general laws of undulatory motion. The velocity is proportional to the elasticity of the tube and inversely as the density of the liquid. It diminishes gradually as the wave progresses and increases with the rapidity with which the liquid is added. With a sudden addition of liquid, secondary waves are formed of continually diminishing amplitude. When the tube is closed or narrow at the end, reflected waves are formed. If the walls of the tube are not very extensible, harmonic vibrations are formed superposed on the primary waves. In branching tubes a very complicated combination of waves is formed passing from tube to tube. But in the case of the blood the aorta does not permit the waves to pass from one artery to another. Its own waves are transmitted to the arteries where they are gradually lost, but like an elastic reservoir it absorbs and extinguishes the reflected waves.—*Journ. de Phys.*, iv, 25. E. C. P.

9. *Transparency of Flame and of the Air.*—M. E. ALLARD has presented to the French Academy several memoirs on the absorption of the light of lighthouse lamps. The first memoir relates to the transparency of flame. From one to six concentric wicks are used in lighthouse lamps, having a diameter of from 3 to 13 cms. A comparison of the luminous intensity of the flames shows that the brightness increases a little less rapidly than the consumption of the oil; comparing the intensity with the dimensions of the flame, it appears that the brightness per square centimeter increases, but that per cubic centimeter diminishes with the size of the flame. This difference may be accounted for by assuming that the flame is not perfectly transparent. Three methods were adopted for measuring the absorption, by comparing the light of the edge and side of a flat flame, by reflecting the light a second time through the flame by a mirror, and by viewing the electric light through a large flame. The results lead to the coefficient of ·80 for the absorption per centimeter in thickness.

After having established the theoretical formulas which give the effective brightness of the flame as a function of its volume and coefficient of absorption, it appears that to satisfy the observations we must assume that the specific brightness increases a little with the diameter. Multiplying then the specific brightness by the volume, it appears that the total quantity of light increases much more rapidly than the weight of oil burned; but as the quantity of light absorbed increases still more rapidly, the light increases a little less rapidly than the oil consumed, as experiment shows.

The second memoir relates to the nocturnal transparency of the atmosphere. Observations are made three times every night by the lighthouse-keepers, as to which of the adjacent lights are visible. Combining the results for several years gives the percentage of nights on which each light is seen. The equation of the range of visibility and a graphical construction serve to show for each light in all cases what degree of transparency of the air is needed to render the light visible. A curve may then be constructed with the transparency of the air and the visibility of the lights as coördinates. From this it appears that during half the year the coefficient of transparency per kilometer exceeds ·91 in the Atlantic and ·932 in the Mediterranean. Similar curves give the transparency at different points along the coast, and during the four seasons.

The third memoir treats of the apparent brightness of a light caused by revolving the system of lenses employed with greater or less rapidity. With a certain velocity, a flickering effect is produced, but with an increased speed the light becomes steady with an intensity one or two tenths less than would be obtained by distributing the light uniformly around the horizon.—*Comptes Rendus*, lxxxi, 1096. E. C. P.

10. *Etheric Force of Edison.*—Prof. E. J. HOUSTON, in an article in the January number of the Journal of the Franklin Institute,

concludes from his experiments—as many physicists may have concluded from the published account of the supposed new force—"that all the phenomena noticed by Mr. Edison are explainable by the presence of inverse electrical currents of considerable quantity, but comparatively small intensity, instantaneously produced at the making or breaking of the battery circuit."

II. Geology and Mineralogy.

1. *U. S. Geological and Geographical Survey of the Territories*, F. V. Hayden in charge. Department of the Interior. *Bulletin No.* 5, *Second series.* Washington, Jan. 6, 1876.—This new Bulletin contains the following important papers: A review of the fossil flora of North America, by L. Lesquereux; New fossil plants of the Lignitic formations, and from the Dakota group of the Cretaceous, by L. Lesquereux; Notes on the Lignitic group of Eastern Colorado and Wyoming, by F. V. Hayden; Geology of localities near Cañon city, by S. G. Williams; On *Zapus Hudsonius*, and on the breeding habits, nest and eggs of *Lagopus leucurus* (the white-tailed Ptarmigan), by Dr. Elliott Coues, U. S. A.; List of Hemiptera of the region west of the Mississippi, including those collected during the explorations of 1873, by P. R. Uhler; On the supposed ancient outlet of Great Salt Lake, by A. S. Packard, Jr.

The question as to the age of the Lignitic beds is here discussed anew by Prof. Lesquereux with the presentation of some additional facts. His conclusions remain unchanged. They are as follows.—Above the Lower Cretaceous beds or those of the Dakota group, in the Rocky Mountain region, the first fossil plants met with are the species of the Lignitic formation. This formation is divided into (1) the *Lower Lignitic*, marked by the presence of a profusion of Palms, especially species of *Sabal* (showing a warm, moist climate, like that of Florida, while the Cretaceous plants of the Dakota group indicate one like the present of Ohio) along with species of *Ficus*, *Cinnamomum*, *Magnolia*, *Myrica*, *Quercus*, *Platanus*, *Diospyros*, *Mamnus*, *Viburnum*, etc. (and as yet no *Acer*), and referable to the *Eocene;* (2) the Evanston group, "considered *Upper Eocene* or *Lower Miocene*;" (3) the Carbon Group (more to the eastward, about long. 106½° W.) "or *Middle Miocene*," above which comes (4) the Green River Group, or *Upper Miocene*. The flora of No. 2 includes thus far 90 species, of which a third are known from No. 1: fruits have been found that have been referred to the Palms, but no leaves; there are also in it dentate and serrate leaves of *Salix*, *Betula*, *Alnus* and *Acer*. The flora of the Carbon Group is "positively Miocene;" 18 species, or nearly a third of all, are identical with European Miocene plants, and 13 with Arctic Miocene, while a few occur also in the Lower Lignitic (No. 1.)

Among 23 species from the Point of Rocks, referred to No. 1, or the Lower Lignitic, two occur also in beds to the north of the

United States boundary, called Tertiary by G. M. Dawson, seven are identical with, and five related to, species of the Lower Miocene of Europe, two occur in the Arctic Miocene, three are found also at Golden, eight at Black Butte, and two have some analogy with Cretaceous types.

Hayden, in his remarks on the Lignitic beds, observes that there are lignitic or coal beds in both the Cretaceous and Tertiary formations of the Rocky Mountain region; but that, so far as Eastern Colorado is concerned, from Raton Hills to Cheyenne, the lignitic beds are not associated with marine deposits, but those of brackish water or freshwater origin, and that these are not Cretaceous, but of Eocene age, the evidence from the plants pointing, according to Lesquereux, to this conclusion. He further states that in Southern and Southwestern Colorado, as shown by Mr. Holmes and Dr. Endlich of the expedition, and also other authorities, heavy beds of coal occur all through the Cretaceous. Hence, taking, he says, the whole Rocky Mountain region into view, there is a *Lower Lignitic* group which is *marine* and *Cretaceous;* above this, the *Middle Lignitic*, *brackish water* in origin, which is *Lower Tertiary* or *transitional;* and next the *Upper Lignitic*, *freshwater* in origin, which is unquestionably *Tertiary.* The coal deposits of Carbon are included in the third of these divisions, and those of Bear River and Coalville in the first. Dr. Hayden observes that Dinosaurian remains occur even in the freshwater or upper division, as noticed by Cope and Marsh; but that the species are not identical with any known Cretaceous species. The Green River beds overlie the Lignitic beds unconformably.

The difference between Prof. Lesquereux's view and those of Dr. Hayden appears to be this: Lesquereux makes the Eocene to include the Bear River and Coalville beds, and all the older Lignetic beds the fossil plants of which he has examined (including those even of Vancouver Island, where Ammonites and Baculites occur in beds *overlying* the coal); while Dr. Hayden admits that there is a series of Cretaceous coal beds, that the Bear River and Coalville deposits are included in it, and that these Cretaceous strata are distinguished by being mainly marine and containing Cretaceous fossils.

Between the views of Prof. Lesquereux and those of the zoological paleontologists the divergence is great. For while he makes the Green River beds (containing remains of fossil plants and fishes) "Upper Miocene," and the Carbon beds "Middle Miocene," Leidy, Cope, and Marsh hold that even higher strata, namely, those overlying the Green River beds conformably (having an estimated thickness of five or six thousand feet) and which contain the oldest Mammalian remains of that part of the continent, are *Eocene;* and that the underlying Green River beds are *Lower Eocene;* and further that all the Liginitic beds, that are older than the Green River beds, are *Cretaceous*, since they contain Dinosaurian remains, and some of them other Cretaceous fossils.

Thus widely the best authorities differ; partly because European tests of geological age are not always good for use in America, and partly, also, from deficient American testimony. We are disposed, with the present light, to argue the case as follows:

First. It is highly improbable that the type of Dinosaurs should have been represented all through the Eocene and into the Miocene—as must be true if Lesquereux's conclusions are right.

Secondly. Mammalian fossils are a far safer criterion of geological age than fossil plants—since the changes in the species of mammals through the successive eras of the Tertiary are vastly greater than in those of plants; and as the mammals of the beds next above the Green River beds are strongly Eocene in their characteristics—as attested to by Leidy, Marsh and Cope—it is exceedingly improbable that the beds affording the fossil mammals should be Upper Miocene, or Miocene at all.

Thirdly. If the beds containing these mammalian remains, together with the *underlying* Green River beds, are *Eocene*, then the Evanston beds, and the Carbon beds also if older than the Green River, are either earlier Eocene or Cretaceous. It follows also, *fourthly*, that the "Miocene" features of the plants of the Liginitic beds are not due to the plants being of Miocene age; and hence, *fifthly*, that the diversities in the cotemporaneous Tertiary flora of Europe and North America are so great that little use can be made of the facts from one continent for fixing the chronology of beds in the other.

It is probable, that part of the diversity in vegetation of different localities was owing to local physical conditions, and to migrations consequent on changes of climate with the progress of time during the Lignitic era; and that much of the diversity between America and Europe was due, as suggested by Lesquereux, to many of the Miocene plants of Europe having previously existed as Eocene or Cretaceous plants in America.

If the fossil plants are an uncertain test of geological age, so may it be also, to some extent, with the fossils animals, when characteristic species are sparingly present. Even the existence of Dinosaurian remains in the later Lignitic, and of Inocerami where Ammonites and Baculites are absent, may not prove absolutely that the beds containing them are Cretaceous rather than Lower Eocene, since some animals species may have survived the changes separating the two eras, as has happened in the case of other successive eras.

In the paper, *on the former outlet of Great Salt Lake*, Dr. Packard points out that General Connor has found, by his railroad surveys, that the lowest part of the including rim of hills is at Skull Valley, west of the lake, and that the height there is "somewhat over 100 feet above the present level of the lake." He adds that the river-bed has been traced southward over 100 miles to the Sevier Lake Valley, passing west of Sevier Lake. It is probable, further, that the river joined the Colorado near the confluence of the Muddy River and Rio Virgen; but it may have had an independent outlet into the Dry Lake Basin north of east of San Diego, a region seventy feet below the present sea-level. Dr. Packard hence concludes that the lake was once fresh, and that it has become salt by evaporation and contributions from salt springs and the soil. J. D. D.

2. *Drift formation and Gold in Missouri;* by G. C. BROADHEAD.—The drift of Missouri is confined to the part of the State north of the Missouri. The upper beds are chiefly sand with some small pebbles and a little clay; lower down are large bowlders, and at base are blue clays. In Sullivan Co., and the western part of Adair the depth is 50 to 60 feet; in Davies Co., 40 to 80 feet; in northeastern Adair, over 100 feet; in Knox, 200 feet; in Putnam, over 70. In Illinois, in Moultrie Co., the depth is over 200 feet, as shown by wells, and in Decatur Co., over 90 feet. Gold has been found in Missouri, in Chariton, Linn, Adair, Putnam, Sullivan and Mercer Counties. It is in very small grains, the largest particles, from Adair Co., are not larger than a grain of wheat.—*Mines, Metals and Arts, St. Louis, Dec.* 9.

3. *Glacial striæ north of Lake Ontario, in the Ontario district, Western Canada.*—Prof. CHAPMAN, in a paper on the Geology of Ontario (Canadian Journ., xiv, 580, Dec., 1875) states that the limestone strata beneath the glacial and post-glacial deposits (which cover a large part of the Lake Ontario district) are found to be generally striated, and that the striæ run commonly in a *southwest* direction. The direction proves that the slope of the upper surface of the glacier in that region was from the northeast to the southwest; or that the greatest height of the ice surface lay somewhere to the northeast of that district.

4. *New Fucoid from the Water-lime Group (Lower Helderberg) of Western New York.*—Messrs. A. R. GROTE and W. H. PITT have described a species of *Buthotrephis*, from the Water-lime, which they call *B. Lesquereuxi.* The stem, originally cylindrical, branches from the base; and the branches are simple or sparingly dichotomous, smooth, 13 to 14 cm. long, 3 to 4 mm. thick, but gradually widening to nearly 1 cm. at the obtuse or round-truncate point.—*Bull. Buffalo Soc. Nat. Sci.*, 1876 (January), p. 88.

5. *Petrifaction.*—CHEVREUL, in a paper before the French Academy, sustains the view that the petrifaction of an organic substance, as wood, comprises two epochs: the *first* is that of the filling of all the interstices and pores of the solid body by a solution of the mineral material through capillary action, to fix it chemically by affinity upon the solid portion—producing a petrifaction which has the figure of the interstices and pores; and the *second* includes the time of the total disappearance of the organic matter itself and its replacement by the mineral material, the result of this action having the actual form of the organic matter.

Daubrée, after the reading of M. Chevreul's paper, mentioned facts from the hot baths of Bourbonne-les-Bains confirming his conclusions. He stated that wood occurs in the waters in all states of change by the petrifying agent, carbonate of lime. In one specimen a portion was nearly 97 per cent carbonate of lime, all the organic tissues but 3·1 p. c. having disappeared; while in another portion, less changed, only the interstices and cellules were filled by the carbonate of lime. In one specimen the wood nearest the bark and the bark contained no carbonate of lime, as was easily proved by an acid.

6. *Green Mountains.*—On page 498 of the last volume of this Journal (Supplementary December number), a note is inserted correcting the blunders which have long circulated in Geographies, Gazetteers, Encyclopedias, and New England Guide-books, as to the Green and White Mountains terminating in trap ridges—called West and East Rocks—in the vicinity of New Haven; the fact being that East Rock is but a short appendage (half a mile long) to the system of trap dikes of the Connecticut valley, and West Rock, a southern portion of the same system. Prof. O. P. Hubbard has informed the writer that this extraordinary error in New England Geography has the following forms in "The Imperial Gazetteer" published by Blackie & Son at Glasgow, Edinburgh and London, in 1855. Under NEW HAVEN, "Surrounded on three sides by spurs of the Green Mountains." Under GREEN MOUNTAINS, "A mountain range commencing near New Haven, Connecticut." Under CONNECTICUT, "Some of its mountains, particularly the Green Mountain range," etc.

The Green Mountains consist of metamorphic rocks and are not younger than Silurian. They have their greatest height in Vermont, and there received the name. The mountain system extends south through *western* Massachusetts and *western* Connecticut, and the whole is rightly called the Green Mountain chain. But the trap ridges of the Connecticut valley, belong to the valley, and are of Jurassic origin. J. D. D.

7. *Geology of New Caledonia.*—The formations of New Caledonia, below the Quarternary, according to M. Garnier, include the Lower Neocomian (Lower Cretaceous); Upper Lias (containing *Nucula Hammeri*); Lower Lias (containing *Ostrea sublamellosa*, &c.); Upper Trias (containing *Halobia Lomelli*); Lower Trias (with *Avicula Richmondiana*); Upper Devonian and Upper Silurian; besides also crystallized rocks. Among the last mentioned are mica schist and argillaceous slate with quartz veins, some of them auriferous, amphibolite, talcose slate and serpentine, with crystalline limestone. At Koé and Karigou the coal is anthracitic, partly graphitic; and at the latter place it is said to have been rendered anthracitic by a dike of euryte porphyry. To the northwest of Mont d'Or it is bituminous, but impure.

The serpentine or magnesian rocks cover a large part of the island. The serpentine contains bronzite or diallage, chromic iron, magnetite, "hydrosilicate of magnesia," and is traversed by "veins" of chrysolite. It passes into white argillaceous [probably hydro-mica] schists, and these are intimately associated with the serpentinous schists [facts which prove that the serpentine is not, as Garnier states, igneous, but, like most serpentine rocks, metamorphic]. Chromic iron is abundant on Mont d'Or; an analysis of it afforded

$\ddot{\bar{F}}e$ 34·00, $\ddot{\bar{C}}r$ 61·33, $\ddot{\bar{A}}l$ 0·11, $\dot{M}g$ 0·01, $\ddot{S}i$ 4·63=100·08.

Ores of nickel occur in the serpentine, and are of workable value. The only ore mentioned is a greenish pimelite-like silicate, a variety of which has been named *garnierite*.

M. Garnier, who is in charge of the New Caledonia mines under the French Government, has published on the Geology of New Caledonia in the *Bulletin of the Geological Society of France*, II, xxiv, 438, 1866, and in the *Annales des Mines*, VI, xii, 1867; and later communications have appeared in the *Moniteur de la Nouvelle Calédonie.—Abstract of part of Address of Rev. W. B. Clarke before the Royal Society of New South Wales, at the Anniversary meeting in May*, 1875.

8. *Achrematite, a new mineral;* by Prof. J. W. MALLET.—This mineral is in general compact, with indistinct crystalline structure; an examination in polarized light suggested that it might belong to either the hexagonal or tetragonal systems. Color, a sort of liver-brown, though under the microscope the pure grains appear pale sulphur-yellow. Streak, pale cinnamon-brown. Luster, between resinous and adamantine. Translucent on thin edges, in minute grains nearly transparent. G.= 5·965 on a solid fragment, but = 6·178 with a fine powder. H.= 3–4. Fracture uneven; brittle. A mean of three analyses gave, after deducting impurities,

$\frac{3}{3}$) As^2O^5, 18·25, MoO^3, 5·01, Cl 2·15, Pb 6·28, PbO 68·31=100·00,

which makes *achrematite* a molybdo-arsenate of lead. Several reasons are given for the conclusion reached that the arsenate and molybdate of lead are in chemical combination, and not mechanically mixed. The name is derived from *ἀχρήματος*, in allusion to the fact that it contains no silver as was alleged. Locality, the mine of Guanaceré, State of Chihuahua, Mexico.—*J. Chem. Soc.*, II, xiii, 1141, Nov., 1875. E. S. D.

9. *Schraufite*, a new fossil resin from Bukowina described by v. Schröckeringer. It occurs in rounded masses imbedded in a bed of slatey sandstone. Its hardness is 2–2·8; specific gravity 1–1·2; fracture conchoidal; color hyacinth-red. It is decomposed with the evolution of gas at a temperature of 326° C. Its composition, according to Dietrich is $C^{11}H^{16}O^2$. It is named after Professor Schrauf of Vienna.—*Verh. G. Reichs.*, May, 1875, p. 134. E. S. D.

10. *Identity of Seebachite with Phacolite;* v. RATH.—The zeolite from Richmond, Victoria, described by Ulrich and later made identical with herschelite by von Lang, was made a new species by Bauer, under the name of seebachite (Dana's Min., Appendix II, p. 50). A recent examination of the mineral, upon some good crystals, by vom Rath, has proved that the mineral called seebachite is not orthorhombic, as claimed by von Lang, but rhombohedral, and that it is really indentical with phacolite, a variety of chabazite.—*Ber. Ak., Berlin*, 1875, 523. E. S. D.

11. *A new species of Dalmania from Port Jervis, New York.*—Dr. S. T. Barrett, of Port Jervis, has recently described a new species of Dalmania from the Lower Helderberg of that vicinity, and named it *D. dentata.* A description by him, accompanied with a plate, will appear in the next number of this Journal.

J. D. D.

III. Botany and Zoology.

1. *Naudin on the Nature of Heredity and Variability in Plants.*—Why is it the nature and essence of species to breed true, and why do species sometimes vary? In other words, why is offspring like parent, and when unlike in certain particulars, what is the cause and origin of the difference? We commonly and properly enough take these two associated yet opposed facts as first principles. But it is equally proper and legitimate to enquire after the cause of them.

M. Naudin, a good many years ago, took up the study of hybrid plants, and followed up, for a series of generations, the course of life of certain self-fertile ones, notably of *Datura.* We gave at the time an abstract of his observations of the manner in which the characters of two closely related common species, *D. Stramonium* and *D. Tatula*, were mixed, and in which the characters of the two began to separate in the close-bred progeny of the next generation, ending in a complete division of the amalgamated forms into those of the two constituent species after a few generations.

The *Comptes Rendus* of Sept. 27th and Oct. 4th, 1875, contain an abstract of a paper communicated by M. Naudin to the French Académie des Sciences, of which the text was suggested by a hybrid between the wild *Lactuca virosa* and a variety of *L. sativa*, the common Lettuce. The hybrid was an accidental one: its seeds were fully fertile; a great number of young plants were raised from them, of which twenty were preserved for full development and study. Like other hybrids the original showed no character which was not evidently derived from the two parents; and, fertilized by its own pollen, the offspring all agreed in this respect, although they varied exceedingly among themselves in the division of the parental heritage, no two being quite alike. This exceeding vacillation between the two parental forms but not overpassing the limits on either hand,—which Naudin finds to be the common characteristic of fertile hybrids, close-bred — he names disordered variation (*variation désordonnée*). His explanation is, that the hybrid is a piece of living mosaic, that two specific natures are at strife in it; in the progeny each endeavors to reclaim its own, like seeks like; whence in the course of a very few generations (as he first showed in *Datura*), a segregation takes place, part of the progeny reverting completely to one ancestral type, part to the other. What Naudin now insists upon is that out of all this disturbance comes nothing new; that there is here no variation beyond the line of inheritance; and therefore from crossing no possible development of species.

To this proposition we accede, so far as respects the direct consequence of crossing. To fill up the interval more or less between two forms or species with intermediate patterns may tend to the fusion or confusion of the two, but not to the origination of new forms or species. Although Naudin's own experiments lead him to deny all tendency to variation overpassing these limits, we do not forget that his countryman, the late M. Vilmorin,—working in a different way and with another object,—arrived at a different conclusion. He succeeded, as we understand, in originating floricultural novelties from species which refused to vary *per se*, by making a cross,—not to infuse the character of the male parent, for he fertilized the progeny with the pollen of the female parent, and thus early bred out the other blood, but to induce variation, which, once initiated in the internal disorder consequent upon the crossing, was apt to proceed, or might be led on by selection, to great lengths, according to Vilmorin. The variations in question, being mainly such as are sought in floriculture, may not have passed the line laid down by Naudin, or actually have introduced new features. But such plants would surely have no exemption from the ordinary liability to variation. If other plants vary, in the sense of producing something new, so may these.

This brings us to another inference which Naudin draws. Having observed that his hybrids in their manifold variation exhibited nothing which was not derivable from their immediate ancestry, he directly (and in our opinion too confidently) concludes that all variation is atavism,—that when real variations are set up in ordinary species, this is not an origination but a reversion, a breaking out of some old ancestral character, a particular and long deferred instance of this *variation désordonnée*, which would thus appear to be the only kind of variation. This view has been presented before, but not, perhaps, so broadly. Adducing some theoretical considerations in its favor—to which we may revert—and some sound reasons against the view that variation is caused by external influences, he declares it "infinitely more probable that variation of species properly so called is due to ancestral influences rather than to accidental actions." We might think so if these two categories were exhaustive, and external conditions must be supposed to act immediately, as the cause rather than the occasion of variation. But the supposition that "accidental actions," whatever they may be, and external influences of every sort do not produce but educe and conduct variation—which is our idea of what natural selection means—avoids the force of Naudin's arguments.

Moreover Naudin's view, regarded as an hypothesis for explaining variation, leaves the problem just where it finds it. To explain the occurrence of present and actual variations, hypothetical ones like those of a former time are assumed; the present diversity implies not only equal but the very same anterior diversity, and so on backwards. Or rather it demands a much greater diversity at the outset than now; for these aberrant forms are the rare exception, and if due to atavism they imply the loss of the many and the incidental reappearance of the few. Else they would be the rule instead of the exception, and atavism would be simply heredity. This comes to the view which Mr. Agassiz strongly maintained, that really there are no varieties,—meaning, we understand, that all the forms are aboriginal, except the transient ones evidently due to circumstances.

That some variation is atavism is clear enough. This is the natural explanation of the appearance of characters wanting in the immediate parents but known in their ancestors or presumed ancestors. But the assumption of hypothetical ancestors to account for variation generally is quite another thing. Besides its inutility as an explanation, to which we have adverted, its improbability as an hypothesis is set in a strong light by Naudin's own forcible conception of the nature of heredity. What is heredity? he asks. In other words, what keeps species so true, offspring like parent, through the long line of generations? He illustrates hereditary force by comparing its action with that of physical force, in which the movement from one state of equilibrium to another is always that in which there is least resistance. From which it follows that when it has once begun to proceed in a certain course, its tendency to continue in that direction increases, because it facilitates its way as it overcomes obstacles. In other words this line becomes fixed by habit; *vires acquirit eundo;* the stream deepens its bed by flowing; and the more remote the commencement of a certain course, the more fixed its direction, and the greater its power of overcoming opposition. The species is kept true in its course by the sum of the heredities which press each individual forward in its actual direction. So that, as Naudin remarks, if we could calculate the energy with which millions of ancestors tend to impel the living representatives of the line onward in the same direction, we should better apprehend the persistence of species, and feel the great improbability that the stream will ever escape from its ancient and well-worn bed, and strike into new courses.

Now, in the first place, the more lively the conception we thus form of the invariability of species, through a happy metaphorical illustration of it, the more unlikely does it appear that early characters, long lost in the flow, should re-appear through atavism as varieties. To continue the simile, the more impetuous the stream, the less the possibility of its turning back upon itself, and resuming old characteristics. The eddies of atavism (the resumption of dropped characters) are not likely to extend back very far; and it seems gratuitous to have recourse to them in explanation of new forms. Moreover, although the stream has made its bed and lies in it, not escaping from its own valley, it is flexible enough to obstacles, is ever changing its particular course as it flows, and may by its own action send off here and there a bayou (variety) or branch into a delta of channels (derivative species).

Like Agassiz, Naudin conceives of species as originating with a large number of individuals of the same structure, and of which numerous reciprocal crosses have determined the direction of the line in which their posterity have evolved. But he mantains that these individuals, and all existing species, had a common origin in a "proto-organism;" and that the various lines of descent acquired fixity into species only as they acquired sexuality. If we rightly apprehend it, Naudin's idea of the purport of sexual reproduction (as contrasted with that by buds) is, to give fixity to species. Our idea is a different one, both as to the essential meaning of sexuality, and as to its operation in respect to fixity. His conception may be tested by enquiring which are the more variable or sportive, seedlings or plants propagated from buds. This we suppose can be answered in only one way.

M. Naudin is a veteran and excellent investigator; and nothing which he writes is to be slighted. We have frankly set down our impressions upon a first perusal of his important communication; but are ready to revise them, if need be, upon more deliberate consideration. A. G.

2. *First Forms in Vegetation;* by the Rev. HUGH MACMILLAN, LL.D., F.R.S.E. With numerous illustrations. Second edition, corrected and enlarged. London: Macmillan & Co. 1874. pp. 438, 18mo.—The first edition under a somewhat different title, was published in 1861. In the present volume it is brought up to the time, and we suppose much amplified. As it stands it forms an excellent popular introduction, of the readable sort, to lower Cryptogamic botany, from Mosses (and even Club-mosses) to Fungi. The materials of its chapters were first used for popular lectures, and this primary form and use gives its character to this re-written and now extended volume. The author wished to recast it in a systematic mold, but was deterred not only by the labor required, but by the doubt whether it were worth the while. We fancy it is better as it stands, and more likely to fulfill its purpose, which is "to kindle the sympathy and awaken the interest of the reader in a department of nature with which few, owing to the technical phraseology of botanical works, are familiar." The book is full of information, possibly too full for the object in view, except that it cannot be amiss to gratify as well as awaken interest in the lower forms of vegetation by referring to as large a number as is practicable. The spirit in which the subject is handled is indicated by the motto: "*Deus magnus in magnis, maximus in minimis;*" and the sermonizings, being apposite, we have no right to intimate that they are too many and too long. There are good indexes of scientific and of popular names. It is not often that an amateur-botanist writes a book of this kind which is more free from serious errors or misunderstandings. But the statement that the antherozoids of mosses, although "furnished with cilia, like animalcules," yet "their motion is simply a hygrometical action, like that of the teeth which fringe the mouth of the capsule," must be one which unaccountably escaped revision, even in the edition of 1861. So also the suggestion that sexual reproduction may be gradually dispensed with in the lower plants and

animals, which is in fact corrected in the latter part of the volume. In referring to Prof. Tuckerman, we hope it may be long before he must be designated as "the *late* distinguished American lichenologist." The "conjecture" which "*may* be hazarded" that the Red Sea "acquired its denomination from the prevalence of this red alga [*Trichodesmium erythræum*] in its waters," *was* hazarded by Ehrenberg, if we mistake not, at the time of its discovery. The late Dr. M. A. Curtis, our American mycologist, informed Mr. Berkeley, as we remember, that he and his neighbors whom he instructed, procured no small supply of excellent food from the edible Fungi, which grew around his home, in the center of North Carolina, so abundantly that he opined he might have supported a regiment on them. This, we suppose, is the whole foundation for the extraordinary statement that, "During the latter part of the American war, when meat was scarce and dear, fungi, which grow in immense profusion and variety in America, formed the principal food of the Southern army." A. G.

3. *Seeds that float in water* have a certain interest in connection with questions about dissemination. Many ranked as such are fruits, botanically speaking, with spongy or cellular pericarp, or with some air-space between the pericarp and the seed itself. But there are a considerable number of true seeds with specific gravity less than that of water, some as low as 0·75. Van Tieghem (Ann. Sci. Nat., ser. 6, i, 383) finds that this is due to different causes. More commonly the seed owes its lightness to its coats, either by a separation in drying between the two, or between the inner and the kernel, leaving an air-space, or by a loose cellular structure of the coat. Sometimes, as in castor-oil seeds, the integument is heavier than water, but the kernel is so much lighter as to float the seed. This comes from a separation of the two cotyledons during the natural desiccation, leaving a considerable cavity filled with air. This is strikingly the case in the large, flat, and very dense-coated seeds of *Entada scandens*, which are well known to have been wafted across the Atlantic from the West Indies to Northern Europe and left in a condition fit for germination; while in those of *Guilandina Bonduc*, which have been known to accompany the former, the air is interposed between the embryo and the bony coat. The embryo itself, in all such cases, is heavier than water. But to this rule Van Tieghem now brings to light a few exceptions, and these in leguminous seeds. In those of *Erythrina crista-galli*, the specific gravity of the whole seed is 0·91, that of the embryo itself 0·87. Those of our *Apios tuberosa* are a little lighter, of our *Wistaria frutescens* a little heavier than this, but still lighter than water; and various common leguminous seeds, although heavier than water, are much lighter than would be supposed. This proves to be owing to a very loose and open structure of the *parenchyma* of the inner or upper side of the cotyledons (that which answers to the upper face of the leaf), leaving abundant intercellular spaces and passages, filled with air, which renders this spongy stratum light enough in certain cases to float the otherwise heavy seeds. A. G.

4. *Use of the hygrometic twisting of the tail to the carpels of Erodium.*—We have no indigenous or common *Erodium* this side of Texas; but there and in California one or two species are common. The narrow carpel is pointed at base; the long awn or style in drying bends at right angles with the carpel, and twists in many turns, depending on the amount of dryness, and untwists in a moister air or when wet. We had wondered that no one seemed to have given an account of the way in which this mechanism acts so as to bury the seed in the ground. Dispersed by the wind over the loose or sandy soil which these species prefer, the seed-bearing end being the heavier lies next to the ground, and is the comparatively fixed point, around which the long awn makes circular sweeps, whether in twisting or untwisting. This gives a rotary movement to the carpel, fixes the sharp end in the soil, and, whether twisting or untwisting, causes it to bore into and bury itself in the ground. It is the same with the grain and awn of Stipa. As to *Erodium*, we have just found that this is described by G. Roux, in the Annals of the Botanical Society of Lyons, France. He adds that, when in this way thus interred, the moisture of the soil soon destroys the epidermis and this allows the long beak to detach itself at its articulation with the style, leaving it planted in good condition quietly to germinate. M. Roux enters into details about the effect of light, heat, chloroform, etc., upon this movement, which seem to us superfluous and wide of the mark. A. G.

5. *The Lemurs not related to the Monkeys.*—In the genealogical tables of Hæckel, the Lemurs are made the point of divergence of lines leading to the Insectivores and Carnivores on one side and to the Rodents and the Monkeys on the other. MM. Grandidier and Alph. Milne Edwards, in their recent work on the Mammals of Madagascar, show that the Lemurs have striking peculiarities in the conformation of the allantois and placenta, and not the close relation to the monkeys generally supposed. By injecting the capillary vessels of the placenta and uterus they have studied the vascular relations of the fetus with the mother, and established thus profound differences between the two types, which begin even in their intro-uterine life.—*L'Institut*, Dec. 29.

6. *Fauna of the Greenland Seas.*—The Fauna of the Greenland Seas, according to results obtained by the "Valorous" (on its return from Disco), agrees with its land flora in being mainly Norwegian, there being (with the exception of the Echinoderms) an absence of many North American forms, which, as it appears, have not been found east of the meridian of Cape Chidley in Labrador. A *Campanularia* was obtained identical with one found by Mr. Eaton, of the British Transit-of-Venus Expedition, at Kerguelen's Island; also, in the towing-net, a sponge-like diatom, *Synedra Jeffreysi* Dickie, with living Globigerinæ entangled in the connecting protoplasmic matter of its frustules. The deep waters of Davis Straits afforded a mollusk which was long since found fossil in the newer Tertiary of Sicily, and was supposed to be extinct.—*Proc. Roy. Soc.*, No. 164, p. 78.

IV. Astronomy.

1. *New Planet.*—The discovery of another planet by Herr Knorre was announced by telegram to Prof. Henry, Jan. 11th.

2. *Harvard Observatory Engravings.*—The last instalments of the engravings from Harvard College Observatory have been distributed to the subscribers. Instead of 30 plates as promised, 35 have been issued. These later plates represent two star clusters, six views of nebulæ, four views of Donati's comet, and three of Coggia's comet. A letter press description of the engravings is, we understand, to be soon issued.

3. *The Uranian and Neptunian Systems investigated with the 26-inch Equatorial of the U. S. N. Observatory;* by Professor Newcomb. Appendix I of the Wash. Obs. for 1873, p. 74.—This paper is the first extended contribution of results obtained by the large Washington telescope. It is a discussion of the observations made between November, 1873, and May, 1875, upon the four satellites of Uranus, and the satellite of Neptune. It closes with tables of the motions of the satellites, for a portion of which credit is given to Prof. Holden.

Prof. Newcomb obtains $\frac{1}{22600}$ as the most probable value of the mass of Uranus, with an estimated probable error of the denominator of 100. He finds no evidence of any mutual inclination of the orbits of the four satellites and but slight evidence of any real eccentricity. The following are the mean distances from Uranus at the mean distance of Uranus from the sun: Ariel, $13''\cdot78$; Umbriel, $19''\cdot20$; Titania, $31''\cdot48$; and Oberon, $42''\cdot10$. The periods of revolution are $2^d\cdot520378$; $4^d\cdot144537$; $8^d\cdot705897$, and $13^d\cdot463269$. The former two are not changed from the determinations of Lassell and Marth. The inclination of the plane to the ecliptic is $97°\cdot85 - 0\cdot013T$, counting from the epoch 1850.

The only means of estimating the masses of the satellites is a comparison of their light with that of the planet. From this Prof. Newcomb infers that they probably do not exceed $\frac{1}{15000}$ of the planet. If so their mutual action, and the sun's action on them, are of no importance. Prof. Newcomb adds: "I think I may say, with considerable certainty, that there is no satellite within 2′ of the planet, and outside of Oberon, having one-third the brilliancy of the latter, and therefore that none of Sir William Herschel's supposed outer satellites can have any real existence. The distances of the four known satellites increase in so regular a way that it can hardly be supposed that any others exist between them. Of what may be inside of Ariel, it is impossible to speak with certainty, since, in the state of atmosphere which prevails during our winter, all the satellites would disappear at 10″ distance from the planet. The planet always presented itself of a sea-green color. No variations of tint were ever seen. Markings on the planet were not especially looked for, but had any been visible they could hardly have escaped notice."

For the mass of Neptune the value $\frac{1}{19380}$ is obtained. In the perturbations of Uranus, Prof. Newcomb used $\frac{1}{19700}$. The distance of the satellite from Neptune is 16 275, its daily motion 61°·25679, its inclination to the ecliptic 145°12; and the orbit so far as observations show is circular.

No trace of a second satellite of Neptune has ever been seen, though it was several times carefully looked for under the finest atmospheric conditions.

V. Miscellaneous Scientific Intelligence.

1. *Report on the Compressive Strength, Specific Gravity, and Ratio of Absorption of Building Stones in the United States;* by Q. A. Gillmore. 38 pp. 8vo, with two plates. 1876. Official Report to the Chief of Engineers, U. S. Army. (D. Van Nostrand.)—This report contains the results of a very careful series of experiments on various building stones of the country. The method of experimenting in the crushing is particularly described, and the results as to crushing-strength with the cubes of stone in different positions, and between wood, lead and leather cushions, etc., are given in detail. The tables contain entries of 99 experiments on granites, 43 on limestones, 12 on marbles, and 62 on sandstones.

2. *Science and Art Department of the Committee of Council on Education, South Kensington.*—The Loan Exhibition of Scientific Apparatus will open on the 1st of April, 1876, and remain open until the end of September. It will consist of instruments and apparatus employed for research and other scientific purposes for teaching, and for illustration of the progress of science and its applications to the arts. Models, drawings, and photographs will be admissible where originals cannot be sent. Forms on which to enter descriptions of objects offered for exhibition may be obtained on application to the Director of the South Kensington Museum, London, S.W., and these forms should be filled up and returned as soon as possible, so that exhibitors may receive early intimation as to the admissibility of the objects they propose to send. The Science and Art Department defrays the cost of carriage, but, while using all possible care, is not responsible for loss or damage. The circular issued expresses the hope that institutions or individuals having instruments of historic interest will be good enough to lend them. The instruments and apparatus desired are of all important kinds connected with the subjects of arithmetic, geometry, measurement, kinematics, statics, dynamics, molecular physics, sound, light, heat, magnetism, electricity, astronomy, applied mechanics, chemistry, meteorology, geography, geology and mining, mineralogy, crystallography, biology, (microscopes, &c.)

3. *Works on the Paleontology of the Rocky Mountain Surveys in progress.*—The first four months of this year will witness the publication of an unusually large number of works on the invertebrate paleontology of the great Rocky Mountain and adjacent regions, some of which have been delayed several years. The following are either partly or wholly in type and will soon be published.

(1.) Paleontology of the Upper Missouri, by F. B. Meek; a quarto volume of between 500 and 600 pages of text and 45 lithographic plates of illustrations. It is confined to fossils of the Cretaceous and Tertiary periods, and is a very exhaustive treatise.

(2.) Paleontology of Clarence King's Geological survey of the 40th parallel, quarto, by F. B. Meek. This Report comprises about 150 pages of text and 17 lithographic plates. It embraces fossils of Lower Silurian, Devonian and Carboniferous ages and of the Triassic, Jurassic, Cretaceous and Tertiary periods.

(3.) Paleontology of the Report of Capt. Simpson's expedition; quarto, by F. B. Meek. Comprises about 100 pages of text and 5 lithograph plates. Cretaceous and Tertiary periods.

(4.) Paleontology of the Report of Capt. McComb's expedition; quarto. Cretaceous fossils, by F. B. Meek and Carboniferous fossils by J. S. Newberry.

(5.) Paleontology of parts of Vancouver's Island and Washington Territory, by F. B. Meek. About 100 pages octavo, and 6 plates.

(6.) Invertebrate Paleontology of Lieut. Wheeler's Explorations and Surveys west of the 100th meridian; quarto, by C. A. White. About 220 pages of text and 21 lithographic plates. This report embraces fossils of the Primordial, Canadian, Trenton, Subcarboniferous, Carboniferous, Jurassic, Cretaceous and Tertiary periods.

(7.) Preliminary Report on the Invertebrate Paleontology of the Plateau Province, by C. A. White, quarto; about 50 pages. It will embrace fossils of the Carboniferous, Jurassic, Cretaceous and Tertiary periods. Among other important facts it will contain an announcement of the existence of open-sea marine deposits at Bijou Basin, forty miles east of Denver, Colorado; the fossils of the deposit belonging to the genera *Venus*, *Mesodesma*, *Dentalium*, *Phorus* and an *Oculina* undistinguishable from the species common in the Vicksburg Tertiary beds. This is to form a part of a report nearly ready for publication by Professor J. W. Powell, Chief of the Second Division of the Geological Surveys of the Interior Department. C. A. W.

4. *Geological Map of the 40th Parallel Survey.*—Map number II, by Clarence King, Geologist in Charge, and S. F. Emmons, Assistant Geologist, has been issued as authors proofs, dated Nov. 15th, 1875.—This map, which covers the Green River Basin and most of the Uinta Mountains, a region of great geological interest, will be regarded as a model, as it has not been surpassed in accuracy and artistic execution by any similar work in this country. It is in two sheets, each 24 by 33 inches, and is on a scale of four miles to the square inch. It is the first of the series issued, and will be noticed more fully when the other parts are published.

5. *Depth of the North Pacific.*—The soundings by the "Challenger" in the North Pacific as given in the Proceedings of the Royal Society, No. 164, afford the following results:

Along a line from California to the Sandwich Islands the mean depth is 15,180 feet; and the least depth, about half way, near 13,000 feet.

Along a line from the Sandwich Islands to the Bonin Islands, south of Japan, the shoalest part is near 177° east longitude, where the depth is 6,650 feet.

Between longitude 177° E. and the Sandwich Islands the mean depth is about 16,000 feet; maximum depth, 19,140 feet; depth within eighty miles of the Sandwich Islands south of Kauai, over 14,000 feet.

Between longitude 177° E. and the Bonin Islands, the mean depth is nearly 16,900 feet; maximum, 19,720 feet.

On a line running north from the Sandwich Islands, between latitude 22° and 38° N., mean depth about 17,000 feet; and between this northern point and Japan, mean depth about 16,000 feet; maximum, 22,800 feet, within 180 miles of Japan, and minimum near 178° E., 12,300 feet.

The region of the minimum on this last route is nearly north of that on the route from the Sandwich Islands to the Bonin Islands; but the depth is greater, being 12,300 feet against 6,650 feet on the latter.

The mean depth for the north Pacific as deduced from all the deep-sea soundings is about 16,200 feet.

6. *An Iceland chain of elevations in the North Atlantic.*—The ship "Valorous," which took out stores to Disco for the British Polar Expedition, made deep-sea soundings on its return. Among the discoveries, as mentioned in a Report to the Royal Society (Proc. No. 164), was an elevation of the ocean's bottom in latitude 56° N. and longitude 34° 42′ W., to the southwest of Iceland, over which soundings of 690 fathoms were obtained between depths of 1450 fathoms on one side and 1230 on the other. Directly between this spot and Iceland, in latitude 59° 40′ N., and 29° 30′ W., H. M. S. "Bull-dog" found a similar elevation. In about the same direction, northeast of Iceland, there lies the island of Jan Mayen. This line is parallel to the Greenland coast, and the whole length thus indicated is over 1300 miles. Iceland and Jan Mayen being volcanic, it may be that the whole range is volcanic in nature or origin—an off-shore volcanic range. The line of this chain of elevations, moreover, if continued southwestward, passes just outside of Newfoundland and the Atlantic border of the United States.

7. *Journal of the American Electrical Society, including Original and selected papers on Telegraphy and Electrical Science.* Vol. I, No. 1. 100 pp. 8vo, with several wood-cuts. Chicago: 1875. Published for the American Electrical Society.—A new, handsomely printed journal, devoted to electrical discoveries, and the various practical applications of electricity. The first paper is on the transmission of musical notes telegraphically, by Elisha Gray. The author closes with the statement that, "by this method, not only may different messages be sent simultaneously, but a tune with all its parts can be distinctly audible at the receiving end."

APPENDIX.

ART. XVIII.—*Principal Characters of the Dinocerata;* by O. C. MARSH. With five plates.

THE huge Eocene mammals, discovered by the writer in 1870, and subsequently placed in the new order *Dinocerata*, prove to be a well marked group of much interest. The Yale College Museum now contains remains of more than a hundred individuals, some of them in such excellent preservation that few points in the osseous structure of these animals need longer remain in doubt. It is proposed, therefore, to give, in the present communication, the more important characters of the members of this order, reserving the detailed description for a separate memoir. Although several distinct genera of *Dinocerata* are now known, as shown below, the typical characteristics of the group are best seen in *Dinoceras*, and hence I describe first that genus, which is especially illustrated in the accompanying plates.

DINOCERAS Marsh, 1872.*

The skull in *Dinoceras* is long and narrow, the facial portion being greatly produced. The basal line, extending from the lower margin of the foramen magnum along the palate to the end of the premaxillaries, is nearly straight. The top of the skull supports three separate transverse pairs of osseous elevations, or horn-cores, which form its most conspicuous feature, and suggested the name of the genus. The smallest of these protuberances are situated near the extremity of the nasals; a second much larger pair rise from the maxillaries in front of the orbits; while the largest are on the parietals, and supported by an enormous crest, which extends from near the orbits entirely around the lateral and posterior margins of the true cranium. (Plate II.) The posterior crest, which curves upward and backward beyond the occipital condyles, is mainly composed of the supra-occipital. The floor of the deep depression in front of this crest is formed by the parietals. These bones also send up the lateral crests. The top of the skull between the orbits is formed of the frontal bones, which are remarkably short. Their superior sutures with the parietals pass just in front of the lateral crest, and then converge posteriorly. There is no postorbital process, but in some species of

* This Journal, iv, 343, v, 117, 293 and 310.

the genus there is a prominence on the frontal, directly over the orbit. The nasals are greatly elongated, being nearly half the length of the entire skull. They unite with the frontals by oblique sutures, directed backward and inward, and nearly parallel with the superior fronto-parietal sutures. (Plate II, figure 3.) The osseous protuberances on the extremities of the nasals are of moderate size in *Dinoceras*, but, like the maxillary horn-cores, vary much with age. Both may possibly have been covered with thick skin, and not with true horns.

The orbit is large, and confluent with the temporal fossa. The latter is of great extent posteriorly, but the zygomatic arches are only moderately expanded. The squamosal forms the lower portion of the temporal fossa, and sends down a massive post-glenoid process, which bounds in front the external auditory meatus. The latter has for its posterior border the post-tympanic process of the squamosal, which unites directly with the paroccipital, thus excluding the mastoid from the external surface of the skull, as in *Rhinoceros*. The tympanic portion of the periotic, also, does not reach this surface. There are small air-cells in the walls of the temporal fossa, both in the squamosal and parietals. The squamosal sends forward a strong zygomatic process, which resembles that in *Tapirus*. The malar completes the anterior portion of the arch, extending to the front of the orbit. (Plate II, figure 1.) The lachrymal is large, and forms the anterior border of the orbit. It is perforated by a large foramen. The maxillaries are massive, and quite remarkable in supporting a pair of stout, conical horn-cores, which vary in form and size in different species. These cones are solid except at the base, which is usually perforated for the fang of the canine tusk. The premaxillaries are elongated, and without teeth. They unite posteriorly with the maxillaries just in front of the canine, and then divide, sending forward two branches, which partially enclose above and below the lateral portion of the narial aperture. (Plate II, figure 1.) The lower portion is slender, and resembles the premaxillary of some Ruminants. The premaxillaries are not united at their extremities. The latter are rough, and probably supported a pad.

The palate is very narrow and deeply excavated, especially in front. The anterior palatine foramina are in the premaxillaries, and vary much in different species. In *D. mirabile* they are elongated fissures, enclosed between the lateral and palatine branches of the premaxillaries, as in *Equus*. In *D. laticeps* they are of small size, and oval in outline. The posterior palatine foramina are in the maxillaries near the anterior border, as in *Hippopotamus*. The posterior nares extend forward between the last upper molars. The occipital condyles are large, and bounded externally in front and below by a deep groove. They project downward and backward, showing that the head was declined when in its natural position. The exoccipitals are perforated by a condylar foramen of moderate size, which is separated from the larger foramen lacerum posterius by a slender partition of bone. Between the post-glenoid process and the basi-sphenoid, there is an irregular cavity filled in part below by the periotic. There is a distinct alisphenoid canal, and the foramen ovale is near its posterior orifice. In front of its anterior opening, is a small foramen lacerum anterius, and further forward, the optic foramen. The infraorbital foramen is large, and partially concealed behind the maxillary ridge which supports the malar.

The brain cavity in *Dinoceras* is perhaps the most remarkable feature in this remarkable genus. It proves conclusively that the brain was proportionately smaller than in any other known mammal, recent or fossil, and even less than in some reptiles. It was, in fact, the most reptilian brain in any known mammal. In *D. mirabile*, the entire brain was actually so diminutive that it could apparently have been drawn through the neural canal of all the presacral vertebræ, certainly through the cervicals and lumbars. The size of the entire brain as compared with that of the cranium is well shown in the accompanying cut, figure 1.

1.

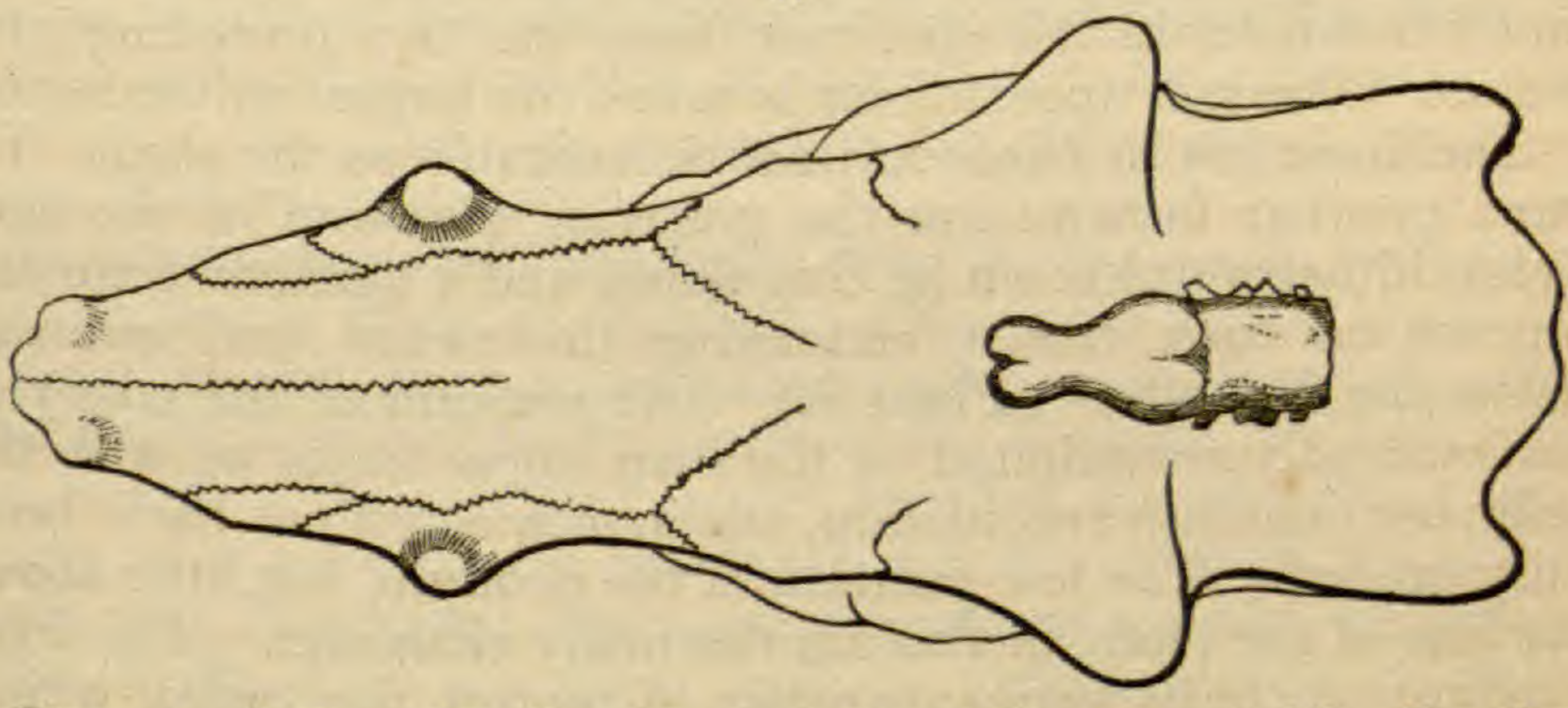


Figure 1. **Outline of Skull and Brain Cavity of *Dinoceras mirabile*. Top view; one-eighth natural size.**

The most striking feature in the brain cavity itself is the relatively small size of the cerebral fossa, this being but little larger than the cerebellar portion. This is shown in Plate IV, the figures of which are drawn from a cast of the brain cavity of *D. mirabile*, the type of the genus. The cerebral hemispheres did not extend at all over either the cerebellum or the olfactory lobes. The latter were large, and continued well forward. The hemispheres were apparently convoluted, and the Sylvian fissure distinctly marked. There was a rudimentary tentorial ridge. The cerebellar fossa is but little larger transversely than the medullar canal, and has lateral cavities which may have been occupied by flocculi. The pituitary fossa is nearly round, and of moderate depth. There are no clinoid processes. The brain as a whole resembled that in some Marsupials more than in any other known mammals. Its small size, as the writer has elsewhere shown, is a character apparently pertaining to all Eocene mammals;* the brain-growth during the rest of the Tertiary period having been gradual, and mainly in the cerebrum.

The teeth in *Dinoceras* are represented by the following formula:

$$\text{Incisors } \frac{0}{3}; \text{ canines } \frac{1}{1}; \text{ premolars } \frac{3}{3}; \text{ molars } \frac{3}{3}; \times 2 = 34.$$

The superior canines are long, decurved, trenchant tusks. They are covered with enamel, and their fangs extend upward into the base of the maxillary horn-cores. There is some evidence that these tusks were small in the females. Behind the canine, there is a moderate diastema. The molar teeth are very small. The crowns of the superior molars are formed of two transverse crests, separated externally, and meeting at their inner extremities. The series is well shown in Plate III, which represents the upper premolars and molars of *D. mirabile.* The first true molar is smaller in this specimen than the two preceding premolars. The last upper molar is much the largest of the series.

The lower jaw in *Dinoceras* is as remarkable as the skull. Its most peculiar features are the posterior direction of the condyles, hitherto unknown in Ungulates, and a massive decurved process on each ramus, extending downward and outward below the diastema. (Plate V.) The position of the condyles was evidently necessitated by the long upper tusks, as, with the ordinary ungulate articulation, the mouth could not have been fully opened. The low position of the condyle, but little above the line of the teeth, is also a noteworthy character. The long pendant processes were apparently to protect the tusks, which would otherwise be very liable to be broken. Indications of similar processes are seen in *Smilodon*, and some other Carnivores with long upper canines. With the exception of these processes, the lower jaw of *Dinoceras* is small and slender. The symphysis is completely ossified. The six incisors were contiguous, and all directed well forward. Just behind these, and not separated from them, was the small canine, which had a similar direction. The crowns of the lower molars have transverse crests, and the last of the series is the largest. (Plate V, figure 3.)

The vertebræ in *Dinoceras*, in their main characters, resemble

* This Journal, viii, p. 66, July, 1874.

those of Proboscidians. The atlas and axis are very similar to those of the elephant, but the rest of the cervicals are proportionally longer. The dorsal and lumbar vertebræ have the articular faces nearly flat, and the lumbars have an inferior ridge on the median line. There are four sacral vertebræ, the last being quite small. The anterior caudals have long, depressed, transverse processes. The ribs resemble those in *Mastodon*. The segments of the sternum were well ossified, and most of them were flattened vertically.

The scapula, in its general form, is similar to that of the elephant, but there is much less constriction above the glenoid fossa. The latter is elongate, deeply concave longitudinally, and nearly flat transversely. The spine extends downward nearly to the glenoid border. The coracoid portion is a rugose protuberance, separate from the margin of the articular fossa. The humerus is short and massive, and in its main features resembles that of the elephant. One of the most marked differences is seen in the great tuberosity, which does not rise above the head, and is but little compressed. The condylar ridge, moreover, of the distal end is tubercular, and not continued upward on the shaft. The lower extremity of the humerus is much like that of the rhinoceros, and the proportions of the two bones are essentially the same. The radius and ulna are nearly of the same size. The head of the radius rests on the middle of the ulnar articulation, and hence the shaft of this bone does not cross that of the ulna so obliquely as in the elephant. The ulna has a small face for articulation with the lunar, as in the elephant.

There are five well developed toes in the manus, which is well shown in Plate VI, figure 2. The carpal bones are eight in number, and form interlocking series, as in Perissodactyls. The scaphoid resembles that bone in the elephant, but is shorter and stouter. Its proximal end is rounded, forming about one-fourth of a sphere. On its distal end, the articular faces are confluent. It supports the trapezium and trapezoid. The pyramidal sends down an outer angle to articulate with the fifth metacarpal, as in *Elephas*. The trapezoid is the smallest bone in the carpus. The magnum is supported by the lunar, and not at all by the scaphoid. The unciform is the largest carpal bone. It has the usual metacarpal faces well marked, and separated by ridges. The metacarpals are of moderate length, and the third is about equally supported by the magnum and unciform. The articulations for the phalanges are nearly flat, indicating but little motion. The phalanges are very short, and the distal ones rugose.

The pelvis is much expanded, as in Proboscidians. The ilium is suboval in outline. The pubis is slender and short, and the ischium has less posterior extension than in the elephant. The thyroid foramen is an elongate oval. The femur is proportionally about one-third shorter than that of the elephant. The head of this bone has no pit for the round ligament, and the great trochanter is flattened and recurved. There is no indication of a third trochanter. The distal end of the femur is more flattened transversely than in the elephant, and the condyles are more nearly of the same size. The corresponding articular faces of the tibia are consequently about equal, and also contiguous, with no prominent elevation between them. When the limb was at rest, the femur and tibia were nearly in the same line, as in the elephant and man. The patella is elongate, and oval in outline. The fibula is slender, and entire, with articular faces well marked at each extremity. The astragalus has no distinct superior groove. Its anterior portion has articular faces for both the navicular and cuboid, thus differing from Proboscidians, and agreeing with Perissodactyls. The calcaneum is very short, its longitudinal and transverse diameters being about equal. It does not articulate with the navicular, as in *Elephas*, and has only a small face for the cuboid. There are four well developed digits in the pes, and a rudimentary or small hallux. The metatarsals are much shorter than the metacarpals. The phalanges and sesamoid bones are smaller, but otherwise similar to those of the manus. The hind foot is shown in figure 1 of Plate VI. None of the bones of the skeleton are hollow.

The known species of *Dinoceras* nearly equalled the elephant in size, but the limbs were shorter. The head could reach the ground, and there is no evidence of a proboscis. All the remains of the genus yet discovered are from the Eocene of Wyoming.

Yale College, New Haven, Jan. 18th, 1876.

(To be continued.)

Explanation of Plates.

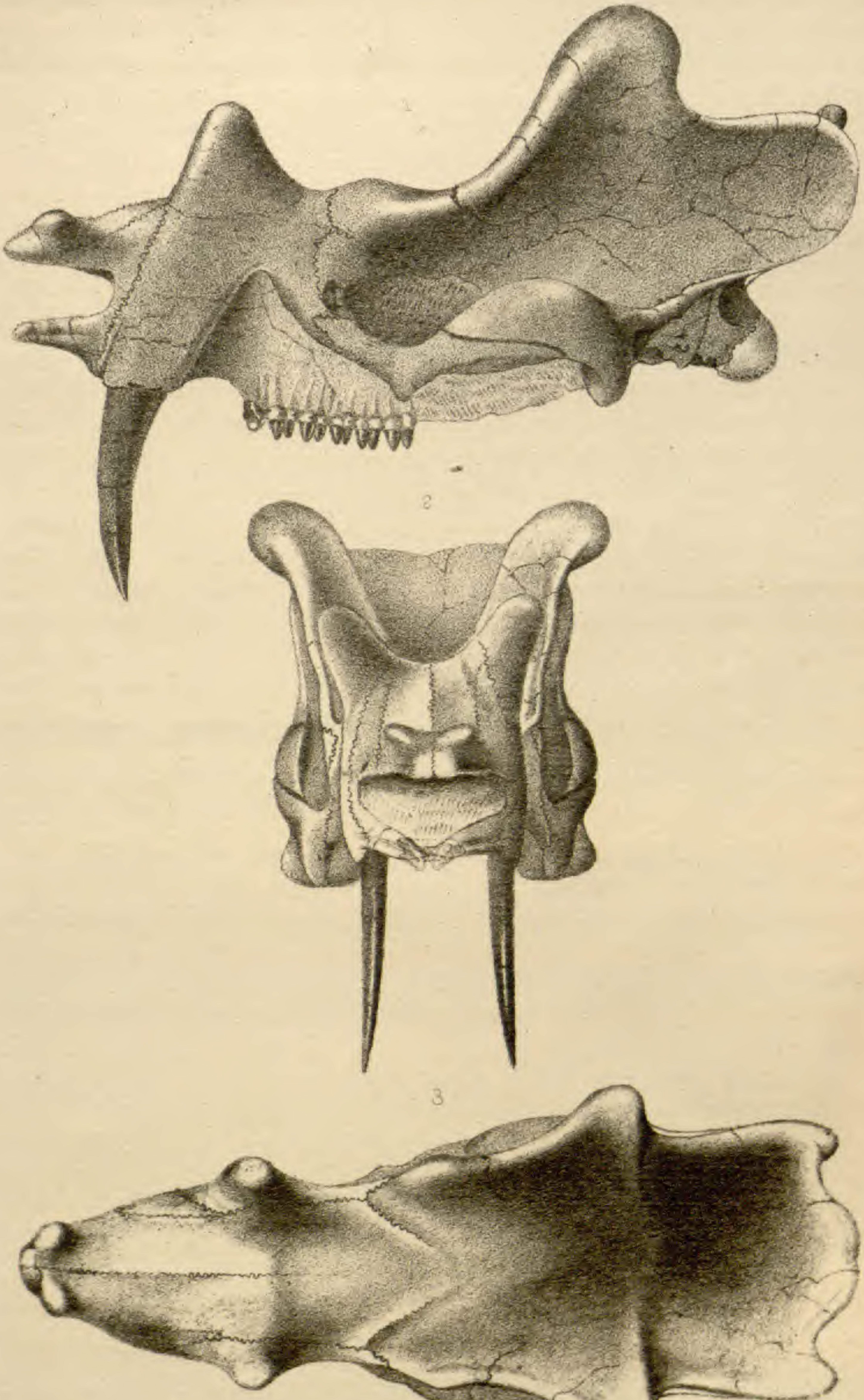
Plate II.—*Dinoceras mirabile* Marsh. Figure 1, side view of skull; figure 2, front view; figure 3, top view. One-eighth natural size.

Plate III.—*Dinoceras mirabile*. Superior premolar and molar teeth; bottom view. Three-fourths natural size.

Plate IV.—*Dinoceras mirabile*. Cast of brain cavity. Figure 1, side view; figure 2, top view; figure 3, bottom view. One-half natural size.

Plate V.—*Dinoceras laticeps* Marsh. Lower jaw. Figure 1, front view; figure 2, side view; figure 3, top view. One-fifth natural size.

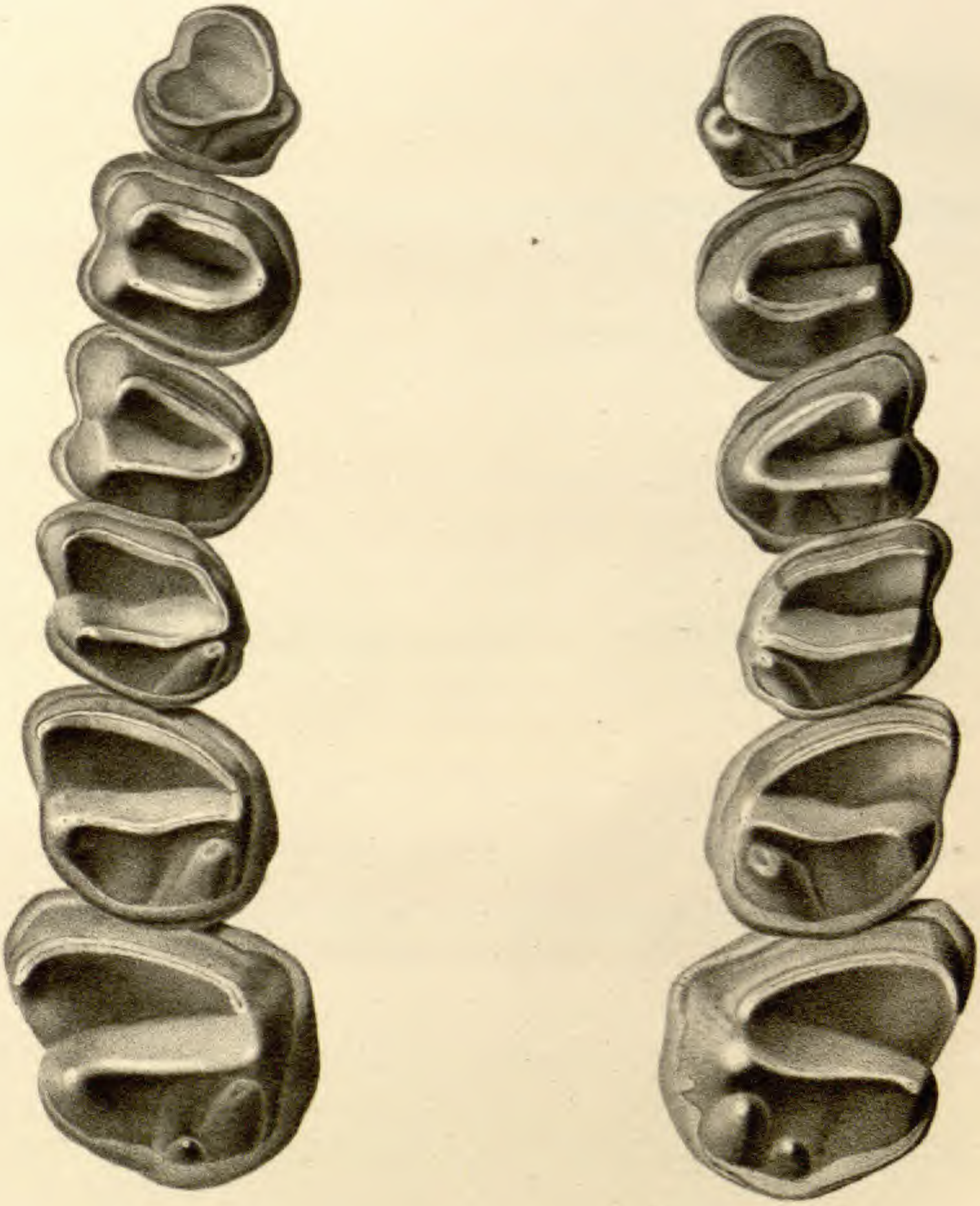
Plate VI.—*Dinoceras*. Figure 1, hind foot; figure 2, fore foot. One-third natural size.



Drawn from nature by E. Crisand.

Punderson & Crisand, New Haven, Ct.

DINOCERAS MIRABILE, Marsh 1/8

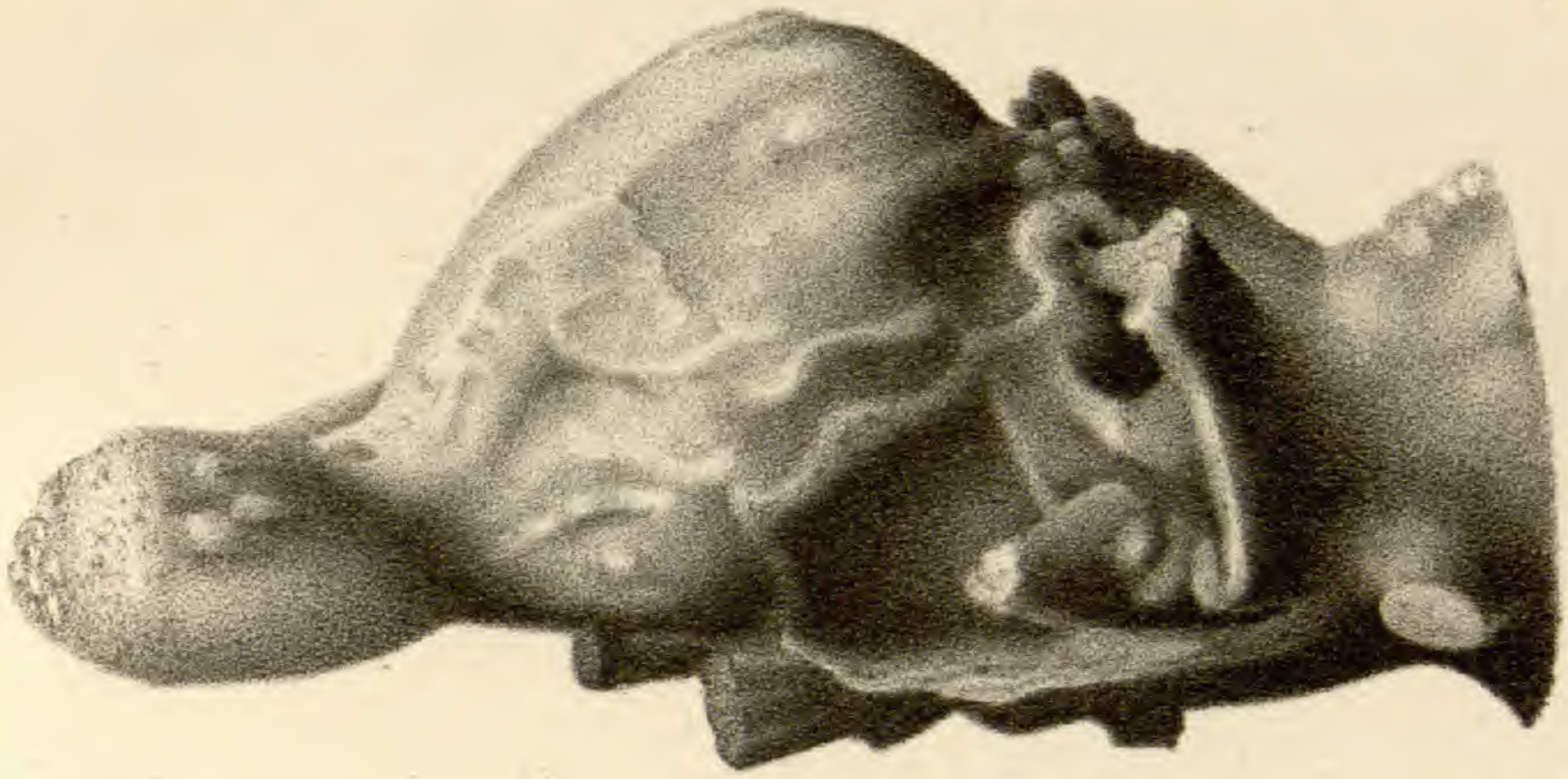


Drawn from nature by E. Crisand.

Punderson & Crisand, New Haven Ct.

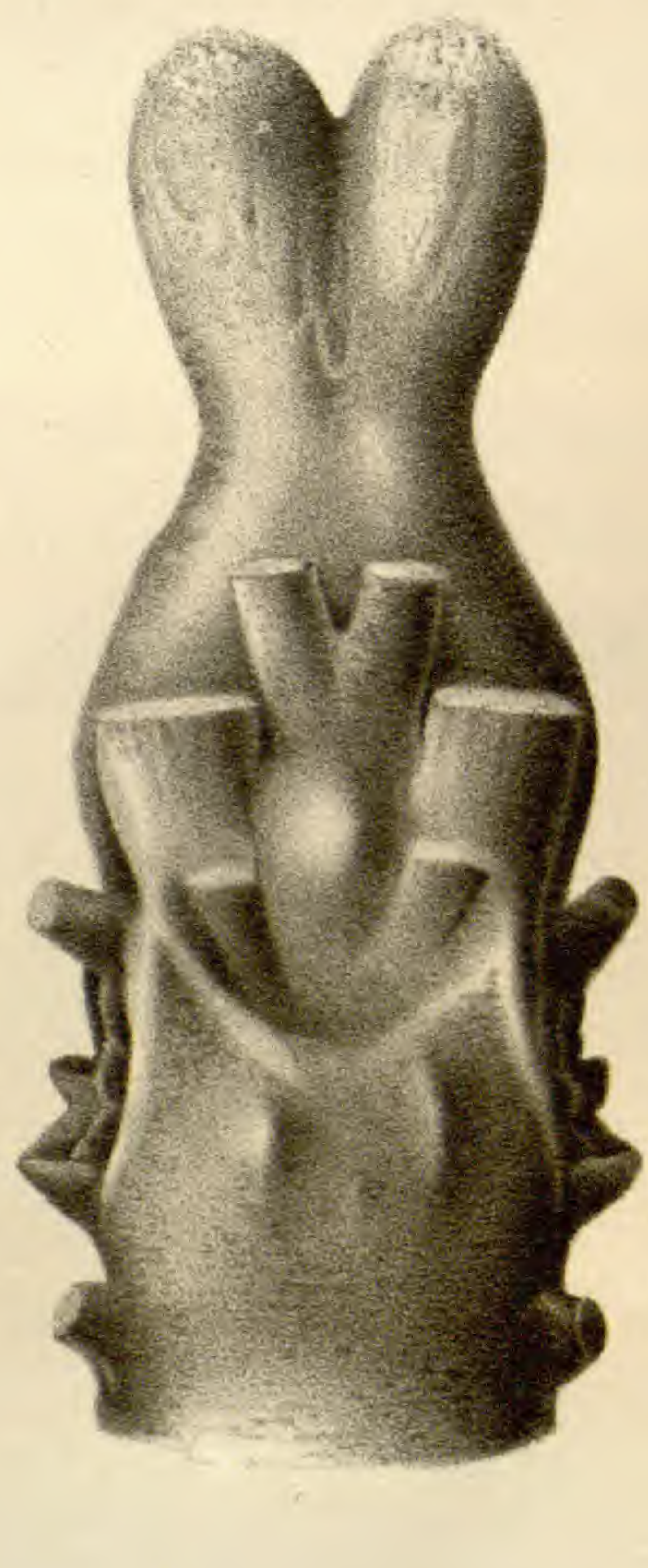
DINOCERAS MIRABILE, Marsh. 3/4

1



2

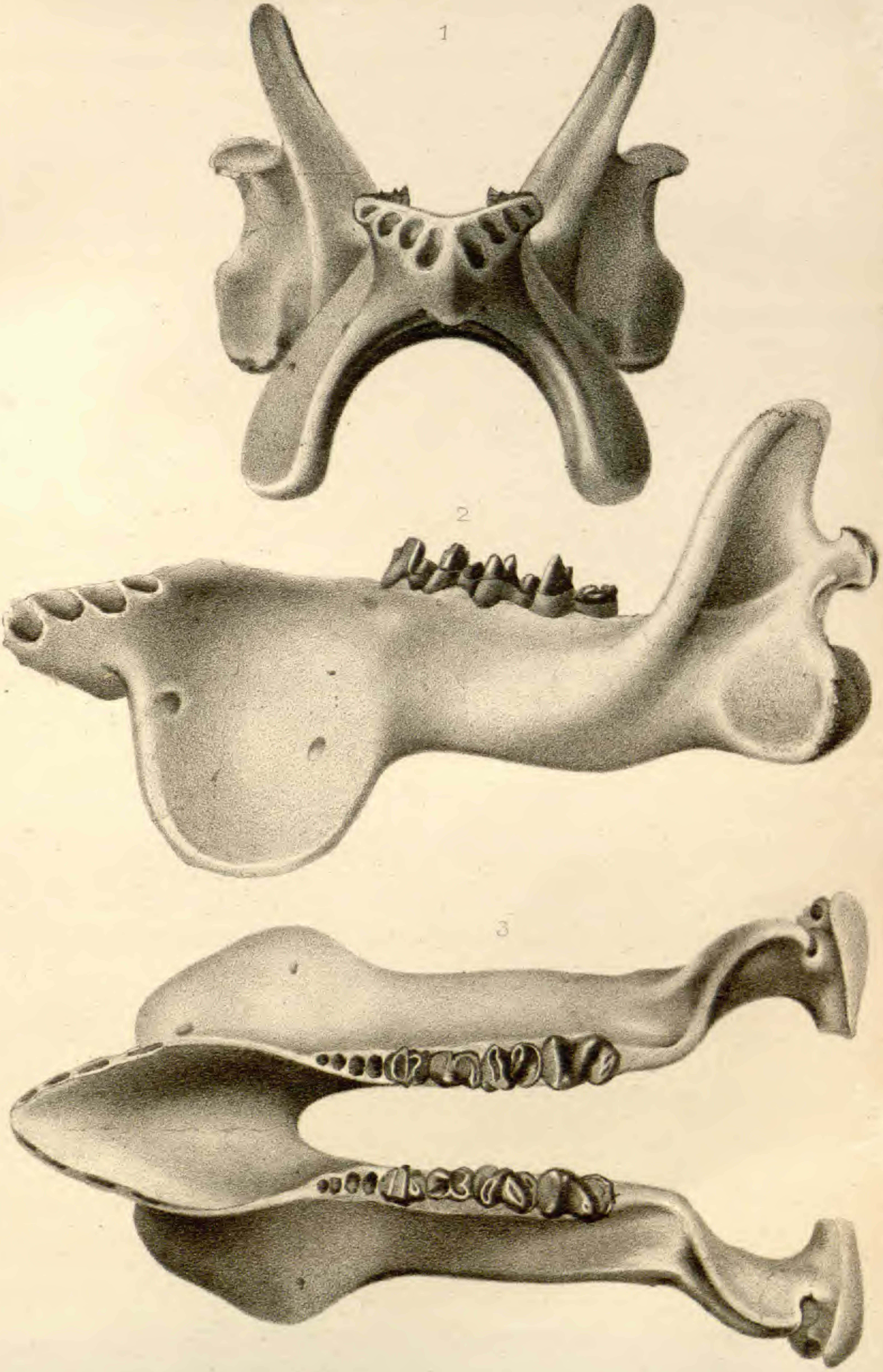
3



Drawn from nature by E. Crisand.

Punderson & Crisand New Haven, Ct.

DINOCERAS MIRABILE, Marsh. ½

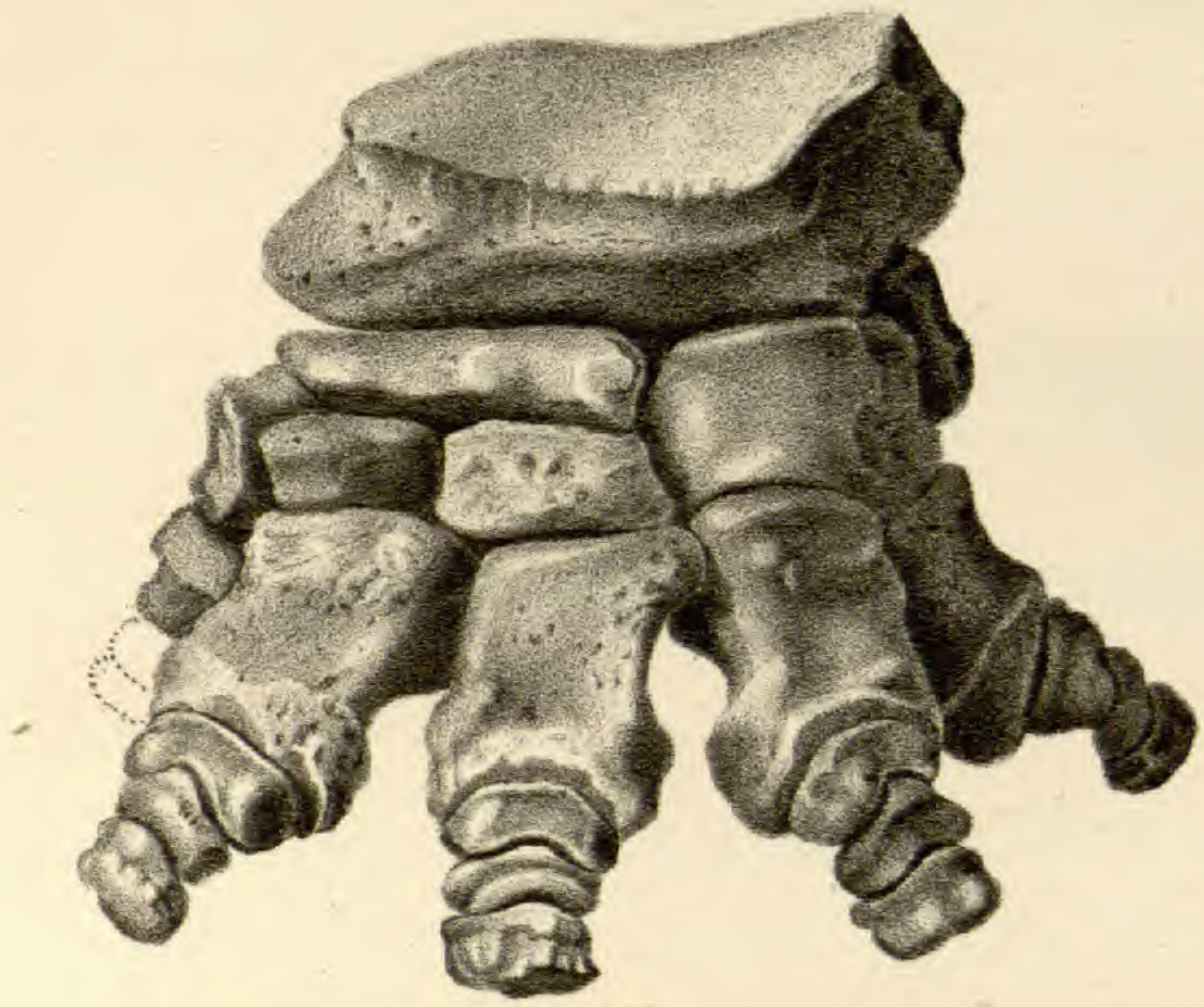


Drawn from nature by E. Crisand.

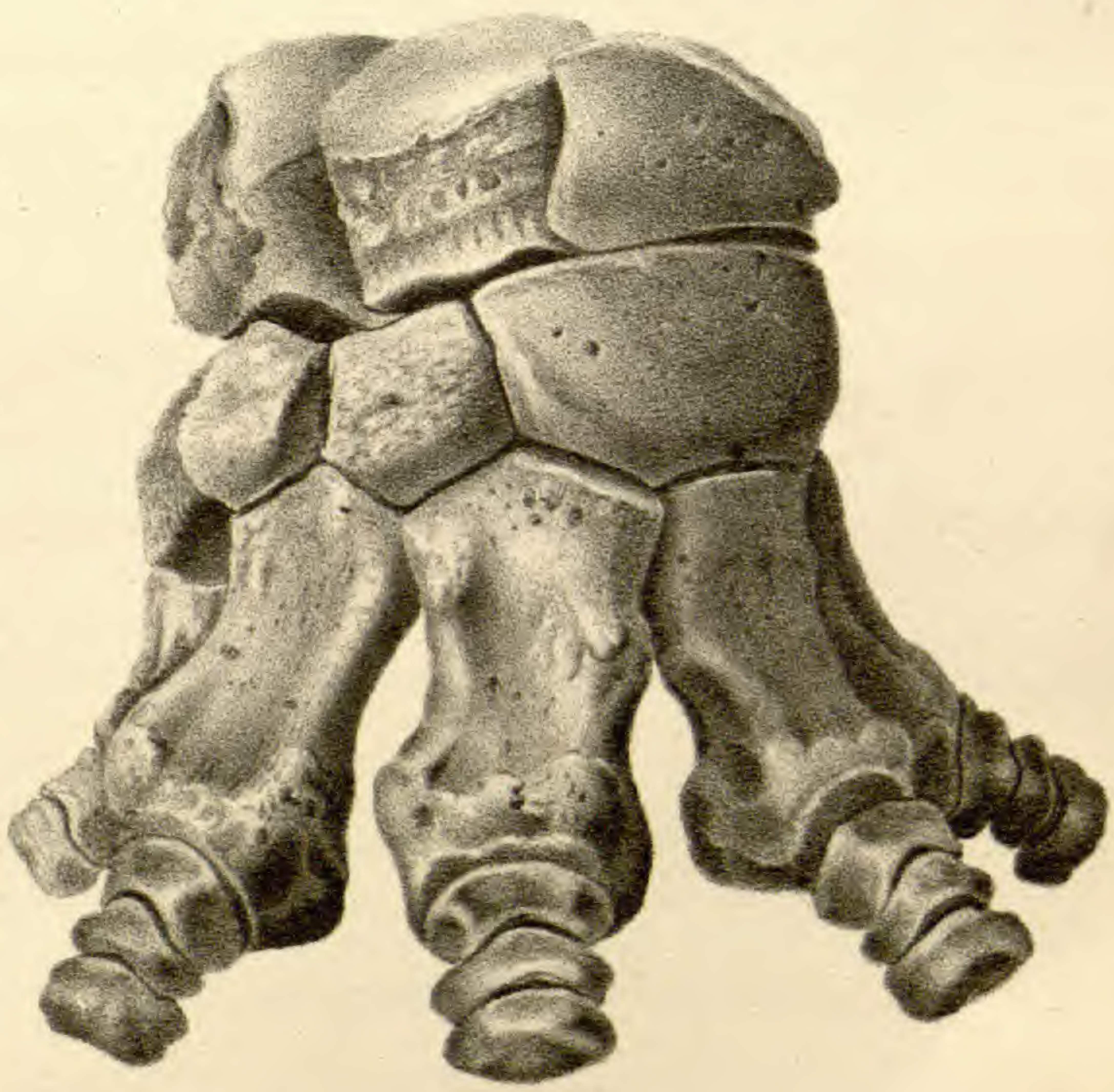
Punderson & Crisand. New Haven. Ct.

DINOCERAS LATICEPS Marsh 1/5

1



2



Drawn from nature by E. Crisand.

Punderson & Crisand New Haven, Ct.

DINOCERAS 1/3

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XIX.—*On the Veiled Solar Spots*;* by L. TROUVELOT.

[Read before the American Academy by William A. Rogers, Oct. 12, 1875.]

IT is now pretty well established that the visible surface of the sun is a gaseous envelope called "the chromosphere;" mainly composed of incandescent hydrogen gas, with which are occasionally associated some metallic vapors, usually occupying the lower strata. To all appearances, the granulations called "rice grains," the faculæ and the protuberances, are phenomena belonging to the chromosphere; in fact they are the chromosphere itself seen under the particular forms and aspects peculiar to it. Ordinarily this envelope has a thickness of 10″ or 15″. This thickness, however, is by no means constant, varying from day to day within certain narrow limits.

At no time since I have observed the sun have I seen the chromosphere so thin and shallow as during the present year, and especially between June 10 and August 18. I had before quite often observed local depressions and upheavals of the chromosphere, sometimes extending over large surfaces, but I had never before observed such a *general* subsidence.

So thin was the chromosphere during this period that it was sometimes very difficult to obtain its spectrum by placing the slit of the spectroscope tangent to the limb of the sun. This was especially the case on the afternoon of August 9.

This unusual thinness of the chromosphere could be easily recognized without the assistance of the spectroscope. Indeed, the phenomenon was even more interesting seen through the telescope, as, with it, the structure of the photosphere, lying as

* From the Proceedings of the American Academy.

it does under the envelope of the chromosphere, could be better seen through the thin veil formed by the greatly attenuated chromospheric gases.

That the gases forming the chromosphere are sometimes thin enough to become transparent is a phenomenon which I have observed hundreds of times; as is abundantly proved by the numerous drawings of protuberances which I have made at the Harvard College Observatory, in which the limb of the sun is seen through the base of the protuberances in front of it. In plate X, figure 3, there occurs a very striking instance, where two small prominences are seen through a large protuberance nearer the observer.

During this period of general subsidence, the granulations appeared to be smaller and farther apart than usual, and consequently the light-gray colored background upon which they are seen projected was more distinct, as it occupied more space than formerly. During this period, the light-giving element would appear to have been less than usual.

I am not aware that the phenomena of which I shall speak in this communication have been before observed; but I cannot speak positively on this point, owing perhaps to the somewhat confused nomenclature of solar physics.

Ever since I have observed the sun with instruments of a large aperture, I have noticed that the light-gray colored background seen between the granulations is by no means uniform, as it is generally stated to be. On the contrary it is greatly and strikingly diversified. Aside from the very small black dots called "pores," patches of a darker gray are irregularly distributed all over the surface of the sun. But partly owing to the effect of perspective, and partly on account of the thicker strata of the chromospheric gases through which they are necessarily seen near the limb, they disappear gradually as they approach the border.

These dark spots have been so remarkable during the present year, and so conspicuous during the period of the greatest subsidence of the chromosphere, that I have availed myself of every favorable opportunity to study them. So strongly were they marked that when one had passed the field of view, it could be easily found again among many others, even after the lapse of several hours. Of the most striking and complicated, I have made sketches.

In order to be able to count how many of these gray spots could be seen in different heliographic latitudes, and also to estimate their area with respect to the whole surface of the sun, Mr. W. A. Rogers, assistant at the Harvard College Observatory, kindly ruled for me on glass a reticule of small squares. Though the problem is apparently a simple one, it nevertheless presented many difficulties; partly owing to the minuteness and delicacy of these objects, partly on account of the unsteadiness of the atmosphere, and partly to the many defects caused by the great amount of heat concentrated at the focus of the objective. However, the observations show clearly that though the number of gray spots varies but very little in different latitudes, in general the spots become larger and more complicated as they approach the equatorial zones.

The most marked characteristic of the gray spots is their vagueness of outline. They are never sharply defined like ordinary spots, but they appear blurred and diffused like an object seen through a mist. As I shall endeavor to show presently, these objects are really seen through chromospheric gases which are spread as a veil over them, causing this vagueness of outline. For this reason, I propose for them the name of *Veiled Solar Spots.*

The veiled solar spots, especially in the lower latitudes, have a remarkable tendency to assemble into small groups after the manner of ordinary spots. Sometimes three or four are seen in contact, while there are comparatively large intervals where none are to be seen. I have in several instances seen the actual formation into groups of distinct veiled spots.

The granulations of the chromosphere are seen projected upon the veiled spots, just as anywhere else, but they are not there so regularly distributed; some being closely crowded together, while others are widely scattered. Small faculæ are often formed in this manner by the aggregation of several granules into one mass. Once in a while the granulations appear as if they were under the power of a propelling force by which they arrange themselves in files, and sometimes in capricious figures which are very remarkable.

In many cases I have observed that the granulations projected upon the veiled spots have an extraordinary mobility, to be seen nowhere else, except perhaps in the immediate vicinity of ordinary spots in full activity. Often their form and position are totally changed within a few minutes, and sometimes even within a few seconds. This was especially the case June 21. At 8^h 30^m on that day, I was observing a group of veiled spots not far from the center of the sun, when my attention was drawn to the extraordinary mobility of the granulations covering this group. In an instant they changed their form and position, some crowding together as though briskly attracting each other, while others would fly apart as if repelled by an invisible force. Under this tumultuous conflict of forces, new veiled spots would appear and disappear in an instant, faculæ would form and vanish; in fact, all was in motion and confusion on that particular part of the sun. It was evident that immense forces were in conflict under the chromosphere.

At 2^h 0^m P. M., on the same day, several small black spots had opened through the chromosphere upon the group of veiled spots observed in the morning. At 8^h 0^m on the following morning, the group of small black spots was considerably increased, having quite a large spot on the preceding side, followed by twelve or fifteen smaller ones. On June 24, this group had attained to its maximum size. It was then very large and complicated. In fact, it was the largest group of sun spots observed thus far during the present year.

On August 8, I noticed a group of veiled spots a little south of the sun's center. The following morning at 7^h 0^m, there was at the same place a small group of half a dozen black spots disposed in a crescent shape. At 2^h 0^m P. M., the black spots had vanished, but the veiled spots still remained, having retained the characteristic crescent form of the black spots and many other details observed in the morning; and, as a proof that the chromosphere covered this spot, *the granulations could be plainly seen upon the whole, indicating clearly that this spot was seen through the veil of the chromospheric gases.*

On August 24, the same phenomenon took place. Just following the principal spot of the only group then to be seen on the surface of the sun, there was a fine group of veiled spots. The following day some black spots had made their appearance upon them. On August 27, the black spots had vanished, but in their place the veiled spots seen at first still remained, and they continued to be seen there for several days.

To all appearances, the black spots which I had seen disappear under the chromospheric gases, and which continued as veiled spots, were exactly alike and undistinguishable from the many other veiled spots scattered all over the sun; and, had I not seen the opening of the photosphere, with the black spots, I could not have had any idea of the true nature of the veiled spots.

So far, I have only spoken of veiled spots observed in the zones where the ordinary sun spots usually make their appearance; but, as I have said, the veiled spots are scattered all over the surface of the sun.

During this period, I had many occasions to observe very remarkable and characteristic veiled spots in very high heliographic latitudes north and south. On July 15, within a few degrees of the north pole of the sun, I observed a remarkable veiled spot, unusually large and dark. Upon it were several bright slender faculæ projected in crest shape to very high altitudes. These faculæ appeared to be precisely like those observed in lower latitudes near ordinary sun spots. Upon this veiled spot could unmistakably be seen a small black spot, not a pore; a real opening of both chromosphere and photosphere.

On August 9, I observed another remarkable veiled spot within about 10° from the north pole, and upon it could be seen three small black spots.

On August 13, at 11^h 0^m, I observed a very dark veiled spot within 6° or 8° from the north pole. It had upon it a group of small faculæ, so characteristic of the spots of lower latitudes. At 4^h 30^m in the afternoon, this veiled spot was still darker, and upon it, near a facula, a pretty large black spot was visible.

On August 24, I observed a remarkable veiled spot at about 75° south latitude.

On September 6, another large group of veiled spots was seen within 10° or 15° of the north pole. At 10^h 20^m, some faculæ had formed upon it, and two black spots were distinctly visible. At 5^h 0^m in the afternoon, this group was still visible.

On September 8, within a few degrees of the north pole, I observed a fine group of two veiled spots, unusually dark and large, and near one of these spots there was a pretty large and bright facula. Ten minutes later the dark veiled spots had vanished, leaving in their place some bright faculæ. One minute later the veiled spots began to reappear, but under another form, to disappear again the next moment.

A little southwest from this last group, but in the same field of view, was another group of veiled spots apparently in full activity. Upon it three or four black spots were visible for some seconds. Upon these veiled spots the granulation had an extraordinary mobility; so much so, that I expected at every moment to see a large spot make its appearance, but in less than a minute the veiled spots and the black spots had both vanished, and in their place were formed in an instant, some very bright faculæ.

To all appearances, the veiled spots seen in high latitudes differ but very little from the ordinary sun spots of the lower latitudes, except in regard to magnitude and activity. The difference seems particularly to be that, in the first, the umbra, instead of being freed from the gases and vapors, is partly or wholly choked with them; while, besides, the chromosphere covers it. The forces which open the photosphere in high latitudes, it would seem, have not sufficient energy to repel or dissolve the chromospheric gases; or, if they have, it is in a very feeble degree, but, even then, the phenomenon is generally of short duration.

Though I had no means of making accurate measurements of the positions of the spots seen in high latitudes, the error of my estimation cannot be very great. In any case a few degrees would certainly cover it, and it remains a fact that I have observed spots at least within 10° of the north pole of the sun. The importance of this observation will appear when it is stated that very few spots have been observed outside of the zones lying 40° on either side of the equator. I know of but two instances on record in which spots have been observed beyond this limit. La Hire observed a spot 70° from the equator, and more recently, in the month of June, 1846, Dr. C. H. F. Peters observed at Naples a spot 50° from the equator.

It is further to be remarked that according to the conclusions of the English observers, the solar spots attain higher latitudes during the years of the maximum number of spots, and recede more and more towards the equator as the minimun is approaching; and it is to be noted that the present year is precisely, or at least very nearly, a minimum year. It is doubtless owing to the unusual thinness of the chromosphere during this period that spots have been observed in so high latitudes this year. It is true that the spots were small, but, nevertheless, they were genuine spots, with all the characteristics of larger spots.

It is difficult for one who has seen the phenomena which I have described, to come to any other conclusion than this: that the veiled spots are breaks or true openings in the photosphere, seen through the imperfectly transparent gases composing the chromosphere, openings themselves partly or wholly filled by the vapors ejected by the forces from the interior of the photosphere. If this hypothesis should prove to be the expression of a fact, then we should expect to find that the photosphere is perforated by thousands of crevasses either partly or entirely filled with the vapors and gases from the interior, which cannot be ejected outside for want of sufficient energy, save for a comparatively very small number situated in the equatorial zones, where this energy appears maximum, and is able to repel and dissolve the gases from the interior.

Before the observations of this year, I had arrived at precisely the same conclusions in regard to the opening of the photosphere in all latitudes, and to the existence of invisible spots concealed by the chromosphere. These conclusions were derived from my observations with the spectroscope, made at Harvard College observatory during a period of thirty-five months. A discussion of these observations is reserved for a future communication.

Though one can hardly form a settled opinion with regard to the cause of the general depression of the chromosphere, on account of the imperfect data, it seems natural, however, to suppose that the phenomenon is connected in some way with the minimum period of sun spots. Judging by the great number of veiled spots observed, and by the myriads of pores seen between the granulations, it would seem that both the chromosphere and photosphere have been much thinner than usual during the present year.

If there are breaks in the photosphere at many points of the surface of the sun, it becomes easy to account for the unusual thinness of the chromosphere this year, because as observed by myself and others, at certain phases of the spots, the chromospheric gases, rushing with impetuosity into the umbra, go down under the photosphere like gigantic waterfalls, diminishing consequently the thickness of the chromosphere. That this takes place I shall give ample proof in another communication.

It seems evident that the chromosphere near a spot is kept from falling into the opening by a force from the interior. As soon as this force decreases in energy, immediately the chromosphere tends to cover it, and even to precipitate itself through the opening when this force becomes extinct. The observations show this plainly.

When a spot is decreasing, it is quite common to observe that the umbra and penumbra appear as if they were seen through a heavy fall of snow, their surfaces being covered by numerous bright flocculent granulations surrounded by a kind of bluish fog. In a few instances of very rare definition, I have been surprised to see faint traces of this flocculent appearance upon almost all the spots; indeed it would seem that the spots are rarely free from some faint traces of the chromospheric gases. Probably the bright flocculent objects observed upon the umbra and penumbra of spots, are the granulations of the chromosphere dissolved to a greater or less degree by the forces emanating from the spots.

Perhaps it may not be idle to remark that, during the period mentioned, I have almost every day observed small groups of faculæ in the polar regions, especially near the north pole of the sun; while, for the most part, they have been entirely absent from the equatorial regions, where they are commonly found.

To conclude, my observations show:

1. That during this year, and especially during the interval from June 10 to August 18, and to a less degree to September 14, the chromosphere has been notably thinner than usual upon the entire surface of the sun.

2. That the granulations have been smaller and less numerous.

3. That the light-gray colored background seen between the granules has been more conspicuous and has occupied more space than usual.

4. That there are spots, which I have named "veiled spots," which are seen through the chromospherewhich is spread over them like a veil.

5. That these veiled spots are true openings of the photosphere, like those of the ordinary spots.

6. That during this period these spots have been larger, darker, and more numerous than I have before seen them.

7. That the veiled spots are scattered throughout all latitudes, though more complicated in the regions where the ordinary spots make their appearance.

8. That I have observed spots at least within 10° of the north pole of the sun.

9. That the flocculent objects sometimes seen projected upon the umbra and penumbra of spots are the remaining portion of the granulations composing the chromosphere, more or less dissolved by the forces emanating from the interior of the photosphere.

Cambridge, October 1, 1875.

ART. XX.—*On the structure of Obolella chromatica;* by E. BILLINGS, F.G.S.

THE genus *Obolella* was founded in 1861,* on the following three species of Brachiopoda:

1. *O. chromatica*, discovered by J. Richardson in 1861, at a place called "L'Anse au Loup," on the north shore of the Straits of Belle Isle, in Labrador.

2. *O. crassa* Hall, from Troy, in the State of New York.

3. *O. polita* Hall, from Wisconsin.

1 2 3

Figs. 1, 2.—Ventral valves. The beak is not seen in either of the specimens. Fig. 3.—Diagram showing the position of the scars in the dorsal valves. All these figures are enlarged about 2¼ diameters.

The specimens exhibited the internal characters very imperfectly, yet enough was seen to convince me that the genus was a new one. During the fourteen years that have elapsed, I have received a number of letters, from both American and European authors, inquiring for more complete details of the structure of *O. chromatica*, which has always been considered to be the type. This information I was unable to give, for want of the facts. We are now in possession of specimens showing the interiors of both valves, almost completely. The following are the characters as nearly as they can be made out:

In the ventral valve there is a groove in the hinge line, for the passage of the pedicel. On each side of the groove there is a small, somewhat deeply excavated cardinal scar. In the cavity of the valve there are two elongated scars, which extend

* Geology of Canada, Palæozoic Fossils, vol, i, p. 7, 1861.

from near the cardinal scars forward about two-thirds of the length of the shell. These diverge from each other, more or less, in their extension forward, and are usually curved but sometimes nearly straight. They may be called laterals. They are, in general, separated from each other about one-third of the width of the shell. A little above the mid-length, and between the two laterals, there is a pair of small scars arranged transversely, with their inner extremities directed somewhat forward. The space above these two scars, between the upper portion of the laterals, is generally tumid from the thickening of the shell. In one of the specimens there is a small pit in the center of this space.

The dorsal valve has a small area, or nearly flat hinge facet. The minute beak is slightly incurved over the edge of the area. Beneath the beak there is a small sub-angular ridge, on each side of which there is a cardinal ? scar. The elongated scars, which seem to correspond to the laterals of the ventral valve, are here altogether in the upper half of the shell. They diverge widely in their extension forward. They are in general very slightly impressed, and would, most probably, escape the observation of any one who did not expect to find scars where they are situated. In the cavity of the valve there is a low rounded median ridge, which extends from a point near the hinge line forward a little below the mid-length of the valve. About the middle of the shell there are two small scars. These are usually striated longitudinally. The median ridge passes between them. The area is coarsely striated.

The above are the principal characters of this species, and they are subject to some variations, one of which is particularly worthy of notice. The two small cardinal scars of the dorsal valve are sometimes elongated laterally. This is carried to such an extent in another species (*O. gemma*) that they not only extend the whole length of the hinge-line, but are curved forward at their outer extremities and continued down into the cavity of the valve. In such cases they present an appearance similar to that of the groove beneath the hinge-line of the genus *Obolellina*. In other species of this genus the lateral scars of the dorsal valve are sometimes connected together by their upper extremities. But this is not a constant character. In different individuals, of the same species, these scars are either connected or not. The laterals are also sometimes connected with the cardinals.

The following are the original figures published in the Paleozoic Fossils, p. 7, (1861):

Fig. 4

Fig. 4, *a*, Ventral valve; *b*, dorsal; *c*, interior of ventral valve, showing the muscular impression; *d*, outline on a side view, restored from detached valves. Natural size.

In the description it is said: "Muscular impressions in the ventral valve, four; one pair in front of the beak, near the middle or in the upper half of the shell." The pair here alluded to are the laterals. Their upper and lower extremities are sometimes not visible, and what remains occupies the middle portion of the length of the shell. The expression "or in the upper half," I can thus explain: I had the dorsal valve of *O. crassa*, from Troy, which I then supposed to be a ventral valve. In this the laterals are in the "upper half." The transverse scars were not then observed and hence four scars instead of six. It must be borne in mind that fourteen years ago nothing was known of the internal characters of these shells. The materials were imperfect and consequently so was the description. It is now certain that the genus is a good one and that all of the three species on which it was founded belonged to it.

The described species which I consider to be truly within the genus are: *O. chromatica*, *O. polita*, *O. crassa*, *O. nana*, and *O. gemma*. They all, so far as is yet known, are confined to the *Potsdam Epoch*. A number of other species have been referred to the genus, but they are all more or less doubtful.

The specimens which have furnished the above additional details of the structure of *O. chromatica* were collected at L'Anse au Loup, the only place where the species has been found, in 1863, by T. C. Weston of our Survey, and by him very skilfully worked out of the matrix.

Art. XXI.—*On the Damming of Streams by drift ice during the melting of the great Glacier;* by J. D. Dana.

When treating of the overflows of the flooded Connecticut, in the Supplementary December Number of this Journal, (p. 497,) I suggested, in view of the fact that the terraces in the Farmington Valley about Tarifville and Simsbury are at least 50 feet higher than those a mile eastward in the parallel Connecticut valley—that the gorge through the Divide Range, by which the Farmington river there passes into the Connecticut valley, had been closed by drift and so remained until the flood had reached its height.

I allude to this subject again to add that the events connected with the opening, in the Spring, of many of our modern ice-covered streams afford abundant reason for believing that, during the breaking up of the long Glacial winter, when the melting was going forward, the gaps, gorges or narrows, along the river courses, would have been liable to obstruction by floating ice.

(*a*) Such obstructions would have been of all grades, from that which could simply impede the free flow of the waters, to the nearly perfect dam.

(*b*) The obstructions in particular cases might have existed for a very long era, instead of for a few weeks such as happens after a modern winter.

(*c*) Again, the slackened or suspended flow of the water, caused by such ice-obstructions, would have favored the deposition and accumulation about them of drift, and some may have thus been converted into complete dams. This process might occasionally have wholly filled with earthy material a gorge or narrow valley, so as to block up and divert the course of the stream.—The well-known case of Niagara River may be an example of this.

In view of these possible results, or rather these probable conditions of many river-valleys in the era of the Glacial flood, we are required to consider whether the height of the upper terraces *above the narrows* on the several rivers,—the Thames below Norwich, the Connecticut below Middletown, the Housatonic below Derby, Westfield River below Westfield, and Farmington River east of Tarifville—was not partly owing, in each case, to the existence of ice-obstructions at the narrows.

It seems to be very probable that this was so. The height of modern spring floods in the Connecticut at Middletown and Hartford is now often due in part to this very cause.

It appears to be certain, that if such obstructions existed in the Thames, Connecticut and Housatonic valleys, they were only partial obstructions; for, in the case of each, the terrace of the valley below the narrows *declines quite gradually in height* from the level above the narrows, instead of abruptly. Had the waters been held back, up to the height of the high upper terrace, by a close dam, they would have fallen over the dam with a plunge to a lower level; and this abrupt fall would have been registered by means of an abrupt fall in the level of the terrace. Instead of this, the terrrace on passing the narrows southward falls off at a rate not exceeding 10 feet a mile, varying in rate only with the varying width of the valley: a fact that seems to testify to the vastness of the flood as its cause, and not mainly to obstructions. Moreover, the material of the terraces below the narrows is like that above: the same in the prevalence of sands below and coarse gravel at top,—though having the latter of greater coarseness because of the more rapid flow of the stream along a narrower valley.

Further evidence with reference to the existence of such ice-barriers is to be looked for in a distribution of gravel and large bowlders across the valley just above the gorge or narrows, where the ice-masses had been brought to a stop and piled together;

for much of the floating ice would have been loaded with bowlders. I have as yet observed no satisfactory evidence of this kind, but think the question needs more investigation. Even if this evidence fails, we can hardly assert that no aid was afforded by ice in producing the great height of the flood-waters above the narrows, or doubt that ice-barriers made of drift ice had much to do with the height and extent of the upper terraces in portions of many other valleys.

There are two questions which should have here a word.

1. *May not the obstructions or dams have been made by the Glacier itself?* On this point we observe that the extent of the terrace formations along the valleys,—sometimes a score of miles in width even in New England—show that water swept in immense streams over the surface; and thus they seem to prove that the glacier was already out of the lower part of the valleys, and hence too far away to have obstructed the flow except through the pieces set afloat by its dissolution.

2. *Were not the dams due to rocky barriers at the narrows, or to the non-excavation of the valley from the narrows southward?* The features of the region about the narrows on each of the rivers mentioned, and of the valleys below, suggest decidedly that the valleys had nearly the same depth and extent then as now. The gradual decline in the height of the terrace on going from the narrows southward to the Sound shows that all was one valley, the part above the narrows and its continuation below. The terraces below the narrows, moreover, are built up in general from the *present* bottom of the valley, or from a lower depth, and this points to a depth for the valley as great as now or greater. It cannot be urged that the lower portions of the terraces were made after the upper. Wherever the hills on one side, at the narrows, retreat so as to give a chance for high terrace deposits, there these deposits are usually found, and sometimes the beds rise abruptly from the water's edge to the level of the highest terrace; and on the Connecticut, in a place of this kind above Middle Haddam, the bottom layers are of clay—like the lower layers in much of the stratified drift on the river.

In fact, the conditions of the terrace deposits of the valley, as well as the features of the valley itself, are explicable only on the view that the part of each valley below the narrows, like the rest of it, the narrows included, had been made before the Champlain period opened. The Glacial period was the era of valley excavation rather than the Champlain period.

ART. XXII.—*Sliding Friction on an Inclined Plane;* by A. S. KIMBALL, Professor of Physics in the Worcester (Mass.) Institute of Industrial Science.

THE following investigation was undertaken with a desire to demonstrate, if possible, by a laboratory experiment, that the law which affirms that the coefficient of sliding friction is constant for all velocities is not strictly true.

Our results seem to establish the point, at least in the case of bodies sliding down an inclined plane. I am aware that the truth of this law has been questioned; indeed the opinion of very many practical mechanics is directly opposed to it. Long ago Prof. Playfair remarked, as the result of some observations made at the slide of Alpnach, that it would appear that friction is neither proportioned to the pressure nor independent of the velocity. Later observations made at the launching of the Raritan and the Princeton (Jour. Frank. Inst., 3d, VII, 108) showed that the coefficient of friction just before the vessel left the ways was much less than during the first five seconds of its motion. More recent still are the experiments of M. Bochet (Comptes Rendus, April 26, 1858,) upon the friction of railway carriages and brakes, which point to the same conclusion; indeed the author goes so far as to give the form of the function which expresses the variation of the coefficient of friction with the velocity, and gives approximate values to its constants for the case of railway trains. His formula is copied by Weisbach with a caution.

Opposed to these views are the careful experiments of Coulomb and Morin, upon which the statements of our text-books are founded.

The apparatus used in our experiments was simple, but it seems capable of giving very sharp and reliable results. A smooth pine plank $10' \times 12'' \times 2''$ was firmly placed at a measured angle with the horizon and supported throughout by stout beams. Upon this plank was a weight box with pine runners, having a bearing surface of 24 square inches. The cover of the box was about six feet in length, and upon it were placed slips of smoked glass. Firmly fixed above the glass, to an independent support, was a verified tuning fork of 435 complete vibrations per second, carrying a style which lightly touched the glass surface beneath it. The weight box was supported in position at the upper end of the inclined plane by a cord fastened to a screw which served to give the box a very slow upward motion. At the proper time the screw was turned, the fork vibrated, the cord cut or burned off, and the box allowed to slide to the bottom of the plane. The style of the fork at the same time would trace upon the smoked glass a waved line, which would be a perfect autographic register of the experiment. The time of sliding, the velocity at any point, the distance passed over in any unit of time, could all be measured or counted directly from the smoked glass.

The graphical method of working up the experiment was employed, as follows: The bottom of a sheet of section paper was made a "time line" ($\frac{1}{435}$ of a sec.= a unit). At various points on this line the corresponding velocities were erected as ordinates. The equation of a line connecting the upper extremities of these ordinates would express the law of the motion studied.

It is evident that this line would have been straight if the acceleration of the slide had been uniform, like that of a body falling in vacuo. If, however, a variable resistance be opposed to the motion of the slide, the acceleration will no longer be uniform, and the line will become curved, concave toward the axis of abscissas, if the resistance is increasing, convex if the resistance diminishes. The acceleration of such a motion at any time will be proportional to the tangent of the angle which the direction of the curve at that point makes with the time line. It is also evident that such acceleration may at once be measured from the paper, since it is the difference between the velocities for two successive units of time. The curve constructed as above, from every experiment made, was decidedly convex toward the time line, showing a constantly decreasing resistance to the motion of the slide as the velocity increased. If we assume that this increase in acceleration was due to a diminished coefficient of friction, the value of the coefficient for any time may be found in the following manner:

Let a, b, and h= the altitude, base, and length of the inclined plane.

W= weight of the slide and contents.

W'= normal pressure on the plane, $= W.\frac{b}{h}$.

g= acceleration of a body falling freely.

g'= theoretical acceleration of the slide $=g.\frac{a}{h}$.

g''= the observed acceleration at any time.

Then the resistance of friction $=F=\frac{W}{g}(g'-g'')$, and the coefficient of friction $=\varphi=\frac{F}{W'}=\frac{g'-g''}{g}.\frac{h}{b}=\left(\frac{a}{h}-\frac{g''}{g}\right)\frac{h}{b}=\frac{a}{b}-\frac{g''h}{g\,b}=$ tangent of inclination $-\frac{g''h}{g\,b}$.

The following tables give the results obtained from a series of four experiments. The load in every case was 40 lbs. The inclinations of the plane were as follows: No. 1 =15° 6′, No. 2 =16° 9′, No. 3 =17° 5′, No. 4 =18° 9′.

Table A shows the accelerations corresponding to different velocities in the four experiments. The units used are the $\frac{1}{1000}$ of an inch and the $\frac{1}{435}$ of a second.

TABLE A.

Velocities.	Accelerations.			
	Expt. 1.	2.	3.	4.
4	·020	·033	----	----
10	·035	·056	·073	·092
15	·044	·070	·090	·112
20	·053	·081	·103	·129
25	·059	.083	·112	·140
30	·065	·094	·120	·150
40	·073	·105	·131	·165
50	·078	·112	·140	·176
60	·083	·117	·148	·184
70	·087	·121	·156	·190
80	·091	·125	·159	·196
90	·093	·128	·163	·200
100	·095	·131	·168	·203
110	----	·133	·171	·206
120	----	·136	·175	----

Table B shows the coefficients of friction in each experiment, deduced by substituting the observed accelerations given in Table A in the formula given above. The observed accelerations were of course reduced to feet in a second.

TABLE B.

Velocities.	Coefficients of friction.			
	Expt. 1.	2.	3.	4.
4	·260	·273	----	----
10	·252	·261	·270	·280
15	·245	·254	·261	·270
20	·243	·248	·254	·260
25	·240	.245	·250	·255
30	·237	·242	·246	·250
40	·233	·236	·240	·242
50	·230	·232	·235	·236
60	·228	·230	·231	·232
70	·226	·228	·227	·231
80	·224	·226	·226	·225
90	·223	·224	·224	·223
100	·222	·223	·221	·222
110	----	·222	·220	·220
120	----	·220	·217	----

From the tables it will be observed: 1st. That with a given inclination of the plane, the coefficient of friction decreases as the velocity increases, rapidly at first but more slowly afterward. 2d. With the same velocity, the coefficient of friction is greater the greater the inclination of the plane, within the limits of the experiments. 3d. The coefficient of friction in each experiment tends toward a constant quantity. 4th. This constant seems to be the same in each experiment.

No simple expression which will show the variations in the coefficient of friction has yet been found; indeed, I have not thought best to attempt to formulate the work till certain errors, which will be referred to, have been corrected. It was found impossible to procure a plank with a perfectly uniform surface. The one used in the experiments given showed at the same inclination and velocity a coefficient which slightly but regularly increased from one end to the other. The end which gave the lower coefficient was placed uppermost. The obvious result of this was to make the coefficients in Table B at high velocities greater than they otherwise would have been. This fact also explains the apparent anomaly in columns 3 and 4 of the same table, where the coefficients at high velocities are seen to fall below the corresponding coefficients in column 2.

In experiment 4 the slide had the velocity 120 at a distance of 40 inches from the upper end of the plane; in experiment 2 it did not acquire that velocity until it had passed over a distance of 60 inches, and consequently was on a rougher portion of the plane. The uniformity of the plane was tested by starting the slide at different points along its length, and comparing the curves on the smoked glass. These experiments have not been corrected for the resistance of the atmosphere. The effect of such a correction would be to diminish still more the coefficients at high velocities.

As the inclination of the plane increases the normal pressure decreases. Thinking that this change of pressure might explain a part of the difference due to a change of inclinations, we made three experiments at the same inclination, with weights of 18, 80 and 140 lbs., in the box. At the end of one second we found the velocities in the three cases to be as 1, 1·18 and 1·32, showing a less resistance in the case of the greater load, and corresponding to a decrease of about 2¼ per cent in the coefficient of friction. This seems to be insufficient to explain the change in the coefficient when the inclination of the plane is changed. But it is interesting as showing that in the case of pine on pine friction is not strictly proportional to the normal pressure.

As soon as possible we propose to repeat these experiments, extending the range of velocities, also to try the effect of a change of pressure, with a view to formulate deviations from the received laws, if simple expressions can be found. We have also designed a modification of apparatus to test our results when a uniform motion is given to the slide.

The experiments in the series (nearly 100 in number) and a greater part of the computations have been very carefully made by Messrs. Butterfield and Wilson, students in the department of Physics.

ART. XXIII.—*On the constitutional formulæ of Urea, Uric Acid, and their derivatives;* by Professor J. W. MALLET, University of Virginia.

FEW classes of organic compounds have given rise to more difference of opinion amongst chemists than that which includes urea and its conjugates.

The remarkable number of such compounds, their complicated relationships, the varied circumstances of their production and decomposition, and their variety of chemical character, have led to nearly every one of them being viewed in several different lights, and represented by several different formulæ, by those who have given the subject special attention.

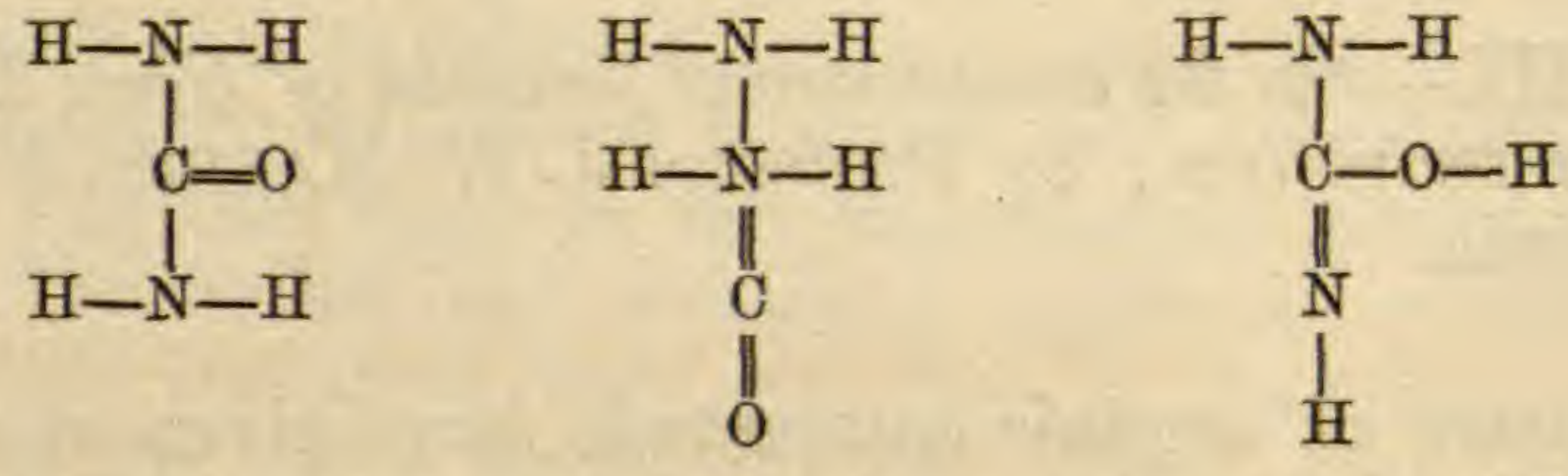
The structure of the simple molecule of urea itself is by no means settled. The arguments of Heintz* and Kolbe† in favor of the view that urea is identical with carbamide ($H_2N—CO—NH_2$) have been opposed by the observation of Wanklyn and Gamgee‡ as to the behavior of urea (unlike that of admitted amides) when oxidized by an alkaline solution of potassium per-manganate. The latter chemists proposed the formula $C\left\{\begin{matrix}(NH)''\\ NH_2\\ OH\end{matrix}\right.$, but, as Watts remarks in his Dictionary of Chemistry,§ without assigning specific reasons (other than the difference of behavior just noted) for adopting this instead of the carbamide formula which they reject. Wolcott Gibbs‖ independently put forward the same view, but *did* give some of the grounds upon which it was adopted by him. It has also been proposed to represent urea as $O=C=NH_2—NH_2$, in which formula one of the nitrogen atoms is pentad. Most recent writers of text-books, however, as Fittig¶ and Naquet,** seem to have fallen back upon the view that urea is simply carb-

* Ann. der Chem. u. Pharm., cxl, 276; cl, 73. † Zeitschr. für Chem., II, iii, 50.
‡ Jour. Chem. Soc., Jan., 1868, 31. § 1st Suppl., 1115.
‖ Amer. Jour. Sci., II, xlvi, 290, Nov., 1868.
¶ Wöhler's Grundriss der org. Chem., 8te. Aufl., 206.
** Principes de Chimie, troisème éd., t. ii, 532–533.

amide. Bunte* has suggested as the means of deciding between these opinions the determination of the maximum number of isomeric products obtainable from the body in question by substituting an alcoholic radical for hydrogen.

The most obvious point of difference between the three formulæ above mentioned consists in the first being symmetrical while the others are not so. If urea be carbonic diamide (1), viz:

1. 2. 3.



the two nitrogen atoms are placed exactly alike, and so are the four atoms of hydrogen. If the third formula (2) be adopted, the two nitrogen atoms will be unlike, while there will be two pairs of hydrogen atoms with no difference between the members of each pair. But if the formula (3) be the true one, the two nitrogen atoms, while exhibiting the same atomicity, are dissimilarly connected, and hydrogen is found in three different relations to the rest of the molecule, only two atoms of the latter element being quite alike in position. Clearly we should the rather expect from this highly unsymmetrical disposition of the atoms such a number and variety of substituted and conjugated products as urea actually affords.

But not only does this last view enable us easily to account for the large class of derivatives furnished by the substance in question, but it seems to lend itself remarkably well to the explanation of the special character which these derivatives severally exhibit, whereas many of the formulæ hitherto proposed for the "ureides" differ much from those of other bodies of the same type, the acid or basic character, degree of basicity, etc., not being satisfactorily accounted for. In this respect Wanklyn, Gamgee and Gibbs seem scarcely to have done justice to the merits of the formula they suggested, and I propose by a few examples of the better known substances related to urea to illustrate the advantages of assuming for it this molecular constitution. In doing so I have to suggest a structural composition for most of the conjugated bodies spoken of unlike that which Gibbs has adopted in the paper above referred to. It will conduce somewhat to clearness to use fully expanded graphic formulæ, and for the conjugated compounds

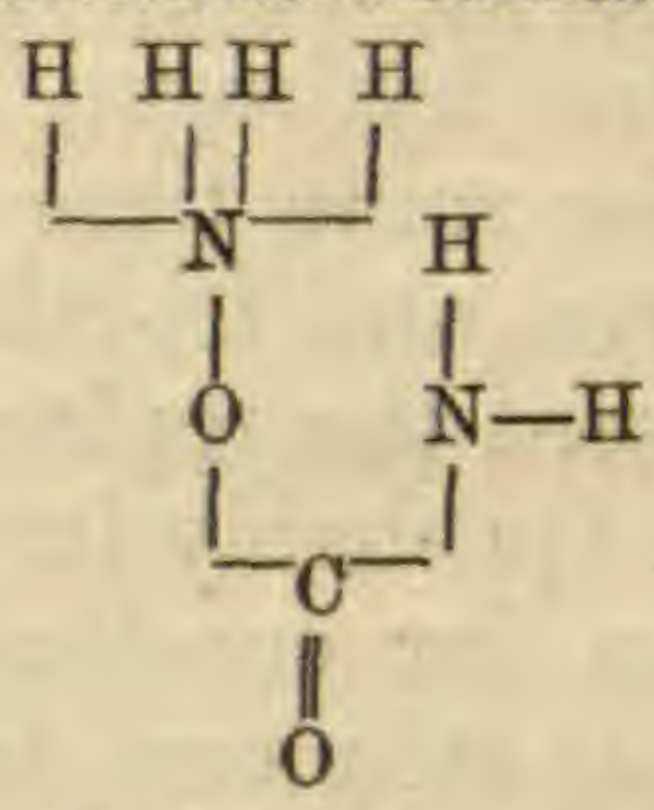
* Ann. der Chem. u. Pharm., cli, 184.

to use dotted lines to represent the bonds connecting residues derived from different molecules.*

Let then urea be represented by the last formula, No. 3, derivable from ammonium cyanate

1. Ammonium cyanate.

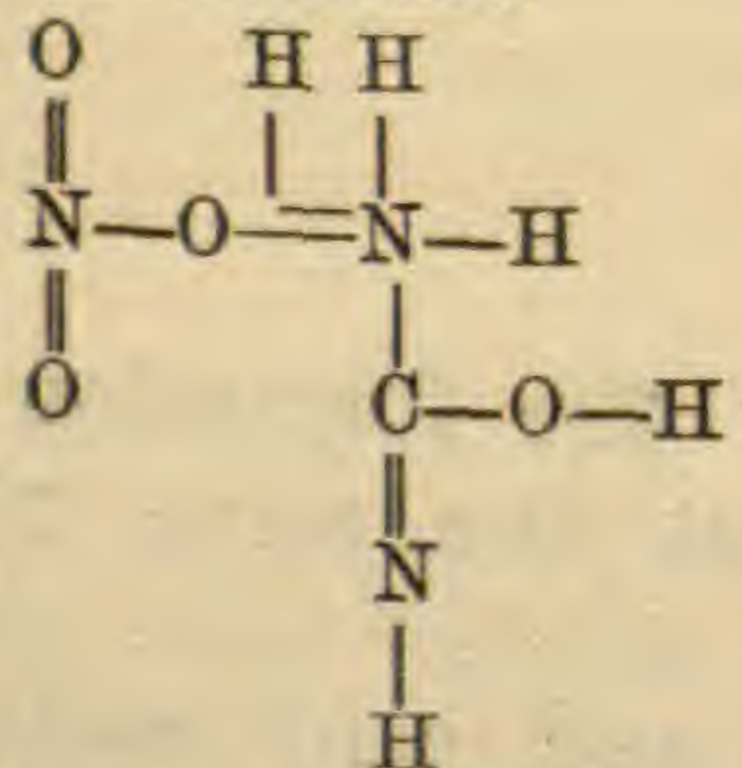
2. Ammonium carbamate.



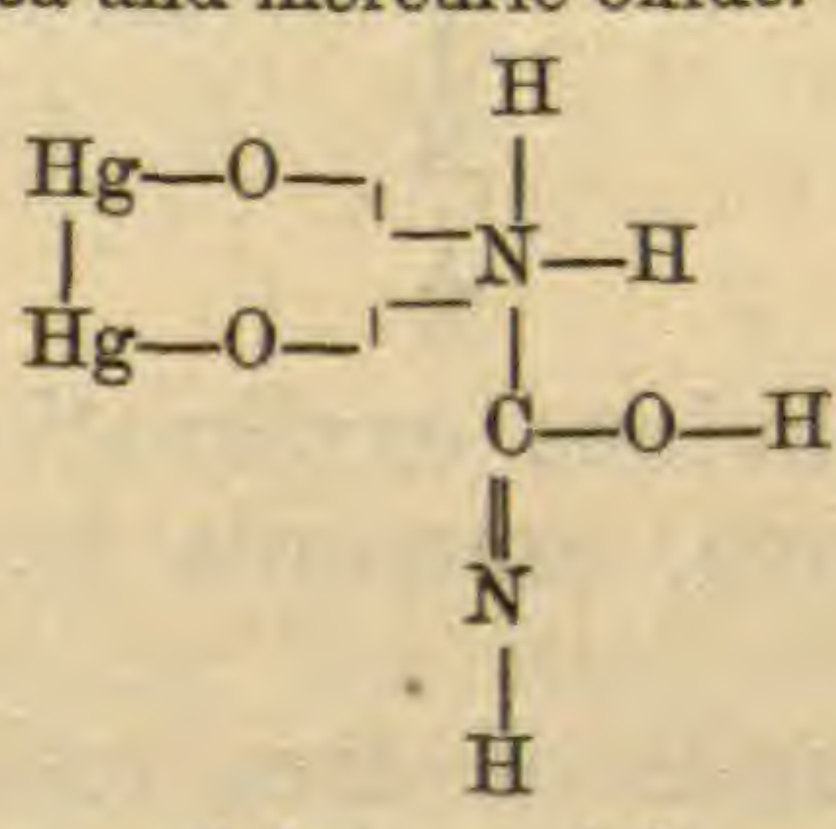
by intermolecular transposition, and from ammonium carbamate by change of the same sort with elimination of water. In both cases one of the two nitrogen atoms shows the usual tendency to revert from pentad to triad character by elevation of temperature.

The *direct compounds* of urea (like those of ammonia) with acids involve a re-assumption of pentad relation by this one atom of nitrogen; not by both, as we might expect if they were alike in position in the molecule; as, for instance, in the case of urea nitrate.

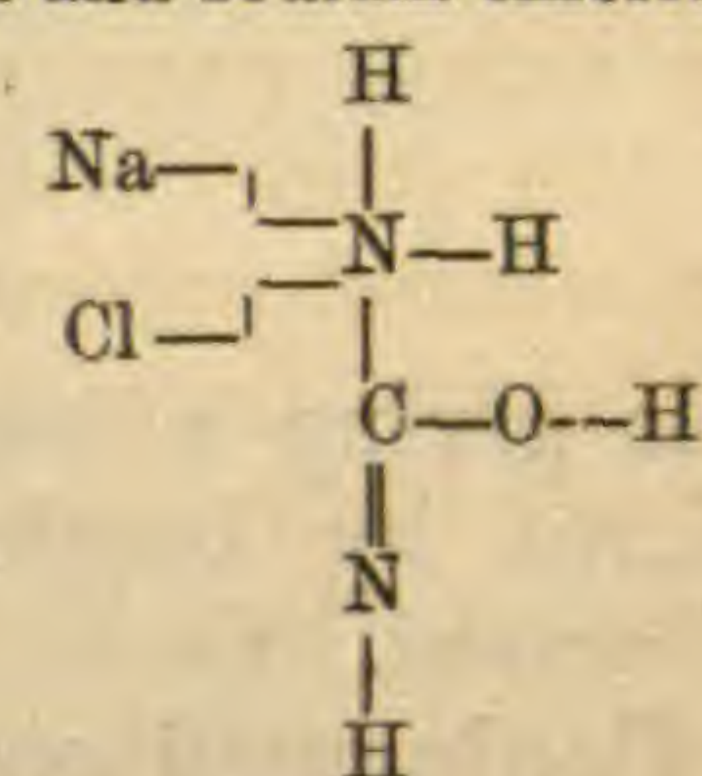
Urea nitrate.



Urea and mercuric oxide.

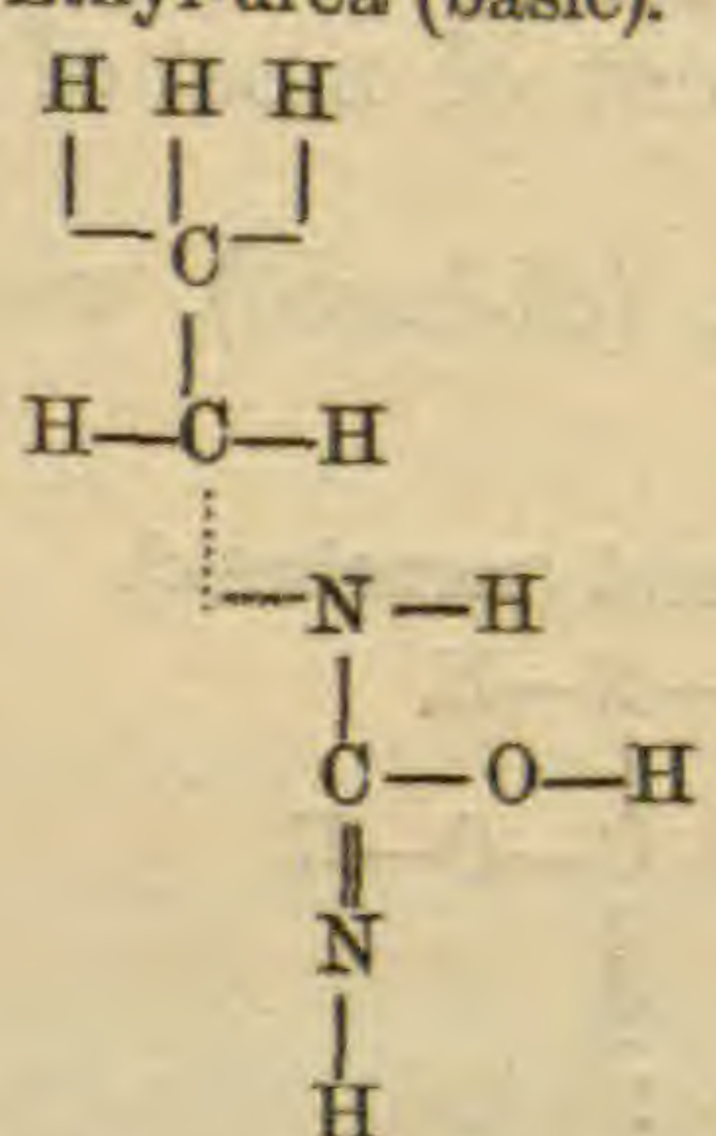


Urea and sodium chloride.

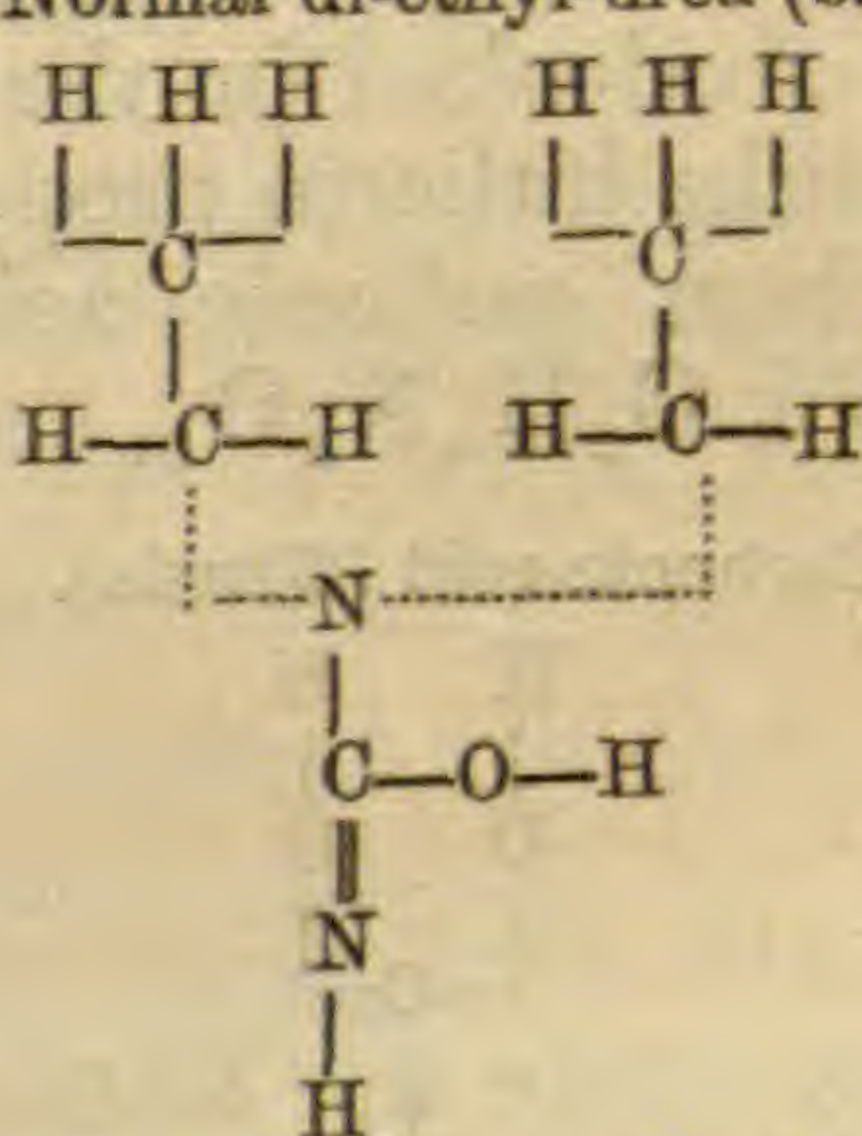


Similar relations are entered into with some metallic oxides, as for instance, mercuric oxide, and certain salts, as with sodium chloride.

1. Ethyl-urea (basic).



2. Normal di-ethyl-urea (basic).



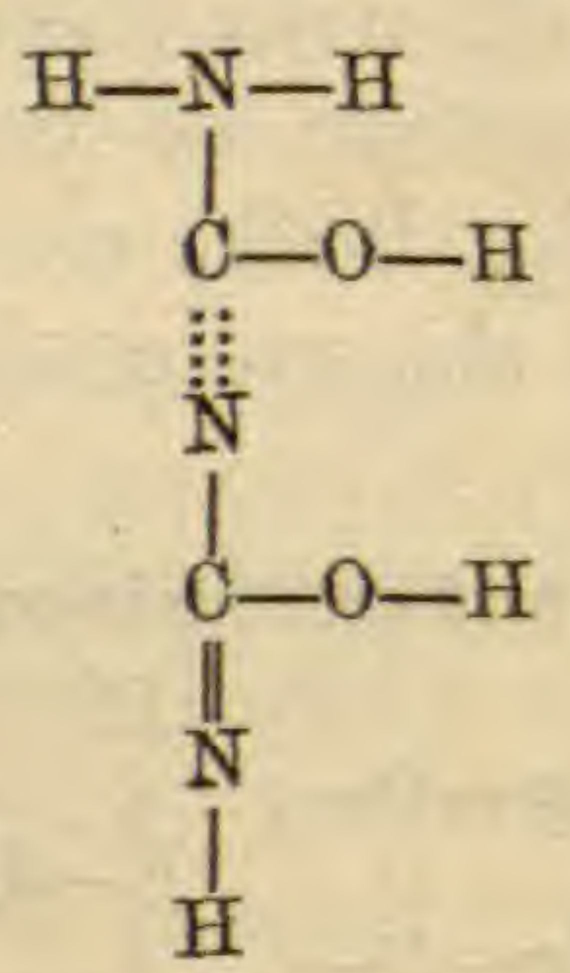
* Some of these graphic formulæ would look rather simpler and better were it not for difficulty on the part of the printer in using *oblique* lines with movable type.

Products of substitution by alcoholic radicals, *in which the urea type and character are preserved*, are exemplified by ethyl-urea, $C_3H_8N_2O$, in which one of the two similarly related hydrogen atoms is replaced, and by normal di-ethyl-urea, $C_5H_{12}N_2O$, in which the replacement extends to both of these atoms. One hydrogen atom of this pair and another (unlike) atom at the same time are probably replaced in the isomeric di-ethyl-urea, and tri-ethyl-urea, if this compound really exist, will represent the replacement of both the similar and one unsymmetric (probably imide) atom of hydrogen.

Formulæ for condensed ureas containing polyatomic radicals, such as ethylene, follow easily enough from the above.

In biuret, $C_2H_5N_3O_2$, we may suppose the residues of two urea molecules united with elimination of ammonia from unlike (amidic and imidic) extremities of the chain of atoms—thus,

Biuret (feebly basic, uniting (by its amidic end probably) with one equivalent of HCl).

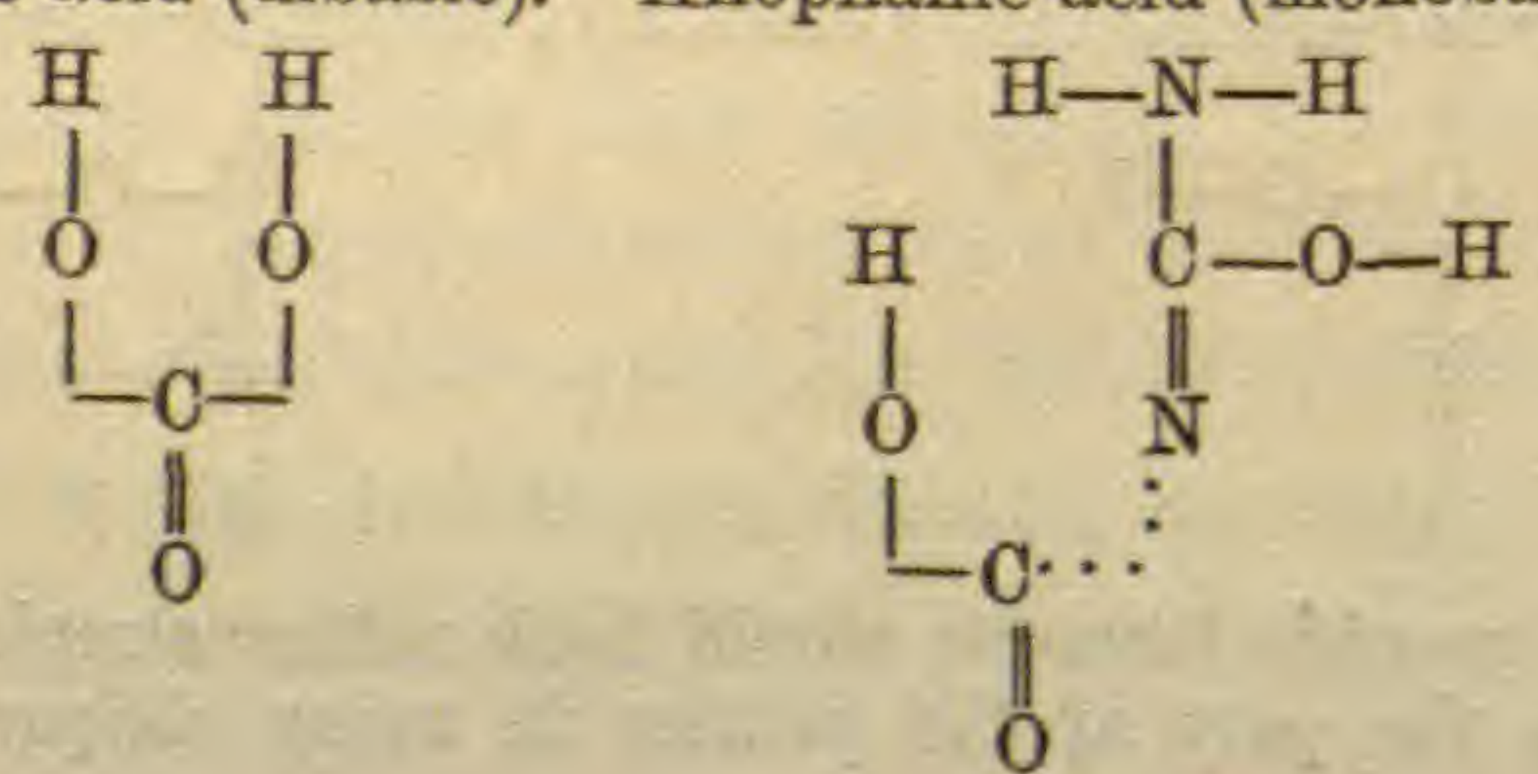


this giving a very obvious explanation of the formation of biuret by heating urea, ammonia being at the same time liberated.

Conjugated compounds of urea residues and acid radicals form a more numerous class. In these the type of the original acid seems usually to predominate, but *the urea residue modifies the character of the substance in different ways according to the mode of attachment.* This last point seems to have been the chief one overlooked in the arrangement of most of the structural formulæ hitherto proposed.

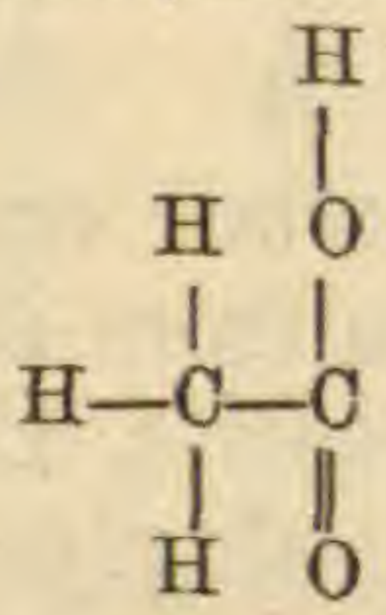
Thus, from carbonic acid, CH_2O_3 (di-basic), we get allophanic acid, $C_2H_4N_2O_3$,

Carbonic acid (dibasic). Allophanic acid (monobasic).

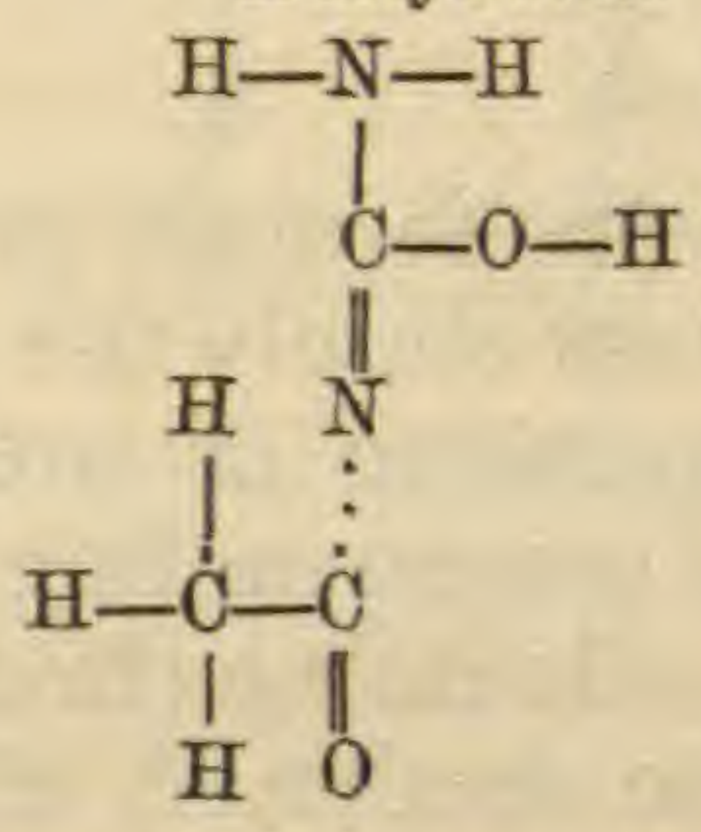


only one of the two basic hydrogen atoms present in the original acid remaining in place. A comparison of allophanic acid thus viewed with the above formula for biuret will show how the latter is produced by the action of ammonia upon ethyl allophanate. From acetic acid, $C_2H_4O_2$, we have acetyl-urea, $C_3H_6N_2O_2$ (exhibiting neither acid nor basic character),

Acetic acid (monobasic).

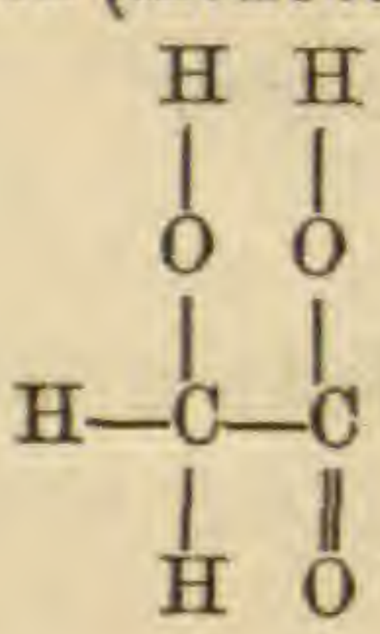


Acetyl-urea.

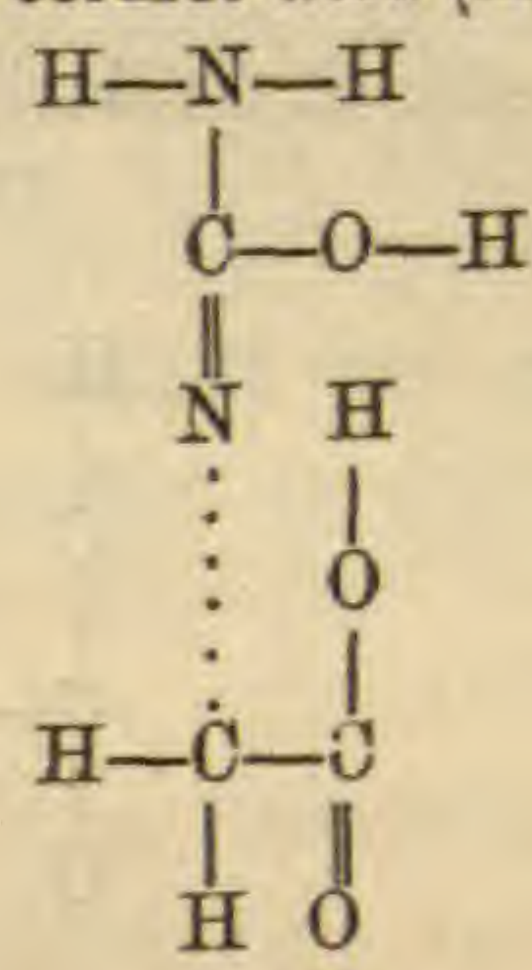


the one basic atom of hydrogen of the original acid having been replaced by the urea residue at its imide extremity. From glycollic acid, $C_2H_4O_3$, we have glycoluric (hydantoic) acid, $C_3H_6N_2O_3$.

Glycollic acid (monobasic).

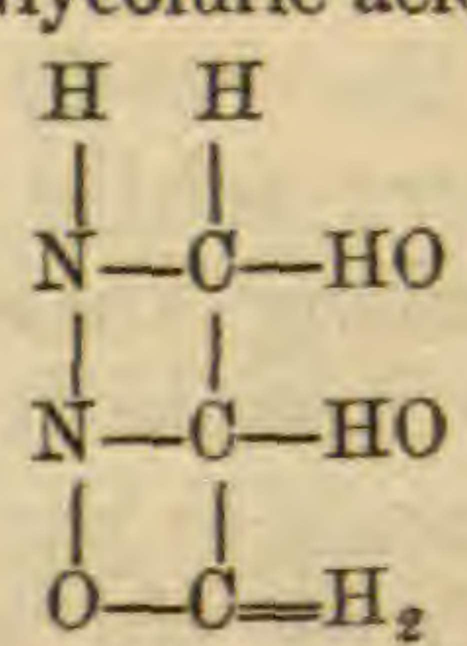


Glycoluric acid (monobasic).



the urea residue replacing the methylic hydroxyl, while the original oxatyl remains unaffected and the acid character is preserved. In the formula for glycoluric acid proposed by Gibbs, viz: (misprinted in his paper in the transposition of the — and = in the bottom line):

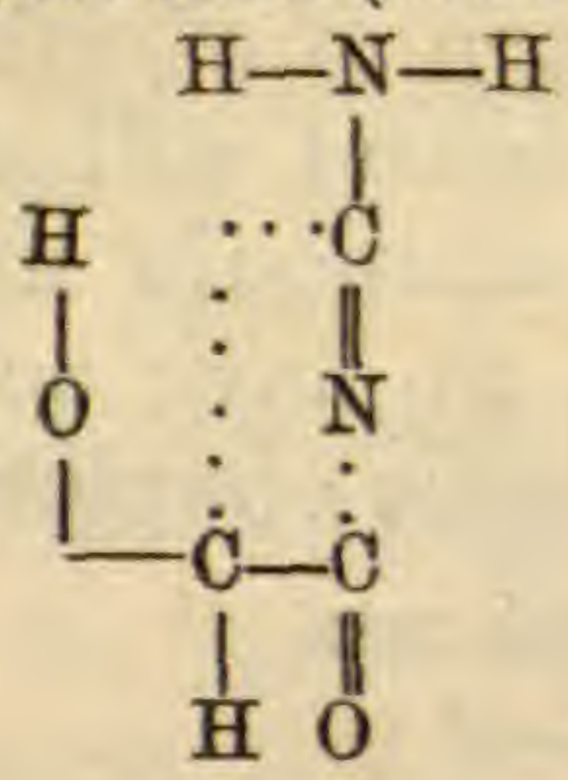
Glycoluric acid.



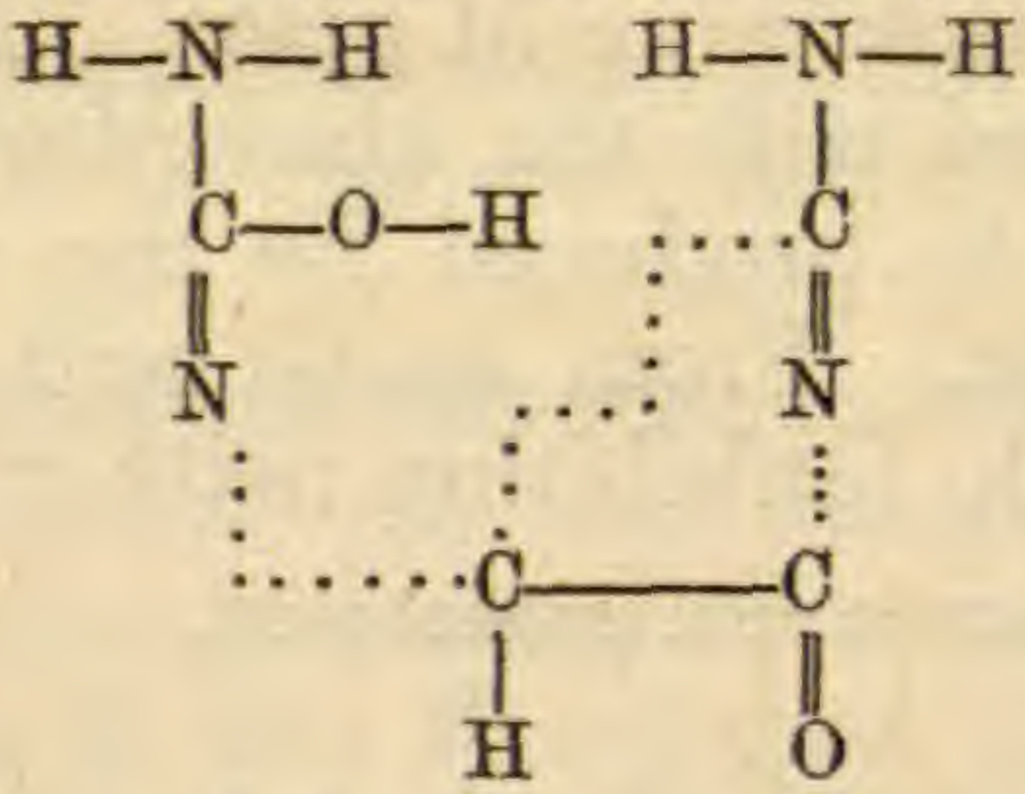
the mono-basic character is not obvious, nor does it readily appear which is the replaceable (basic) atom of hydrogen.

If glycollic acid be differently conjugated with urea, the residue of the latter attaching itself in place of the *basic* hydroxyl, and a molecule of water being eliminated, we get hydantoine, $C_3H_4N_2O_2$ (1).

1. Hydantoine (neutral).

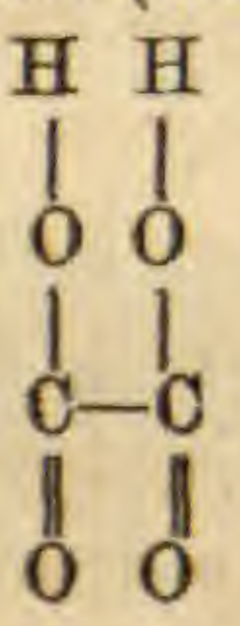


2. Glycolurile (neutral).

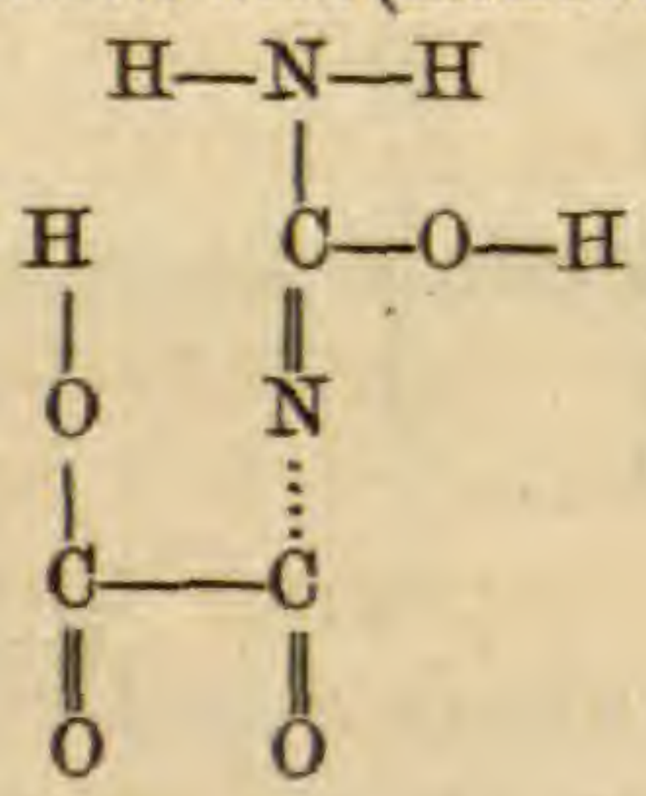


By reference to the formula proposed further on for allantoine it will be seen how simply the production from it of hydantoine occurs with separation of urea. Glycolurile, $C_4H_6N_4O_2$, appears (2) as the corresponding di-ureide of glycollic acid, and its relation to allantoine (from which it is producible by the action of sodium amalgam) will be easily seen by reference to the formula for the latter, although I prefer to view these bodies as derivatives of two different acid nuclei. The breaking up of glycolurile by an acid in the presence of water into hydantoine and urea is easily traced. From oxalic acid, $C_2H_2O_4$, we get oxaluric acid,* $C_3H_4N_2O_4$,

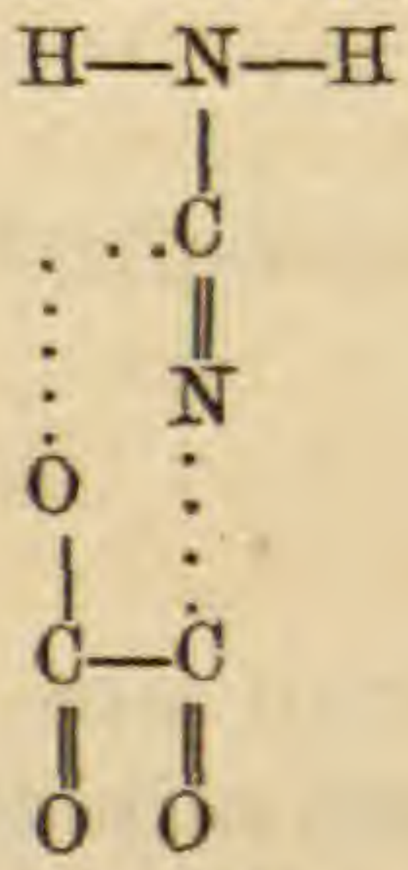
1. Oxalic acid (dibasic).



2. Oxaluric acid (monobasic).



3. Paraban.



easily breaking up in presence of water into oxalic aci(..nd urea.

The absence of well determined acid character in paraban (parabanic acid), $C_3H_2N_2O_3$, justifies the above formula, No. 3, which well explains Ponomareff's† synthesis of this body by the action of phosphorus tri-chloride on a mixture of oxalic acid and urea, as well as the ready conversion into oxaluric acid by assumption of water; while the so-called metallic salts formed by this body (including those described by Menschutkin),‡ remarkable for their instability, may probably represent merely the substitution of amidic hydrogen in the urea residue, as cholestrophane results from the substitution of both these hydrogen atoms by methyl. It will be seen presently that this formula makes paraban bear exactly the same relation to oxalic acid that alloxan does to mesoxalic acid.

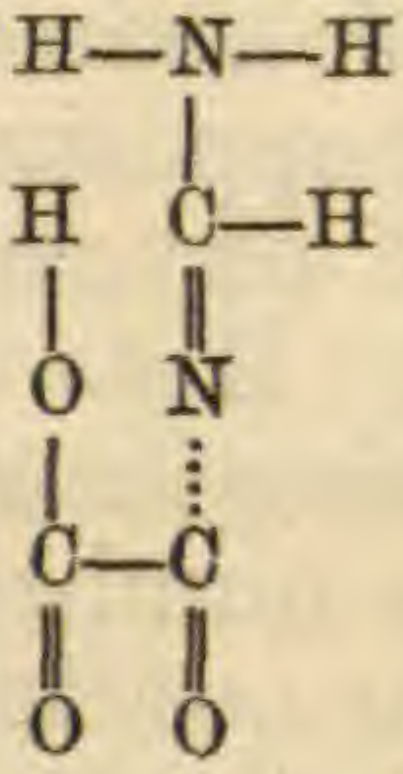
* There is some misprint in the formula for this acid in the memoir of Gibbs (loc. cit., p. 292), since it contains an atom of oxygen too much.

† Bull. Soc. Chim. de Paris, II, xviii, 97.

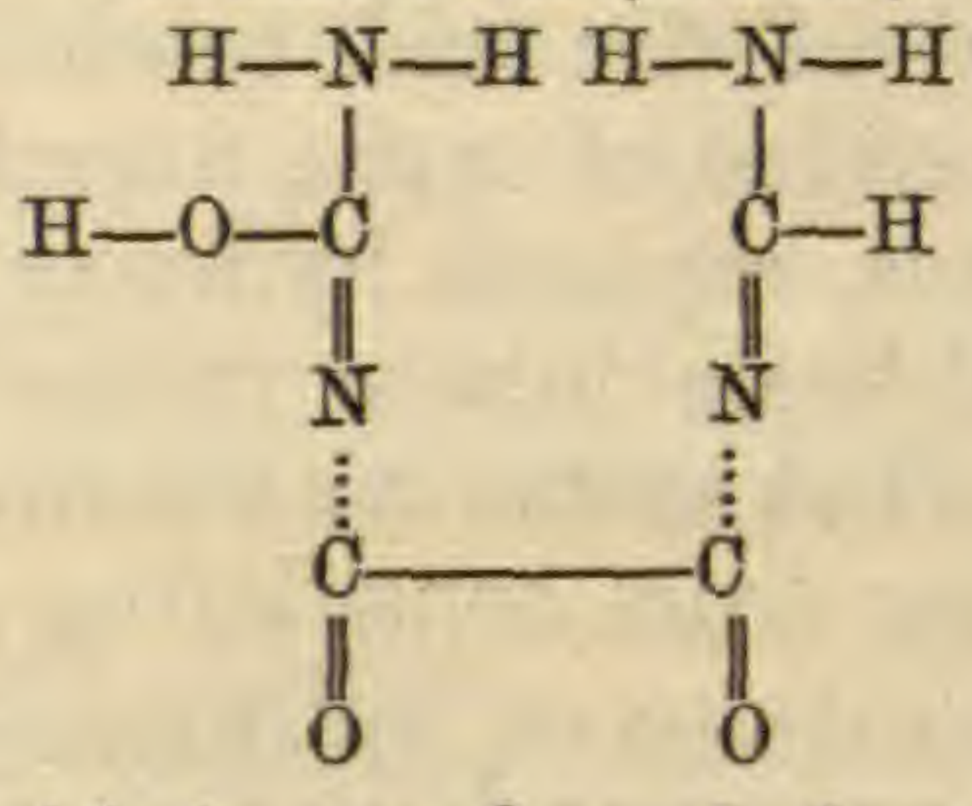
‡ Ann. der Chem., clxxii, 73.

If in the ureic residue of oxaluric acid we substitute hydrogen for hydroxyl, we get the formula for allanturic (lantanuric) acid, $C_3H_4N_2O_3$,

1. Allanturic acid (monobasic).

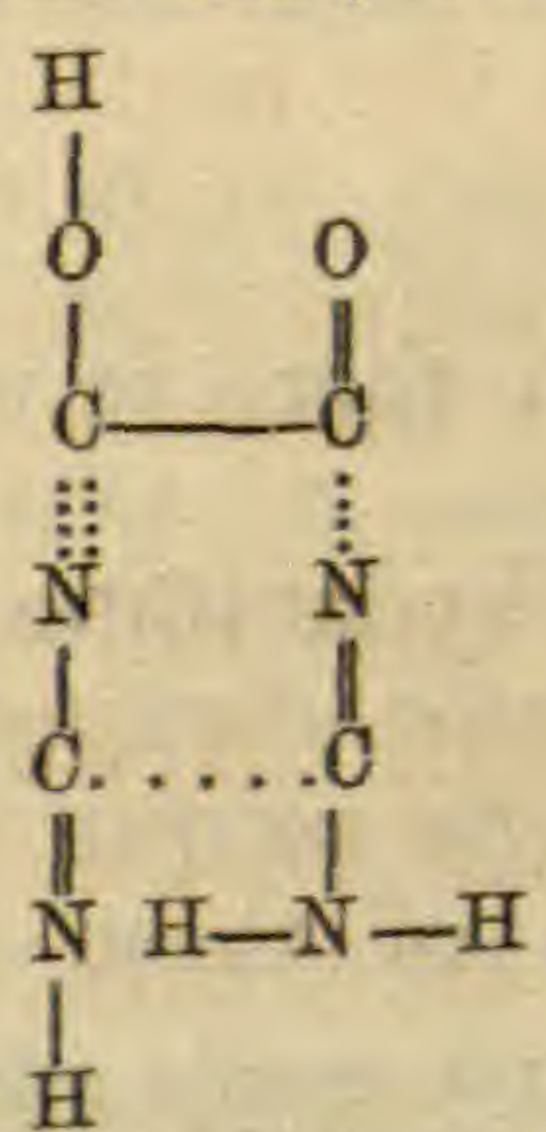


2. Allantoine (neutral).



and the mode in which this acid is produced from allantoine by assimilation of water and separation of urea becomes evident if we give to allantoine, $C_4H_6N_4O_3$, the following formula (2), viewing it as a di-ureide derivative of oxalic acid, with which it appears in fact to be closely connected, producing oxalates by heating with alkaline solutions, by fermentation with yeast, &c. By reference to the formula proposed further on for uric acid it will appear how this latter yields allantoine on boiling an aqueous solution with lead dioxide, the middle carbon atom of the mesoxalic acid residue being removed as carbon dioxide, and hydroxyl and hydrogen respectively taken up from a molecule of water by the two ureic residues, which at the same time assume a different mode of attachment to the oxalic acid nucleus; the further action of an excess of lead dioxide decomposing the allantoine itself, with formation of urea and lead oxalate. The formula of Gibbs for allantoine (as for hydantoine and glycolurile) would lead us to expect an acid character, whereas such compounds as are formed by this body with metals and metallic oxides manifestly are of the same order as those produced with similar substances by urea itself. In the other di-ureide, viz: mycomelic acid, $C_4H_4N_4O_2$,

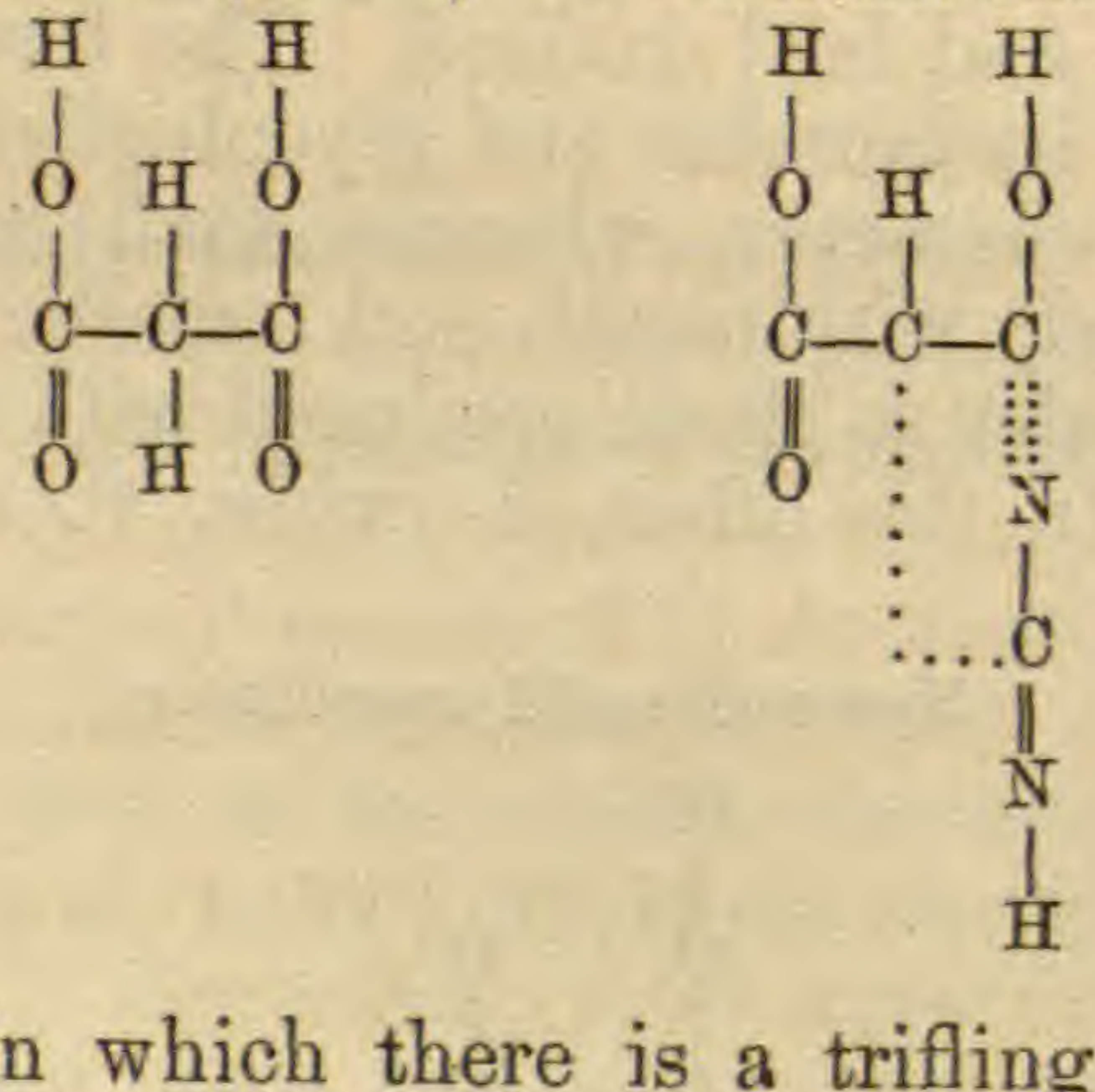
Mycomelic acid (monobasic).



we have an example of what I agree with Professor Gibbs in assuming as very probable, namely, the similarity of function of nitrogen with two free bonds to the outside oxygen in oxatyl,* so that I represent one of the two such oxygen atoms in oxalic acid as replaced by a urea residue connecting itself by its amidic extremity, the hydrogen of the corresponding hydroxyl of the acid retaining its basic character; while the second residue of urea, attached by the opposite end of its chain of atoms, replaces hydroxyl instead of oxygen, and thus changes a di-basic into a mono-basic acid. Here we have an instance of what seems to me the error arising in many of the older formulas from considering merely the number of atoms, with their additions or subtractions, without noticing the character of the compounds in question as an indication of molecular structure. Odling† says of mycomelic acid that it bears "exactly the same relation to oxalic acid that uric has to mesoxalic acid." So it does, in so far as the summation of the atoms present is concerned, but the two last named acids are both di-basic, while oxalic and mycomelic acids are di-basic and mono-basic respectively. It will be seen presently that the formulæ I propose account fully for this, the two urea residues in uric acid being similarly connected with the residue of the original acid, while in mycomelic acid they are connected by what I have called the amidic and imidic ends respectively.

Passing to the 3-carbon acids, from malonic acid, $C_3H_4O_4$, No. 1, may be derived barbituric acid, $C_4H_4N_2O_3$, No. 2.

1. Malonic acid (dibasic). 2. Barbituric acid (dibasic).



Gibbs' formula (in which there is a trifling misprint) would imply a mono-basic acid.

Naquet* speaks of "l'hydantöine, qui représente de l'acide allanturique moins un atome d'oxygène, et qui est, par conséquent, à l'acide allanturique ce que l'acide barbiturique est à l'acide dialurique." But of these two pairs of substances,

* As in the polymerides of true cyanic acid. Prof. Gibbs proposes to call (CNOH)″ cyanyl, as analogous to (COOH)′, oxatyl.

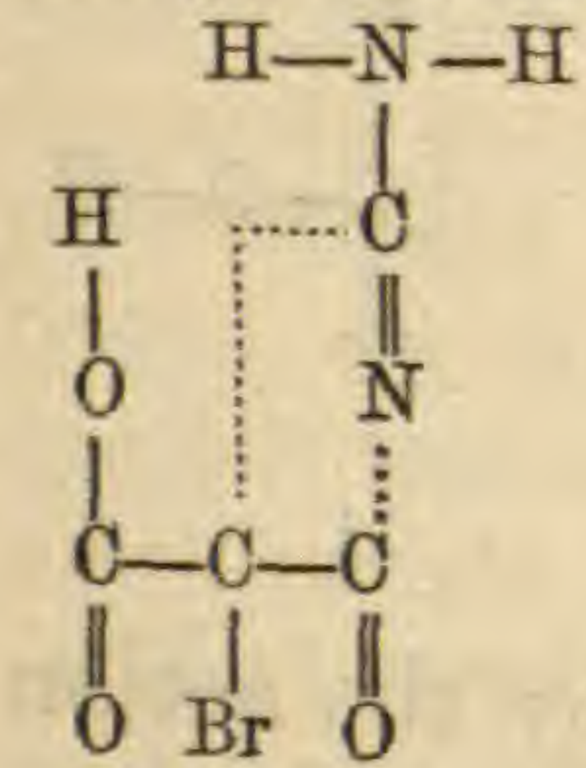
† Lectures on Animal Chemistry, London, 1866, p. 132.

‡ Principes de Chimie (1875), ii, 578.

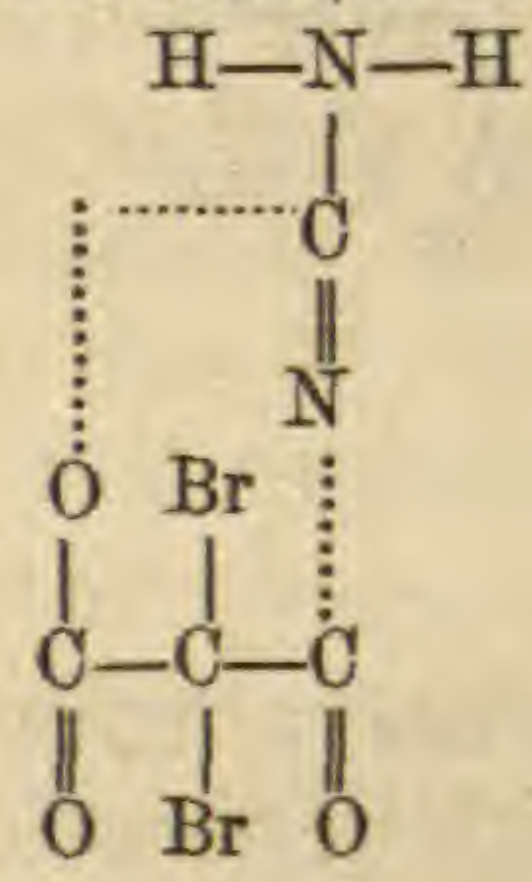
hydantoine is neutral, and allanturic acid a mono-basic acid, while barbituric and dialuric acids are di-basic and mono-basic respectively. The formulæ proposed in this paper furnish an explanation of the difference.

Any formula I have seen for bromo-barbituric acid, $C_4H_3BrN_2O_3$, would lead one to expect for it exactly the same degree of basicity as that of barbituric acid. But the following (No. 1) with the urea residue oppositely attached) will show how the former acid is mono-basic, while the latter is di-basic.

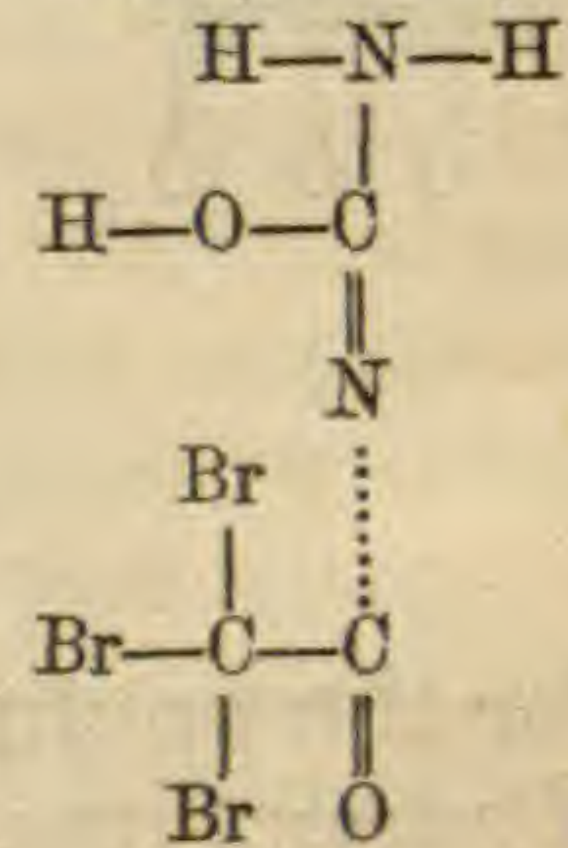
1. Bromo-barbituric acid (mono-basic).



2. Brom-alloxan or di-bromo-barbituric acid (non-acid).



3. Tri-brom-acetyl-urea (neutral).

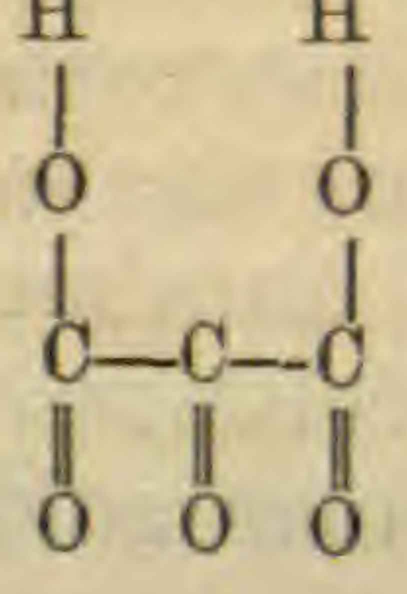


A further replacement of hydrogen by bromine gives us what has been called di-bromo-barbituric acid, $C_4H_2Br_2N_2O_3$, a body which is, however, really devoid of acid character, not forming salts. With the formula now proposed (No. 2, above), this non-acid character becomes intelligible, and the name brom-alloxan, originally employed by Baeyer, becomes fully justified on comparison with alloxan as represented further on. The conversion of this body into dialuric acid by the action of hydro-sulphuric acid in the presence of water is explained by the formula for dialuric acid given further on.

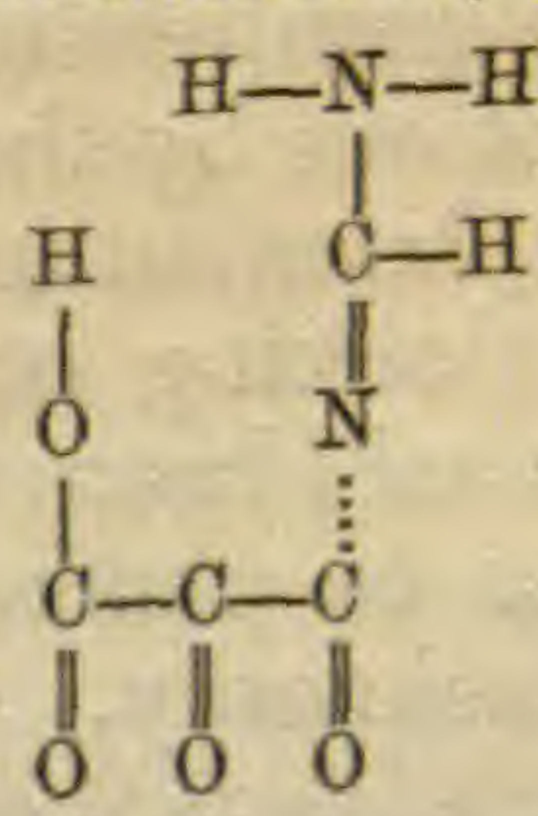
On pushing the action of bromine still further, brom-alloxan is converted, with separation of carbon dioxide, into tri-brom-acetyl-urea, $C_3H_3Br_3N_2O_2$ (from the 2-carbon acid residue), the formula of which (No. 3, above) is very simply derived from No. 2, and brings us back to that of acetyl-urea as already given.

From mesoxalic acid, $C_3H_2O_5$ (No. 1), we get the acid mon-ureide dialuric acid, $C_4H_4N_2O_4$ (No. 2),

1. Mesoxalic acid (di-basic).



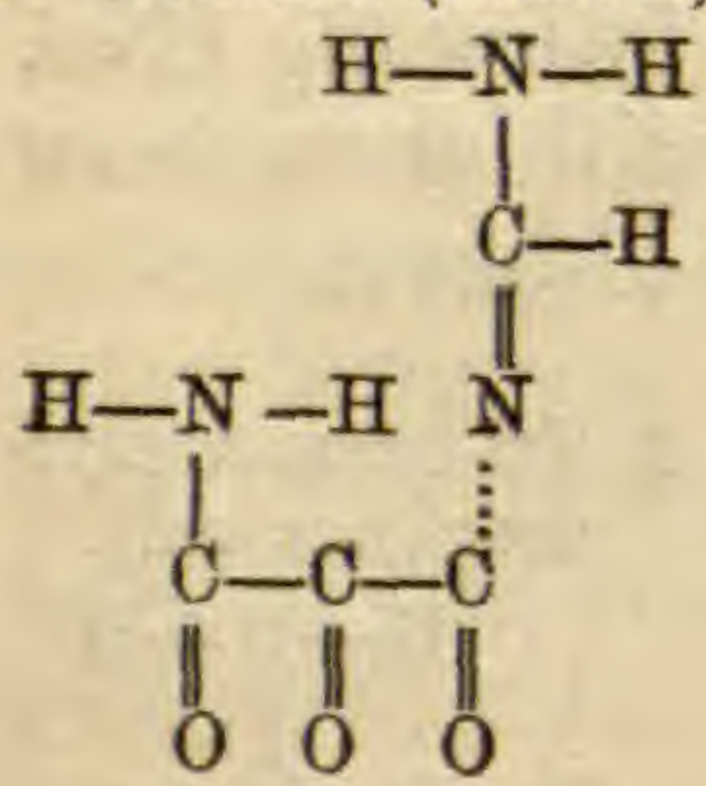
2. Dialuric acid (mono-basic).



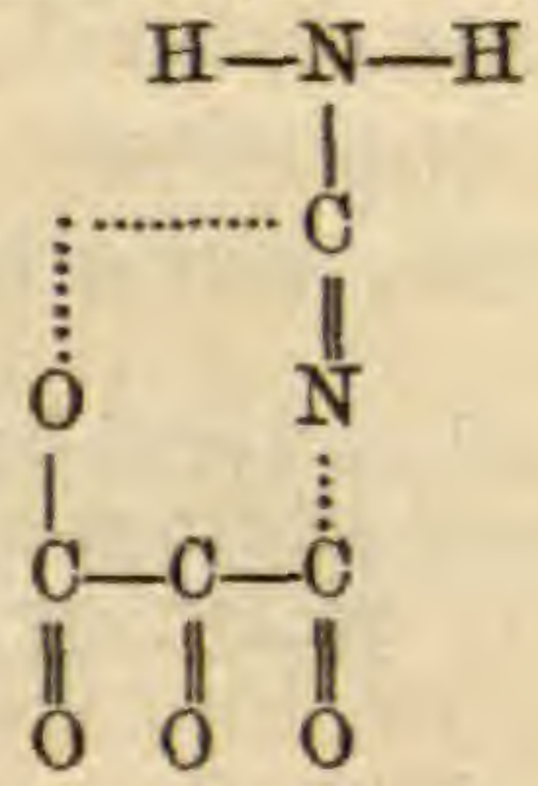
in which the ureic hydroxyl is replaced by hydrogen. The production of this acid by hydrogenation of alloxan will be seen presently to be readily intelligible.

And from dialuric acid is derived the amide, uramile (dialuramide), $C_4H_5N_3O_3$ (No. 1).

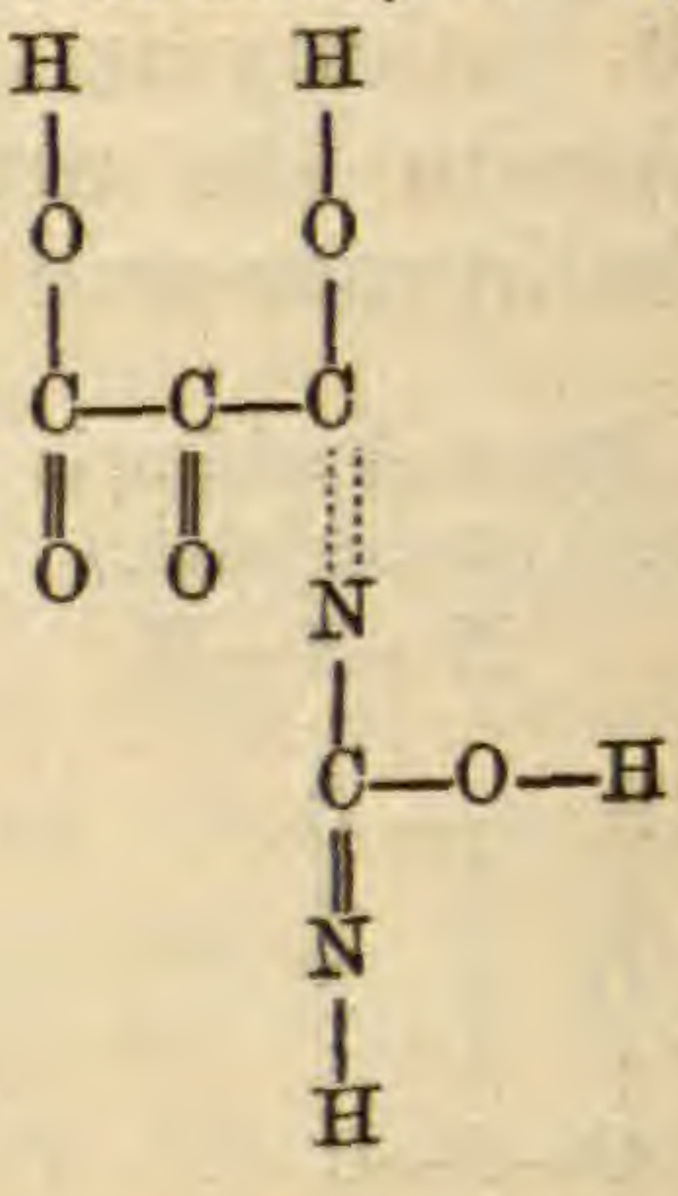
1. Uramile (neutral).



2. Alloxan (neutral).



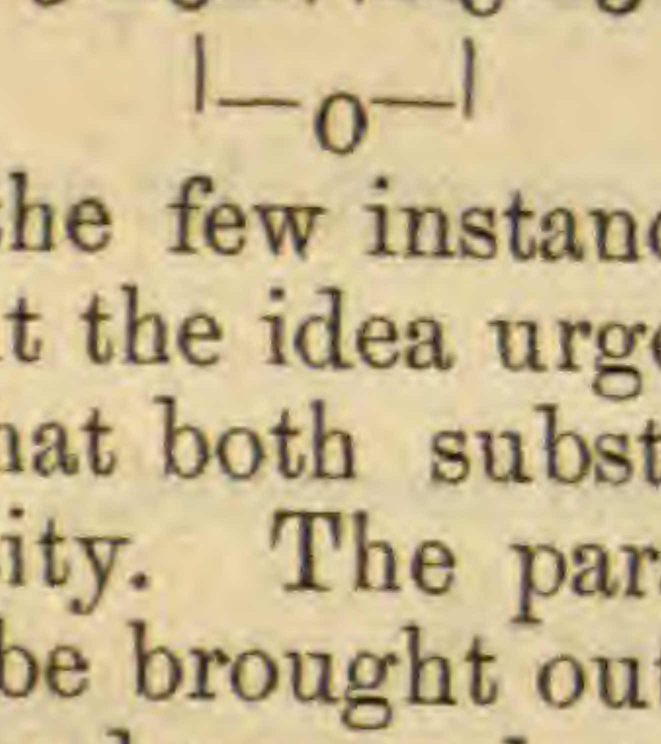
3. Alloxanic acid (di-basic).



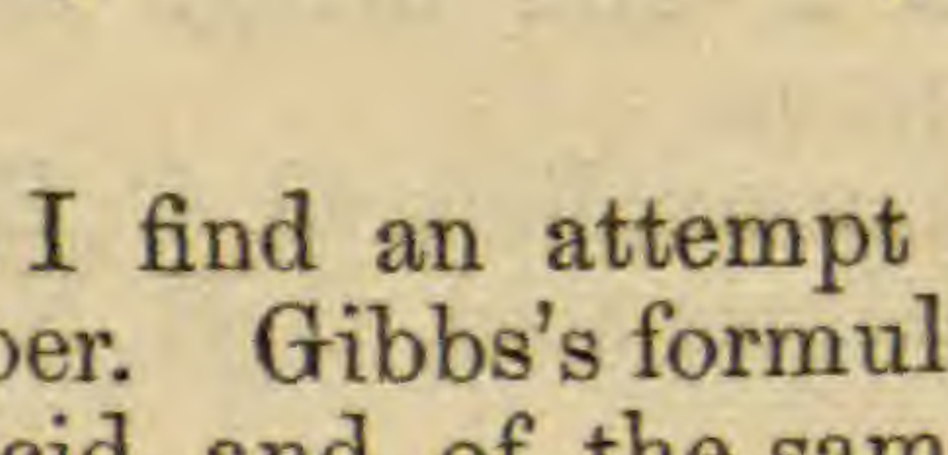
For the production of this body from alloxantine by the action of ammonium chloride, with separation of alloxan and hydrochloric acid, see the formula given beyond for alloxantine.

The formula of alloxan, $C_4H_2N_2O_4$, also a mon-ureide, becomes as represented in No. 2, while alloxanic acid, $C_4H_4N_2O_5$, formed by attachment of the urea residue by its opposite extremity, and with assumption of a molecule of water, may be viewed as in No. 3 above. Claus and Emde* have noticed the difference of character between the last two substances, and suggested in explanation the following formulæ:

1. Alloxan.



2. Alloxanic acid.



one of the few instances in which I find an attempt made to carry out the idea urged in this paper. Gibbs's formulæ would imply that both substances were acid, and of the same degree of basicity. The parallelism between alloxan and paraban is seen to be brought out by the mode of representation now suggested, and an explanation is afforded of the fact that whereas, as Naquet† says (looking only at the number of atoms concerned), alloxanic acid bears the same relation to alloxan that oxaluric acid does to paraban, the former acid is di-basic and the latter mono-basic only.

* Ber. d. deutsch. chem. Gesellsch., vii, 226. † Principes de Chimie (1875), ii, 578.

(To be continued.)

ART. XXIV.—*On Flint-implements from the Stratified Drift of the vicinity of Richmond, Virginia;* by CHARLES M. WALLACE.

THE James River, upon the left bank of which Richmond is situated, approaches the city from the southwest—running in the Mannikin country over fields of bituminous coals, and pouring its waters in headlong rapids over a broad belt of granite, through which it has cut a channel sixty feet or more deep. Huge bowlders, some of them weighing many tons, crowd the drift-beds near the falls, and strew the surface of the uplands below them. Many of those which were in the way of the early settlers have been broken up and removed, but others that remain, sufficiently indicate the course and level of the Drift. In some instances that I have noted, the marks of ancient pot-holes are legibly impressed upon them, proving unmistakably the fact of their descent from the rapids above.

The trend of the prehistoric river is distinctly traced on its south side by the great upper terrace, which probably formed one of its borders before the bed-rock on the Richmond side had been scooped out. As far as I have explored this even and lofty plane—say twenty miles up the basin—it appears to be capped on its inner slope with gravel of the same general character as that which has been excavated at corresponding levels on the opposite shore. A succession of parallel slopes of limited extent show how the current has been diverted from a straight-forward course, and how, upon approaching the tide-water, it has slid away to the north side, forming a wide horseshoe of several miles in extent.

The shelving of the left bank with its relief of hills is quite in contrast with the picturesque island-terraces upon which the neighboring city of Manchester is built. Main street in Richmond runs through the center of the drift-field which abuts upon the steep sides of the city hills, and converges to a point before being swept by the freshets of Gillies Creek and James River. It is on the exposed flank of this field that I have found some of the best specimens of drift-flints in my collection.

My first discoveries were made a little more than a year ago in the elevated beds of the Appomattox, below its falls, and in the brick-earths that uniformly overlie the drifts of this valley.

One of the implements I extracted from a deep bed of brick-clay on the left bank of James River, which has been recently cut away for an avenue to the Free Bridge; it was firmly imbedded in the stiff clay—on its flat side—about seven or eight feet below the surface of the terrace, which at this point attains an elevation of forty or more feet above the rapids. It is of clay-slate, and has the ordinary English hatchet-like shape, with slight chippings at its lower cutting-edge, but no side-grooves for the clasp of a handle. The drift-bed, as evidenced by bowlders of granite and thin seams of pebbles interspersed through the clay, lies at a distance of 300 yards from the river, upon a sloping trough of the granitic rock.

My next discoveries were made in the clay and gravel of the famous Powhatan terrace. This upland has been to me a prolific field for the finding of palæolithic implements. It may be cursorily described as a shelf-like bank on the north side of the James River, at the lower turn of the horseshoe, three-quarters of a mile long by one-third of a mile in breadth. It rises in bluff-like style from the river—with its upper surface at least thirty feet above the ordinary flooding of the tide. For many years the top clay has been used by brick-men who find the bed thicker as it recedes from the bluff and approaches the hills. The following is a section of this shelf-terrace near the river, where the strata appear to lie conformably.

	Feet.
1. Top soil, with occasional bowlders,........	0·6
2. Brick-earth, yellowish hue,................	3
3. Whitish clay, hard and stiff when dry,.....	6
4. Old river gravels, large pebbles at base,.....	7
5. Gray-brownish sand compacted, resting upon Tertiary earth, depth not ascertained.	

This gives a fair idea of the terrace along its river-front, which has been excavated for a railway, say half a mile or more.

In the angle formed by the river and Almond Creek, a wide area of the terrace has been laid bare, exposing to view some interesting features of the Drift. The Old River gravel has been shaved off a little below its surface—as much as 450 feet long by 150 feet wide. A large bowlder of quartz, and several bowlders of granite rest upon beds of reddish, rounded gravel, giving the air of having been transported hither by floating ice, and deposited in gentle waters. One of the larger group measures eight feet one way by twelve the other, and still bears upon one of its sides the mark of an ancient pot-hole. The elevation of this excavated bed above the tide—a few yards distant—is about twenty-four feet.

Carefully inspecting the upright walls on either hand, I found, *in situ*, at a depth of four feet below the surface, the implement which is described below. It was lying at the base of the brick-clay—which here is very scant—on its flat side, as if it had been dropped in the ooze of the marsh mud. Indeed the color is not of the earth from which it was taken, but whitish or porcelaneous, like pebbles from the spring gravel.

It has been split in two by a single blow of the workman's hammer along its longer axis. There are two clearly-marked side-grooves obviously designed for being wrapped by withes or fastened in a haft. The fractured face has been worn a little less smooth than the natural crust of the pebble, and the flakings at the sides and edges look worn as if by being rubbed in gravelly beds.

This implement does not materially differ, except in the peculiar coloring of its outside, from some in my collection from the surface. I regard it as a most interesting specimen from the drift, as it appears to link the discoveries of the older gravels with those of the immediate surface. Two other strange-looking tools were taken by me from the gravel four and eight feet below the surface. Both of them are hoe-like in structure. From the dumps which serve to ballast the track of the railway, I picked up quite a number of worked pebbles, which evidently came from this bed.

Lest I might be deceived as to the archæological value of these discoveries, I requested Judge Clopton, who had manifested a warm interest in my researches, to accompany me on my next excursion.

The next field selected lay along the bluffs of the river, and on either side of the York River Railroad. At the north side of the Yuengling Brewery, immediately overlooking the tide, we picked from the old river-gravels several flint-chips, but no well-defined forms. Below this point a few paces, I had previously picked out of a gully a beautiful disk of quartz, which, being crusted with the bowlder clay, I concluded belonged to a bed of that formation close by. It was originally a flat, round pebble, which had been struck into its present form by a skillful hand. The face which retains the natural crust, suggests the idea of its having been flattened by grinding ice under heavy pressure. One third of its periphery has been chipped to a sharp, jagged edge, as if for the purpose of barking trees or bruising bones. It may have been hafted—but it is more likely that it was used directly by hand. It is a little larger and thicker than a biscuit of hard tack.

The most important discovery of the day's excursion was reserved for the close. A few paces below Main street, in a deep cut of the York River road, and high above the highest water-mark of the river, among many imperfect indications, Judge Clopton was the first to notice a brownish looking flint, stuck fast in the cemented gravel, eight feet below the surface. It was lying with its point on a downward slide, as if it had acquired that position by a landslip over the Tertiary beds.

The shelf of land, from which this unique spear-head was taken, is distant about 150 yards from the river-shore, and forms the extreme or turning point of the Richmond terrace. The height of the gravel-bed above the tide is forty feet or more.

A few days later I inspected alone the Richmond terrace on its bluff-side, near the Dock, from which I have received some of the most interesting relics of the Quaternary man of Virginia. The order of the formation at this point appears to be:

	Feet.
1. Brick-earth underlying grayish clay, --------	9
2. Rounded gravel, reddish hue, --------------	4
3. Deposit of fine bluish sand, ----------------	12
4. Bed of gravel and bluish pebbles, ----------	4
5. Alternate seams of compacted sand, gray and yellow, above the level of base of excavation,	4
Depth of formation as far as known, -----	33

The brick-earth of this section of the terrace has been topped off to allow of the extension of Cary street, and the natural wall has been pushed back, so to speak, as much as fifty feet or more, to make room for the foundation of the York River Railroad Station. From the surface of the lower gravel bed, I extracted several worked flints, two of which closely resemble those of the European Drifts. A remarkable feature of the lower seam of gravel is the presence in large numbers of the pebbles from which the implements for the most part appear to have been fashioned.

One of them is somewhat like an implement from the Reculver Pits, a sketch of which may be seen at page 534 of Mr. John Evans's elaborate work on The Ancient Stone Implements of Great Britain. The other is of lanceolate form, and will be readily recognized by those familiar with the relics of the Caves and Drifts of the old world. It is much worn by long association with the older gravels. It was probably used as a scraper.

Deeming such discoveries of interest to the scientific world, I lost no time in reaching its ear, through the medium of Professor Spencer F. Baird, who very readily acknowledged my labors, in the most cordial and encouraging way.

I have extended my inquiries farther away from the river, with continued success. The Great Upper Southside Terrace already referred to, has been recently explored by me, and found to contain worked pebbles of the same general character as those derived from the high-level gravels on either side of James River.

An excavation eight feet deep has been made on the inward slope of this old river-shore disclosing fine and coarse gravel intermixed with reddish clay. The field of the surface below this excavation has afforded many similarly worked pebbles—which circumstance favors the conjecture that they have been washed out of the overlying beds.

I dug out of the vertical wall of excavation two well-defined implements—one three feet below the surface, the other two feet deeper. Both were imbedded in firmly cemented reddish gravel. The deeper-lying pebble is worn smooth on its chipped edge, the other has the appearance of being rolled but slightly.

This section of the old-river shore is half a mile distant from the present bank of the river. I may hereafter refer to it as the Fonticello gravel. Similar beds of gravel on the right bank of the river I have found to contain worked pebbles.

Mr. Mann S. Valentine, to whom I have shown my drift-specimens, has examined a bed of old river-gravel a mile away from the falls, and found some interesting flints. I have not seen them, but do not doubt that they are of the same general character as those contained in the high-level beds on either bank of the river.

In a deep cut of the Petersburg road, a little beyond the High bridge on the south side, I found several flint chips and worked pebbles, which appear to take the staining of the light gray matrix from which they had been taken. The elevation of the terrace at this point is seventy feet above the rapids—the depth of the specimens below its surface ten feet. My son Charles, who has been trained to look for worked flints, dug out of the clay-bed a rude stone hatchet. An exceedingly beautiful adze or hatchet was found here by me, though not in place. There can be no doubt that it belongs to the same stratum of clay from which other but not similar looking flints were extracted by me. It is shoe-shaped grayish-looking quartzite flint, and has been chiseled into form by a half dozen blows given with a downward stroke. It is not worn. It may have been used either in the hand, or with a haft.

The whitish clay from which I took it lies in a trough of the granite which attains an altitude at this point of sixty feet or more above the level of the river close by. It is capped by the usual brick-earth, which, however, is rather scantily deposited at this place.

It will be understood by the reader that all the discoveries herein mentioned were made in deposits forming parts of the clay and gravel. The implements could not have been introduced into the formations by any other agencies than those which deposited at the same time the containing beds.

Richmond, Va., Jan. 13, 1876.

ART. XXV.—*Description of a new Trilobite, Dalmanites dentata;* by Dr. S. T. BARRETT.

THE Trilobite described below is from the upper compact beds of the Delthyris shale, a member of the Lower Helderberg formation, near Port Jervis, Orange County, New York. The name, *Dalmanites dentata*, refers to the dentate margin of the cephalic shield. The following are its characters.

Dalmanites dentata.—Outline of head parabolic; posterior side concave, and posterior angles prolonged into mucronate, slightly falcate extensions; its outer margin throughout dentate. Eyes having about the same position as in *D. pleuroptyx*, but nearer the outer margins of the cheeks because of the less breadth of the head; number of lenses in a large specimen about 180, eight ranges of them on the highest side. Pygidium triangular, transversely convex; posterior extremity prolonged into a gradually attenuate spine, which is a continuation of the lateral margin, and averages half the length of the axis. Axis sloping evenly throughout, its inferior extremity nearly merged in the border below. Two rows of minute spines extending the entire length of the axis near its center, and scattering minute spines either side over the surface of the segments.

Fragments of what I suppose to be thoracic segments of this species are common. Each terminates laterally in a slender terete spine curved outward and upward at right angles to the rest of the segment; it has a deep narrow longitudinal groove upon its lateral portion, which runs out backward toward the spine, and a deeper transverse groove over its middle portion, the part posterior to which is much larger than that anterior; the surface has minute spines, and otherwise resembles that of the pygidium.

This species has a considerable vertical range, and some layers of the rock are mainly made of its remains. It is associated with *Rensselaeria mutabilis*, *Homalonotus Vanuxemi*, *Loxonema Fitchiana*, *Chonetes complanata*, and other Lower Helderberg species, kindly identified for me by Professor Hall.

The excellent photograph illustrating this paper was taken, from one of the best of my specimens, by the skillful photographer of Port Jervis, Mr. E. P. Matterson. It is one and a half times larger lineally than the specimen. The writer will furnish those desiring it a second photographic plate, giving a view of the pygidium, eye prominence, and thoracic segment, and has specimens for exchange.

Port Jervis, Dec. 29, 1875.

ART. XXVI.—*Mineralogical Notes;* by EDWARD S. DANA. No. II.—*On the Samarskite of Mitchell County, North Carolina.*

THROUGH the kindness of Mr. Joseph Willcox of Philadelphia, and of Rev. J. Grier Ralston of Norristown, I have had an opportunity of examining a considerable number of more or less perfectly crystallized specimens of samarskite, which belonged to their cabinets. The results are sufficiently definite to give a pretty exact knowledge of the relations of the species which have been till now very uncertain.

According to information obtained from Mr. Willcox, and also from Professor Bradley, the samarskite is found in the mica mines situated in the mountains of Mitchell County, North Carolina. The rocks of the region are gneiss and mica slate, and the mines are worked in the granite veins which intersect them. Other localities also exist, under similar circumstances, in Yancey, McDougal and Rutherford Counties. The samarskite occurs in masses, generally irregular in shape but sometimes coarsely crystallized, imbedded in a reddish feldspar, which is very much decomposed, sometimes to a kaolin. The masses vary in size, some being very large; one obtained by Mr. Willcox weighed upwards of twenty pounds.

The immediately associated minerals are two other species of the same tantalic group, described further on, and a yellow mica, which may prove upon chemical examination to be of interest.

The samarskite when pure has a deep velvet-black color, though brown by transmitted light on very thin edges. The luster is resinous and very brilliant, and the fracture distinctly conchoidal. The mineral from this locality has already been analyzed by Miss Ellen H. Swallow,* with the following results (specific gravity 5·755): Metallic acids, tantalic group, 54·96, SnO^2 0·16, UO 9·91, FeO 14·02, MnO 0·91, CeO 5·17, YO 12·84, MgO 0·52, insoluble residue from oxalate of cerium 1·25, ignition 0·66 = 100·40. The metallic acids were not separated in consequence of the want of material. Attention may also be called here to the analysis, by Dr. Hunt, of the samarskite from Rutherford County, N. C., published in this Journal, II, xiv, 341, 1851.

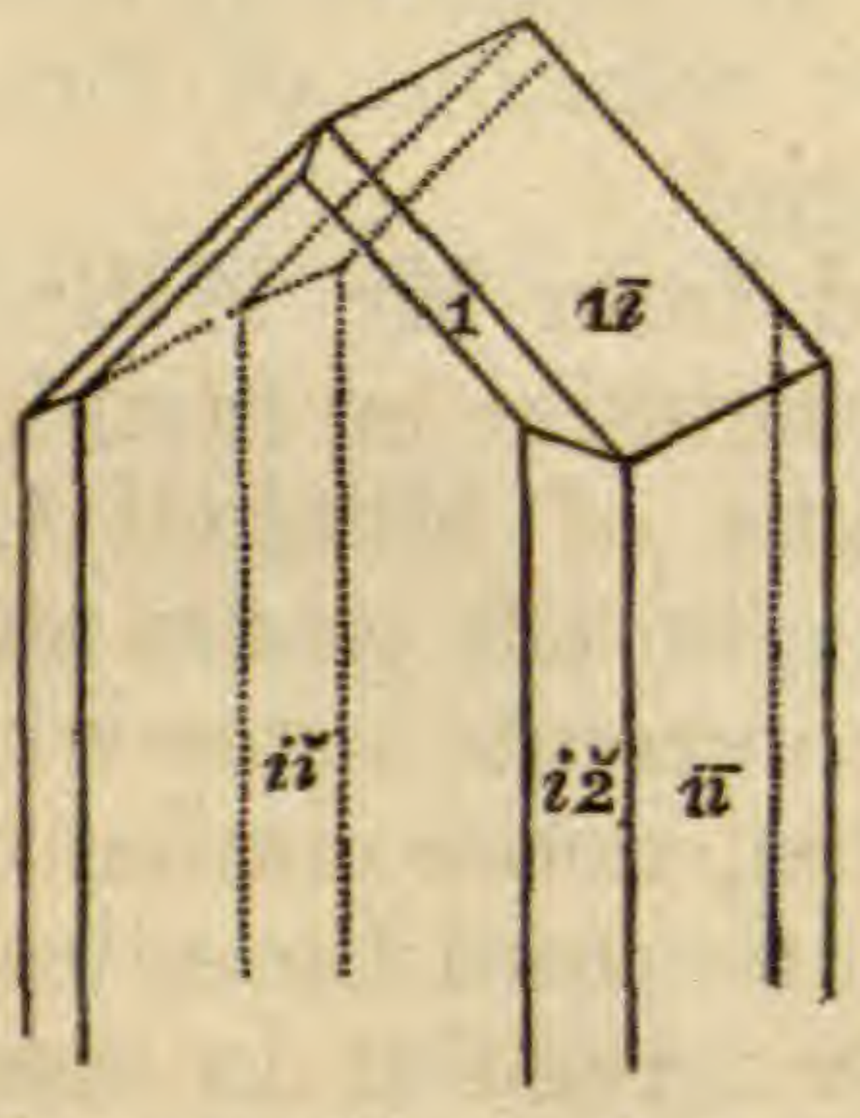
The samarskite exists in all states of purity, being sometimes intimately mixed with the gangue of decomposed feldspar. There are also connected with it several more or less distinct decomposition-products which deserve a chemical examination. A yellow coating over the surface of the masses is very com-

* Proceedings of the Boston Society of Natural History, vol. xvii, 424, 1875.

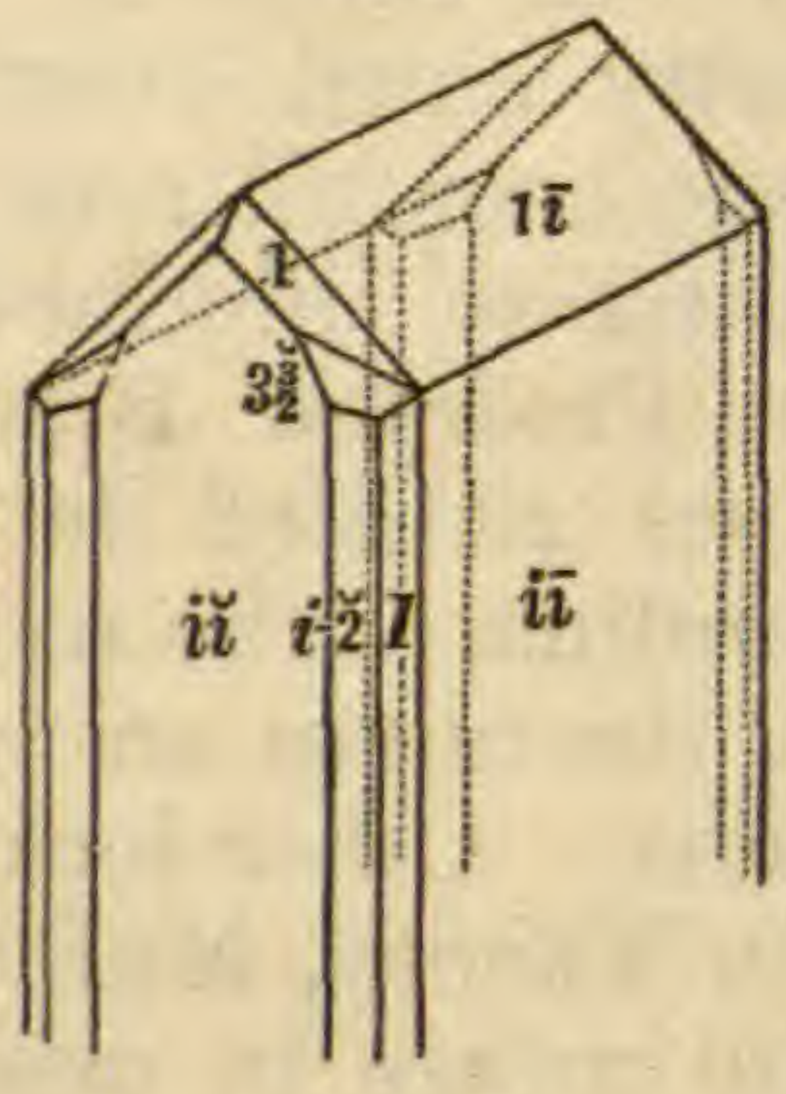
mon; and in some cases the exposed exterior of the pure mineral has taken a chocolate-brown color.

As has been stated, indistinct crystals and crystalline masses are not uncommon, and some few specimens, especially those placed in my hands by Mr. Ralston, admitted of exact determination. The general habit, and the more common of the occurring planes are shown in figure 1, the additional planes of figure 2 are rather rare.

1.



2.



The characteristic feature of all the crystals, almost without exception, is the nearly right-angled edge between the macrodomes, $1\text{-}\bar{i}$. Not infrequently the elongation of this terminal edge gives the crystals a prismatic appearance in that direction. More generally, however, the crystals are elongated vertically in the direction of the prism as taken in the figures; a radiated arrangement in the groupings of the crystals, sometimes observed, is a feature deserving mention. The prismatic planes (*I* and $i\text{-}\breve{2}$) are in all cases narrow so that the general habit is that of a rectangular prism; frequently the crystals are flattened in the direction of the brachypinacoid $i\text{-}\breve{i}$, and upon the surface of this plane are sometimes observed a number of small outlined crystals similar to those often occurring on the diametral planes of columbite. Occasionally, also, $i\text{-}\bar{i}$ is the more prominent, giving rise to forms flattened in this direction.

The occurring planes, as seen in the figures, are as follows: $i\text{-}\bar{i}$, $i\text{-}\breve{i}$, *I*, $i\text{-}\breve{2}$, $1\text{-}\bar{i}$, 1, $3\text{-}\breve{\tfrac{3}{2}}$. The planes are without luster and often quite rough, so that approximate measurements alone were possible; and in different crystals some of these angles varied considerably. The angles obtained from the best formed crystals are as follows:

$$i\text{-}\breve{2} \wedge i\text{-}\breve{2} = 95° ;\ 1\text{-}\bar{i} \wedge 1\text{-}\bar{i} = 93°.$$

From these measurements the following axial ratios are obtained:

$$c' \text{ (vert.) } 0·949,\ \bar{b} \text{ (macr.) } 1·833,\ \breve{a} \text{ (brach.) } 1·000.$$

Some of the calculated angles for the other forms are as follows, the angles obtained by measurement being given in parentheses:

$I \wedge I = 122° 46'$, $I \wedge i\text{-}\bar{\imath} = 151° 23'$ (152°), $i\text{-}\breve{\imath} \wedge 1 = 110° 35'$ (110°), $i\text{-}\bar{\imath} \wedge 1 = 130° 7'$, $I \wedge 1 = 137° 14'$, $i\text{-}\breve{\imath} \wedge 3\text{-}\breve{\tfrac{3}{2}} = 125°55'$(126°), $i\text{-}\bar{\imath} \wedge 3\text{-}\breve{\tfrac{3}{2}} = 135° 46'$ (136°).

The position here adopted shows most favorably the probable relation of the samarskite, of North Carolina, to the crystals of yttrotantalite described by Nordenskiöld.

	Samarskite (Dana).	Yttrotantalite (Nordenskiöld).	Columbite.
$I \wedge I$	122° 46′	123° 10′	(122° 48′, $i\text{-}\bar{\tfrac{3}{2}}$)
$i\text{-}\breve{2} \wedge i\text{-}\breve{2}$	95°	94° 32′	(94° 58′, $i\text{-}\breve{\tfrac{4}{3}}$)
$1\text{-}\bar{\imath} \wedge 1\text{-}\bar{\imath}$	93°	(87° 24′, $\tfrac{1}{2}\text{-}\bar{\imath}$)	90° 15′
$i\text{-}\breve{\tfrac{3}{2}} \wedge i\breve{\tfrac{3}{2}}$	(101° 24′)	(101° 52′)	101° 26′ (I)

The parentheses indicate that the forms referred to have not been observed. The pyramidal planes of samarskite (1 and $3\text{-}\breve{\tfrac{3}{2}}$) are not known on yttrotantalite. It will be observed that while in the prismatic zone the agreement between the two species is close, in the domes the variation is considerable.

The prism of samarskite referred to the axes of columbite (Dana's Min., p. 516) is $i\text{-}\bar{\tfrac{3}{2}}$, and on this basis the other planes become as follows: $i\text{-}\breve{2} = i\text{-}\tfrac{4}{3}$, $1 = 1\text{-}\bar{\tfrac{3}{2}}$, $3\text{-}\breve{\tfrac{3}{2}} = 2$, $1\text{-}\bar{\imath} = 1\text{-}\bar{\imath}$.

The crystalline form of euxenite has not been very clearly made out, but it seems to be closely related to that of samarskite; for $I \wedge I$, Dahl gives 126°, Greg 120°? (122° 46′ samarskite); $i\text{-}\bar{\imath} \wedge m\text{-}\bar{\imath} = 154\tfrac{1}{2}$ and 153°, but $i\text{-}\bar{\imath} \wedge 2\text{-}\bar{\imath} = 152° 13'$ (samarskite); also two pyramids are mentioned giving the angles $i\text{-}\breve{\imath} \wedge p^1 = 107$ ($i\text{-}\breve{\imath} \wedge 1 = 110° 35'$ samarskite), $i\text{-}\bar{\imath} \wedge p^2 = 136°$ (135° 46′ samarskite).

The method of association of crystals of samarskite and columbite at Miask (to be mentioned later) seems to suggest that the broad plane, $i\text{-}\breve{\imath}$ of the figure, may possibly correspond to the plane $i\text{-}\bar{\imath}$ of columbite. (To avoid confusion it must be noticed that $i\text{-}\bar{\imath}$ columbite, Dana's Mineralogy $= i\text{-}\breve{\imath}$ Naumann, and I Dana $= i\text{-}3$ Naumann.) This idea is supported by a single one of the specimens under examination, where of two associated crystals, the cleavage plane (probably $i\text{-}\bar{\imath}$) of the columbite was exactly parallel with the plane of the samarskite called $i\text{-}\breve{\imath}$ above. If now this change is made, the planes, before mentioned, become as follows: If $i\text{-}\breve{2} = I$ and $i\text{-}\breve{\imath} = i\text{-}\bar{\imath}$ then $I = i\text{-}\breve{2}$, $1 = 1\text{-}\breve{2}$, $3\text{-}\breve{\tfrac{3}{2}} = 2\text{-}\breve{\tfrac{4}{3}}$. The consideration of all the facts, however, seems to show that the method first proposed should be adopted.

It may also be mentioned here that several of the minerals of this group show angles of 91°–95°, 128°, etc., in the prismatic zone, although in the other zones there is no apparent correspondence, and the habit is quite different.

The occurrence of two other minerals of this tantalic group has already been mentioned. One of these minerals occurs in regular octahedrons, sometimes nearly an inch across, with the cubic planes, and also the form 3-3. It has a yellowish-brown color and resinous luster. Professor Brush reports, from his examination, that in blowpipe characters it agrees closely with pyrochlore; but its specific gravity as determined by him on a pure crystal is 4·794, which is considerably higher than that of pyrochlore (4·203, Hermann), so that it may approach more nearly to microlite. For a definite knowledge of its character we must consequently wait for the chemical analysis which Professor Allen proposes soon to undertake. These octahedrons occur generally in a rusty gangue, the mass of which seems to consist mostly of the same mineral. They are also sometimes observed implanted directly upon the samarskite.

The second associated mineral is *columbite*. It occurs in crystalline masses of considerable size, imbedded in the samarskite, or implanted upon it. The form where distinct is very similar to those given in Dana's Mineralogy, figures 429, 430, p. 516, and the angles agree closely. From some qualitative experiments Professor Allen finds that it contains a considerable quantity of tantalic acid. On this account it is a matter of some surprise that its specific gravity is only 5·476.

This intimate association of columbite and samarskite at this locality is the more interesting in that, as long ago shown by Hermann, these two species occur together at Miask in the Urals. Some Uralian specimens recently examined by me have the minute crystals of columbite, well formed, implanted on the samarskite, the crystals of the two appearing to occupy a parallel position. It would here hardly be suspected that the two minerals were distinct, except from the cross fracture, in which the two decidedly differ. The American specimens, on the other hand, with the single exception alluded to, show no relation at all in the position of the crystals of the two species.

Professor Allen is at present engaged in a thorough chemical investigation of the various minerals, which have been mentioned, and the results of his work will be awaited with much interest.

Art. XXVII.—*The Effect of Silicic Acid upon the Estimation of Phosphoric Acid by Ammonium Molybdate;* by E. H. Jenkins.

The idea seems to be general that the presence of silicic acid in solutions, impairs the accuracy of the estimation of phosphoric acid by the molybdic method. In Rose's Handbuch der Analytischen Chemie, 6th edition, volume ii, page 519, under a description of this method the fact is stated that silicic acid gives a precipitate similar to the ammonium phospho-molybdate, and Fresenius (Quantitative Analyse, 5th edition, page 334) advises the separation of silicic acid as a preliminary. W. Knop has observed (Chemisches Centralblatt, 1857, page 691) that ammonium molybdate, added to a solution containing silicic acid and a large quantity of ammonium chloride, produces a lemon yellow precipitate much like ammonium phosphomolybdate. Without this excess of ammonium chloride no such precipitate forms, either in the cold or after heating to boiling.

It might readily be supposed, however, that, though not precipitated by itself with ammonium molybdate, silicic acid could come down in a precipitate of ammonium phosphomolybdate and introduce an error. To ascertain whether this actually happens the following experiments have been made. A solution of potassium silicate was employed, made by boiling pure silicic acid with potassium hydrate, and acidifying slightly with nitric acid. It contained in 50 c.c. ·2055 grs. of silicic acid, and bare traces of phosphoric acid. A solution of pure hydrodisodic phosphate was prepared, and the phosphoric acid estimated by the ammonium molybdate method. 25 c.c. gave

(1) ·0844 $Mg_2P_2O_7$ = ·05398 P_2O_5
(2) ·0845 " ·0540 "

Estimations of phosphoric acid were made in varied quantities of the solution of sodium phosphate with the addition of potassium silicate, by the molybdic method.

The amounts of the solutions employed and the results obtained are given below.

·0492 SiO_2	+1 c.c.	sodic phosphate	= ·0022 P_2O_5	gave	·0023 P_2O_5
·0492 "	+5	"	= ·0108 "	"	·0114 "
·0492 "	+12·5	"	= ·0270 "	"	·0267 "
·0123 "	+25	"	= ·0540 "	"	·0540 "
·0246 "	+25	"	= ·0540 "	"	·0545 "
·0492 "	+25	"	= ·0540 "	"	·0544 "
·2055 "	+25	"	= ·0540 "	"	·0538 "
·2055 " ·5000 $CaSO_4$ ·2000 MgO ·5000 Fe_2O_3 1·0000 Alum	+25	"	= ·0540 "	"	·0544 "

A solution of tricalcic phosphate in nitric acid containing in 50 c.c. ·0379 grs. P_2O_5 gave with ·3100 SiO_2 ·0381 grs. P_2O_5

·3100 }
1·0000 alum } gave ·0382 grs. P_2O_5

The ammonium molybdate and magnesium chloride solutions used in these determinations were made as recommended by Abesser, Jani and Märcker in their paper on the estimation of phosphoric acid (Fresenius Zeitschrift, 12th year, p. 252), and all the operations were conducted as there advised. The above results show that in no ordinary case is a previous separation of silicic acid necessary to ensure all desired accuracy in the estimation of phosphoric acid by the molybdic method.

Prof. Kolbe's Laboratory, Leipzig, Dec. 17, 1875.

ART. XXVIII.—*On the youngest Huronian Rocks south of Lake Superior and the age of the Copper-bearing Series;* by T. B. BROOKS.

IN the summer of 1874, Chas. E. Wright and myself, while exploring the country west and south of the Menominee River about ninety miles from its mouth, under the auspices of the Wisconsin Geological Survey, observed a large granitic area, the north edge of which was bounded by dark-colored hornblendic and micaceous schists of Huronian age, which I have since concluded are the equivalents of the youngest member of that series yet observed in the Marquette Iron Region.* The prevailing form was a medium to coarse-grained gray granite, with rectangular crystalline facets of feldspar.† In places it passed through gneissoid granite to a true gneiss, which was once hornblendic, the schistose structure of which always conformed with the underlying schists.

The lithological character of this wide granitic belt bore so much general resemblance to the Laurentian rocks, which are extensively developed on the waters of the Sturgeon River in Michigan, 10 to 20 miles to the northeast, that we were disposed at the time to believe that some phenomena of folding or faulting had brought rocks belonging to that system to the surface in an unexpected quarter. Professor Pumpelly and myself, several years previously had observed, farther to the north and west, similar granitic rocks crossing the Michigamme and Paint Rivers (branches of the Menominee), presenting similar puzzling relations with beds known to be Huronian. This formation is noticed in my Michigan Report, vol. i, p. 175, and the probability of its being Huronian, and younger as well as lithologically different from any rocks then known to be of that period, is pointed out.‡

* The staurolitic mica schist, Bed XIX. of my scheme. See vol. i, pp. 83 and 130, Michigan Geological Report, 1873.

† A few small granite dykes were observed penetrating the hornblende schists along the granite border.

‡ It is not improbable that some of the granitic rocks S. W. of Michigamme Lake in the Marquette Region, may belong to the same horizon.

A careful consideration of all the facts to be observed in the Menominee Region confirms me in this hypothesis,* which is further supported, as it seems to me, by observations in the Penokie Iron Region (Bad River), Wisconsin.

Colonel Whittlesey's maps and sections, given in Owen's Report, 1852, represent a belt of granite, syenite, and hornblende rocks as dividing the Penokie series (Huronian) from the overlying Copper-bearing amygdaloidal traps and sandstones, which lie to the north and nearer the lake.

I observed these rocks at several points in 1871, and noted their general lithological resemblance to the Laurentian, as well as the almost insurmountable structural difficulties in assigning to them that age, and recorded in my notes the probability of their being Upper Huronian. Rowland Irving mentions these rocks† as being coarsely crystalline aggregates "chiefly of labradorite and orthoclase feldspar, hornblende, and some variety of pyroxene," with occasional evidences of bedding, which points toward their entire conformability with the underlying Huronian. He regards them as of the period of the Copper-bearing series, constituting its lowest and oldest portion.

Having been, so far as I know, but little studied, it is perhaps impossible at this time to determine their age: but what is known can here be briefly surveyed, and an inference drawn, which will not be without value in directing further investigations.

1 The general lithological similarity of this granitoid belt to the Laurentian, has been remarked. It has quite as much similarity, if not more, to several members of the Huronian; and is, I believe, not identical with any rock known to belong to the Copper series.

2. Its geographical extension is peculiar in this: it wedges out rapidly to the east from the vicinity of Penokie Gap, entirely disappearing at the Montreal River, which divides Michigan from Wisconsin. Professor Pumpelly and myself traced the boundary between the Copper and Huronian rocks 30 miles farther eastward beyond Lake Gogebic, without again observing it, which we should certainly have done if it had existed there; for we often found the two series very near together, although the actual contact was not seen.

* Dr. H. Credner regarded the entire Marquette series as the equivalents of the lowest member (a quartzyte) of the Menominee Huronian, a position not at all borne out, as it seems to me, by the facts. He seems to have based this geognostic reasoning largely on a rough section which I sketched for him (and which he has reproduced) of the Negaunee District, where the Upper Huronian, so well developed at Michigamme Lake, is wanting. His great overestimate of the thickness of the Menominee rocks has also led him astray. (See Zeitschrift der deutschen geologischen Gesellschaft, Band xxi, 1867, p. 553.) No attempt was made in my Michigan Report to correlate the Marquette and Menominee series, each being provisionally numbered independently.

†Am. Jour. Sci., vol. viii, 1874, p. 49.

3. Not only does this granitoid formation thin out and disappear in its eastward prolongation, but the same is true of the whole Huronian series, the belt of which becomes narrow as followed east, and finally disappears in the neighborhood of Gogebic, where the Laurentian is seen very near the Copper series.*

4. The fact that the granite mass does not cross either the Copper or Huronian series, or, so far as observed, give off dikes in either, renders it improbable that it came into its present position as an eruptive mass subsequent to the formation of both series of rocks.

5. The various ores of iron, which are so generally and abundantly diffused in the Lower and Middle Huronian, are entirely absent so far as observed from the upper three or four members as developed in the Marquette and Menominee regions, and also in the Penokie series if the following hypothesis is true; but they occur in all forms, although, it is believed, not abundantly, in the uppermost *exposed* member on Black River.† If we suppose this iron to have been mostly precipitated as a carbonate, then we might expect it would be more generally diffused through the rocks of certain epochs than those materials derived from the erosion of adjacent coasts.

There is evidently but one hypothesis which will reconcile these facts, which is: that the granitoid formation in question is of the Huronian period, and probably the youngest member; which series are here *non-conformably* overlaid by the Copper-bearing rocks. I conceive that this view is supported by the observations in the Menominee region above recorded, and suppose this Penokie granitoid formation may be the equivalent of granitic bed XX of the Huronian series as developed in that region. On this hypothesis, it is possible that the valley dividing the Penokie Range proper from the granitoid belt may be underlaid by a soft slate, the equivalent of the micaceous schist, bed XIX.

I would anticipate the objection which many will make to attaching much weight to lithological evidences in determining the age of formations 100 miles apart, by repeating that the staurolitic mica schist formation (XIX) maintains its mineralogical character for over one half this distance. I fail to understand why conditions favorable to the formation of extensive areas of particular rocks may not have existed occasionally in Archæan Time, since they were so prevalent in the following ages. This idea of equivalency is further supported by facts given in my "Revised Descriptive Catalogue of the Michigan State Suite of Huronian Rocks," in preparation.

* Pumpelly and Brooks, this Jour., vol. iii, 1872.

† The best point for observing the Huronian between Lake Gogebic and Montreal River.

The approximate conformability in strike and dip of the Huronian and Copper series, observed by Prof. Pumpelly and myself between the Montreal River and Lake Gogebic,* would, in this view, be only accidental and not prove identity of age, as we were at the time inclined to suppose, and with which view Mr. Irving agreed.

As supporting the view that these pre-Silurian systems † are of distinct periods, I would call attention to their well-known points of difference The Huronian series of stratified greenstones, chloritic and related schists, clay slates, quartzytes, marbles, micaceous and hornblende schists, gneisses and granites, containing no copper or other metallic ores, except great conformable beds of magnetite, hematite, and limonite, differ as widely as may be from the compact and amygdaloidal melaphyres, friable sandstones, conglomerates with porphyry pebbles, which constitute the bulk of the Copper series, the whole more or less charged with native copper and silver; all of which points strongly toward a different origin for the two systems.

In their metamorphoses and movements subsequent to their deposition, there is a not less wide divergence noticeable. The friable sandstones of the Copper series, showing no greater metamorphism than the overlying Silurian for which they are often mistaken, has no counterpart in the highly crystalline schists and quartzytes of the Huronian, where we have only just enough of the arenaceous character left in some of them, to leave no doubt as to their fragmentary origin. But the difference in the amount, sharpness, and regularity of the folding and bending of the rocks of two systems into existing wave-forms, is if possible wider than their lithological variations. Contrast the magnificent regular sweeps of the Copper series, the main ranges of which preserve the same strike and direction of dip from Keweenaw Point westward for 150 miles, presenting for half the distance only the south upturned edge of the broad synclinal which embraces one fourth of the great lake in its basin;‡ with the older system, everywhere sharply folded into narrow troughs and irregular basins, trending in every direction, the upturned edges of whose enclosing rocks box the compass, winding and zig-zaging in outcrop like a sluggish river.§

* This Jour., vol. iii, June, 1872.

† I regard the non-conformability and difference in age of the Copper-bearing series and Lower Silurian rocks of Lake Superior, as established by the facts recorded in the papers of Prof. Pumpelly and myself and of Mr. Irving, in this Journal, already referred to. The hypothesis that the Copper rocks are the youngest Silurian formations of Lake Superior and were deposited during a period of elevation and depression which ceased at the beginning of the St. Mary's (Potsdam) epoch, I conceive is not supported by recently observed facts.

‡ See Irving's interesting remarks, this Jour., vol. viii, July, 1874.

§ Dr. J. P. Kimball, called attention to this structure in 1865, in this Journal.

If we extend our observations to the older and again non-conformable* Laurentian, we find the rocks still more plicated and metamorphosed, often even to the extent of entirely obliterating all evidences of stratification. If we suppose the forces which have produced the metamorphosis and the wave forms to have acted regularly and constantly from the beginning of Archæan time to the beginning of the Paleozoic, we may easily suppose the above results produced, viz.: the Laurentian most disturbed and changed, the Huronian next, and the Copper series least, the Silurian practically not at all.

A fact not without interest is the entire absence, so far as I know, of any patch even of rocks of the Copper period south of the great Keweenaw belt. If the two systems were conformable and of the same age, it is difficult to suppose it possible that erosion should have entirely denuded all the Huronian area which must have been covered by the Copper series of the rocks of that period. One would expect that somewhere a mass of these supposed younger Huronian beds would have been embraced in some one of the numerous sharp, deep synclinals, and have been found by those indefatigable mineral prospectors who have so thoroughly searched this region. On the hypothesis of non-conformability, it is much easier to conceive how it was possible for Silurian breakers coming from the south, slowly advanced by a subsidence from the same direction, to have done their work in completely uncovering the present Huronian area and leaving the great Copper range escarpment one of the most striking topographical features as well as the most difficult geological problems in the Northwest. It is easy to suppose for example, the horizontal Silurian rocks being entirely eroded from any Archæan terrains, but not of the Huronian rocks being entirely eroded from a Laurentian area, for the reason already given. Lastly, Logan states, Geology of Canada, 1863, p. 77, that "certain conglomerates of the Lower Copper-bearing rocks north of Lake Superior repose non-conformably on the upturned chloritic schists of the Huronian."

We are therefore justified, I think, in regarding the Copper-bearing rocks of Lake Superior as a distinct and independent series, marking a definite geological period which separates the Silurian from the Huronian ages. Should future observations confirm this view, it would be advisable to have some more convenient and geologically acceptable name for the series than that now in use. Since Keweenaw Peninsula forms one of the most striking geographical features in Lake Superior and is the locality where the Copper series are best exposed and were first studied, I suggest the name Keweenawian for this period.

* Pumpelly, Credner and myself have observed and recorded this in publications already referred to.

The difference in age of the Huronian and Laurentian having been proven, as already remarked,* by observed non-conformability, by the great rarity, in the younger series, of granite and greenstone dykes so numerous in the older, warrants us in reasserting the same kind and degree of unity and independence regarding the Huronian series.

The considerable amount of carbon distributed through the Huronian, indicating much organic life in that period, leads us to hope that those "imperfect fucoidal impressions" seen by Mr. Julien (Mich. Report, vol. ii, p. 5.) may not prove delusive, and that we shall yet be able to avail ourselves of paleontology in determining the age of this system.

The Laurentian rocks have been too little studied to justify an opinion as to whether they may be separated into two or more non-conformable systems, as has been attempted in Canada.

Art. XXIX.—*On a new Method of Measuring the Velocity of Electricity;*† by Joseph Lovering, of Cambridge, Mass.

Perhaps it is not too strong a statement to say that a question is half answered when it is properly asked. Now when it is asked, *What is the velocity of electricity*, there is no strict propriety in the question. For electricity has no *velocity*, in the common sense of the word *velocity*. There is no analogy between the transmission of an electrical disturbance and the propagation of light, or sound, or radiant heat, for example. The mathematical theory of the galvanic circuit, as stated by Ohm in 1827, and the more recent analysis on the same subject by Kirchhoff and Sir William Thomson, have appeared to prove that the time of transmission of an electrical disturbance is proportional to the total electro-statical capacity of the conductor, multiplied by its total resistance. As each of these factors increases with the length of the conductor, the time of transmission is proportional to the *square* of the length of the conductor. Therefore, it cannot be told with what velocity electricity will move until it is known through what distance it must travel. If it be asked, not what is the velocity of electricity, but what is its time of transmission, in any particular case, there would be more hope of a definite answer. The distinction just indicated will do much towards reconciling the contradictory results of experiment in regard to what is erroneously called the velocity of electricity; these experiments making the velocity appear to be sometimes as great as 288,000 miles a second, and sometimes no more than 800 miles a second. In the first case the experiment was made on a very short conductor, and in the second case on a conductor of great length.

* See my Michigan Report, vol. i, pp. 126, 156.

† From the Proceedings of the American Association for the Advancement of Science.

When experiment undertakes to deal with such amazing rates of transmission as those of light or electricity, one of two things is indispensable; it must possess the means, either of operating over enormous distances of space, or of measuring excessively small intervals of time. When the propagation of light is under consideration, there is a free choice between the two methods. If we choose the first, which may be called the direct method, astronomy will supply ample spaces, and no extraordinary nicety of measurement in the other element is demanded. But the practicability of the second method, even when the spaces traversed by the light do not exceed the limits of the physical laboratory, has been demonstrated by Fizeau, Foucault and Cornu.

If we turn now from the propagation of light to that of electricity, it is obvious that nothing less than the largest lines of telegraph wire furnish the conditions required by the first method. On the 28th of February, and again on the 7th of March, 1869, the late Professor Winlock, of the Harvard College Observatory, sent electrical signals from Cambridge to San Francisco, and thence by other lines to Canada, and back again to Cambridge, over a loop of wire measuring 7200 miles. This long journey was performed by electricity in about two-thirds of one second; and no small portion of this brief interval was lost in bringing into action the thirteen repeaters which were interpolated into the circuit. In the determination of longitude by telegraphic signals, the transmission time of the signals comes out as an incidental result. When the signals are sent eastward, the apparent difference of longitude exceeds the real difference of longitude by the transmission time. When the signals are sent westward, the apparent is less than the true longitude by the same quantity. The average of the two values is the true difference of longitude, and half the difference of the two values is the transmission time of electricity. For example, in the campaign conducted by officers of the United States Coast Survey, in 1869–70, for the determination of transatlantic longitudes, I obtained the following results. The total transmission time between Brest, France, and Duxbury, Mass., by the way of St. Pierre, was ·816 of one second. The total distance by cable is 3329 nautical miles; the distance from Brest to St. Pierre being 2580 nautical miles, and that from St. Pierre to Duxbury 749 nautical miles. When the differences of length, caliber and materials as between the two branches of the cable are all taken into account, I find that the transmission time between Brest and St. Pierre was ·639 of a second, and between St. Pierre and Duxbury ·177 of a second, so that the two branches were traversed, one at the rate of about 4000 nautical miles a second, the other at the rate of 4230 nautical miles a second.

Wheatstone's remarkable experiments on the velocity of friction electricity, first published in 1834, offer an example of the second method of measuring great velocities. In this case, the experiment was made upon a length of only one quarter of a mile; and the exceedingly small fraction of time required by electricity to traverse this short distance (amounting to only $\frac{1}{1152000}$ of one second) became distinctly measurable by the relative displacement which it produced in the images of two sparks, formed in a rapidly revolving mirror. Hence the hasty conclusion was adopted that the velocity of electricity was 288,000 miles per second. The immense discrepancy between this result and those afterwards reached by experiments on land and ocean lines of telegraph could not be overlooked, and an explanation was sought in the different tensions of friction and voltaic electricity. This explanation was unsatisfactory because direct experiments on telegraph wires appeared to indicate that the velocity of electricity was independent of the strength of the battery. The discrepancy itself vanishes, or changes its character, when attention is given to the law that the transmission time of electricity is proportional to the square of the distance. Wheatstone's experiment simply proved that electricity will go through one-quarter of a mile of wire at the rate of 288,000 miles per second, and that it would pass over only 268 miles of similar wire in one second. Now this is a much *smaller* velocity than is found by experiments on either land or ocean lines of telegraph; the reason being, probably, that in the inferences from Wheatstone's experiment no account has been taken of the intervals of air which separated the different branches of the conducting wire.

The theoretical law, already stated, viz.: that the transmission time increases with the square of the velocity, has been verified experimentally by Gaugain. He used two threads of cotton, each of which was 1·65 meters in length. When tried separately, the transmission time on each was eleven seconds. When they were placed end to end, so as to double the length, the time was forty-four seconds.

As Wheatstone's experiment on the velocity of electricity has never been repeated, and as direct experiments upon telegraph lines are not numerous and are not likely to be rapidly multipled, and have not been hitherto very harmonious in their results, some other indirect method of conducting the investigation may be found of scientific value. For this purpose, I have availed myself of Lissajous' method of compounding the rectangular vibrations of two tuning forks, and amplifying the resultant motion, by the twice reflected beam of light, which afterwards enters a telescope.

The tuning forks and telescope are permanently fixed to a base-board, so as to preserve their adjustment. Each tuning fork is provided with an electro-magnet, in order to maintain its vibration for a long time. The tuning forks, when vibrating independently, are nearly in unison, each making about 128 vibrations in one second. When the electro-magnets are brought into action, by a voltaic current circulating continuously through them and a standard tuning fork, furnished with an electro-magnet and a break-circuit attachment, the first two forks are forced into exact unison with the standard, and, therefore, with each other. Under these circumstances, the resultant orbit seen in the telescope is invariable. If the instrumental corrections for the two electro-magnets are equal, this orbit will be the first of the series for the unison; that is, an oblique straight line. If this is not the case, it will be convenient to make it so, by introducing resistances at the proper place in the circuit. Then, the apparatus is ready to be put to the work of measuring the velocity of electricity. An additional length of resistance coil is introduced, sufficient to change the orbit to some other in the series. The best one to select is the straight line which inclines in the opposite direction. The new orbit proves that one of the forks begins a vibration by half a period behind the other fork; which, in this particular case, is $\frac{1}{256}$ of one second. This fraction of a second is the transmission time for the passage of the current through the additional resistance coil. Unison forks of higher pitch would register smaller fractions of time. So would also forks, in which the ratios of vibration were less simple; but the orbits would be more complex and could not be observed with the same precision as the straight lines.

I have perfected the apparatus, just described, to such an extent as to feel assured of its adaptation to the purpose which has been specified. But I wish to make a larger number of observations, upon different lengths of resistance and under various combinations, before I give numerical results. I propose, hereafter, to subject in this way to experimental trial, the theoretical law that the transmission time increases with the square of the distance, and that the velocity is inversely as the distance. If this law holds good, the unit time and the unit velocity may be found for a unit distance, or a unit resistance, and then the time and the velocity can be computed for any other distance or resistance. This unit time and unit resistance must be accurately calculated from a combination of all the results of the various experiments. It is also desirable to ascertain the time and velocity for coiled and uncoiled, for naked and covered conductors; as also for air lines and ocean lines. It is to be observed that, in all cases, the time and velocity ascribed to the passages of the electricity apply to that amount of electricity which is required to work the receiving instrument.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Problems in Chemical Dynamics.*—In continuing his valuable researches in thermo-chemistry, Berthelot has developed some important facts in chemical dynamics. He finds that sodium butyrate when crystallized contains three molecules of water, all of which it loses in a dry vacuum or when heated to 110° C. The last half molecule of water is very persistent; so that by careful management, a definite hydrate of this composition

can be isolated. If now, these salts be dissolved in 120 parts of water at 6°, the anhydrous salt dried at 110° sets free 4·27 calories, the same salt dried in a vacuum 4·21 calories, the lower hydrate 3·66 calories, and the ter-hydrate 3·44 calories. Hence (1) the anhydrous salt is identical, however dried; and (2) heat is set free when a salt already abundantly hydrated, is dissolved in water. From the above numbers also, it appears that the union of the half molecule of water (liquid) with one molecule of the anhydrous salt sets free 0·58 calory; while the subsequent union of this with the two and a half other molecules of liquid water, sets free only 0·22 calory. If these values for liquid water be converted into those for water in the solid state by subtracting from them the heat of fusion of water, 0·715 for each half molecule, then the curious fact appears that the union of the first half molecule *absorbs* 0·135 calory, that of the subsequent two and a half 3·55 calories, while that of the three together is 3·49 calories; or in other words the union of solid water to solid sodium butyrate to form a crystallized hydrate, causes a considerable absorption of heat, contrary to the general fact. Consequently it is clear that the formation of hydrated sodium butyrate at a temperature at which water is liquid, i. e., above zero, must be attended with the evolution of heat, while the same hydrate produced with solid water, below zero, would cause an absorption of heat in its production. Berthelot calls attention to the change of sign in the heat-relations produced by combination at different temperatures as being a fact of the same order as that observed in allotropic elemental changes, such as for example, those of sulphur. The thermic relations then of allotropic changes of state are thus closely approximated to those of a chemical reaction properly so called; the stability of the bodies formed being intimately related to the changes of sign in the heat-relations attending their transformation.—*Ann. Chem. Phys.*, V, vi, 433, Dec., 1875. G. F. B.

2. *Action of Light on Silver Bromide, colored and uncolored.*—H. Vogel has given a *resumé* of the results of his recent experiments upon the chemical action of light upon silver bromide both pure and when mixed with some coloring matter. He finds: (1) that pure silver bromide shows by sufficiently long exposure to a strong light, a sensitiveness even to the ultra-red rays—having obtained plates showing not only the line A but a line beyond this, at a distance equal to that between A and B. Silver chloride is also sensitive as far as A and silver brom-iodide even beyond. (2) To the substances already mentioned, which increase the sensitiveness of silver bromide for the special rays which they absorb, may be added methyl-violet and cyanin, the latter increasing remarkably the sensitiveness for the orange. (3) In place of putting the coloring matter into the collodion as formerly, Vogel now prefers to flow the previously prepared plate with an alcoholic solution of the coloring matter which is then allowed to dry. (4) Experiments are necessary to determine the strength of these alcoholic solutions, since when they are too strong, the light is seri-

ously weakened before reaching the collodion. If, however, the prepared plate be exposed to the spectrum from the back side, this difficulty will be avoided. Moreover, in this way the action of imperfectly transparent coloring matters may be tested.—*Ber. Berl. Chem. Ges.*, viii, 1635, Jan., 1876. G. F. B.

3. *Corrosion of Platinum Stills by Sulphuric Acid.*—In 1862, SCHEURER-KESTNER communicated to Dr. Hofmann the results obtained by him in concentrating sulphuric acid in stills of platinum, which were published by the latter in his Report. The figures there given having been criticised as exaggerated, the author now publishes further facts upon this question. From 1851 to 1861, 4309 tons of sulphuric acid were concentrated to 66° B. in an alembic, the body of which weighed 40 kilograms. The entire loss of this part of the still was 12295 grams or 2·859 grams for each ton of acid. To destroy the nitrous products which were the cause of this large loss, ammonium sulphate was added in amount just sufficient for the purpose. In 1862, 1843 tons of acid were concentrated in the still, with a loss of 2490 grams; being 1·22 grams of platinum for each ton of acid, a marked decrease. From 1864 to 1875, 17516 tons of acid (of 1000 kilograms each) were concentrated to 66° in a still the body of which weighed 50 kilograms. The acid contained no nitrous compounds, and only sulphurous acid. The loss of the still was 16178 grams, or 0·925 grams to the ton of acid. To produce acid therefore of 66° B. containing 94 per cent H_2SO_4, there is a loss to the still per ton of acid of one gram when nitrous compounds are absent, and of 2½ to 3 grams when they are present. These numbers are much increased however, by carrying the concentration above 66°. In a still weighing 30 kilograms, 180 tons of acid were produced, containing 97–98 per cent real acid. The still lost 1092 grams platinum, or 6·07 grams per ton of acid. In producing 47 tons of acid of 99½ per cent, there was a loss of 8·80 grams platinum per ton of acid. An analysis of the acid itself showed 8·38 grams of platinum to the ton, in solution in it. To the figures here given for the loss of the body of the retort, about 13 per cent should be added for the other parts. It appears then that this loss of platinum in concentrating sulphuric acid is actual, and that it is a chemical not a mechanical one. The use of a platinum-iridium alloy for the stills prevents to a large extent this action, but the brittleness and consequent fragility of the alloy is a serious objection to it.—*Bull. Soc. Ch.*, II, xxiv, 501, Dec., 1875. G. F. B.

4. *Production of a Secondary Hexyl Alcohol.*—OECHSNER DE CONINCK has studied the products obtained by the hydrogenation of ethyl-butyryl, a mixed acetone discovered by M. Friedel among the products of the dry distillation of calcium butyrate. To obtain the ketone, two kilograms of this salt were distilled in portions of 150 grams, and gave 660 grams of distillate, which when fractionated yielded a little butyral, considerable methylbutyral, 80 grams of a limpid highly refractive liquid of a strong

ethereal odor, boiling at 122°–124°, which analysis showed to be ethyl-butyryl, and a considerable quantity of butyrone. To hydrogenate this mixed ketone, a layer of water was placed in a flask, and on this the ketone was placed. On adding sodium in fragments, these sank through the upper layer, came in contact with the water, evolved hydrogen and rose through the ketone again, and so on. On fractioning, a limpid mobile highly refracting liquid passed over at 134°, which had an agreeable ethereal odor and a burning taste, and which afforded on analysis the formula $C_6H_{14}O$. Hence it is a hexyl alcohol, and since it is formed by the fixing of hydrogen on ethyl-butyryl ketone as isopropyl alcohol is from acetone, it is a secondary alcohol. The reaction is expressed as follows:—

$$\left\{\begin{array}{l}C_2H_5\\CO\\C_3H_7\end{array}\right. + H_2 = \left\{\begin{array}{l}C_2H_5\\CHOH\\C_3H_7\end{array}\right.$$

Isohexyl alcohol is soluble in ethyl alcohol and ether, scarcely in water. Its density at 20° is 0·81825, it etherifies readily with hydriodic acid, yielding an iodide boiling at 164°–166°, and an acetate boiling at 149°–151°. Beside this alcohol, an oily liquid was obtained in the distillation after treatment with sodium which boiled at 252°–255°, and had the composition $C_{12}H_{26}O_2$. Investigation showed it to be a pinacone produced by the union of two molecules of the ethyl-butyryl, with a fixation of an atom of hydrogen on each. Its formula is—

$$\begin{array}{cc}H_5C_2 & C_2H_5\\ \mid & \mid\\ HOC & -COH.\\ \mid & \mid\\ H_7C_3 & C_3H_7\end{array}$$

Treated with sulphuric acid, and fractionated, it yielded on analysis numbers indicating a mixture of a pinacolin and a hydrocarbon, but the quantity was too small to effect their separation.—*Bull. Soc. Ch.*, II, xxv, 7, Jan., 1876. G. F. B.

5. *On Rosolic Acid.*—GRAEBE and CARO have made a more complete investigation of the acid which Wanklyn and Caro obtained from rosaniline by converting it into the diazo-compound and then decomposing it by water, with a view to throwing some light upon the constitution of rosaniline itself. This acid, which had been called rosolic acid, was supposed to be identical with a substance obtained by the action of oxalic acid on phenol, by Kolbe and Schmitt. But this latter product was shown by Dale and Schorlemmer to be a mixture, and they isolated from it a coloring matter which they termed aurin, and Fresenius separated a second substance which he termed corallin. Graebe and Caro propose therefore to retain the name rosolic acid for the rosaniline derivative. The rosolic acid was prepared by the method given above and gave on analysis the formula $C_{20}H_{16}O_3$. It forms ruby-red crystals with a green reflection, is soluble in hot alcohol, glacial acetic acid and ether, insoluble in benzine and

carbon disulphide, slightly soluble in water. It acts like a weak acid, the ammonia salt crystallizing in needles. By reducing agents it is converted into hydro-rosolic acid, and by KCy into hydro-cyan-rosolic acid. Acid alkali sulphites dissolve it to a colorless solution. Oxidizing agents convert it into a minium-red substance. By heating with acetic oxide no acetyl-derivatives are produced. Warmed in alkaline solution with zinc dust, leucorosolic acid is obtained in colorless silky crystals. This yields a triacetyl derivative. Triacetyl-hydrocyan-rosolic acid, tetrabromrosolic acid, tetrabromleucorosolic acid and hydrocyantetrabromrosolic acid are described, and the analogy of this body to the phthaleins in this respect is noted.—*Liebig's Ann.*, clxxix, 184, Nov., 1875. G. F. B.

6. *On the Synthesis of Betaine.*—Betaine (or oxyneurine as it is also called) is known to be a tri-methylated glycocoll. GRIESS has succeeded in effecting a new synthesis of it by acting on an alkaline solution of glycocoll with methyl iodide. The glycocoll (one atom) is dissolved in an excess of concentrated potassium hydrate solution, the methyl iodide (three atoms or more) is added, and as much methyl alcohol as is needed to make a homogeneous mixture. Soon a reaction sets in, the mixture becomes acid and alkali must be added. The liquid is neutralized with hydriodic acid, the methyl alcohol distilled off, the residue diluted and a solution of iodine in hydriodic acid added. On standing, blackish-brown brilliant needles of betaine periodide separate. These suspended in water and treated with H_2S afford pure betaine hydriodate, from which the other salts and the free base can be easily prepared.—*Ber. Berl. Chem. Ges.*, viii, 1406, Nov., 1875. G. F. B.

7. *A New Acid isomeric with Alizarin.*—SCHUNK and ROEMER have discovered in the commercial alizarin made by Perkin in London an acid of the formula $C_{14}H_8O_4$, isomeric not only with alizarin itself but also with anthraflavic acid. From alcohol it crystallizes in long brownish-yellow needles. It is soluble in baryta water with a dark red color, but possesses no coloring properties. The authors are engaged in studying it more thoroughly.—*Ber. Berl. Chem. Ges.*, viii, 1628, Jan., 1876. G. F. B.

8. *On the Constitution of Acids and Salts.*—BERTHELOT has given a resumé of his results obtained by a thermo-chemical investigation of the composition of acids and salts when in solution, which affords a ready method for their classification. According to his experiments, the relative energy of acids and bases may be measured by the inequality of decomposition of their salts by water, added in gradually increasing amount; this decomposition being indicated by the amount of heat absorbed or evolved. Thus in the first class are placed strong acids and bases. These, when dissolved separately in water and mixed in equal equivalents, form stable neutral salts, and set free a quantity of heat nearly constant for all the bodies of this class, and which does not vary when more water is added, nor by the addition of more of

the same or of another base. To this class belong the chlorides, nitrates, and normal sulphates of the fixed alkalies. In the second class of acids and bases, Berthelot places those which are decomposed by water progressively, the decomposition increasing with the amount of water added. Sometimes this increase is continuous either indefinitely, or possessing a definite limit. To this class belong borates, carbonates, cyanides, sulphides, alkali phenates, acetates, butyrates and valerianates. Sometimes the decomposition takes placed in its entirety by the first portion of water added; so that the thermometer shows an absorption of heat nearly equal to that which is evolved in the original formation of the salt. Such are the salts formed by the alkalies with alcohol, glycerin, mannite, etc. Of course the decomposition by water is the more marked when the base is also feeble, like the oxides of the heavy metals. In this case, the decomposition is apparent even with acids of the first class. Even ammonium salts with strong acids are thus decomposed, though to a less degree than when the acid is weak. The author draws the conclusion that in solution, strong acids always unite with strong bases, leaving the feebler members to each other. To account for the stability of the alkali-salts of strong acids, he supposes that the formation of definite hydrates by the union of water with the acid and the base, taken separately, under the conditions of the experiment, sets free a total amount of heat which is less than that evolved in the formation of the normal salt itself. So, reciprocally, if the alkali-salts of feeble acids are decomposed by water, the reason is to be found in the excess of the thermic effects due to the formation of hydrates over those which result from the formation of the normal salt. Several of the many important considerations which flow from this hypothesis are given at length in the paper.—*C. R.*, lxxxi, 844, Nov., 1875. G. F. B.

9. ***Division of an Acid among several Bases.***—M. BERTHELOT has endeavored to solve the question whether, if an acid is present with several bases, it will unite with one, or be divided among them. Berthollet believed that each base would take an amount of the acid determined by its capacity for saturation and its quantity; for equal weights that each body would take an amount inversely as its equivalent: or, if the bases were employed in amounts proportional to their atomic weights, each would take half the acid. Gay Lussac believed that a complete mixture took place, and that the salts were formed only when separated by their insolubility, crystallization or volatility. To test these views, two bases were selected which disengaged an unequal amount of heat in uniting with the same acid. Thus mixtures of equivalent weights of chloride of ammonium and caustic soda were mixed. The difference in this case of the two bases would equal 1·12 units of heat. Were the theory of Berthollet correct, half of the acid should go to the soda setting free ·56 units of heat. Other divisions of the acid might set free any quantity from 0 to 1·12. According to Gay Lussac there should be no

generation of heat, or at least the amount should be uncertain. Finally, if all the acid passed over to the soda, the amount of heat set free should be 1·12. The amount actually observed equalled 1·07, which as the probable error is ±·04, evidently sensibly equals 1·12. The very small difference of ·05 is also explained by the purely physical action exercised by ammonia on a solution of chloride of sodium. In fact a mixture of the same quantities of ammonia water and chloride of sodium absorbs ·05 units of heat. Similar effects are also obtained by replacing the chloride by the nitrate or sulphate, or by using potash instead of soda. Not only can ammonia be replaced by soluble bases such as potash or soda, but even by those which are insoluble, such as lime. According to Berthollet there should be a division at first, then a precipitation of the lime, and so on until it was wholly separated. But this effect is not produced, the lime is not precipitated but dissolved in the chloride of ammonium. To determine the real nature of the reaction a certain amount of lime was precipitated from the chloride by soda; 1·18 units of heat were thus absorbed. It was then dissolved by chloride of ammonia when 2·24 units of heat were disengaged. But in the latter operation the solution of the hydrate of lime by the ammonia should give out 1·10 units of heat; and the redissolving of the lime should give out 1·10, or in all 2·20, which agrees closely with 2·24, the observed amount. Again, the total heat set free, −1·18+2·24 =1·06 is very nearly equal to 1·07, the amount set free in the previous experiment by the direct action of the soda on the chloride of ammonia. These facts and measures prove that the double salts and the change of the solvent are not the cause of the phenomena; while they are completely explained by the complete substitution of the lime, a nearly insoluble base, by the ammonia, a soluble base. We see therefore that this substitution may take place contrary to the laws of Berthollet.—*Ann. Chim. et Phys.*, vi, 662. E. C. P.

10. *A new Pyrheliometer.*—M. A. Crova has measured the calorific intensity of the solar radiation and its absorption by the atmosphere of the earth. With the pyrheliometer of Pouillet it appeared that the indications varied with the method of preparing the surface. If the silvered chamber containing the water is simply covered with one or more coatings of lampblack, a portion of the heat after passing through the coating is reflected by the metal and thence passes out through the lampblack which is diathermanous. The absorption is rendered more complete by employing an absorbing layer which is wholly metallic. A rough layer of copper is deposited galvanically on the box and on this a layer of platinum black. A large thermometer having a bulb 40 mm. in diameter and a tube 300 mm. long replaces the ordinary silvered box. The bulb is covered with silver, copper, platinum, and finally with a thin coating of lampblack. The tube ends with a second bulb containing a little mercury, which may be introduced as an index into the tube. This thermometer is

introduced into a hollow brass sphere polished without and blackened within, and having an aperture 30 mm. in diameter through which the sun's rays penetrate. The observation of the heating after numerous corrections gives with great delicacy the relative heat of the sun at different times.—*Comptes Rendus*, lxxxi, 1205.
E. C. P.

11. *Thermal Equivalent of Magnetism.*—M. A. CAZIN has published in full a series of experiments on the relations of heat and magnetism. In the first portion of the memoir three methods are described of measuring the relative values of the heat created by the disappearance of magnetism, in the core of an electro-magnet. The second section demonstrates several laws of the magnetic heat developed, and shows that this heat is really due to the disappearance of the magnetism. But in the induction of the core on the coil, and of the coil on itself, causes of heat are found which should be allowed for. The fundamental law deduced from these experiments is, that the disappearance of magnetism in the core of a bar electro-magnet having two poles is accompanied by the creation of a quantity of heat Q proportional to the polar interval l, and to the square of the quantity of temporary magnetism m which the core acquires when the circuit is closed. The product m^2l is a magnitude of the same kind as the quantity of heat Q and may be called the magnetic energy. The ratio $\frac{m^2l}{Q}$ will be the mechanical equivalent of heat. In the third section the value of Q is determined in units of heat while the effects of induction are inconsiderable. The first series gave as a mean of five experiments while the spark was broken in all 110600000, as the magnetic equivalent. A second more reliable series with the spark broken in ether gave 106000000. Both are a little too great because the induced current on breaking the circuit is not zero. Hence, probably the true value does not differ materially from 100000000.—*Ann. Chim. et Phys.*, vi, 493–554. E. C. P.

12. *Leyden Jar Regulator.*—Professor McLEOD at a recent meeting of the Physical Society showed an arrangement for ensuring that the charge given to a Leyden jar shall not exceed any fixed limit. Through a cork in the upper end of a bell-glass passes a brass rod, insulated through its entire length by a glass tube, through which it passes freely. To the upper end is attached a brass knob, and the lower end is pointed and provided with a screw thread, so that it can be set at any distance within, or through, a hollow brass ball, perforated below and rigidly fixed to the glass tube. Within the bell-glass is a loose cage of perforated sheet zinc and a vessel containing strong sulphuric acid. The whole stands on a metallic plate to secure a good earth connection. The action is as follows: if the rod be screwed down so that the point projects through the hollow ball, the upper knob and the lower metallic plate being connected with the two poles of a Holtz machine, only short sparks can be obtained, because a large amount of electricity escapes at the point; but if the rod

be raised so that it barely enters the hollow ball at the top, no escape takes place from it, and the machine will give its full length of spark. By varying the position between these two extreme limits, any required length of spark or amount of charge for an interposed Leyden jar can be obtained.—*Nature*, xiii, 140.

E. C. P.

13. *Report to the Philosophical Society of Glasgow on the Production of Nitric acid from the free Nitrogen of the air.* Part I. By E. M. Dixon. 18 pp. 8vo. Glasgow, 1875.—The author first discusses the researches connected with ozone made by Schönbein, Marignac, De La Rive, Baumert, Prof. Andrews of Belfast, and others, and states, as the accepted conclusion, that ozone is the only allotropic form of oxygen, in other words that antozone has no existence. The report then considers the production of nitric acid through the agency of ozone; four alleged methods of which are mentioned, viz.: (1) by contact of nitrogen or the atmosphere with bodies undergoing oxidation; (2) during electrical discharges in the air; (3) the combining of ozone with nitrogen in the presence of water; (4) through the evaporation and condensation of water in the air. The consideration of nitrification by the *first* method is pronounced to be as yet doubtful, but the consideration of it is deferred to the second part of the report. With regard to the *second* and *third* methods, it states that there is clear proof of the fact that the electric spark is capable of effecting the combination of oxygen and nitrogen in a dry mixture of these gases; but that there is little or no doubt that nitrification does not occur in nature from the action of ozone upon the nitrogen of the air; and that the production of peroxide of hydrogen in nature, as shown by Engler, Nasse, Carius, Schöne, must be ascribed to some other cause than the action of ozone upon either aqueous vapor alone, or upon it and nitrogen together.

Upon the *fourth* method, the report remarks as follows: "In 1862 Schönbein announced the fact that, if water is partially evaporated in the air, the residue contains nitrite of ammonia, and that the same salt is to be found in the water formed by the condensation of vapor in air. Of these facts there is no doubt. Schönbein, however, without ascertaining whether the salt in question did not exist ready formed in the air employed in his experiments, rushed to the conclusion that it must have been formed during these experiments, by the combination of free nitrogen with water. Obvious as the precaution indicated now seems to be, it must also be said that it does not appear to have occurred at the same time to any one else; and some, while accepting Schönbein's explanation of the production of nitrite of ammonia from free nitrogen and water, even thought to contest his claim to all the merit of having discovered so remarkable a property in free nitrogen. The following quotation from a recently published volume, by Dr. T. Sterry Hunt, entitled Chemical and Geological Essays, will show that he still claims a considerable amount of credit for having predicted, on theoretical grounds, the possibility of producing nitrite of ammonia from free nitrogen and water, and for having framed thereupon a theory of nitrification.

"'On September 15, 1862, I read before the French Academy of Sciences a note on The Nature of Nitrogen, and the Theory of Nitrification, published in the Comptes Rendus of that date, and translated in the Philosophical Magazine for January, 1863, in which I repeated the points above given, and then proceeded to consider the results announced by Schönbein in 1862. I said, "The formation of nitrite of ammonia by the combination of nitryl NN with H_4O_2 must necessarily be limited to very minute quantities by the instability of this ammoniacal salt which, as is well known, decomposes readily into nitrogen and water. In order, therefore, to produce any considerable quantity of a nitrite by this reaction, there is required the presence of active oxygen, or of a fixed base to separate the ammonia. The recent experiments of Schönbein have furnished new evidences of the direct formation of a nitrite at the expense of the nitrogen of the atmosphere. According to him, when sheets of paper moistened with a feeble solution of an alkali, or an alkaline carbonate, are exposed to the air, especially in the presence of a watery vapor, and at a temperature of 50° or 60° C., the alkaline base soon fixes a sufficient quantity of nitrous acid to give the characteristic reactions. Appreciable traces of nitrite are, according to Schönbein, obtained in this way, even without the intervention of an alkali. He, moreover, found that distilled water, mixed with a little potash or sulphuric acid, and evaporated slowly at a temperature of about 50° C. in the open air, fixes in one case a small portion of ammonia, and in the other a little nitrous acid. Traces of a nitrite are also formed in pure water under similar conditions. Schönbein explains all these results by the combination of nitrogen with the elements of the water, producing at the same time ammonia and nitrous acid. As he has well remarked, this reaction serves to explain the absorption of nitrogen by vegetation, and through the oxidation of nitrite, the formation of nitrates in nature. By these elegant experiments he has confirmed, in a remarkable manner, my theory of nitrification, and of the double nature of free nitrogen. It is, however, evident that since the publication of my note of March, 1861, above referred to, we cannot say with Schönbein that the generation of nitrite of ammonia from nitrogen and water is 'a most wonderful and wholly unexpected thing.'"

"It is, however, unfortunate for Dr. Hunt's theoretical anticipations that no sooner did experimentalists begin to purify the air that they used in repeating Schönbein's experiments, than the production of nitrite of ammonia suddenly stopped. The experiments of Bohlig and, more recently, of Carius, show that neither during the evaporation of water in air, nor during the condensation of its vapor, does a trace of nitrite of ammonia manifest itself. The experiments of Carius are especially decisive on the point, as they were both numerous and most carefully performed. The verdict here, then, is very clearly adverse to the statements that have been made regarding the evaporation of water and the condensation of aqueous vapor as sources of nitric acid."

14. *On the Electrical Conductivity of Stretched Silver Wires;* by J. G. MacGregor, M.A., B.Sc.—A paper on the above subject, communicated by Prof. Tait to the Royal Society of Edinburgh on the 3d of January, contained a description of a series of experiments, conducted by the author to find the effect of stretching on the conductivity of silver wires. The wires were stretched by weights. The measurements of resistance were made by means of a Wheatstone's bridge, the wire under examination being joined up as one of its arms. The dimensions of the wires before and after stretching, were determined by means of cathetometer observations and specific gravity determinations. The increase in length and decrease in thickness of the wires, caused by stretching, must of course be attended by a corresponding increase in their resistance. The question to be determined was whether there was not also a change produced in their resistance by the change produced by stretching in their molecular state. To get this effect, if it should be present, at its maximum the wires were heated to just below the melting point before the weights were hung on. The results were such as to warrant the statement that if any such change is produced it must be very slight, the difference between the resistances before and after stretching being (when that due to change of dimensions had been allowed for) so small as to be within the limits of observational error. No former determinations of this kind have been made for silver wires. For copper, iron and steel, Mousson has found that the change in resistance is not completely accounted for by the change in dimensions. In another respect also silver appears to differ from copper wires. Meik and Murray have found that the total increase in the resistance of copper wires, due to stretching, is directly proportional to the weights by which they are stretched. Some of the experiments of this paper show that this is not the case for silver wires.

15. *The Nature of Light, with a general account of Physical Optics;* by Dr. Eugene Lammel, Professor of Physics in the University of Erlangen. With 188 illustrations and a plate of spectra in chromolithography. No. xviii of the International Scientific Series.—This is a very excellent popular treatise, intended to afford an answer to the question "What is the Nature of Light?" A mathematical treatment of the subject is avoided in the text, but simple and concise analytical discussions of the more important topics are given in appendices to the different chapters. It is illustrated with numerous wood-cuts, many of which are novel and ingeniously devised, but most of them would have been more effective had they been engraved in a style worthy of the book. The work is an admirably clear and well arranged exposition of its subject, and is, in the main, well translated.

16. *Manual of Introductory Chemical Practice, for the use of Students;* by George C. Caldwell, S.B., Ph.D., Professor of Agricultural and Analytical Chemistry, and Abram A. Breneman, S.B., Assistant Professor of Applied Chemistry in Cornell University, Ithaca, N. Y. Published by the authors. 124 pp. 12mo. 1875.—This manual is an experiment on the part of the authors in a novel mode of chemical instruction, devised by them, with a view to cultivate on the part of the student habits of careful observation, attention to and appreciation of phenomena, and the deduction of legitimate results. In short it seeks to make the student his own teacher by simple synthetic or analytic experiments, and to lead him on by easy steps to an understanding of principles and of chemical philosophy—in a way unattainable from text-books alone. The student is required to give his results and conclusions in writing, an excellent way to secure accuracy and conciseness of statement. He is presumed to be in attendance on a course of experimental lectures, and to be reciting at the same time from a text-book. The work bears marks of careful preparation.

17. *Note on the Electrical Conductivity of Saline Solutions;* by J. G. MacGregor, M.A., B.Sc., Communicated to the Royal Society of Edinburgh, May 17, 1875; by Professor Tait (Proc. Roy. Soc. Edinb., 1875, 545.)—This note is a reply to criticisms by Professor Beetz published in the Sitzungsberichte of the Berlin Academy (and in Poggendorff's Annalen) on a paper of Mr. MacGregor's published in the Transactions of the Edinburgh Royal Society, xxvii, pp. 51–70. Mr. MacGregor shows that the criticisms are based in part on a misunderstanding of his paper and of his method of experimenting.

II. Geology and Mineralogy.

1. *Supposed Agency of Ice-Floes in the Champlain Period;* by Professor A. Winchell, Syracuse, N. Y.—I have lately discovered some new instances of huge limestone masses, anomalously detached from the formation to which they belong; and have embraced references to localities, in a paper read before the American Association at Detroit. These are masses of Carboniferous limestone from 10 to 60 feet in length and often of unknown thickness, floating in the sands of Oceana county, apparently 100 or 200 feet above the bed rock. Some of these, I feel constrained to believe, must be genuine exposures of the formation, in place; but others, by being worked out, or by their downhill dip, far exceeding, and disagreeing with, the normal dip of the formation, are demonstrably dissevered and displaced portions of it. For example, one region of exposures of this class in the town of Claybanks, is within half a mile of the shore of Lake Michigan, where we have a bluff 250 feet in height, and attaining, a few rods back, an elevation of 275 feet, serving as a station of the U. S. Lake Survey. The vicinity, for miles around, is elevated 250 to 300 feet above the lake.

But the section of materials exposed in the bluff upon the lake shore is wholly Post-tertiary. It consists of intimately mingled sand and clay, confusedly stratified above, horizontally stratified lower down, and followed downward by an increase of argillaceous material and pebbles, interrupted by a bed of bowlders, beneath which for 10 feet is a mass of bowlder clay seen above a lake-border talus of ten feet. According to the prevailing constitution of the drift of the Peninsula, there should lie, still lower, a thick bed of fine, horizontally-stratified clay, with few pebbles, resting on a bottom-sheet of pebbles and bowlders. The drift here is presumably not less than 300 feet thick. Now it is possible that, 2,500 feet back from this bluff, the bed-rock should appear at the surface: but my experience in Michigan strongly inclines me to believe that such is not the fact; and that hence, the numerous outcrops near the lake are mere detached masses.

We have, then, in Michigan, in regions widely separated, the well-established phenomenon of extensive tabular masses of limestone floating in the midst of semi-stratified sands, generally believed to have been moved and deposited by an aqueous action, which, obviously, could not have transported at the same time these enormous tables of rock. We have, in addition, in some parts of the State, the evidence that this action was sometimes exerted in a northerly direction. Geological theory must attempt to account for these facts.

The generally accepted doctrine of continental glaciation, recognizes a time when the broad glacier underwent a rapid dissolution. The volume of water arising is believed to have been sufficient to produce a long-continued, torrential flood, which moved and assorted whatever detritus existed in its path. Disregarding the detrital material, which must have originated from atmospheric, pluvial and fluvial action over the preglacial surface, a vast volume of detritus must have been originated during the prevalence of the glacier, and chiefly through its action. Most of this must have rested at or near the bottom of the glacier; but probably no small portion had become incorporated with the ice, or intruded into its fissures, or deposited upon its back. The first glacial film embraced the original projections of the ancient surface, which, with the movement of the glacier, must have been displaced to become ultimately a part of the glacier debris. These and the materials derived from sub-glacial detrition must have found their way, to some extent, into the *bottom crevasses* caused by any diminution in the steepness of the slope down which the glacier moved, and still more when, as was often the case, the change of slope became, in reality a northward declivity. These ordinary conditions of the continental glacier—but feebly represented in the steeper slopes and narrowed limits of modern glaciers—must have resulted frequently in extensive disruptions of the ice, faintly typified in the pyramids and seracs of the Alpine ice-streams. Such upheavals of the lower beds—still more, occasional complete overturnings of portions of the glacier, must have brought considerable earthy detritus to the very surface of the glacier. In the process of ages, as the ice may be supposed to have gradually diminished, through evaporation, if not through thawing, the superficial earthy material, which never evaporated, must have accumulated to a large extent. However we account for this fact, every one knows that human bodies or other objects, accidentally lost in the glaciers of Mont Blanc, reappear at the surface after a series of years, at points some thousands of feet below.

I infer, therefore, that the material moved by the diluvial waters may have been afforded by some of the interior portions and even the surface of the glacier, as well as by the subjacent rock-rubbish. I will only add that some portions of the material in and upon the ice may have been let down *in situ* by the slow disappearance of the glacier, without having been subjected to the assorting action of the glacial torrents.*

This process is impressively illustrated along the borders of the Mer de Glace and other Alpine glaciers; and more instructively still in the buried glacier stumps found in the gulches of the Sierra Nevada, and elsewhere in the Pacific States.

I know of no certain evidences, in Michigan, of a Champlain depression of such extent as to bring the surface of any portion of the State below the sea-level. In a district so nearly horizontal, however, there must have been a period, before the erosions of the modern drainage courses had begun, during which the drainage was exceedingly obstructed and slow. The supply of water from the dissolving glacier was greater than could be discharged through the forming outlets; and the extensive areas must have lain submerged until the deepening of the outlets permitted their drainage. But this period was, by hypothesis, that when a geologic winter was merging into a geologic spring. There was not yet a summer climate; and the annual winter must have congealed the surfaces of the surrounding lakes, and arrested the superglacial torrents, if it did not materially diminish the flow of the subglacial ones.

I think the steps of this reasoning safe and sound. But we have here an overlooked condition of glacial agency in the natural order of sequence, which, it seems to me, is adequate to explain the transpositions of rock-masses to which I have referred. There were regions in these lakes where rocky formations rose nearly to the surface, or projected to a slight extent above it. On the freezing of the watery surface, these would be firmly embraced in the ice. Meantime, as the supply of water is diminishing through the advance of the annual winter, the lake subsides, and the frost takes hold of the exposed rock at a greater depth. But the annual spring and summer return. The supply of water increases, the surface of the lake rises and the floating field of ice lifts sheets of previously half-disjointed limestone, and floats them in the direction whither the current sets or the wind blows. They may be dropped some

* This idea was first impressed upon my attention by my brother, N. H. Winchell, who has studied the Drift with much assiduity. See his papers in Proc. Amer. Assoc., Dubuque Meeting, 1872, and in the "Popular Science Monthly" for June and July, 1872.

miles northward from their native bed, and may lodge upon an accumulation of sand moved by aqueous agencies quite inadequate to move cubic yards of solid rock. I think that ice floes are capable of such work; and I believe it is not essentially different from work in progress in the tracks of Arctic currents in modern times.

The same agency would have picked up and transported the rounded northern bowlders, which we find scattered, also, to some extent, through the same sands.

It could not be expected that the existing configuration of the surface of the State should preserve the features which determined the existence and boundaries of such local lakes as I have supposed; but, after all, are not our existing interior lakelets examples of the same, perpetuated by the delayed erosions of outlets? If it be asserted that neither the less nor the greater lakes are engaged in transportation of limestone masses, in our times, it will be a sufficient rejoinder to remind the reader that the supply of movable masses of limestone must ultimately have become exhausted. Still, it is not a fact that work of this kind has entirely ceased, as any one familiar with the flotsam thrown upon a lake beach will be led to admit.

Nor has time obliterated all traces of that topographical configuration, which in Southern Michigan may have determined an ice-float toward the north. Between Saginaw Bay and the mouth of the Grand River is a broad depression, the highest part of which rises but 72 feet above Lake Michigan. The southern tier of counties in the State presents an elevation of 300 to 600 feet above Lake Michigan. The Corniferous limestone barrier, passing through Monroe and Lenawee counties, still maintains an elevation of 100 to 150 feet above the same lake. Have we not here some vestiges of that ancient conformation of the surface which resulted in a northward drainage into the great channel once intersecting the State, and the northward transposition of ice-born sheets of limestone and sandstone, wrenched from the elevated barriers in Hillsdale, Lenawee and Monroe counties, and the contiguous portions of Ohio and Indiana?

2. *On the outlet of the Great Salt Lake;* by Professor G. K. GILBERT. (Letter to J. D. Dana, dated Washington, Feb. 4, 1876.)—I had not seen Mr. Packard's paper, when my attention was called to it by your letter of the 29th ult. Since he had "not observed personally any facts bearing on the subject," but merely advanced the ideas of others, it is not surprising that everything which is novel in his paper is erroneous.

When the water of Great Salt Lake was at its maximum altitude it carved and molded a beach, which yet remains—a conspicuous monument to its former greatness. Within the circle of this beach-line are included also Utah and Sevier lakes. The level of the ancient beach is 970 feet higher than Great Salt Lake, about 700 feet higher than Utah lake, and about 550 feet higher than Sevier Lake. From the upper beach the water slowly subsided by desiccation, recording its lingerings in a series of fainter shore-lines. When it had fallen to the level of the divide between the Sevier and Salt Lake basins, it was separated into two unequal portions. In one of these the evaporation exceeded the inflow from rivers, and the subsidence continued; in the other the inflow exceeded the evaporation and the surplus was discharged over the divide into the former portion, just as the surplus of Utah Lake is now discharged into Great Salt Lake. In the course of time, as the climate became drier, this overflow ceased; but not until it had carved a channel of some magnitude. The channel is crossed by the old overland stage road, and is known as "the Old River Bed." It is doubtless this ancient water-way which has been described to Mr. Packard. I am not aware that it has ever been determined whether the channel slopes toward Sevier, or toward Great Salt Lake; but a consideration of the forms and dimensions of the two basins, and of the present relative salinity of the two lakes, leads to the belief that it was the Sevier Basin which overflowed into the other. The summit of the divide cannot be far above the present level of Sevier Lake.

In the early part of the field-season of 1872, I crossed the Salt Lake and Sevier deserts as a geologist of the Wheeler Expedition, and gave especial attention to the beaches and other phenomena of the ancient lake. Later in the season my associate, Mr. Howell, carried the observations farther south. Our examinations were sufficiently thorough to enable us to draw a map of the southern half of the old lake, but we found no evidence of an outlet in that direction, although we made diligent search. According to the conjecture of Professor Bradley, and the unpublished observation of Professor Marsh, the overflow was northward, and the Columbia River carried the water to the ocean. There assuredly was an overflow.

In the progress report of Lieut. Wheeler's Surveys for 1872, I have expressed the opinion that the humid climate which was marked by this inundation of Utah, was preceded by one as arid as the present; and that the humidity was a phenomenon of the Glacial Epoch. A fuller statement and discussion of the facts will appear in the geological volume (now in press) of the reports of the Wheeler Surveys; and the accompanying atlas will contain a map of the ancient lake.

3. *Second Report of Progress of the Mineralogical, Geological and Physical Survey of the State of Georgia, for* 1875; by George Little, State Geologist. 8vo, 16 pp.—This brief report shows a large amount of work done during the past year. The several parties have traversed, in all, over 6,000 miles of road, making careful examinations and large collections along their routes. They have visited 105 out of the 137 counties in the State, and a list is given of the minerals, metals and building-stones, of economical value, which have been found in 76 of these counties.

Under the head of Geology, Dr. Little says: "In the Northwestern portion of the State, the coal-formation has been found, by Mr. McCutchen, to be somewhat more extensive than observed hitherto. There has also been some addition to our knowledge of the fossiliferous iron ore beds.

"The metamorphic rocks, on the western border of the Cohutta mountains, have been found to contain lead, copper and silver; while barite has been found at the base in Murray county, and large beds of the same near Stegall's station, in Bartow county. The relation of the metamorphic rocks in these mountains, as well as that in the Blue Ridge and across the Chattahoochee ridge, along the Tugaloo and Savannah rivers, to the corresponding adjacent parts of Tennessee, North Carolina and South Carolina, have been studied, and a regular succession of Potsdam, Quebec and Cincinnati rocks found, in alternating bands, while the whole of this metamorphic region appears to be of Silurian age.

"Prof. Bradley reports 'the extension of the gold-belt over large areas not previously recognized as gold-bearing; the determination of the age, equivalency and position of nearly every important stratum in the Blue Ridge of Georgia, including the copper ores of Fannin and Gilmer, as well as those of Lumpkin and Towns, and the corundum belts of Union, Towns and Rabun, (with the probable position of the equivalents of these latter in Habersham, White, Lumpkin and Dawson,) and the determination of numerous levels which affect both the working of large areas of the gold-field and the location of projected railroads. The points of greatest scientific interest are the identification of the serpentines, chrysolites, chlorites and steatites of the corundum belts, with the magnesian limestones of the Quebec group, (the Knox dolomyte of Safford,) and that the underlying schists of the gold-belt with the Knox shale of the lower part of the Quebec.'

"Prof. Loughridge has found in the Southern portion of the State, that the Cretaceous rocks extend from Columbus nearly to Ft. Gaines, affording valuable beds of marl, and that the Tertiary rocks continue, from a line drawn from Ft. Gaines via Macon to Augusta, over the whole of the Southern counties, abounding in deposits of marl and limestone, while the more recent formations, of Okefenokee and smaller swamps, afford an unlimited supply of marsh muck, which is already being utilized to the great advantage of crops.

"We are now prepared, after this preliminary survey, to enter upon the detailed, systematic and acurate survey of each county in the several divisions of the State; and it is proposed, during the next season, to begin this work at three points on the western border of the State—one party beginning with Dade county, another with Haralson, and a third with Muscogee."

We are glad to see that this State, although the last in the Union, except Florida, to commence the systematic survey of her mineral wealth, is pushing forward the work so well begun last year. It has long been needed, and is evidently in good hands. The results above noticed are of great interest, and we shall look rather impatiently for the detailed reports "now in preparation." This work in Georgia fills the only blank hitherto existing in our knowledge of the *general* structure of the Appalachians; and its vigorous prosecution promises soon to furnish all the more especially desirable *detailed* information concerning the area within that State.

4. *Geological Survey of Illinois*, A. H. WORTHEN, Director. Vol. VI. *Geology and Paleontology:* Geology, by A. H. WORTHEN and Assistants, G. C. BROADHEAD and E. T. COX; Paleontology, by O. ST. JOHN, A. H. WORTHEN and F. B. MEEK. 532 pp., roy. 8vo, with 34 plates. Springfield, Ill., 1875.—This sixth volume of the Illinois Geological Report commences with a chapter, by Mr. Worthen, on the Coal-Measures of the State, which cover 35,000 square miles, and have a thickness of about 1,400 feet. A detailed section, given on pages 2 to 5, includes 16 beds of coal, large and small, with intervening marine beds, proving that each era of terrestrial vegetation was followed by one of marine submergence and abundant marine life. This chapter on the Coal-Measures is followed by others on the special geology of several of the Counties of the State.

Part II continues the reports on the Paleontology. The Vertebrate portion is by Messrs. St. John and Worthen, and the Invertebrate by F. B. Meek. Previous volumes contain descriptions and figures of a large number of new Paleozoic species of fossil plants, Mollusks, Crinoids, and Fishes, with several of Corals, Crustaceans, Myriapods, Scorpions, Insects and Amphibians. This new volume adds largely to the new species of fishes and Crinoids, and somewhat also to those of Mollusks. The contributions of the Survey, through its paleontologists, to the departments of fossil fishes and crinoids greatly surpass all that have been made by other State Surveys; and those of Crinoids are unequalled by the publications of any other country. The number of new species of fishes described, from the teeth, in this sixth volume alone, is over 100 (divided nearly equally between Hybodonts and Petalodonts, with one Cochliodont), and besides these there are 45 species of fish-spines. The plates are full of excellent figures beautifully engraved.

Mr. Worthern states that with this volume the series of reports closes, the "law-making power" desiring "to cut off all appropriations not deemed by them absolutely necessary;" but that there are many fossils yet undescribed, including nearly all the corals and bryozoans, and many common fossils.

The Reports issued make a most honorable exhibit of the liberality of the State; yet the fact that the volumes are so full and excellent in all respects excites the earnest desire that the remaining volume should be issued which would make the series complete.

5. *U. S. Geological Survey of the Territories under* Dr. F. V. HAYDEN. (1.) *Bulletin No.* 6.—This new Bulletin contains the following papers: (1) An account of the various publications relating to the travels of Lewis and Clarke, with a commentary on the results of their expedition, by Dr. E. COUES; (2) Notice

of a very large Goniatite from Eastern Kansas, by F. B. MEEK; (3) Fossil Orthoptera from the Rocky Mountain Tertiaries, by S. H. SCUDDER; (4) Studies of the American Falconidæ: a monograph of the Polybori, with five plates, by R. RIDGWAY. Dr. Coues gives a critical review of the spurious and genuine works that have purported to give the results of the travels of Lewis and Clarke over the Rocky Mountain region, and closes with notes on the Zoology of the expedition. The Goniatite described by Mr. Meek must have had, he observes, a diameter in one direction of sixteen inches; it is a globose species, and is made var. *excelsus* of the Illinois species *G. globulosus*, M. and W. The fossil *Orthopters*, in Mr. Scudder's paper, are a cockroach, *Homœogamia ventricosus*, and an earwig, *Labidura tertiaria.*

This sixth Bulletin contains a general index to Nos. 1 and 2, first series, and Nos. 1 to 6, second series, and thus closes the first volume.

6. *Geological Sketches by L. Agassiz.* Second series. 230 pp. 12mo. Boston, 1876. (James R. Osgood & Co.)—Geological science owes to Agassiz the first distinct announcement of the glacial origin of the northern drift, and also the collection and publication of facts from Europe and North and South America establishing the truth of his theory. This beautifully printed volume contains some of his recent papers on the subject, as they were written out, in a popular form, for the Atlantic Monthly. The chapters treat, severally, of "the Glacial period;" the "Parallel Roads of Glen Roy, in Scotland;" the "Ice-period in America;" "Glacial phenomena in Maine;" and the "Physical History of the Valley of the Amazon." They consist of clear and vivid descriptions and reasonings from one who had seen the facts and scenes he describes, and whose mind was large enough to appreciate their significance and grandeur. We think that Professor Agassiz has attributed too wide a range to the ice-covering of the Glacial period in making it extend over the tropics. But if not right in this opinion, his chapter, on the valley of the Amazon, will still be read with interest and profit.

7. *Geological Survey of Victoria. Report of Progress;* by R. BROUGH SMYTH, Secretary for Mines and Chief Inspector of Mines for the Colony. No. II; with Reports by A. W. Howitt, R. A. F. Murray, R. Etheridge, Jr., N. Taylor, F. M. Krausé, W. Nicholas, G. H. F. Ulrich, J. Cosmo Newberry. 142 pp. Royal 8vo, with views and sections. Melbourne and London. Also, *Observations on New Vegetable Fossils of the Auriferous Drifts;* by BARON F. v. MUELLER. 32 pp. royal 8vo, with maps and plates of figures of fossil plants.—The earlier Reports of the Victoria Survey are noticed in vol. ix, (1875) of this Journal. From the Report of Mr. Smyth we take the following facts. The area of the auriferous grounds of Victoria is about 680,000 acres. The mining surveyors report that there are 3,398 distinct auriferous quartz veins, which have been investigated, besides many others unexplored; and some have been traced for seven miles. One is worked to a depth of 1,000 feet, and another goes down 200 feet

below the sea-level and yields more than one ounce per ton. In 1874, 6,725 tons of auriferous pyrites yielded 18,911 ounces of gold.

The vegetable fossils described by Baron F. v. Mueller are fruits, of kinds unlike existing Australian species, and all are referred to new genera. They come mostly from the auriferous drifts at a depth of about 150 feet, and are referred to the "Pliocene." They include fruits of *Spondylostrobus*, cypress-like conifers; of *Trematocaryon*, supposed to be related to the Verbenaceæ; of *Rhytidotheca*, allied to Chloroxylon; of *Plesiocapparis*, near Capparis; of *Celphina*, supposed to be Proteaceous and most allied to Helicia of East and North Australia; *Odontocaryon*, not referred to any natural order, the author "being unaware of any existing or extinct genus to which it bears really close resemblance;" of *Conchotheca*, having fruit like that of Grevilleæ, but not certainly Proteaceous; of *Penteune*, a large nut, but of doubtful relations; of *Dieune*, perhaps related to Capparideæ or Pittosporeæ; of *Platycoila*, of doubtful relations.

8. *Glacier phenomena along the Kittatinny or Blue Mountain, in Carbon, Northampton and Monroe Cos., Pennsylvania.*—Mr. C. E. Hall describes extensive deposits of gravel and bowlders south of the Lehigh Gap and along the Lehigh River; and also at Wind Gap, and the Delaware Water Gap. Four miles from the mouth of Marshall's Creek, on the road to Craig's Meadow, there are scratches on the Oriskany sandstone, having the direction S. 28° W.—which is toward the gap, following the course of the river. Mr. Hall also shows that the gravel deposits in and about the city of Philadelphia are glacial. Between Spruce and Walnut streets, west of Forty-fifth street, bowlders of Oneida conglomerate, Medina sandstone, and of other rocks, have been exposed to view which vary from one to twenty-five cubic feet in size, some of them glacier-scratched. He mentions also other localities of bowlders within the city limits.—*Proc. Amer. Phil. Soc.*, xiv (No. 95), pp. 620 and 633, 1875.

9. *Wisconsin Geological Survey.*—The Report on the Geological Survey of Wisconsin is ready for the press and awaits only the action of the legislature. A prospectus of its contents shows that it contains a large amount of valuable material. Prof. T. C. Chamberlin, of Detroit, has been placed at the head of the Survey for the present year.

10. *Frequency of Earthquakes relatively to the age of the Moon.*—Prof. Alexis Perrey continues his study of earthquakes, and has recently published in the Comptes Rendus a new statement as to the relation between the age of the moon and the frequency of earthquakes.* Dividing the period of a lunation into quarters, with the time of the syzigies, and quadratures as the centers of these quarters, he finds that the earthquakes are distributed as follows:

* For a translation of a former paper by Prof. Perrey on this subject, see this Journal, II, xxxvii, 1.

	Total.	Syzigies.	Quadratures.	Diff. in favor of the syzigies.
From 1843–1847	1604	850·48	753·52	96·96
1848–1852	2049	1053·53	995·47	58·06
1853–1857	3018	1534·13	1483·87	50·26
1858–1862	3140	1602·99	1537·41	65·98
1863–1867	2845	1463·42	1381·58	81·84
1868–1872	4593	2333·48	2259·52	73·96
1843–1872	17249	8838·03	8410·97	427·06

The reported earthquakes between 1751 and 1843 are shown to conform to the same rule—that is, a large preponderance of earthquakes about the syzigies.

Professor Perrey also finds that of the reported earthquakes between the years 1843 and 1872, 3,290 occurred at the moon's perigee and 3,015 at the apogee.

11. *Fossil Fishes of the Devonian of Tula, and Carboniferous limestone of Mjatschkowa, Russia;* by H. Trautschold (N. Mém. Soc. Imp. Nat. Moscou, xiii, 263, 277.)—The Devonian fish remains here described and figured include Hybodonts of the genus *Cladodus;* Cestracionts of the genera *Orodus, Helodus, Psammodus;* also species of *Ctenacanthus.* The Carboniferous limestone has afforded the author the Illinois species, *Cladodus lamnioides* of Newberry and Worthen: species of *Helodus, Psammodus, Pœcilodus, Cochliodus, Orodus, Solenodus, Petalodus, Dactylodus,* (one of Newberry and Worthen's genera), and *Polyrhizodus;* besides some fish-spines, of which one, *Ostinaspis acuta,* is *Petrodus acutus* N. and W.

12. *On the occurrence of native Zinc.* (Letter to one of the Editors.)—Mr. W. D. Marks of Chattanooga, Tennessee, announces the occurrence of fragments of metallic zinc in the soil along the course of a vein intersecting the blue limestone of Sand Mountain, in the northeastern corner of Alabama. The circumstance is supposed to indicate that the metal came originally from the adjoining rock. Further than this, he states that pieces of metallic zinc have been picked up along a range of thirty miles, over the Racoon Mts. on the southern border of Tennessee, Sand Mt., and the northern portions of Georgia and Alabama. The vein is now being explored, and Mr. Marks hopes to find the zinc in place.

13. *Brookite.*—Exact measurements made by vom Rath upon an excellent crystal of brookite from Atliansk in the Urals show that the mineral from this locality at least is not monoclinic, but orthorhombic. E. S. D.

14. *On the Serpentines of Zöblitz, Greifendorf and Waldheim;* by J. Lemberg in Dorpat.—The chemical examination of the serpentines, from the above mentioned localities in Saxony, by Lemberg, has led to the conclusion that they have arisen from the alteration of rocks consisting originally of chrysolite, garnet and hornblende. An analogous conclusion has been reached by other investigators for similar occurrences. In the case in hand it is shown that the readily-decomposed chrysolite has been changed

for the most part into serpentine; the garnet into minerals of the chlorite group; while the hornblende has generally withstood alteration. The paper of Mr. Lemberg contains a considerable number of analyses showing the composition of the original minerals, as well as of the products of decomposition.—(*Zeitschrift d. Deutsch. geol. Gesellschaft*, 1875, 531.) E. S. D.

15. *Selwynite*, *Noumeïte*, *Garnierite*.—Mr. G. H. F. Ulrich, in a letter dated Melbourne, Nov. 3d, 1875, states that the new species Selwynite, described by him, is not a homogeneous mineral. A microscopic examination shows it to consist of a felsite-like base, through which hydrous chromic oxide is disseminated, with occasionally a small octahedron of chromite. A similar method of examination has shown that the new nickel minerals (noumeite, garnierite), described by Professor Liversidge, are not homogeneous. There is here a soapstone-like base, composed of hydrous silicate of magnesia through which either hydrous oxide of nickel, or hydrous silicate of nickel is densely distributed in small veins and roundish patches. Some of the ore gave an assay up to twenty per cent of nickel, and others as low as two per cent.

16. *Manual of Geology of J. D. Dana.*—The following changes and corrections (besides some others merely typographical) have been made in the stereotype plates of the work since its first publication in 1874, and are needed by the copies of the earlier issues.

Page xv, 17 l. from top, P. C. Carpenter for J. G. Cooper. Page 3, 8 l. fr. top, 1–1,200,000 for 1-200,000. P. 82, fig. 61f has been inverted; and the same on p. 546. P. 147, 4 l. fr. foot, C. for F. P. 166, under fig., 4a Trenton *for* "4 Trenton." P. 338, 2 l. fr. top, fig. 521, *for* "p. 521." P. 344, in map, 9, 9, 9, *for* "8," "8," "8," and 8 for "9." P. 345, 18 l. fr. top, east *for* "west." P. 419, 4 l. fr. foot, southeast *for* "southwest." P. 427, 3 l. and 4 l. fr. top, over two *for* "three or four." P. 538, paragraph beginning with "The absence" has been changed so as to make it state that between the meridian of 100° in Dakota and the eastern boundary of Oregon and California the mean annual precipitation is not, with small exceptions about the higher mountains, over 16 inches. P. 675, 15 l. and 14 l. fr. foot, former *for* "latter," and latter *for* "former." P. 699, 16 l. fr. foot, yards *for* "feet." P. 743, 18 l. fr. top, along the strike *for* "transverse to the strike;" and for the closing part of the paragraph has been substituted:—an effect due to compression by the pressure to which the rocks had been subjected and a consequent expanding in a transverse direction. P. 756. To the first paragraph has been added the remark [a suggestion to the author by Prof. Verrill] that the retaining of the warm Gulf Stream waters in the Atlantic would give the ocean a higher temperature than it now has, and that this higher temperature would be the occasion of an unusual amount of evaporation, and, therefore, of an extraordinary amount of precipitation and frequency of storms along the cold borders of the continent in the Glacial latitudes; so that the theory adopted for the origin of the cold of the Glacial period accounts for both the cold and the abundant precipitation. P. 589, Cetacean area removed from Cretaceous column. D.

III. Botany.

1. *Notes on Agave;* by Geo. Engelmann, M.D.—This is a modest title of a paper in the Transactions of the Academy of Science of St. Louis, Missouri, vol. iii, December, 1875. Separately issued it forms a pamphlet of 35 pages, 8vo. If we mistake not it begins that volume; so that the pages of the pamphlet

are those of the volume, as ought always to be the case, for convenience and uniformity of reference.

Dr. Engelmann deserves high praise and many thanks for taking in hand, one after the other, our difficult botanical subjects, concentrating his attention upon them for a while, elucidating them to the full extent of his opportunity, and leaving them in such a state that they can be easily understood, or readily followed up as occasion serves, by ordinary observers and collectors. His latest essay of this sort was upon *Yucca*. He passes from that to the analogous American genus, *Agave*, the "American Aloe," first distinguished from the old-world *Aloe* genus by Linnæus, who gave them the present name, *Agave*, "because that word indicates something grand and admirable." The headquarters of the genus are in Mexico, but a considerable number inhabit our southwestern borders, and one reaches well into the northern States. There are "perhaps 100 species,"—possibly a high estimate, but the catalogues of cultivators give twice that number of names. Most of them are nearly unrepresentable in the herbarium, while in cultivation they seldom blossom. The century plant, *A. Americana*, may sometimes in our cool regions literally answer to its popular name: semi-centennial specimens at least are not uncommon.

Dr. Engelmann first devotes a few important pages to the general structure and conformation of the trunk, foliage, inflorescence and fructification in the genus, and passes to a systematic arrangement and description of the N. American species as now known to him, and of a few extra-limital species upon which he is able to throw some light. They fall into three groups. 1. *Singulifloræ*, with flowers in a simple spike, a single one to each bract. Our northern *Agave Virginica* is the familiar representative: there are also *A. maculosa* of Texas, and *A. variegata* from just over the border, both in cultivation. 2. *Geminifloræ*, with a denser spike, a pair of flowers to each bract. Our species are arranged by obvious characters of the margin of the leaves, viz: with rough serrulate margins, *A. falcata*, n. sp.; with filamentose margins, *A. Schotti*, n. sp., and *A. parviflora*, Torr.; with aculeate-toothed margins, *A. heteracantha*, Zucc. (which is Torrey's *A. Lechuguilla*), and *A. Utahensis*, Engelm. 3. *Paniculatæ*, the typical Agaves or Century-plants, with paniculate inflorescence. There is a division with tube of the perianth much shorter than its lobes. Under this *A. Newberryi*, n. sp., is marked by the insertion of the stamens on the base of the tube. The others, with stamens borne in the throat, are *A. deserti*, n. sp., *A. Parryi*, n. sp. (doubtfully regarded by Dr. Torrey as a variety, *latifolia*, of *A. Americana*), and *A. Antillarum* Desc., with orange-yellow flowers, now elucidated from materials brought from San Domingo by Parry and Wright in 1871. The division with tube of the perianth shorter than its lobes, and bearing the stamens about its middle, contains a very striking species, *A. Shawii*, from the southwestern corner of California, which, having broad and deep-green leaves with a brown horny margin, set off by the large light red-brown spines, is thought to be one of the finest of the genus for ornamental cultivation. It was discovered by Dr. Parry in 1850, but good specimens only now obtained, and it is appropriately dedicated to the founder of the Missouri Botanic Garden, from which much is confidently expected. Finally, there is a division known by the tube of the perianth equaling the lobes or hardly shorter, and bearing the stamens: to this belong *A. rigida* Miller, with the Yucatan doubtful variety, *Sisalana*, introduced nearly forty years ago into S. Florida by the unfortunate Dr. Perrine; *A. Palmeri*, n. sp., from S. Arizona; and *A. Wislizeni*, n. sp. (which has had the utterly false name of *A. scabra* in Germany) in Northern Mexico. A reference to one or two very imperfectly known species is appended. Of *A. Americana*, there is a mere mention that it has a stipitate capsule.

In all species, so far as known to Dr. Engelmann, the anthers discharge their pollen about forty-eight hours before the style matures and the stigma can receive pollen. After the expansion of the lobes of the latter, at least in *A. Virginica*, a viscid liquid fills the cavity of the apex of the style, "whether stigmatic, or only intended to allure insects, has not been ascertained." The figures which so commonly represent bursting anthers and a fully elongated style in the same blossom are probably factitious, as they certainly are in many otherwise excellent plates of various kinds of flowers. In conclusion, those who have the opportunity to examine species of *Agave* in flower are particularly requested to note at what hour of the day the anthers begin to shed their pollen, and at what time they become effete, and in what state the style is at these periods. The anthesis, so far as Dr. Engelmann has observed, is vespertine or nocturnal, as well as proterandrous. The time and nature of the nectariferous secretion in the lower part of the flower should also be recorded. A. G.

2. *Structure of the Leaves of Grasses: Histotaxie des feuilles de Graminées;* par J. Duval-Jouve.—An elaborate article in Ann. Sci. Nat., tome i, of Ser. 6, 1875, with four admirable plates of anatomical details. It appears to be an excellent piece of work, upon an almost neglected subject. Many of the text-books still say of the leaves of grasses, and indeed of Monocotyledons generally, that their veins or nerves are simple and unconnected by anastomosis; although what was meant must have been that the only anastomosis was by ultimate transverse veinlets. Duval-Jouve cites a long list of grasses in which these are conspicuous; and there are many in which the reticulating veinlets are of different orders. The stomata of grasses are in some confined to the lower surface of the leaf; in others divided between the two faces; in several they are restricted to the upper face, but in these the blade makes a turn or twist, so as for the most part to present this upper surface to the ground. *Triticum junceum*, *Calamrogastis* (*Psamma*) *arenaria*, and *Gynerium argenteum* (Pampas Grass) are cited as instances. Many grasses have under the epidermis of their upper

face, and sometimes of the lower also, rows or bands of large thin-walled cells, which our author names *bulliform* cells. These in their presence, absence, number, and arrangement, are uniform in each species, but often quite different in the same genus, so that they may be used for critical specific characters; and they are, moreover, connected invariably with the vernation of the leaf, and with the opening and closing (either by conduplication or convolution, according to the vernation of the species) which are so prompt in many grasses. That this movement takes place in virtue of the hygrometric expansion of these cells under moisture and their contraction in dryness, was made plain by the behavior of sections of the leaf under the microscope, the closed conduplicate leaf of *Sesleria* opening instantly upon the application of a drop of water, when these cells in a band on each side of the midrib, before flattened or collapsed, became turgid and prominent. The leaves of *Leersia orvyzoides* are described as rolling up instantly upon being bruised or roughly handled, as if endowed with real irritability. We trust some of our young botanists will look to this, next summer.

The split sheath of the leaves is one of the diagnostic characters of the *Gramineæ.* Exceptions in *Glyceria*, &c., were familiar. M. Duval-Jouve states that about a fifth part of the species have entire sheaths. Also that various grasses bear two, three, and even four leaves on one node ! A. G.

3. *Botryopteris Forensis*, an interesting fossil fern, which occurs with fructification preserved in a silicified state in the rich deposits of Autun and Saint Étienne, France, has recently been investigated microscopically by B. Renault (Ann. Sci. Nat., 6 ser., i, 1875). In one plate he has illustrated the anatomy of the stem; in four others its fructification, and the anatomy, developing fructification, &c., of a *Trichomanes*, a *Helminthostachys*, and a *Botrychium*, for comparison. He concludes that in this fossil genus we have a type intermediate between true *Filices* and the *Ophioglosseæ.* A. G.

4. *Silicified fossil Fruits or Seeds*, from the coal beds of St. Étienne, are discussed by Brongniart in a preceding volume of the Ann. Sci. Nat. (with figures), and classified by the form of their transverse section. They are thought to be gymnospermous. Among those with binary symmetry, *Cardiocarpus* in its affinity is thought to answer to *Salisburia ; Rhabdocarpus*, a new genus, to *Torreya ; Diplotesta* and *Sarcotaxus* (new genera), to *Cephalotaxus ; Taxospermum* and *Leptocaryon* to *Taxus.* Those of radiate symmetry of three, six, or eight divisions or a circular section, of various kinds, including *Trigonocarpus*, are conjectured to be the fruit of *Sigillaria*, *Calamodendron*, and the like, which Brongniart takes to be an extinct type of Gymnosperms. A. G.

5. *Respiration of Plants ; some Researches by* Mayer and Wolkoff : a paper in Ann. Sci. Nat., in the volume above cited; apparently translated from a prior publication in German, to which there is no direct reference. That plants have a true res-

piration like that of animals, correlative with decomposition, is so well made out of late years, (and besides is understood to be inevitable if the plant is to do any work), that it was hardly necessary to refer back to a work of Liebig fourteen years old, and even then a little antiquated, for an enunciation of the opposite doctrine. Then the process answering to respiration was overlooked or thought unessential, being overshadowed by the vaster, larger and more important counterpart process of assimilation. The researches of which the results are given in this paper were made to ascertain the relations between vegetable respiration, i. e., the expiration of carbonic acid, and light, temperature, growth, &c. The results, on the whole, were, that changes of temperature within normal limits were of little effect and transient when the change was sudden; that the influence of light, although generally appreciable, was feeble, and probably indirect. This action, as is well known, goes on both in light and darkness, but under the latter it is not masked by the assimilative process. Growth also proceeds indifferently under either, or, it would appear prefers darkness. But Mayer and Wolkoff conclude (contrary to some of their predecessors) that there is no direct relation between growth in length and respiration, so that one should in any sense serve as the measure of the other. A. G.

6. *Classification of Nostochineæ.*—Dr. Bornet, in a recent number of the Annales des Sciences Naturelles, has published a most useful key to the genera of the *Nostoc* tribe, which was drawn up by the lamented Thuret, shortly before his death. Although it was not intended for publication in its present state, it cannot but be useful. The appended enumeration mentions most of the species, with leading synonyms. A. G.

7. *Gymocladus in China.*—If M. Baillon is right in his identification by means of pods and loose flowers, there is a second species of *Gymocladus*, our Kentucky Coffee-tree, indigenous to China, in the vicinity of Shanghai, where the gummy substance in the legume is used as a substitute for soap. This is an additional instance of a supposed monotypic genus of Atlantic North America being represented in the corresponding part of N. E. Asia. Baillon's notice of it is in Bull. Soc. Linn., Paris, Jan., 1875. A. G.

8. *Flora Brasiliensis*, fasc. 68, issued in March, 1875, has just come to hand. It contains the *Amarantaceæ*, by Prof. M. Seubert of Carlsruhe, with 26 plates; and this fascicle completes vol. v, part 1. There are 13 Brazilian genera; of which much the largest is *Gomphrena*, with 66 species. The species figured which concern the North American flora are *Alternanthera achyrantha* and *Amarantus hypochondriacus.*

9. *Das Haustorium der Loranthaceen und der Thallus der Rafflesiaceen und Balanophoreen;* von H. Grafen zu Solms-Laubach. Halle, 1875. 4to. The present paper is supplementary to an article on the vegetative organs of phanerogamic parasites which appeared in Pringsheim's Jahrbücher, Bd. vi. The writer divides his subject into three parts. The first is

devoted to a consideration of the modes of attachment of different species of *Loranthaceæ* to the foster plant. This is accomplished by the growth inward of suckers (*Saugfortsätze*) which penetrate through the bark to the wood. The shape which any sucker assumes depends on the relative activity of the growth of the sucker itself and of the cambium. In some cases, as *Loranthus Europæus* and *L. Sternbergianus*, the sucker sends out processes which penetrate into the wood itself. The writer confirms the suggestion made by John Scott that the vascular bundles of the parasites communicate with those of the plants on which they are growing.

Part II is devoted to the vegetative organs of the *Rafflesiaceæ*, which had previously been studied only in *Pilostyles Haussknechtii* Boiss. and *Cytinus Hypocistus* L. The writer gives the results of his examinations of *Pilostyles Æthiopica* Hook., *P. Blanchetii* Gardn., and *P. Caulotreti* Karst., which closely resemble one another. The vegetable organs of these species consists of threads or, at times, flat expansions which are found in the last and from which suckers are given off which penetrate into the wood. The name given to the thread-like expansions is thallus, from its resemblance to the structures of the same name in cryptogams. The flower buds are produced as adventitious offshoots from the threads of the thallus, and finally burst through the bark of the foster-plant. *Pilostyles Thurberi* A. Gray, a plant of our own country which is parasitic on species of *Dalea*, differs somewhat from other species of the genus. Its thallus, which is found in the inner bark, is not composed of threads but of flat expansions of considerable size. They are at first destitute of vessels, which, however, make their appearance about the time of the formation of the flower buds. Part III is devoted to the vegetative organs of the *Balanophoreæ*, and the writer concludes as follows: "It is the object of the present essay to call attention to the fundamental uniformity of the development and conformation of the assimilating organs of the phanerogamic parasites. This object has been attained if we have been successful in showing that they all have a common characteristic in the absence of any sort of differentiation of organs of vegetation such as we find in the *Cormophytes*, that their organs can be neither roots nor stems, but that we are compelled to recognize them as thalline structures equivalent and completely analogous to those of the Thallophytes. This would have pleased Lindley, as indicating a structural foundation for his class of *Rhizogens*. W. G. F.

10. *The Movements and Habits of Climbing Plants;* by CHARLES DARWIN, M.A, F.R.S., etc. 2d ed., revised, with illustrations. 208 pp. 8vo.—This work by Darwin, noticed at page 69 of this volume, has recently been republished by D. Appleton & Co., New York.

IV. Astronomy.

1. *A series of Astronomical Drawings for the Centennial Exhibition.*—A unique feature of the Centennial exhibition will be a series of thirty-six Astronomical drawings of interesting celestial objects, executed in pastel by L. Trouvelot, the artist who produced the series of Astronomical engravings undertaken by Professor Winlock at Harvard College Observatory. The pictures vary in size between eighteen by twenty-two inches, and twenty-three and one-half by twenty-eight and one-half inches, exclusive of the frames. The following have already been completed, viz: Nebula in Orion, Nebula in Andromeda, Horse-Shoe Nebula, Winged Nebula, Trifid Nebula, Ring Nebula, Dumb-Bell Nebula, Cluster in Hercules, Coggia's Comet, the planets, Mars, Jupiter and Saturn, Sun-Spots in full activity, Solar Protuberance eruptive form, Solar Surface with Chromosphere, Protuberances and Corona, Aurora Borealis, Group of Sun-Spots with bridges, Milky Way in two parts, Zodiacal Light, Shower of Shooting Stars, and Tempel's Nebula in the Pleiades. The original sketches have been for the most part made with an excellent refractor, of six and one-half inches aperture, mounted in Mr. Trouvelot's Physical Observatory at Cambridge. Their production has been a work of immense labor. From fifteen to twenty-five nights have been spent in the study of each nebula. The sketch of Tempel's Nebula in the Pleiades is the result of sixty-five hours' study. In the drawings of the Milky Way, the stars are plotted with considerable accuracy. Over a year was spent in the preparation of these two sketches. Of the sun-spots, protuberances, auroras and the zodiacal light, the most typical forms have been represented. In the shower of falling stars, every one represented was observed on the night of Nov. 13, 1869. It is Mr. Trouvelot's design to make these drawings available at the close of the exhibition, in producing a series of Astronomical Charts for educational purposes.

W. A. R.

2. *Our Place among Infinities;* by Richard A. Proctor. 324 pp. 8vo. New York, 1876. (D. Appleton & Co.)—This work consists of "A series of essays contrasting our little abode in space and time with the infinities around us," with also "Essays on the Jewish Sabbath and on Astrology." Mr. Proctor aims, in his various works, to put science, especially astronomical science, in an attractive form for the general reader. In presenting his subjects he does not always make it clear as to what are speculations and what known facts; but he is dealing with the marvellous, and this method in his hands makes things the more marvellous. His range of knowledge is considerable, and his style perspicuous and forcible. Astronomers would not accept of all his conclusions, neither would geologists, and probably not biblical critics. After perusing his note on the origin of crater-cavities on the moon's surface by the blows of meteorites, or the passage (p. 84) in which

he describes the encounter and destruction of a comet by a meteoroid stream, the reader will probably be led to question his judgment on other topics.

V. Miscellaneous Scientific Intelligence.

1. *Mt. St. Elias.*—In a notice of Mr. W. H. Dall's paper on Mt. St. Elias (from the Coast Survey Report for 1875) on pp. 77 and 78 of this volume, the remark is made that the views are evidently vertically exaggerated. The author has informed us that the proportions of the mountains are rightly given, from determinations by instruments. The view of Mt. St. Elias was taken from Yakutat Bay (Port Mulgrave), 53 [nautical ?] miles to the southeast. The southern face of the mountain, from a line about 5,000 feet above its base, is "an immense rock-face, inclined at an angle of 45° to the sea, and rising 8,000 to 10,000 feet without a break in its continuity." "The apex is pyramidal, sharp and clearly cut, leading to the inference that it is precipitous on the invisible northern side. The whole of the rock-face is marked by straight rigid lines of bedding, which are inclined uniformly to the eastward at an angle of about ten degrees." Mr. Dall concludes from its features and this appearance of stratification, that the mountain is not volcanic but consists, with the high range to which it belongs, mainly of non-volcanic crystalline rocks.

The height of Mt. St. Elias was determined trigonometrically by measurements from four stations, that at Port Mulgrave, 69 miles distant from Mt. St. Elias, and the others (off Lituya Bay, off Dry Bay, and at sea to the south-southwest) over 100 miles distant. The following are the angles of altitude from each station, and the calculated height:

1. From Port Mulgrave,	69·11	miles	distant,	2° 38′ 1″	19,464 feet.
2. Off Lituya Bay,	167·62	"	"	0° 9′ 5″·1	18,033 "
3. Off Dry Bay,	132·25	"	"	0° 50′ 8″·1	19,956 "
4. At Sea,	127·25	"	"	0° 47′ 34″	18,350 "

It is time that the geology and altitude of Mt. St. Elias were determined by observations made on the mountain itself.

J. D. D.

2. *Harbors of Alaska and the Tides and Currents in their vicinity;* by W. H. Dall, Assist. U. S. Coast Survey. From the Coast Survey Report for 1875; Appendix No. 10. *Report of Geographical and Hydrographical Explorations on the Coast of Alaska;* by W. H. Dall. Ibid; Appendix No. 11.—The first of these papers contains new facts on the tides, currents, ocean and land temperatures, hydrography, topography and other characteristics of the vicinity of Alaska and some of the Aleutian Islands. The Shumagin Islands, south of the extremity of the Alaska Peninsula, are described as composed of granite, various metamorphic rocks and sandstones, overlaid by Tertiary beds, "of which the upper beds contain fossiliferous layers of Miocene age, the lower ones containing remains of warm temperate vegetation, and the

uppermost remains of marine animals, including mollusks and cetaceans." There are also recent lavas.

The second paper contains, besides geographical and hydrographical observations, tables of magnetic declinations at positions among the Aleutian Islands, according to different observers, including new results obtained by the Coast Survey. From them it appears that there is a decrease of the easterly variation at the stations where observations have been taken, when the results are compared with those heretofore published. The following are some of the results obtained:

At Amchitka Island, Constantine Harbor, 51° 23′ 32″·9 N., 179° 12′ 12″·2 E., variation 7° 15′ 33″ E.

At Chichagoff Harbor, Attu Island, 52° 55′ 57″·23 N., 173° 12′ 22″·2 E., variation 7° 44′ 36″ E.

At Kyska Island, 51° 58′ 59″·11 N., 177° 29′ 46″·3 E., variation 11° 06′ 27″ E.

At Adaka Island, Bay of Islands, 51° 49′ 15″·6 N., 176° 51′ 58″·2 W., variation 13° 52′ 03″ E.

At Unalaska Island, Iliuliuk village, 53° 52′ 37″·7 N., 166° 31′ 36″ W., variation 18° 59′ 44″ E.

At Shumagin Island, Popoff Straits, 55° 19′ 16″·7 N., 160° 31′ 14″·1 W., variation 20° 29′ 23″·7 E.

3. *Memoirs of the Peabody Academy of Science*, Vol. I, No. 4. 94 pp. Roy. 8vo, with plates. Salem, Mass., Dec. 1875.—This fourth number of the "Memoirs" is occupied with a paper by the late Dr. Jeffries Wyman, on the Fresh-water Shell Mounds of the St. John's River, Florida.—The facts published by Dr. Wyman in former articles are here brought together along with the results of new observations by him, and they are presented with the usual thorough and cautious method of the author. The mounds are often five or six hundred feet in length, and vary from a few feet to eighteen or twenty in height. Dr. Wyman, after a full description of them, states as his conclusions, that, at the least, two or three hundred years, and probably more, have passed since they were finished; that the fact that the human bones are broken in the same manner as the bones of edible animals proves the makers to have probably been cannibals; that fragments of pottery, while common in the later mounds, are not found in the older; that stone implements are few in the older mounds and rudely made; that the shell heaps contain fragments of the Mastodon, Elephant, Horse, Ox, and some other extinct animals, but that these show by the changes they have undergone, that the animals were not cotemporaries of the mound-builders; that the only skull found differs from the skulls of the Indian burial mounds of the country, in being longer, with the ridges and processes more pronounced, and that among the bones of two other individuals the tibia was flattened; that, while it is uncertain whether the makers of the mounds were the same people that were found there by the Spaniards and French, the absence of pipes and pottery, and the rarity of ornaments, are consistent with the conclusion that they were a different people.

4. *Annual Report of the Chief of Engineers to the Secretary of War for the year* 1875. Part I, 990 pp. 8vo. Part II, 1254

pp.; each with many maps and illustrations.—The Annual Report of the Engineering Department is of high importance in a scientific point of view. Besides details as to work done in the improvements of harbors and rivers, and discussions of the methods of carrying on such improvements, it contains a great amount of new information, on the geography, resources and trades of the regions examined, results of hydraulic investigations, discussions of the modes of wear, transportation and deposition by rivers, the topography, and on the productions and resources of the territories, besides facts and views on other topics.

Among the articles in the Report for 1875, the following are especially noteworthy: Major Warren's Report on the Minnesota River, which is both historical, descriptive and geological, and contains a map showing the Mississippi when Lake Winnipeg was its head (this Journ., ix, p. 313); Commissioner H. L. Abbot's analysis of the Mississippi floods; Gen. T. G. Ellis's Report on the Connecticut River, in which the amount of discharge of the river at Hartford is given for each day, from Feb. 1, 1871, to Dec. 31, 1874, and, as an incidental result, the parabolic form of the curve of subsurface velocities in a river, as made known by Humphreys and Abbot (in their Report on the Physics and Hydraulics of the Mississippi), is fully confirmed by observations at Thompsonville; Col. Gilmore's Report on the compressive strength and specific gravity of the building stones in the United States in most general use; Report of Clarence King with reference to the geological exploration of the 40th parallel; Report of Lieut. G. M. Wheeler, on geographical explorations and surveys west of the 100th meridian, noticed beyond; and Col. Ludlow's Report on the expedition to the Black Hills, already noticed in this Journal.

5. *Annual Report upon the Geographical Explorations and Surveys west of the* 100*th Meridian;* by GEORGE M. WHEELER, 1st Lieut. of Engineers U. S. A. 196 pp. 8vo. Washington, 1875.—This report is included in the Annual Report of the Chief of Engineers for 1875, as above mentioned. Besides the Report on the Geographical, Geodetic, Hypsometrical, Astronomical and Meteorological work of the survey, this volume contains the following: a discussion on Aneroid barometers; a Report on the Geology of part of northwestern New Mexico examined in 1874, by E. D. Cope, containing, besides geological observations, descriptions of fossil vertebrates of the Santa Fé Marls, on the *Typothorax coccinarum* Cope, from beds supposed to be Triassic (already noticed in this Journal, III, vol. x, p. 153), on the Eocene plateau, and a list of fossil vertebrates from beds of the horizon of the Green River horizon; Geological and Mineralogical Report, by O. Loew, on portions of Colorado and New Mexico; Preliminary Botanical Report, by Dr. J. T. Rothrock; Report upon the Agricultural resources of northern New Mexico and southern Colorado, by Dr. O. Loew, in which several analyses of soils, plants, etc., are given; general itinerary by Surgeon H. C. Yarrow; Ornithological notes, by H. W. Henshaw, and also by Mr. C. E. Aiken; Report on the

Remains of population observed on and near the Eocene plateau of northwestern New Mexico, by E. D. Cope, illustrated by a number of wood-cuts giving plans of structures; a report on the ruins of New Mexico, by O. Loew, and also another, by Lieut. R. Birnie, Jr.; on the Pueblo Languages of New Mexico, and of the Moquis of Arizona, by A. S. Gatschet.

Dr. Loew's papers contain analyses of the basalt of Abiquin, of a zeolite, of garnets from the region of the "memorable diamond excitement," chrysolite, the green feldspar of Pike's Peak, soils and grasses.

6. *Geological Survey of the Territories, under the Interior Department*, Dr. F. V. HAYDEN in charge.—(1.) *New Publications to be issued during the year* 1876. The following publications connected with the U. S. Geological Survey of the Territories under the direction of Prof. F. V. Hayden, are in press, and will be issued during the year 1876: 1. The Invertebrate Palæontology of the Western Territories, by F. B. MEEK, making Volume IX of the quarto series. It will contain 600 pages of text and 45 plates; over 500 pages are already printed. 2. The Fossil Flora of the Lignitic Group, by LEO LESQUEREUX, making Volume VII of the quarto series. It is illustrated by 65 plates. 3. Monograph of the North American Rodentia, by Messrs. COUES and ALLEN, to constitute Volume X of the quarto series, and to contain numerous plates. 4. Monograph of the Geometrical Moths, by Dr. A. S. PACKARD, to constitute Volume XI of the quarto series. It will be illustrated by 13 plates, some of which contain from 75 to 100 figures. 5. Ethnography and Philology of the Hidatsa Indians, by Dr. WASHINGTON MATHEWS, U. S. A. This volume is now passing through the press, and will prove one of great interest; it will contain about 500 octavo pages. 6. Annual Report of the U. S. Geological Survey for 1874, in octavo, now in the press. 7. The Annual Report of the Survey for 1875, which will go to press about May 1st. 8. Bulletin of the Survey for 1876, Volume II. This volume will be issued in numbers, and will comprise about 200 pages of text, with 30 octavo plates. The ancient remains of Southern Colorado, Utah and Arizona will be described by Messrs. Holmes and Jackson. It will also contain an important paper on the ancient skulls, with numerous illustrations. Other volumes are in process of preparation, and may be printed before the end of the year.

(2.) *Descriptive Catalogue of the Photographs*, W. H. JACKSON, Photographer. The Catalogue of photographs of the Survey is enlarged in this edition by a list of those taken during the past year. These include three series: one, of the very unusual size of twenty by twenty-four inches; another, measuring five by eight inches; and a third, stereoscopic. The first, as we know from an examination of them, are of unusual beauty and perfection—the largest and grandest the Rocky Mountain region has yet afforded. Those of the second series, fifty-six in number, include many views of the ancient stone cliff ruins and cave towns of the San Juan region, besides others of the Moquis adobe villages, and many landscapes; and all are admirable specimens of the photographic art. The country owes much to the Survey under Dr. Hayden for the knowledge of the Rocky Mountain territories which has been distributed through the country by means of its numerous and excellent photographs, as well as through its Reports.

(3.) *Models.* To the Survey, the science of the country is indebted also for a model in plaster of the Elk Mountains. It is made on a scale of 1 inch to a mile, aud corresponds to an area of 200 square miles. One copy is to be colored to show the actual features of the region, and thus to exhibit its geological structure. The model has been prepared by the artist, W. H. Holmes. The same artist has executed a model of one of the two-story cliff houses of the San Juan Region, and another of a ruined village in southwestern Colorado. The cliff in the former has a height above the house of 200 feet vertically. (See Bulletin, 2nd Ser., No. 1, p. 20.) A model of a cliff house in Arizona has been made by the Photographer of the Expedition, Mr. W. H. Jackson, on a scale of six feet to an inch. The model is colored so as to represent exactly the appearance of the ruins. Still other models are in course of preparation. We learn that copies of these models will be furnished at cost to institutions desiring them.

7. *Specific gravity Balance of R. Parish.*—A balance, constructed on the same principle with that brought out by Mr. Parish, of Worcester, Mass., in the number of this Journal for last November, has been described and figured by President F. A. P. Barnard, in the second volume of Johnson's "New Universal Cyclopedia," published two or three months since in New York. It appears also that its author presented a paper on the instrument to the National Academy in November, 1874.

A charge of plagiarism on the part of Mr. Parish has been thrown out. The editors of this Journal deem it a duty to say that they know the charge to be without foundation. The paper presented to the National Academy has never been published, and even the editors knew nothing of it. Mr. Parish communicated with them on the subject of his balance first in February, 1875, more than a year since; and Prof. Thompson of the Institute of Industrial Science at Worcester has recently published the statement that Mr. Parish showed him a model of his balance in October, 1874, or before the time of the meeting of the National Academy above referred to. Moreover, Mr. Parish's paper in this Journal was in our hands a month before the publication of the 2nd volume of Johnson's Cyclopedia.

8. *Bulletin of the Bussey Institution*, Harvard University, Jamaica Plain. Part iv, pp. 285–372. 1875.—This fourth part of the Bussey Institution Bulletin contains the following papers: Applied Zoology; the importance of its study to the practical agriculturist, by D. D. Slade, M.D.; Report of the Director of the Arnold Arboretum, presented to the President and Fellows of Harvard University; A record of trials of various fertilizers upon

the plain-field of the Bussey Institution, results obtained in 1874, by F. H. STORER; the potato-rot, by W. G. FARLOW; A report on some Analyses of salt-marsh hay and of bog hay, by F. H. STORER; On the fodder value of Apples, by F. H. STORER.

In his memoir upon the composition of hay prepared from the natural grasses of the salt marshes on the seaboard, and of the fresh-water marshes, or "meadows," in the interior, Prof. Storer gives the following analyses:

Name of the Hay.	Water.	Ash (free from C and CO_2)	Albuminoids.	Carbohydrates, including fat.	Cellulose.	Dry Organic Matter.	Fat, *i.e.*, Matters soluble in Ether.
Better kinds of Salt Hay from brackish marshes,	8·23	7·06	7·47	44·53	32·71	84·71	2·94
Black Grass Hay (*Juncus bulbosus*),	8·71	5·19	6·79	46·15	33·16	86·10	2·30
Rush Salt Grass (mean of two samples),	8·65	6·74	4·63	46·67	33·31	84·60	1·76
The coarse Salt-Marsh Grass (*Spartina stricta*),	15·93	10·41	5·09	39·18	29·39	73·67	2·35
Bog Hay (*Carex stricta*) carefully cut and cured in June,	7·40	6·34	9·90	42·61	33·75	86·26	2.17
Bog Hay (*Carex stricta?*), taken from barns,	8·17	5·54	6·88	45·99	33·42	86·29	2·46
Dead Bog Hay collected in a field in December (*Carex stricta?*),	9·32	4·42	4·63	41·64	39·99	86·26	0·74
Common Rush (*Juncus effusus*) (taken from a barn),	6·88	2·63	6·75	42·26	41·48	90·49	
Flowering Fern (*Osmunda regalis*) (taken from a barn),	8·23	6·73	7·38	52·07	25·59	85·04	2·97
Buttercups (*R. acris*),	8·24	5·21	10·66	45·19	30·70	86·55	3·64
White Weed (*Leucanthemum vulgare*), cut in flower,	10·87	6·44	7·00	44·69	31·00	82·69	2·42
Beach-pea Vines (*Lathyrus maritimus*),	7·62	7·38	18·70	37·53	28·79	85·01	4·32

In conclusion, Prof. Storer discusses the economical value of rough, low grade hays as compared with the "English" or upland hays.

9. *American Museum, Central Park, New York.*—This Museum is rapidly becoming one of the first of the country in scientific value. With Prof. Hall's collection of fossils, and the addition soon expected of a suite of Barrande's Bohemian species, it will take the lead of all as regards Paleozoic paleontology. The Museum has also very large collections of birds, including the collections of Prince Maximilian of Neuwied and extensive selections from those of M. Verneaux of Paris, and others of shells, insects, etc. The city of New York appropriated $500,000 for a building, and part of it is now completed. Already the persons visiting the Museum occasionally number over 10,000 in a day. The Museum is under the general charge of Professor Bickmore, a former student of Professor Agassiz.

10. *Summer Schools of Zoology and Geology at Cornell University.*—These schools will commence soon after July 7th, and be continued for six weeks. In the Zoological department there will be instruction through lectures and laboratory work, by Prof. W. S. Barnard, in Mollusks, Radiates, Worms and Protozoans; by Mr. J. H. Comstock, in Insects and Crustaceans; by Prof. B. G. Wilder, in vertebrates, excluding Birds; by Dr. E. Coues, in Birds. Specimens, living or in alcohol, will be furnished the students for study, including "two specimens of *Amphioxus*, one for dissection and the other for preservation." Prof. Wilder will give information concerning the school to those desiring it. The Geological School is under Prof. T. B. Comstock. Instruction will be given by lectures, study of specimens, and by field excursions. Fee for each school, $30.00; $10.00 of it to be paid in April, or on the day of registration, and the rest in July, when the school opens.

11. *Annuaire De La Officina Central Meteorologica De Santiago De Chile*, 1873.—The third and fourth year of the Annuaire of the Central Meteorological Office in Chili gives in detail the observations made at 13 stations during 1871 and 1872; as also an appendix in which is found a very excellent monograph on the earthquake of the 7th of July, 1873, by J. I. Vergara. The preface to the volume, which extends through 280 pages, gives very complete catalogues of earthquakes since 1849, and reviews of the meteorological conditions as shown by monthly and annual means during 1870, 1871 and 1872. The whole constitutes an important addition to our scanty knowledge of the meteorology of that section of the world; and it is to be hoped that Vergara will soon be able to extend the duties of the Meteorological Office, so as to add, to these climatological studies, those other special investigations into atmospheric phenomena, which the peculiar nature of the territory of Chili especially invites. C. A.

Statement and Exposition of Certain Harmonies of the Solar System; by Stephen Alexander, LL.D., Professor of Astronomy, College of New Jersey. Smithsonian Contributions to Knowledge, No. 280. 96 pp. 4to. Washington, D. C., March, 1875.—Professor Alexander's Memoir does not admit of an abstract, and we therefore announce it only, referring to it for his arguments and conclusions.

Half-Hour Recreations in Natural History of Estes and Laureat, Boston. Half-Hours with Insects, by A. S. Packard, Jr. Part 8. pp. 225–256, 1875.

Half-Hour Recreations in Popular Science of Estes and Laureat. No. 16, the Ice age in Britain, by Prof. Geikie, and Causes of the degeneracy of Teeth, by Prof. H. S. Chase. pp. 105–136. 1875.

Förster, E. Geschichte der Italienischen Kunst. Vierter Band. 8vo, pp. 525. Leipzig, 1875. P. O. Weigel.

Wessely, J. E. Anleitung zur Kenntniss und zum Sammeln der Werke des Kunstdruckes. Mit zwei Tafeln Monogramme. 8vo, pp. viii, 338. Leipzig, 1875. P. O. Weigel.

Emile Kopp.—For the biographical notice of Prof. Kopp, on p. 80, this Journal is indebted to Dr. F. H. Storer.

OBITUARY.

George Poulett Scrope, the eminent author of works and memoirs on volcanoes, died on the 18th of January, at his residence near Cobhan, Surrey, at the age of 79 years.

APPENDIX.

ART. XXX.—*Principal Characters of the Tillodontia:* by O. C. MARSH. (With two plates.)

THE Eocene deposits of North America have yielded two new orders of extinct mammals, the *Dinocerata*, and the *Tillodontia*, both of great interest, and widely different from all known groups, as well as from each other. The latter order, recently established by the writer,* is comparatively little known, as the animals representing it are of moderate size, and but few of their remains have yet been discovered. The typical genus of this order is *Tillotherium*, the more important characters of which can now be readily determined from specimens in the Yale Museum. This genus, therefore, will be mainly used in the present article to illustrate the order.

Tillotherium Marsh, 1873.†

The skull in this genus resembles in its general form that of *Ursus.* It is of moderate length, much elevated in the frontal region, and with the zygomatic arches widely expanded. (Plate VIII.) The posterior portion of the cranium is depressed, and much constricted behind the fronto-parietal suture. The temporal fossæ are large, and separated by an obtuse sagittal crest. There is no postorbital process. The frontal bones are large, and inflated with air cavities. The nasals are elongate, broad posteriorly, and narrow in front, where they unite with the premaxillaries. The latter are massive, and project forward beyond the nasals. They are united only by a slender bridge of bone, below the anterior narial aperture.

The orbit is confluent with the temporal fossa, which is largely formed below by the squamosal. The latter sends outward and forward a strong zygomatic process, and, downward, a short, obtuse, post-glenoid tubercle, which bounds in front the external auditory meatus. This opening is bounded behind by the posttympanic process of the squamosal, which unites directly with the paroccipital. The tympanic portion of the periotic does not reach the external surface. The articular face for the condyle of the lower jaw is but very slightly concave. (Plate IX.) The malar bone is slender, and forms the anterior

* This Journal, vol ix, p. 221, March, 1875. † Vol. v, p. 485.

portion of the zygomatic arch. The lachrymal is of moderate size, and is perforated by its foramen in front of the anterior border of the orbit. The infra-orbital foramen is large.

The palate is broad behind, narrow in front, and somewhat excavated. The anterior palatine foramina are confluent, and are enclosed between the premaxillaries and maxillaries. The posterior palatine foramina are in the latter bones, near the first premolars. The posterior nares are behind the last upper molars. The occipital condyles are small, and sessile. The exoccipitals are perforated by a condylar foramen of moderate size. There is no auditory bulla, but in its place, an irregular opening, partially occupied by the periotic. There was a distinct alisphenoid canal.

The brain cavity in *Tillotherium* is small, but proportionally larger than in *Dinoceras.** The size of the brain compared with the entire skull is shown in the accompanying cut, fig. 1.

1.

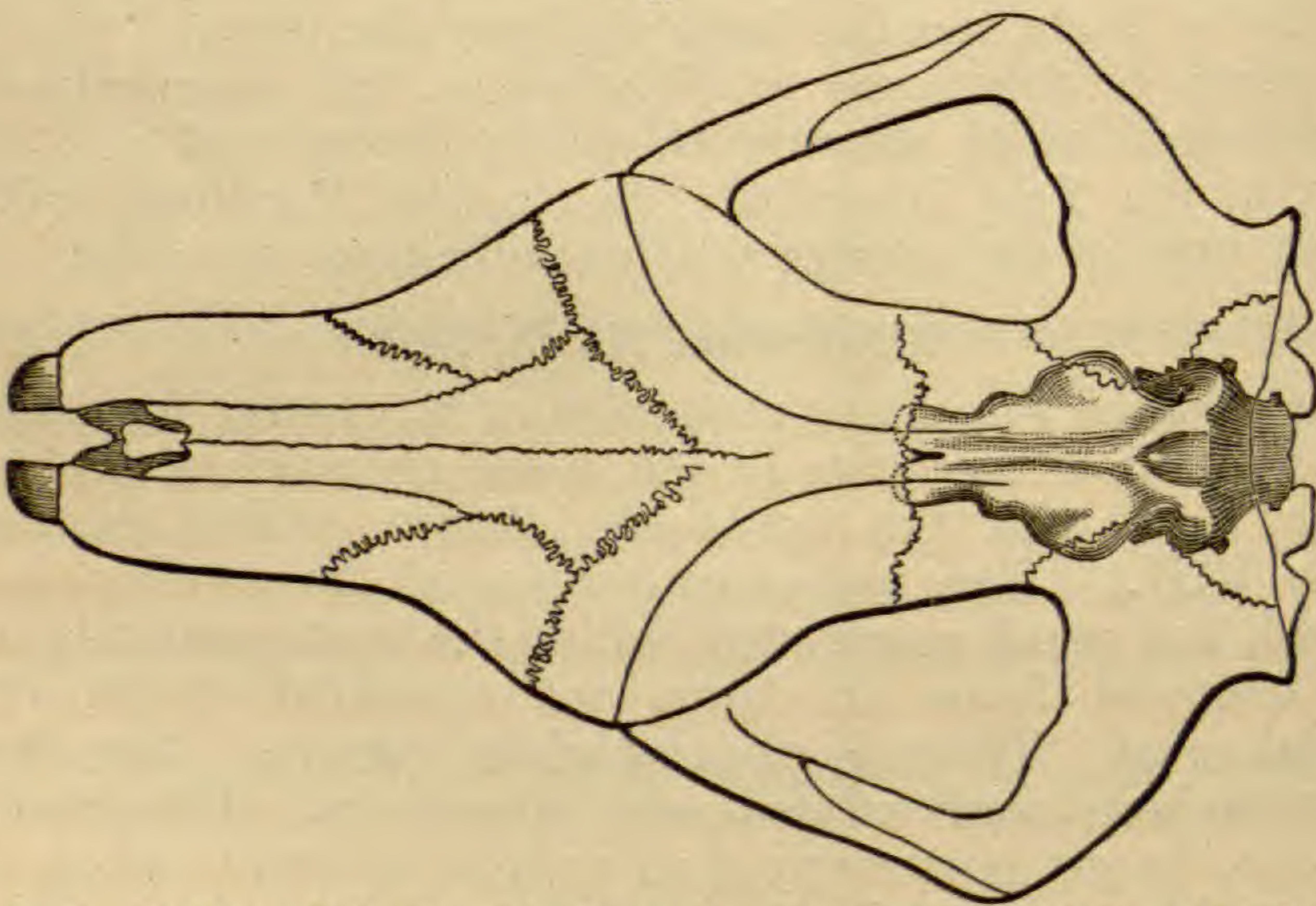


Figure 1. Outline of skull and brain cavity of *Tillotherium fodiens* Marsh. Top view. One-fourth natural size.

As in most, if not all, Eocene mammals, the cerebral hemispheres were small, and did not extend over the cerebellum or olfactory lobes. The latter were large, and projected well forward. The hemispheres were evidently more or less convoluted. There was no distinct tentorial ridge. The cerebellar fossa is large, much expanded transversely, and elevated above the cerebral cavity. There is a shallow pituitary fossa, and no clinoid processes. The exit for the optic nerve is quite large.

The adult dentition of *Tillotherium* is represented by the following formula:

$$\text{Incisors } \frac{2}{2}; \text{ canines } \frac{1}{1}; \text{ premolars } \frac{3}{2}; \text{ molars } \frac{3}{3}.$$

* This Journal, vol. ix, p. 165, Feb., 1876.

The two anterior upper incisors are large and scalpriform, and faced in front with enamel. They grew from persistent pulps, and strongly resemble the corresponding teeth in Rodents. (Plate IX, figure 1.) The upper canines were quite small, and separated by a diastema from the first premolar. In the upper true molars, the fore and aft diameter is much less than the transverse, and the crowns are very short. The form of these teeth is well shown in Plate IX, figure 4, which represents a nearly unworn last upper molar, natural size.

The lower jaw in *Tillotherium* is elongate and massive, and the symphysis is completely ossified. The condyle is broad, convex transversely, and raised above the line of the teeth. The coronoid process is stout, and of moderate height. The angle is thin, and not inflected. The anterior incisors are large and scalpriform, and faced in front with enamel. The canine was quite small. The lower molar series is of the *Palæotherium* type, and the last lower molar has a well developed third lobe.

The vertebræ of *Tillotherium* resemble those of some carnivores. The cervicals are short, and the ends of the centra nearly flat. The dorsals are of moderate length, and also amphiplatyan. The lumbars are quite large. The humerus is stout, and broad transversely at the distal end, which has a supra-condylar foramen. The radius and ulna are separate, and of nearly equal size. The radius is short, and both ends are expanded transversely, indicating but little rotation. The scaphoid and lunar bones are distinct,* and the pisiform is large and stout. The feet were plantigrade. There were five digits in the manus, the first being well developed. The metacarpals are short, and the terminal phalanges long, compressed and pointed, somewhat similar to those in the Bears. (Plate IX, figure 3.) The femur is of moderate length, and its head has a pit for the round ligament. There is a well marked third trochanter. The distal end of the femur is compressed in a fore and aft direction. The tibia and fibula are distinct, and the latter is curved and slender. The calcaneum is elongate, and the astragalus, depressed, with only a slight superior groove. The hind feet were plantigrade, and the five digits were similar to those of the manus.

The remains of this genus at present known are from the Eocene of Wyoming. The specimens preserved indicate animals from one-half to two-thirds the size of a tapir.

Yale College, New Haven, Feb. 18, 1876.

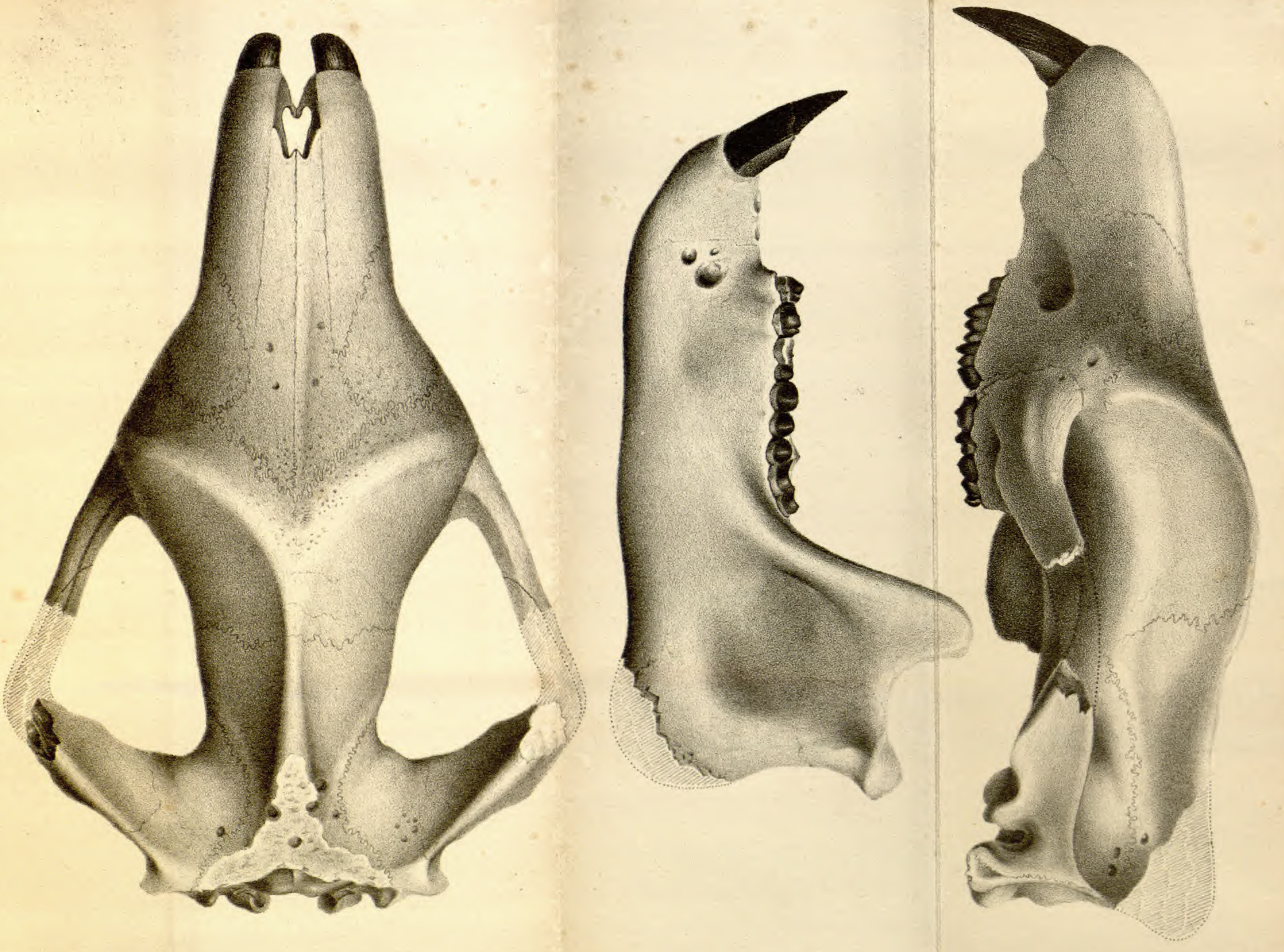
* The scaphoid and lunar bones have not yet been found united in any Eocene mammal.

[To be continued.]

EXPLANATION OF PLATES.

Plate VIII—*Tillotherium fodiens* Marsh. Figure 1, side view of skull; figure 2, lower jaw, side view; figure 3, top view of skull. All one-half natural size.

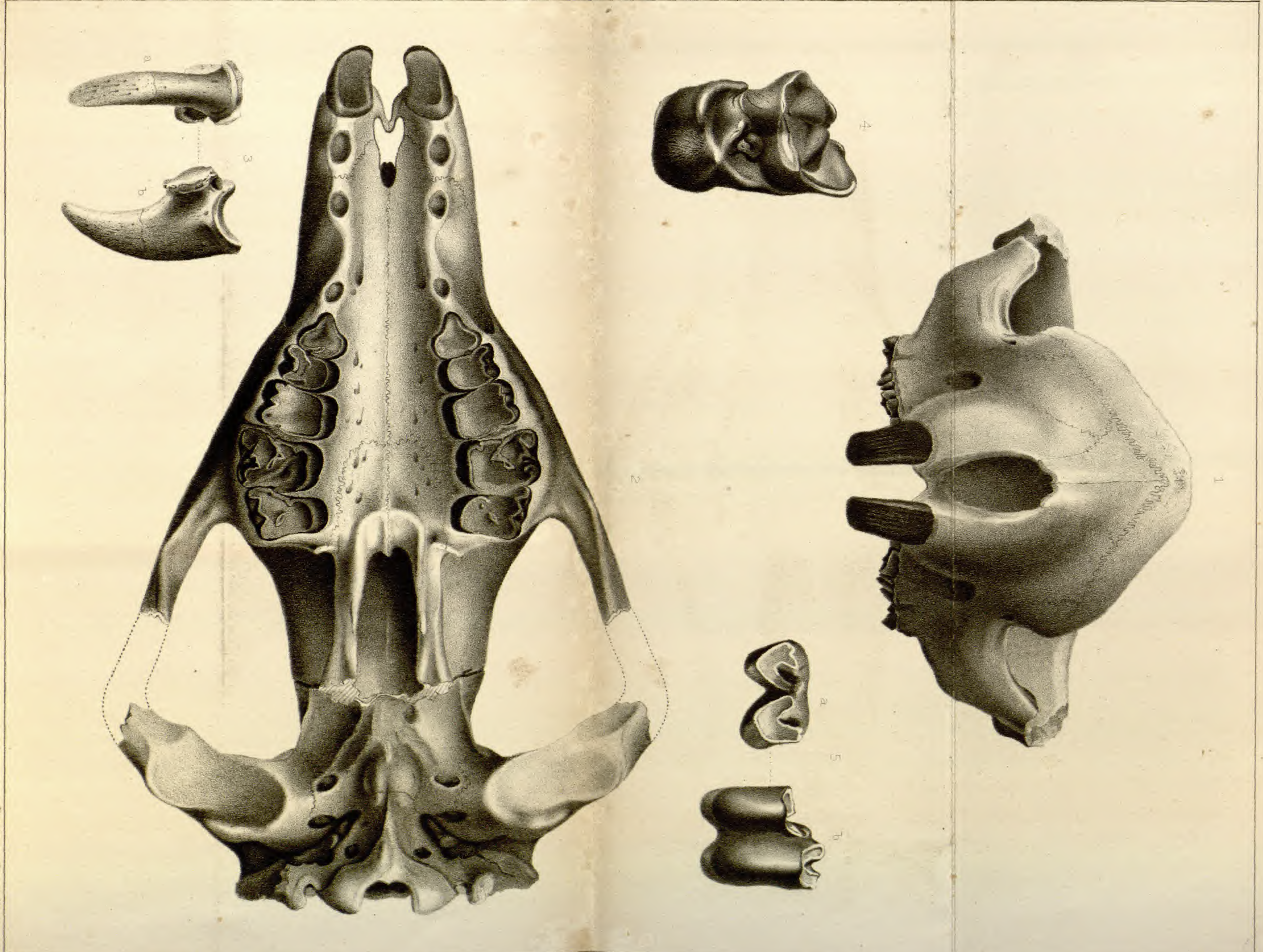
Plate IX—Figure 1. *Tillotherium fodiens* Marsh. Front view of skull: One-half natural size; figure 2, bottom view of same skull, one-half natural size; figure 3, ungual phalanx of same specimen; *a*, front view; *b*, side view; natural size. Figure 4, last upper molar of *Tillotherium latidens* Marsh, natural size. Figure 5, lower molar of *Anchippodus minor* Marsh. (*Trogosus castoridens* Leidy) natural size.



Drawn from nature by F. Berger.

TILLOTHERIUM FODIENS Marsh. ½

Lindgren & Chesnut New Haven Ct.



TILLOTHERIUM, Marsh.

Drawn from nature by E. Crisand.

Pendleton & Crisand, New Haven Ct.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

Art. XXXI.—*On the Gases contained in Meteorites;* by Arthur W. Wright, Professor of Molecular Physics and Chemistry, in Yale College.

In an article published by the writer in this Journal, for July, 1875, an account was given of an examination of the gases obtained from the meteorite of Iowa County, Iowa, which fell on February 12, 1875. This meteorite is of the ordinary stony kind, containing 12·54* per cent of nickeliferous iron, and the investigation was undertaken chiefly with a view to ascertain whether the spectrum of the gases evolved from such a body, by the application of heat, would afford any information respecting the recent theories connecting such meteorites with the comets. An analysis of the gases obtained at moderate temperatures developed the unexpected fact that their chief constituent was carbon dioxide, with a small proportion of carbonic oxide, these two gases constituting more than nine tenths of the product evolved at a temperature of 250°, and nearly one half of that given off when the heat was just below redness. As was to be expected from such a composition, the spectrum obtained from the earlier portions of gas given off was chiefly that of the carbon compounds, and showed a very close resemblance to those of several of the comets.

Among the conclusions drawn from the investigation, it was stated, that the nature of their gaseous contents establishes a marked distinction between the stony meteorites and the irons

* Analysis of Prof. J. L. Smith, this Journal, III, x, p. 362.

"hitherto examined," provided the Iowa meteorite could "be taken as a representative of its class."* With a view to obtain data for a more extended comparison, the investigation was continued, and a number of meteorites of both classes examined. The results of this work are given below, and it will be seen that they tend to justify completely the conclusions in my former paper, so far as any limited number of determinations could do so.

The method of experiment was the same as that described in the former paper, except in some of the minor details, and need be but briefly described here. The specimen to be examined was placed in a tube of very hard and refractory glass, which was merely softened at a red heat, and which, when filled with the meteoritic substance, could be maintained for a long time at this temperature without yielding more than so much as merely to deform the tube. In no instance was air admitted by the cracking or drawing in of the hot glass. The air was exhausted and the gas collected by means of a Sprengel pump of such perfection that it would produce a vacuum of but a fraction of a millimeter, and maintain it for days unchanged. The specimen tube having been attached to the pump, the latter was set in action and kept running until the air was thoroughly removed, as could be seen by the state of the gauge. The meteorite was then heated cautiously and the gas pumped out into the tube in which it was to be examined. Further details of the mode of procedure, where varied in the different cases, will be given in their appropriate places.

The problem of determining the exact nature and relative proportion of the gases in a meteorite is less simple than it might at first sight appear. For not only, as Grüner has shown,† is metallic iron attacked by carbon di-oxide, but it also, in the presence of this gas, or other oxidizing agents, determines the reduction of carbonic oxide, and its disappearance therefore from the gaseous products. In the case of the stony meteorites the question is still more complicated, as there is always present a greater or less quantity of oxide of iron, which at an elevated temperature must exert no inconsiderable influence upon the constitution of the gaseous mixture obtained from the mass. Grüner's very careful experiments showed that pure carbonic oxide progressively reduces the oxide of iron, at a temperature of 400° C. On the other hand it is itself reduced by metallic

* This conclusion has been criticized as hasty by Prof. J. W. Mallet (this Journal, III, x, 206), and a second one by M. M. Delafontaine, (Bibliothèque Universelle, Oct., 1875, 188), both of whom have overlooked or ignored the fact that they were given as merely provisional, conditioned upon the assumed general agreement of other iron and stony meteorites, as respects the gases derived from them, with those to which the statement referred.

† Comptes Rendus, lxxxiii, p. 28, et al., lxxiv, p. 231, etc.

iron, with a deposition of pulverulent carbon, though the action is very slight at temperatures less than 400° C. The commission who reported upon his memoir, in repeating some of his experiments, found that the temperature must exceed 350° in order that this effect may be produced at all. At higher temperatures the action is very marked. More recently Sir I. Lowthian Bell, in his work containing the results of a very elaborate and admirable series of researches upon the mutual action of the two oxides of carbon in the presence of metallic iron and oxide of iron,* has, in the main, confirmed Grüner's conclusions, but has shown that the results vary, not only with the temperatures, but also with the relative proportion of these substances present. He found that pure carbonic oxide begins to reduce Fe_2O_3 at from 140° to 200° C., according to the substance used, while at a moderate red heat the oxygen is rapidly removed, with the formation of carbon di-oxide. On the other hand the latter gas was partially reduced by spongy iron at a low red heat, with the formation of carbonic oxide. We have further to consider the action of the hygroscopic moisture upon the metallic iron, as well as the mutual action of hydrogen and oxide of iron, at elevated temperatures.

It is very evident then that the composition of the gases obtained at or above the temperature of red heat cannot be considered to represent accurately the true constitution of the gaseous contents of a meteorite, and especially is this true in the case of the stony ones. On the other hand we can hardly assert with confidence that the different gases are expelled in exactly their proportionate amounts at all temperatures. In fact the experiments show that the proportions of the gases vary with the temperatures of their evolution in a manner not satisfactorily explainable on the assumption that such an effect is due to chemical action alone. It is important therefore that the experiments should be conducted in such a way as to facilitate as much as possible the evolution of the gases, while at the same time they are exposed for as short time as possible to the action of high temperatures. The first of these conditions is attained in a good degree by reducing the material examined to a state of minute subdivision. The second is approximated by continuing the application of the high temperatures for the shortest time consistent with a satisfactory effect in driving off the gases sought.

In the case of the iron meteorites the material was generally prepared by boring out the solid iron with a steel drill upon a lathe, the substance being rendered as fine as possible. In

* Chemical Phenomena of Iron Smelting.

some instances this was not practicable from deficiency of material, and chips produced by a planing machine were used. The stony meteorites were reduced to powder in a diamond mortar. The iron contained in them being for the most part in very minute particles no further operation was necessary in this case. The powder from the irons, when the tube containing it was deprived of air, gave off a small quantity of gas from the mere diminution of pressure, without the application of heat, in one instance enough having been evolved to allow of its collection in a tube. A qualitative examination of it showed that hydrogen and the oxides of carbon were present, leaving no doubt that the mere pulverization of the iron was sufficient to cause it to part with a portion of its gaseous contents at ordinary temperatures, and greatly to facilitate the process at higher temperatures.

The heat was applied by means of a Bunsen burner, carried slowly back and forth beneath the tube, which was wrapped with wire gauze. For the irons the temperature was carried, in the first instance, to a point below redness, in order that the action of the iron upon the gases should be as little as possible. It was about 500° C. The gauge was watched during the heating and, as soon as it ceased to rise perceptibly, the flame was slowly withdrawn, and the gas at once pumped out. The evolution of gas, at this temperature, generally ceased very nearly in twenty or thirty minutes. After the gas was thoroughly removed, the iron was heated to redness with a cluster of four Bunsen burners, the heat being continued as long as any considerable amount of gas appeared to come away. This required usually but thirty or forty minutes, though in one or two instances it was continued somewhat longer. It will be seen from the results given below that the larger portion of the gas was obtained at the lowest temperature, in every instance but one.

The iron meteorites examined were the following: First, that from Tazewell Co., Tennessee, described by Professor J. L. Smith, in this Journal, II, xix, 153. Its composition is Fe, 83·02; Ni, 14·62; other substances, 1·93. No carbon was found. Specific gravity 7·9.

Second, that of Shingle Springs, Eldorado Co., California, described by Professor B. Silliman, this Journal, III, vi, 1. It contains Fe, 81·48; Ni, 17·17; C, 0·07, other substances, 1·27. Sp. gr. 7·875.

Third, the meteorite of Arva, in Hungary, noticed in this Journal, II, viii, 439. The analysis of A. Löwe gives Fe, 90·471; Ni, 7·321; residuum of carbon, silica, and cobalt, 1·404. Sp. gr. 7·814. Another analysis by Bergemann, Pogg. Ann., c. 256, gives for its composition, exclusive of the sulphide of iron contained in it, Fe, 82·25; Ni, 8·12; Co, 0·364; P, 0·74; C, 1·54; Graphite, 2·00.

Fourth, the great Texas meteorite in the cabinet of Yale College, described by Professor C. U. Shepard, this Journal, I, xvi, 216, also, with an analysis, by Professors B. Silliman, and T. Sterry Hunt, this Journal II, ii, 370. It contains Fe, 90·91; Ni, 8·46; residue containing carbon, 0·50. Sp. gr. 7·543.

Fifth, that from Dickson Co., Tennessee, described by Professor J. L. Smith, this Journal, III, x, 349, and examined at his request. It contained Fe, 91·15; Ni, 8·01; Co, 0·72; Cu, 0·06. Sp. gr. 7·717.

The following table gives the results obtained, the numbers expressing parts in one hundred. The numbers in the third line in each case give the percentage of each gas in the total amount obtained. They are not the simple averages of the numbers above them, but the means reduced according to the volumes in each case. The totals in the last column are the sums of the volumes given off at the different temperatures.

Name.	Temperature.	CO_2.	CO.	H.	N.	Volumes.
Tazewell Co.,	500°,	18·34	38·45	41·51	1·70	1·87
	Red heat,	7·76	45·75	44·76	1·73	1·30
	Total,	14·40	41·23	42·66	1·71	3·17
Shingle Springs,	500°,	19·98	13·52	60·92	5·58	0·65
	Red heat,	1·10	10·39	84·40	4·11	0·32
	Total,	13·64	12·47	68·81	5·08	0·97
Arva,	500°,	18·20	38·72	40·62	2·46	8·89
	Red heat,	11·25	74·59	12·84	1·32	38·24
	Total,	12·56	67·71	18·19	1·54	47·13
Texas,	500°,	9·76	8·43	81·81	----	1·10
	Red heat,	2·18	48·58	49·24	----	0·19
	Total,	8·59	14·62	76·79	----	1·29
Dickson Co.,	Total,	13·30	15·30	71·40	----	2·2

The small quantity of the iron available in the examination of the Dickson Co. meteorite rendered it necessary to be content with a single heating to redness. The iron was in the form of coarse chips which were cut by a planing tool. The same was true of the Shingle Springs iron, and this accounts in part for the smaller volume of gases obtained in these two cases.

We may add to this list the Lenarto iron examined by Professor Graham,* and the meteorite of Augusta Co., Virginia, the gases from which were analyzed by Professor J. W. Mallet.† The former yielded CO, 4·46; H, 85·68; N, 9·86, the whole amount of gas being 2·85 times the volume of the iron. The

* Proc. Royal Soc., xv, 502. † Ibid., xx, 365.

latter gave CO_2, 9·75; CO, 38·33; H, 35·83; N, 16·09, and 3·17 volumes of gas in all. In both these instances the iron was very strongly heated, the temperature in the latter case being carried nearly to whiteness, and continued for several hours. The volume of gas was divided into three parts, and the portions obtained at the beginning, middle, and end of the operation separately analyzed. Reducing the volumes given by Professor Mallet for each of the gases in these portions to parts in one hundred, we have the following numbers:

	CO_2.	CO.	H.	N.
Beginning,	15·09	30·74	42·52	11·65
Middle,	4·23	46·12	43·64	6·01
End,	3·69	47·00	13·36	35·95

The percentages in the total amount of gas obtained are given above. It will be seen that the results for the first two portions closely resemble those given for the Tennessee iron in the table.

In the experiments with meteorites of the stony class the same method, in general, was pursued, except that the first temperature was somewhat lower, being about 350°. This was adopted in order to lessen as much as possible the chemical action of the substances upon each other, and at the same time because the relative proportions of the amounts of gas obtained at this and the higher temperature were more convenient for the analyses.

The meteorites examined were the following: First, that from Guernsey Co., Ohio, which fell on May 1, 1860, and is described by Professor J. L. Smith, in this Journal, II, xxxi, 87. It contains 10·7 per cent of nickeliferous iron, and has a specific gravity of 3·55.

Second, one from Pultusk, in Poland, which fell on January 30, 1868. This was subjected to an elaborate investigation, and described, by Dr. G. vom Rath.* Several thousand small masses were collected, of which, some examined by vom Rath were found to contain 10·06 per cent of nickeliferous iron, though other specimens analyzed by Werther and Rammelsberg gave 21·08, and 21·78 per cent respectively.† It resembles somewhat the Iowa stone in its general character, and has a specific gravity of 3·725. The writer is indebted to the courtesy of Professor G. J. Brush, who sacrificed an excellent specimen for the examination, from his private cabinet.

Third, the meteorite of Parnallee, India, Feb. 28, 1857, found by Pfeiffer‡ to contain 6·84 per cent of meteoric iron. It has a specific gravity of 3·44.

* Festschrift der Niederrheinischen Gesellschaft für Natur- und Heilkunde zum 50jährigen Jubiläum der Universität Bonn.

† C. Rammelsberg, Die chemische Natur der Meteoriten. Abhandlungen der Königl. Akad. der Wissenschaften zu Berlin, 1870.

‡ Wien. Akad. Ber. 47.

Fourth, the meteorite of Weston, Conn., which fell Dec. 14, 1807. This is one of the most interesting meteorites known, and is remarkable both for its lithological character, and for the large amount of iron contained in it, this being estimated as from 30 to 40 per cent. Its specific gravity is 3·6.* These, together with the Iowa County meteorite, all belong to the class of chrondrites, of G. Rose, or sporadosidères, of Daubrée, and are good representatives of the ordinary or most numerous class of the stony meteorites.

In the examination of the Iowa County meteorite already referred to, the determinations were made for a number of different temperatures, the results being as follows:

	At 100°.	At 250°.	Below red heat.	At low red heat.	At full red heat.
CO_2	95·46	92·32	42·27	35·82	5·56
CO	0·00?	1·82	5·11	0·49	0·00
H	4·54	5·86	48·06	58·51	87·53
N	0·00	0·00	4·56	5·18	6·01
	100·00	100·00	100·00	100·00	100·00

The separation of the gaseous volume into so many small portions rendered the estimation of minute quantities of any constituent less certain, and it is probable that the percentage of the nitrogen, which was estimated as a residue, may have had thus set down to it, besides the errors of the determinations, very small amounts of carbonic oxide and possibly of marsh gas, which was found in all the cases in the present investigation. But they were at all events too small to be certainly distinguished from errors of observation. In the re-examination of this meteorite for carbon di-oxide mentioned below, the nitrogen was directly determined in gas given off after exposure to a red heat for a considerable time, and corresponding nearly to the portion referred to in the last column of the above table. The amount found was 3·41 per cent. But no great stress should be laid upon such a discrepancy, considering the manner and the purpose of the preceding determination. The latter determination agreed with the former as to the absence of carbonic oxide and marsh gas, at that temperature.

The results obtained for the different cases are shown in the following table. The numbers given for the Iowa County meteorite are reduced from the former analysis, the volumes being obtained from the notes made at the time, and 500° being assumed as approximately representing the temperature there given as "below red heat." The first temperature in the case of the Ohio meteorite was also 500°, this being the first one determined. The second heating was also continued for a longer time, which accounts for a slight difference between this

* B. Silliman, Sen., Memoirs Conn. Acad., vol. i, p. 142.

and the other cases. As it was found that this degree of heating left too small a proportion of the gas for the second determination, as also for other reasons mentioned above, the temperature of about 350° was employed in the succeeding experiments:

Name.	Temperature.	CO_2.	CO.	CH_4.	H.	N.	Volumes.
Ohio,	500°,	82·28	2·16	2·26	12·37	0·93	2·06
	Red heat,	16·79	8·71	1·66	69·43	3·41	0·93
	Total,	59·88	4·40	2·05	31·89	1·78	2·99
Pultusk,	350°,	81·01	1·99	1·73	13·36	1·91	0·99
	Red heat,	33·97	7·35	6·00	49·99	2·69	0·76
	Total,	60·29	4·35	3·61	29·50	2·25	1·75
Parnallee,	350°,	87·53	1·13	1·22	8·72	1·40	1·56
	Red heat,	72·43	2·53	3·22	20·03	1·79	1·17
	Total,	81·02	1·74	2·08	13·59	1·57	2·63
Weston,	350°,	86·29	1·84	1·19	8·59	2·09	2·69
	Red heat,	62·18	3·43	3·10	28·16	3·13	0·80
	Total,	80·78	2·20	1·63	13·06	2·33	3·49
Iowa,	500°,	58·04	4·01	0·0	34·82	3·13	1·04
	Red heat,	19·16	0·21	0·0	74·49	6·14	1·46
	Total,	35·44	1·80	0·0	57·88	4·88	2·5

The heat was continued, in the case of the Iowa meteorite, longer than in the subsequent experiments with the others, and the result shows a greater diminution in the amount of carbon di-oxide obtained. Rejecting the last column in the analysis quoted above, we have for the total average percentage up to red heat, CO_2, 49·51; CO, 2·64; H, 43·93; N, 3·92, which corresponds more nearly with the results in the other cases. The numbers given in this table show a very satisfactory concordance, though there are slight differences, doubtless arising from the fact that the temperatures employed, and the times of exposure to the heat, though approximately the same in the different instances, could not be made absolutely identical. The mass of material operated upon was also not always the same, which would produce a slight difference in the time required for the evolution of the gas, and the completeness of its elimination.

It will be observed that a small amount of marsh gas was found in each of the portions of gas obtained in the present investigation. This might possibly be accounted for, in the case of the higher temperatures, by the decomposition of organic matter taken up by the meteorite subsequently to its fall, or of carbonaceous matter originally contained in it; but as such decomposition would not be likely to take place, to any great extent, at so low a temperature as 350°, there is reason for believing that it is really one of the constituents of the meteoritic gases. The determinations made both by absorption of the carbonic oxide with cuprous chloride, and by the production of carbon di-oxide after the explosion with oxygen, agreed very well, and the analyses in each case were best satisfied by the assumption of the amounts of marsh gas indicated in the table. The Ohio meteorite was also examined at a number of different temperatures, the different portions of gas having the following proportions of carbon di-oxide: at 100°, 95·92; at 250°, 86·36; at 500°, 82·28; at incipient red heat, 33·55; at red heat, 19·16, showing a progressive decrease similar to that observed in the case of the Iowa meteorite.

On comparing the results given in the two tables a marked difference is at once evident. Not only do the stony meteorites give off a much larger volume of gas at low temperatures, but the composition of it is in all the cases examined quite distinct from that of the gas evolved from the irons. In no case among the results obtained from the latter is the amount of carbon di-oxide greater than 20 per cent at 500°, nor than 15 per cent from the whole quantity evolved, while in every case but one the volume of carbonic oxide is considerably larger. In the chondrites, on the other hand, the percentage of the latter gas is conspicuously small, while the carbon di-oxide is more than half of the total quantity of gas obtained up to red heat, except in the case of the Iowa meteorite, and in that the percentage is not much less, especially if we reject the numbers in the last column above, for the amount obtained by a second and long-continued application of red heat. At a temperature of about 350°, it constitutes from 80 to 90 per cent of the gaseous products, in all cases, while at the heat of 100° C. it forms somewhat more than 95 per cent of the gas evolved in the only two cases examined in this respect. The hydrogen, on the other hand, progressively increases in quantity with the rise in the temperature of evolution, and in the last portions given off at red heat is generally the most important constituent. Its proportion in the total percentage would, no doubt, be considerably increased if the heat were greatly intensified, as for instance, if carried to a point approaching whiteness, but the results obtained in such a way would be entirely unreliable, from the action of the metallic iron and the oxide of iron on the carbon compounds, or upon the hydrogen itself.

In the examination of the Parnallee, Pultusk and Weston meteorites, a small quantity of the moisture given off at a high temperature was collected in a glass tube attached to the pump and surrounded with a freezing mixture. This, when tested, gave distinct traces of chlorine for the Parnallee and Weston, but that from the Pultusk seemed to contain little or none. The latter however, as well as the Parnallee, showed the presence of a minute quantity of sulphurous oxide, the Weston meteorite less certainly.

A question naturally suggests itself as to the manner of the occurrence of the carbon di-oxide in conditions which admit of its being separated so much more readily than the other gaseous substances. The most probable supposition seems to be that it is condensed upon the fine particles of iron as well as absorbed within them. That it is produced by the decomposition of some carbonate is not likely to be the case, since the carbonates that could occur in meteorites all require high temperatures for the evolution of this gas, and the quantities obtained should increase constantly with an increase of temperature, whereas the reverse is true; and certainly none of them would give up the gas at the temperature of boiling water. Another hypothesis might be that it is absorbed in part from the atmosphere. To test this, a re-examination of the Iowa meteorite was made, the material being heated until it yielded as nearly as possible the same volume of gas as in the experiments of the preceding year, a short time after its fall. Had it been constantly gaining carbon di-oxide from the air it should have given the same amount of gas as before at a lower temperature. On the contrary it required a more intense heating, and a longer continuance of the process. The percentage of CO_2 was found to be 32·65. If any difference exists therefore it has lost rather than gained, at least in this interval of nearly a year. It is very probable therefore that no considerable part of the gas is derived from the atmosphere, though this cannot be asserted absolutely, and the question must remain for further investigation. The portions of gas from each of the stony meteorites, except the Pultusk, which was not examined, gave cometary spectra, similar to that from the Iowa specimen.

On reviewing the results of the investigation there appears no reason for modifying the conclusions arrived at in the former article. The evolution of such volumes of carbon di-oxide may well be taken as a characteristic of the stony meteorites, and its relation to the theory of comets and their trains is certainly of great significance. The further discussion of some of the results of the investigation, and certain interesting questions suggested by them, are reserved for another communication.

Yale College, March 18, 1876.

ART. XXXII.—*Review of Croll's Climate and Time with especial reference to the Physical Theories of Climate maintained therein;** by SIMON NEWCOMB.

THE present notice of Mr. Croll's work is confined to an examination of his physical theories of climate, avoiding all those portions which have a geological bearing. The physical theories propounded have two distinct applications; the one to the present climate of the earth; the other to the changes of that climate during past geological ages. In the latter department of the work the principal object is to account for the glacial epoch or epochs, the author conceiving that there may have been several such epochs. The data from which his conclusions respecting the past are derived are necessarily founded on his theories of the causes of present climate, since it is only by a thorough discussion of the way in which all climatic causes operate, and by tests of all the conclusions by a comparison with the present climate of the globe, that any safe rules can be formed for judging of the climate of the past.

We are forced to say at the outset that the physical data for forming a reliable estimate of the separate effects of various causes on climate are almost entirely wanting. The physical theory of cosmical heat is, at the present time, in a state nearly approaching the chaotic, a circumstance all the more surprising when we consider the advanced state of other departments of the theory of heat. Cournot and his successors have devoted to the mechanical theory of heat an amount of profound research which has made it a branch of the most exact of the sciences. On the other side, Melloni and his successors have done a great deal for what we may call the chemical theory of heat. Between these two lie the physical theory, as affecting climate and cosmical temperature, which has, comparatively speaking, been neglected entirely. To illustrate what we mean let us consider the temperature of the earth from the widest point of view. Practically, there is but one source from which the surface of the earth receives heat, the sun, since the quantity received from all other sources is quite insignificant in comparison. There is but one way of losing heat, by radiation into space. The temperature of the surface being in a state of permanent equilibrium, the quantity of heat radiated and reflected must be equal to the total quantity received from the sun. It is this equality which determines the mean temperature of the surface of the globe.

If the earth were not surrounded by an atmosphere, if, consequently, the amount of heat radiated from each square foot of the land, as well as from thè whole surface, were equal to that

* Climate and Time in their geological relations: a theory of secular changes of the Earth's Climate. By James Croll. New York, D. Appleton & Co., 1875.

received from the sun, the problem of climate would be a quite simple one. But the atmosphere, and especially the vapors suspended in the atmosphere, exert a powerful influence in various ways. Perhaps the most general and wide-spread source of this influence may be found in the probable unequal diathermacy of the atmosphere to solar and terrestrial heat which may result in the mean temperature being higher than it would be if there were no atmosphere. To investigate this influence the first datum necessary is the mean temperature, first of the whole earth and then of its various zones, which would be maintained if there were no atmosphere. In other words, we wish to know what would be the temperature of a small solid body revolving round the sun at the mean distance of the earth, and presenting all its sides equally to the sun in rapid succession. *This temperature may be called the normal temperature of the region in which the earth is moving.*

We repeat that the foundation stone of any reliable investigation of terrestrial climate, with respect to its causes, must be a knowledge of this normal temperature. Without it we may have any quantity of material for discussion but nothing on which we can base a theory worthy of the slightest confidence. There are of course many other questions to follow it, but this is the one which the investigator of this subject meets at the very threshold of his investigation, just as the surveyor who attempts to make a geodetic measurement first meets with the question of the length of his measuring rod. Now, no stronger example of the chaotic state of the theory of cosmical heat can be given than the simple fact that not only is this normal temperature entirely unknown, but, so far as we are aware, no attempt has ever been made to determine it. What adds to our surprise is that while no one has attempted to determine what temperature a body like the earth would acquire in free space exposed to the solar rays, there have been a number of attempts to answer the experimentally impossible question what temperature such a body would acquire if the solar heat were cut off, so that the body should be exposed to stellar radiation alone, a temperature known in our books as that of space.

In justice to physicists it must be said that one step toward determining this fundamental temperature was taken many years ago. Pouillet and Herschel determined the actual quantity of heat radiated by the sun, and their results have been of the greatest value in investigating the thermal relations of the solar system. The remaining part of the problem is more laborious, but not, we conceive, more difficult.

Since Mr. Croll had not at hand the means of commencing a complete investigation of the causes on which terrestrial climate depends, his theory must, of necessity, fail to be entirely conclusive. Still it is worked out in a manner so laborious as to render it worthy of very careful consideration, although, owing to the diffuse mode of treatment adopted, the complete mastery of his views is a very difficult task. For this reason it is not easy for the reviewer to feel sure that he is giving such a statement of the author's views as the author himself would regard as entirely satisfactory. We may say, however, in brief, that one great object of the author is to insist upon the important agencies played by ocean currents in influencing climate. Indeed, beyond the regular astronomical variation of climate with the latitude, this seems to be the only influence which he will allow to be important. The influence of the Gulf Stream in modifying the climate of Northeastern Europe receives especial attention, and his views of this influence seem quite well grounded. We had supposed the view that the warm and equable climate of that region was due to the Gulf Stream to be one universally held, although no one had attempted to render it plausible by an actual calculation of the amount of heat conveyed by that stream. This calculation Mr. Croll has made, and having reduced his own estimate of the volume of the stream to one-half, in deference to the views of some of his opponents, he shows that the amount of heat annually conveyed away by the stream is equal to the whole amount which a belt of the earth sixty-four miles broad, extending all round the equator, receives from the sun. We make the quantity a little less, but yet equivalent to more than the total amount of heat which falls on a million of square miles at the equator. Making all allowance for the uncertainties of these data, and for the fact that only one of the two branches of the Gulf Stream passes over to Northeastern Europe, it must yet be admitted that the quantity of heat which that region receives from the Gulf Stream is not an inconsiderable fraction of that which it receives from the sun.

An essential part of Mr. Croll's system is the wind theory of oceanic circulation, essential, however, to his views of the climate of the glacial period rather than to the climate of the present. This is a point on which there is some difference between Mr. Croll and his numerous opponents, especially Dr. Carpenter. Having made no examination of the views of Dr. Carpenter, we cannot pronounce them wrong, but the view maintained by Mr. Croll, that the winds are the principal causes of ocean currents seems well sustained. The direction of these currents may be materially modified by the earth's rotation, a cause which can be investigated only by mathematical methods, and until the mode of operation of this cause is fully understood, we cannot feel sure that the theory is complete. So far as we are aware Mr. Ferrel is the only mathematician who has entered upon this investigation, but Mr. Croll does not seem to have made much use of his results. The principal support of the wind theory is found in the very obvious general correspondence between the winds and currents of the ocean, a correspondence so striking that it is difficult to see how the strongest presumption of a causal connection can be avoided. That the winds are, in a general way, amply sufficient to produce regular currents in the ocean seems to be shown by a familiar phenomenon on our Eastern coast. It is well known that the tides are there materially modified by the winds, so that the time of high water may be delayed or accelerated by an entire hour or more, and the height changed by one or more feet in consequence of a heavy wind. The effect of a wind thus determined must be the same as that of a difference of level equal to that which the wind is found to produce, and this again must be sufficient to produce a very strong surface current. Moreover, a continuous surface current must, in time, extend itself to a great depth through friction.

In thus sustaining the wind theory, we must not be understood to deny the existence of a general law of oceanic circulation which we understand to be due to Dr. Carpenter, and by which an undercurrent of cold water runs from each pole to the equator, to return as a surface current of warm water. That the mass of ice-cold water which forms the depths of the ocean came from the poles, and that to keep it cold, the supply must be constantly though slowly renewed will, we conceive, be disputed by no one. And the renewal of the water necessarily implies a surface set from the equator toward the poles. But, when we inquire whether the quantity of water thus interchanged can be so great as to give rise to the observed ocean currents, the answer is not quite clear, and the probabilities seem to incline to the negative. At the same time, we may have here an important feature among the causes which produce ocean currents, and the scientific method of investigating the subject is not by mere arguments, but by actual calculating the effect of each cause with judicial impartiality. Perhaps it would be unfair to say that Mr. Croll does not attempt to do this, but the impression left on the mind of the reader is that the "gravitation theory" of oceanic circulation is examined rather to refute it than to determine with mathematical precision what part differences of gravity between the polar and equatorial waters do really play in the phenomena in question.

While we agree with Mr. Croll in the important part he assigns to oceanic currents in modifying climate, we cannot accept the reasoning by which he attempts to prove that the corresponding influence of ærial currents is entirely insignificant. Speaking of the possible amount of heat conveyed by the upper currents, or anti-trades, from the equatorial to the polar regions, he says:

"The heated air rising off the hot burning ground of the equator, after ascending a few miles, becomes exposed to the intense cold of the upper regions of the atmosphere; it then very soon loses all its heat and returns from the equator much colder than it went thither." * * * "During all this time [while the upper current is traveling from the equator toward the poles] the air is in a region below the freezing point; and it is perfectly obvious that by the time it begins to descend it must have acquired the temperature of the region in which it has been traveling."

This passage is quoted as showing the weakness which everywhere marks Mr. Croll's reasoning on the subject of temperature. With all the care and study he has devoted to the subject, we are entirely unable to reconcile his views with the known laws of heat. The facts that the same amount of heat is given off when water freezes or vapor condenses which is necessary to melt the ice or to evaporate the water; that the amount of heat developed by the compression of air is equal to that absorbed by its expansion; that if, from any cause, heat passes very slowly from a warm body A to a cool body B, it will also pass slowly from B to A when B is the warmer; that a body cannot abstract heat from another without itself becoming warmed, belong to a class which he does not seem to bear in mind. In the passage we have quoted, he speaks of the hot air rising from the earth and becoming exposed to the intense cold of the upper regions of the atmosphere. But, what can this cold be but the coldness of the very air itself which has been rising up? If the warm air rises up into the cold air, and becomes cooled by contact with the latter, the latter must become warm by the very heat which the former loses, and if there is a continuous rising current, the whole region must take the natural temperature of the rising air. This temperature is indeed much below that which maintains at the surface, for the simple reason that air becomes cold by expansion according to a definite and well known law.

Having thus got his rising current constantly cooled off by contact with the cold air of the upper regions, it has to pass on its journey toward the poles "in a region below the freezing point." Here again the question arises whether Mr. Croll conceives that the temperature of a region can be anything materially different from the temperature of the air or other substance which fills the region. Apparently he does, for he speaks of the air "acquiring the temperature of the region," but what the difference is, or can be, he does not explain. There is such a thing as temperature expressive of the amount of radiant heat passing through a diathermanous region, but the "upper regions" are exposed to the radiation of the sun on the one side, and of the earth's lower atmosphere on the other, and there is no proof that these do not equal the surface temperature. Having thus cooled off his upper current still farther by its passage through this "cold region," and that without the region becoming any warmer, he leaves it to find its way to the earth's surface, entirely oblivious of the fact that an amount of heat will be evolved by compression in the polar regions, or whereever the current reaches the earth again, fully equal to that which it lost when it rose from the equator. If he had treated this aërial current precisely as he did the Gulf Stream, computed the probable amount of the current, its temperature when it rose from the earth in the tropics, and again when it reached the earth in northern regions, and thus determined the amount of heat given out during its passage, his course would have been much more logical.

We do not propose to enter into the question of fact, how much of an upper current there really is passing from the tropics to the poles. But, if it is as great as is commonly supposed, it must be as powerful as ocean currents in tending to equalize the temperature of the globe. The fact that it is cold during its passage, instead of being a disadvantage, is a positive advantage, because the heat which it carries being latent in form is not liable to be dissipated by radiation.

Another proposition which the author attempts to prove, by reasoning which seems equally inconclusive, is, that the mean temperature of the ocean is greater than that of the land over the entire globe. We may examine his argument, for the reason that the proposition is a fundamental one in his theories of climate. The most natural and conclusive way of establishing such a proposition, would be by actual observations of temperature, but no attempt to do this is made. The author rests his doctrine wholly on four *a priori* reasons, which we may consider in their order.

(1.) "The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles, and its transparency for heat rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great." We can hardly stop to criticise these sentences, implying as they do, that the rapidity with which solar heat is absorbed by a body determines its temperature, and also depends on its diathermancy, and that a body which is heated only by the slow process of conduction must be permanently colder than one into which the radiant heat of the sun can penetrate.

(2.) "The air is probably heated more rapidly by contact with the ground than with the ocean; but on the other hand it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapor of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated." According to the usually received laws of heat, one body can be heated from another only when the latter is the warmer of the two, and the rapidity with which the heating process goes on depends on the difference of temperature, no matter whether the heat passes by conduction, or by radiation. If, then, the air is really heated by contact with the ground more rapidly than by contact with the ocean, it can only be because the ground is hotter than the ocean, which is directly contrary to the theory Mr. Croll is maintaining. The statement that the aqueous vapor of the air is diathermanous to radiation from land, but not to that from water, is quite new to us, and very surprising ; but if it be true, Mr. Croll assigns directly contrary effects to the same cause in (1) and (2). Reasoning as in (1), he would have said that the air over the land, owing to its transparency for the heat rays from the land, becomes heated to a great height rapidly, while the air over the ocean, not being transparent, can acquire heat from the ocean only by the slow process of convection.

(3.) "The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does." Here we have the air giving back to the ocean the same heat which it absorbs from it, and thus heating it. Apparently, Mr. Croll thinks that air and ocean can thus alternately heat each other up to an indefinite extent, by natural radiation, without any necessity for more than a mere nest-egg of heat to start with from any outside source. (4) seems to be little more than a repetition of (2) in a different form.

Another idea of the author which calls for explanation is that solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned. For instance, in comparing the relative amount of heat received from the sun by the equatorial and the arctic regions, he thinks it a mistake not to allow for the fact that a greater percentage of the heat is absorbed by the atmosphere in the polar regions than at the equator. From the care he takes to subtract this percentage from the amount of heat received by the polar regions, he seems to think that the heat thus absorbed is totally lost, and does not warm the atmosphere at all. But a moment's reflection must show that as all this absorption must occur within three or four miles of the earth's surface, and probably half of it within a single mile, or two miles at most, while the arctic regions are more than 2,000 miles in diameter, it makes no difference what portion of the heat is absorbed by the atmosphere. In the one case, the atmosphere is warmed directly by the absorption of heat, in the other, by contact with the earth; but the temperature of the region is substantially the same in either case.

We may now pass to the consideration of the author's views of the cause of the Glacial epoch, or of glacial epochs in general, as, according to his view, there must have been several of them. He maintains that such a phenomenon may be fully accounted for by the great eccentricity which the orbit of the earth is known to assume at certain very long intervals. Using Le Verrier's formulæ for the secular variation of the planetary orbits, he has computed this eccentricity for a number of periods, extending back nearly 3,000,000 years, and has thus found a number of epochs at which it was three or four times as great as at present. That, in the course of each million of years, there are from time to time such periods of great eccentricity is a well established result of the mutual gravitation of the planets, but whether the particular epochs of great and small eccentricity computed by Mr. Croll are reliable is a different question. The data for this computation are the formulæ of Le Verrier, worked out about 1845, without any correction either for the later corrections to the masses of the planets, or for the terms of the third order subsequently discussed by Le Verrier himself. The probable magnitude of these corrections is such that reliance cannot be placed upon the values of the eccentricity computed without reference to them for epochs distant by nearly a million of years. This fact, of itself, does not militate against Mr. Croll's theory, since the correct formulæ would no doubt show other epochs of great eccentricity which would entirely satisfy his conditions. The proposition with which we are more especially concerned is the general one that a great eccentricity, with the perihelion in one of the solstices, will give rise to a glacial epoch in the hemisphere corresponding to that solstice, and it is the reasoning by which Mr. Croll endeavors to sustain this proposition which we next propose to examine.

The difficulty which the sustainer of this proposition encounters at the outset is the demonstration of D'Alembert that, whatever changes the earth's eccentricity and perihelion may undergo, the total amount of heat received from the sun in the course of a year is still the same for each hemisphere. Consequently, if the mean temperature of the hemisphere depends on the total amount of heat received, it must be the same for the two hemispheres, and but slightly different from the mean temperature of the present time. To understand the question clearly let us suppose the perihelion to coincide with the June solstice, so that the earth is nearest the sun in our northern summer, and farthest from it in our northern winter. Then, in this hemisphere, considering only the heat of the sun, we shall have a short and hot summer, and a long and cold winter, while in the southern hemisphere the summer would occur when the sun was farthest off, and the winter when he was nearest, so that the rigor of both seasons would be greatly mitigated. Still, as just stated, the mean temperature due to solar heat would be the same in each hemisphere, the northern hemisphere having the hotter summer as well as the colder winter.

While admitting this equality in the total amount of heat received from the sun, Mr. Croll endeavors to show that the northern hemisphere would be colder and that there would be an accumulation of snow and ice during the long winter which would not be melted by the sun's rays during the short and hot summer. His principal arguments in favor of this view are given in Chapters II and IV. Beginning with the winter he says that the reduction in the amount of heat received from the sun during this season, owing to his greater distance, would lower the mid-winter temperature to an "enormous extent." Precisely how great a diminution of temperature he considers enormous he does not state in this connection, but in a subsequent chapter he computes a diminution in the mid-winter temperature of Great Britain sometimes amounting to more than 30° from this cause. This computation we regard as entirely untrustworthy, being founded on purely hypothetical laws with purely hypothetical data, but we need not challenge the result at present. The effect of this lowering of the temperature would be, he says, a great increase in the amount of snow which would fall during the winter. This conclusion we cannot accept. During the long cold winter the evaporation must be lessened, and hence the amount of precipitation also, unless warm and moist air is brought from the warmer regions of the globe. In this case the latent heat set free by precipitation, as well as the heat of the air itself, would mitigate the winter temperature. On the whole, we may consider the equivalent of twenty inches of solid ice to represent a very liberal estimate of the probable average amount of snow which would accumulate over any very great extent of surface during any one winter.

Mr. Croll's next point is that the presence of so much snow would lower the summer temperature, and prevent to a great extent the melting of the snow. He gives three reasons for this extraordinary proposition. In the first place the air will be cooled by radiation to the snow more rapidly than it will be heated by the sun. Of the fact that this cooling of the air would itself be necessarily accompanied by a melting of the snow he shows no consciousness. A simple calculation will show that a cooling of the air by some 30° Fahrenheit would melt the whole twenty inches of snow.

The second reason is that the rays which fall on snow and ice are to a great extent reflected back into space, the grounds of this statement being apparently the transparency of the atmosphere to the calorific rays of the sun on which he had just been insisting, and which he must have had in mind as explaining how the reflected rays can be got back into space. How great the reflecting and transmitting power of the snow and ice must be, to keep the snow unmelted all summer, may be inferred from the fact that during this perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt from four to six inches of ice per day, over its entire surface, that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months.

The third cause of non-melting of the snow during summer is that snow and ice chill the air and condense the vapor into thick fogs which "would effectually prevent the sun's rays from reaching the earth, and the snow in consequence would remain unmelted during the entire summer." Here again, he says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled. The task of arguing with a disputant who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the next breath that thick fogs effectually prevent the rays ever reaching the snow at all, is not free from embarrassment. We can accept calmly any and every possible hypothesis respecting the properties and nature of the atmosphere which he chooses to propound, but must insist that the conclusions be drawn in accordance with the first principles of the theory of heat. We might therefore show that if the snow, air, fog, or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator, and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter.

Perhaps the shortest way of meeting the case is to refer to the best known facts of our own climate. Every winter a large portion of the northern hemisphere is covered with snow precisely as Mr. Croll supposes was the case during the Glacial epoch. According to his theory this accumulation of snow ought to offer a great resistance to the melting power of the sun's rays. How much resistance it does offer everyone knows. What effect the difference of astronomical conditions during the epochs of great eccentricity might have had may be inferred from the fact that, adopting Mr. Croll's method of estimating solar temperature, the mid-summer heat of the northern hemisphere due to solar radiation, was from 40° to 50° higher than it now is. His layer of snow must therefore resist, not merely our present heats, but temperatures ranging from 100° to 150° Fahrenheit.

The remainder of the argument can be dealt with quite briefly, because it is based on the utterly fallacious results we have just described. The northern hemisphere being cooler the trade winds are thrown farther south. The Gulf Stream being caused, in part, by the trade winds, is thrown into the southern hemisphere, and thus the northern hemisphere is deprived of this latent source of heat and its temperature falls to a point far below the normal astronomical temperature. Considering separately the propositions that a cooler northern hemisphere would throw the trade winds south, and that this change in the winds would change the Gulf Stream, they both rest on too slender a basis to be worth consideration. We cannot therefore regard Mr. Croll's theory of a connection between the form and position of the earth's orbit and the Glacial epoch as having any reasonable show of foundation. The working out of such complex theories is of the less importance that there is no astronomical reason to believe that the solar radiation has been constant during a period of a million of years.

Washington, February 21, 1876.

Art. XXXIII.—*On crystals of Tourmaline with enveloped Orthoclase;* by Edward H. Williams, Jr.

[Read before the Chemical and Natural History Society of Lehigh University.]

While at Port Henry, N. Y., last July, visiting the newly constructed furnace at Cedar Point, I noticed large sized crystals of tourmaline in some heaps of quartz and feldspar in process of shipment up the lake.

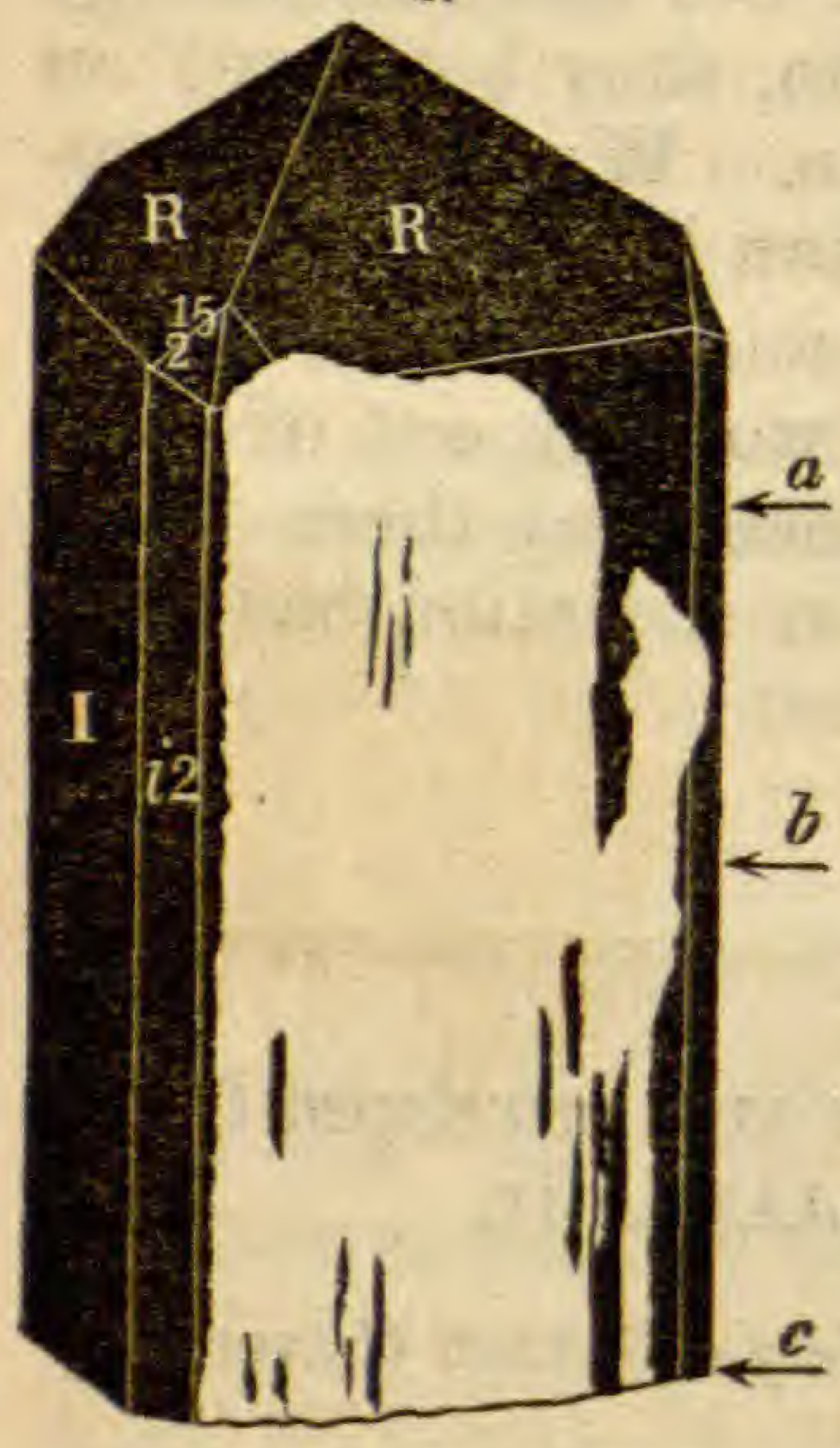
The feldspar was orthoclase, and of two varieties: one white and compact; the other reddish and much weather-stained; the cleavage planes, especially the basal, were more or less covered with a film of sesquioxide of iron. The crystals of tourmaline in the quartz were simple prisms. Those in the feldspar, and in the second variety in particular, were peculiar; when the feldspathic matrix was fractured they readily separated from it, and proved to be mere shells of tourmaline filled with feldspar.

From want of time I was then unable to visit the locality from which they were taken. When again at Port Henry I found that the minerals came from a quarry worked by Messrs. Roe and Witherbee, five miles west of the town, and about 1500 feet above the lake.

The tourmaline which presented the peculiarity was mainly in the reddish feldspar, that in the white variety being, as a rule, solid. The tourmaline occurs in long prisms with rarely more than one termination. The observed faces are: Rhombohedral, $\frac{1}{4}$, $1(R)$, $-\frac{1}{2}$; scalenohedral, $-\frac{1}{2}^{5}$; prisms, I, i-2, i-$\frac{5}{4}$. The common form is shown in Fig. 1. The crystals are commonly distorted, and are frequently terminated by but a single rhombohedral plane. Specific gravity, 3·11. Fuses before the blowpipe easily, with intumescence, to a dark bead.

In the specimens obtained, there seem to be two varieties of combination of the feldspar with the tourmaline: in the first, the tourmaline has imposed its form upon the feldspar; in the second, each has influenced the other.

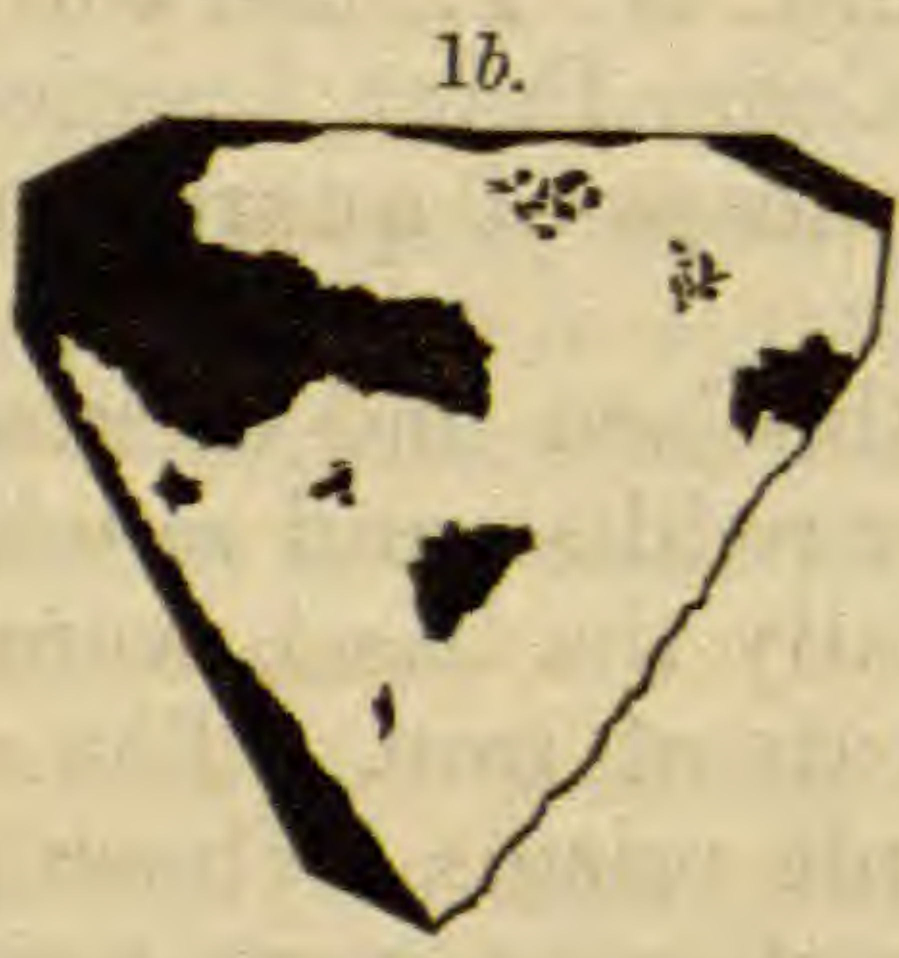
1.



There are two types of the first variety. Fig. 1 shows the tourmaline with a solid termination, and the enclosed feldspar pierced with small tourmaline prisms which descend from the solid end with their vertical axes parallel to that of the enclosing shell. These shafts, as well as the shell, decrease in thickness as they recede from the head. The tourmaline has thus a pyramidal cavity filled with feldspar; and in one instance this cavity is terminated by a face of -1. The shell is absent in places, with the feldspar apparently crystallized according to the prismatic planes of the tourmaline. In figure 1, the dark and light parts represent tourmaline and feldspar respectively;

1*a*.

1*b*.

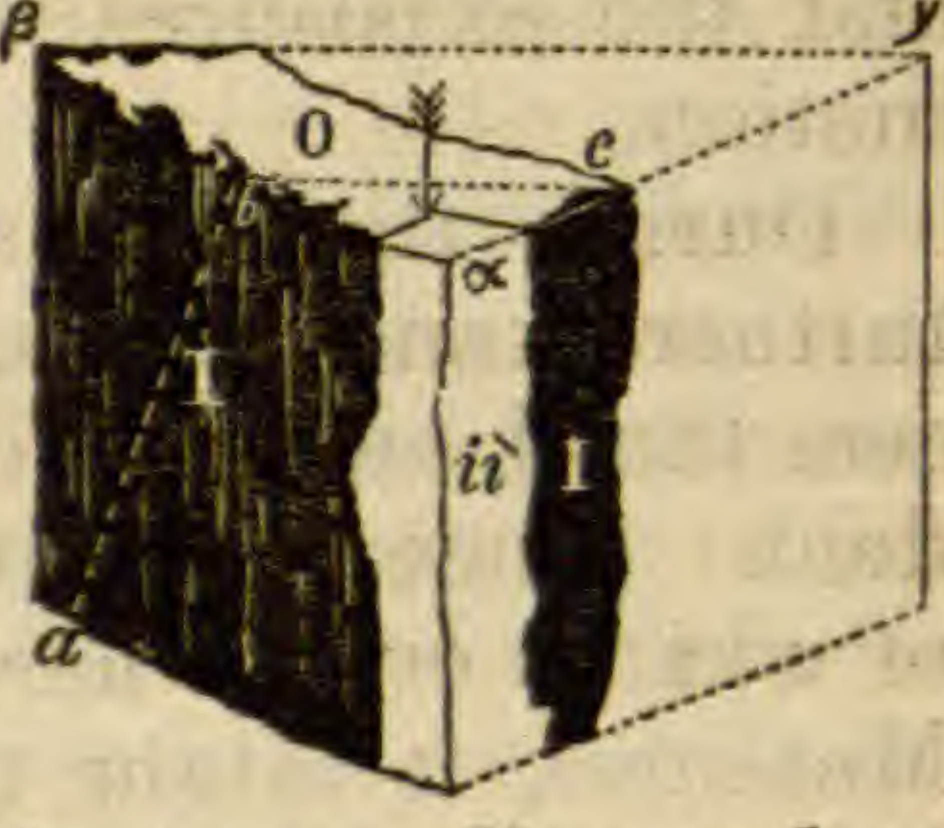


1*c*.

figs. 1*a*, 1*b*, 1*c* are sections of the same crystal (fig. 1) parallel to the basal plane of the tourmaline, and taken at the points indicated by the letters *a*, *b*, *c*; they show the gradual disappearance downward of the tourmaline.

In the second type the rhombohedral faces, as well as those of the prism, are shells, and the tourmaline does not seem to decrease in thickness with its distance from the termination. After removing the shell from these combinations the enclosed feldspar has been obtained having the form of a distorted tourmaline crystal.

2.



Under this second variety there are cases in which the tourmaline prism encloses the feldspar and has no terminations. The feldspar often replaces the tourmaline, as is seen in Fig. 2, where the angles between the prismatic faces are distorted; one face is found parallel to a cleavage plane, usually *i-ī* of the orthoclase. In the figure the angles $\alpha\beta\gamma$ and $\beta\alpha\gamma$ are respectively 45° and 90°; the tourmaline prism is perpendicular to the basal plane of the feldspar; the edge toward the eye has been replaced by the orthoclase, while the tourmaline makes a re-entering angle behind it at the arrow-point; the plane *abc* is the cleavage plane parallel to *I* of the feldspar. These prisms are readily separated from their feldspathic matrix, and cleave parallel to its basal plane; each section thus given shows an uneven shell of tourmaline that in places entirely disappears. In all the cases mentioned the cleavage of the enclosed feldspar is parallel to that outside.

3.

Fig. 3 shows where the tourmaline has been deposited on a cleavage face of the feldspar, and covers about one square inch; the vertical edge toward the observer is rounded, and presents no measurable angle; the plane *abcd* is a cleavage plane parallel to *i-ī* of the feldspar. In one instance the feldspar inside the tourmaline encloses another shell of tourmaline filled with orthoclase.

The few specimens thus far obtained seem to point to simultaneous crystallization. There is no law governing the relations in position of the two minerals; for, in the same mass of matrix, there are crystals of tourmaline whose vertical axes make with the basal plane of the orthoclase angles varying from 0° to 90°. Some of these enclose the feldspar in prisms whose angles correspond exactly with those of theory, while others are distorted and are found between the cleavage planes of the feldspar. A more extended study of these interesting forms, afforded by a greater number of specimens, may throw more light on this singular combination.

Philadelphia, Dec. 28, 1875.

ART. XXXIV.—*The Conglomerate Series of West Virginia*; by WILLIAM M. FONTAINE.

IN the May and June numbers of this Journal for 1874, I gave some account of the strata which, on New River, West Virginia, underlie the massive sandstone exposed at the Falls of the Kanawha. This account was necessarily imperfect, since at the point examined the base of the series was not exposed, and the exposures were very unfavorable for a detailed examination.

During the past summer I revisited this field, and made further examinations, at points more to the east, with such success that I am now able to present a detailed section of this field. Since the white sandstone of the Falls is the equivalent of what is everywhere called "the Conglomerate" of the Coal Measures, it might to some seem more fitting to call the rocks in question "Sub-conglomerate," or "Lower Carboniferous." In West Virginia, the strata which occupy the interval between the floor of the productive coals and the Devonian, are so greatly expanded, and so much diversified, that these terms are not definite enough to distinguish them. Besides, this New River system occupies precisely the horizon which is elsewhere commonly filled by conglomeritic sandstone alone, lying, as it does, between the red shales of the Umbral, and the lower productive coals. For these reasons I prefer to use the name "Conglomerate Series" for it. For like reasons it will be necessary to retain the names "Vespertine" and "Umbral," of the first Pennsylvania Survey, in describing rocks equivalent to those bearing these titles in the above named survey. A single instance will show this necessity. The system about to be described contains important coals. We find also far below them, in the Vespertine of Montgomery County, Virginia, near the White Sulphur, West Virginia, and elsewhere, well developed coals. To call these Sub-conglomerate or Lower Carboniferous coals would fail to distinguish them.

In my second visit to this region I made a re-examination of the strata at Sewell Station, the point at which most of the facts given in my first paper were obtained. In this last visit I found the strata quite well disclosed in the cuttings of the "Incline," made since my previous inspection. I also made a careful and detailed examination of the same strata at Quinnimont, a point on the Chesapeake and Ohio Railroad, distant by railroad twenty-one miles to the east of Sewell Station, but about ten miles by air line.

While the base of the series is not exposed at Sewell Station, yet, owing to the fact that the westerly dip is more rapid than the fall of the river, even the underlying Umbral red shales are fully disclosed, and the entire series in question is contained in the lofty hills at Quinnimont, while in their summits they still retain a small remnant of the lower productive coals, with one and sometimes two coal beds.

While making my examination at Quinnimont I received valuable aid from Mr. S. F. Morris, C. E., and I take this opportunity to make my acknowledgments to him. Mr. Morris had, by levelling, determined the height of many points, and examined the character of the strata around Quinnimont, in behalf of the company owning the furnace and coal mine at that point. The data which he kindly put at my disposal were of great assistance in checking my own observations.

During the same summer I also made an examination of the country to the east of the Quinnimont, especially that portion in the vicinity of the White Sulphur Springs, Greenbrier County. It will perhaps be well to give here some of the facts thus obtained, bearing on the general geology of the region, in order more clearly to define the relations of the series to be described in this paper. In order to do this I will commence at the east and proceed west along the line of the Chesapeake and Ohio Railroad, whose general course is across the strike of all the strata underlying the rocks in question.

We may for a clearer exposition commence at Lewis Tunnel, a point six miles east of the White Sulphur. Here we find Vespertine strata which run in a narrow belt along the east face of the main Alleghany range, and contain the small coal beds, and plant-bearing shales, found near the Tunnel. The main range and the country westward for twelve miles is occupied by highly disturbed Devonian strata, mainly Hamilton, Portage, and Chemung, with probably the Catskill group. In the center of this belt the Springs are situated. Six miles west of the Springs, we find, on the east side of a small creek, highly contorted Devonian strata, and on the west side within 100 yards, the upper portion of the Vespertine, dipping gently eastward toward the contorted Devonian. Just above the Vespertine, in the hill across this stream, the base of the Umbral or Lewisburg limestone may be seen. The contortions and other evidences of great disturbance which follow us from the east up to this point now cease, and throughout the wide belt of country lying between this point and the Ohio River, the strata undulate more and more gently, until before Quinnimont is reached the rolls cease to reverse the dip, but serve to keep the strata longer at the surface than they would otherwise remain in that position.

This sudden change in structure is not found here alone, although it seems to be more marked here than elsewhere. It may be traced far to the southwest, and probably to the north, and is to be explained by the existence of a fault, apparently the most westerly of the system affecting the Appalachian Region in this quarter. The development of this fault seems to have, in a great measure, relieved the strata lying to the west, from the disturbing force which so highly affected them on the east and it is not necessary to suppose a gradual dying away to the westward of the lateral thrust from the east. The conditions seem to imply a certain amount of unconformability between the Devonian and Vespertine, which is not incompatible with other facts observed here.

Proceeding westward along the line of the railroad from the fault, the gentle rise of the Vespertine to the west brings into view its middle or coal-bearing portion, here also containing small coal seams. This is in the vicinity of the bridge over Greenbrier River, and explains the presence near this stream, of the coal seam mentioned by Prof. Wm. B. Rogers in his reports. The Vespertine as it crosses the stream passes into a low anticlinal, which, west of the river, finally brings down the Umbral limestone to the level of the railroad. This position it maintains for a long distance, as far as Great Bend Tunnel, near the mouth of the Hungert's Creek, Summers County, where it dips under the red rocks of the Umbral series, which in this district are greatly developed. The Umbral series seems to possess a threefold character, being at bottom blood-red shales and sandstones, in the middle, grayish, bluish and brownish sandstones and shales, mainly the former; and at top brownish sandstones, blood red and varigated shales. The shales throughout the series have the texture of marlites, and the sandstones, although chiefly argillaceous are sometimes highly siliceous, forming huge cliffs along the railroad, as seen near Richmond Falls. These three series, the Vespertine, the Umbral limestone, and Umbral shales and sandstone, thicken rapidly in proceeding from northeast to southwest. Prof. Rogers measured them in Greenbrier Mountain, Pocahontas County, a point about sixty miles northeast of Richmond Falls on New River, where the Umbral shales and sandstones are extensively exposed. With respect to the limestone I have no data for comparison but the indications are that on the railroad it is thicker than the measurement given by Prof. Rogers, viz: 822 feet. For the Umbral shales and sandstones in Pocahontas he gives a thickness of 1,310 feet. My estimates along the line of the railroad, which, however, have not the accuracy of measurements, give for this series a probable thickness of 1,450 feet in the vicinity of Richmond Falls, distributed as follows: 1. Lower red shales and sandstones, 320 feet; 2. Middle gray and greenish sandstones, 820 feet; 3. Upper red and variegated shales, 310 feet. If we compare this series with the character of the Umbral in the vicinity of Blossburg, as given by H. D. Rogers, we find an almost identical distribution of similar strata. The upper portion of the Umbral continues to be shown up to a short distance west of Quinnimont, where the prevailing westerly dip takes it out of sight. These upper strata form the base of the hills around Quinnimont, contrasting strongly in all their physical features with the overlying conglomerate series. This latter, whose entire thickness lies above the level of the river at this point, gradually sinks as we pass west, down stream, being kept above water level for a long distance by several broad rolls. It finally passes out of sight two or three miles below Kanawha Falls, and is succeeded by the series of the Lower Productive coals in the Kanawha Region. In this latter series there is a four-feet bed of coal, about forty feet above the massive sandstone which closes the conglomerate series. This is the equivalent of coal B of Lesley. This bed still remains uneroded in the tops of some of the high hills around Quinnimont.

About two miles down the river, on the Raleigh County side, Piney river empties into New River. A well graded road from the mouth of this stream, passes over the outcropping edges of the entire conglomerate series, and the numerous cuttings made in grading afford excellent exposures of almost every member of the series throughout its entire thickness. My section was made along this road. It was verified by a second section taken at Quinnimont by another road, which also passed over the entire series. These two sections were compared with observations made at Sewell Station, and with measurements made by Mr. Morris. The data in the cases in the section, marked as not seen, are given on the authority of this gentleman. The dip is northwest about fifty feet to the mile.

Section from the mouth of Piney River, Raleigh County.

21. Upper conglomerate, 150–200 feet.
20. Black slate, with thin coal partings, coal not seen, 10 feet.
19. Olive gray sandstones and shales, 100 feet.
18. Dark blue slates and sandstones, 80 feet.
17. Quinnimont coal seam, or coal No. 9, consisting of semi-bituminous coal, 4 feet; fire clay, $2\frac{1}{2}$ feet; and at bottom, splint coal, 14 inches, =8 feet.
16. A thick mass of rocks not fully exposed, which may be divided as follows: 16 *e*, olive gray shaly sandstones, 40 feet; 16 *d*, coal 8, not seen, given as 20 inches thick; 16 *c*, bluish sandy shales, 60 feet; 16 *b*, coal 7, not seen, given as 2 feet thick; 16 *a*, gray sandstone, 50 feet. Total, 154 feet.
15. Fire clay and a 12-inch outcrop of coal, seen imperfectly exposed, given as 2–$4\frac{1}{2}$ feet of imperfect splint coal, coal No. 6, =2–$4\frac{1}{2}$ feet.

14. Coal system; at bottom interstratifications of coal and slate, with one seam one foot thick; (coal No. 5), and on top, flags passing into firm sandstones. Good plant impressions occur here. Thickness, 80 feet.
13. Olive marlites, 40 feet.
12. Massive firm gray sandstones, 50 feet.
11. Coal No. 4, not fully exposed, given as 2½ feet thick.
10. Firm gray flags and sandstones, 90 feet.
9. Coal system, coal No. 3; at bottom interstratifications of thin coals and strata; on top, shales, flags and sandstones, 80 feet.
8. Gray sandstones, 75 feet.
7. Ferruginous limestone, 2 feet.
6. Variegated marlites, 40 feet.
5. Bright red shales and marlites, 30 feet.
4. Coal system, coal No. 2, consisting of coal 8 inches, slate 2½ feet, coal 8 inches, sandstone 8 feet, and at bottom coal and slate 1 foot; total =13 feet.
3. Olive and reddish sandstones, passing below into olive marlites, 100 feet.
2. Black slate, not seen, said to contain 18 inches of coal, (coal No. 1), given as 11 feet thick.
1. Lower conglomerate, 80 feet. Total =1,197 feet.

Under the lower conglomerate is found a transition series, of which the following is a section determined mainly at Quinnimont, where the strata are more fully exposed.

Transition Series at Quinnimont.

2. Black fissile slates and shales, 20 feet.
1. Thinly laminated gray flags and calcareous shales, with drifted leaves of Lepidodendra near the base; and near the top having numerous impressions of marine shells, while at the top it passes into carbonaceous shales with strings of coal, leaves of Lepidodendra and other impressions too much obscured for determination, 50 feet. Total =70.

To complete the section of the strata exposed in the vicinity of Quinnimont, I give below a section of so much of the Umbral series as is to be seen there.

Section of the Umbral Series at Quinnimont.

3. Variegated marlites with some nodular limestone, 70 feet.
2. Gray calcareous sandstone, 20 feet.
1. Bright red shales, seen 50 feet. Total =140 feet.

Some of the above mentioned strata merit a more particular description, which I will now give.

I make in this place no further mention of the remnant of the Lower Productive coals found in this vicinity, but refer to my former paper, where some account was given of their character as found in the Kanawha Valley. It is not known how much farther east they extend, but it cannot be to any considerable distance. No. 21 of the conglomerate series is the only persistent member. As it is found everywhere throughout the Appalachian Coal Field, being in many places the sole representative of the series, and as it is always at a uniform distance below the lowest workable coal-seam of the Lower Productive coal it would seem to been titled to be called, as it has been, "The Conglomerate of the Coal Measures." In Raleigh County, and along New River, it is usually a coarse white sandstone, with some conglomeritic portions in its middle and upper parts. In its lower portions it is more flaggy and argillaceous. It varies in thickness from 150 to 200 feet. In the section I have in my summation taken it at the lower figure.

No. 20, near Piney River, shows at its outcrop only black slate. It has been opened near Quinnimont, and is said there to contain thin strings of coal. Nos. 20, 19, and 18, have no features of special interest.

No. 17. This is the coal-seam which is worked extensively at Quinnimont, where it is coked and used in the furnace at that place. It is the most persistent and best developed seam of the series, being easily recognized everywhere in this region by its peculiar structure. From the flaggy sandstones over this bed at Sewell Station were obtained the plants of Devonian type mentioned in my former paper. At Quinnimont I could find none of these, and it is there remarkably free from plant-impressions of all kinds. In Raleigh also it showed no plants. At Sewell Station, this seam was at first opened for the purpose of working it, but was soon abandoned, owing to an apparent thinning out which was in fact caused by a slide.

No. 16, on the Raleigh road, was not fully exposed owing to slides, which also obscure its outcrop at Quinnimont. It presents the subdivisions founded on the character of the sandstones given in the section, but the coal beds are given on the authority of Mr. Morris, and others, who claim to have opened them. I have no doubt of their existence, for at Quinnimont the black slate accompanying 16 *d*, or Coal No. 8, was seen.

No. 15 was only partially exposed at its outcrop on the Raleigh road. Next to the Quinnimont seam, it appears to me to be the most promising seam of the field. The fire-clay is of fine texture, and sharply distinct from the coal, features not usually seen in the coals of this series.

No. 14, (Coal 5.) This presents in a marked manner a feature very common in this field. The coal at its base exists in the form of numerous interstratifications of coal in thin partings, and black carbonaceous shales; the whole being topped by strata which become more and more siliceous, and firmer as they ascend. There is enough carbon diffused through the base of this mass to make an important bed of coal, were it collected in one mass. The condition of things here shown indicates that there was no deficiency of vegetable matter, but that the alterations of level were too rapid to permit a great accumulation of coal in one mass. The same features to a greater or less extent are shown in every coal bed of the series, and it is safe to say that the instability alone of the surface, prevented the accumulation, in this series, of coal beds as thick as those found in the more productive series which lies above it. Many good plant impressions occur here.

No. 11, from its outcrop, seems to be a promising bed of coal. Its thickness was not fully disclosed. Mr. Morris gives it as two and one-half feet thick. It shows at its base Stigmaria rootlets.

No. 10, stands out in high cliffs. Some of the other sandstones of the series also present firm perpendicular outcrops.

No. 9, is well exposed on the road in a high cliff. It presents the same features as number fourteen, even more strikingly. Numerous thin seams of coal, intermixed with carbonaceous shale, some of them three or four inches thick, form the lower portion for a space of seven feet. Vegetable matter in the form of films of coal, and impregnations of the sandstones and shales, occur to the height of thirty feet. Only a few Lepidodendron leaves were found here.

No. 8, is a massive and siliceous sandstone, forming high cliffs, and resembling to some extent No. 21.

Nos. 6 and 5, are interesting for the recurrence here, in the middle of this coal-bearing series, of the same conditions which prevailed in the formation of the upper part of the Umbral series. These two strata are most strikingly like the red and variegated marlites and shales, found in that portion of the Umbral, and might easily be mistaken for them.

No. 4, is well exposed on the Raleigh road. No plants were found in it.

No. 2, is not exposed anywhere so far as I have seen. The interval occupied by it, lies between the massive rock, No. 1, and the crumbling strata of No. 3, which are especially prone to slide down over the precipitous cliffs formed by No. 1. Hence at all the places examined by me, this portion was buried under a mass which had come down from above. Its character is given on the authority of Mr. Morris.

No. 1. This member of the series I consider to be the base of the conglomerate series. It is one of the most prominent features in the hills, standing out as it does, not far above their bases, in immense precipitous ledges. It forms the first stratum, which indicates a decided change from marine to terrestrial conditions. It is much nearer a true conglomerate than No. 21, for many of the layers contain pebbles, a half inch in diameter.

It is usually a coarse, open-grained, purely siliceous sandstone, lying in very thick beds. Near the bottom it is brownish in color, but above it is white, having many ferruginous stains. In many parts of this sandstone, particles of carbonaceous matter, in the condition of charcoal, are seen, produced from drifted fragments of trunks and limbs of trees. This condition of the vegetable matter is no doubt due to the ready escape of the bituminous matter from the porous sandstone. Sometimes pretty large angular fragments of the brown sandstones of the Umbral are found associated with these fragments of trees, and in some cases the pebbles of the conglomerate portions are of limestone. This rock is no doubt the heavy sandstone mentioned by Professor Rogers as found some distance to the east of this point, forming the summit of Little Sewell Mountain.

Underlying this rock is found a series of beds which are evidently the products of a period of transition. They are well exposed near Quinnimont, and exhibit some interesting features. No. 1 of this series is a thinly-laminated, argillaceous, gray sandstone in its lower part, but becomes more and more calcareous toward its upper portion, where numerous impressions of shells are found, a list of which will be given farther on. At its summit, which is not seen at Quinnimont, but is well exposed on the Raleigh road, there is a good deal of vegetable matter mixed with the shale, and which is the product of plants which have grown on the spot. This is the lowest indication of an attempt at coal formation, seen in this region. From the indications, there is little doubt that in some places this horizon may show a little coal. Professor Rogers mentions that near the top of Little Sewell, and immediately over the red shales of the Umbral, he saw a small coal-bed. It is no doubt the stratum now described. The other strata given in the sections above present no points of interest.

From this account of the coal-bearing series in question it will be seen that it occupies the horizon of the so-called "Coal-measures Conglomerate," and it would seem to be simply a greatly expanded portion of this widely extended formation. Lying between two huge plates of massive sandstone, either of which has equal claims to the title of conglomerate, the name which I have given it seems justified.

Almost no exploration has been made in the country to the east of Quinnimont, and hence the limits in that direction of this series cannot be given. That it does extend farther east is known. Since my inspection last summer, I have been informed that a five-foot bed of coal is found near Hinton, 800 feet above the level of the river. Hinton is near the mouth of Greenbrier river, about fifteen miles farther east than Quinnimont, measured in an air-line across the strike of the strata.

To the southeast and south, it is found in the counties of

Wise, Russell, and Tazewell, as may be seen from the account of these counties given by Professor Lesley, in his paper read before the Am. Phil. Soc., April 21, 1871. Professor Lesley shows that under the so-called "Sheep Rock" in Wise county, about 700 feet of coal-bearing rocks are disclosed, with the base not shown. The "Sheep Rock" is No. 21 of the Piney River section. In this space two coal beds are to be seen; one, a six-foot bed, lies at the very base of the hills, and the other, a two-foot bed, is a short distance above it. A similar formation exists in Russell and Tazewell counties. These coals are not to be confounded with the beds seen in Montgomery county, for the latter are found in the Vespertine strata, and are of the same age with those near the White Sulphur in Greenbrier county. The basin, in which these conglomerate coals were formed, evidently extended still farther east than the counties described in Professor Lesley's paper, as the considerable development of this series in them shows. But in the more easterly extension of the field, the number of seams have diminished, especially in the upper part. On New River in Raleigh county the most important coals are found within 700 feet below the upper ledge.

As we proceed northward, along the eastern outcrop of the series, it has been more extensively affected by erosion, and has been swept off from the greater part of Monroe and Greenbrier counties, these being occupied mainly by the Umbral shales and limestone. Professor Rogers mentions finding at the top of Greenbrier Mountain, in the northeast part of the county of that name, a massive sandstone resembling the conglomerate. This is no doubt a remnant of the series. North of this point, in Rich Mountain, in Randolph county, the entire series is presented, capping the mountain, according to Dr. Stevenson. But here it has undergone an important modification, from the loss of the shaly central portion, and the almost entire disappearance of the coals.

[To be continued.]

ART. XXXV.—*Results of Experiments on the Set of bars of Wood, Iron, and Steel, after a Transverse Stress;* by WM. A. NORTON, Professor of Civil Engineering in Yale College.

AT intervals, during the last two years, I have carried on a systematic series of experiments, with the view of determining the laws of the set of materials resulting from a transverse stress under varied circumstances. The experiments were made with the testing machine which I devised several years since, for the purpose of experimenting on the deflection of bars under a transverse stress. A detailed description of this machine is given in the Proceedings of the American Association for the Advancement of Science, Eighteenth Meeting, Aug., 1869, (p. 48). The depressions of the middle of the bar experimented on,—while under a transverse stress, or remaining after the stress has been withdrawn—are measured by it to within $\frac{1}{10000}$ of an inch. The experiments on set have been fully discussed in two papers read before the National Academy of Sciences, Washington, (April, 1874 and April, 1875). The first paper set forth the results of the experiments on bars of wood, and contained a detailed account of the course of experiments instituted for the purpose of detecting instrumental errors, and of the precautions taken to reduce the incidental errors, from variations of temperature and other causes, to a minimum. The second paper discussed the experiments on the set of bars of wrought iron and steel; which gave results generally similar, under corresponding circumstances, to those obtained with wood. I propose, in the present communication, to give a succinct statement of the general conclusions that follow from the whole discussion.

The experimental investigation was prosecuted under three general heads:

I. Sets from momentary strains.

II. Sets from prolonged strains.

III. Duration of set; and variation of set with interval of time elapsed after the withdrawal of the stress.

Each of these embraced several special topics of inquiry. The bars used in most of the experiments consisted of one of white pine, 3 in. by 3 in and 4 ft. long; another of wrought iron, $\frac{1}{4}$ in. wide, 1 in. deep, and 4 ft. long; and a third of steel of the same dimensions. The discussion of the entire series of experiments has brought out the following results, as alike applicable to bars of wrought iron, steel, and white pine.

1. The immediate set,—that is, the residual deflection which obtains immediately after the transverse stress is withdrawn,—increases in nearly the same proportion as the stress applied; until this exceeds a certain amount, beyond which the set increases according to a more rapid law than that of proportionality to the strains. It is to be understood here that the varying strains are applied at considerable intervals of time.

2. The immediate set augments with the duration of the stress, up to a certain interval of time. In the experiments with white pine, the duration of strain which gave the maximum immediate set, varied, with the strain, from ten minutes to one hour. The immediate set resulting from a prolonged strain, was found to be from five to nine times as great as that which succeeded a momentary strain.

3. The residual depression below the original line of the bar,

is greater if the stress is reached by a series of increasing weights than if the full stress is directly applied.

4. When the same strain is repeated on the same bar, after a short interval of time, the set first obtained is not augmented, unless the load applied exceeds a certain amount, varying with the material and dimensions of the bar. With loads greater than this limit each repetition of the load augments the total set. The amount of the increase varies with the interval of time since the previous application of the load and the number of previous applications.

5. The set, or residual depression of the middle of the bar, experiences marked variations as the interval of time subsequent to the removal of the stress increases. When the immediate set is less than about 0·0005 in. it passes off in a few minutes (10 m. or less). When it is greater than this it habitually varies as follows: it invariably decreases for a short interval of time, and then ordinarily increases for a longer interval, with moderate fluctuations. The period of decrease varies from about 5 m. to 20 m.; and is the longer in those instances in which the stress is prolonged. The subsequent increased set, or augmented depression of the line of the bar, may attain in less than an hour to an amount even greater than the set observed immediately after the stress is withdrawn. In some of the experiments the depression increased until it came to be about double that first observed. The proportionate increase of set is usually, however, much less than this. This increase of set is eventually succeeded by another decrease. These remarkable fluctuations observed in the line of the bar were more conspicuous in the experiments with white pine, than in those with iron and steel. The difference was, however, only in degree. Under similar conditions the general character of the fluctuations was the same whichever material was used. The fluctuations observed with the bars of iron and steel, as well as with the wooden bar, far exceeded any errors to which the observations were liable. They were also much too slow, and too prolonged, to be regarded as simple vibrations of the bar, consequent on the removal of the downward pressure.

6. Abnormal variations from the general law of variation of the set just noticed, may occur under especial circumstances. Such deviations were observed after the bar had been subjected to repeated strains from day to day. Under these circumstances the bar may be in such an abnormal condition that the set observed immediately after the stress is withdrawn may pass off rapidly, and the line of the bar may even rise considerably above the position held when the stress was applied—though not above its original line some days previously, before any strain was applied.

7. When the load, or stress at the middle of the bar, exceeds a certain amount, the set resulting from one or more applications of the load on any one day is not only still discernible on the following day, but the actual result may be that the middle of the bar may be lower than at the close of the observations on the previous day. Such effects were observed, in the experiments with white pine, when the load was sufficient to produce a longitudinal strain on the upper or lower fibers of 500 lbs. per square inch; and in the experiments with the steel bar, resting edgewise on its supports, when the strain on the outer fibers amounted to 1500 lbs. per square inch.

8. Repeated applications of the same load, from day to day, are attended with an indefinite augmentation of the residual depression of the middle of the bar, if the load exceeds a certain amount. When a smaller load is similarly applied, the set attains after a few days to a maximum, and subsequently subsides more or less. The load answering to the critical point here referred to, is obviously the maximum safe value for a variable load that can be applied, with an indefinite number of repetitions, to the bar. In the case of a white pine stick (3 in. by 3 in., and 4 ft. long) the experiments show it to be less than $\frac{1}{4}$ the theoretical breaking load. Under repeated applications of 500 lbs. (or about $\frac{1}{4}$ the theoretical breaking weight) the set steadily increased from day to day—that is, the middle of the stick became more and more depressed—during the entire period (seven days) that the prolonged effects were noted. Under daily repetitions of a load equivalent to $\frac{1}{10}$ the breaking weight, the depression increased for three days, and after another interval of three days the stick had recovered its original line. The depressions here referred to are those which obtained on the morning of each day just before the first application of the stress on that day.

9. In connection with the phenomena of set which have been signalized, it is important to note that during any interval in which a bar was kept under a transverse stress, the resulting deflection commonly experienced a continual variation. In general the deflection increased as the strain was prolonged. But the deflection of the steel bar in some instances diminished, under the prolonged strain. This unusual result was apparently dependent on some molecular condition of the bar, induced by previous strains. The comportment of the wrought iron bar, as regards varying deflection under a continual strain, was not particularly examined.

It is also noteworthy, in this connection, that the deflection resulting from any single stress was found to be more or less dependent on the previous strains to which the bar had been subjected. The wooden bar, when it had been exposed to a cross strain not long before, was generally in a condition to suffer a greater deflection than it had before experienced under the same load. The same was true of the steel bar during several successive days of experiments with loads of 4 lbs. and 6 lbs.; but as the result of these repeated strains the bar came eventually to be in a condition in which each renewal of the stress gave, for the most part, a less and less deflection.

10. It is apparent from the foregoing experimental results, that every application of a transverse stress to a bar must induce some change in its molecular condition, which continues, with variations that may be either progressive or fluctuating, for a greater or less interval of time. The duration of sensible influence varies with the amount and duration of the stress. For the smaller strains it is but a few minutes; for the larger several days. The prolonged influence of strains applied from day to day to a bar, was apparent from the fact that the same stress did not on different days produce either the same deflection or the same set. It was strikingly shown in the experiments with the steel bar by causing the bar, to which loads had been repeatedly applied for several previous days, to rest on its opposite side, and comparing the deflection and set with those obtained immediately before the reversal. It was found that the deflection produced by $18\frac{1}{3}$ pounds was $\frac{1}{16}$ greater than the deflection produced by the same weight just before the reversal; and the set obtained was now many times greater than before. The deflection also now increased with a prolongation of the strain, whereas it before decreased. Also the set now increased for a considerable interval of time after the withdrawal of the strain, whereas it before decreased.

11. There was no discernible limit of elasticity, revealed by the experiments, with either wood, iron, or steel. A perceptible set obtained, with each material, immediately after the stress was removed, however small its amount, until the set fell below the lowest possible determination of which the apparatus was capable (viz: $\frac{1}{10000}$ of an inch, as the experiments were ordinarily conducted.) To test the question still farther, the delicacy of the measuring apparatus was largely increased, by the adaptation of a device for magnifying the movements to be observed: and it was found that the least perceptible immediate set was still limited only by the capability of detecting, with the apparatus, minute displacements.

If we take for the limit of elasticity the condition of things at which a *permanent* set is obtained, the case is different. Thus it was found that the set which subsisted after the pine stick (3 in. by 3 in. and 4 ft. long), had been loaded at its middle with 200 pounds ($\frac{1}{10}$ the theoretical breaking weight), eventually passed off entirely. This was the case whether the stress was momentary or prolonged, and whether it was applied but once or repeatedly. But with a load of 500 lbs. a permanent set was obtained, as the result of a single application of the stress; and repetitions of the stress were attended with a continual increase in the depression of the middle of the bar. It may accordingly be affirmed that a practical limit of elasticity exists, but not a theoretical one.

12. If a bar, on the withdrawal of a transverse stress, fails to recover its original line of position, or, technically speaking, has a set, it is plain that its integrant molecules have not returned precisely to their original positions, and that the distances between contiguous molecules have either increased or diminished —increased in the line of the longitudinal fibers that have experienced a tensile strain, and decreased in the line of those which have experienced a compressive strain. Now we have seen that, as the result of a series of increasing transverse stresses, the set increases continuously with the stress, from the lowest amount capable of detection with the measuring apparatus employed. We must therefore conclude that, after the application of a series of increasing strains, in which the molecules are relatively displaced by minute fractions of their intervening distances, they take up, when the strain is removed, a series of new positions of equilibrium, differing by excessively minute degrees from those previously occupied. We may draw the same conclusion from the experiments on the set produced by a series of direct tensile and compressive stresses, made by Hodgkinson, Chevandier and Wertheim, and other experimenters. This general conclusion, to which experiments on set, under every variety of strain, conduct, leads to the inevitable inference that *the effective forces exerted by the molecules on one another have suffered some change of intensity, in consequence of the stress applied to the bar under experiment.* Viewing the residual displacement of the molecules, in their relative positions, as a mechanical problem, we are constrained to regard the effective molecular forces, that take effect at a given distance, as having acquired a different intensity. We have confirmatory evidence of this induced molecular condition of the bar in the fact that all the diverse effects, which may ensue on subsequent applications of a transverse stress, are found to be either less or greater than those previously observed under similar conditions.

13. The fluctuations that have been noticed as occurring in the set with the lapse of time, reveal the fact that the change in the intensities of the effective molecular forces, which results from the temporary application of the stress, is not permanent but fluctuating; and may, according to the amount of the stress applied, rapidly pass off, or, after a partial collapse, be slowly recovered again. It should be observed, however, that the curious fact of the increase of set which ordinarily succeeds the first sudden fall, may be in part attributable to the gradual

propagation inward of the greater disturbed condition of the molecules of the upper and lower fibers.

14. The general correspondence in the phenomena of set and altered deflection, that obtain with different materials altogether precludes the idea that they may result, either wholly or in a considerable degree, from irregular strains subsisting in certain parts of the bar before the stress is applied, and which are more or less modified by the stress; as some persons have conjectured. The change that supervenes must be a general one, or one in which all the molecules participate, though in diverse degrees according to the amount of molecular displacement. The especial character of the change, for each individual molecule, must depend upon the kind of strain to which the molecule is exposed, whether tensile. compressive, or shearing; and not on the nature of the material subjected to strain.

15. If, as experiment has established, when the distance between two contiguous molecules has been forcibly altered, the molecules, when again left to their mutual actions, no longer exert, at the same distance, effective actions of the same intensity as before, it is apparent that *the molecules in the act of displacement have experienced some change, either in their dimensions, or in their internal mechanical condition.* This change must result from the change that took place in the mutual action of the molecules when they were urged nearer to each other, or separated to greater distances. It must be experienced by the ultimate molecule, whether this be indentical with the integrant molecule or not—that is whether we regard the integrant molecule as a single ultimate molecule, or as a group of ultimate molecules. For it is plain that a group of ultimate molecules could not undergo an internal change, that abides after all external actions have ceased, unless its constituent molecules have suffered a change, by reason of which they no longer act upon one another with the same intensities of force as before.

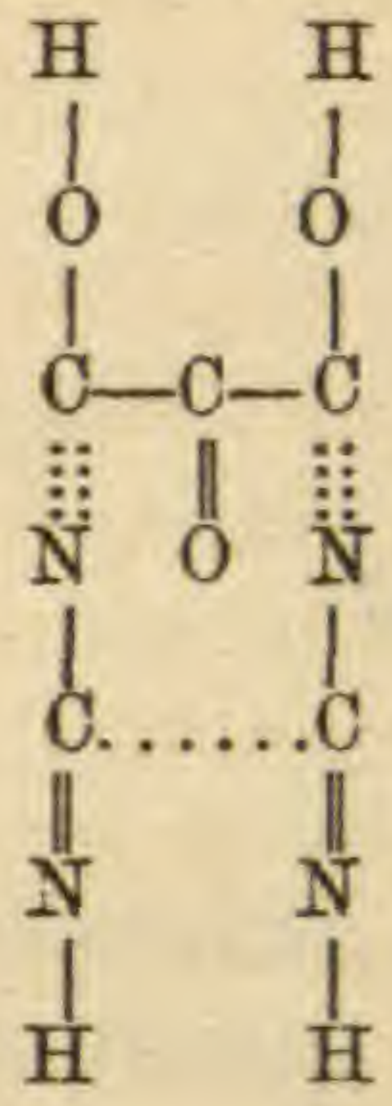
It is well known that with Physicists the "chemical atom" has come to be replaced by the "ultimate molecule." Of the probable physical constitution of the ultimate molecule different conceptions have been formed. To those Physicists who regard it as made up of a limited number of precisely similar atoms, endued with unvarying forces—of attraction at certain distances, and repulsion at other distances—I leave it to reconcile this conception with the legitimate inference to be drawn from experimental results, that the ultimate molecule is liable to a change of mechanical or physical condition, with every slight displacement it may experience—a change which subsists after the constraining cause of the displacement has ceased to act; and may, under different conditions, either be permanent, or gradually subside with fluctuations.

ART. XXXVI.—*On the constitutional formulæ of Urea, Uric Acid, and their derivatives*; by Professor J. W. MALLET, University of Virginia.

[Continued from page 194.]

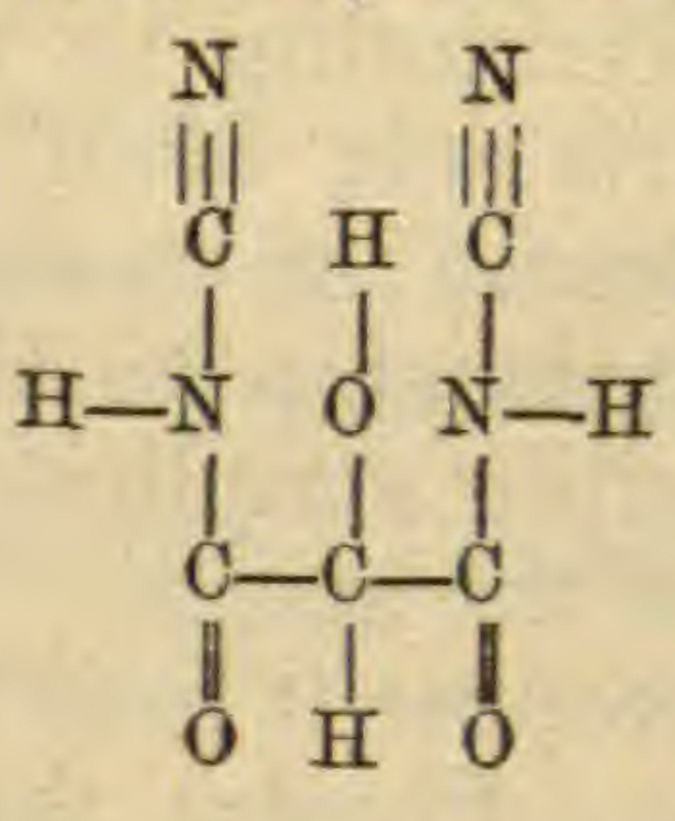
FOR uric acid itself, $C_5H_4N_4O_3$, the di-ureide of mesoxalic acid, I would propose the formula (No. 1).

1. Uric acid (di-basic).

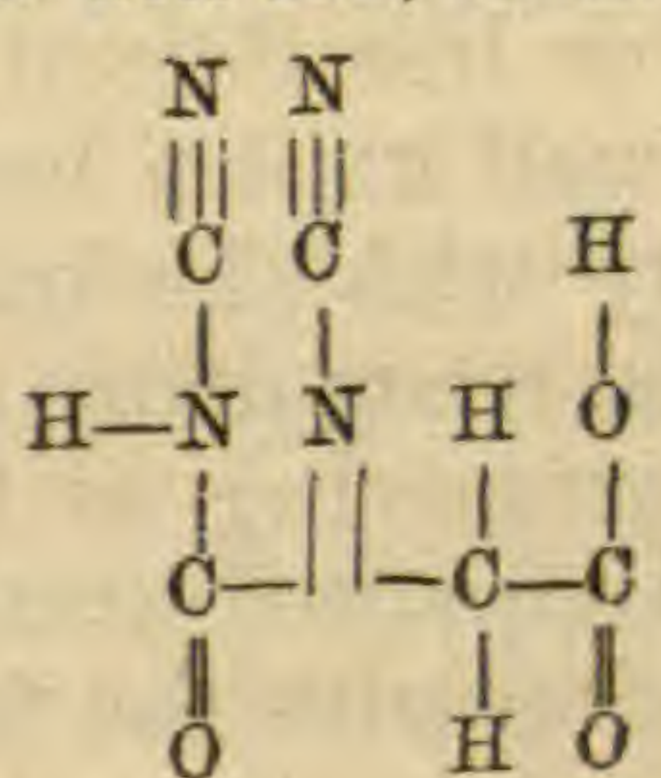


Nos. 2 to 9 show, for the sake of comparison, some of the numerous formulæ which have of late years been given for this important substance.

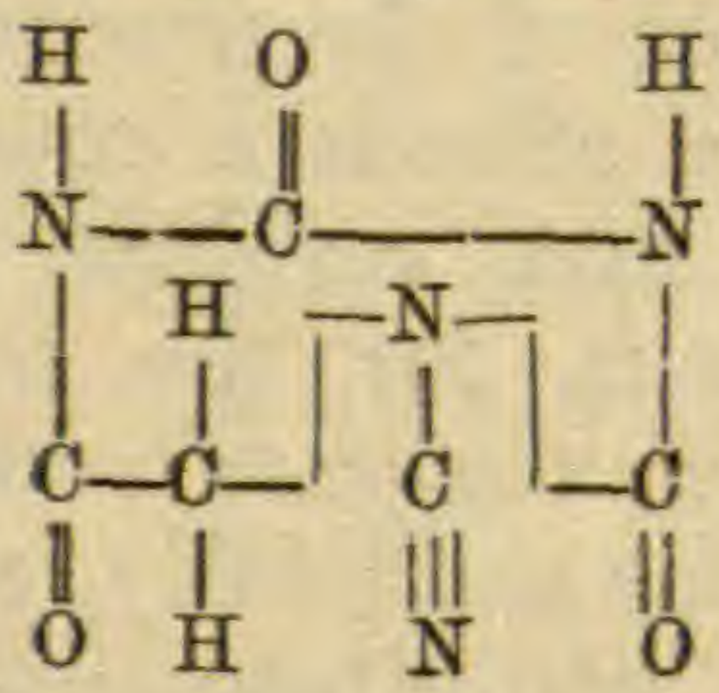
2. Uric acid, Baeyer* and Kolbe.†



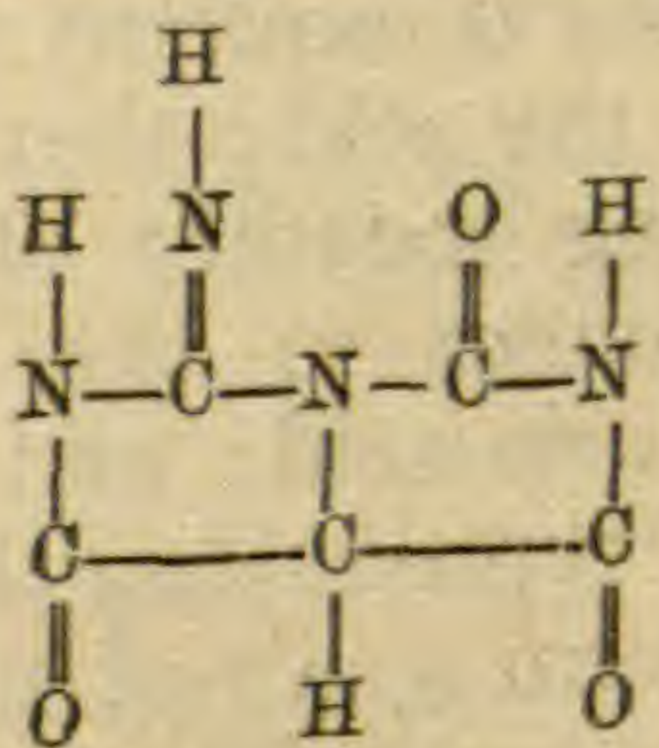
3. Uric acid, Hüfner.‡



4. Uric acid, Mulder.§

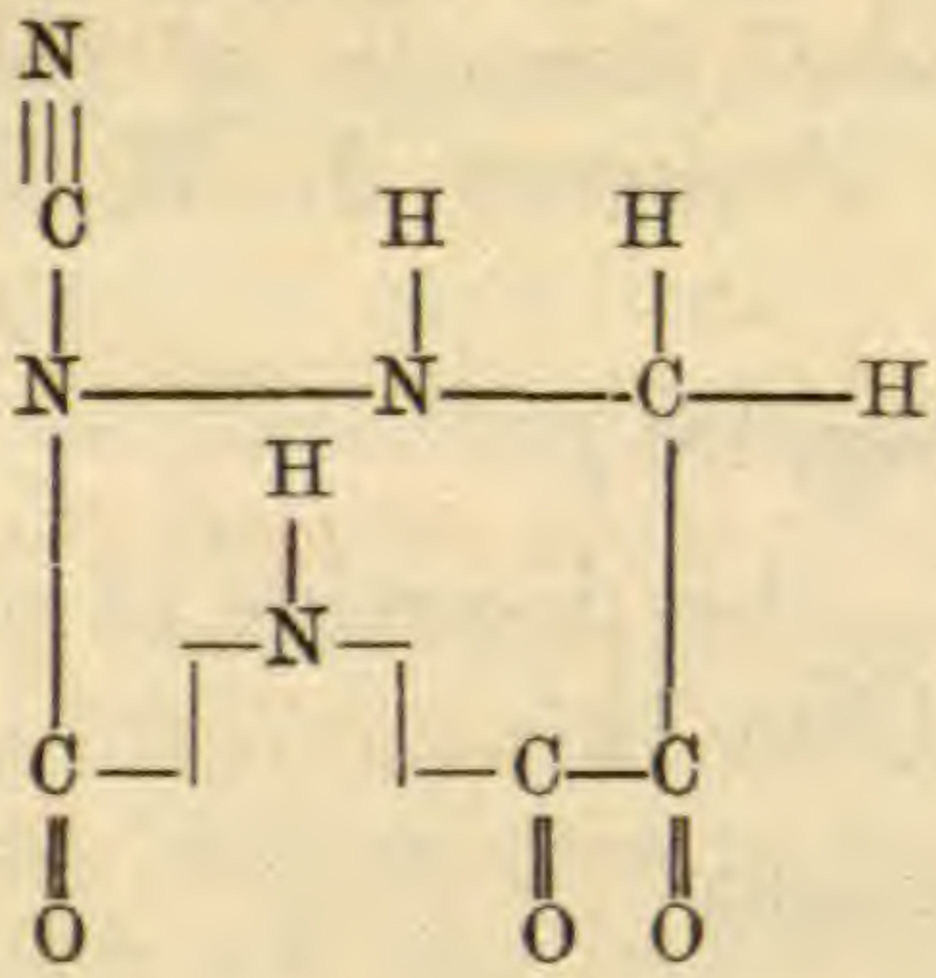


5. Uric acid, Erlenmeyer.‖

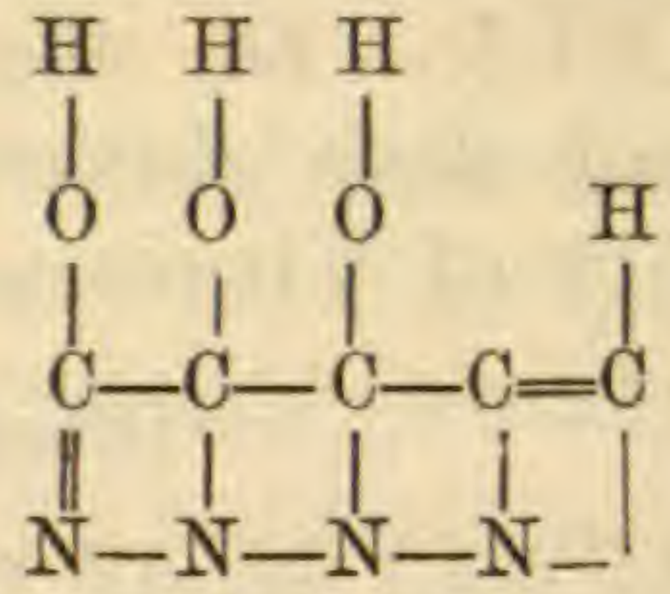


* Ann. der Chem., cxxvii, 235.
† Journ. für prakt. Chem., II, i, 134.
‡ Journ. für prakt. Chem., II, iii, 23.
§ Bericht. d. deutsch. chem. Gesellsch., vi, 1237.
‖ Münch. Akad. Ber., ii, 276.

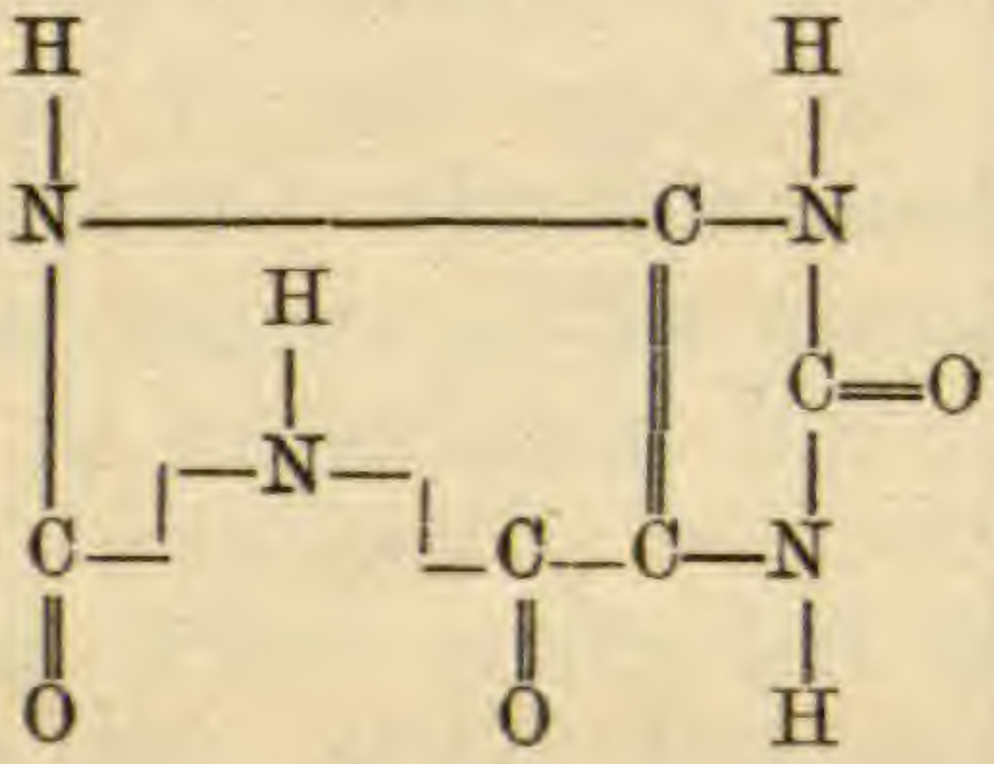
6. Uric acid, Strecker.*



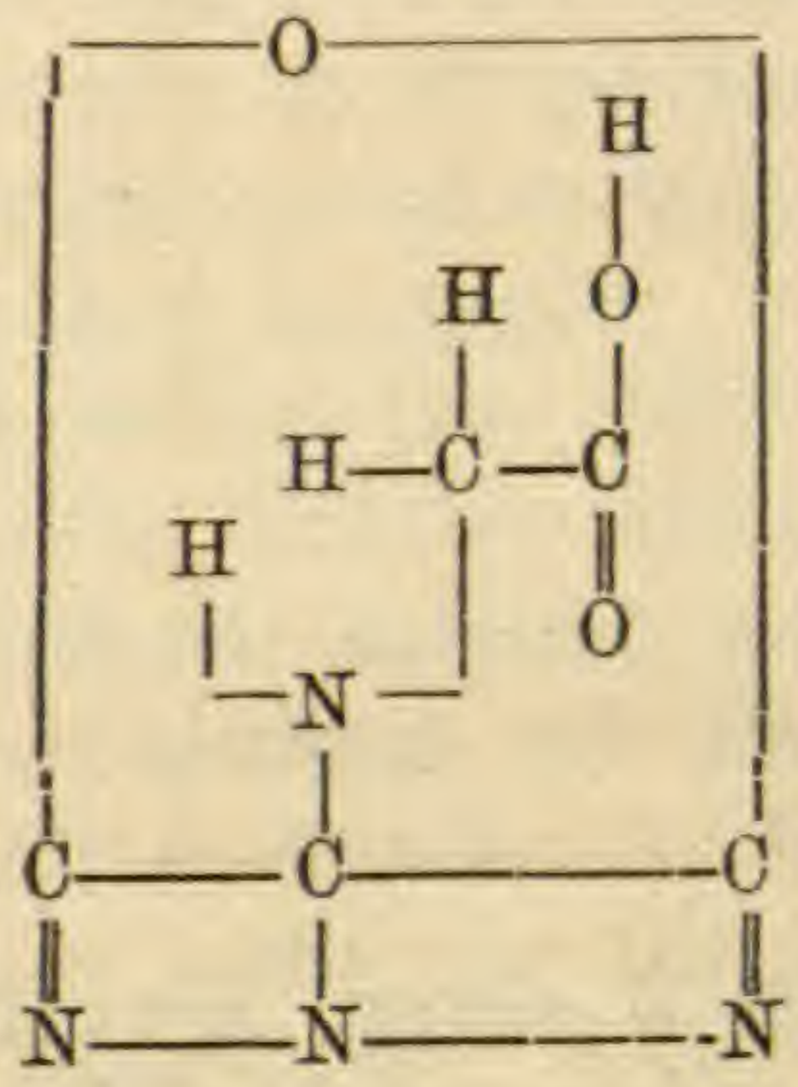
7. Uric acid, Gibbs.†



8. Uric acid, Medicus.‡



9. Uric acid, Drechsel.§



While each of these formulæ possesses advantages for the explanation of certain cases of decomposition and certain derived products, an attentive study will show, I think, that all are more or less defective as to accounting in the simplest way for the well known basicity of uric acid itself, bringing it into harmony with the general structure of non-nitrogenous organic acids, recognizing a close relationship to the 3-carbon series, and preserving as far as possible simplicity and symmetry in the supposed arrangement of the atoms.

In connection with the formula I propose it may be noticed: that it does account for uric acid being dibasic; that it derives it as directly as possible from a residue of the 3-carbon mesoxalic acid; that it explains simply most of the observed decompositions of the acid; that it perhaps affords a reason, in the direct linking together of the two urea residues as well as their attachment to the acid nucleus, for the comparative stability of uric acid; and that it also suggests a cause of the difficulty of reproducing this substance artificially, since in the attempt to form a salt of urea with a non-nitrogenous acid and then remove water the basic hydroxyl might be eliminated and the normal acid type destroyed, whereas this type is pre-

* Lehrb. d. org. Chem., 1868, 800. † This Journal, Nov., 1868, xlvi, 293.

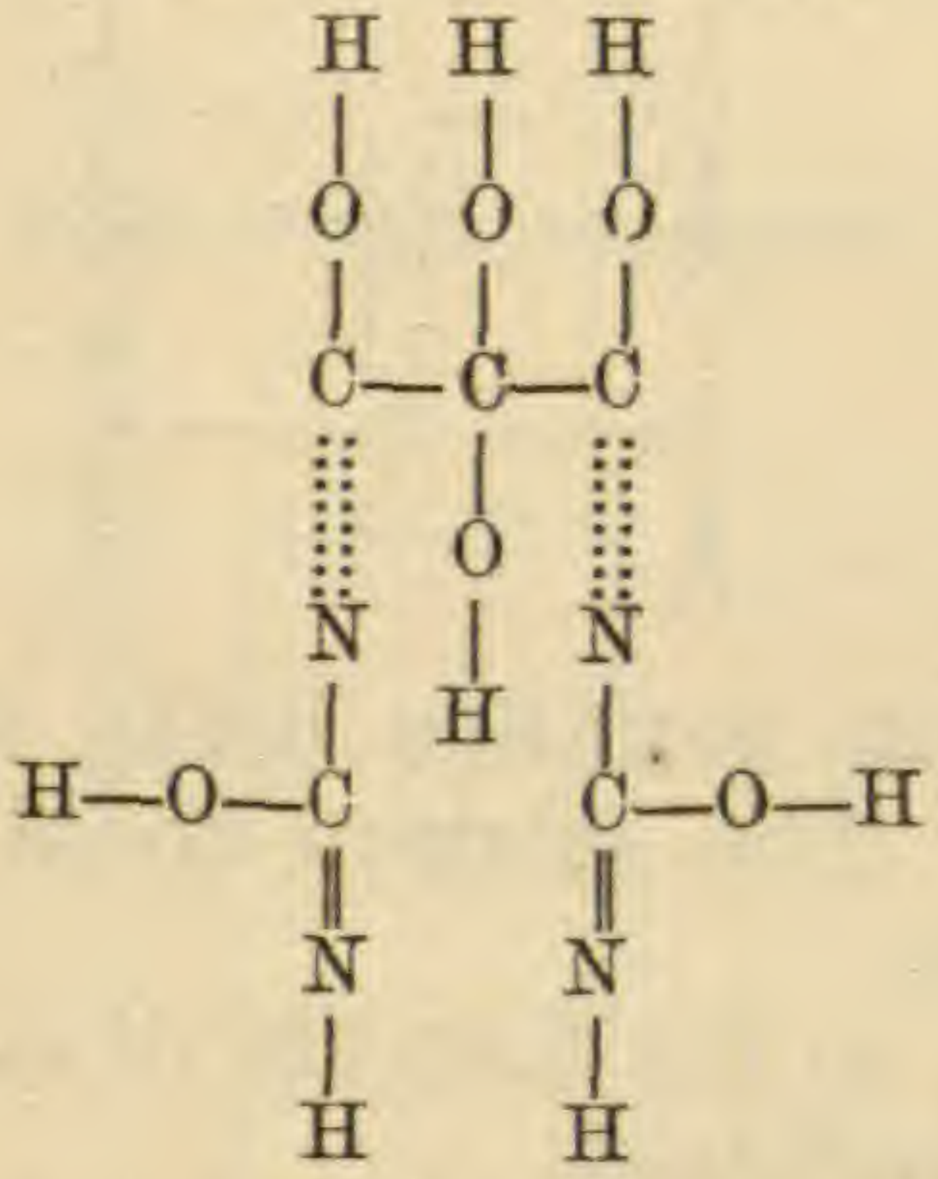
‡ Ann. der Chem., clxxv, 243; where most of the formulæ above quoted are reviewed.

§ Chem. Centralbl., 4 Aug., 1875, 493.

served by the different mode of attachment of the urea residues exhibited in the formula now put forward. The mode of production from uric acid of allantoine, alloxan, paraban, etc., will be seen by comparison of the preceding formulæ.

Probably uroxanic acid, $C_5H_8N_4O_6$, is represented by No. 1

1. Uroxanic acid (dibasic).

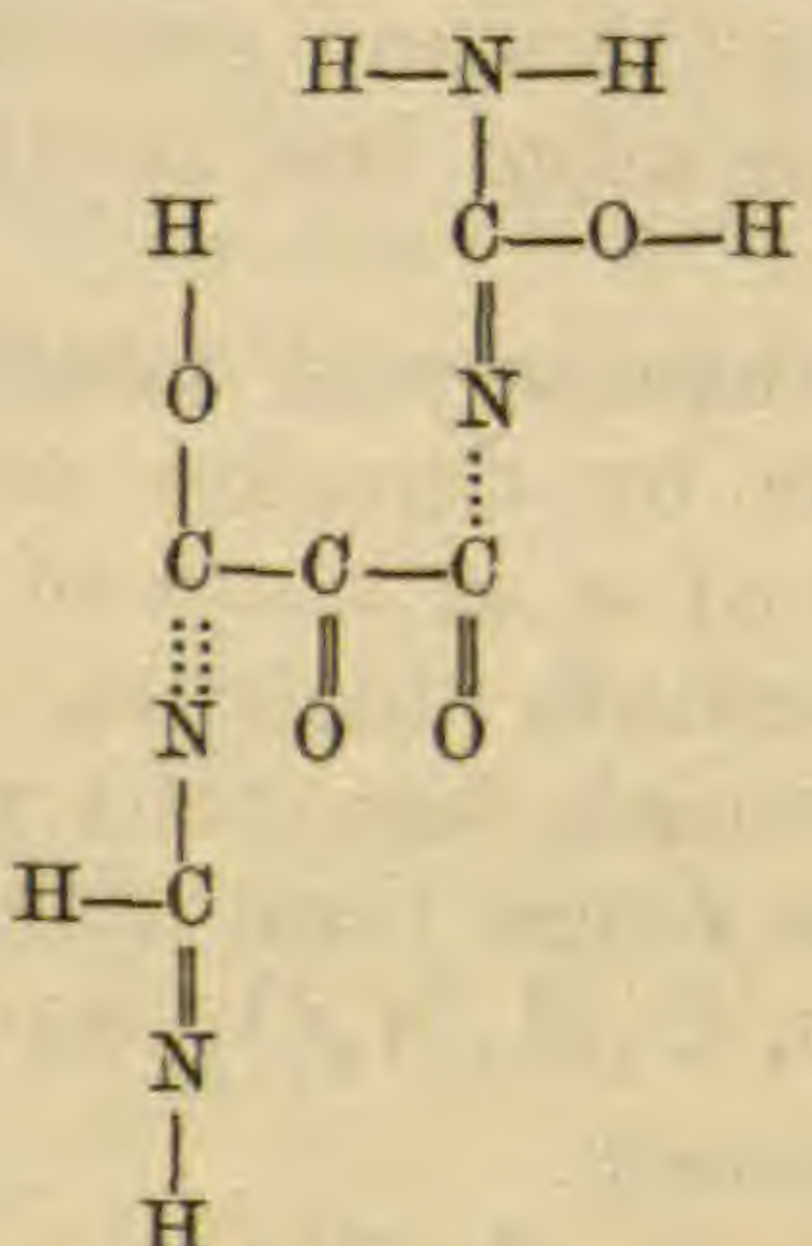


2. Oxonic acid (dibasic).

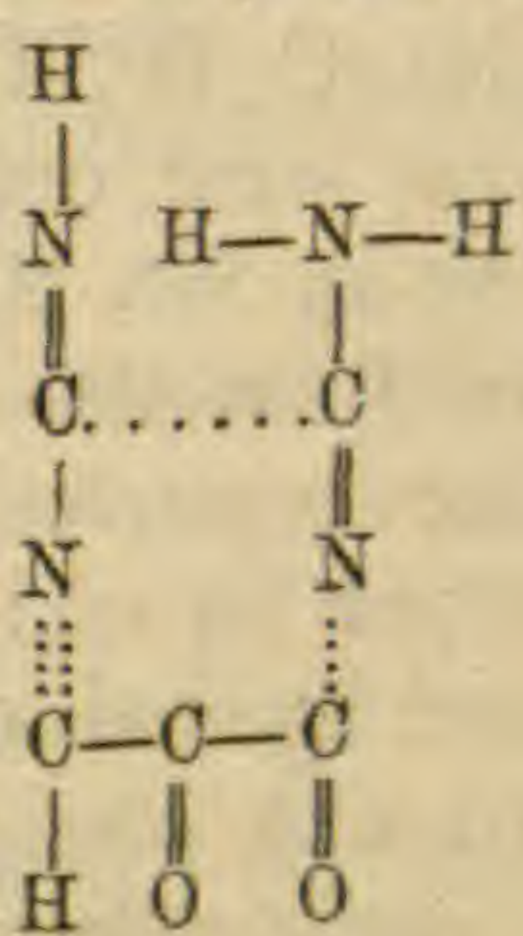
while the oxonic acid of Strecker and Medicus,* $C_4H_5N_3O_4$, produced under similar conditions, but showing by its containing only three atoms of nitrogen that it cannot include two complete urea residues like those of uric acid, may perhaps have the structure of No. 2.

The basicity of pseudo-uric acid, $C_5H_6N_4O_4$, may be explained by assuming its two urea residues differently attached, and in one of them an atom of hydrogen taking the place of hydroxyl—thus:

1. Pseudo-uric acid (monobasic).



2. Xanthine (weak base).

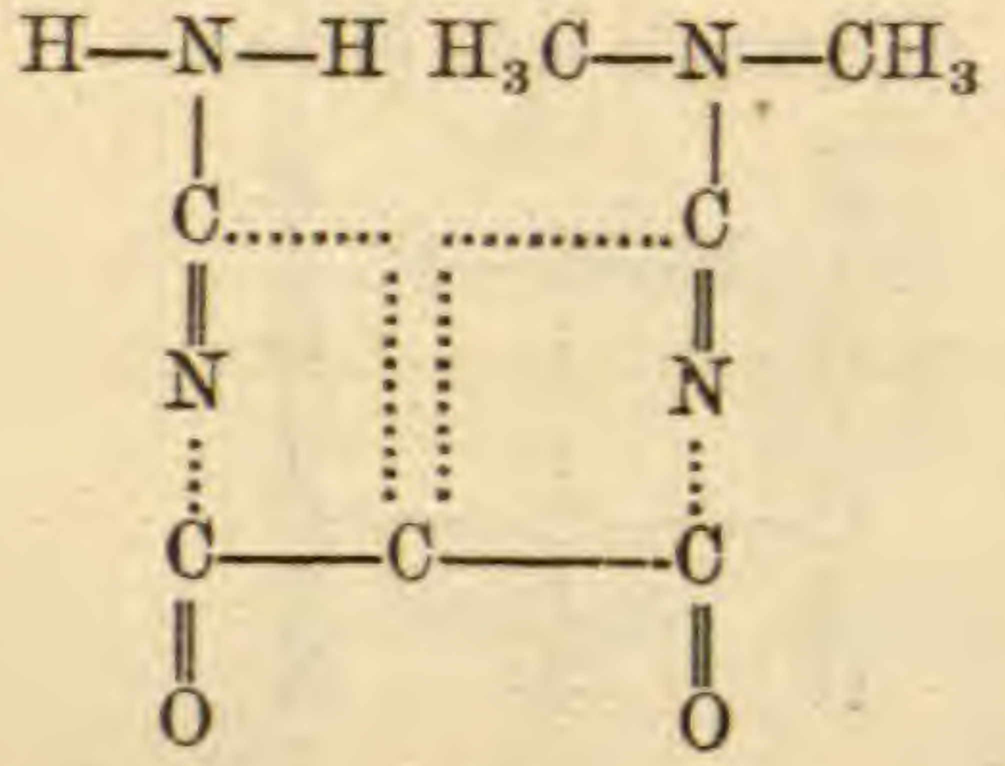


In xanthine, $C_5H_4N_4O_2$, whose empirical formula differs from that of uric acid only by an atom of oxygen, we have also two dissimilarly attached residues of urea, but the basic hydroxyl disappears altogether and with it the true acid character, while like urea itself xanthine is capable of uniting with metallic oxides as well as with acids.

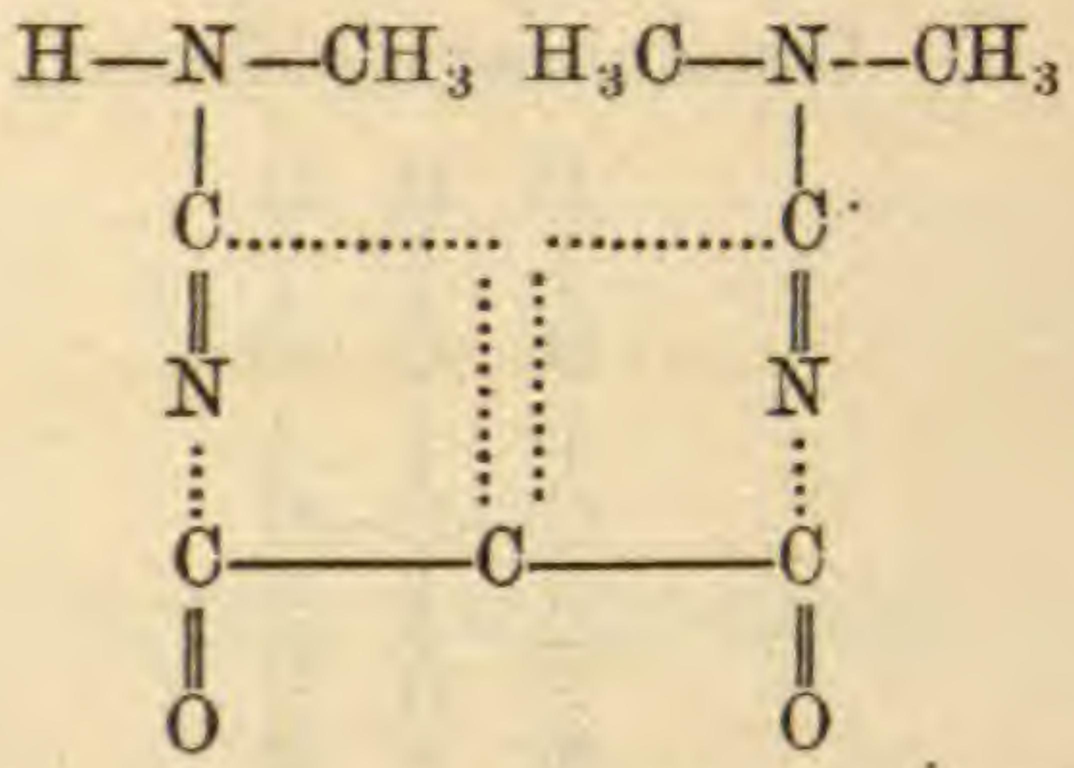
* Ann. der Chem., clxxv, 230.

This formula (No. 2) may serve to explain the fact that Strecker's di-methyl-xanthine is isomeric, not identical, with theobromine, $C_7H_8N_4O_2$, if we assume the latter to be as in No. 1, and caffeine, $C_8H_{10}N_4O_2$, as in No. 2,

1. Theobromine (weak base).



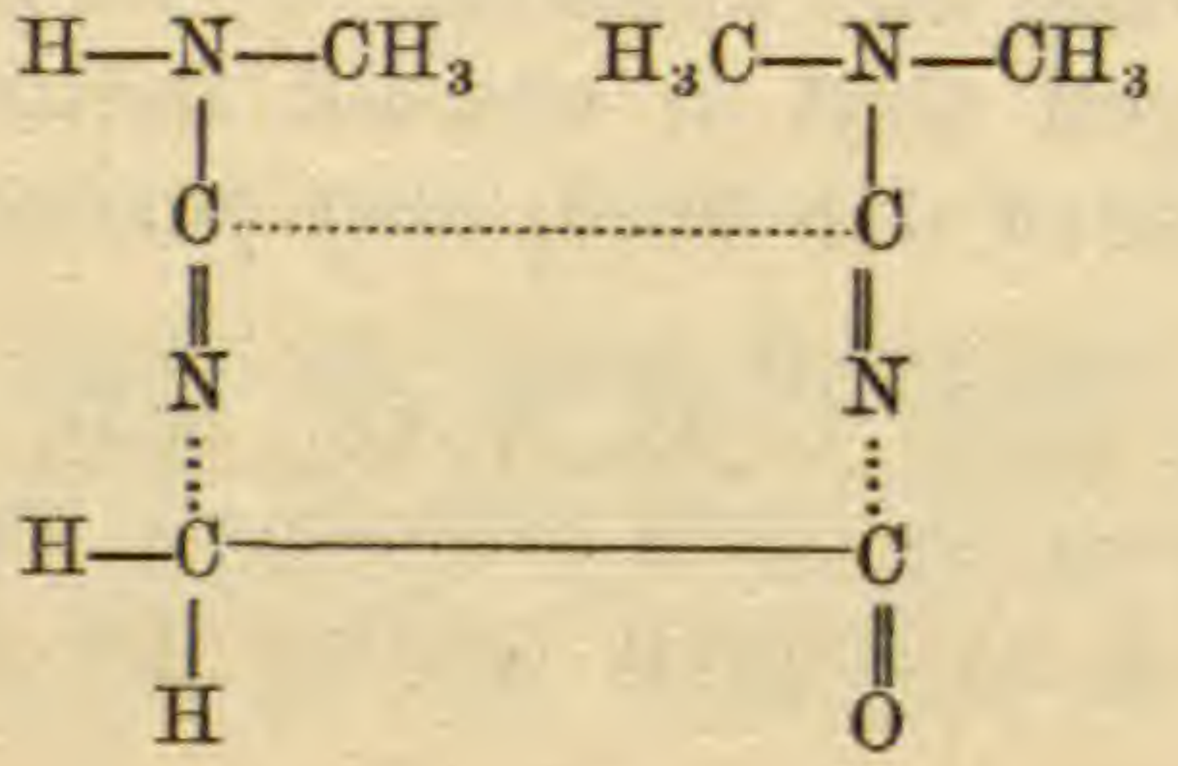
2. Caffeine (weak base).



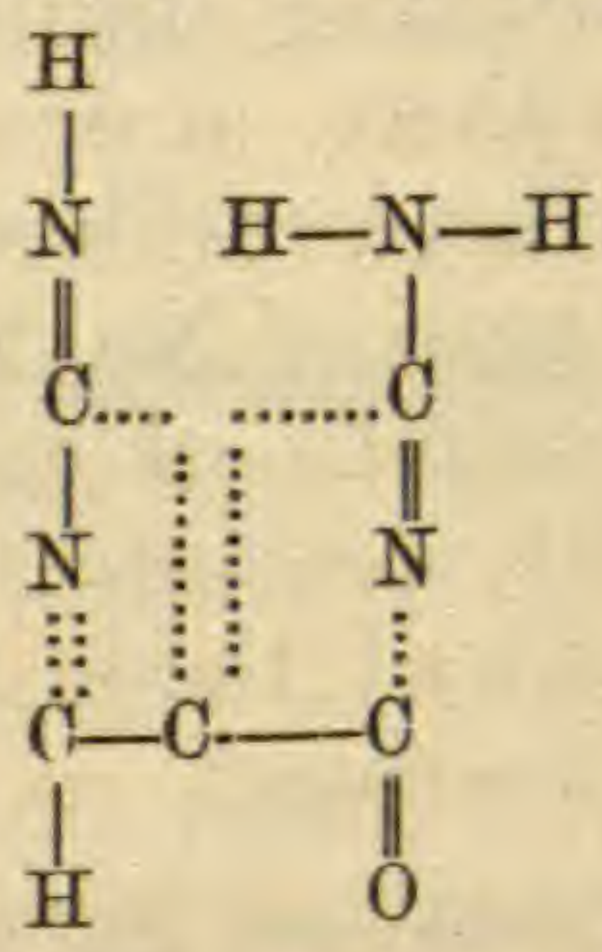
both urea residues in each of these formulæ being similarly connected with the acid nucleus. The relation to di-methyl-paraban (cholestrophan) is obvious.

We may probably assume No. 1 as the formula of caffeidine, $C_7H_{12}N_4O$ (a stronger base than caffeine).

1. Caffeidine.



2. Hypoxanthine (sarcine)—(weak base).

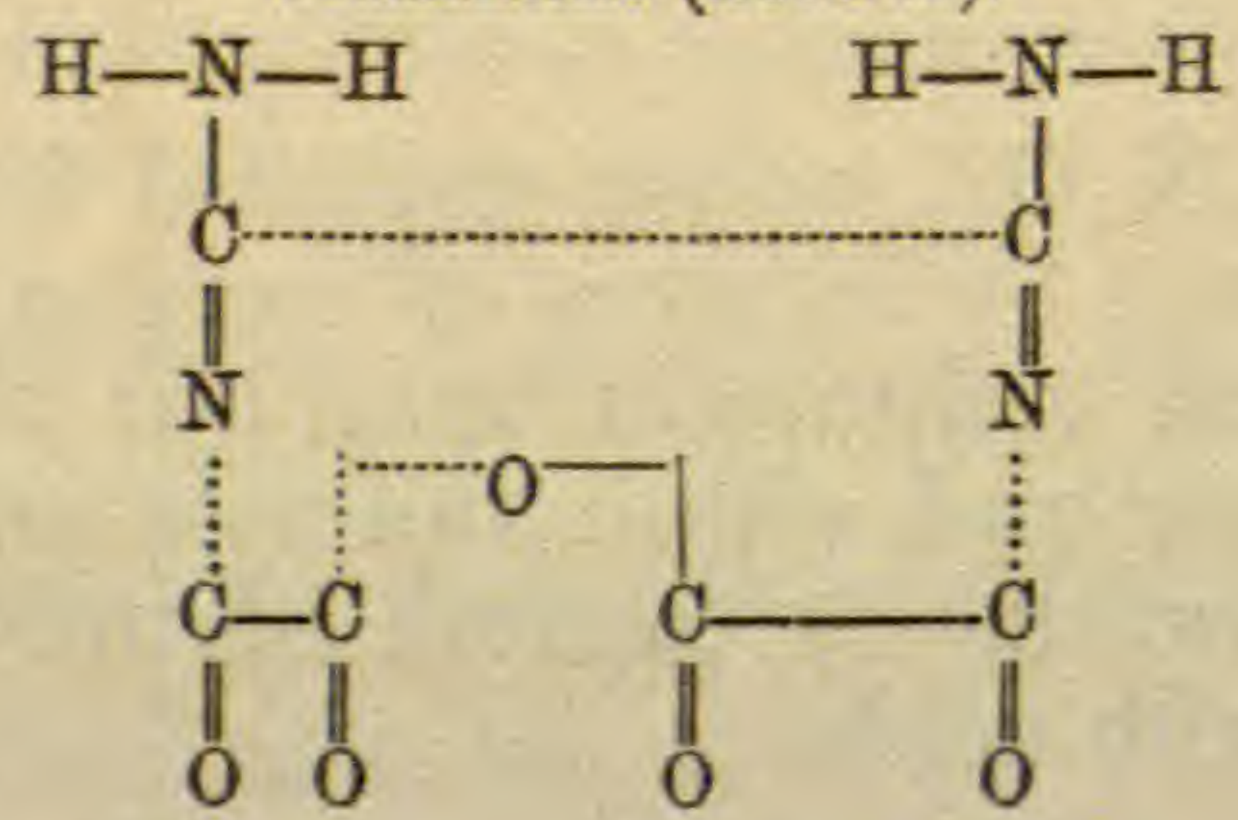


Hypoxanthine, $C_5H_4N_4O$, exhibits the same general character with xanthine, and containing one atom less oxygen may be represented as in No. 2, above.

The above formulæ for hypoxanthine and xanthine accord well with the reported production by Strecker of the latter from the former by oxidation, and of a mixture of both bases from uric acid by reduction with sodium amalgam.

Passing to the compounds in which two acid residues are united with each other and at the same time with residues of urea, we may formulate oxalantine, $C_6H_4N_4O_5$, as follows:

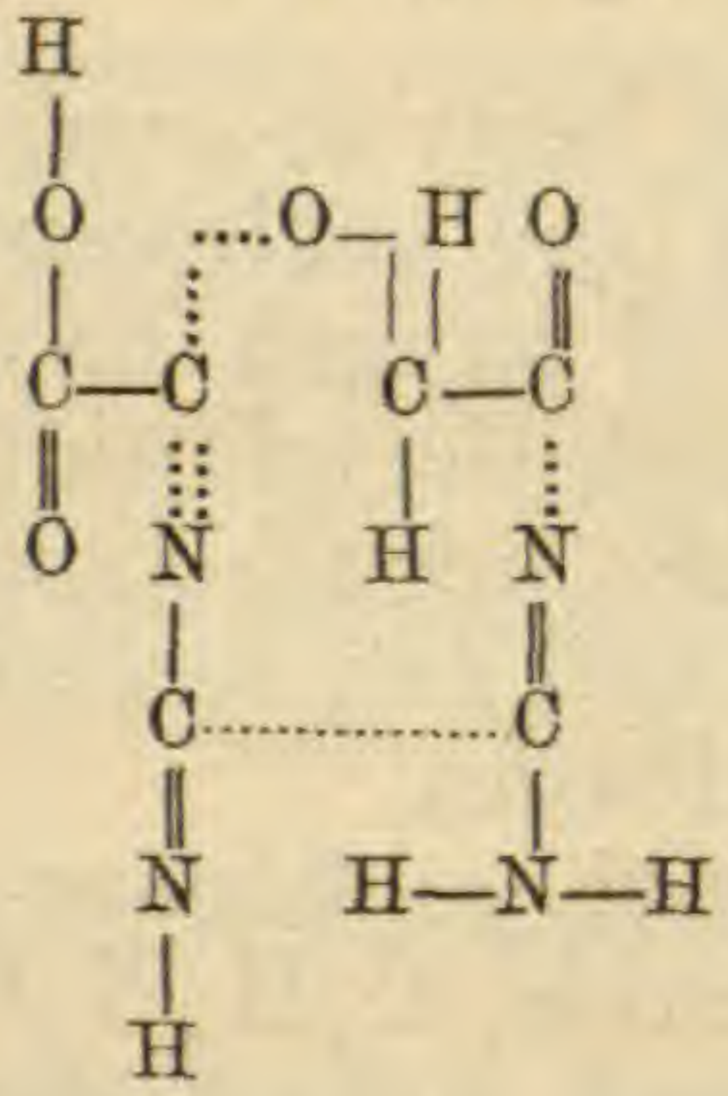
Oxalantine (neutral).



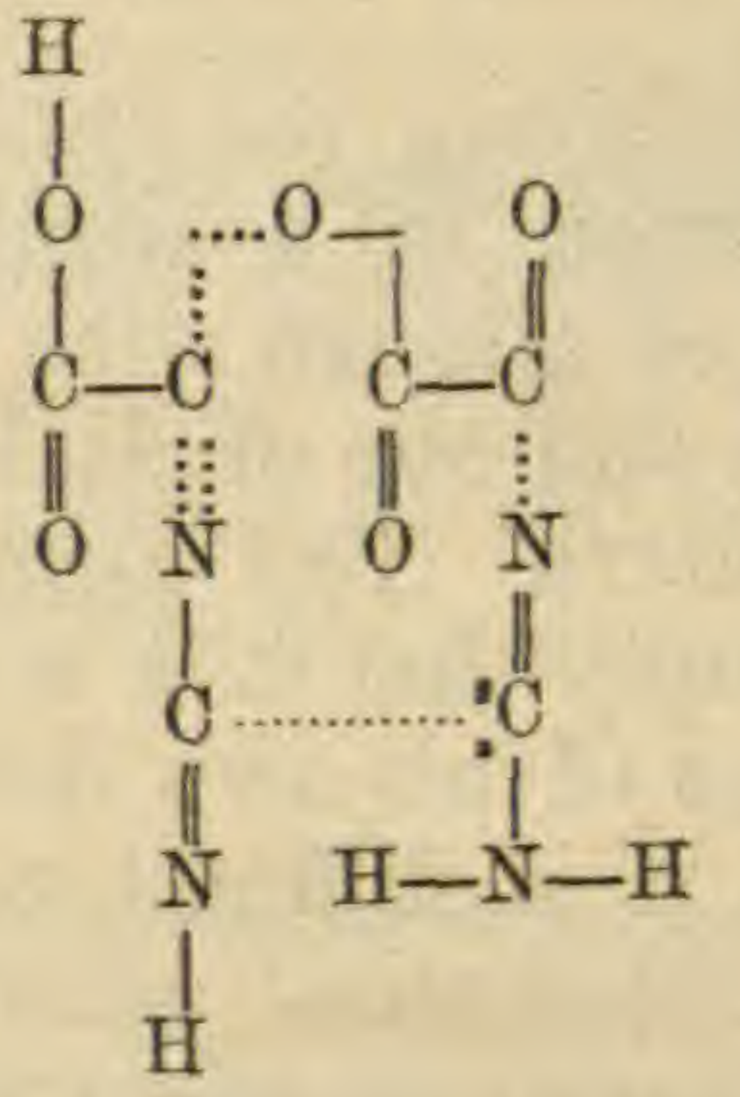
thus directly explaining the relation to paraban, and production therefrom by coalescence of two molecules with elimination of an atom of oxygen (or from a molecule each of oxaluric acid and paraban, with removal of an atom of oxygen and a molecule of water).

The union of hydantoine with allanturic acid, with separation of water, gives for allituric acid, $C_6H_6N_4O_4$, the formula No. 1,

1. Allituric acid (monobasic).



2. Leucoturic acid (monobasic).

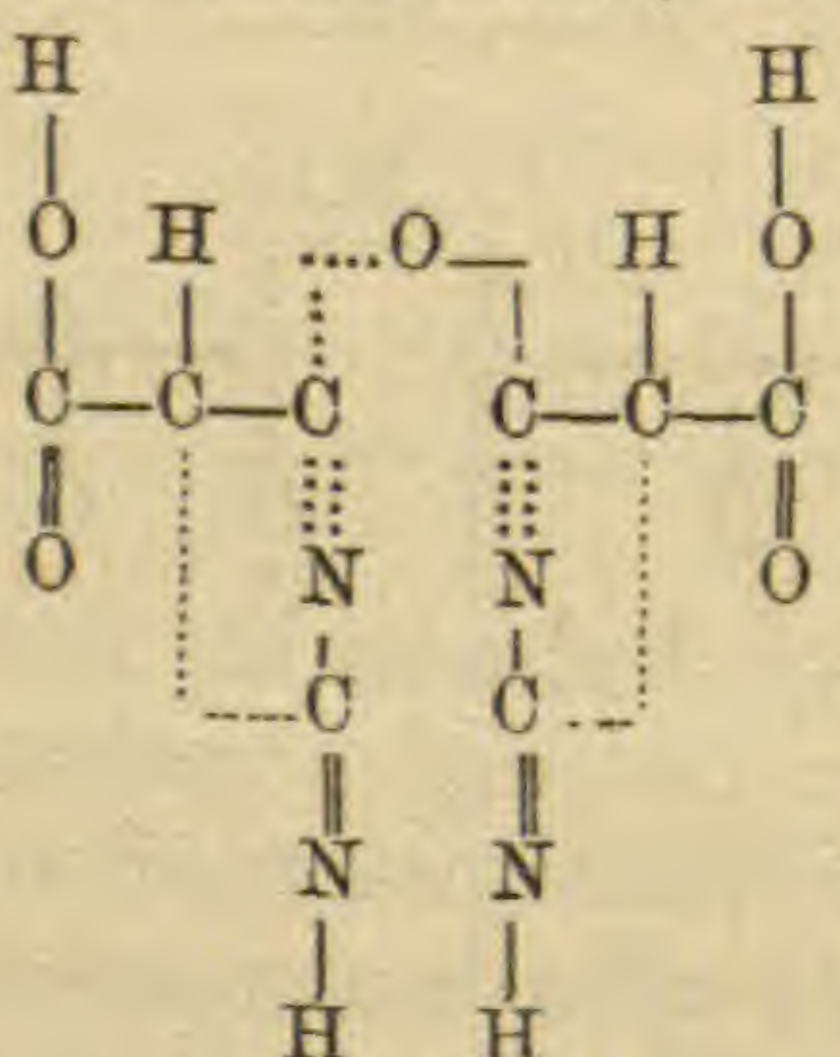


and the like union of paraban and allanturic acid leads to No. 2 for leucoturic acid of Schlieper, $C_6H_4N_4O_5$. This last formula explains the possibility at least of a difference between leucoturic acid and oxalantine, the identity of which does not seem clearly established.

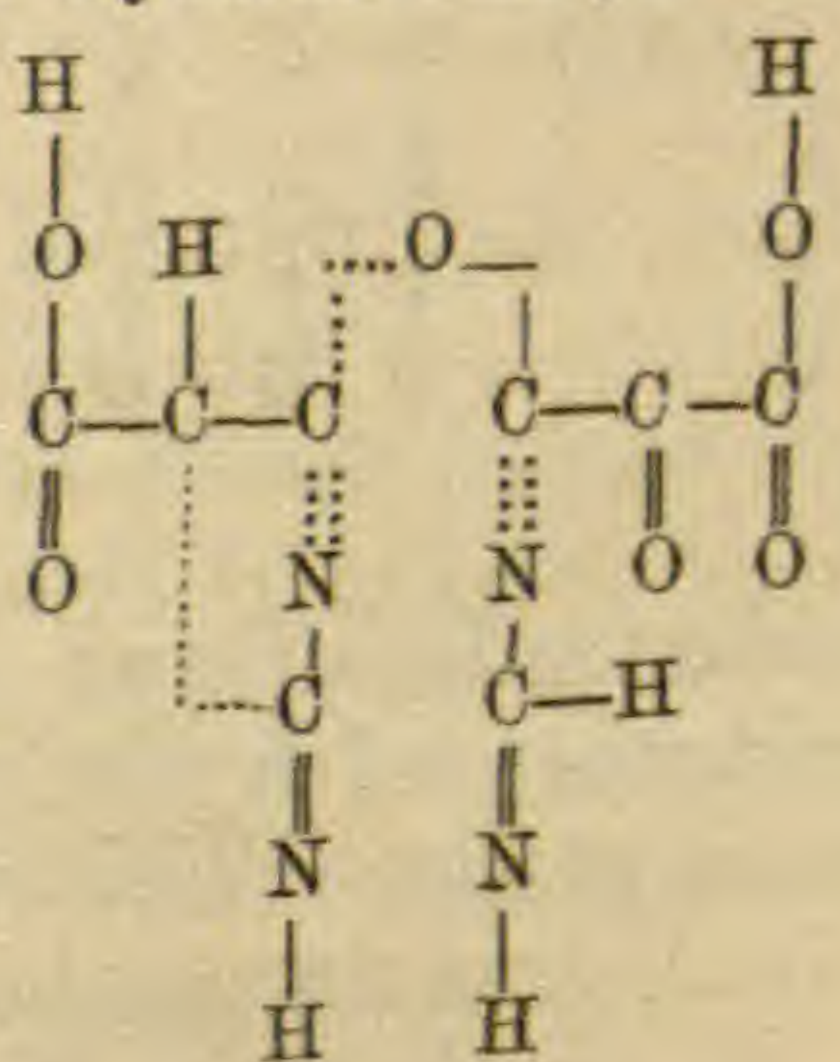
In each of the last two cases we have one ureic residue *inverted* as regards its mode of attachment to the acid nucleus when the coalescence takes place.

Two molecules of (dibasic) barbituric acid unite with separation of a molecule of water, giving rise to di-barbituric acid, $C_8H_6N_4O_5$, with unchanged basicity,

1. Di-barbituric acid (dibasic).



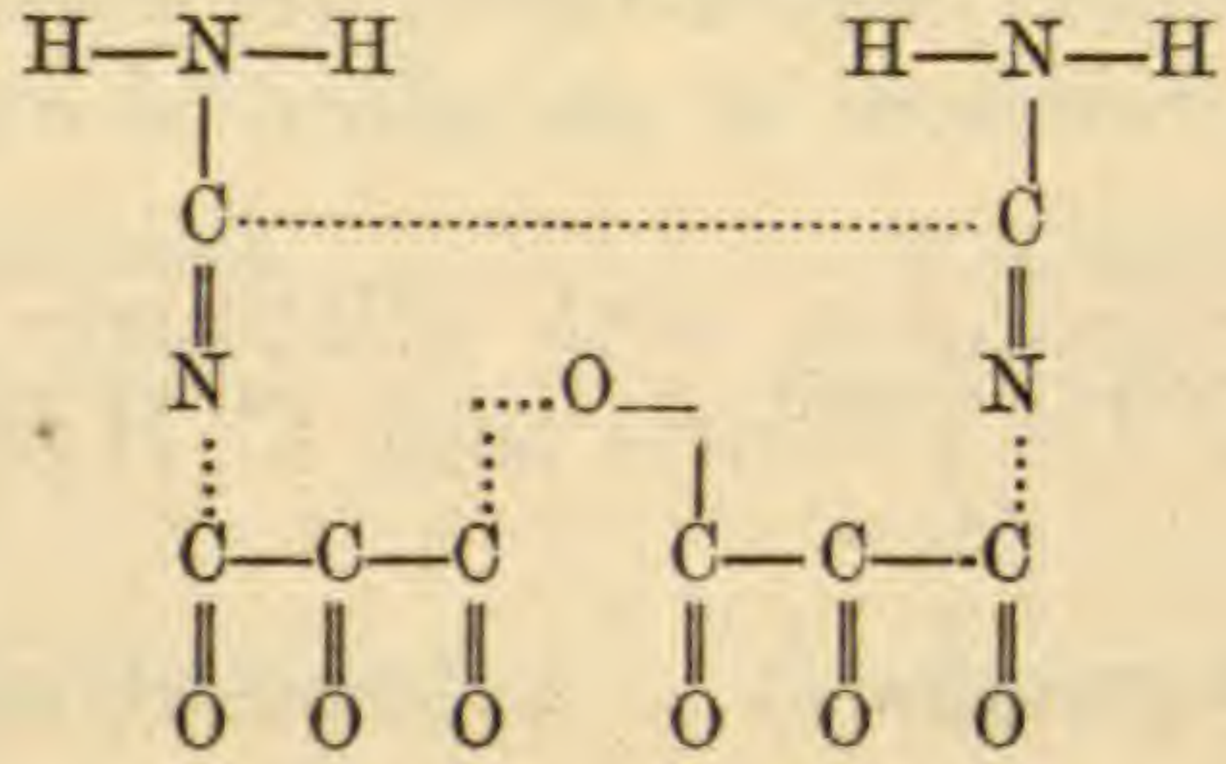
2. Hydurilic acid (dibasic).



while the union of a molecule of barbituric acid and one of dialuric acid, with *inversion* of the ureic residue of the latter and elimination of a molecule of water, gives us hydurilic acid, $C_8H_6N_4O_6$, (No. 2).

Alloxantine, $C_8H_4N_4O_7$, may be represented as

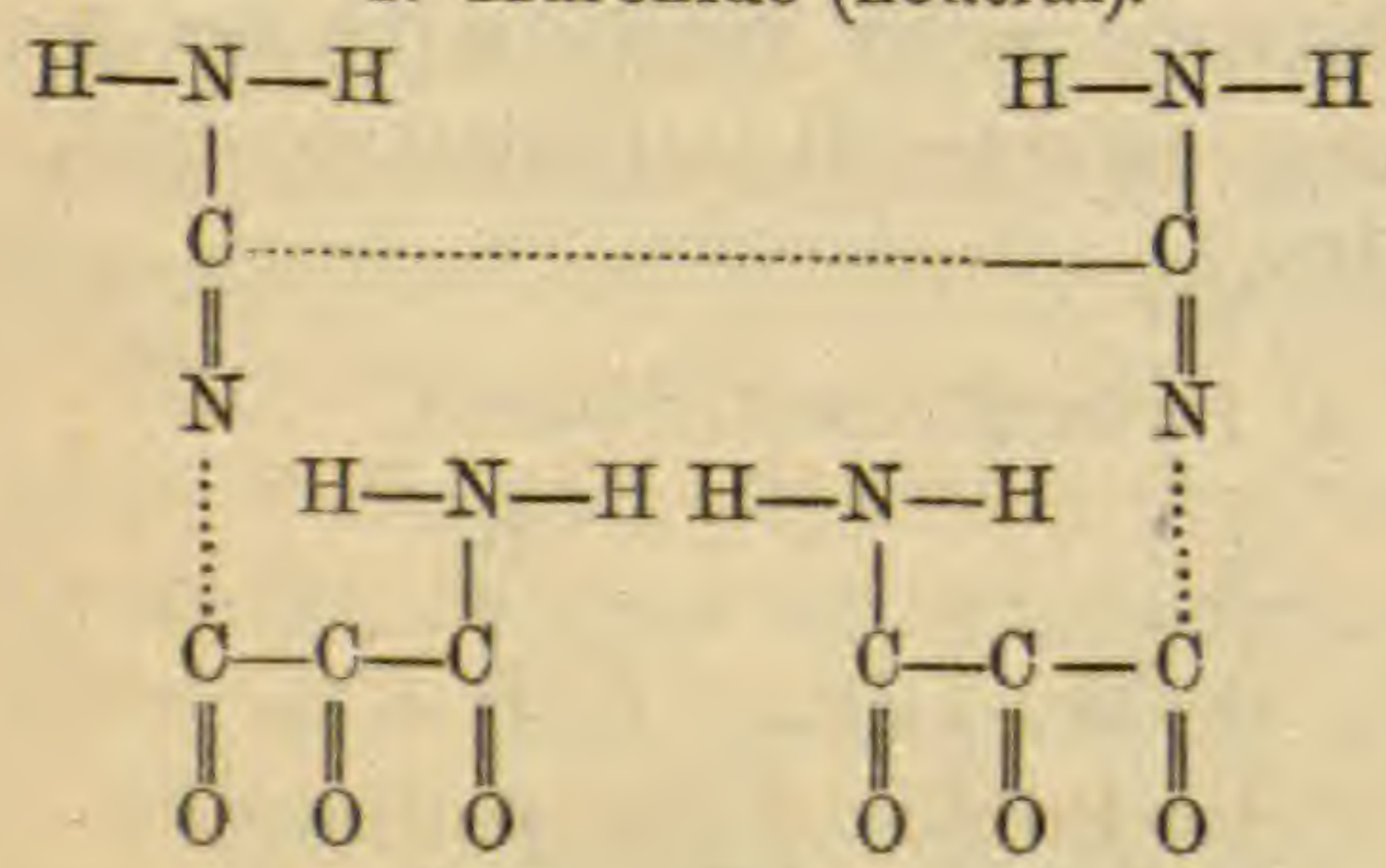
Alloxantine (neutral).



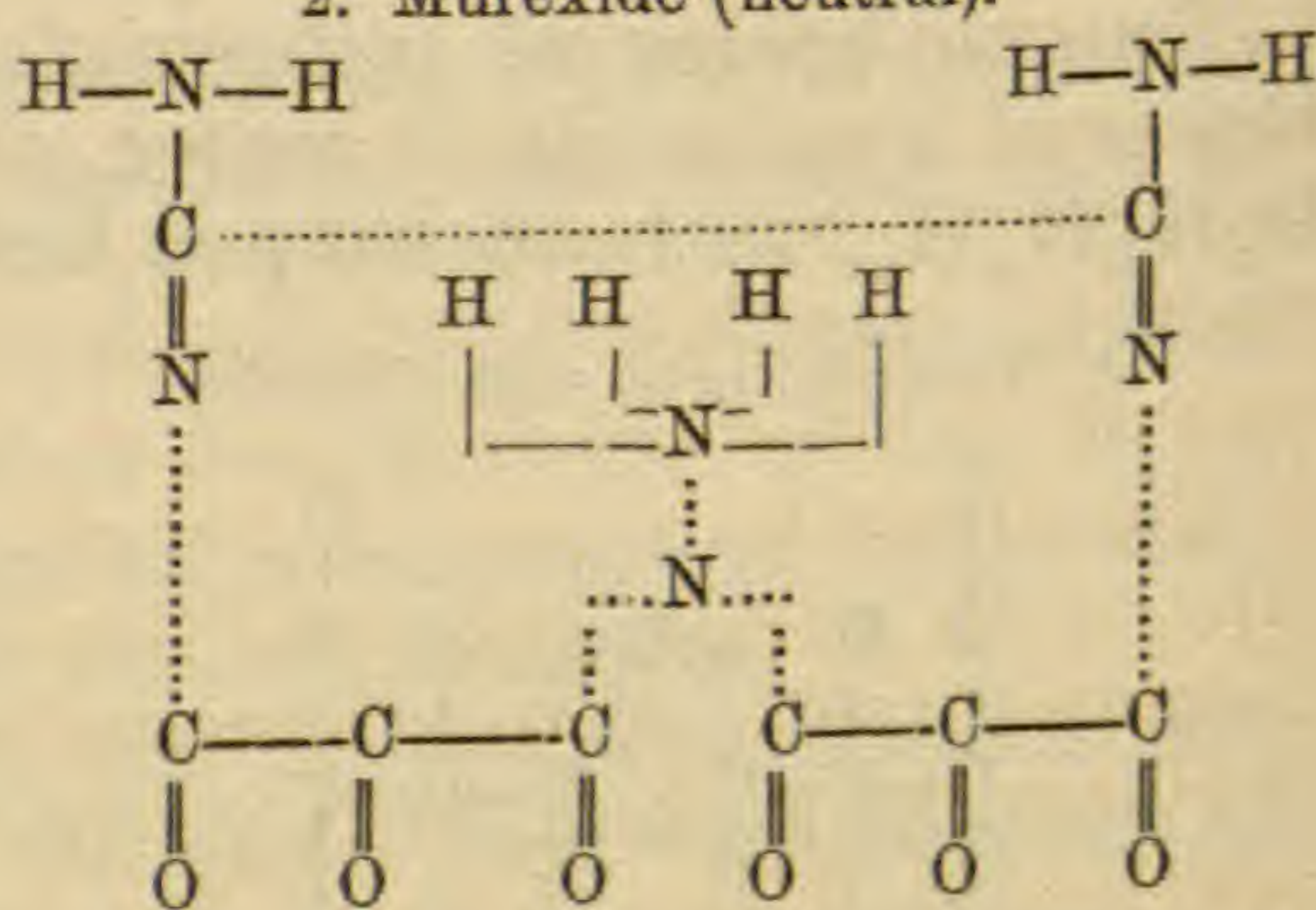
clearly exhibiting the analogy of this body to oxalantine, and its production in a similar way by the coalescence of two molecules of alloxan with separation of oxygen (or a molecule of alloxan and one of dialuric acid with the additional separation of water). The formula of Gibbs for alloxantine would seem to imply that it is a monobasic acid (or, according to the exact terms of his own definition,* tribasic).

Finally, it is difficult to suggest with confidence a formula for the problematical substance murexide, $C_8H_8N_6O_6$. If an amide character be admitted for it; and it does seem that evidence is still wanting to conclusively prove that it is an ammonium salt, especially in view of non-production of purpuric acid and the undoubted existence of isomeric iso-purpurates (and possibly other salts) which may have led to an undue assumption of identity of type between murexide and its metallic derivatives; we may perhaps assume this substance to have the formula No. 1,

1. Murexide (neutral).



2. Murexide (neutral).



in which the union of two molecules of dialuramide is effected, with elimination of hydrogen, by the linking together of the ureic carbon atoms. This view of the constitution of murexide (making it alloxantine-amide) obviously affords a simple explanation of its production from dialuramide by oxidation, from ammonium dialurate by heating, from alloxantine and alloxan by the action of ammonia, &c., and also suggests the probable

* Loc. cit.,—"cyanyl may be regarded as the acidifying term. Its quantity, therefore, determines the basicity of the acid."

ease with which isomeric changes may be brought about. If murexide be ammonium purpurate, the formula might perhaps be changed to the form in No. 2.

While the views above stated as to the structure of the numerous and interesting compounds derivable from urea and uric acid are liable to objection at sundry minor points, and in several instances other arrangements of the elements might be adopted without interference with the main idea, I believe that on the whole the constitutional formulæ set forth in this paper more nearly represent the present state of our knowledge of this group than any others which have been proposed, and especially possess the advantage of better explaining the chemical character or function of the substances referred to,* while at least equally well exhibiting the nature of the changes by which they are produced from each other.

University of Virginia, Nov. 4, 1875.

ART. XXXVII.—*On the Evidences of horizontal crushing in the formation of the Coast Range of California;* by JOSEPH LECONTE.

[Read before the National Academy of Sciences, November, 1875.]

IT will be remembered that in a former paper "On the formation of the greater inequalities of the earth's surface,"† I sustained the view that mountain ranges are formed wholly by a yielding of the crust of the earth along certain lines to horizontal pressure; not, however, a yielding by bending of the crust into a convex arch, filled and sustained by a liquid beneath, as has been supposed by some; but by a crushing or mashing together horizontally of the whole crust, with the formation of *close folds* and a thickening or swelling upward of the squeezed mass. I believe the structure of all mountain ranges, in which the stratification has not been obscured by metamorphism, would demonstrate this mode of formation.

I have long thought that the Coast Range in this vicinity is peculiarly adapted to exhibit in its structure the mode of its formation. It is destitute of granite axes, and it has been but little, in many places not at all, changed by metamorphism or overlaid by igneous ejections. A good section ought to clearly reveal its structure and its structure ought no less clearly to reveal the mode of its formation.

With this conviction, on the 5th of January last, in company

* The formulæ of Baeyer (in common with all those representing urea as carbamide) seem to fail in securing this advantage, although involving in part the same sort of view of the ureides as that above stated.

† This Journal, III, vol. iv, pp. 345 and 460.

with Mr. W. Jackson, a recent graduate of the university, and now a special student of mineralogy, I set off afoot, and walked very leisurely through the cut made by the Central Pacific Railroad from the plains adjoining the Bay of San Francisco through the mountains to the San Joaquin plains, a distance of about thirty miles, taking the angle and direction of the dip at every available point. The following diagram is a generalized section made from these observations, showing the structure of this range. The section is supposed to extend from southwest to northeast, i. e., at right angles to the direction of the chain, L. V. being Livermore Valley and J. P. San Joaquin Plains.

1.

The range where crossed by the railroad is divided into two sub-ranges separated by Livermore Valley. Both of these sub-ranges, it will be seen, are composed wholly of crumpled strata, those of the western sub-range or Contra Costa hills being crumpled in the most extraordinary manner.

The strata throughout the railroad cut are entirely unchanged and very distinct, and their dip may be taken with the greatest ease and certainty: but unfortunately they consist mostly of thin bedded shales and sandstones destitute of fossils, and so similar in appearance that identification of individual strata would be impossible without the most careful and detailed examination. Only in one place did I find any fossils, and these were easily identified as Miocene Tertiary. On account of the infinite number and the sameness of the strata I found it impossible to identify, and therefore I have not attempted in the diagram to trace the individual strata through the successive folds. But the general structure of the range is, I am sure, truly represented in the section.

A glance at the section shows that the southwestern sub-range or that next the bay is far the more complex. We have here at least five anticlinals with corresponding synclinals, all in a distance of about six miles in a straight line. The angles of dip vary from 40° to 90°, the average being about 65° to 70°. This would make the actual length of the folded strata two and a half to three times the horizontal distance through the mountain. Now it is not only impossible to conceive of the origin of such structure except by horizontal mashing, but the amount of horizontal mashing must have been enormous. Estimating in the usual way, i. e., taking the present length of the folded strata as the original length of the strata when horizontal, there must have been fifteen to eighteen miles of original sea-bottom crushed into six miles, with *corresponding* upswelling of the whole mass.* I say estimating in the usual way: for the *real* breadth of the original sea-bottom was probably considerably less, since as I shall show hereafter, the strata themselves are probably lengthened in the direction of the dip.

Nor is this particular section an exaggeration of the general structure of this range. On the contrary it is far less complex here than elsewhere. A glance at Whitney's map of Central California will show that the range is small and low at this part. This exceptional lowness is due primarily to the less horizontal mashing, and therefore less *upswelling*, and therefore *less complexity* of folding, and therefore less metamorphism, and therefore less hardness of the rocks, and therefore also *greater erosion* of this part. Whitney has nowhere attempted to give a general section of this very complex range, but in fig. 1 on p. 14 of vol. i, of the Geological Survey, he gives a section of a small portion of the Contra Costa hills farther north, which shows much more crushing than any portion of the range cut by the railroad.

The diagram section is supposed to be made at right angles to the general trend of the range, i. e., northeast and southwest. The folds are of course represented as striking in the direction of the range, and dipping in the direction of the section. This is very decidedly the average direction of the folds; but there is considerable variation to either side of this average direction. This shows that the horizontal or folding pressure came from several slightly different directions, perhaps consecutively. The same is clearly shown in the external features, also, of this very complex range; for the sub-ranges and ridges of which it is composed trend in many different directions.

But there is another minuter structure which I have observed in some of the strata, both of the Contra Costa and the Mt. Diablo sub-ranges, which demonstrates, in the completest manner, the mashing together horizontally and the extension vertically, even of the *constituent particles* of the stratified sediments.

* In my paper "On the great Lava flood of the northwest," this Journal, III, vol. vii, p. 167 and seq., I have stated, pp. 170 and 180, that under all circumstances, whether the surface be *uppushed* by horizontal mashing of sediments or *upbuilt* by the *outsqueezing* of melted matter, the increase of height would be the same, being measured by the amount of horizontal crushing. Prof. E. W. Hilgard, in his paper "On Mallet's theory of vulcanicity," Am. Jour., vol. vii, p. 535, note on p. 544, takes exception to this statement. He does not see "on what ground a simple *uplifting*, can be considered the precise mechanical equivalent of an *upbuilding* by eruption of liquid rock." I take this occasion to say that Prof. H. has entirely mistaken the point. It is not a question of *mechanical*, but of *geometrical* equivalence—not an equivalence of *force*, but an equivalence of *magnitude*.

I have already stated that the mountain mass lying between the Bay of San Francisco and the San Joaquin plains is divided, by the Amador and Livermore valleys, into two sub-ranges; the Contra Costa, overlooking the Bay, and the Mt. Diablo,* overlooking the plains. Both Cretaceous and Tertiary strata are found in the latter, although their distribution has not yet been thoroughly worked out; but the former consists wholly of Tertiary, principally Miocene. In both these sub-ranges seams of lignite of good quality have been found. Those found in the Cretaceous of Mt. Diablo have proved of great value and are extensively worked; but as yet nothing but very thin unprofitable seams have been found in the Contra Costa.

Several months ago I was asked to examine the *croppings* of some thin seams of lignite near the town of Hayward, which had been opened to a depth of 100 to 150 feet. The coal-bearing strata dip nearly perpendicularly and strike in the general direction of the range. The place examined was on the lowest foot hills of the Contra Costa, corresponding in position to *a* in the section, fig. 1.

While examining the mode of occurrence of this lignite, my attention was drawn, by the intelligent Superintendent of this mine, to certain slabs of shale in immediate contact with the seam, which were literally covered with small rounded flattened masses looking somewhat like flattened pebbles. In fact he supposed them to be pebbles or shingle which had fallen into fissures between the perpendicular strata. Examination, however, quickly convinced me that they were not pebbles nor extraneous matter of any kind, *but clay pellets or nodules in the original sediment which had been flattened by strong pressure in the formation of the mountain range.* Here then, I saw at once a means of determining the amount of mashing to which the sedimentary strata had been subjected in the process of mountain-making. I immediately commenced closer examination.

The nodules were all greatly flattened and nearly all greatly elongated. Their shape therefore were mostly flattened ellipsoids, though some were flattened discs. The flattened ellipsoids were nearly all set on end between the strata, i. e., with their long diameters vertical, though some varied considerably from this position to one side or the other, and a few were nearly horizontal. They were found in close contact with the seam on both sides, and some in the seam itself; and in such numbers that they covered the surface of the strata. When small and disc-shaped, or not much elongated, the surface of the over-clay blackened by contact with the coal presented a

* The term Mt. Diablo range is usually used in a wider sense for the whole range on the east side of the Bay, as distinguished from the Santa Cruz Mountains lying on the west side. I use the term here in a narrower sense to distinguish a subdivision of this range.

striking resemblance to impressions of the trunks of Lepidodendrids. In other cases when greatly elongated they looked like parallel flattened root-fibers. The material of the nodules was similar to that of the containing clay, unless perhaps a little finer.

A few months afterward, March, 1875, in company with a party of students and graduates of the University, I examined the coal mines of Mt. Diablo, and there also observed, in the roof of the seam, flattened nodules of sandstone often surrounded with a thin layer of coaly matter; but the sandstone was coarse and the nodules were imperfect. Subsequently Mr. Christy, an assistant in the chemical laboratory, who is now engaged in an examination of the coals of this coast, visited the same mines more extensively and brought me some very fine specimens of flattened elongated nodules. In these also I am assured the long diameters were in the direction of the dip.

Now, there cannot be the slightest doubt that these nodules were once *clay pellets*, of all sizes, from that of swan shot to that of hazel nuts, which existed in, and on the surface of, the original clay sediments, having been taken up from finer deposits rolled along by gentle currents and deposited along with coarser material, precisely as we find at the present day; and further, that their present shape is due wholly to subsequent pressure, precisely as in the case of the greenish elliptical spots found in cleaved slates, and described by Prof. Tyndall:* and, therefore, finally, that by means of their shape and position, as in the case of the greenish spots, it is quite possible to determine the amount of mashing together in one direction and the extension or upswelling in another, which the sedimentary mass has suffered since its deposition.

I take the case of the Hayward seam as the simplest because the strata are vertical. Taking three *equal* rectangular diameters of the original unmashed pellets, one in the direction of pressure, i. e., horizontal and at right angles to the strata; another also horizontal but in the direction of the strike, and the third in the direction of the dip or vertical, it is evident that the first would be *shortened*, the third would be *elongated*, while the second would, on the average, be *unaffected*, since extension of this diameter in some places must be compensated by shortening in contiguous places right or left. We may assume, therefore, that the elongation vertically is strictly correlated with the mashing or shortening horizontally, and the one is a measure of the other. Now I found by careful measurement of a great number of these nodules that the shortest diameter bears to the longest the ratios of 1 : 3, 1 : 4, 1 : 6, 1 : 9, and even 1 : 12 and 1 : 14. I believe a fair average would be about 1 : 6 or

* Phil. Mag., xii, 35, 1856.

1 : 9. Now as this ratio is the result of *both* compression in one direction and extension in another, it follows that either the compression or the extension would be expressed by the square roots of these ratios. Therefore there has been a crushing together of every 2·5 to 3 parts into 1 and a corresponding extension in another direction of every 1 part into 2·5 to 3. But since the short diameters were horizontal and the long diameters vertical, it is evident that throughout the whole squeezed mass every 2½ to 3 feet were crushed together horizontally into one foot, and every foot of vertical thickness was increased or swelled up to two and a half or three feet. This seems to have taken place principally after, by folding, the strata had taken a vertical position. Therefore by the pressure *the strata were thinned and extended vertically.* No allowance has hitherto been made for this change in the estimates of the original thickness of folded strata.

There are several thoughts suggested by the above which I think worthy of mention.

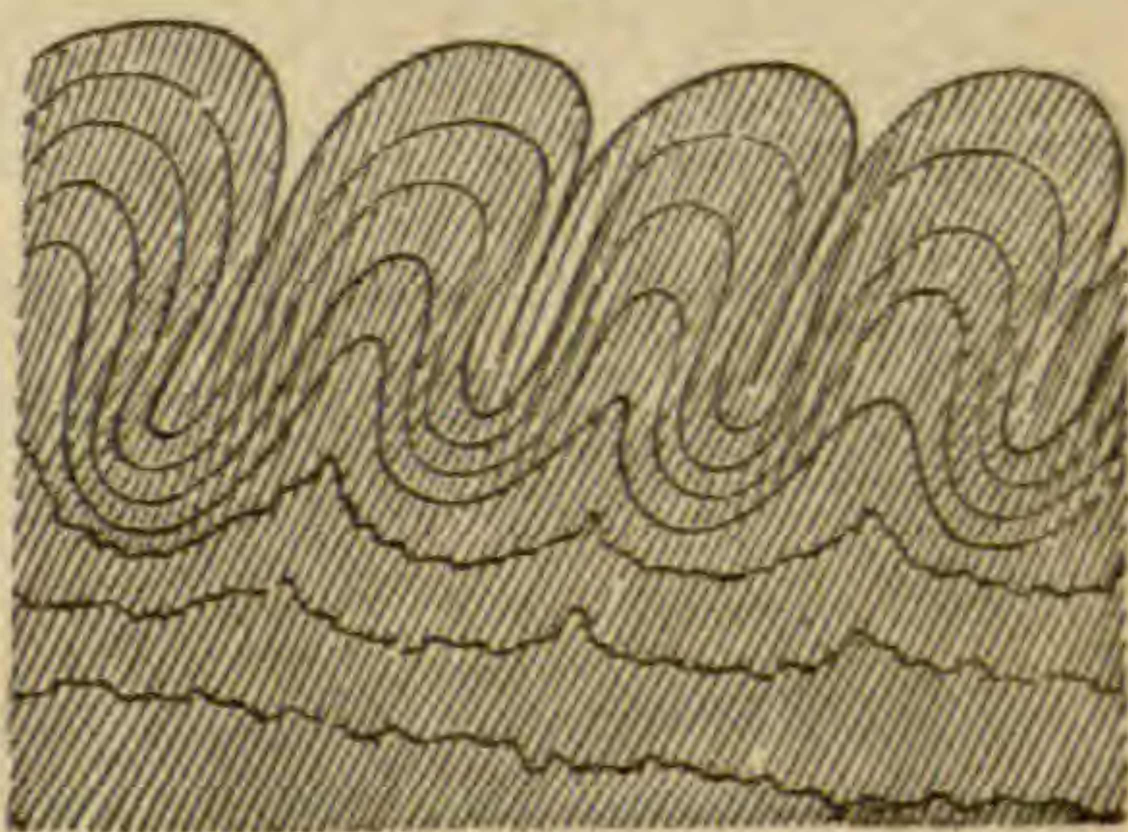
1. The position of the nodules, sometimes on the surface of the coal seam, sometimes half buried, and sometimes wholly buried in the coaly matter, clearly proves that at the time when the nodules were first rolled along and deposited there, *the coaly matter was in the condition of very soft semi-liquid peat.*

2. It is well known that slaty cleavage is produced by powerful pressure compressing the once plastic mass in one direction and extending it in another. The absence of slaty cleavage, under precisely these conditions, is evidently due, in the case under discussion, to the fact that the materials are unsuitable for the development of that structure, being *far too coarse.* If cleavage had been produced, however, *the planes of cleavage would have been parallel to the planes of stratification;* and, therefore, the structure would have been almost undistinguishable from, and liable to be mistaken for, a fine *lamination structure.*

Now, in many cases this parallelism actually occurs. On the foothills of the Sierra, especially about Snelling, Hornitos and Mariposa, are found fine clay slates beautifully fissile with their planes apparently perpendicular, but in reality dipping at a very high angle to the northeast, i. e., under the range. These are evidently true cleaved slates, and the very thin planes into which they easily split are true cleavage-planes and not lamination-planes. Yet I looked in vain for any stripe or other indication of stratification in any other direction. Also Whitney has shown (Geol. Surv., vol. i, p. 226), and I have myself observed, that these slates pass by insensible gradations into, and are even interstratified with, coarser materials, showing distinct stratification in the same direction, i. e., underdipping the range at high angle. There can be no doubt, therefore, that *all* the strata of this foot-hill region, including the slates, underdip the range at high angle. Evidently, therefore, the cleavage planes of these slates are parallel to the stratification planes instead of cutting through them at high angle as is most common.

The diverse relation of the cleavage to the stratification planes I explain as follows: In a thick mass of very fine sediments mashed together horizontally it is evident that the surface and upper portions would first be thrown into one or more close folds by which the strata are brought into a nearly perpendicular position, and then these would be thinned and extended vertically by the pressure as already shown in the previous portion of this paper: but the *deeper portions* would be less and less folded, until, very deep, the folding would cease altogether and the mashing would be by *thickening* only and not by folding. I have rudely represented these facts in the diagram, fig. 2, in which the parallel, nearly vertical lines, represent the cleavage. In such a mass of horizontally squeezed fine sediments, therefore, the cleavage of the upper parts would be *parallel* with the strata while that of the lower parts would be *perpendicular* or nearly so to the strata. If, therefore, the upper parts only should be exposed by denudation we would have an example of cleavage parallel to the strata, and we might be in doubt whether to call the planes cleavage-planes or fine lamination-planes; but if greater denudation should expose the deeper portions we would have an example of cleavage-planes cutting through the lamination-planes at a high angle and therefore very distinct from them.

2.



3. It is evident from the above that in many cases the thickness of the strata as we now find them may be very different from that of the original sediments. In estimating the latter, therefore, we must make due allowance for the great thinning in some cases and thickening in others produced by pressure.

4. In my paper on the formation of the great features of the earth surface, already referred to, I have attributed mountain elevation to horizontal crushing. Prof. Dana,* however, thinks that, although the idea of plication is evidently included in my view, yet it ought to have a larger place than my words seem to give it—for the amount of elevation by plication is many times ("ten-fold") greater than by simple crushing.

* Am. Journ., III, v, 428.

Perhaps I ought to have been more explicit in my statement, but it seemed to me unnecessary, because on the assumption of a solid earth the amount of elevation would be the same, or nearly the same, whether, by the horizontal pressure, the strata be thrown into *closed folds* (as is most common in mountain chains) or only thickened without folding. If every two or three parts in horizontal extent of sediments be crushed into one part, there must be a corresponding thickening of the whole squeezed mass, and, therefore, a corresponding elevation of the surface, whether the strata be closely folded or only thickened without folding. In reality, doubtless, both occur in every case; close folding in the upper parts and thickening without folding in the deeper parts of the same squeezed mass. In fact it is impossible that the folding should occur above without a corresponding crushing and thickening below.

Again, I am satisfied that Prof. Dana greatly under-estimates the amount of elevation by simple mashing as compared with folding: 1st, because folding is a superficial phenomenon and therefore always exposed to view, while crushing without folding is deep seated and only rarely exposed; 2d, because folding is *always* revealed by stratification, while crushing is only *sometimes* revealed by cleavage, for this structure is only developed in *suitable* materials; and 3d, because even after folding, extension upward may take place by mashing together of the folds, as I have shown in the early part of this paper.

I have spoken thus far of *closed* folds. In *open* folds such as occur on the skirts of mountain chains where the horizontal crushing has not been sufficient to bring the folds together, the case might seem to be different; but even in these there must be a mashing of the strata below each anticlinal and proportioned to its height, unless we assume a hollow arch beneath, or else such an arch supported by a liquid, an assumption which is expressly set aside in my paper.

Berkeley, Oct. 11, 1875.

ART. XXXVIII.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXVII.—*Description of Mancasellus brachyurus, a new fresh water Isopod;* by O. HARGER.

THE genus *Asellopsis* * was proposed by the writer for the reception of *Asellus tenax* Smith, on account of the absence of mandibular palpi. A second species of this interesting genus has lately been collected by Mr. Fred. Mather, in Rockbridge

* This Journal, III, vol. vii, p. 601, 1874.

Co., Virginia Since the name *Asellopsis* proves to have been preoccupied I propose in its place *Mancasellus*,* retaining *M. tenax* as the typical species, while the new species may be called *M. brachyurus*, from the short caudal stylets. This species resembles *M. tenax*, described and figured in the Report of the United States Commissioner of Fish and Fisheries, Part II, Report for 1872–3, p. 659, plate I, fig. 3, differing principally from it in the following points: The lateral margins of the head are entire; the proximal segment of the caudal stylets is short, being but little longer than the third segment of the antennæ; the rami are also short, the inner being much stronger and somewhat longer than the outer; in the males the propodus of the first pair of legs is armed with a prominent acute tooth on the palmar margin near the base, and, in the appendages of the seventh segment, the terminal portion of the outer pair is smaller and less expanded externally than in *M. tenax*, and the distal segment of the internal ramus of the inner pair is but little swollen at the base, and approaches the form seen in *Asellus communis* Say. The largest specimen measures 16mm. in length exclusive of the antennæ and caudal stylets. The locality is worthy of mention as being on the Atlantic side of the Appalachian water-shed while *M. tenax* is yet known only from the Lakes.

Art. XXXIX.—*Professor Tyndall on Germs.*†

The author refers, in an introduction, to an inqury on the decomposition of vapors and the formation of actinic clouds by light, whereby he was led to experiment on the floating matter of the air. He refers to the experiments of Schwan, Schröder and Dusch, Schröder himself, to those of the illustrious French chemist Pasteur, to the reasoning of Lister and its experimental verification, regarding the filtering power of the lungs; from all of which he concluded, six years ago, that the power of developing life by the air, and its power of scattering light, would be found to go hand in hand. He thought the simple expedient of examining by means of a beam of light, while the eye was kept sensitive by darkness, the character of the medium in which their experiments were conducted, could not fail to be useful to workers in this field. But the method has not been much turned to account, and this year he thought it worth while to devote some time to the more complete demonstration of its utility.

* From *mancus*, maimed. and *Asellus*.

† On the Optical Deportment of the Atmosphere in reference to the Phenomena of Putrefaction and Infection. Abstract of a paper read before the Royal Society, January 13th, by Professor Tyndall, F.R.S. From Nature of Jan. 27 and Feb. 3.

He also wished to free his mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of "spontaneous generation." Pasteur has pronounced it "a chimera," and expressed the undoubting conviction that, this being so, it is possible to remove parasitic diseases from the earth. To the medical profession, therefore, and through them to humanity at large, this question is one of the last importance. But the state of medical opinion regarding it is not satisfactory. In a recent number of the *British Medical Journal*, and in answer to the question, "in what way is contagium generated and communicated?" Messrs. Braidwood and Vacher reply that, notwithstanding "an almost incalculable amount of patient labor, the actual results obtained, especially as regards the manner of generation of contagium, have been most disappointing. Observers are even yet at variance whether these minute particles, whose discovery we have just noticed, and other disease germs, are always produced from like bodies previously existing, or whether they do not, under certain favorable conditions, spring into existence *de novo.*"

With a view to the possible diminution of the uncertainty thus described, the author submits, without further preface to the Royal Society, and especially to those who study the etiology of disease, a description of the mode of procedure followed in this inquiry, and the results to which it has led.

A number of chambers, or cases, were constructed, each with a glass front, its top, bottom, back and sides being of wood. At the back is a little door which opens and closes on hinges, while into the sides are inserted two panes of glass, facing each other. The top is perforated in the middle by a hole two inches in diameter, closed air-tight by a sheet of india-rubber. This sheet is pierced in the middle by a pin, and through the pin-hole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar two inches in diameter and one inch and a half high, surrounds the pipette, the space between both being packed with cotton-wool moistened by glycerine. Thus the pipette, in moving up and down, is not only firmly clasped by the india-rubber, but it also passes through a stuffing box of sticky cotton-wool. The width of the aperture closed by the india-rubber secures the free lateral play of the lower end of the pipette. Into two other smaller apertures in the top of the case are inserted, air-tight, the open ends of two narrow tubes, intended to connect the interior space with the atmosphere. The tubes are bent several times up and down, so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air.

The bottom of the box is pierced with two rows, sometimes with a single row of apertures, in which are fixed, air-tight, large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

On Sept. 10 the first case of this kind was closed. The passage of a concentrated beam across it through its two side windows then showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days of quiet sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air. During ebullition aqueous vapor rose from the liquid into the chamber, where it was for the most part condensed, the uncondensed portion escaping, at a low temperature through the bent tubes at the top. Before the brine was removed little stoppers of cotton-wool were inserted in the bent tubes, lest the entrance of the air into the cooling chamber should at first be forcible enough to carry motes along with it. As soon, however, as the ambient temperature was assumed by the air within the case, the cotton-wool stoppers were removed.

We have here the oxygen, nitrogen, carbonic acid, ammonia, aqueous vapor, and all the other gaseous matters which mingle more or less with the air of a great city. We have them, moreover, "untortured" by calcination and unchanged even by filtration or manipulation of any kind. The question now before us is, can air thus retaining all its gaseous mixtures, but self-cleansed from mechanically suspended matter, produce putrefaction? To this question both the animal and vegetable worlds return a decided negative.

Among vegetables, experiments have been made with hay, turnips, tea, coffee, hops, repeated in various ways with both acid and alkaline infusions. Among animal substances are to be mentioned many experiments with urine; while beef, mutton, hare, rabbit, kidney, liver, fowl, pheasant, grouse, haddock, sole, salmon, cod, turbot, mullet, herring, whiting, eel, oyster have been all subjected to experiment.

The result is that infusions of these substances exposed to the common air of the Royal Institution laboratory, maintained at a temperature of from 60° to 70° Fahr., all fell into putrefaction in the course of from two to four days. No matter where the infusions were placed, they were infallibly smitten. The number of the tubes containing the infusions was multiplied till it reached six hundred, but not one of them escaped infection.

On the other hand, in no single instance did the air, which had been proved moteless by the searching beam, show itself to possess the least power of producing Bacterial life or the associated phenomena of putrefaction. The power of developing such life in atmospheric air, and the power of scattering light, are thus proved to be indissolubly united.

The sole condition necessary to cause these long-dormant infusions to swarm with active life is the access of the floating matter of the air. After having remained for four months as pellucid as distilled water, the opening of the back-door of the protecting case, and the consequent admission of the mote-laden air, sufficed in three days to render the infusions putrid and full of life.

That such life arises from mechanically suspended particles is thus reduced to ocular demonstration. Let us inquire a little more closely into the character of the particles which produce the life. Pour Eau-de-Cologne into water, a white precipitate renders the liquid milky. Or, imitating Brücke, dissolve clean gum mastic in alcohol, and drop it into water, the mastic is precipitated and milkiness produced. If the solution be very strong the mastic separates in curds; but by gradually diluting the alcoholic solution we finally reach a point where the milkiness disappears, the liquid assuming, by reflected light, a bright cerulean hue. It is, in point of fact, the color of the sky, and is due to a similar cause, namely, the scattering of light by particles, small in comparison to the size of the waves of light.

When this liquid is examined by the highest microscopic power it seems as uniform as distilled water. The mastic particles, though innumerable, entirely elude the microscope. At right angles to a luminous beam passing among the particles they discharge perfectly polarised light. The optical deportment of the floating matter of the air proves it to be composed, in part, of particles of this excessively minute character. When the track of a parallel beam in dusty air is looked at horizontally through a Nicol's prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a considerable portion of the light from the finer matter is extinguished. The coarser motes, on the other hand, flash out with greater force, because of the increased darkness of the space around them. It is among the finest ultra-microscopic particles that the author shows the matter potential as regards the development of Bacterial life is to be sought.

But though they are beyond the reach of the microscope, the existence of these particles, foreign to the atmosphere but floating in it, is as certain as if they could be felt between the fingers, or seen by the naked eye. Supposing them to augment in magnitude until they come, not only within range of the microscope, but within range of the unaided senses. Let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand filled with nutritious earth, with which we mix our unknown particles; and that in forty-eight hours subsequently buds and blades of well-defined cresses and grasses appear above the soil. Suppose the experiment when repeated over and over again to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the products of dead dust or mineral particles; or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly consider the experiment with the flower-pot as clearing up our pre-existing ignorance; we should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been "spontaneously generated."

This reasoning applies word for word to the development of *Bacteria* from that floating matter which the electric beam reveals in the air, and in the absence of which no Bacterial life has been generated. There seems no flaw in this reasoning; and it is so simple as to render it unlikely that the notion of Bacterial life developed from dead dust can ever gain currency among the members of a great scientific profession.

A novel mode of experiment has been here pursued, and it may be urged that the conditions laid down by other investigators in this field, which have led to different results, have not been strictly attended to. To secure accuracy in relation to these alleged results, the latest words of a writer on this question, who has influenced medical thought both in this country and in America, are quoted. "We know," he says, "that boiled turnip or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and vibriones in the course of from two to six days." Who the "we" are who possess this knowledge is not stated. The author is certainly not among the number, though he has sought anxiously for knowledge of the kind. He thus tests the statements in succession.

And first, with regard to the filtered air. A group of twelve large test-tubes were caused to pass air-tight through a slab of wood. The wood was coated with cement, in which, while hot, a heated "propagating glass" resembling a large bell jar was imbedded. The air within the jar was pumped out several times, air filtered through a plug of cotton-wool being permitted to supply its place. The test-tubes contained infusions of hay, turnip, beef, and mutton—three of each—twelve in all. They are as clear and cloudless at the present moment as they were upon the day of their introduction; while twelve similar tubes, prepared at the same time in precisely the same way and exposed to the ordinary air, are clogged with mycelium, mould, and *Bacteria.*

With regard to the calcined air, a similar propagating glass was caused to cover twelve other tubes filled with the same infusions. The "glass" was exhausted and carefully filled with air which had passed through a red-hot platinum tube, containing a roll of red-hot platinum gauze. Tested by the searching beam, the calcined air was found quite free from floating matter. Not a speck has invaded the limpidity of the infusions exposed to it, while twelve similar tubes placed outside have fallen into rottenness.

The experiments with calcined air took another form. Six years ago it was found that to render the laboratory air free from floating matter, it was only necessary to permit a platinum wire heated to whiteness to act upon it for a sufficient time. Shades, containing pear juice, damson juice, hay and turnip-juice, and water of yeast, were freed from their floating matter in this way. The infusions were subsequently boiled and permitted to remain in contact with the calcined air. They are quite unchanged to the present hour, while the same infusions exposed to common air became mouldy and rotten along ago.

It has been affirmed that turnip and hay-infusions rendered slightly alkaline are particularly prone to exhibit the phenomena of spontaneous generation. This was not found to be the case in the present investigation. Many such infusions have been prepared, and they have continued for months without sensible alteration.

Finally, with regard to infusions wholly withdrawn from air, a group of test-tubes, containing different infusions, was boiled under a bell-jar filled with filtered air, and from which the air was subsequently removed as far as possible by a good air-pump. They are now as pellucid as they were at the time of their preparation, more than two months ago, while a group of corresponding tubes exposed to the laboratory air have all fallen into rottenness.

There is still another form of experiment on which great weight has been laid—that of hermetically sealed tubes. On April 6 last, a discussion on the "Germ Theory of Disease" was opened before the Pathological Society of London. The meeting was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The following important summary of these was then given:— "With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blowpipe-flame, we may boil the fluid, seal the vessel during ebullition, and keeping it in a warm place, may await the result, as I have often done. After a variable time the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with *Bacteria* and allied organisms."

Previous to reading this statement the author had operated upon tubes of hay and turnip-infusions, and upon twenty-one tubes of beef, mackerel, eel, oyster, oat-meal, malt, and potato, hermeti cally sealed while boiling, not by the blowpipe, but by the far more handy spirit-lamp flame. In no case was any appearance whatever of *Bacteria* or allied organisms observed. The perusal of the discussion just referred to caused the author to turn again to muscle, liver, and kidney, with a view of varying and multiplying the evidence. Fowl, pheasant, snipe, partridge, plover, wild duck, beef, mutton, heart, tongue, lungs, brains, sweetbread, tripe, the crystalline lens and vitreous humor of an ox, herring, haddock, mullet, codfish, sole, were all embraced in the experiments. There was neither mistake nor ambiguity about the result. One hundred and thirty-nine of the flasks operated on were exhibited, and not one of this cloud of witnesses offers the least countenance to the assertion that liquids within flasks, boiled and hermetically sealed, swarm, subsequently, more or less plentifully with *Bacteria* and allied organisms.

The evidence furnished by this mass of experiments, that errors either of preparation or observation have been committed, is, it is submitted, very strong. But to err is human; and in an inquiry so difficult and fraught with such momentous issues, it is not error, but the persistence in error by any of us, for dialectic ends, that is to be deprecated. The auther shows by illustrations the risks of error run by himself. On Oct. 21 he opened the back-door of a case containing six test-tubes filled with an infusion of turnip which had remained perfectly clear for three weeks, while three days sufficed to crowd six similar tubes exposed to mote-laden air with *Bacteria*. With a small pipette he took specimens from the pellucid tubes, and placed them under the microscope. One of them yielded a field of Bacterial life, monstrous in its copiousness. For a long time he tried vainly to detect any source of error, and was perfectly prepared to abandon the unvarying inference from all the other experiments, and accept the result as a clear exception to what had previously appeared to be a general law. The cause of his perplexity was finally traced to the tiniest speck of an infusion containing *Bacteria*, which had clung by capillary attraction to the point of one of his pipettes.

Again, three tubes containing infusions of turnip, hay, and mutton, were boiled on Nov. 2 under a bell-jar containing air so carefully filtered that the most searching examination by a concentrated beam failed to reveal a particle of floating matter. At the present time every one of the tubes is thick with mycelium and covered with mould. Here surely we have a case of spontaneous generation. Let us look to its history.

After the air has been expelled from a boiling liquid it is difficult to continue the ebullition without "bumping." The liquid remains still for intervals, and then rises with sudden energy. It did so in the case now under consideration, and one of the tubes boiled over, the liquid over-spreading the resinous surface in which the bell-jar was imbedded, and on which, doubtless, germs had fallen. For three weeks the infusions had remained perfectly clear. At the end of this time, with a view of renewing the air of the jar, it was exhausted, and refilled by fresh air which had passed through a plug of cotton-wool. As the air entered, attention was attracted by two small spots of penicillium resting on the liquid which had boiled over. It was at once remarked that the experiment was a dangerous one, as the entering air would probably detach some of the spores of the penicillium and diffuse them in the bell-jar. This was, therefore, filled very slowly, so as to render the disturbance a minimum. Next day, however, a tuft of mycelium was observed at the bottom of one of the three tubes, namely that containing the hay-infusion. It has by this time grown so as to fill a large portion of the tube. For nearly a month longer the two tubes containing the turnip and mutton-infusions maintained their transparency unimpaired. Late in December the mutton-infusion, which was in dangerous proximity to the outer mould, showed a tuft upon its surface. The beef-infusion continued bright and clear for nearly a fortnight longer. The recent cold weather caused me to add a third gas-stove to the two which had previously warmed the room in which the experiments are conducted. The warmth of this stove played upon one side of the bell-jar: and on the day after the lighting of the stove, the beef-infusion gave birth to a tuft of mycelium. In this case the small spots of penicillium might have readily escaped attention; and had they done so we should have had three cases of "spontaneous generation" far more striking than many that have been adduced.

In further illustration of the dangers incurred in this field of inquiry the author refers to the excellent paper of Dr. Roberts on Biogenesis, in the "Philosophical Transactions" for 1874. Dr. Roberts fills the bulb of an ordinary pipette to about two-thirds of its capacity with the infusion to be examined. In the neck of the pipette he places a plug of dry cotton-wool. He then hermetically seals the neck and dips the bulb into boiling water or hot oil, where he permits it to remain for the requisite time. Here we have no disturbance from ebullition, and no loss by evaporation. The bulb is removed from the hot water and permitted to cool. The sealed end of the neck is then filed off, the cotton-wool alone interposing between the infusion and the atmosphere.

The arrangement is beautiful, but it has one weak point. Cotton-wool free from germs is not to be found, and the plug employed by Dr. Roberts infallibly contained them. In the gentle movement of the air to and fro as the temperature changed, or by any shock, jar, or motion to which the pipette might be subjected, we have certainly a cause sufficient to detach a germ now and then from the cotton-wool which would fall into the infusion and produce its effect. Probably, also, condensation occurred at times in the neck of the pipette; the water of condensation carrying back from the cotton-wool the seeds of life. The fact of fertilization being so rare as Dr. Roberts found it to be is a proof of the care with which his experiments were conducted. But he did find cases of fertillization after prolonged exposure to the boiling temperature; and this caused him to come to the conclusion that under certain rare conditions spontaneous generation may occur. He also found that an alkalised hay-infusion was so difficult to sterilise that it was capable of withstanding the boiling temperasure for hours without losing its power of generating life. The most careful experiments have been made with this infusion. Dr. Roberts is certainly correct in assigning to it superior nutritive power. But in the present inquiry five minutes boiling sufficed to completely sterilise the infusion.

Summing up this portion of his inquiry, the author remarks that he will hardly be charged with any desire to limit the power and potency of matter. But holding the notions he does upon this point, it is all the more incumbent on him to affirm that as far as inquiry has hitherto penetrated, life has never been proved to appear independently of antecedent life.

Though the author had no reason to doubt the general diffusion of germs in the atmosphere, he thought it desirable to place the point beyond question. At Down, Mr. Darwin, Mr. Francis Darwin; at High Elms, Sir John Lubbock; at Sherwood, near Tunbridge Wells, Mr. Siemens; at Pembroke Lodge, Richmond Park, Mr. Rollo Russell; at Heathfield Park, Messrs. Hamilton; at Greenwich Hospital, Mr. Hirst; at Kew, Dr. Hooker; and at the Crystal Palace, Mr. Price, kindly took charge of infusions, every one of which became charged with organisms. To obtain more definite insight regarding the diffusion of atmospheric germs, a square wooden tray was pierced with 100 holes, into each of which was dropped a short test-tube. On Oct. 23, thirty of these tubes were filled with an infusion of hay, thirty-five with an infusion of turnip, and thirty-five with an infusion of beef. The tubes, with their infusions, had been previously boiled, ten at a time, in an oil-bath. One hundred circles were marked on paper so as to form a map of the tray, and every day the state of each tube was registered upon the corresponding circle. In the following description the term "cloudy" is used to denote the first stage of turbidity; distinct but not strong. The term "muddy" is used to denote thick turbidity.

One tube of the 100 was first singled out and rendered muddy. It belonged to the beef group, and it was a whole day in advance of all the other tubes. The progress of putrefaction was first registered on Oct. 26; the "map" then taken may be thus described:

Hay.—Of the thirty specimens exposed one had become "muddy"—the seventh in the middle row reckoning from the side of the tray nearest the stove. Six tubes remained perfectly clear between this muddy one and the stove, proving that differences of warmth may be overridden by other causes. Every one of the other tubes containing the hay infusion showed spots of mould upon the clear liquid.

Turnip.—Four of the thirty-five tubes were very muddy, two of them being in the row next the stove, one four rows distant, and the remaining one seven rows away. Besides these six tubes had become clouded. There was no mould on any of the tubes.

Beef.—One tube of the thirty-five was quite muddy, in the seventh row from the stove. There were three cloudy tubes, while seven of them bore spots of mould.

As a general rule organic infusions exposed to the air during the autumn remained for two days or more perfectly clear. Doubtless from the first germs fell into them, but they required time to be hatched. This period of clearness may be called the "period of latency," and indeed it exactly corresponds with what is understood by this term in medicine. Toward the end of the period of latency, the fall into a state of disease is comparatively sudden; the infusion passing from perfect clearness to cloudiness more or less dense in a few hours.

Thus the tube placed in Mr. Darwin's possession was clear at 8.30 A. M. on Oct. 19, and cloudy at 4.30 P. M. Seven hours, moreover, after the first record of our tray of tubes, a marked change had occurred. It may be thus described:—Instead of one, eight of the tubes containing hay-infusion had fallen into uniform muddiness. Twenty of these had produced Bacterial slime, which had fallen to the bottom, every tube containing the slime being covered by mould. Three tubes only remained clear, but with mould upon their surfaces. The muddy turnip-tubes had increased from four to ten; seven tubes were clouded, while eighteen of them remained clear, with here and there a speck of mould on the surface. Of the beef, six were cloudy and one thickly muddy, while spots of mould had formed on the majority of the remaining tubes. Fifteen hours subsequent to this observation, viz. on the morning of Oct. 27, all the tubes containing hay-infusion were smitten, though in different degrees, some of them being much more turbid than others. Of the turnip-tubes, three only remained unsmitten, and two of these had mould upon their surfaces. Only one of the thirty-five beef-infusions remained intact. A change of occupancy, moreover, had occurred in the tube which first gave way. Its muddiness remained gray for a day and a half, then it changed to bright yellow green, and it maintained this color to the end. On the 27th every tube of the hundred was smitten, the majority with uniform turbidity; some, however, with mould above and slime below, the intermediate liquid being tolerably clear. The whole process bore striking resemblance to the propagation of a plague among a population, the attacks being successive and of different degrees of virulence.

From the irregular manner in which the tubes are attacked, we may infer that, as regards *quantity*, the distribution of the germs in the air is not uniform. The singling out, moreover, of one tube of the hundred by the particular *Bacteria* that develop a green pigment, shows that, as regards *quality*, the distribution is not uniform. The same absence of uniformity was manifested in the struggle for existence between the *Bacteria* and the penicillium. In some tubes the former were triumphant; in other tubes of the same infusion the latter was triumphant. It would seem also as if a want of uniformity as regards *vital vigor* prevailed. With the self-same infusion the motions of the *Bacteria* in some tubes were exceedingly languid, while in other tubes the motions resembled a rain of projectiles, being so rapid and violent as to be followed with difficulty by the eye. Reflecting on the whole of this, the author concludes that the germs float through the atmosphere in groups or clouds, with spaces more sparsely filled between them. The touching of a nutritive fluid by a Bacterial cloud would naturally have a different effect from the touching of it by the interspace between two clouds. But as in the case of a mottled sky, the various portions of the landscape are successively visited by shade, so, in the long run, are the various tubes of our tray touched by the Bacterial clouds, the final fertilization or infection of them all being the consequence. The author connects these results with the experiments of Pasteur on the non-continuity of the cause of so-called spontaneous generation, and with other experiments of his own.*

On the 9th of November a second tray containing one hundred tubes filled with an infusion of mutton was exposed to the air. On the morning of the 11th six of the ten nearest the stove had given way to putrefaction. Three of the rows most distant from the stove had yielded, while here and there over the tray particular tubes were singled out and smitten by the infection. Of the whole tray of one hundred tubes, twenty-seven were either muddy or cloudy on the 11th. Thus, doubtless, in a contagious atmosphere, are individuals successively struck down. On the 12th all the tubes had given way, but the differences in their contents were extraordinary. All of them contained *Bacteria*, some few, others in swarms. In some tubes they were slow and sickly in their motions, in some apparently dead, while in others they darted about with rampant vigor. These differences are to be referred to changes in the germinal matter, for the same infusion was presented everywhere to the air. Here also we have a picture of what occurs during an epidemic, the difference in number and energy of the Bacterial swarms resembling the varying intensity of the disease. It becomes obvious from these experiments that of two individuals of the same population, exposed to a contagious atmosphere, the one may be severely, the other lightly attacked, though the two individuals may be as identical as regards susceptibility as two samples of one and the same mutton infusion.

The author traces still further the parallelism of these actions with the progress of infectious disease. The *Times* of January 17th contained a letter on Typhoid Fever signed "M.D.," in which occurs the following remarkable statement:—"In one part of it (Edinburgh), congregated together and inhabited by the lowest of the population, there are, according to the Corporation return for 1874, no less than 14,319 houses or dwellings—many under one roof, on the 'flat' system—in which there are no house connections whatever with the street sewers, and, consequently, no water-closets. To this day, therefore, all the excrementitious and other refuse of the inhabitants is collected

* In hospital practice the opening of a wound during the passage of a Bacterial cloud would have an effect very different from the opening of it in the interspace between two clouds. Certain caprices in the behavior of dressed wounds may possibly be accounted for in this way. Under the heading "Nothing new under the Sun," Prof. Huxley has just sent me the following remarkable extract:—"Uebrigens kann man sich die in der Atmosphäre schwimmenden Thierchen wie Wolken denken, mit denen ganz leere Luftmassen, ja ganze Tage völlig reinen Luftverhältnisse wechseln." (Ehrenberg, "Infusions Thierchen," 1838, p. 525.) The coincidence of phraseology is surprising, for I knew nothing of Ehrenberg's conception. My "clouds," however, are but small miniatures of his.

in pails or pans, and remains in their midst, generally in a partitioned-off corner of the living room, until the next day, when it is taken down to the streets and emptied into the Corporation carts. Drunken and vicious though the population be, herded together like sheep, and with the filth collected and kept for twenty-four hours in their very midst, it is a remarkable fact that typhoid fever and diphtheria are simply unknown in these wretched hovels."

This case has its analogue in the following experiment, which is representative of a class. On Nov. 30 a quantity of animal refuse, embracing beef, fish, rabbit, hare, was placed in two large test-tubes opening into a protecting chamber containing six tubes. On Dec. 13, when the refuse was in a state of noisome putrefaction, infusions of whiting, turnip, beef and mutton were placed in the other four tubes. They were boiled and abandoned to the action of the foul "sewer gas" emitted by their two putrid companions. On Christmas-day the four infusions were limpid. The end of the pipette was then dipped into one of the putrid tubes, and a quantity of matter comparable in smallness to the pock-lymph held on the point of a lancet was transferred to the turnip. Its clearness was not sensibly affected at the time; but on the 26th it was turbid throughout. On the 27th a speck from the infected turnip was transferred to the whiting; on the 28th disease had taken entire possession of the whiting. To the present hour the beef and mutton tubes remain as limpid as distilled water. Just as in the case of the living men and women in Edinburgh, no amount of fetid gas had the power of propagating the plague, so long as the organisms which constitute the true contagium did not gain access to the infusions.

The universal prevalence of the germinal matter of *Bacteria* in water has been demonstrated with the utmost evidence by the experiments by Dr. Burdon Sanderson. But the germs in water are in a very different condition, as regards readiness for development, from those in air. In water they are thoroughly wetted, and ready, under the proper conditions, to pass rapidly into the finished organisms. In air they are more or less desiccated, and require a period of preparation more or less long to bring them up to the starting-point of the water-germs. The rapidity of development in an infusion infected by either a speck of liquid containing *Bacteria* or a drop of water is extraordinary. On Jan. 4 a thread of glass almost as fine as a hair was dipped into a cloudy turnip infusion, and the tip only of the glass fiber was introduced into a large test-tube containing an infusion of red mullet. Twelve hours subsequently the perfectly pellucid liquid was cloudy throughout. A second test-tube containing the same infusion was infected with a single drop of the distilled water furnished by Messrs. Hopkin and Williams; twelve hours also sufficed to cloud the infusion thus treated. Precisely the same experiments were made with herring with the same result. At this season of the year several days' exposure to the air are to produce so great an effect. On Dec. 31 a strong turnip-infusion was prepared by digesting thin slices in distilled water at a temperature of 120° F. The infusion was divided between four large test-tubes, in one of which it was left unboiled, in another boiled for five minutes, in the two remaining ones boiled, and after cooling infected with one drop of beef-infusion containing *Bacteria*. In twenty-four hours the unboiled tube and the two infected ones were cloudy, the unboiled tube being the most turbid of the three. The infusion here was peculiarly limpid after digestion; for turnip it was quite exceptional, and no amount of searching with the microscope could reveal in it at first the trace of a living Bacterium; still germs were there which, suitably nourished, passed in a single day into Bacterial swarms without number. Five days have not sufficed to produce an effect approximately equal to this in the boiled tube, which was uninfected but exposed to the common laboratory air.

There cannot, moreover, be a doubt that the germs in the air differ widely among themselves as regards *preparedness* for development. Some are fresh, others old; some are dry, others moist. Infected by such germs the same infusion would require different lengths of time to develop Bacterial life. This remark applies to and explains the different degrees of rapidity with which epidemic disease acts upon different people. In some the hatching-period, if it may be called such, is long, in some short, the differences depending upon the different degrees of preparedness of the contagium.

The author refers with particular satisfaction to the untiring patience, the admirable mechanical skill, the veracity in thought, word, and deed, displayed throughout this first section of a large and complicated inquiry by his assistant, Mr. John Cottrell, who was zealously aided by his junior colleague, Mr. Frank Valter.

NOTE. *Jan.* 31.—The notion that the author limited himself to temperatures of 60° and 70° Fahr. is an entire misconception. But more of this anon.

ART. XL.—*Discovery of a new Planet*; by C. H. F. PETERS. (From a letter to one of the Editors, dated Litchfield Observatory of Hamilton College, Clinton, N. Y., Feb. 26, 1876.)

A new planet, eleventh magnitude, was first seen here on the night of the 20th inst., and, after several days of bad weather, again observed on the following nights. The positions are as follows:

1876.	Ham. Col. m. t.	α(160)	δ(160)	
Feb. 20,	15^h —^{m} —^{s}	10^h 20^m 59^s	+14° 14.′1	
" 24,	12 25 41	10 17 26·8	+14 28 45	(12 comp.)
" 25,	11 46 11	10 16 33·92	+14 32 26·0	(15 comp.)

On the first night only an approximate observation was made. For the second night the comparison star still needs determination by means of the meridian circle.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On a Crystallized Hydrate of Hydrochloric acid.*—Pierre and Puchot have observed that, when a saturated solution of hydrochloric acid gas is cooled to −21° or −22° C., the dry gas being passed continuously into the liquid, after a few minutes the temperature rises to −18° and an abundant crystallization begins, during which the temperature remains constant at −18°. Before the crystallization commences, there is always observed this lowering of 3° or 4° in temperature, which is a phenomenon analogous to sursaturation. A synthetic experiment showed that, to produce the crystals, the water absorbed about its own weight of the gas; and hence showed that the probable formula was $HCl.(H_2O)_2$. In the air the crystals decompose readily, giving off dense fumes of hydrogen chloride. In a flask, kept near 0°, they slowly melt, the temperature remaining at −18°; in one experiment 115 grams of the crystals required an hour and a quarter to melt. Water dissolves them readily. Since they sink in the solution where they are formed, they must be denser than it. They set free the gas in melting, and hence must contain more of it than the mother liquors. In the analysis, a known weight of the drained crystals was treated with a definite quantity of distilled water, in amount sufficient to prevent the evolution of gas. The chlorine was then determined in the solution, and from this the ratio between the HCl and the H_2O could be calculated. In the first two determinations, the ratio was 1:2·19; in the second it was 1:2·085 and 1:2·075. Hence the authors conclude upon the formula $HCl.(H_2O)_2$; this is the best defined hydrate of hydrochloric acid yet observed. A mixture of snow two parts and hydrochloric acid one part gives a temperature of −32°C.; or of −35° if the materials are previously cooled.—*C. R.*, lxxxii, 45, Jan. 1876. G. F. B.

2. *On the Decomposition of Water by Platinum.*—Sainte Claire Deville and Debray state that if potassium cyanide be heated in a glass tube to 500° or 600°, in the vicinity of a boat full of warm water, the tube having been previously exhausted, the pressure rises to half an atmosphere, and remains constant for hours; but if, before the operation, some platinum sponge has been mixed with the cyanide, hydrogen is abundantly evolved, and a potassio-platinum cyanide is formed. This hydrogen contains from 4½ to 12 per cent of carbonous oxide, produced according to the following reaction.

$$(KCN)_2+(H_2O)_4=K_2CO_3+(NH_3)_2+H_2+CO$$

If the principal reaction in the foregoing experiment be written—

$$(KCy)_4+(H_2O)_2=PtCy_2(KCy)_2+(KOH)_2+H_2$$

it would appear as if the platinum decomposed water under the influence of the potassic cyanide. But the authors show from thermal considerations that the potassium hydrate formed is really the important product; that in its formation the greatest amount of heat is developed, and hence it is really the determinant of the reaction. From the same causes, a boiling concentrated solution of potassium cyanide attacks platinum, setting free hydrogen; an experiment the authors recommend as a convenient one to illustrate the principles of thermo-chemistry upon the lecture-table. So also a boiling solution of mercuric cyanide does not attack platinum unless potassium cyanide be present; then the mercury at once separates.—*C. R.*, lxxxii, 241, Jan. 1876. G. F. B.

3. *On a New Compound of Sulphur and Oxygen.*—For many years it has been known that the action of sulphur on sulphuric oxide or on disulphuric acid produces an intense blue color. R. WEBER has successfully investigated the cause of this color, and has shown that it is due to a new oxide of sulphur which he has isolated. To prepare it, a portion of sulphuric oxide is prepared, containing some sulphuric acid, and into this is thrown, in small portions, carefully dried flowers of sulphur. At the instant of contact the sulphur is converted into dark blue liquid drops which sink to the bottom of the liquid and there solidify. Care should be taken to keep the temperature at 15° C., since below this point the whole liquid solidifies, and above it the blue body decomposes. After the operation, the excess of liquid is poured off, the blue crystalline crusts are drained and the excess of sulphuric oxide driven off at a temperature not exceeding blood heat. Bluish green crusts are thus obtained, which are very friable and which have a structure similar to malachite. They decompose without fusion slowly at ordinary temperatures, more rapidly on heating, evolving sulphurous oxide and leaving sulphur behind. In a cool place the decomposition is so slow that the substance may readily be weighed for analysis. Moist air decomposes it rapidly and it hisses when thrown into water. Alcohol and ether also decompose it, and set free sulphur. A mean of five closely accordant analyses showed that it contained 57·12 per cent of sulphur; thus giving it the formula S_2O_3. The author names it sulphur sesquioxide or dithionic oxide. No compounds of it have yet been made. Selenium gives an analogous compound having the formula $SeSO_3$. It is dirty-green in mass, yellow in powder.—*Pogg. Ann.*, clvi, 531 Dec., 1875.

G. F. B.

4. *On the Purification of Carbon disulphide.*—FRIEDBERG proposes to effect the final purification of carbon disulphide by treating it with fuming nitric acid. The crude disulphide is first purified by repeated distillation with a vegetable fat, such as palm oil, and is then treated with the acid and frequently agitated. After twenty-four hours two layers are observed, nearly of the same color, the red vapors of the acid having been absorbed by the CS_2. If water be added, the disulphide becomes reddish violet, and this separated from the acid, washed, and gently heated, gives up pure disulphide in the distillate, while the violet-colored solution of the disulphide remains behind, and is not broken up except at a higher temperature. The colorless distillate, washed with water dried and distilled, is chemically pure. The author is investigating the solvent

power of this substance for gaseous substances.—*Ber. Berl. Chem. Ges.*, viii, 1616, Jan. 1876. G. F. B.

5. *The New Metal Gallium.*—In the session of the French Academy on September 20, the Secretary opened a sealed note deposited by Lecoq de Boisbaudran, the first paragraph of which reads thus:—"Day before yesterday, on Friday the 27th of August, 1875, between three and four o'clock in the afternoon, I obtained indications of the probable existence of a new simple body among the products of the chemical examination of a blende coming from the mine of Pierrefitte, valley of Argeles, Pyrenees." The evidence relied on to prove this discovery, a part of which evidence was given in the sealed note and another part in a note read at the same meeting, is: (1) the oxide (or perhaps a basic salt) is precipitated slowly by metallic zinc in a solution containing chlorides and sulphates; (2) its salts are easily precipitated by barium carbonate in the cold; and (3) it gives a spectrum showing two violet lines of wave lengths 417 and 404 respectively. In all its other chemical reactions, it closely resembles zinc; though in the precipitations it has always the preference when these are incomplete. To the metal thus indicated, Lecoq de Boisbaudran gave the name *Gallium.* In a more recent paper he gives additional facts regarding the new metal, which he has been able to free almost entirely from zinc. From it he has prepared a salt which he believes to be gallium-alum. It is soluble in cold water, but is decomposed on heating, unless acetic acid be present. It crystallizes in octahedrons and cubes, presenting the appearance of common alum, especially under the microscope; the crystals do not polarize light. Placed in a super-saturated solution of ammonio-aluminum alum, they act as nuclei and begin to grow. Treated with ammonia, a part only of the oxide is thrown down. In ammoniacal solution, the metal is precipitated by electrolysis on the negative electrode. In the first trial 1·6 milligrams were deposited in 4½ hours; in the second, 3·4 milligrams was deposited in 5 hours 40 minutes. (This sample was submitted to the Academy.) The metal adhered strongly to the platinum on which it was deposited. When burnished its surface is brilliant, and has a color between silver and platinum. With a feeble current, the metal comes down frosted and crystalline. It does not decompose water at ordinary temperatures, and tarnishes slowly in the open air. With HCl, it evolves hydrogen. On the evidence of the alum, he fixes the formula of the oxide as Ga_2O_3, and assigns the metal to the aluminum group. In a subsequent note, the author gives the results of the more accurate measurement of the wave-lengths of the two lines of the gallium spectrum, α and β. A concentrated solution of the chloride gave only the two lines at first observed, of wave-lengths 417 and 403·1, the former being the stronger.—*C. R.*, lxxxi, 493 (Sept.) 1100, (Dec. 1875); lxxxii, 168, Jan. 1876. G. F. B.

6. *Conductibility of Gases.*—M. A. Winkelmann has measured the conductibility of gases for heat by an apparatus like that of Stephan, except that a peculiar manometer and method of

closure is used. The apparatus consists of a brass cylinder serving as an air thermometer enclosed in a second concentric cylinder. The top of the inner cylinder may be unscrewed and it carries a conical cavity in which is placed a rubber cork with a hole through it. A glass tube passes through this hole and a metallic cap screwed on below the rubber presses it against the glass. A similar closure carries the tube through the outer cylinder. The latter is connected with a mercury pump. A comparison of the time and variations of the pressure when the outer cylinder was immersed in ice-water gave the following coefficients of conductibility:

Name.	Conduct.
Air	·0000525
Hydrogen	·0003324
Carbonic Acid	·0000317
Ethyl	·0000414
Marsh Gas	·0000647
Nitric Oxide	·0000460
Carbonic Oxide	·0000510
Oxygen	·0000563
Nitrous Oxide	·0000363
Nitrogen	·0000524

—*Pogg. Ann.* clvi, 497. E. C. P.

7. *Thermal Properties of Liquids.*—M. Pictet has applied the mechanical theory of heat to the study of volatile liquids, making use of the experiments of Regnault, and deduces the following simple relations between their latent heats, atomic weight and vapor tension:

(1.) The cohesion of all liquids is constant.

(2.) The differential coefficient of the Naperian logarithm of the tension divided by the temperature is constant for all liquids when referred to the same pressure and temperature.

(3.) The latent heat of all liquids referred to the same pressure, multiplied by the atomic weight referred to the same temperature, gives a constant product.

(4.) For all liquids the difference of the internal latent heats at any two temperatures, multiplied by the atomic weight is a constant number.

It thus appears that quantities at first sight wholly independent are really connected by very simple relations, which dispense with long empirical formulas based on observations more or less open to criticism.

Furthermore, admitting the law of Dulong and Petit for specific heats, we can further say that the latent heat of all liquids are multiples of their specific heats.—*Bibl. Univ.*, ccxvii, 66.

E. C. P.

8. *Dependence of Electrical Resistance on the Motion of the Conductor.*—M. Edlund has brought to bear a new argument in favor of his theory of electricity, by showing that the resistance of a conductor is affected by its motion. Water is allowed to

flow through a long tube having three electrodes of gold wire admitted at its ends and center. A battery of two Daniell's cells has one terminal connected with the center electrode, and the other with two of the terminals of a delicate differential astatic galvanometer. The two end electrodes are connected with the other terminals of the galvanometer. The current from the battery divides, and half passes through the tube in each direction. By suitably varying the resistance, the galvanometer needle will now be at rest. When the water is caused to flow through the tube, however, the resistance in one direction will be increased, and that in the other diminished, since, according to Edlund's theory the current is proportional to the amount of ether flowing through a given section per second. Accordingly the needle should deviate, as, in fact, it does. To eliminate the effects of polarization, the current was inverted without changing the result. That the deviations may be regular, it is essential that the liquid should have a great resistance and the amount of deviation is almost independent of this resistance. Two series of observations were made, one with distilled water, the other with alcohol and water, and gave similar results. A third series with aqueduct water gave the same result. Finally, equal currents were sent in opposite directions through the pipe, when they produced no effect on the needle, but as soon as the liquid was set in motion a deviation was always obtained indicating that the resistance was greater in one direction than in the other. These two methods of observation lead to the same result, foreseen by the theory of Edlund, namely, that the galvanic resistance diminishes if the conductor moves in the same direction as the galvanic current and increases on the contrary if the other two currents move in opposite directions.—*Royal Swedish Acad.*, III., *No.* 11, *Phil. Mag.*, l, 89. E. C. P.

9. *Electric Spark with large Batteries.*—Messrs. WARREN DE LA RUE and H. W. MULLER presented to the Royal Society at a recent meeting a paper having the following title: On the length of the Spark from a Battery of 600, 1200, 1800 and 2400 rod-chloride of Silver, and some Phenomena attending the discharge of 5640 cells. A year ago some experiments on the stratification of the discharge in vacuo of a battery of 1080 cells were described. This battery has now been augmented to 5640 cells, and two other batteries will soon be added making 9120 cells. Having completed 2400 cells and charged them up in a single day, they were exactly in the same condition as to electro-motive force and internal resistance, consequently they afforded the means of testing the truth of the law of the length of the spark in a manner more efficacious than had hitherto been obtained, the more especially as by the use of paraffin corks and other precautions we had obtained an excellent insulation. A discharger with a micrometer-screw was constructed by which the length of spark could be measured to ·001 of an inch, or by estimation to one-tenth of that quantity. In making measurements the terminals were separated

to a greater distance than the anticipated striking-distance and gradually approached until the spark passed. The discharger was then detached from the battery, and after reading the scale, connected with a separate battery of 10 cells with a detector-galvanometer in circuit. The terminals were again approached until the motion of the galvanometer indicated contact between them. The scale was again read, and the change in reading gave the required length of spark. With 600, 1200, 1800 and 2400 cells the striking-distances were found to be ·0033, ·0130, ·0345 and ·0535 inches. These numbers are nearly proportional to the square of the number of cells, which would give the distances ·0033, ·0132, ·0297, ·0528. The length of spark is much influenced by the form of the terminal. Generally a copper-pointed plane was employed and the current reversed 352 times per second by a revolving commutator, or a double key discharger.

When the point was negative, a glow in form like a paraboloid was seen surrounding it long before the spark passed, and as the distance was diminished gradually extending to the positive terminal. With 1800 cells the glow was seen at a distance of ·0545 and with 2400 cells at a distance of ·0865 inches. Moreover when the disc was positive it became covered with a peach-like bloom which became stronger in the center as the terminals were approached, giving rise to Newton's rings. This effect was next studied with the whole series of 5640 cells. The glow was now visible at 1·073 in. and the spark passed at ·139. Replacing the flat disc by one that was slightly convex the glow occurred at 1·124 in. and the spark at ·140. Reversing, the current gave sparks of ·154 and ·164 in.

To ascertain whether a current really passed when the glow appeared, vacuum-tubes were interposed when they were illuminated even before the glow appeared. Of course the striking distance was in this case shortened. With a hydrogen tube having a resistance of 190,000 ohms the glow occurred at ·939 and the spark at ·092 inches. A 31-inch tube was brilliantly illuminated when interposed between one terminal and the battery, when the terminals were separated to the extreme range of the discharger or 1·2 in. and before any glow was visible at the negative electrode. Later a current was obtained with the negative point 5·1 in. distant from a positive plate 6 in. in diameter.

Considerable difficulty was experienced in measuring the resistance of the tubes, and it soon appeared that this resistance rapidly increased as the current passed. After a time, however, they recovered their original resistance. Ultimately it was found to be better to discard the indications of the galvanometer and to rely solely on the appearance of a luminosity in the tubes placed on one side of Wheatstone's bridge as soon as the insertion of a balancing resistance was made in the other. A curious conclusion is derived from the law that the length of spark is proportional to the square of the number of cells, if it proves to be correct. One cell would give a spark about ·00000001 in. long while a hundred thousand which come within the limit of experimental possibility would give a spark about 92 inches long. Probably a million would never be made but they should give a spark 9166 inches or 764 feet long.—*Nature*, xiii, 277. E. C. P.

10. *Acoustics.* Letter to the editors by Professor A. M. MAYER, dated Stevens Institute of Technology, Hoboken, New Jersey, March 17th, 1876.—Gentlemen: Having in hand researches whose completion will occupy several months, I desire to place on record my invention of the two following methods of research. The first is a plan for *the determination of the relative intensities of sounds of the same pitch.* The second is *a method of determining the direction of sounds.* I request the privilege of being permitted first to attempt to develop these ideas after I have finished the original work which at present occupies all my leisure.

First method. A loose membrane, or a slip of gold or aluminium foil, is placed anywhere between the centers of origin of two sounds of the same pitch. The plane of this membrane is at right angles to the line connecting these sonorous centers. If both sides of the membrane are simultaneously acted on by sonorous vibrations of the same phase and of equal intensity, the membrane will remain at rest. The above condition is thus attained. Attach to the center of the membrane a short delicate glass thread whose end can be observed through a microscope, or, place a reflecting metallic film on the central part of the membrane so that one can observe the motion of a beam of light reflected therefrom. If we place, at hazard, the membrane between the sonorous centers it is probable that it will be set in vibration. Now if it is moved from its position its vibrations will either increase or decrease in amplitude. Move it in the direction that causes the amplitude of the vibrations to decrease, and until the vibrations have a minimum of swing. The membrane is now in a plane where the phases of vibration are the same but of *unequal* intensity. The membrane is now moved one-half wave-length either from or toward one of the sonorous centers and is thus brought into another plane of minimum vibration. Thus move the membrane until it is brought into that plane where vibrations of the membrane are either entirely destroyed or have their least amplitude. If the membrane vibrates, then move it and the source of one of the sounds so that they both approach to or recede from the other sonorous center always by the same quantity. This is accomplished by moving a board to which is attached the membrane and one of the sources of sound. By the last adjustment we can soon reach a plane where the membrane remains at rest and where the intensities of the two sonorous vibrations are equal. It appears that I have thus devised a *phonometer* which is analogous to the photometer of Bunsen.

The above method appears to me preferable to the phonometer I described in this Journal, Feb., 1873. There are objections to the use of resonators and reflection which I cannot here explain.

Second method. If the plane of a free membrane be placed at right angles to a wave-front it cannot vibrate, for both of its sides are simultaneously acted on by impulses of the same phase and of equal energy. Thus, by bringing the plane of a membrane into that azimuth where it remains at rest we shall have found the plane passing through the center of origin of the sound. I also propose the use of two resonators, or of two ear trumpets, placed at the ends of a long horizontal rod which rotates around a vertical axis. I may thus obtain an increase in the aural parallax. By rotating the horizontal rod around its center I may be able to bring the two sonorous sensations either to disappear, or to become of equal intensity, and by these indications to arrive at the direction of a sound. The last mentioned idea may develop into something useful to the mariner who has to ascertain the *direction* of a fog signal.

II. Botany and Zoology.

1. *Botanical Contributions*, separately issued from the current (eleventh) volume of the *Proceedings of the American Academy of Arts and Sciences.*—These are, first, a series of miscellaneous contributions, characters of new species, and several new genera, mainly Californian, by Asa Gray. The paper begins with the discrimination of two plants which have long been confounded, namely, *Sedum pusillum* of Michaux and *Diamorpha pusilla* of Nuttall. They grow together, but are distinct enough in appearance as well as in structure when seen in the living state, as they were by the author of this paper a year ago. There is a revision of the genus *Collinsia*, of the North American species of *Mimulus*, twenty-nine in number, and of *Monardella*, eleven in number.

The other papers are by Sereno Watson. 1. *On the Flora of Guadalupe Island*, Lower California, founded upon a unique collection of dried plants made by Dr. Edward Palmer. 2. List of the collection with Dr. Palmer's notes upon them. There is a notable amount of new species, and two new genera. These are characterized, as are the new *Gamopetalæ*, by Prof. Gray in his paper above mentioned, as to the remainder by Mr. Watson in his third article, entitled, *Descriptions of New Species of Plants chiefly Californian, with Revisions of certain Genera.* The other new species described and the revisions are mainly such as came to light in Mr. Watson's work on the *Polypetalæ* of the Botany of California, soon to be published. Among them are many plants of much interest, such, for instance as second species of the genera *Crossosoma*, *Lyrocarpa* and *Adolphia*. *Cercidium* is shown to belong to *Parkinsonia*. A revision is made of the North American species of *Trifolium*, also of *Lathyrus* and *Peucedanum*, both very critical works; and the cucurbitaceous genus *Megarhiza* of Torrey is re-established upon five species. Mr. Watson's descriptions, as usual, are in the English language,—an advantage in home but not for foreign use. A. G.

2. *Botanical Necrology* of 1875. On the home list only one name recurs to memory, that of

INCREASE ALLEN LAPHAM, LL.D. He died September 14, in the 64th year of his age. A beautiful tribute to his memory, read before the Old Settler's Club, of Milwaukie, by S. S. Sherman, Esq., has just been printed, and is noticed on a following page. An excellent portrait is prefixed.

The following botanists have deceased in Europe:

FRIEDRICH GOTTLIEB BARTLING, one of the oldest professors at Göttingen, a veteran teacher, but not a voluminous author; aged 77.

ALEXANDRE BOREAU, of Angers, France, author of the Flora du Centre de la France.

JOHN EDWARD GRAY, March 7, at the age of 75. Principally known as a zöologist, some of his earliest work was in botany. A notice of his life and services appeared in this Journal, vol. x, p. 78.

JEAN CHARLES MARIE GRENIER, one of the authors of the classical Flora de France, died at Besançon, in the 69th year of his age.

DANIEL HANBURY, died at Clapham, March 24, in his 50th year. A notice appeared in this Journal, vol. ix, p. 75. We learn that his scattered writings are to be collected.

RUDOLPH FREIDRICH HOHENHACHER, died at Kirchheim in Wurtemburg, late in the preceding year, November 14, 1874. He was in early life a missionary at Astrakan, and was afterward in the Caucasian provinces. He was one of the founders of the Unio Itinerario, and he survived his associates, Steudel and Hochstetter.

LIEUT. GENERAL JACOBI, the monographer of *Agave*, died at Berlin, early in the year.

ERNST FERDIND NOLTE, of Kiel, a veteran botanist, who had retired from his professorship a year or two ago, died February 13, at the age of 84.

GUSTAVE THURET, died suddenly at Antibes, France, May 17, at the age of 58. A brief notice of this sad loss was given in vol. x, p. 67. Among other tributes to his memory is one by Rostafinski in the Botanische Zeitung, for July 30. Also one by Professor Farlow in Trimen's Journal of Botany, for January last.

ADOLPHE BRONGNIART. We learn that this veteran botanist and vegetable paleontologist died at Paris. A. G.

3. *Life Histories of Animals, including Man, or Outlines of Comparative Embryology;* by A. S. PACKARD, Jr. 8vo, 239 pp. with 268 cuts. New York, 1876. (Henry Holt & Co.)—This work consists of a series of papers published in the American Naturalist during the past year, with the addition of a few pages on mammals and nearly three on man. Although, according to the preface, the original papers have undergone "careful revision," we regret to notice many serious errors and inaccurate statements that have been allowed to remain, and are much less excusable in

the permanent book form than in mere magazine articles. Most of these errors must evidently be attributed rather to carelessness in the mode of statement than to lack of knowledge on the part of the author. Thus on page 187, after properly describing the zoëa of crustacea as having only "antennæ, jaws and foot-jaws" for locomotive appendages, he states that he has examined *Gelasimi* carrying eggs which "contained zoëæ, with the two claws alike, and it is probable that the strange inequality in size of the claws in these animals does not show itself until after one or more moults." What such an incongruous statement means can scarcely be imagined, for the "claws" and other legs are not even formed until a much later period, and it is well known that no such inequality exists in the adult females, nor even in the young males until after they become genuine little fiddler-crabs. On page 216 he says: "the tadpole is much less developed than the larval fish or any other vertebrate; the intestine is not yet formed," but in the next sentence he adds: "It is also a vegetarian, eating decaying leaves; the mouth is small and round, the alimentary canal is remarkably long, the intestine coiled up in a spiral, the mouth is small, destitute of a tongue." On page 157 he says of the Gephyrea: "In none of these worms are there bristles or indications of segments," forgetting the well-marked segments and conspicuous setæ of *Echiurus*, and the less numerous ones of *Thalassema* and other genera. On page 120 he says: "There is in the Annelids a dorsal and ventral blood-vessel, the circulatory apparatus being closed and more highly developed than in the Crustacea and Insects, *Limulus* excepted," but this is by no means true of all Annelids, for in many genera (*Polycirrus*, etc.,) there are no blood-vessels whatever, and many others have a very imperfect system of vessels. On page 117, fig. 126 is said to represent a later stage of *Loligo* than fig. 125, but the reverse is true. On page 78 it is said that "in the star-fishes and Holothurians, the alimentary canal opens into five voluminous cæcal appendages," and that these "are in connection with the complicated water tubes" (ambulacral tubes and suckers). The latter statement is entirely erroneous, or at least misleading, and the former is not true of most Holothurians, nor even of all star-fishes. The statement on page 79, that "in those star-fishes in which the alimentary canal is a blind sac, the eggs are emptied into the body cavity" is also incorrect, at least for most species. The statement (p. 70) that "in the Hydroids also the ovaries hang outside the body cavity" is inaccurate, as are also the statements (p. 63) that only *one case* of multiplication by fission has been observed among Hydroids, and on page 58, that the "ovaries" of *Hydra* "differ entirely in their mode of formation from the ovaries (gonophores) of the marine Hydroids, which are genuine buds." The account of *Physalia* (p. 65); that of the growth of septa in Polyps (p. 71); of the development of barnacles (p. 169), and many other paragraphs need revision. The peculiar mode of development of *Tubulariæ*, in which the embryo becomes an "actinula" without going through a "planula" stage, is not alluded to, the planula stage being given as a stage of all Hydroids (p. 66). On page 336 "*Didelphia*" is used where *Monodelphia* is intended. The five diagrammatic figures on p. 225, given to illustrate birds (fowl), would serve better for mammalian embryology. The lists of works given at the end of each chapter are very incomplete and unsatisfactory, many of the latest and most important books being omitted from most of them. Thus under Hydroids the works of Hincks and Allman, both of which contain much of importance on embryology, are omitted. The magnificent treatise on Tubularians by the latter is certainly one of the most important hitherto published, both for the embryology and structure of Hydroids. The recent extensive work on the Nemerteans by McIntosh is not mentioned, though it contains the embryology of the group. Cobbold's works are only once alluded to, and are not mentioned under those groups of Helminths upon which he has done the most. The very valuable works of G. O. Sars on various Crustacea, including the discovery of the very remarkable phenomena in the embryology of *Cladocera*, are not referred to. Most of these are works written in English, and should be well known to the author of a work on embryology. Certainly references to such works would have been more useful for most of his readers than those that he gives to special papers in the German and Russian periodicals, which are generally inaccessible, however valuable they may be. In spite of these defects the book will doubtless prove to be a very useful one, there being no other work in English covering the same ground. V.

4. *On some Remarkable Forms of Animal Life from the great deeps off the Norwegian Coast.* II. *Researches on the Structure and Affinity of the genus Brisinga, based on the study of a new species, Brisinga coronata;* by George Ossian Sars, (University-Programme for the last half-year, 1875. Christiania).—In this valuable memoir, which is illustrated by seven excellent plates, Professor Sars has given a detailed description of the anatomy, physiology, and development of the genus *Brisinga*, perhaps the most remarkable form of star-fish hitherto discovered. The author also discusses, at considerable length, its relations to other star-fishes, recent and fossil, as well as to Echinoderms in general, and the relation of Echinoderms to the Annelids. He regards *Brisinga* as the most generalized form of star-fish, and consequently of Echinoderms, and supposes it to be one of the little-modified survivors of a primitive type from which the other forms of Echinoderms have descended. It has affinities to the most ancient fossil starfishes of the Palæozoic rocks (*Protaster*, etc.)

The existence of a genuine vascular system, distinct from the general perivisceral cavity and its extensions, is denied both in the case of this genus and of other star-fishes. The author also states that there is no anal orifice, although there is, as in other star-fishes, a dorsal gland, with a narrow duct opening on the dorsal surface, and he suggests that this duct has in other star-fishes been mistaken for an intestine, and its outlet for an anus, the existence of which, in any star-fish, he doubts. Prof. Sars adopts the view, previously advanced by Duvernoy, Huxley, and Hæckel, that an Echinoderm is a cluster (or "cormus") composed of several articulated zoöids ("persons") united by their anterior ends. V.

5. *A Course of Practical Instruction in Elementary Biology;* by T. H. HUXLEY, assisted by H. N. MARTIN. 268 pp. small 8vo, without illustrations. (Macmillan & Co.) 1875.—The publishers have seen fit to over-burden this otherwise excellent little manual with an exorbitant price ($2.50 in the U. S.), which will doubtless prevent its adoption in many cases where it should be used. It contains very full descriptions of the structure of a number of diverse forms of plants and animals, with plain and explicit directions how to dissect and study them. V.

6. *Crustacea of Mexico and Central America.* Mission Scientifique au Mexique et dans l'Amérique Centrale, publié par ordre du Ministre de l'Instruction Publique. *Etudes sur les Xiphosures et les Crustacés de la région Mexicane*, par M. ALPHONSE MILNE-EDWARDS. 4to, Paris: 1re et 2e livraisons, pp. 56, plates 1–14, 1873; 3e livr., pp. 57–120, plates 15–20, 1875.—These memoirs are published in the same elaborate and sumptuous style as the other works of this series, and are by far the most extensive and important contribution yet made to our knowledge of the crustaceans of the tropical region of America. The first forty pages and twelve plates are devoted to an elaborate study of the anatomy of Limulus, the substance of which had previously appeared in the Annales des Sciences Naturelles during the delay in the publication of the present work. The second memoir is devoted to a systematic account of the stalk-eyed Crustacea of the Mexican region, including Central America, Lower California, the Galapagos, and the West Indies. This memoir, beginning with the Maioidea, treats, thus far, of the *Pericerinæ*, *Pisinæ*, and *Mithracinæ*, including twenty-three genera, or sub-genera, and seventy-four species. A large proportion of the species are represented on the plates by beautiful figures of the entire animals, and numerous details. Twelve of the species and five of the genera are described as new. The author states, in the introduction, that a large part of the collections obtained by the Mexican Commission were destroyed, during the bombardment of Paris, by the explosion of a Prussian shell which had passed through the cases containing the conchological collections. On this account the work is based to a considerable extent on collections received from the United States, particularly from the Smithsonian Institution and the Museum of Comparative Zoology at Cambridge. S. I. S.

7. *Cumacea from great depths in the Arctic Ocean;* by G. O. SARS. 12 pp. 4to, with 4 plates. (From the Svenska Vetenskaps-Akademiens Handlingar, Bandet xi; Stockholm, 1873.)—This is the third of Professor Sars's richly illustrated memoirs on the Cumacea, and treats of five species from the Arctic Ocean. *Diastylis polaris* and *D. stygia* are from the remarkable depths of 950 and 2600 fathoms respectively. S. I. S.

8. *Moa or Dinornis of New Zealand.*—Remains of skeletons of fifteen Moas have been discovered along the beach, north of Whangarei Heads, sixty miles north of Auckland. Several human skulls and a complete human skeleton in sitting posture (the usual burying posture among the natives) were found with the Moa bones. Previously, no Moa bones had been found north of Auckland.—*Nature*, Feb. 3.

9. *Carnivorous Reptiles having some features of Carnivorous Mammals from the Triassic (?) of South Africa.* —Professor OWEN has described, in a paper read before the Geological Society of London on February 2d, a carnivorous reptile, named by him *Cynodracon major*, which has the compressed sabre-shaped canines of the Lion of the genus *Machærodus*, and resembles Carnivores both in the canines and incisors. In the lower jaw the bases of eight incisors and of two canines (very inferior in size to the canines of the upper jaw) are visible, and the canines are separated by a diastema from the incisors. In this character, as in the number of incisors, the fossil resembles a *Didelphys*. "The left humerus is 10½ inches long, but is abraded at both extremities; it presents characters, in the ridges for muscular attachment, in the provision for the rotation of the forearm, and in the presence of a strong bony bridge for the protection of the main artery and nerve of the forearm, which resemble those occurring in carnivorous mammals, and especially in the Felidæ, although these peculiarities are associated with others having no mammalian resemblances." "Prof. Owen discusses these characters in detail, and indicates that there is, in the probably Triassic lacustrine deposits of South Africa, a whole group of genera (including *Galesaurus*, *Cynochampsa*, *Lycosaurus*, *Tigrisuchus*, *Cynosuchus*, *Nythosaurus*, *Scaloposaurus*, *Procolophon*, *Gorgonops and Cynodracon*), many represented by more than one species, and all carnivorous, which have more or less decided mammalian analogies; and to them he gives the general name of *Theriodonts.*

The common characters of the Theriodonts are as follows: dentition of the carnivorous type; incisors defined by position, and divided from the molars by a large laniariform canine on each side of both jaws, the lower canine crossing in front of the upper; no ecto-pterygoids; humerus with an entepicondylar foramen; digital formula of the fore foot 2, 3, 3, 3, 3 phalanges.—*Proc. Geol. Soc. of Feb.* 2, *in Ann. Mag. Nat. Hist.*, for March, 1876.

10. *The Crustacean, Artemia salina, changed in some of its characters by changing the saltness of the water in which it lives.* —W. J. SCHMANKEWITSCH announces, that by increasing the saltness of the water in which the *Artemia salina* lives, a modification goes on from generation to generation, until the caudal lobes finally disappear, and the form is that in the *Artemia Mühlhausenii;* and by reversing the process, the caudal lobes grow out again and become those of *A. salina.* In 1871 the salt marshes about Odessa contained great numbers of *A. salina*; the waters then marked only 8° Baumé. Afterward, on the repair of a dyke,

the saltness increased to 14° Baumé in the summer of 1872, and 25°, in August, 1874. The changes in the species were then first noted. The author afterward corroborated the fact by experiments on Artemiæ reared in captivity in water of which the saltness was gradually increased. A change also takes place, correspondingly, in the form of the branchiæ, and in the number of apodal segments. —*Ann. Mag. N. H.*, March, 1876, from *Zeitschr. wiss. Zool.*, xxv, Suppl. i, 1875, p. 103, pl. 6.

III. Astronomy.

1. *Astronomical and Meteorological Observations made during the year* 1873, *at the U. S. N. Observatory, with Appendix;* Rear-Admiral B. F. Sands, Superintendent. Gov. Printing Office.

This volume contains the record of a year's work at the Observatory, and is evidently the record of first rate work. The Appendix by Professor Newcomb has been already noticed in our February Number.

If the use for several years of one value of the latitude of the Observatory for reducing observations, made by the mural circle, and of another value for observations made by the transit circle, were the deliberate choice of the Superintendent, astronomers would probably think his decision unwise. The same also must be said of using a different latitude each year for the final tables of north polar distances, derived from the observations of the transit circle. It would also look better, to say the least, if the results were given in the same denomination, instead of north-polar distance for one instrument, and declination for the other.

These things look not so much like the deliberate choice of the Superintendent, as the kind of little irregularities that must be expected from a system that makes little account of scientific fitness in appointing the Superintendent of a scientific institution. We hope for better things from the present Superintendent. H. A. N.

2. *Auxiliary Tables for determining the angle of position of the sun's axis and the latitude and longitude of the Earth referred to the Sun's equator:* by Warren De La Rue; 20 pp. 4to, London, 1875. Printed for private circulation.

IV. Miscellaneous Scientific Intelligence.

1. *Third Report of the Settle Caves (Victoria Cave) Committee of Exploration;* by R. H. Tiddeman. (Rep. Brit. Assoc. 1875.) —Mr. Tiddeman was Secretary of the Committee in charge of the exploration, the other members being Sir John Lubbock, Prof. Hughes, Prof. Dawkins, and Mr. L. C. Miall. The Victoria Cave afforded the preceding year, among remains of *Ursus spelæus*, *U. ferox*, *Hyæna*, *Rhinoceros hemitæchus*, *Bison*, *Cervus elephas*, and molars of *Elephas antiquus*, a bone pronounced by Prof. Busk to be a human fibula. Thick bowlder deposits covered the entrance to the cavern, and hence the remains were pronounced to

be pre-Glacial. This *third* Report states that great progress had finally been made in uncovering the glacial deposits at the entrance, in which were bowlders of all sizes up to several tons in weight. The question whether "these glacial deposits, which rest upon the older bone-beds containing the remains of extinct mammals and man, are in the position which they occupied at the close of the Glacial conditions, or have subsequently fallen into their present site," is answered by stating that the new facts "go to prove the first alternative." In one chamber (numbered D) the upper bone-bed afforded remains of the *Badger*, *Horse*, *Pig*, *Reindeer*, *Goat* or *Sheep*, and was peculiar in the abundance of *Reindeer* remains and the absence of the Elephant, Rhinoceros, Hippopotamus, Hyæna, as if it were of the Reindeer epoch, or later; the lower afforded bones of *Hyæna*, *Brown Bear* (*?*), *Elephas antiquus*, *Rhinoceros hemitœchus*, *Hippopotamus*, *Bos primigenius;* while, in both, there occur remains of *Man*, *Fox*, *Grisly Bear* and *Red Deer*. A piece of a human rib was found during the year in the lower bed, near where the fibula was taken out.

2. *Air and its Relations to Life;* by WALTER NOEL HARTLEY, F.C.S., Kings College, London. 263 pp. 12mo. New York, 1875. (D. Appleton & Co.)—This very readable little volume contains the substance of a course of six summer lectures delivered in 1874 at the Royal Institute of Great Britain. The author exhibits the rare faculty of presenting the results of exact science in a form perfectly intelligible and attractive to intelligent people not familiar with the technical language of science. The researches of the most trustworthy investigators are cited with good judgment from the days of Black and Lavoisier to those of Retenkofer, Angus Smith and Pasteur. Indeed it is not easy to say where else in English we can find so full a statement of the researches of Pasteur as in chapter four of Mr. Hartley's essay.

3. *Geological and Geographical Survey of the Territories*, Prof. F. V. HAYDEN *in charge.*—Bulletin No. 1, Vol. II, of this Survey has appeared It contains seven articles. Three, by, severally, Messrs. Holmes, Jackson and Bessels, treat of the Ancient Ruins of Southwestern Colorado, Utah and Arizona, and are illustrated with twenty-nine octavo plates, of cliff dwellings and other ruins, pottery, utensils, crania, etc. Of the remaining four, three are short articles on the Ute Indians, by E. A. Barber; and a fourth consists of descriptions of thirty-one new species of fossil Coleoptera from the Tertiary formations of the West, by S. H. Scudder. The volume is full of interesting facts in American Archæology, and the maps and plates illustrate well the subjects discussed.

3. *Compressed Peat.*—Peat pressed into blocks and made so compact that a cubic foot weighs 85 to 100 pounds, is manufactured by Mr. A. E. Barthel, of Detroit, Michigan, and sells for one and a half dollars per ton.

4. *Report of the Superintendent of the U. S. Coast Survey*, showing the progress of the Survey during 1872. This report contains 18 appendixes, among which we note the report of Assis-

tant Cutts and Prof. Young of Astronomical and Meteorological observations made at Sherman, Wyoming Ter., pp. 75–172; and a preliminary report on transatlantic longitude, by Prof. Hilgard, pp. 227–234.

Notices of the following new publications have been excluded from this number by want of space.

Geological Survey of Ohio. Paleontology, vol. ii. 436 pp. 8vo, with numerous plates. A volume of great value.

Mineral Resources West of the Rocky Mountains. 7th Annual Report, by R. W. Raymond. 540 pp. 8vo. Washington, 1875.

Second Geological Survey of Pennsylvania. Report of Progress in the Clearfield and Jefferson District of the Bituminous Coal-fields of Western Pennsylvania, by Franklin Platt. 296 pp. 8vo, with 139 wood-cuts and 10 maps and sections. Harrisburg, Pa., 1874.

Bulletin of the U. S. National Museum. No. 1 by E. D. Cope, and No. 2 by J. H. Kidder. Smithsonian Institution, 1875.

Report on the Geology of a portion of Colorado, by John J. Stevenson. 372 pp. 4to, 1876. Reprinted from Lieut. Wheeler's Survey Report, vol. iii.

Report of the Chief Signal Officer, War Department, 1875. 476 pp. 8vo, with numerous maps.

OBITUARY.

I. A. Lapham, LL.D.: a Biographical Sketch, by S. S. Sherman. 80 pp. 8vo, 1876.—This memoir is a very just tribute to the memory of Dr. Lapham, who died at Milwaukee, Wisconsin, in September, as already stated in this Journal. Dr. Lapham was a man of very varied knowledge and scientific labors. Early in life, while engaged in engineering duties, he began a collection of plants, which at his death numbered 8,000 species, and he also published papers on the geology of portions of Ohio. Moving to Milwaukee in 1836, he commenced observations on the topography, soil, mineral and other industrial resources, of Wisconsin, and on the commerce and navigation of the lakes, and kept tables of the daily temperature, rain-fall, and other meteorological phenomena; and in 1844 he published for Wisconsin a volume of 250 pages, on these topics. He afterward contributed Agricultural, Botanical and Geological papers to the Transactions of the Wisconsin State Agricultural Society, among them a valuable treatise of nearly 100 pages on "The Grains of Wisconsin and adjacent States," a paper which he afterward extended to a manuscript volume of 574 pages on the Gramineæ of the United States, but which remains unpublished. The fluctuations in the level of Lake Michigan early engaged his attention, and in 1849 he announced his discovery of "a slight lunar tide in Lake Michigan." The study of Indian mounds of Wisconsin occupied much of his time, and as early as 1836 he called attention to a turtle-shaped mound at Waukesha. He was the first to notice that many of these aboriginal earthworks are "gigantic basso-relievos of men, beasts, birds and reptiles." His well known and highly valued "Antiquities of Wisconsin," printed by the Smithsonian Institution, is a large quarto volume, containing 55 plates and numerous wood-engravings, all from his own drawings. One of his last labors was the preparation of a series of models of the Indian mounds for the Centennial Exhibition of 1876.

Another subject which occupied him was Meteorites; and peculiar crystalline markings in a Wisconsin meteorite, first noticed by him, were designated by Dr. J. Lawrence Smith, in a paper on the meteorite in this Journal (II, xlvii, 271), *Laphamite markings.*

The establishment of the Signal Service Bureau at Washington in 1869 was due largely to personal effort and influence on the part of Mr. Lapham.

Dr. Lapham was placed, in 1873, at the head of the Geological Survey of the State of Wisconsin, a position for which he was well fitted: and the Survey went forward with energy and important results through that year and 1874. To the misfortune of science and the State, he was deposed at the close of 1874, and, through political management, a man ignorant of geology was substituted. It was a serious disappointment to Dr. Lapham, and not less so to all friends of science in the land. "His abrupt dismissal was all the more cruel because this was the only opportunity he ever had of perfecting and giving to the public in a permanent form the results of a life-work in the geology, natural history and industrial resources of the State."

Dr. Lapham was active also in all educational movements; a founder of the Milwaukee Female College, a liberal contributor to the Cabinet of the Wisconsin State University, and one of the founders of the Wisconsin Historical Society, and of the Wisconsin Academy of Sciences.

Rev. Augustus Wing, of Rochester, Vermont, died in Whiting, in that State, on the 19th of January, aged sixty-seven years. Mr. Wing was a graduate of Amherst College, of the class of 1835. Although not a geologist by profession, a large part of his time for many years had been spent in the study of the rocks of Vermont, and especially of the crystalline limestone, quartzyte and slates of the central portion of the State. By the discovery of Lower Silurian fossils in the crystalline limestone at several different localities he threw much light on the geology of metamorphic New England. In August of the past year the writer had the pleasure of accompanying Mr. Wing on a visit to some of his localities that were of special interest for their fossils or for their illustration of the stratification of the rocks, and this was his last scientific excursion, excepting one of a few days during the following fortnight. His almost child-like delight over the places of his remarkable discoveries as he pointed them out; his earnestness in making known his conclusions and in supporting them against all expressed doubts; his eager, rapid gait as we walked over the rocks and hills, made him an especially agreeable companion, and suggested no thought of the end that was so soon to come. Before parting, he promised to send for this Journal an account of his discoveries, as in fact he had done before. But he disliked writing, and it was not sent. We hope that his notes may yet afford material for such a paper.

J. D. D.

APPENDIX.

ART. XLI.—*Principal Characters of the* BRONTOTHERIDÆ; by O. C. MARSH. With four plates.

THE remains of a well-marked group of gigantic mammals are abundant in the lowest deposits of the Miocene, on the eastern slope of the Rocky Mountains. These animals, which have been named by the writer, *Brontotheridæ*, equaled the Eocene *Dinocerata* in size, and resembled them in some important features. They do not, however, belong to the same order, but constitute a distinct family of Perissodactyles. Four genera of this family are now known, as shown below, but *Brontotherium* is the only one represented by sufficient remains to clearly indicate its structure and affinities; and hence this genus will be first described, and mainly used to illustrate the group.

Brontotherium Marsh, 1873.*

The skull in *Brontotherium* is long and depressed, and resembles that of *Rhinoceros.* The occipital region is extended vertically, and deeply concave posteriorly. The vertex is concave longitudinally, and convex transversely. The general form of the skull is shown in the cut given below, figure 1.

1.

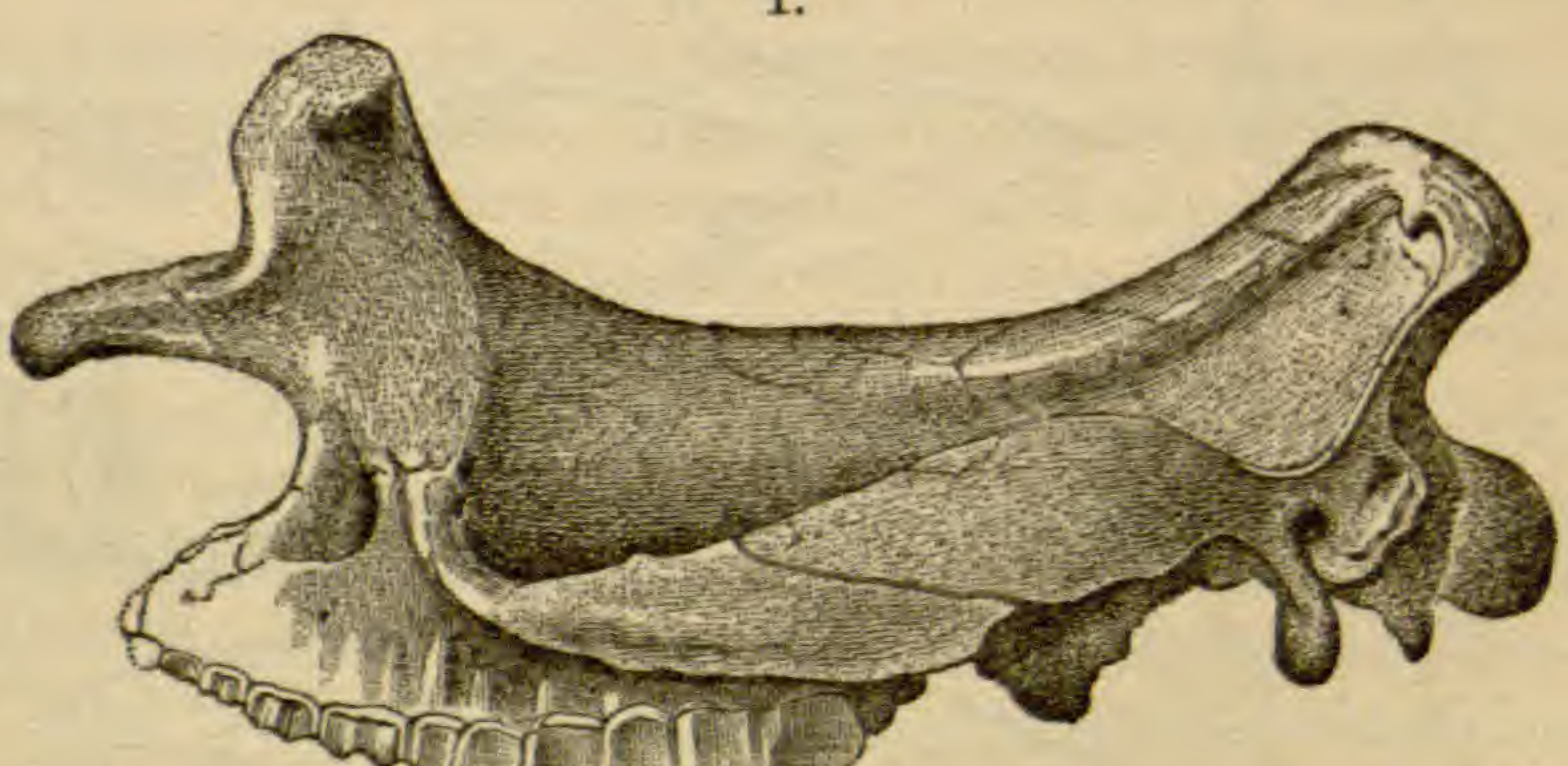


Figure 1. Skull of *Brontotherium ingens* Marsh. Side view; one-twelfth natural size.

There is a pair of large horn-cores on the anterior part of the skull, in front of the orbits. They stand on the maxillaries,

* This Journal, vol. v, p. 486. Also vol. vii, p. 81, Jan., 1874, and vol. iv, p. 245, March, 1875.

like the middle pair in *Dinoceras*, the nasals forming only the inner margin of the base. These protuberances are placed transversely, as in modern Artiodactyles, and extend upward and outward. They vary much with age, and probably differed with the sex. There are large air cavities in the base of these horn-cores. The nasal bones are greatly developed, and firmly co-ossified. Their anterior extremities are produced, and overhang the large narial orifice. The premaxillaries are diminutive, and do not usually extend forward so far as the end of the nasals. The infra-orbital foramen is very large. The lachrymal forms the anterior border of the orbit. The latter is small, and continuous with the elongated temporal fossa. There is no postorbital process on the frontal. The zygomatic arches are massive, and much expanded. The malar extends forward beyond the lower margin of the orbit. The zygomatic process of the squamosal is elevated, and more or less incurved above. There is a large postglenoid process, which forms the anterior border of the external auditory meatus. The latter is bounded behind and below by the post-tympanic process of the squamosal. There is a large par-occipital process. The occipital condyles are large, and well separated. Their position indicates that the head was declined when in its natural position. There is a large condylar foramen, and a distinct alisphenoid canal. The palate is deeply excavated, especially in front. The posterior nares extend forward between the last upper molars.

The brain cavity in *Brontotherium* is small, and its form is shown in Plate XI, the figures of which are drawn from a nat-

2.

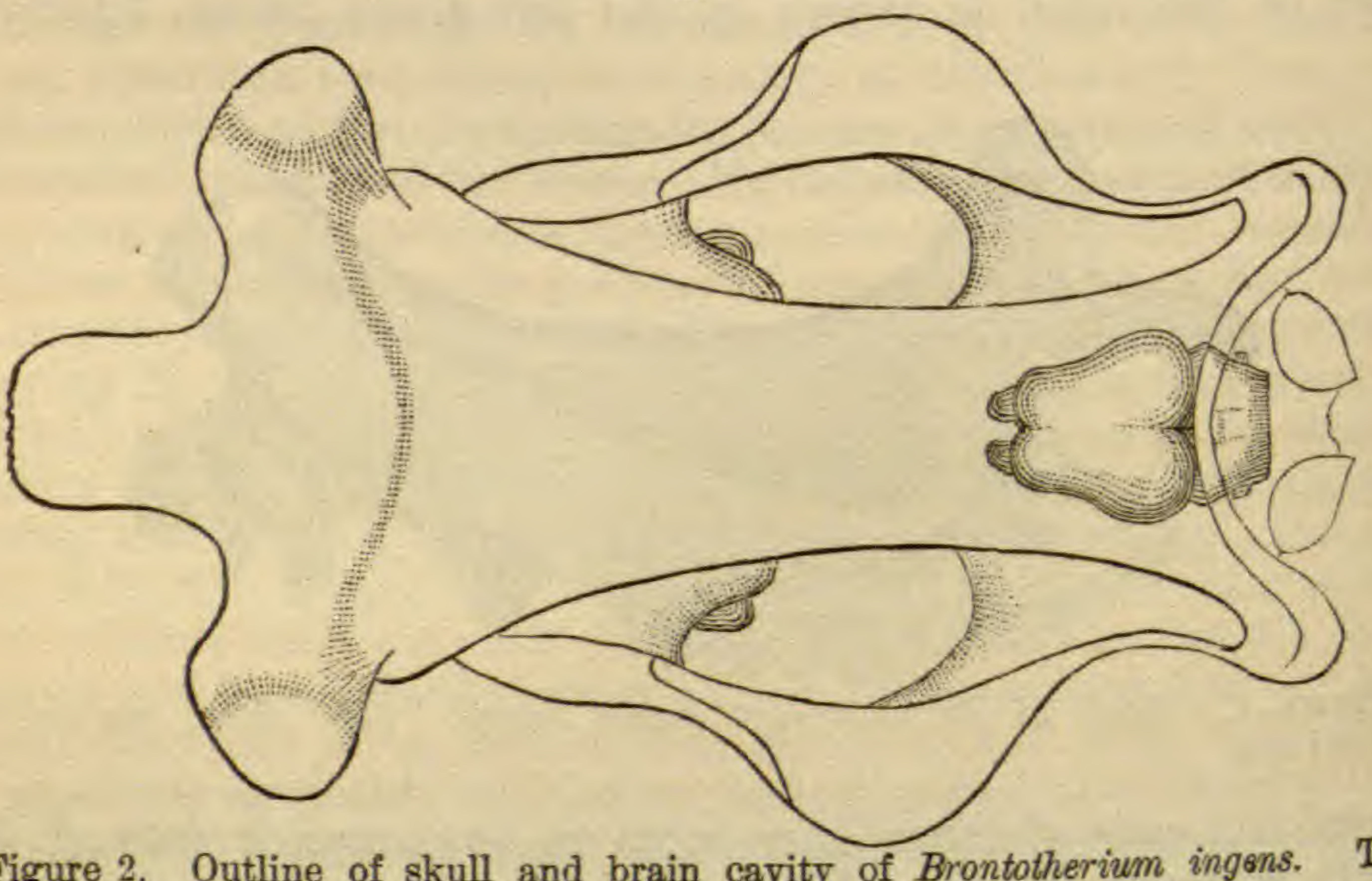


Figure 2. Outline of skull and brain cavity of *Brontotherium ingens*. Top view; one-tenth natural size.

ural cast of the brain-case of *B. ingens* Marsh. The size of the entire brain compared with that of the cranium is shown in the accompanying cut, figure 2.

The cerebral hemispheres did not extend at all over the cerebellum, and little if any over the olfactory lobes. The latter were of moderate size, and separated by a wide osseous septum. The hemispheres were comparatively large, and much convoluted. The Sylvian fissure is well-marked in the cast, and some of the other principal divisions are indicated. The cerebellum was small. There was a rudimentary tentorial ridge. The pituitary fossa is distinctly marked. The foramina for the optic nerves are quite small.

The mandible in *Brontotherium* has a wide condyle, and a slender coronoid process. The angle is rounded, and slightly produced downward. The symphysis is depressed, elongated, very shallow in front, and completely ossified. (Plate XII.)

The dental formula of *Brontotherium* is as follows:—

$$\text{Incisors,}\frac{2}{2};\ \text{canines,}\frac{1}{1};\ \text{premolars,}\frac{4}{3};\ \text{molars,}\frac{3}{3}\times 2=38.$$

The upper incisors are quite small. (Plate X.) The canine is short and stout, and placed close to the first premolar. The upper premolars have all essentially the same structure, viz: two external connate cusps, with their outer faces nearly plane, and two inner cones closely united. The anterior cone is connected with the opposite outer cusp by a transverse ridge, which has behind it an elongated depression, more or less divided by projections from the outer posterior cusp. In the upper true molars, the external cusps have their outer surfaces deeply concave, while the inner cones are low and separate. The lower incisors were small, and evidently of little use. The two next the symphysis were separated from each other. The lower incisors are not unfrequently wanting, and in old animals the alveoli may, perhaps, disappear. Careful examination, however, will usually show indications of them. The lower canine is of moderate size, and separated from the premolars by a short diastema. The lower molars are of the *Palæotherium* type, and agree essentially with those of *Menodus*.

The neck in *Brontotherium* was stout, and of moderate length. The cervical and most of the dorsal vertebræ are distinctly opisthocœlous. The atlas is large, and much expanded transversely. The axis is massive, and has its anterior articular faces much broader than in the *Dinocerata*. The odontoid process was stout and conical. The posterior articular face is concave, and oblique. The transverse processes apparently had no foramen for the vertebral artery. The epiphyses of the vertebræ are loosely united in most specimens, as in the Proboscidians. The lumbars are slender, and smaller than the dorsals. There are four vertebræ in the sacrum. The caudal vertebræ indicate a long and slender tail.

The limbs of the *Brontotheridæ* were intermediate in proportion between those of the Elephant and the Rhinoceros. The scapula is large, with a prominent spine and small coracoid process. The humerus is stout, and its great tuberosity extends above the head. The radial crest is prominent, and the entire distal end is occupied by the articulation. The olecranon cavity is shallow, and the condylar ridge similar to that of the Elephant, but not continued so far up the shaft. The radius and ulna are separate. The ulna has its olecranon portion much compressed. Its distal end is much smaller than in *Rhinoceros*, and has no articular face for the lunar. The radius is stout, and its distal end expanded. The carpal bones form interlocking series. They are shorter than in *Rhinoceros*, and support four well developed toes of nearly equal size. (Plate XIII, figure 2.) The metacarpal bones are shorter than those of *Rhinoceros*, the first phalanges longer, and the second series shorter. All the toes had "navicular" sesamoid bones, similar to that on the coronary bone of the horse. The ungual phalanges are short and tubercular, as in the *Dinocerata* and *Proboscidea*.

The pelvis is much expanded transversely. The femur has a small third trochanter, and its head a deep pit for the round ligament. At the distal end, the anterior articular surface is narrow, and the two edges are of nearly equal prominence, as in the Tapir. The patella is elongate, and has a strong vertical keel on its articular face. The tibia is stout, and has a distinct spine. The fibula is separate and entire, but quite slender. The calcaneum is much elongated. The astragalus is shorter than in the Rhinoceros, and the superior groove more oblique. The cuboid face is larger than in *Rhinoceros*. The navicular has its distal facets subequal. There were three toes of nearly equal size in the pes, the first and fifth being entirely wanting. (Plate XII, figure 1.) None of the bones of the skeleton are hollow.

There appear to be four well marked genera in the *Brontotheridae*, now known, which may be distinguished as follows:

1. *Menodus* Pomel.* (*Titanotherium* Leidy, 1852.)

$$\text{Dentition} = \text{Incisors}\frac{2}{2};\ \text{canines}\frac{1}{1};\ \text{premolars}\frac{4}{4?};\ \text{molars}\frac{3}{3}.$$

Diastema behind upper canines. Basal ridge on inner side of upper premolars not continuous. Nasals short. A postorbital process. Third trochanter rudimentary or wanting. Type *M. Proutii*.

2. *Megacerops* Leidy. (*Megaceratops* Cope), (*Symborodon* Cope in part.)

$$\text{Dentition} = \text{Incisors}\frac{2}{0};\ \text{canines}\frac{1}{1};\ \text{premolars}\frac{4}{3};\ \text{molars}\frac{3}{3}.$$

* Bib. Univ. de Genève, x, p. 75, Jan., 1849.

Diastema behind upper canines. Inner basal ridge on upper premolars not continuous. Nasals more elongated. A postorbital process. Third trochanter rudimentary or wanting. Type *Megacerops Coloradensis* Leidy.

3. *Brontotherium* Marsh, (*Symborodon* Cope, in part.) (*Miobasileus* Cope.)

Dentition =Incisors $\frac{2}{2}$; canines $\frac{1}{1}$; premolars $\frac{4}{3}$; molars $\frac{3}{3}$.

No superior diastema. Strong continuous basal ridge on inner side of upper premolars. No postorbital process. Third trochanter distinct. Type *B. gigas* Marsh.

4. *Diconodon* Marsh (*Anisacodon*).

Dentition =Incisors $\frac{0}{1}$; canines $\frac{1}{1}$; premolars $\frac{4}{3}$; molars $\frac{3}{3}$.

No superior diastema. Strong inner basal ridge on upper premolars. Last upper molar with two inner cones. No postorital process. Type *D. montanus* Marsh.

In the dentition and skeleton, the *Brontotheridæ* more nearly resemble the Eocene *Diplacodon*, than any other American genus, and they may yet prove to be nearly related. The animals of that genus were of much smaller size, and entirely without horns. The relations of the *Brontotheridæ* to the genus *Chalicotherium* Kaup, cannot at present be determined.

In comparing the *Brontotheridæ* with the equally gigantic *Dinocerata* of the Eocene, several striking points of resemblance will be at once noticed; especially the presence of horn-cores in transverse pairs; the general structure of the limbs; and the short and thick toes. The differences, however, between these two groups are still more marked. In the *Brontotheridæ* there is but a single pair of horn-cores, and no crest around the vertex. The structure and number of the teeth are quite different, while the small canines and huge molars contrast strongly with the elongated canine tusks and diminutive molars of the *Dinocerata*. The latter, moreover, have two very large dependent processes on each ramus of the mandible; the cervical vertebræ flat; the femur without a third trochanter; and at least an additional toe in each foot.

Among the features which this group shares with the *Proboscidea* may be mentioned: the superior extension of the condylar ridge of the humerus; the short thick toes; and the late union of the epiphyses with the centra of the vertebræ. The last character appears to belong especially to mammals of very large size, and probably indicates late maturity, and great longevity.

The *Brontotheridæ* nearly equaled the Elephant in size, but the limbs were shorter. The nose was probably flexible, as in the Tapir, but there was evidently no true proboscis.

All the known remains of the *Brontotheridæ* are from east of the Rocky Mountains, in the Miocene beds of Dakota, Nebraska, Wyoming, and Colorado.

Yale College, New Haven, March 16, 1876.

EXPLANATION OF PLATES.

Plate X—*Brontotherium ingens* Marsh. Superior premolar and molar teeth; bottom view. One-third natural size.

Plate XI—*Brontotherium ingens.* Cast of brain cavity. Figure 1, top view; figure 2, side view. One-half natural size.

Plate XII—*Brontotherium gigas* Marsh. Lower jaw. Figure 1, top view; figure 2, front view; figure 3, side view. One sixth natural size.

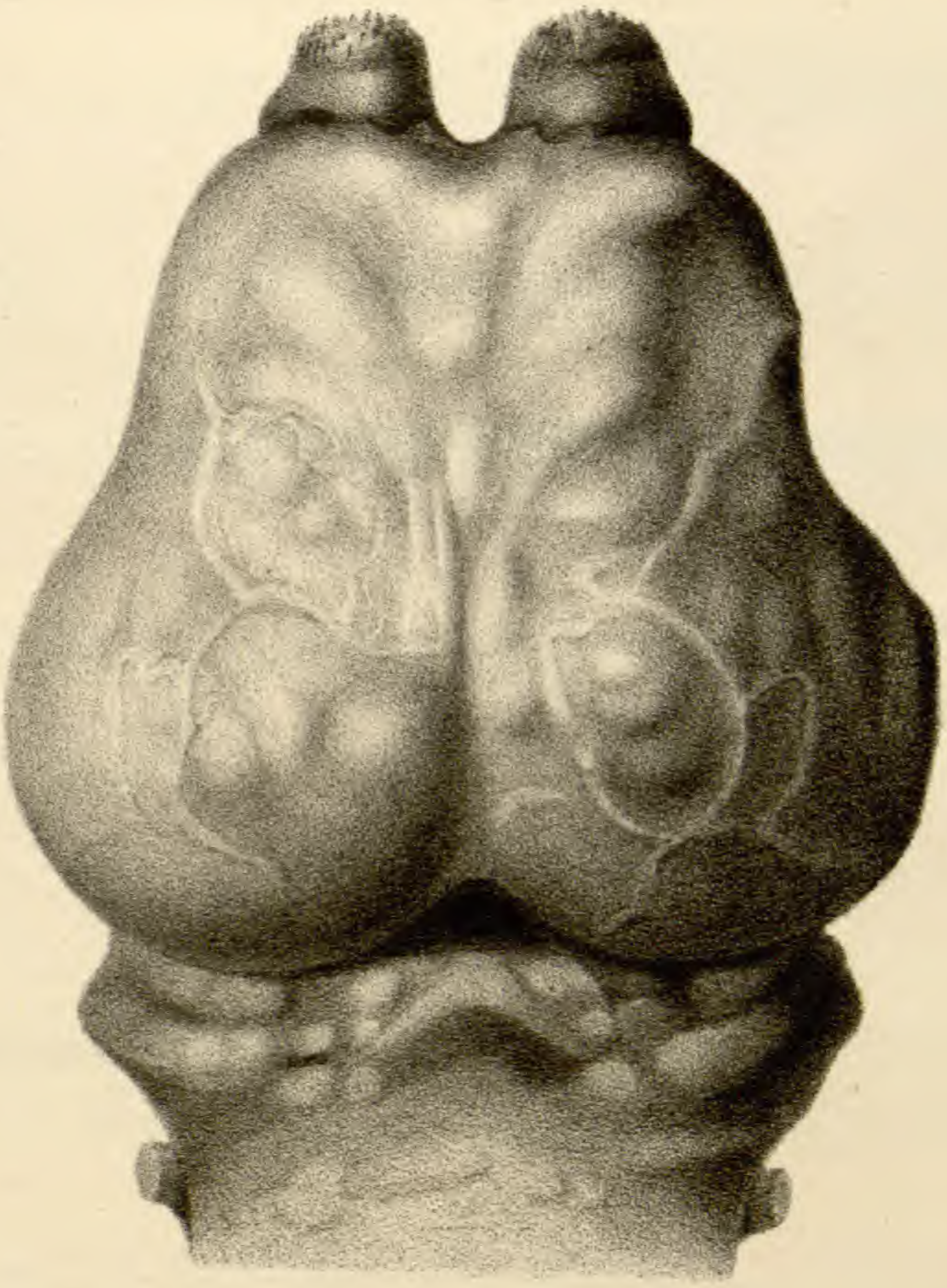
Plate XIII—*Brontotherium.* Figure 1, hind foot; figure 2, fore foot. One-sixth natural size.

Drawn from nature by E. Crisand.

Punderson & Crisand New Haven, Ct

BRONTOTHERIUM INGENS. Marsh 1/3.

1

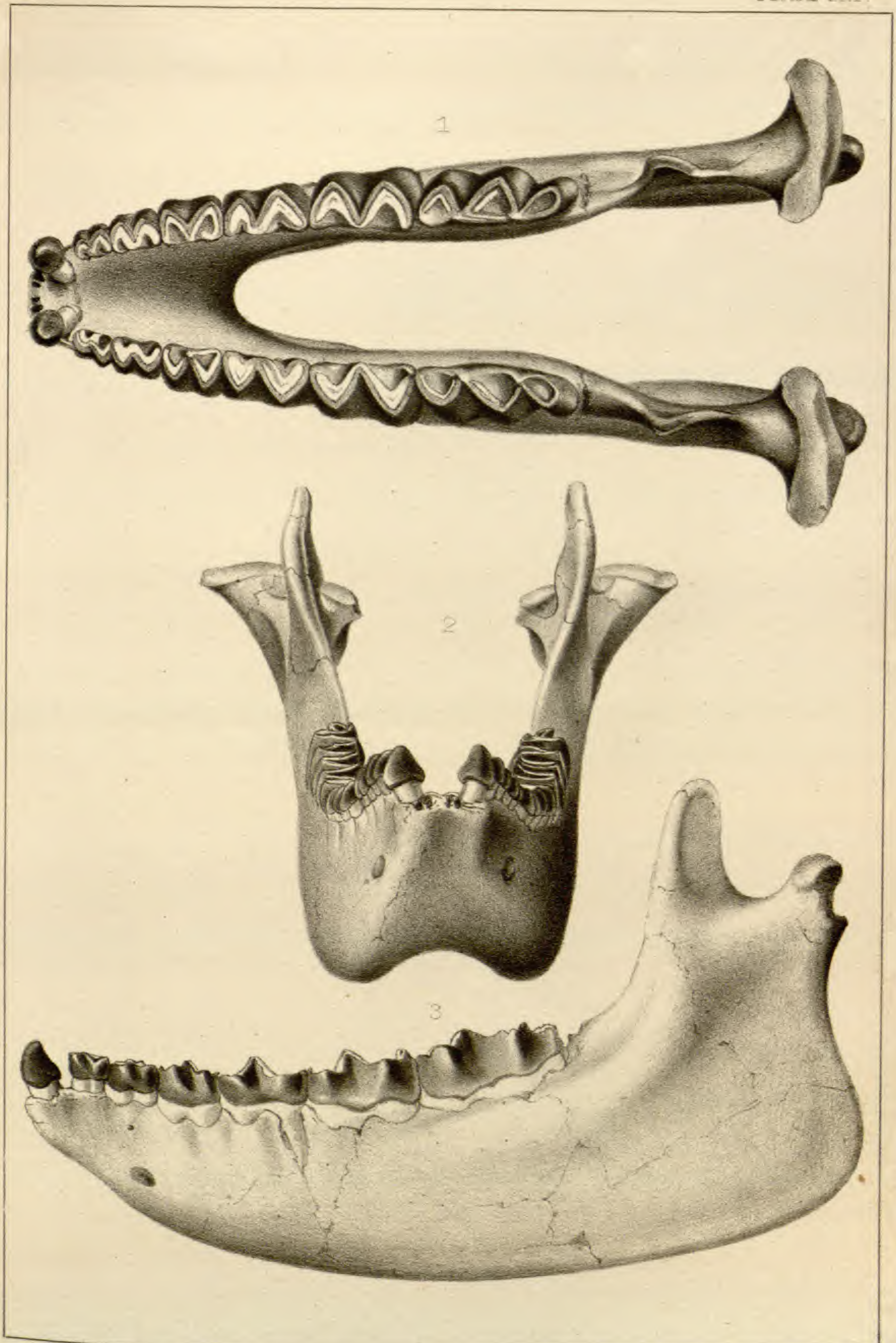


2

Drawn from nature by E. Crisand.

Punderson & Crisand New Haven, Ct.

BRONTOTHERIUM INGENS. Marsh. ½

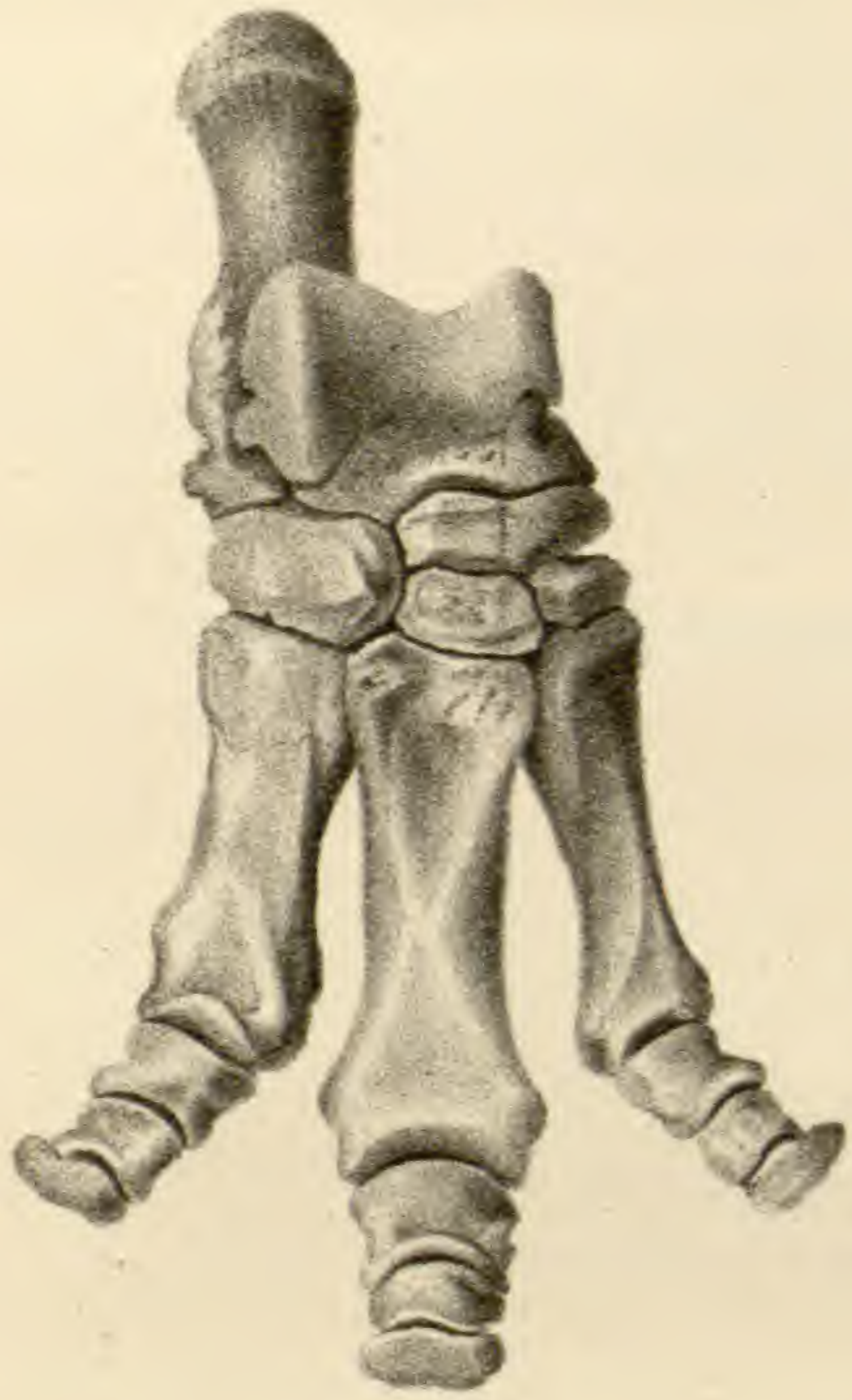


Drawn from nature by E. Crisand.

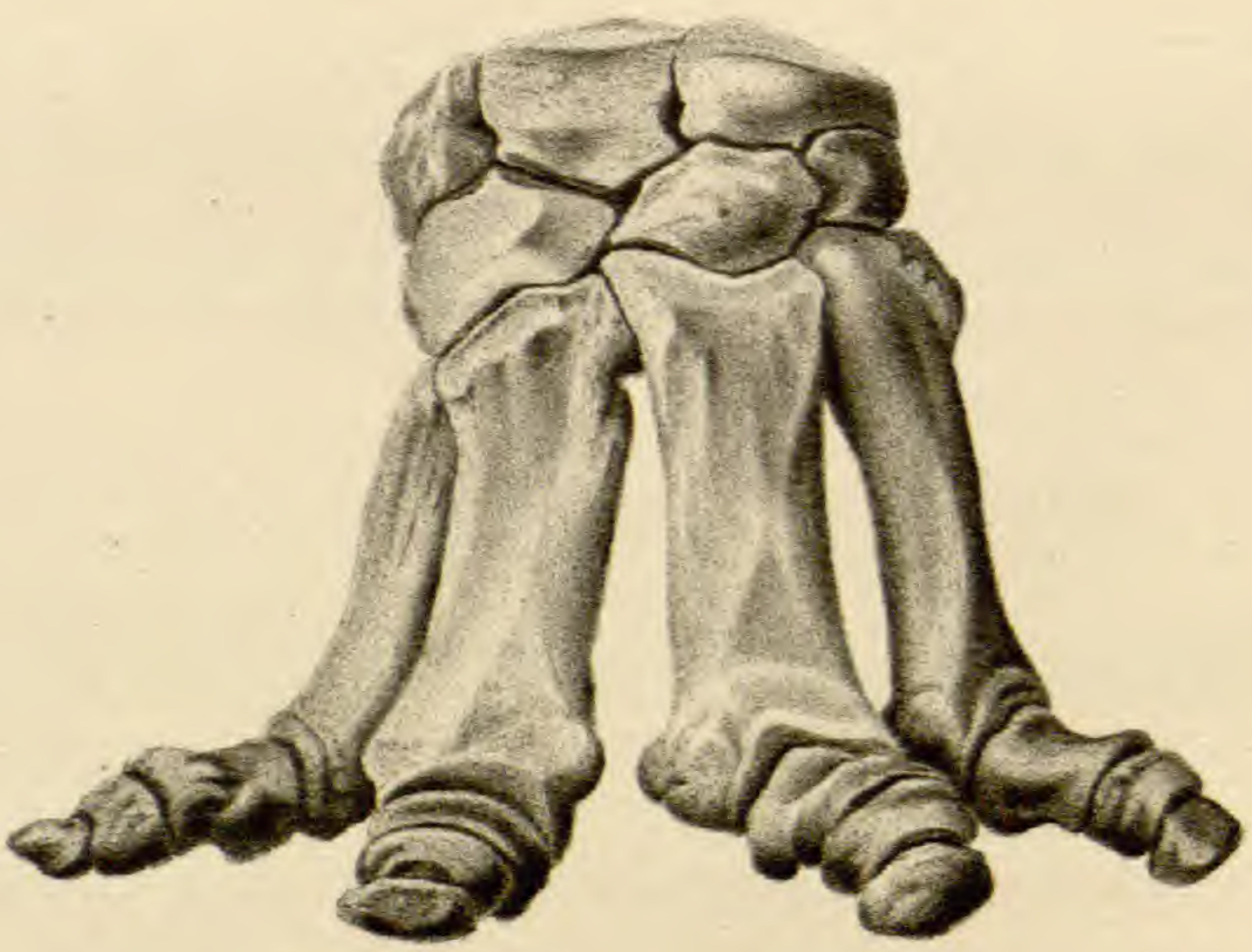
Punderson & Crisand New Haven, Ct.

BRONTOTHERIUM GIGAS. Marsh 1/6.

1



2



Drawn from nature by E. Crisand

Punderson & Crisand New Haven, Ct

BRONTOTHERIUM ½

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XLII.—*On supposed changes in the Nebula* M. 17 = h. 2008 = G. C. 4403. (R. A. $18^h\ 12^m\ 33^s.1$; N. P. D. 106° 13′ 36″; 1860·0); by EDWARD S. HOLDEN.*

I. *Historical Notes—Observations*:—This nebula was discovered by Messier and is number 17 of his list (*Connaissance des Tems* 1784). It has been carefully studied since 1800, by Sir John Herschel (1833–37), Lamont (1837), Mason (1839), Lassell (1862), Huggins (1865), Trouvelot (1875), and Trouvelot and myself (1875). These observations, so far as they are published, are to be found in the following works:—

HERSCHEL: Observations of Nebulæ, etc., made at Slough; *Phil. Trans.*, 1833, p. 498 and Plate XII, fig. 35.
HERSCHEL: *Results of Astronomical Observations at the Cape of Good Hope*, p. 8 and Plate II, fig. 1.
LAMONT: *Ueber die Nebelflecken*, 1837, fig. X.
LAMONT: *Annalen der K. Sternwarte bei München*, band xvii, p. 332 and fig. 21, Plate VIII.
MASON: *Transactions American Phil. Soc.*, vol. vii, 1840, p. 165, Plate VI.
LASSELL: *Mem. R. A. S.*, vol. xxxvi; Plates VII, VIII. figs. 33, 33A.
HUGGINS: *Philosophical Transactions* 1866, p. 385.

The later observations are unpublished.

I extract from these various authorities such portions as will be of use for subsequent reference.

From Herschel's paper (*Phil. Trans.*, 1833):—

"The figure of this nebula is nearly that of a Greek capital omega, *Ω*, somewhat distorted, and very unequally bright. Messier perceived only the bright [eastern] branch of the nebula now in question, without any of the attached convolutions which were first noticed by my father. The chief peculiarities which I

* This article has in part appeared in the Popular Science Monthly; and this Journal is indebted to Messrs. Appleton & Co. for all but one of its excellent illustrations.

have observed it it are—1. The resolvable knot in the [eastern] portion of the bright branch, which is in a considerable degree, insulated from the surrounding nebula; strongly suggesting the idea of an absorption of the nebulous matter; and, 2. The much feebler and smaller knot at the [northwestern] end of the same branch, where the nebula makes a sudden bend at an acute angle. With a view to a more exact representation of this curious nebula, I have at different times, taken micrometrical measures of the relative places of the stars in and near it, by which, when laid down as in a chart, its limits may be traced and identified, as I hope soon to have better opportunity to do than its low situation in this latitude will permit."

S.

W.  E.

N.

Fig. 1.* HERSCHEL 1833.

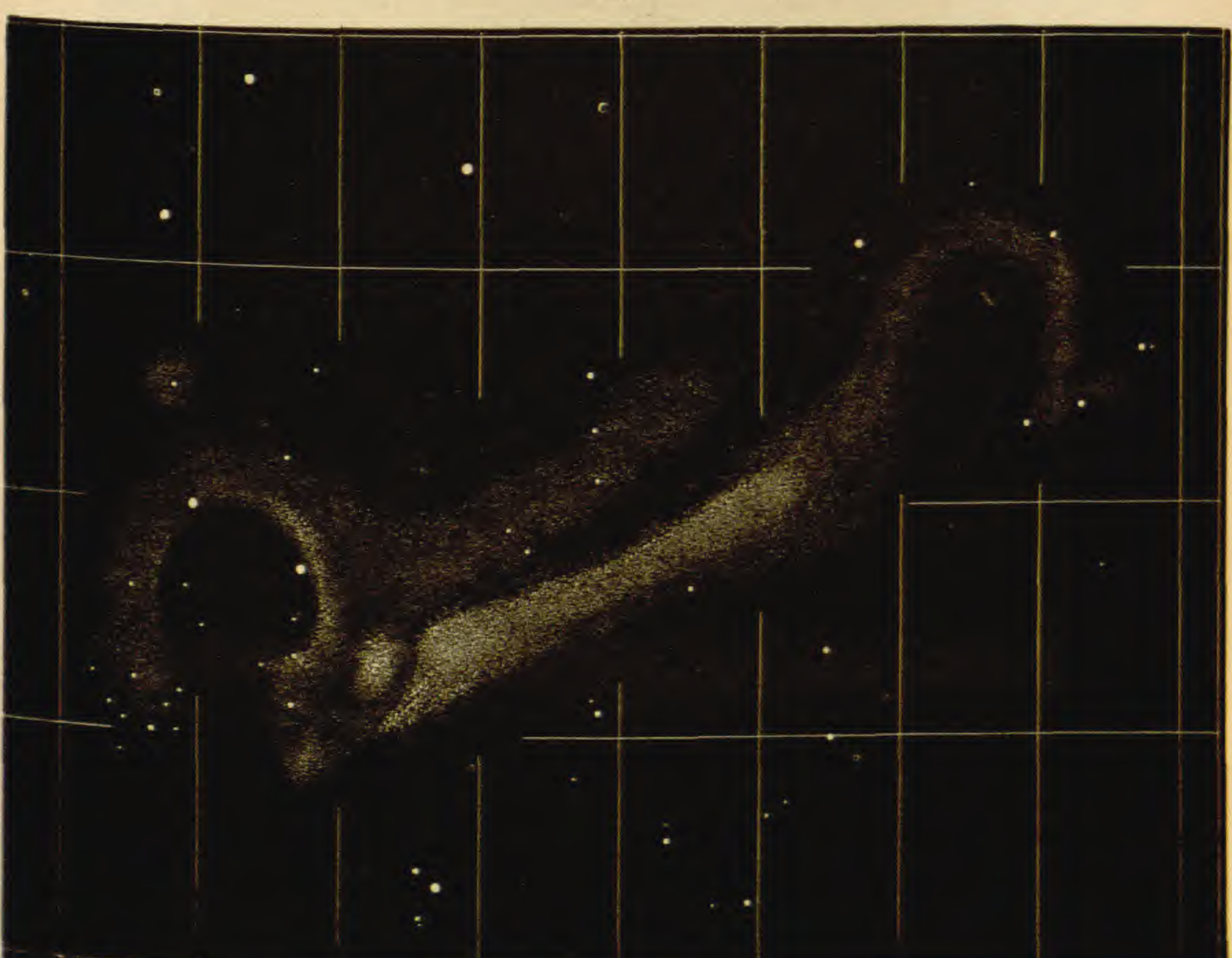
From *Ast. Obs. at the Cape of Good Hope*:—After explaining that his first figure is far from accurate Herschel says:—

"In particular the large horseshoe shaped arc . . . is there represented as too much elongated in a vertical direction and as bearing altogether too large a proportion to [the eastern] streak and to the total magnitude of the object. The nebulous diffusion, too, at the [western] end of that arc, forming the [western] angle and base-line of the capital Greek omega (Ω), to which the general figure of the nebula has been likened, is now so little conspicuous as to induce a suspicion that some real change may have taken place in the relative brightness of this portion compared with the rest of the nebula; seeing that a figure of it made on June 25, 1837, expresses no such diffusion, but represents the arc as breaking off before it even attains fully to the group of small stars at

* For the use of the cuts which are given with this article, I am indebted to the courtesy of Dr. Youmans, Editor of the Popular Science Monthly.

the [western] angle of the Omega. . . . Under these circumstances the arguments for a real change in the nebula might seem to have considerable weight. Nevertheless, they are weakened or destroyed by a contrary testimony entitled to much reliance. Mr. Mason, a young and ardent astronomer, ... whose premature death is the more to be regretted, as he was, so far as I am aware, the only other recent observer who has given himself with the assiduity which the subject requires to the exact delineation of nebulæ, and whose figures I find at all satisfactory, expressly states that *both* the nebulous knots were well seen by himself and his coadjutor Mr. Smith on August 1, 1839, i. e., two years subsequent to the date of my last drawing. Neither Mr. Mason, however, nor any other observer, appears to have had the least suspicion of the existence of the fainter horseshoe arc attached to the [eastern] extremity of Messier's streak. Dr. Lamont has given a figure of this nebula, accompanied by a description. In this figure [our Fig. 3], the nebulous diffusion at the [western] angle and along the [western] base line of the Omega is represented as very conspicuous; indeed, much more so than I can persuade myself it was his intention it should appear."

S.



N.

Fig. 2. HERSCHEL 1837.

Herschel's Star-positions obtained in 1837 are given in Table II.

From Lamont's observations, *band* xvii, *Annalen der K. Sternwarte bei München*, p. 332 *et seq.*, I extract the following:—with regard to Sir John Herschel's drawing in the Cape of Good Hope Observations he says " . . . . bei mehreren Sternen scheint . . . . die Abweichung so gross, dass erst durch künftige Beobachtung entschieden werden muss, ob Aenderungen vorkommen, oder ob die Unterschiede in den Messungen ihren Grund haben."

S.

W.  E.

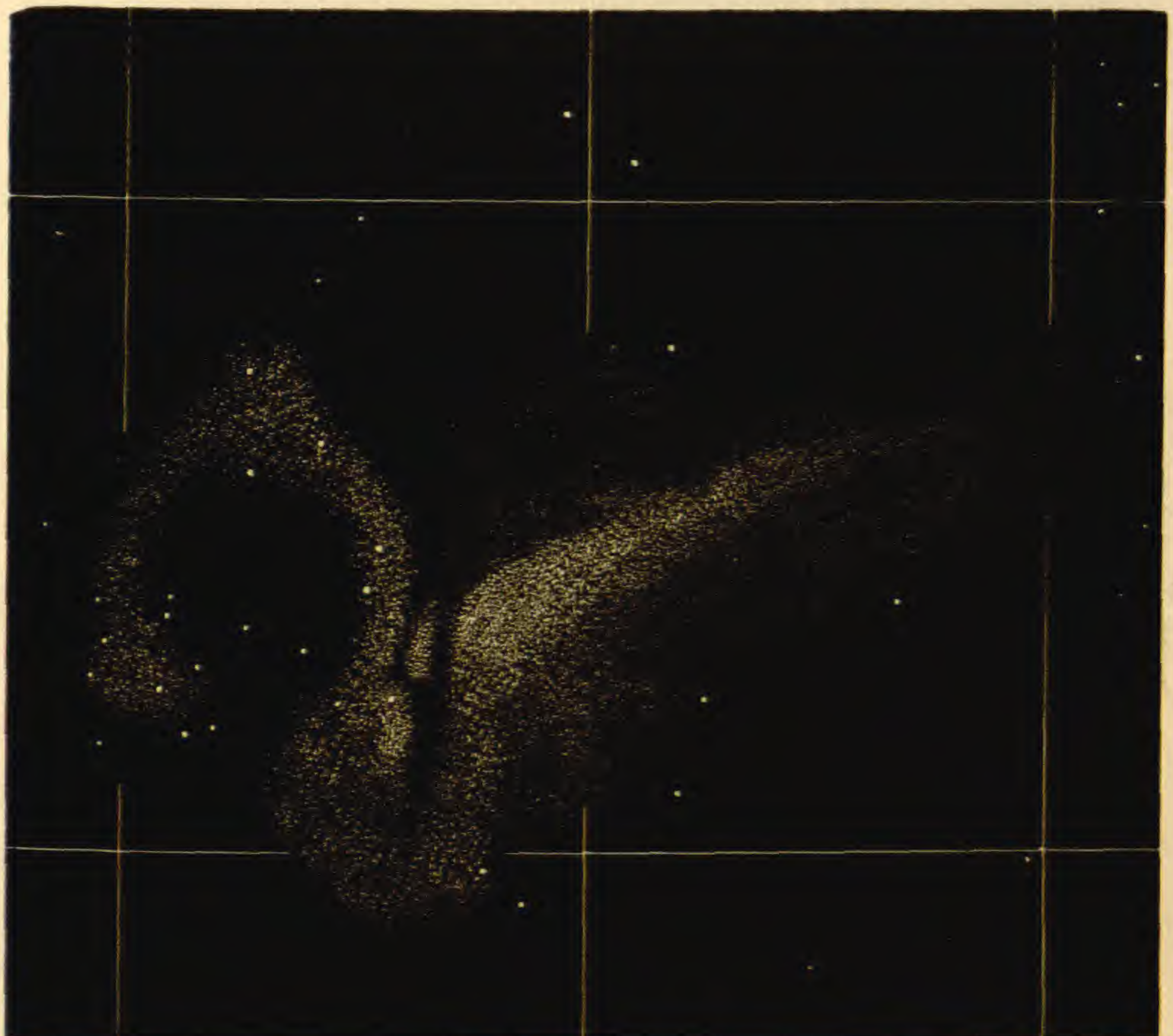
N.

Fig. 3. LAMONT 1837.

Lamont's measures are mostly of position angle, and all of these which are directly comparable with Lassell's measures I have placed in Table III. We find two important notes on the physical aspects of the nebula as follows:—"Im Nebel kommt ein Knoten vor, den Sir J. Herschel als auflösbar betrachtet, während ich von Auflösbarkeit keine Andeutung bemerken konnte. Die Länge des Nebels-Knotens schätzte ich = Distanz (2) – (28) [Lassell's nomenclature] und die Breite=½ Distanz (2)—(7). An 2 August [1837] fiel mir eine Stelle zwischen (2) und (40) auf, wo ich einen verschwindend kleinen Stern oder ein Häuflein solcher Sterne wahrzunehmen glaubte."

In regard to the credence due to Lamont's sketch the following sentence is important (*op. cit.* p. 305): "Was die . . . beigefügten Zeichnungen betrifft, so sind sie nur als Skizzen zu betrachten, welche blos den Zweck haben, die Messungsresultate verständlich zu machen."

S.

W.  E.

N.

Fig. 4. MASON and SMITH 1839.

From Mason's paper above cited :—

"*Things certain* :—1. The 'resolvable knot' mentioned by Herschel is isolated or nearly so, from the rest of the nebula.

2. The smaller knot is apparently not affected with this peculiarity.

3. Of the faint bend or loop [horseshoe] the *following* half is brighter than the *preceding*.

4. The bright branch fades away gradually to the [east]; it is convex [towards the south.]

5. The external angle of the nebula stretches down from the star [8] (of Lassell's nomenclature; see our sketch map, figure

8) towards the *n. p.* much farther than in Herschel's drawing." (our fig. 1.)

"*Nearly certain:*—1. The bright branch is more definitely bounded on its southern side than upon the northern. [??]

2. The 'resolvable knot' of Herschel has either a second nucleus or involves a faint star in its [south] margin."

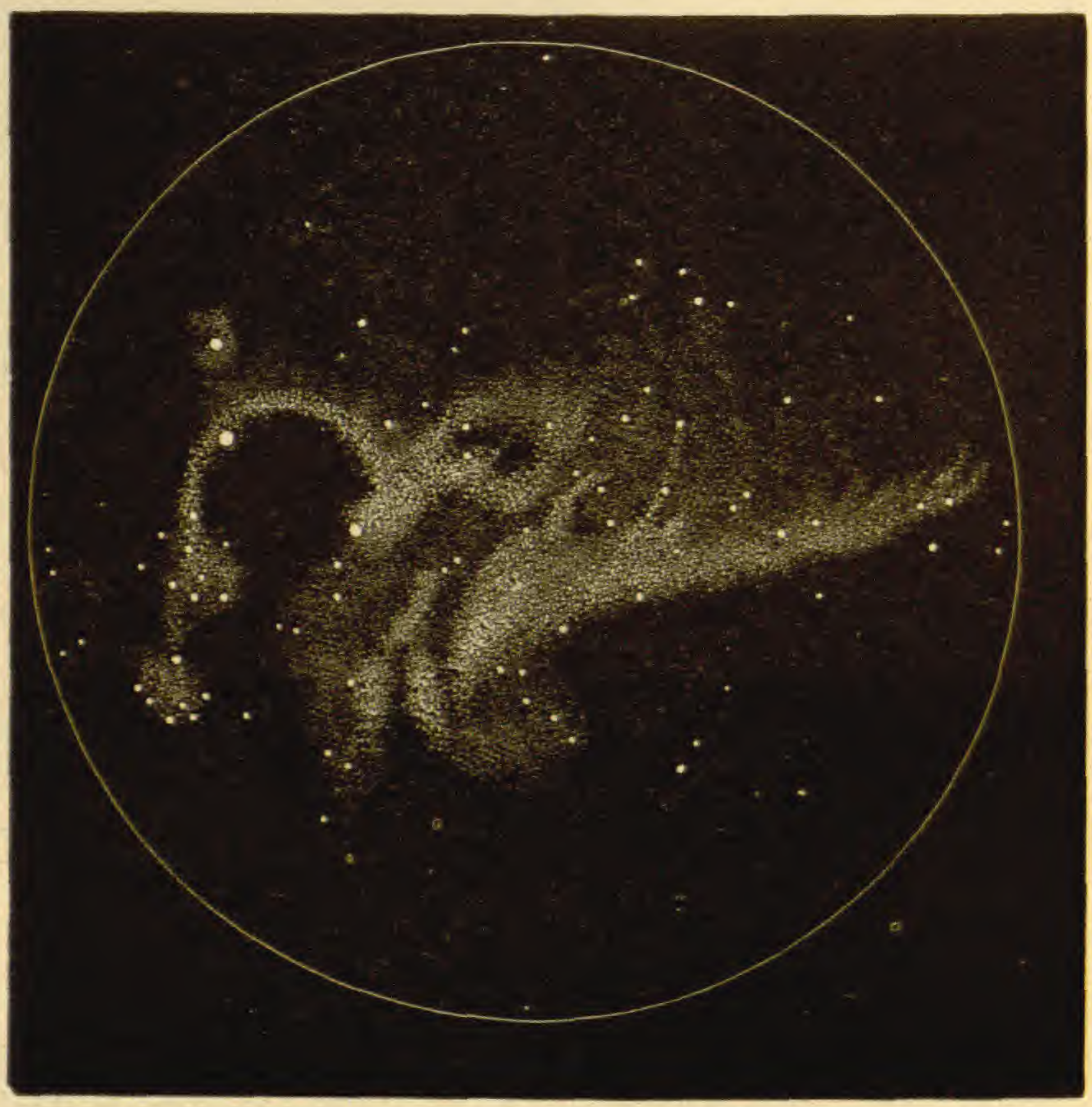
"*Strongly suspected:*—5. Just [south] of star [2] is a portion a little brighter than the rest of the bend."

Mason's star positions are given in Table I.

Lassell gives no description of the nebula, and we extract only his measures of the co-ordinates of eleven of the brightest stars, which are contained in Table IV, and which we shall use as standard positions to which all others are to be referred.

S.

W.



E.

N.

Fig. 5. LASSELL 1862.

We give Lassell's figure above, remarking that it was constructed, as indeed all the preceding ones have been, by first measuring the relative position of the brighter stars, then inserting by careful eye-estimates the fainter ones, and finally by drawing among these stars, guided by their configurations, the details of the nebula itself.

In Huggins' paper already cited (p. 385) we find the following:—

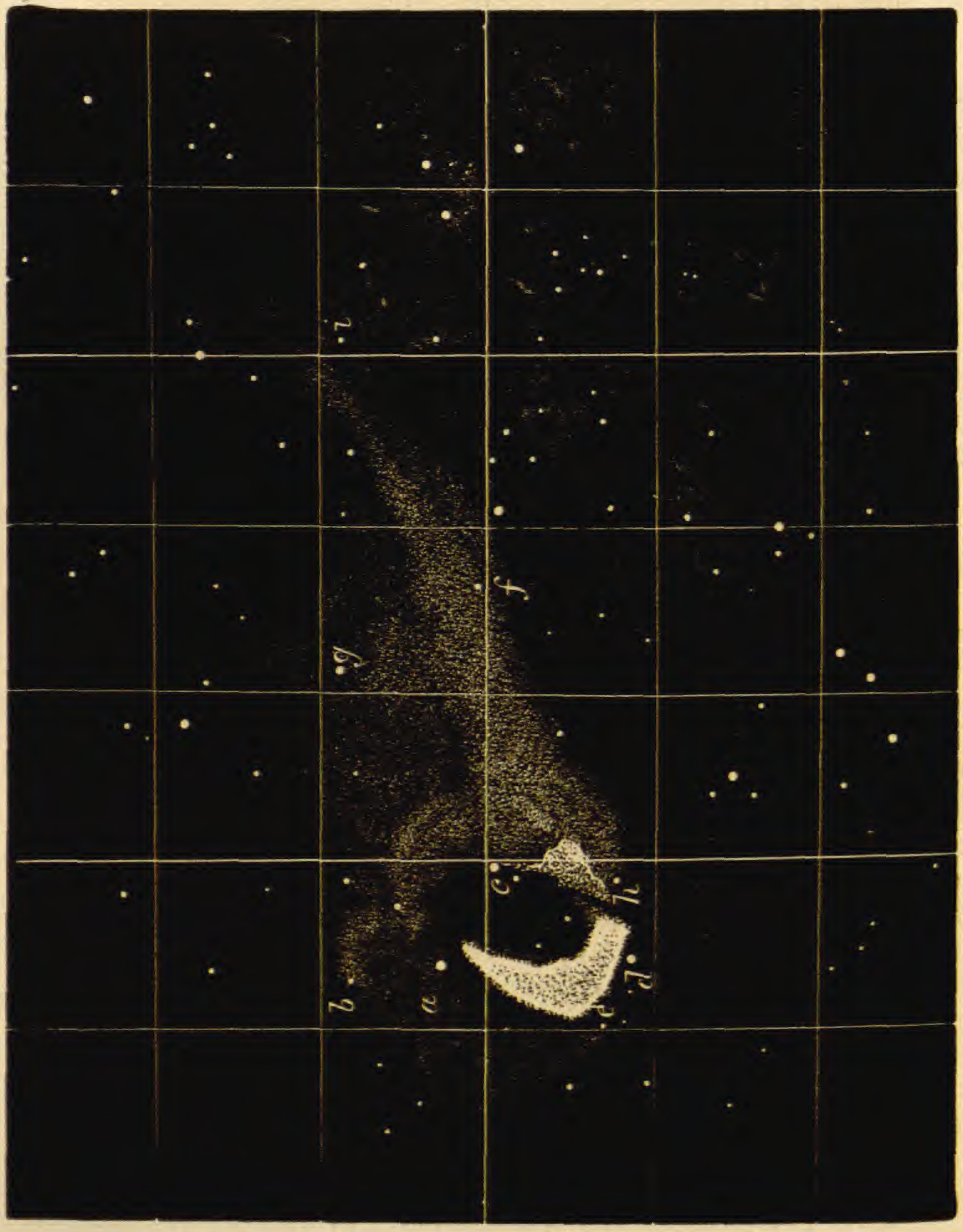
"Lord Oxmantown informs me that in the observations of this nebula at Birr Castle, there is no mention of resolvability; and that 'the central part to the right [east?] of star α [No. 2?] consists of bunches or patches of bright nebulosity with fainter nebulosity intervening.' The spectrum of this nebula indicates that it possesses a gaseous constitution.

One bright line only was seen, occupying in the spectrum apparently the same position as the brightest of the lines of nitrogen. When the slit was made as narrow as the intensity of the light would permit, this bright line was not so well defined as the corresponding line in some of the other nebulæ under similar conditions of the slit, but remained nebulous at the edges.

When the brightest portion of the nebula containing the nucleus or 'bright knot' was brought upon the slit, in addition to the bright line a faint narrow continuous spectrum was seen. The bright knot appeared in my telescope smaller and more condensed than it is represented in the drawings of Sir John Herschel."

A very rapid method of drawing nebulæ, is the following: it yields to the first in the accuracy of the positions of the stars, but it is probably even superior to it in facilities for the correct representation of the nebula and stars considered as one mass. A piece of glass ruled carefully into squares (see figs. 6 and 7) and this is placed in the focus of the eyepiece so as to be plainly visible; the telescope is then directed upon the nebula, and a clock-work motion is applied to the telescope so that it follows the nebula accurately. Some one of the brighter stars is chosen, and it is kept by means of the clock-work accurately in the corner of one of the squares. A piece of paper ruled into squares similar to those of the glass reticle is provided, and on it the observer dots down the various stars in and about the nebula. This may take two, three or four nights, according to circumstances, but in all cases it requires much less time than the micrometric measurements of the brighter stars and the troublesome allineations required to fix the positions of the smaller stars and it has the great advantage that the work can be done in a perfectly dark field of view, whereas the micrometric measures demand the use of illuminated wires at least. After the stars are inserted, the principal lines of the nebula are put in, not only by the star groups, but also by the squares themselves. For my own use I have had constructed two reticles: one ruled in squares like those seen in figs. 6 and 7, and another in which the heavy-lined large squares (each containing nine small squares, see fig. 6) are still present, but are subdivided into small squares by lines parallel to their own diagonals. After making all the use possible of the first reticle, the second is put in, and an entirely new set of reference-lines is obtained, making an angle of 45° with the old set. This, of course, could be equally obtained by revolving the first reticle through an angle of 45°, but it is not quite so convenient.

E.

S.  N.

W.

Fig. 6. TROUVELOT 1875.

After the stars and the principal lines of the nebula are inserted a new and higher power eye-piece is used, and the drawing is concluded by means of this. Fig. 6 is an example of a drawing of the Horseshoe Nebula made in this way by M. Trouvelot, of Cambridge. Massachusetts, with his 6½ inch refractor.*

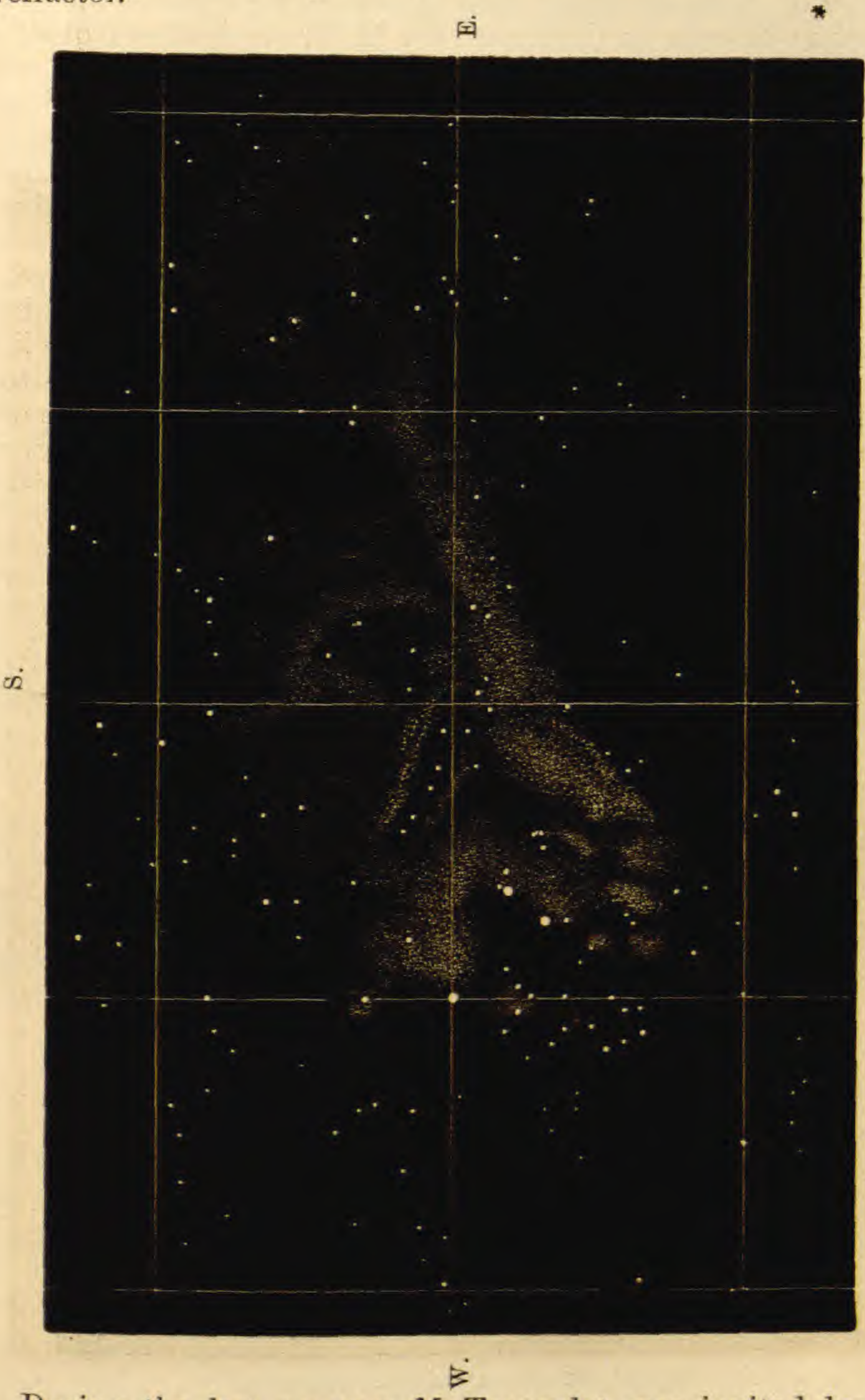


Fig. 7. Trouvelot and Holden 1875.

During the last summer M. Trouvelot was invited by the Superintendent of the United States Naval Observatory to visit

* That portion of the nebulosity within the horseshoe has been made far too bright by the engraver in this figure, but the shape is properly kept.

Washington for the purpose of making drawings of nebulæ, etc., by means of the twenty-six-inch Clark refractor. By the courtesy of Admiral Davis I am able to give a drawing of the Horseshoe Nebula as delineated by M. Trouvelot from observations made jointly by him and myself.

Pretty much the same method was adopted in this drawing as in fig. 6, but the vastly more complex structure of the nebula itself is what might have been expected from an increase of eighteen times in the light, over M. Trouvelot's six-inch telescope.

It may be said of the drawing from which fig. 7 was copied, that nothing is there laid down about which the slightest doubt is entertained; and although, in some respects, it was made in greater haste than is desirable, yet it is sufficiently accurate to found an argument on, for or against variation in the shape of any of the *brighter portions* of the nebula. The fainter portions of fig. 7 are too well defined and too bold, but it is, in general, a good representation.

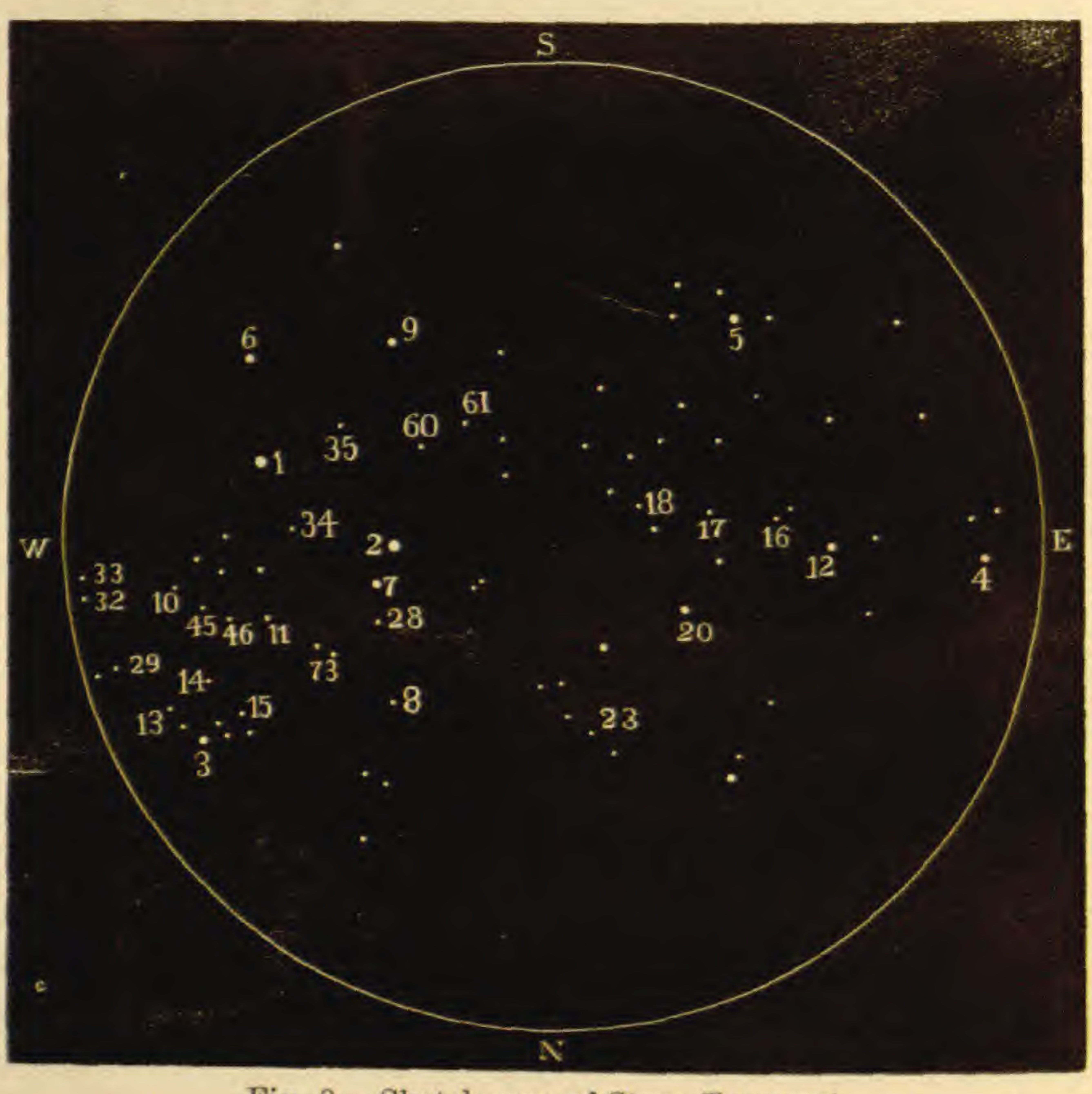


Fig. 8. Sketch-map of Stars (Lassell).

For the purpose of studying this and other nebulæ, I have had the original drawings photographed *on a uniform scale and in such a way as to cause all the photographic prints to represent* *the nebulæ as they would appear in a refractor.* It is only in this way that several drawings, made both by reflectors and refractors can be satisfactorily and minutely compared. The cuts of the present paper were reproduced from such photographic prints, and they are on a scale of one inch = 266″·2; in the present paper, however, all *conclusions* are drawn from an examination of the original engravings, although references are made to the cuts for convenience.

My own notes on the physical aspect of this nebula are as follows:—

"The brighter portions of this nebula give evidence of resolvability. In particular all the brighter parts of the "horseshoe," and of the long branch extending from star 8 to star 71, seem under intense looking, to be just ready to break into small stars. Many small points of light were put down both by myself and by M. Trouvelot, but it was soon found that such a task was endless and of far less importance than the correct delineation of the nebulosity. For nearly all the details of the drawing M. Trouvelot is responsible, as time did not allow of that careful and independent comparison which it is desirable should always be made; but it may be said briefly, that there are no conjectures laid down in the original. Everything is as it was seen. I confined my attention principally to the space limited by stars, 1, 6, 9, 61, 63, 70, 20, 8, 3, 10, on account of the evidence from older drawings that this portion has moved relatively to the stars; and I can vouch for its general accuracy.

This portion is sufficiently accurate to found an argument for or against variability upon. For example. I am sure that the brightest mass of nebulosity *follows* star No. 1, as M. Trouvelot has drawn it, and I am sure that the general direction of the dark channels between stars 7 and 53 is correct in the drawing."—[Sept. 21 to Oct. 2, 1875.]

Trouvelot's star co-ordinates were obtained graphically from the original sketch and are given in Table IV.

II. *Comparison of Star-positions.*

The only relative star-places completely determined which are of the highest accuracy are those of Lassell. "Eleven of the principal stars were measured and the remainder laid down by careful estimation, while some approximation to their proportionate magnitudes is preserved in the drawing. The numbers are generally, but not uniformly, in the descending order of magnitude." The star-positions of Lamont are of high accuracy, but unfortunately only angles of position were measured in most cases. Mason's work was done while he was an undergraduate at Yale College, with a telescope of his own construction mounted as an alt-azimuth, and in the intervals of his collegiate duties, and although every endeavor was made to obtain high accuracy, and above all to determine the limits of error, yet his instrumental means were comparatively inadequate. He estimates the average error of his star-positions in this nebula to be about $1^s \cdot 6$ in R. A. and 25″ in δ (*see* pp. 192 and 193 of his memoir already quoted).

Most of Herschel's stars were inserted by allineations. The method chosen by M. Trouvelot and myself has already been noticed. It may be of interest to remark that the mean error of Trouvelot referred to Lassell is about 7″·7 in R. A. and 5″·3 in N. P. D. The larger residual in R. A. is due to imperfect running of the driving clock of the Equatorial. When this clock is in its usual good condition I believe that this error will not exceed 6″, a quantity which is not appreciable in any drawing of a considerable nebula which can be put upon a quarto page.

TABLE I.—MASON'S STAR-POSITIONS.

Lassell's number.	Mason's number.	Mason's magnitude.	Mason's R. A. 1830·0.	Mason's δ 1830·0.	Remarks.
33 ?	1	14·15	18h 10m 28·9s	−16° 13′ 47″	When the identifica-
32 ?	2	14.15	29·3	13 11	tion of one of Mason's
13	4	13	32·9	12 24	stars with one of Las-
10	5	14	33·3	13 51	sell's is at all doubtful a
3	6	12·13	34·74	11 45	question-mark (?) is add-
14	7	15	35·3	12 43	ed in the first column.
15	8	15	36·5	11 50	
6	9	12	38·51	17 15	
11	10	15	38·5	13 23	Other stars can be
1	11	11	38·56	15 44·4	identified but only so
73 ??	12	14·15	42·2	13 5	many are included as
35	13	14	43·3	16 15	will suffice to determine
7	14	12·13	45·32	13 58·9	the limits of the nebu-
28	15	16	46·0	13 30	losity. The same re-
2	16	11	46·84	14 36·0	marks apply to the fol-
8	17	14·15	10 47·3	12 24	lowing tables.
18 ?	22	16	11 1·0	15 21	
23 ?	24	14	6·1	10 57	
5	25	12	6·19	18 17	
17 ?	26	16	7·7	14 58	
20	27	14	8·3	12 15	
16 ?	28	16	11·1	14 31	
12	29	12	18 11 20·3	−16 14 4	

TABLE II.—HERSCHEL'S STAR-POSITIONS.

Lassell's number.	Herschel's number.	Herschel's magnitude.	ΔR.A. from 11 in arc.	ΔN.P.D. from 11 in arc.	Class.	Remarks.
13	2	12	−91″·5	−201″·6	3	In the column "Class" the numbers have the following signification: — 1. denotes that the differences of R. A. and N.P.D. were *measured:* 2. denotes that these differences were derived from allineations from known stars; 3. denotos that these differences were derived from the position of the star as laid down by the eye on the chart. Columns 4 and 5 have been deduced from the corresponding data given by Herschel, Obs. C. G. H. p. 9.
10	4	14	−61·5	− 82·6	2	
3	6	11	−54·0	−232·8	1	
15	9	15	−15·0	−198·0	3	
6	10	11	−13·5	+115·2	1, 2	
1	11	9	0·	0· .	1	
11	13	13	+ 4·5	−122·4	2	
73 ??	15	13	+73·5	−158·4	2	
35	16	12	+96·0	+ 49·2	2	
8	17	13	+109·5	−201·6	2	
7	18	11	+109·5	−112·8	2	
28	19	15	+112·5	−151·2	2	
2	20	10	+123·0	− 66·0	1	
9	21	12	+129·0	+134·4	2	
5	31	11	+423·0	+134·4	2	
12	35	13	+528·0	− 86·4	2	

TABLE III.—COMPARISON OF LASSELL'S AND LAMONT'S STAR-POSITIONS.

Lassell's number.	Lamont's number.	1862. Lassell's pos. angle, from 2.	1837. Lamont's pos. angle, from 2.	Δp. Lassell — Lamont.	Remarks.
1	3	239·1°	238·3°	+0·8°	No. 1. Lassell's distance 133·1″; Lamont's 134·3″; Δs= −1·2″.
3	15	314·5	314·0	+0·5	
5	28	124·7	125·0	−0·3	
7	2	342·4	341·3	+1·1	* Correcting Lamont's measure of Aug. 2 from 173° 0′ to 177° 0′: omitting it altogether, Lamont's angle becomes 357·8.
8	17	357·6	*357·4	+0·2	
10	10	279·8	279·4	+0·4	
11	8	299·1	299·6	−0·5	

TABLE IV.—STAR-POSITIONS OF LASSELL, MASON, HERSCHEL AND TROUVELOT.

The co-ordinates are measured from star 2 (Lassell).

No.	Herschel 1837.		Mason 1839.		Lassell 1862.		Trouvelot 1875.		Remarks.
	Δα.	Δδ.	Δα.	Δδ.	Δα.	Δδ.	Δα.	Δδ.	
1	−123·0″	+ 66·0″	−124·2″	+ 68·4″	−114·2″	+ 68·3″	−116·1″	+ 64·5″	N+, S−; E+, W−.
3	−177·0	−166·8	−181·5	−171·0	−162·9	−160·3	−163·4	−157·0	
4	----	----	----	----	+492·6	− 18·6	+473·0 ?	*− 8·6 ?	
5	+300·0	+200·4	+280·2	+221·0	+284·3	+196·8	+281·7	+187·0	
6	−136·5	+181·2	−125·0	+159·0	−120·7	+158·2	−124·7	+165·6	
7	− 13·5	− 46·8	− 22·8	− 37·1	− 12·3	− 38·8	− 30·1	− 43·0	* Some doubt as to identity.
8	− 13·5	−135·6	− 6·9	−132·0	− 5·9	−137·8	− 17·2	−137·6	
9	+ 6·0	+200·4	----	----	+ 4·6	+177·5	+ 10·8	+178·4	
10	−184·5	− 15·6	−203·1	− 45·0	−189·8	− 32·7	−187·0	− 21·5	
11	−118·5	− 56·4	−125·1	− 73·0	−113·3	− 63·0	−118·2	− 64·5	
12	+405·0	− 20·4	+351·9	− 32·0	+365·9	− 12·8	+352·6	− 6·4	

TABLE V.—COMPARISON OF LASSELL, MASON, HERSCHEL AND TROUVELOT.

No.	Lassell—Herschel,		Lassell—Mason,		Lassell—Trouvelot.		Remarks.
	Δa.	Δδ.	Δa.	Δδ.	Δa.	Δδ.	
1	8″·8†	2″·3†	10″·0	0″·1	1″·9	3″·8	* Some doubt as to identity.
3	14·1†	6·5†	18·6	10·7	0·5	3·3	Those of Herschel's stars whose
4	----	----	----	----	19·6*	10·0*	position was micrometrically deter-
5	15·7	3·6	4·1	24·2	2·6	9·8	mined are marked †, the rest are esti-
6	15·8†	23·0	4·3	0·8	4·0	7·4	mated.
7	1·2	8·0†	10·5	1·7	17·8	4·2	It will be noted that Mason's resi-
8	7·6	2·2	1·0	5·8	11·3	0·2	duals are quite within his limits of
9	1·4	22·9	----	----	6·2	0·9	error.
10	5·3	17·1	13·3	12·3	2·8	11·2	
11	5·2	6·6	11·8	10·0	4·9	1·5	
12	39·1	7·6	14·0	19·2	13·3	6·4	

It may be seen from table III that the relative positions of the principal stars of this group are the same during the period of observation. Precession will cause changes of relative position far less than the errors of observation.

III. *Comparison of Star-magnitudes.*

It will be remembered that Lassell's numbers represent generally the order of brightness of the stars. Mason's magnitudes were reduced by himself so as to correspond to those of Herschel, and this was done in his own thorough way, by comparing his estimates of the brightness of stars in *Nebula Cygni*, with those previously given by Herschel. Lamont and Trouvelot give no magnitudes.

Table VI contains most of the information available on this subject.

TABLE VI.—COMPARISON OF STAR-MAGNITUDES.

Lassell's number,	1	2	3	5	6	7	8	10	11	12	13	15	28	35	73 ??
Herschel's magnitude,	9	10	11	11	11	11	13	14	13	13	12	15	15	12	13
Mason's magnitude,	11	11	12·13	12	12	12·13	14·15	14	15	12	13	15	16	14	14·15

From this we see that the order of magnitude is as follows:

Lassell,	1,	2,	3,	5,	6,	7,	8,	10,	11,	12,	13,	15,	28,	35,	73;
Herschel,	1,	2,	3,	5,	6,	7,	13,	35,	8,	11,	12,	73,	10,	15,	28;
Mason,	1,	2,	5,	6,	12,	3,	7,	13,	10,	35,	8,	73,	11,	15,	28.

A *suspicion* of variability exists as to stars 12, 13, 10, 35, 8, 73, 11, 15,* 28, but 35, 8, 15 and 73 are the only cases in which a *probability* of variability exists and even in these cases it is slight. If my identification of Lassell's 73 on the other drawings is correct, there is a mistake in his magnitude. A tolerably bright star is very near its place.

* See p. 359.

IV. *Examination of the Question as to the Motion of the Nebula relative to the Stars.*

It will be remembered that the seven drawings of this nebula which we possess were made in the years 1833, 1837, 1839, 1862 and 1875 quite independently, by different observers and different telescopes. It is evident that in cases where these different drawings *agree*, there can be no doubt as to the existence of the feature delineated. The non-existence of any prominent feature not given is probable, although not certain. To examine the question proposed at the head of this section, it will be advantageous to divide the drawings into three groups, the *first* consisting of all figures made before 1840 (Herschel's, Lamont's, Mason's), the *second*, of Lassell's fine delineation, which is entitled to very great weight, and the *third*, of the two drawings made by M. Trouvelot, one at Cambridge and the other at Washington. It is well to recall the fact that Herschel's two figures were made with his 20-foot reflector of 18½ inches aperture; Lamont's with the Munich refractor of 11 inches; Mason's with a 14-foot reflector of 12 inches; Lassell's with his 4-foot reflector; M. Trouvelot's first drawing with a 6½ inch Merz refractor, and the Naval Observatory drawing with the very perfect Clark refractor of 26 inches.

To prove the existence of a change it is necessary and sufficient to show that a prominent feature which the first group of drawings gives, is in a different position relative to the stars in Lassell's drawing and that the motion thus shown is confirmed and continued by the two figures of 1875, much greater weight being given to the work of the larger instrument in 1875. It must be remembered that with two instruments of *equal light*, hardly more discrepancy in the positions of the *brighter* portions of the nebula is to be expected than in the star-positions, for these positions are determined by the stars themselves and can be assigned with almost no error, in a nebula which contains so many stars as the one under consideration. The fainter portions may vary greatly from the smaller to the larger instruments. No relative numerical weight can be assigned to the various drawings, even if it were desirable to do this, but it may be remarked that Mason's, Herschel's (1837), Lassell's and that of the Naval Observatory are of the greatest authority. Herschel's first drawing, he himself does not consider comparable in accuracy to his second: Lamont's is of great weight as to the relative star-positions, but was not intended as anything more than a "sketch," and Trouvelot's first figure while undoubtedly of high accuracy as far as his instrumental means were satisfactory, is yet not strictly comparable with the work of Herschel's and Lassell's reflectors, or to his own work with the Naval Observatory refractor.

In Table VII, I have thrown into a convenient form the evidence for or against a motion of the "horseshoe" with reference to its contained stars. It will be remembered that so far as our evidence goes (see Tables III and V) the relative position of the stars themselves has not changed; as the agreement of the careful measures of Lamont and Lassell (whose observations are separated by 25 years) is quite as good as can be expected. In Table V none of the residuals are greater than the limits of error supposed by the various observers to exist in their own work; and most of them are less. It is moreover, plain that if the star-positions had changed while the nebula also moved, even this fact would not effect the question as to whether the line joining two stars, say 10 and 11, was *inside* or *outside* of the nebulosity. It is therefore in this way that I have presented the evidence concerning the motion of the "horseshoe" relative to the principal stars. That is, I take a certain line, as that one joining stars No. 10 and 11 and terminated by them, and arranging the drawings chronologically, I enquire how this line is situated with regard to the nebula in the various delineations Is it all inside, or partly outside? If partly outside, what fraction of its length is outside? The single exception to this method is that of star 11, whose remarkable situation in the various drawings of the nebula first led me to suspect a change.

The number of *independent* proofs of the change, is not quite so great as the number of columns in Table VII, since most of the evidence rests upon the positions of stars 1, 34, 35, 2, 10, 11.

It will be seen that not only is the change progressive from group I to group II and from this to group III but that in general it is even progressive from drawings 1 to 7.

The exception to this general progression is fig. 6, made by M. Trouvelot with his 6½ telescope shortly before the drawing No. 7. It will be observed that in many particulars his description corresponds to that given by the figures 1 to 4; which might indicate that the differences observed are only such as might be expected from the employment of different telescopes by different observers. This explanation I do not believe to be the correct one, for the reason that, *first*, no explanation is thus attained of the large and consistent differences between the drawing of Lassell and that of the Naval Observatory; and *second*, that an examination of the position of the line of maximum brightness in the early figures, and in that made by M. Trouvelot at Cambridge, shows that so far as the evidence goes, *the line of maximum brightness* in the western half of the horseshoe, is now further to the east than it was in 1834–9. It is evident that if the line of maximum brightness was plainly laid

TABLE VII.

Group.	Authority.	Star, or Line joining two Stars, under examination.							
		No. 11.	10–11.	10–14.	10–13.	10–3.	1–34.	2–35.	2–7.
I.	1. Herschel 1833.	not inside on foll'g edge.	all inside.	all inside.	all inside.	all inside.	34 not laid down.	middle outside ends inside.	inside.
I.	2. Herschel 1837.	outside.	E.$\frac{5}{6}$ outside.	all inside.	all inside.	all inside.	all outside.	inside.	inside.
I.	3. Lamont 1837.	outside.	E.$\frac{9}{16}$ outside.	all inside.	inside.	inside.	N.$\frac{2}{3}$ outside.	all inside.	all inside.
I.	4. Mason 1839.	outside.	E.$\frac{14}{16}$ outside.	all inside.	all inside.	all inside.	34 not laid down.	all inside.	all inside.
II.	5. Lassell 1863.	inside on following edge.	all inside.	N.$\frac{4}{5}$ outside, star 14 quite outside.	nearly all outside.	N.$\frac{3}{8}$ inside.	all outside.	at least $\frac{1}{2}$ outside.	all inside.
III.	6. Trouvelot 1875.	not inside near following edge.	nearly all inside.	inside.	all inside.	all inside.	N.$\frac{2}{3}$ outside.	middle outside ends inside.	outside?
III.	7. Trouvelot and Holden 1875.	inside on following edge.	W.$\frac{1}{3}$ outside?	S.$\frac{1}{2}$ outside, star 14 quite inside.	nearly all outside.	N.$\frac{1}{3}$ outside.	all inside.	all inside.	inside on preceding edge.

Abbreviations used in above Table. N. = north; S. = south; E. = east; W. = west.
For a *star:* (see column 1) inside *or* outside = quite inside *or* quite outside of the nebulosity.
For a *line joining two stars:* inside *or* outside is not so expressive of *certainty* as *all* inside *or* outside.

down by each observer no difficulty would be found in making an exact comparison, and it will be found from an examination of the original engravings that the tracing of such a line on them is a matter of some difficulty. Still, it is believed that sufficient definiteness can be attained to show that on the whole Trouvelot's Cambridge drawing is consistent with the conclusion above given. In particular, his star No. 1 is not at all on the *following* edge of the nebulosity, but well within it, towards the *preceding* side, thus totally differing from the appearances laid down by all earlier drawings. Again, he represents the space *within* the horseshoe and *north-following* star No. 1, to be largely filled with nebulosity, quite consistently with the Naval Observatory drawing, and utterly different from the drawing of Lassell (see Fig. 6). This fact is of great importance, since, if the nebulosity *followed* star No. 1, in 1862, Lassell would have so represented it (as he did not) and as Trouvelot has so drawn it, it is plain that an important change must have occurred to render it possible for a six-inch aperture to show nebulosity in 1875, in a space perfectly void of nebulosity to Lassell's great reflector in 1862. *On the whole, then, these drawings show that the western end of this nebula has moved relatively to its contained stars from* 1833 *to* 1862, *and again from* 1862 *to* 1875, *and always in the same direction.*

I conceive that this is the best conclusion that can be drawn from the *ensemble* of the drawings. If we confine our attention to the three best ones, viz: Herschel's (1837), Lassell's (1862), and that of the Naval Observatory (1875), this conclusion comes out with greater distinctness. There is only one important feature in these three drawings which does not strictly agree with this supposition, namely that star No. 1 is in the same position with reference to the nebulosity in the first two of these which were made at an interval of twenty-five years and that they both differ from the last drawing made thirteen years after Lassell's. In every other respect the agreement is strictly with the above conclusion, and however much weight I might have been inclined to give to this disagreement, if the only data were those of the drawings, I cannot regard it as final in the light of a most careful examination of the nebula on the very fine night of March 21, 1876, when the various drawings were compared with the heavens. I add from the observing books a literal copy of my recorded observations on that occasion.

Extract from Observing-Book.

"1876. March 21. 16^h–17^h. Observer HOLDEN, Recorder, D. P. TODD.

Omega–Nebula. R. A. $18^h\ 12^m$ N. P. D. $106^\circ{\cdot}2$. Magnifying power 175.

Star No. 1 brighter, but not much than 2. It *precedes* $\frac{9}{10}$ of the nebulosity of the *preceding* hook of the horseshoe. The line joining 1 and 10 is barely inside the nebulosity. The line joining 10 and 3 is inside the nebulosity but not much. 11 is just on *following* edge of the nebulosity. 34 *follows* the *west* branch of horseshoe, about $\frac{1}{2}$ the distance 2–7 [this is a rough estimate only] and is clear of any nebulosity. The middle $\frac{1}{2}$ of the line 6–1 is in the dark. The line 6–35 is all in the nebula. 8, 73, 11 about the same magnitude; 73 is in faint nebulosity, on the *preceding* edge of it. Between 11 and 73 very faint nebulosity which joins these two stars. Line 3–13 is just inside the nebula.—Star No. 1 certainly *precedes* $\frac{9}{10}$ths of west branch of horseshoe. Line 1–34 has its *west* $\frac{7}{10}$ths in nebula. 34 seems to be in the dark. Certainly a connection across 11–73 more distinct than in Naval Observatory drawing —more definite.

Line 10–11 is all inside nebulosity.
" 1–36 " " "
" 10–13 runs through fainter nebulosity.
13, 14, 10 are on *following* side of a bay which is filled with very faint nebulosity. 43 brighter than 15. 44>42>15 but the inequality is not great. 15 is the faintest star of 43, 44, 42 and 15.

The dark space within the horseshoe and bounded by 2, 7, 73, 11 is elliptic; the largest diameter is perpendicular to the line 8–14, and the diameters are as 6 to 4. It seems more regular in shape than in Naval Observatory sketch. Line 1–36 crosses fainter part of nebula about $\frac{1}{3}$ of the way from 1 to 26 and nearer 1. *Preceding* the line 1–10 is a darker space about equal in width to the distance 1–6. *Preceding* that, the sky is nebulous for 10′ at least. [This requires confirmation.] There is a faint prolongation from 6 towards *south preceding*. The shape of Herschel's resolvable knot is correctly laid down by Trouvelot. Two stars at its southern point and a star at or near junction of the two prongs of this knot. Sky more transparent than I have ever seen it. Much annoyed by the forming of clouds."

It will be seen that it cannot be doubted that star No. 1 is *now* in a position relative to the nebulosity quite different from that laid down by Lassell in 1862. Further, all the earlier drawings except Herschel 1837, put star No. 1 well within the nebula, and hence we are forced to the conclusion that the west half of the horseshoe has moved with reference to *this* star. Trouvelot's drawing with his small telescope is the only one not showing a similar and consistent motion with reference to the group of stars 10, 11, 3, 13, 15, etc., and even here we find evidence of changes in the same direction from the earlier drawings and the conclusion of the motion of the west one-half of the horseshoe with reference to its contained stars acquires new weight. The eastern half of the horseshoe, or at least that portion of it north of stars 2 and 7 shows on the contrary no evidence of such motion.

The observed changes in the *drawings* may be best accounted for by supposing a bodily shifting of the whole of the horseshoe in a plane nearly perpendicular to the line of sight, and on a pivot situated somewhere in the region of star No. 8, though, of course, it is not supposed that this is a real explanation of the physical changes.

A careful study of the evidence relating to the *Messierian* streak indicates no motion with reference to the contained stars. Graphic methods lead to the angles of position of this portion given below, with which I have incorporated the results of measures by D'Arrest and Schoenfeld.

Herschel, 1833:	p=119°.	†Schoenfeld, 1862: mean of two, 115°, 110°.	p=112°,
Herschel, 1837:	p=119°.	Lassell, 1863: ?	
Lamont, 1837:	p=113°?	Trouvelot, 1875:	p=113°.
Mason, 1839:	p=115°.	Naval Observatory, 1875:	p=119°.
*D'Arrest, 1855: mean of two, 128°, 116°.	p=122°,		

To sum up:—Tables III and V show that the stars have remained in their relative positions from 1837 to 1875; and a consideration of the drawings, whether taken as a whole or considered according to their relative importance, shows that the horseshoe has moved with reference to the stars while the *Messierian* streak has not moved, and that therefore *we have evidences of a change going on in this nebula.*

This may be a veritable change in the structure of the nebula itself such as was suspected by Schrœter, confirmed by Otto v. Struve and again confirmed by myself in the Nebula of Orion, or it may be the bodily shifting of the whole nebula in space in some plane inclined to the line of sight.

A remarkable instance of a proper motion of this latter kind is that of the *Trifid Nebula* G. C. 4355, which has moved since 1833 so that the remarkable triple-star which was then quite clear from the nebulosity in a dark space formed by the junction of the three dark channels, is now by the evidence of Lassell (1863) Winlock and Trouvelot (1874) and myself (1875) well involved, the motion being confirmed by Herschel's drawing at the Cape of Good Hope (1837) and Mason's of about the same date.

The importance of the theoretical conclusions as to the constitution and distance of the nebulæ, to be derived from the first well-authenticated instance of the variation in form of any one nebula, have seemed to me to justify the discussion of the

* Abhand. d. K. Sach. Ac. d. Wissenschaften, Bd. v.

† Ast. Beob. Mannheim. Zweite Abth., 1875.

imperfect evidence now existing in regard to the one under consideration; and it must be remembered that no matter how inadequate this evidence may at first sight appear to be, it is yet as full, minute and complete as that relating to any single nebula except that of Orion; and if the drawings here considered are not sufficient to prove a change or the absence of a change, we are reluctantly driven to the conclusion that the work done by astronomers in this direction has been largely wasted. I hope that the evidence here adduced may be deemed of sufficient importance to warrant the great expense of time and labor necessary to a detailed monographic study of this nebula, which may serve for future reference. There is probably no nebula visible in the northern hemisphere more worthy of such examination.

U. S. Naval Observatory, April, 1876.

ART. XLIII.—*Brief Contributions from the Physical Laboratory of Harvard College.* No. XVIII.—*On the effect of thin plates of Iron used as armatures for Electro Magnets, and a new form of Induction Coil;* by JOHN TROWBRIDGE, assistant Professor of Physics.

IN a paper presented to the American Academy of Arts and Sciences, April 13, 1875, I showed that the application of armatures to two straight electro-magnets, which formed the primary circuit of a Ruhmkorff coil more than doubled the strength of the induction current produced by breaking the primary circuit. When, however, the circuit of the secondary coil was not closed, and a spark was allowed to jump across the interval between its poles, the striking distance of the spark and its power to charge a condenser did not seem to be notably increased by the application of armatures to the electro-magnets of the primary circuit. My experiments at that time were made with solid iron cores; and I now resume these experiments with bundles of fine iron wires in place of the solid iron cores. The mechanical difficulty of making the ends of the bundle of fine iron wires constituting the cores plain surfaces was overcome by dipping them in melted solder and then filing the ends smooth. In this way I had no trouble in applying the armatures so that they should lie upon a plain surface. The resistance of each of the two induction coils covering the two straight electro-magnets was 6000 ohms; and that of each of the straight electro-magnets, ·34 of an ohm. The diameter of the bundles of fine iron wires constituting the cores was 5 cm. and the length of the electro-magnets 28 cm. Condensers of various sizes were placed in the primary circuit. The results given in this paper were obtained by the use of a condenser of about one farad. The method of experimenting was to charge a condenser of one-third of a farad; and then to discharge this condenser through a galvanometer. If we express the quantity of electricity received by the condenser by Q, the electromotive force by E, and the capacity of the condenser by C, we have $Q=\frac{2nt}{\pi}\sin\frac{1}{2}\theta$, where n is the reduction factor of the galvanometer, t the time of vibration of the magnet, and θ the angle through which it swings under the effect of the change. Knowing the reduction factor of my galvanometer, I had thus the means of reducing my results to absolute measure. But I speedily found that the relative results obtained by the proportions

$$Q : Q'=\sin\tfrac{1}{2}\theta ; \sin\tfrac{1}{2}\theta'=E : E'$$

would present the points of this investigation in as clear a manner as if the results had been reduced to absolute magnetic measure. My first experiments were made with solid armatures.

TABLE I.

Without armatures. Tan θ.	With armatures. Tan θ'.
80	90
70	80
90	100
60	70
70	85
80	90
Mean, 75	86

TABLE II.

Without plates.	With plates.
80	400
70	380
90	370
60	400
70	370
80	400
Mean, 75	Mean, 386·6

In table I the numbers are the deflections of the reflecting galvanometer expressed in millimeters, and the distance of the scale from the magnet was one meter. In this case the gain by the use of the armatures was trifling, being only about fourteen per cent. These results were obtained by charging the condenser of one-third of a farad, by sparks one millimeter in length. On a closed secondary circuit, however, a gain of one hundred per cent was clearly seen in the strength of the induced currents produced by breaking the primary circuit. The question, how to make this increase in the strength of the induced current by the employment of armatures apparent on a broken secondary circuit, became an interesting one. It seemed at first as if the application of armatures, by maintaining the temporary magnetization of the iron cores would be detrimental rather than otherwise. I next tried the effect of bundles of thin iron plates, which were placed, as armatures, upon both poles of the electro-magnet, thus making a magnet of a horse-shoe form. On charging the condenser, I found a very great increase in quantity, which was manifested by the swing of the galvanometer needle; the indicator being entirely off the scale.

Table II shows the results obtained by the use of iron plates one and one-quarter of an inch in thickness, twenty in number, constituting each armature.

Here a gain of four hundred per cent was manifested by the use of thin plates. The next step was to ascertain how many plates were necessary to obtain the maximum effect. The difficulty of obtaining plates of the same homogeneity, made it impossible to obtain smooth curves. To this difficulty was added that of breaking the primary circuit in a regular manner. If the results of Table III are plotted, it will be seen that the increase within small limits, is very nearly proportional to the number of thin plates, which were $\frac{1}{24}$ of an inch in thickness.

TABLE III.

No. of Plates.	Deflection of Galv.	No. of Plates.	Deflection of Galv.
1	11	6	15
2	12	7	15·5
3	13	8	16
4	13	9	18
5	14	10	18·5

On increasing the number of plates a point was reached where there was no additional effect. The best result was obtained when the mass of the armatures was approximately equal to that of the cores of the electro-magnets. Plates of $\frac{1}{32}$ of an inch were also used, but no advantage resulted in their employment over those of $\frac{1}{24}$ of an inch. It would seem that the thin plates followed the same law as that of the bundle of fine iron wires which constitute the cores of induction coils of the present day, and that only a moderate degree of discontinuity in the mass of iron submitted to magnetic influence is necessary to prevent the formation of currents of induction, which prolong the magnetism of the cores, and prevent the quick demagnetization necessary to produce intense currents of induction. The effect of insulating the thin plates with the dielectrics was also tried with no gain in effect. There appeared to be a slight gain by placing the plates edgewise on the poles of the magnets instead of allowing them to repose on their flat faces. This was doubtless due to better contact of the metallic surfaces.

Since the above results proved conclusively a very great gain in quality and electrometric force by the application of thin plates as armatures, I next measured the striking distance of the spark. Table IV gives the results which are the mean of many trials.

TABLE IV.

Without armatures.	With armatures.
15 cm.	32 cm.
14	30
15	32
Mean, 14·5	31·3

A curious fact came to light in this connection; the lengthening of the spark was not shown when the spark leaped directly between the poles of the induction coil; the increase in quantity and electromotive force, was only made manifest to the eye by the employment of condensers in the secondary circuit. The results in Table IV were obtained by the employment of a leyden jar of large capacity. The increase in the quantity and electromotive force was not only shown by the increased length of the spark, but also by its increase in volume, and its louder snap. The spark consisted of a thick central bolt surrounded by curious thin, detached sparks. An attempt was made to measure the increase of light in the Geissler tubes by Vierodt's photometric apparatus, but it was found too inexact for this purpose; if, indeed there was any increase of light, which remains to be proved. I know of no results which bear upon the relation of the increase of light to the increase of electromotive force of the induction spark. Without condensers in the secondary circuit, however, the increased electromotive force of the spark was shown by its greater constancy in leaping over a given resistance of air.

Unless an instrument is desired for popular scientific lectures, length is not so much to be desired as quantity of electricity of a spark, and in this form of induction coil the gain is principally in quantity, although it is true that with the aid of leyden jars, the striking distance is increased one hundred per cent. The principal points of this paper can be thus summed up:

1. The application of thin plates of soft iron upon the poles of two straight electro-magnets, with bundles of fine iron wires for cores, increases the strength of the spark produced at the poles of the secondary coils surrounding the electro-magnets, four hundred per cent.

2. The length of the spark is increased one hundred per cent. This gain in length is only manifested by the employment of leyden jars of large capacity, which are connected with the secondary circuit.

3. Instead of distributing the fine wire of a Ruhmkorff coil upon a straight electro-magnet, as is done at present, this wire should be distributed equally upon two straight electro-magnets, whose poles should be provided with armatures of bundles of thin plates of soft iron.

ART. XLVI.—*Communications from the Laboratory of Williams College.* No. VI.—*Concerning Phosphorus Oxychloride;* by IRA REMSEN.

THE fact was recently established* that carbon monoxide, though it must be considered as an unsaturated compound, does not readily combine with the oxygen from ozone to form the saturated dioxide. Indeed it was impossible to discover any conditions under which such a combination takes place.

Although it is known that ozone does readily oxydize many substances, it seemed to me desirable to further test its action upon bodies which are generally recognized as unsaturated. For this purpose I have first employed phosphorus trichloride in the hope of obtaining the oxychloride, $POCl_3$. The method of formation of the oxychloride thus indicated would be interesting from more than one stand-point, as will be pointed out below.

It has already been shown by Brodie† that, when oxygen is passed into phosphorus trichloride at the boiling temperature of the latter, a partial transformation into the oxychloride takes place; and Michaelis ‡ subsequently showed that this transformation or oxydation is exceedingly incomplete, even though the process be continued for two or three days. An analogous experiment has also been performed by Henry,§ who proved that, when sulphur and phosphorus trichloride are heated together in a sealed tube at 130°, the sulphochloride $PSCl_3$ is formed. It is plain that, in both of these experiments, one of the forces which opposes the combination is that which binds together the atoms of oxygen in the molecule of oxygen, and the atoms of sulphur in the molecule of sulphur; and hence, if we could employ free atoms of oxygen or sulphur instead of their molecules, we would expect the action to take place much more readily. In the case of sulphur, it is not possible, as far as we know at present, to obtain free atoms or unstable molecules which by their breaking up yield free atoms. In ozone, however, we have such an unstable molecule of oxygen. As we have seen in the experiment with carbon monoxide, above referred to, ozone does not always appear to furnish free atoms of oxygen when we might expect it to, and hence the formation of phosphorus oxychloride by the action of ozone could not be predicted with any certainty. Experiment proved, however, that the formation actually does take place with

* This Journal, vol. xi, p. 136.
† Odling's Handbook, i, 297.
‡ Gmelin-Kraut's Handbuch der Chemie, I, i, 391.
§ Berliner Berichte, ii, 638.

great ease, and that phosphorus oxychloride may be obtained in this way in any quantity.

Pure phosphorus terchloride boiling at 77° was placed in a flask. In the cork of the flask were three openings. In one of these was inserted a thermometer which dipped into the liquid; in another was a tube, leading from the ozone-generator, which served to conduct the ozone into the liquid; in the third was placed a tube which in turn was connected with an inverted Liebig's condenser, the latter serving to condense and return to the flask any vapors that might be formed. Oxygen, thoroughly dried by sulphuric acid and calcium chloride, was now passed through the tube which served as ozone-generator. At first I employed a Siemen's ozone-tube which was connected with an induction-coil. The action in this case was not marked, although I soon observed that the thermometer indicated a rise in the temperature of the trichloride. At the beginning of the operation the temperature of the liquid in the flask was the same as that of the air in the room, viz: 15°. In a short time it rose to 36° where it remained stationary, as long as ozone was conducted into the liquid. As soon as the current was stopped, the temperature began to fall and continued to fall gradually until the ordinary temperature was reached.

In about an hour the process was interrupted, and the liquid subjected to distillation. Its boiling point was markedly changed. Only a drop or two passed over before the thermometer indicated 80° and then the mercury rose gradually to 110° when all had passed over. About half the liquid boiled below 90°. This was again subjected to the action of ozone, but now instead of using Siemen's tube, as at first, Wright's tube connected with the Holtz electrical machine was used, and with much better results. The arrangement of the apparatus in this second experiment was the same as in the first. The temperature of the liquid in this case also began to rise as soon as the ozone was passed into it. In a few minutes the thermometer, which at the beginning of the operation indicated 15·5° stood at 44°, where it continued to stand with slight fluctuations during the entire process. At the surface of the liquid the increase in the temperature was so marked, that a portion was converted into vapor, which was returned to the flask by means of the condenser. In the flask, a few drops of a yellowish, resinous material made their appearance, principally at the end of the ozone delivery-tube. The quantity of this substance formed was so small, as to preclude the possibility of an investigation. On repeating the experiment subsequently, this substance always appeared as a product. Whatever it is, it seems to be somewhat volatile with the vapor of the oxychloride of phosphorus.

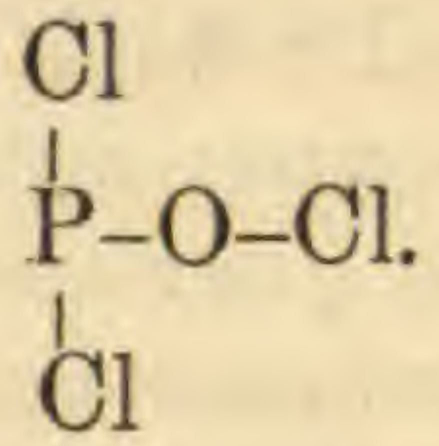
The passage of ozone into the flask was continued for about half an hour in the second experiment, and the product was then examined. About three quarters of the liquid now boiled above 100°, and it did not commence to boil below 85°. By the first distillation it was thus separated into two parts, one boiling below 100°, and the other boiling above 100°. The latter, without further treatment, was at once analyzed. A small quantity was weighed in a sealed bulb, and the bulb then broken under water. After decomposition, pure nitric acid was added and the solution then precipitated with silver nitrate.

0·251 grams of the substance gave 0·6842 grams AgCl= 0·1692 grams Cl.

This corresponds to 67·41 per cent of chlorine, while phosphorus oxychloride contains 69·37 per cent of chlorine and the trichloride 77·4 per cent chlorine. This analysis could hardly be expected to give more satisfactory numbers, as very little precaution was taken to separate the pure oxychloride from the mixture. It proves, however, that the liquid under examination contains markedly less chlorine than the trichloride with which we started, and nearly the same amount as the oxychloride which we would expect to be formed under the circumstances described. The deficit in chlorine may be accounted for by considering the yellow resinous material, above referred to, as being free from chlorine and being also somewhat volatile with the vapors of phosphorus oxychloride.

In addition to the above facts, it was found that the liquid was decomposed slowly by cold water—much less readily than the trichloride—and as a product of the decomposition with water, a large amount of phosphoric acid was formed. Taking then everything into consideration, there cannot be much doubt that, when ozone acts upon phosphorus trichloride, the latter is readily converted into the oxychloride.

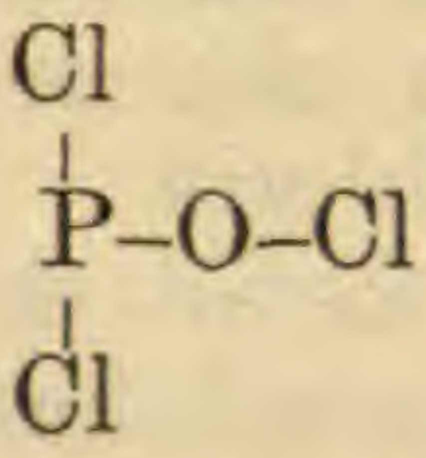
The reaction is analogous to those referred to above, viz: the production of the oxychloride by the action of oxygen upon boiling trichloride, and that of the sulphochloride by the action of sulphur on the trichloride at 130°. Further, it is analogous to the reaction which gives rise to the formation of phosphorus perchloride by the action of chlorine upon the trichloride. The most natural thought that suggests itself is that all these bodies have a constitution similar to that of phosphorus perchloride, and that, in the above reactions, phosphorus passes from the triad to the pentad condition. Those who hold the view that the valence of an element is invariable are inclined to consider phosphorus oxychloride as having a structure essentially different from that of the perchloride, and they write its formula thus:



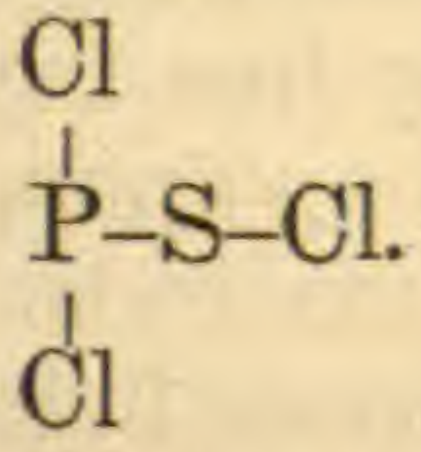
According to this, phosphorus is trivalent in the oxychloride, the same as in the trichloride. The grounds for this assumption are as follows:

1. The oxychloride can be converted into the form of vapor without undergoing decomposition, which shows that it cannot belong to the class of compounds known as molecular compounds, inasmuch as these latter are decomposed into simpler molecules by the action of heat. But, if it is not a molecular compound its structure must be similar to that of phosphorus trichloride, which is the type of the atomic compounds of phosphorus.

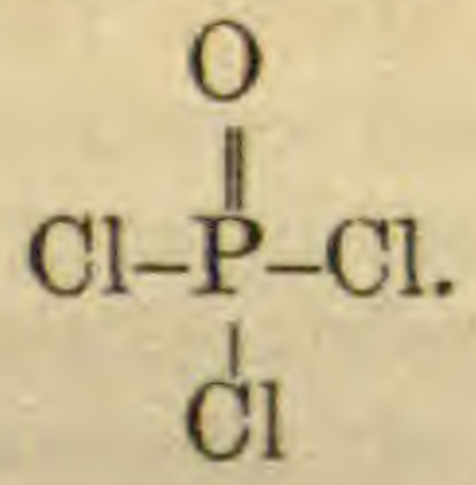
2. Thorpe* has recently shown that the specific volume of oxygen in phosphorus oxychloride is 7·89 and that of sulphur in the thiochloride 22·66. These values agree closely with those formerly found by Kopp for oxygen and sulphur which are united with another element with only one affinity each. From this the conclusion is drawn that in phosphorus oxychloride and thiosulphide, the oxygen and sulphur are united to phosphorus by only one affinity each, and hence that the structures of these bodies are represented by the formulas:



and

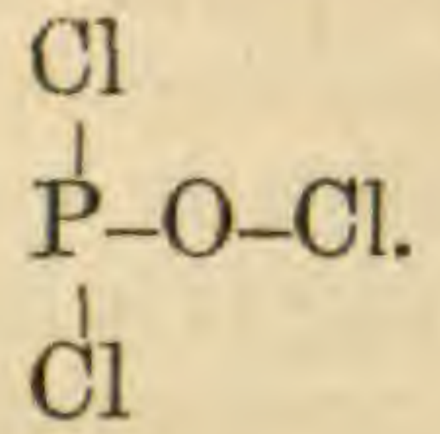


It may be remarked in regard to the former of these grounds that Würtz† has shown that phosphorus perchloride itself, the most decided representative of molecular compounds, may, under proper conditions, be converted into the form of vapor without undergoing decomposition, and hence there is no good reason for assuming that the perchloride differs from ordinary chemical compounds in any essential particular. If, however, the perchloride is a true chemical compound, an atomic in contradistinction from a molecular compound, then phosphorus is in it quinquivalent, while it is certainly trivalent in the trichloride. If, further, we once grant that phosphorus can and does act as a quinquivalent element, we would naturally suppose it to act so in the oxychloride, and hence to the latter would be given the formula,

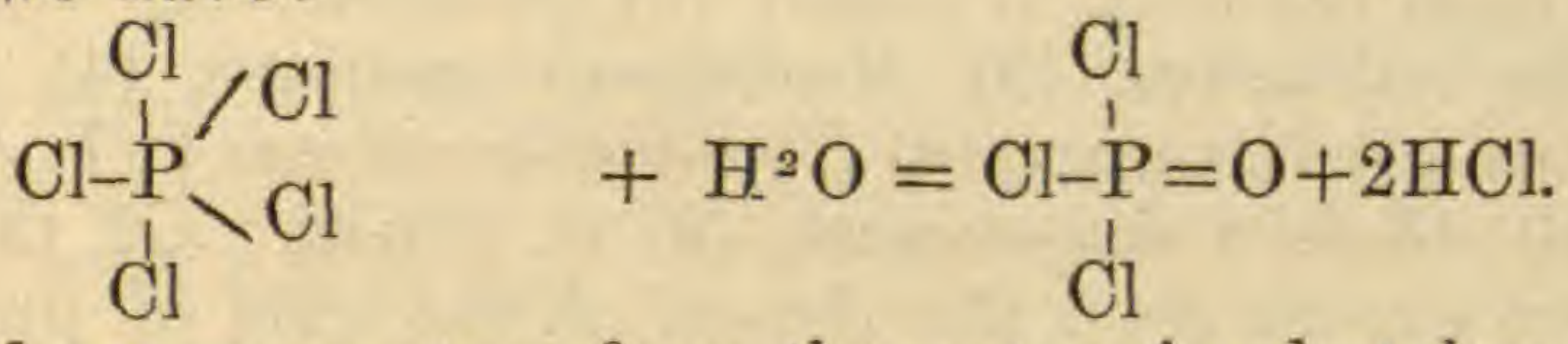


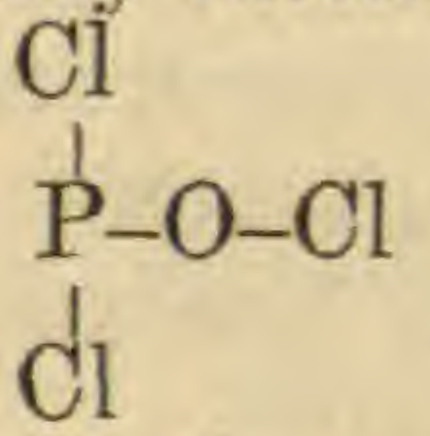
* Berliner Berichte, viii, 326. † Comptes Rendus, lxxvi, 601.

This formula is more in accordance with known facts than the former one. We can much more readily understand how a compound of this formula may be produced by the action of water on phosphorus perchloride, than we could understand its formation if its formula were



Thus we have:



Here the oxygen atom from the water simply takes the place of two chlorine atoms without any further disturbance of the molecule. But, if the formula  is correct, then it is plain that the reaction with water is much more complicated.

So too the formation of the oxychloride from the trichloride by the action of ozone is much more comprehensible, on the assumption that the reaction consists simply in the taking up of an atom of oxygen, without any accompanying displacement and subsequent binding of chlorine.

In regard to the experiments of Thorpe, it may be said that, if Kopp's principle is correct, i. e., if the specific volume of an atom is determined by the manner in which it is held in combination, and, if Thorpe's numbers are correct, then there can be no doubt that the conclusion drawn by him is also correct. While there is no reason for doubting the correctness of the numbers obtained by Thorpe, we may perhaps be justified in not accepting Kopp's principle as so firmly established, that we can employ it as a means of proving the correctness or incorrectness of formulas which seem to be well established by other means.

March, 1876.

ART. XLV.—*On additional species of Fossils from the Primordial of Troy and Lansingburgh, Rensselaer County, N. Y.;* by S. W. FORD.

IN a paper communicated to this Journal about a year ago (March, 1875) I gave a short account of the Primordial rocks in the neighborhood of Lansingburgh, N. Y., and of the finding of several species of fossils in a deposit of conglomerate limestone occurring there. I had hoped to be able to devote a good deal of time to the further study of this field during the past season, but other matters prevented, and while I have made a number of additional observations upon the structure of the region, I am not, as yet, prepared to place them upon record.

From the conglomerate-limestone* I have, however, made collections on several different occasions and with very gratifying results. The following is a list of the species known to me from this deposit at the present time, all of which have been obtained from the rock in place: (1) *Olenellus asaphoides*, (2) *Conocephalites trilineatus*, (3) *Microdiscus speciosus*, (4) *Hyolithes Americanus*, (5) *H. impar*, (6) *Hyolithellus micans*,† (7) *Stenotheca rugosa*, (8) *Obolella desquamata*, (9) *O. nitida*. Of these 2, 4 and 9 were known from this deposit at the time of publication of my former paper. All of these species occur likewise in the Lower Potsdam limestones at Troy. This gives nine species in common to the two localities and is, I believe, conclusive as to the age of the conglomerate in question. I have no doubt but that, when further studied, this rock will furnish yet other species of the Troy series.

Fossils from the Primordial at Troy.—For a long time my examinations of the slates at Troy for fossils were unrewarded, notwithstanding I gave a good deal of attention to the subject. But on one occasion last summer I succeeded in finding several slabs containing undoubted plant remains. There appear to be two or three species. Of these, however, but one is represented in the collection by specimens sufficiently well preserved to admit of anything like a satisfactory determination. This is of the genus *Palæophycus*, and is, according to Mr. Billings, to whom I have lately submitted my specimens for comparison, perfectly identical with his *Palæophycus incipiens* (Pal. Fos., vol. i, p. 2), from rocks of the same age in Vermont and on the north shore of the Straits of Belle Isle in Labrador. The principal specimen in my collection is six inches long, nearly straight, of uniform width and without any evidences of branching. On the same slab there are several shorter fragments. This adds a

* This rock is of other than paleontological interest. I have recently ascertained that good typical specimens of it are susceptible of a fine polish. The limestone fragments of which it is principally composed are, for the most part, of a dark blue color, compact and of a flint-like fracture, and, in polished sections, contrast finely with the yellowish calcareo-arenaceous matrix in which they seem almost to float. Along with these are lighter colored, less coherent fragments in which the fossils mostly occur. It has been quarried to a considerable extent for building purposes, and appears to be a good durable stone. There is a bridge on the Troy and Boston R. R. in the village of Lansingburgh composed of it.

† Mr. C. F. McMurray of Lansingburgh, and a graduate of Yale College, who accompanied me on one of my excursions, has recently presented me with a specimen of an unusually large individual of this species, collected by him subsequently on the occasion of an independent visit made to the locality.

new class of fossils to the Troy fauna, and gives us, moreover, another example of a fossil species having an extensive geographical range.

It occurs in the coarse red-and-yellow-weathering slates of the Lower Potsdam group at Troy.

During the past season I paid a large number of visits to the first band of limestone met with in going eastward from Troy, and which, in a former paper, I have characterized as limestone band No. 1. (This Journal, Aug., 1873) As the result of this I succeeded in obtaining several species of fossils not previously known from this band, although known to occur in the other limestone beds of the Troy Primordial, and along with these a single head of a new and very pretty Trilobite of the genus *Microdiscus*. This I shall describe for the present as follows: Head, broadly rounded in front, nearly semi-oval in outline, greatest width at about the mid-length, slightly narrowed in passing backward from this point to the angles. Glabella conical, about two-thirds the length of the head, with two straight, moderately deep furrows extending all across, dividing the glabella in advance of the neck-furrow, into three parts of nearly equal length. Neck-furrow extending all across and deeper than the other glabellar furrows. The form of the neck segment cannot be clearly made out owing to the damaged condition of the specimen at this point. Dorsal furrows narrow, not deep, dying out toward the front of the glabella. Cheeks prominent, much swollen in the posterior third, without eyes or sutures. Marginal rim well defined all around, widest in front, with a conspicuously raised edge, inside of which there is a nearly flat or feebly concave space, and so bent upward in front as to give to the head on a side view a kind of slipper-like appearance On either side of the head, just inside of the raised marginal edge, there are three small tubercles situated within the limits formed by a line drawn across the head through the middle of the cheeks and another drawn parallel with it just in advance of the front of the glabella.

Greatest width of the head 1½ lines; length along the median line, including the neck-segment, the same. Differs from *Microdiscus* (*Agnostus*) *lobatus* Hall (Pal. N. Y., vol. i, p. 258, pl. lxvii, figs. 5 a–f), from the same locality, in its shorter and transversely furrowed glabella, its tuberculated margin, and in its general proportions.

For this species I propose the name *Microdiscus Meeki*, in honor of Mr. F. B. Meek, whose labors in the cause of science have so vastly contributed to advance our knowledge of American Paleontology.

Occurs in conglomerate limestone of the Lower Potsdam group at Troy.

Troy, N. Y., Jan. 15, 1876.

ART. XLVI.—*On a simple and very accurate method of tuning two Forks to unison;* by ROBERT SPICE, F.C.S.

THOUGH the optical method of tuning, of Lissajous, gives good results, I find that two forks thus tuned to unison, may be a *fraction* of a vibration *out*, without in *any way* disturbing the steadiness of the figure.

In the 2d edition (English) of Tyndall's "Sound," in lecture VII, the author says, "I divide this jar by a vertical diaphragm, and bring one of the forks over one of its halves, and the other fork over the other. The two semi-cylinders of air produce beats by their interference. On removing the diaphragm, the beats continue as loud as before, one half of the same column of air interfering with the other."

Dr. Tyndall does not, however, mention the fact, that precisely the same result would have been obtained if no diaphragm had been employed, yet this is so. When two unison forks are struck on the knee, (or by a piece of lead covered with leather,) and then held *together* over their proper resonant column, the following phenomena will be observed.

If there is a difference between their rates, of several vibrations, there will of course be rapid beats; if the forks are *very* nearly in tune, the beats will succeed each other at long intervals; further, when they are *almost* perfectly in tune, there will not be any beats properly so called, but after the sound of the forks has *nearly* died away, it will rise or swell out again *very slightly*, proving that there had been interference.

Finally, when the forks are absolutely alike, there will be a gradual decrease of sound, down to silence, without any reinforcement at any time.

I find that to carry out this tuning absolutely, both forks must be at the same temperature; consequently, after using a file on one of them, I place both forks in a vessel of water to equalize their temperatures, wipe them dry, and test them. To show the accuracy of this method I select the following example:

A pair of Ut^3 forks (256 vibrations) will sound over a column for about 135 seconds; suppose that the sound decreases up to the 100th second, and then begins to rise; obviously 100 seconds is the time of *half a beat*, or 200 seconds the beating time; that is to say, it will have been demonstrated that one of the forks gave $\frac{1}{200}$ of a vibration *per second* more than its fellow.

What has been said of the unison, applies to other intervals.

I have recently executed by this method, Ut_4, Ut_5, Ut_6 and Ut_7 forks for the physical cabinet of Columbia College.

230 Bridge Street, Brooklyn, Jan., 1876.

ART. XLVII.—*Silica of grasses and other plants carried up as Diatoms or other siliceous grains, and not in solution or as soluble silicates;* by Prof. P. B. WILSON.

MY attention was called, some time since, in the examination of the ash of plants obtained by slow incineration in a platinum crucible, to the fact that when the ash is treated with dilute acid, and evaporated to dryness on the water bath, it does not pass into the gelatinous condition prior to complete decomposition of the *hydrated* mass, as is the case with the silicates soluble in acid, or those decomposed with sodium and potassium carbonates. If, however, the ash, prior to the treatment with acid, is subjected to a high temperature, a combination of silicic acid with the alkalies, the alkaline earths, and the earths takes place, if all are present; then the silica separates in the gelatinous form and presents all of the chemical reactions of silicic acid obtained from the natural silicates. The silica obtained from ash by either of the processes indicated, on close examination, was observed to be entirely free from any combination, showing that it had been assimilated in the free state.

To demonstrate this theory, my friend G. I. Popplein, Esq., of this city, suggested the application of infusorial earth of the Richmond formation—found in large quantities on the western shore of the Chesapeake bay—to land sown in wheat. I have obtained straw from wheat so grown, and have found, after it has been treated with nitric acid, and the siliceous remains placed on the field of the microscope, that it consisted wholly of the siliceous shields of Diatomaceæ, the same as found in the infusorial earth, excepting that the larger discs in their perfect form were absent (*Actinocyclus Ehrenbergii* and *Actinoptychus undulatus*). My conclusions are that they, and there probably may be other forms, are too large to enter the root capillaries. During the coming summer I will attempt if possible to make micrometer measurements of both.

The discovery of Diatomaceæ in their original form in this wheat straw precludes the possibility of the infusorial earth having undergone any chemical change in the soil, either by forming chemical combination with the alkalies, or the earths, or by suffering physical disintegration from any catalytic action of any salts present in the soil

In the particles of silica placed upon the glass slide, when they were completely separated from each other, the outlines of the individual diatoms were sharply and distinctly defined. On the other hand, when the physical action of ebullition with nitric acid was not sufficient for the complete separation of the particles of the epidermal shield, there was observed a marvelous interlacing of the various forms, showing that they were conveyed by the sap cells directly to the section of the plant where they were destined to complete its structure. I have examined several specimens of straw, taken at random in the market; the silica in each specimen consisted of plates, very thin, and truncated at the corners.

The result of these investigations shows the necessity of finely divided silica in the soil, so minute as to be capable of passing with facility through the sap cells; secondly, that simple or compound silicates are useless as fertilizing agents, either natural, or artificially prepared. We have no valid reason for forming any theory that vegetation can, through any known chemical law, separate the elements or their compounds from combinations so positive in their character.

In this case we have a practical result capable of being verified at any stage of growth of a plant, produced by the application of silica to the soil in the form of certain well defined microscopic organisms; for, finding these in the ash to the exclusion of other particles of silica, they seem to be more acceptable for the plant structure. Free silica is hence the only condition in which it can enter the plant.

I look upon this discovery as leading agricultural investigations in a new direction, and it must eventually change many of the views expressed and accepted by scientists.

Every precaution was used in having all the material thoroughly cleansed, with a view both for accuracy and for removing suspicions that these microscopic forms were the result of dust showers.

Washington University, Medical department, Baltimore, Md., February, 1876.

ART. XLVIII.—*The Conglomerate Series of West Virginia;* by WILLIAM M. FONTAINE.

[Concluded from page 284.]

DR. STEVENSON, in his "Notes on the Geology of West Virginia," (read before the Am. Phil. Soc., Feb. 5, 1875,) speaks as follows of the "Great Conglomerate" of Randolph county. "This rock forms the crest of Rich Mountain for nearly sixteen miles within the region examined. For the most part it is a coarse sandstone, loaded with pebbles from one third of an inch to two inches in diameter. Along the Staunton Pike it shows some layers of slightly micaceous and very compact sandstone near the bottom. Here it is greatly increased in thickness; near the northern line of the state it is barely 350 feet thick, but in Randolph county it is not less than 600." He further says, "On the

Staunton Pike, along the east slope of the mountain, there was seen midway in the conglomerate, what appeared to be the blossom of a coal bed. As I had observed no evidences of coal in the conglomerate northward from this locality, this exposure was studied with some care, but nothing definite could be ascertained. Six miles farther south, on the same side of the mountain, a small coal bed occupies this place on the property of Mr. Bradley. There it is three feet thick." Dr. Stevenson also points out the mistake made both by himself, and myself, in admitting the presence of coal in the conglomerate of Monongahela county.

Dr. Newberry has shown that the Sharon coals of Pennsylvania, which are in the reports of the first survey put under the conglomerate, are really of later age. There remains then no case where coals are found within the rock in its extension northward.* As is well known, the conglomerate in Pennsylvania north of this part of West Virginia, has thinned down to a homogeneous rock of 100 feet and less. We must then look for the north extremity of the special basin in which the expansion of this rock took place, somewhere in Randolph county. In farther confirmation of this, Dr. Stevenson mentions the curious fact that in that county the upper Umbral shales, at one point, thin out entirely, and the conglomerate is in contact with the limestone.

In Ohio, the reports of that State show that the conglomerate has become too thin to form a continuous stratum.

Proceeding southwest, from Ohio into Kentucky, we find the conglomerate series forming the west outcrop of the east Kentucky coal field, being the sub-conglomerate coals of that State. Mr. Joseph Lesley, (Proc. Am. Phil. Soc., No. 91,) in his account of this outcrop belt, shows that in that quarter the Umbral shales are entirely wanting, and that the coals under the conglomerate lie immediately on the sub-carboniferous limestone. He traces this outcrop from Carter county southwest to Clinton county, on the south border of the State. The thickening of the series in that direction shows plainly that his line of investigation diverged from the edge of the basin and approached nearer and nearer toward the central portions. He states that the series consists of two members, the upper one a conglomeratic sandstone, and the lower one a coal-bearing portion. The upper member thins in proceeding southwest, while the lower

* If Professor Tyson's section of the Cumberland basin be correct, then it is clear that northeast of Randolph Co., in Maryland, the conglomerate again has coal in its central portion. He gives on Savage River, above the Umbral shales, and including the so-called "Coal-Measure Conglomerate," a thickness of 451 feet. In this space he places three coal beds, two feet, two feet six inches, and two feet thick. The entire mass is begun and ended with massive sandstones, having a similar structure to the New River field.

member thickens at a more rapid rate. At Grayson, in Carter county, both together are only 90 feet thick, and the lowest coal is a mere streak formed between the base of the formation and the underlying limestone. Farther southwest, in Morgan county, the upper member is 150 feet thick, and the lower member only eight feet, and contains a twelve-inch bed of coal. In Estell county, the upper member measures 200 feet and the lower fifty feet, with the coal bed increased to twenty-seven inches. On the southern border of the State, the lower member increases to 225 feet, and contains two workable and three other thin beds of coal. The upper member does not now exceed eighty feet. This upper member is No. 21 of the Raleigh section.

Passing into Tennessee, we learn from Safford that the western outcrop presents the same essential features as in Kentucky. The red rocks of the Umbral are wanting, and the coal system rests on the sub-carboniferous limestone. But in Tennessee the thickness attained on the south border of Kentucky does not seem to be maintained in the counties immediately south of that point. Here also along the west outcrop in Fentress, White, and Franklin counties, the series is double, consisting of an upper sandstone and a lower coal-bearing portion. Of this latter Safford says, "It consists of shales and sandstones, the latter sometimes absent, and ranges from a few feet to about 200." It contains two, sometimes three, rarely more, seams of coal. These are often too thin for mining, but locally swell out and form valuable deposits, from two and a half to four or five feet in thickness."

This thinning out in the direction immediately south of Clinton county, Kentucky, seems to indicate that in Tennessee the west end of the basin sweeps around more to the south. Safford's sections show that these rocks increase in thickness from west to east with an increase of the sandstones, while the coals diminish greatly in the most easterly portions of the field; for while the west outcrop has the character above given, we find at the Ætna mines, a point farther east, a thickness of 563 feet, including the upper conglomerate, and at Lookout Mountain, the most easterly point of the sections given in the southern part of the field, we have 673 feet, composed mainly of coarse sandstone, with hardly any coal. This diminution of coal eastward agrees with the facts given by Professor J. P. Lesley for Wise, Russel, and Tazewell counties, Virginia.

One of the most striking points of difference between the strata shown on New River, West Virginia, and the west outcrop, is the disappearance in the latter of the thick deposits of the red Umbral shales underlying the conglomerate series in West Virginia. This is explained by the fact that these sediments are derived from the incoherent red shales, so abundant in the upper Devonian of the Alleghany region on the east border of the basin. In this upper Devonian we find abundant red shales, which in physical character cannot be distinguished from those of the Umbral. None such are found in the Cincinnati anticlinal on the west border. It would also seem from what has been given above, that the quarter from which the greatest part of the sediment came, was the east, while the coals grew from the west.

If we may draw any conclusions from the data given above, we may believe that the coal-bearing rocks accompanying the conglomerate were formed in a comparatively restricted basin, which commenced in the northeast corner of Alabama, and extended as far northeast as Randolph County, West Virginia. Its deepest part was probably along a northeast line which passes east of the Cumberland Mountains, and crosses New River near the center of Raleigh County. The strata occupying the interval between the Devonian and the lowest of the "Lower Productive Coals" are on New River in this region at least 4,200 feet thick. It is highly probable that the formation of this deep narrow trough caused that intense strain in the crust of the earth which finally resulted in the production of that wonderful system of faults which is found on the east border of the coal-bearing strata described above, and which seems to be mainly developed along this border near the deepest part of the depression.

As to the character of the coal found on New River, in the series in question, it is a semi-bituminous coal, a fair example of which may be found in the seam mined and coked at Quinnimont. An analysis of this seam furnished me by Mr. Morris, shows the following: Carbon 75·89; vol. matter 18·19; ash 4·98; water ·74 The coke contains: Carbon 93·85; ash 6·15; sulphur ·23. This small amount of bituminous matter is noteworthy when we consider the undisturbed condition of the coal beds. This disturbance is no greater than that found in the highly bituminous coals of later age in other parts of the State. We must explain this loss of volatile matter in some other way than by mechanical action.

Plants.—There are only two horizons which have yielded me plants. These are the Quinnimont coal seam, or coal No. 9, and coal No. 5, or No. 14 of the Piney River section. At the locality which I shall give as horizon of coal 5, Quinnimont, this coal bed does not show, owing to the imperfect exposure, but the shales containing the plants occur at the horizon of coal 5.

I cannot pretend to have made anything like an exhaustive search for plants, even at these horizons. I made my explorations on foot, armed only with a small hammer and satchel as the implements for procuring and transporting specimens. On the Raleigh road, and at the locality at Quinnimont, all my material was obtained from the weathered outcrop of the plant-bearing shales. In my second visit to the locality at Sewell Station I was in hopes of making large additions to my stock of the interesting plants found there on a former occasion. I was led to entertain this hope from the fact that in my previous visit I spent only an hour or two in the collection of the plants. But I found on my second visit that the impressions were restricted to a very thin layer in the roof, and that from the small amount of material on the "Dump" but little in addition could be obtained. The opening was inaccessible, being filled with water. It is an interesting fact regarding most of the plants found here, especially the *Megalopteris*, that they were seen nowhere else. The locality on the Raleigh road of the Piney River section, is a very promising one, affording a number of good specimens, in the weathered outcrop.

The following are the plants obtained from this series; some of them were procured in my first visit and were noticed in my former paper.

1. *Sphenopteris Hœninghausi* Brongt. Quite common in large and beautiful specimens with coal No. 5, Raleigh County.

2. *Calamites Rœmeri?* Göpp. Fragments resembling this calamite more closely than any other, were found with coal No. 9, at Sewell Station.

3. *Lepidodendron Selaginoides* Sternb. Good impressions of the bark and leafy branches, are not uncommon with coal No. 5, Raleigh County. I found associated with this plant very small leafy branches closely resembling the figures by Lesquereux, of *Lycopodites Meekii*. They are no doubt small branches of *S. Selaginoides*.

4. *Sphenopteris Adiantoides* Lindl. and Hutt. A single specimen but well marked, was found with coal 5, Raleigh County.

5. *Bornia radiata* Brongt. Found with coal 9, at Sewell Station, and coal 5, Raleigh County.

6. *Odontopteris gracillima* Newb. A single pinnule was found at Sewell Station with coal 9.

7. *Neuropteris, species?* Of this plant I have some detached pinnules, and one pinna with five pinnules, not enough for positive determination. It was obtained at Sewell Station from coal No. 9. The following features are shown in the specimens obtained: Fronds bipinnate; pinnules placed obliquely, and remotely; attached by the central portion of the base; subalternate, 2½ cm. long, 8 mm. wide, margin strongly repand, oblong lanceolate, acute, upper portion of the base rounded obliquely, lower portion forming a short round lobe, midrib slender, but strongly defined, diminishing in size, and near the end, splitting up into nervules, rather flexuous. Side nerves

closely placed, leaving the midrib at a very acute angle, strongly arched so as to meet the sides at a right angle and forking repeatedly.

This plant so far as can be gathered from the imperfect specimens obtained, is of the same type with the *Alethopteris obscura* of the Pennsylvania reports. Fig. 13 of these reports would give a good representation of it if the pinnules were separate to the base, more remote, and inserted on the rachis as above described. The nervation is the principal point of difference. It will be noted that this plant has many features in common with the Mesozoic *alethopterids*.

8. *Cordaites Robbii* Dawson? This plant is rather rare and occurs at Sewell Station with coal 9, and at Quinnimont at the horizon of coal 5.

9. *Alethopteris Serlii?* Brongt. This plant, which is the most abundant one in coal 9 at Sewell Station, differs in some points from *A. Serlii*. The pinnules are more slender, and are decurrent by their lower base which forms a narrow wing, while the upper portion of the base is obliquely cut away so as to cause the midrib to spring from the upper margin, which is barely reached by the decurrent portion of the pinnule next above. The pinnules are usually strongly recurved. In these points it resembles Dawson's *A. discrepans* of the Devonian. It may be a new species.

10. *Calamites cannæformis* Schloth. This is not rare in the slates over coal No. 9, at Quinnimont.

11. *Alethopteris grandifolia* Newb. This plant which is common at the horizon of coal 5 at Quinnimont, at first sight of the upper entire pinnules, resembles No. 9, but in its nervation, which more resembles that of *Neuropteris*, and in other points, it is very different, I have placed this under *Alethopteris grandifolia* from which it is somewhat different, for the same reason that I included No. 9 under *A. Serlii*, being unwilling, without more abundant material, to place these plants apart. The forms of No. 11 seem all to come from the upper part of the frond, and belong to the broad leaved variety of *A. grandifolia*, if they are identical with it. They represent the terminations of fronds or compound pinnæ, showing in their lower parts, pinnæ pinnately cut into pinnules, which become more and more united toward the summit of the frond or pinna; changing first to pinnules, with deeply undulated margins, and finally passing into pinnules with entire borders. These closely resemble the broad leaved variety of *A. grandifolia*, but are relatively narrower, and less united at the base. In their mode of insertion, they resemble to a certain extent No. 9, being somewhat cut away above and decurrent below. In No. 9 the nervation is that of *A. Serlii*, while in the plant in question it

resembles somewhat that of a *Neuropteris* in the fact that the side nerves spring obliquely from the midrib, fork two or three times and curve strongly to meet the margins. The nervation is like that of No. 7, but in the insertion of the pinnules by the entire base, in their decurrence, and other points, this is a true *Alethopteris*.

12. *Neuropteris Lindleyana* Sternb. *Var.* This beautiful *Neuropteris* is common at Sewell Station, where it forms the only plant found at the horizon of coal 5. It also occurs with this coal, in the Piney River section, on the Raleigh road. While in the main point it clearly resembles *N. Lindleyana*, figured in the Fossil Flora of Great Britain, as *N. Löschi*, there are some points of difference. Our plant has not the same degree of sharpness in the terminations of the upper simple pinnæ, and these are not so much narrowed at the base. Again the rounded pinnules of the lower compound pinnæ have a much more slender midrib than that indicated in the figure of the British plant. They are also more closely placed. In the greater bluntness of the upper simple pinnæ, and in their nearer approach to a heart-shaped base, this plant approaches nearer to *N. Loschi*, but it is very different, and may perhaps be best considered as a variety of *N. Lindleyana*.

13. *Neuropteris tenuifolia* Schloth. Found with coal 5 on the Raleigh road, apparently rare.

14. *Sphenopteris, species?* This plant was found with coal 5 on the Raleigh road. The fragments found do not enable me to determine it satisfactorily. It belongs to the Schimper's section, *Sphenopteris cheilanthides*. It is in some respects like *S. Dubuissonis*, but differs in others. The following features are shown in the fragments obtained: Pinnæ oval lanceolate, placed alternately at right angles on the rachis. Pinnules obliquely and alternately placed on the secondary rachis which at the base is narrowly winged and from which they diverge; oblong, narrowing considerably to the summit, and slightly so at the insertion of the base, where they are decurrent. The pinnules diminish rapidly in size from the base to the apex of the pinnæ. At the base they are one cm. long and four mm. wide. The pinnules of the base of the pinnæ are cut obliquely into three oval tooth-shaped lobes on a side, which are remotely placed, and diverge slightly from the midrib. In ascending, the number of the lobes and the depth of their incision diminishes until the pinnules become entire, when they are very small and mere lobes of the wing of the rachis, which at the extremity of the pinnæ is much widened. The plant seems to have leaflets of leather-like consistency; the nervation is obscure

15. *Sphenopteris, species?* This plant, which is not uncommon with coal 5 on the Raleigh road, has a strong resemblance to

Sphenopteris Newberryi, but shows some features not seen in the figure of that plant given in the Pennsylvania reports. From a study of the isolated fragments, in which form alone I could get it, the plant shows the following features: The pinnules of the lower pinnæ have the form of those similarly placed in *S. Newberryi*, but are proportionally narrower at the base, distinctly separate, and more obliquely placed. In ascending, the pinnules of the upper pinnæ are finally reduced to circular segments of the laminæ of the pinnæ, and now if seen apart would be taken to belong to a different plant. These upper pinnæ are placed obliquely and alternately. They are ovate lanceolate, 12 mm. long and 6 broad at base; having the general shape and mode of incision shown in *S. decipiens* of the Pennsylvania reports. But unlike that, the termination of the pinnæ is prolonged into an acute point. Proceeding still higher on the frond, these pinnæ are reduced to semicircular lobes of the broad wing of the rachis which now forms the entire lamina of the upper part of the frond or compound pinna. The nerves are to a great extent masked by the thick leathery character of the leaflets, but, so far as mode out, are as follows: In the lower distinct pinnules there is a strong midrib which disappears before reaching the extremity of the pinnule, and gives it at first sight the appearance of a *Pecopteris*. The side nerves spring very obliquely from the midrib, diverge very slowly from it, curving gently out to the margin, and fork once or twice, being quite distant from each other. In the rounded lobes of the upper pinnae, the nerves rise from the whole base of the lobe curving gently outward, and downward, while forking as before. Here the nervation resembles that of *S. dilatata* as figured in the Fossil Flora of Great Britain. The same nervation marks the extremity of the frond. The most characteristic feature is the rarity of the nerves, and if the plant should prove to be new, would justify the specific name *varinervis*.

16. *Hymenophyllites spinosus* Göpp. ? Fragments were found on the Raleigh road with coal 5, of a plant showing the basal portion of several pinnæ, which seem to be identical with the above named plant. Not enough material was obtanied to identify it with certainty.

17. *Sphenopteris macilenta* Lindl. and Hutt. This plant seems to be abundant on the Raleigh road, associated with coal 5. Good specimens were obtained.

18. *Equisetites, species?* A single sheath, resembling that of an equisetites, was found on the Raleigh road with coal 5.

19. *Asterophyllites acicularis* Daws. ? A specimen showing several whorls of leaves having the character of the above named plant, was obtained from coal 5 on the Raleigh road. Not enough is shown for positive identification.

20. *Trigonocarpon triloculare* Hildreth. A single nut was found at Sewell Station with coal 9. A nut of a different character from any figured or described to my knowledge, was found at the horizon of coal 5 at Quinnimont. It is perfectly smooth with no markings, is about 12 mm. long and 5 mm. wide, cylindrical in shape, and bluntly rounded at the ends, one of which is furnished with curved stem-like appendages, as if for attachment. Nuts are quite rare, only these two being found.

21. *Megalopteris Hartii* Andr. In my last visit to the plant locality at Sewell Station, which furnished me on my former visit the specimens of *Megalopteris*, and which is coal in 9, I procured a few additional specimens of this plant, among which, by comparison with the plates which Professor Andrews has had the kindness to send me, I recognized his *M. Hartii.* Of this plant I have one specimen having the ends of the two leaves at the summit of the frond, showing about six inches of their length. Another specimen shows the termination of a much smaller frond, with three leaves. Along with these leaves I find several of a small *Megalopteris* which may prove a different species although the nervation, so far as it can be made out in the obscure state of all the plants found here, seems to be very near that of *M. Hartii.* Of the small plant, no more than two leaves together have ever been found, and no specimen shows the point of junction of these. The small size seems to be a constant feature. Such leaves are about 6 cm. long and 1 cm. wide; they are narrowly elliptical in shape, with a rather more acute termination than that of *M. Hartii.* The midrib seems to have been large for a leaf of this size, and very prominent. It leaves a deep rectangular impression.

22. *Megalopteris, species?* This plant is the most common form found at Sewell Station. It differs from all the forms of Professor Andrews' plants figured in the decided acuteness of the leaves or pinnules; in which respect it is more like *M. Dawsoni.* From this latter plant it differs in the more decided elliptical outline of the leaves, the more rapid narrowing of the leaves toward their extremities, and most of all, in the nervation, which so far as can be made out, is near that of *M. Hartii*, being fine, from closely placed slender nerves, which fork near the base and again near their middle; apparently, higher up, than in *M. Hartii.* The nerves are nearly parallel in their course; and curve very slowly outward to meet the border of the leaf. I have one specimen which shows one entire leaflet, and the base of another, which diverges from the rachis at the base of the first, showing the ordinary alternate arrangement on a winged rachis, of the leaflets in plants of this genus. The entire leaflet of the specimen, is 14 cm. long, and 2½ to 3 cm. wide. It is strongly

narrowed toward the base, giving it an oblanceolate shape. Near the extremity, it is rapidly narrowed to an acute point. It is most probably a new species, and if so, might receive the specific name *Sewellensis.*

23. *Sphenopteris obtusiloba* Brongt. Found rarely at Sewell Station, with coal No. 9.

24. *Palæopteris Jacksoni* Schimp. *Cyclopteris Jacksoni* Daws. Only one or two small fragments of this plant, were found at Sewell Station with coal 9.

25. *Sphenophyllum antiquum* Daws. A small fragment only was found at Sewell Station with coal 9.

26. *Odontopteris Neuropteroides* Newb. Good specimens of this plant were obtained from the horizon of coal No 5 at Quinnimont, where with the variety of *Alethopteris grandifolia*, it forms the most abundant plant. Some pinnules show an obscure lobing not unlike some of the pinnules of *Sphenopteris Lesquereuxii.* Some scattered broad pinnules, with undulating borders, were found here which, from their appearance, would seem to have belonged to some part of this plant.

27. *Calamites approximatus* Schloth. Found at Sewell Station, with coal No. 9.

The plants above named, with the exception of the few got from Sewell Station previously, and mentioned in my former paper, were all obtained in my last visit to the New River region. As I stated before, this list can by no means be taken as exhaustive of the plants, even at the locality where they were gathered. In no case could I spend more than a couple of hours in collecting, and, having no tools, my collection was made by picking up fragments fallen from the disintegrated outcrop. This was the case with the coal on the Raleigh road, which furnished so many of the above specimens.

The following fossils were obtained from near the top of No. 1 of the transition beds at Quinnimont and consequently just from the base of the conglomerate series. They were kindly determined for me by Dr. J. J. Stevenson. He states that in most cases they were too badly preserved for specific determination.

Invertebrate fossils from the base of the Conglomerate Series at Quinnimont.

1. *Productus cora* D'Orb.
2. *Athyris* sp.
3. *Spirifera Leidyi* N. & P.
4. *Aviculopecten.*
5. *Lima.*
6. *Myalina.*
7. *Macrodon.*
8. *Lithophaga.*
9. *Chænomya?*
10. *Fenestella.*

Of these Dr. Stevenson states that the *Macrodon* is a new species, which also occurs commonly in the middle portion of the

Umbral limestone in Monongalia Co., W. Va. He thinks that the *Myalina* and *Chænomya* are new species. Also that the *Lithophaga* is so near that referred with doubt to *L. lingualis* of Phillips, by M. & W., that he cannot distinguish it, although this is a species of the St. Louis group.

I may state here that the *Palæopteris Jacksoni* of the Conglomerate series, is the typical plant, and very different from the plant found at Lewis Tunnel, and given in my previous paper as *P. Jacksoni.* I have additional specimens from Lewis Tunnel, which show without doubt that, as Professor Andrews has suggested, this latter is a new species.

It will be seen from the above, that the representatives of the Devonian flora of Canada are quite common in the Conglomerate Series. The uppermost strata of the Devonian in West Virginia are at least 3500 feet below coal 5; and still farther below coal 9, which affords the plants of most decided Devonian type. Besides, the forms of *Megalopteris*, the *Palæopteris*, and *Sphenophyllum*, we have probably *Asterophyllites acicularis*, and *Cordaites Robbii*, identical with the Devonian plants. The *Cordaites* has the nervation and termination of the leaves of *C. Robbii*, but I mark it doubtful, as I have no entire leaves. The *Sphenopteris adiantorides*, is a good deal like the plant figured as *Cyclopteris obtusa*, by Dawson in the Acadian Geology, although smaller. The plant marked doubtfully *Alethopteris Serlii*, is very near *A. discrepans.* The *Sphenopteris* allied to *S. Newberryi*, in its upper pinnæ shows the mode of lobing, and has something of the aspect of *S. marginata.*

It will be noted that along with these plants we have some of the forms found in coal No. 1 of Ohio. The upper pinnæ of the plant identified with *Sphenopteris macilenta* I cannot distinguish from Dawson's *Cyclopteris valida.* Besides these, we have plants first found in Felling Colliery, England, associated with the "Low Main coal seam."

ART. XLIX.—*Mineralogical Notes;* by EDWARD S. DANA. No. III.—*On new twins of Staurolite and Pyrrhotite.*

1. ON STAUROLITE CRYSTALS FROM FANNIN CO., GEORGIA.

THROUGH the instrumentality of Prof. F. H. Bradley a large number of staurolite crystals have been recently received in New Haven, some of which show forms which are new and interesting. Prof. Bradley mentions two distinct localities, visited by him, which afford the staurolite in considerable quantities. The first is at Valley River, near Murphy, Cherokee Co., North Carolina. The crystals at this place are large and coarse. They occur in a metamorphic rock which Prof. Bradley states belongs to the Cincinnati group.

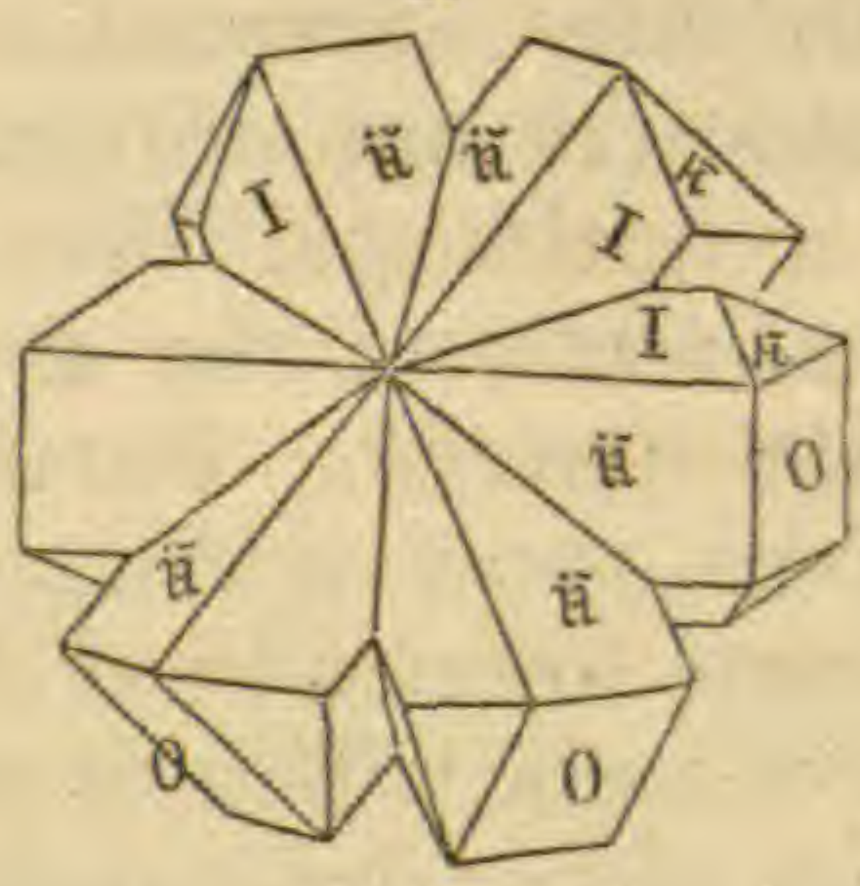
The second locality is in Fannin Co., Georgia, 12 miles southeast of Ducktown, Tenn. The crystals obtained here are of quite uniform size, averaging an inch in length, and are very perfectly developed, the faces being smooth, though usually without polish. They occur in a soft mica schist, belonging geologically, according to the same authority, at the base of the Quebec Group. The rock is extensively decomposed so that the crystals are found abundantly loose on the surface of the ground. Large quantities of them may thus be picked up, of which perhaps one-tenth are perfect crystals. They are in general nearly free from the gangue.

The crystals are with rare exceptions twins. The most common twins are those well known in this species; that is, (*a*), those having the composition face $\frac{3}{2}$-$\bar{i}$, with the vertical axes nearly at right angles to each other; and (*b*), those having the composition-face $\frac{3}{2}$-$\breve{\frac{3}{2}}$, with the vertical axes crossing at a angle of about 60°. Under these two types there is a very wide diversity of form arising from the extension or partial suppression of various of the occurring planes. A very common feature in the twins of the first kind mentioned is the almost entire suppression of one pair of the macrodome planes 1-$\bar{i}$, opposite each other, with the corresponding extension of the other pair; this same kind of hemihedral development is also extended to the prismatic planes giving rise to forms of very oblique appearance.

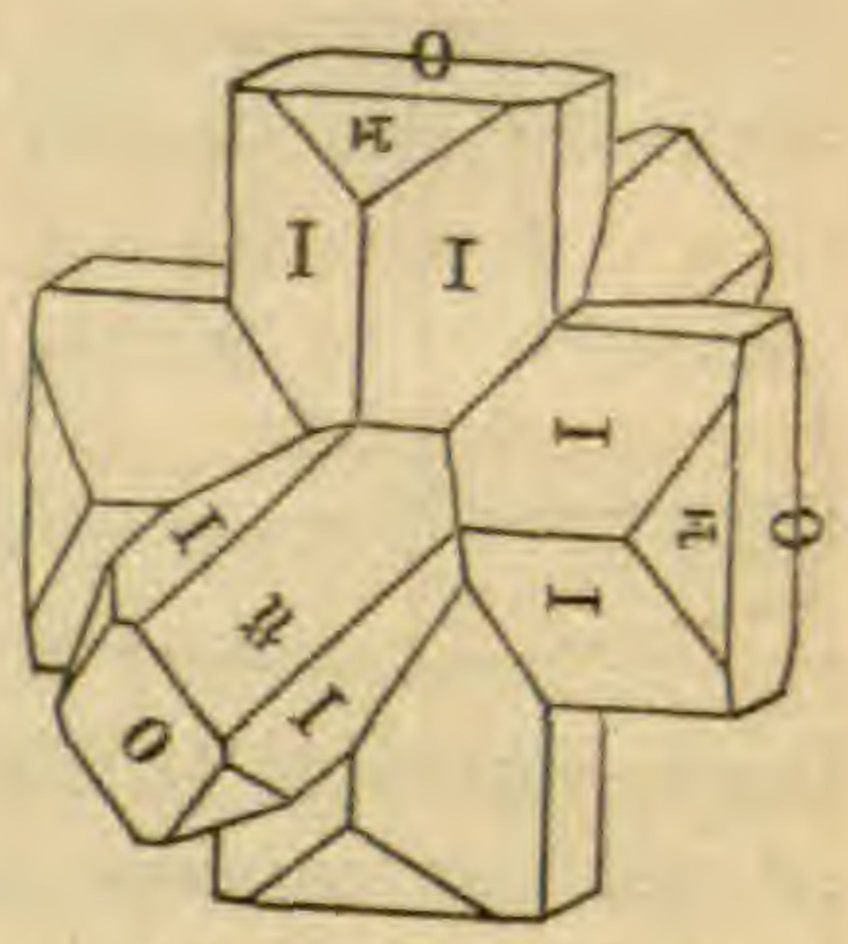
Three new forms are shown in figures 1, 2 and 3. Figure 1 exhibits a new method of twinning not before observed in this

1.

2.



3.



species, and making the third type of penetration twins peculiar to staurolite. The measured angle of the two brachypinacoids (i-$\breve{i}$) for the crystals in their twinning position is 70° 30′. This gives for the composition-face i-$\breve{3}$, which requires for the above angle 70° 18′. In this case the axis of revolution very nearly coincides with the plane i-$\breve{\frac{3}{2}}$ and the i-$\breve{i}$, of the second individual is very nearly parallel to the prism I of the first. Attention has been called, by Prof. Dana, to the fact that if the occurring prism on staurolite be made i-$\frac{3}{2}$, the twinning planes will be then planes of simple axial ratios in accordance with the usual law, namely 1-$\breve{i}$ (instead of $\frac{3}{2}$-$\breve{i}$) and 1 (instead of $\frac{3}{2}$-$\breve{\frac{3}{2}}$). The new method of twinning here described would then have for its composition-face i-$\breve{2}$.

It has already been mentioned that when the twinning plane is $\frac{3}{2}$-$\breve{\frac{3}{2}}$ the vertical axes cut each other at an angle of about 60°. It would be expected from this fact that compound crystals of three individuals would be found, the entire circumference being thus divisible by six, and this has been the case. Figure 2 represents one of a considerable number of crystals of this description in which three interpenetrating individual crystals cut each other respectively at angles of 60° and 120°, and, being symmetrically developed, form thus a six-rayed star. Still another form is shown in figure 3; it is interesting as being a combination of two methods of twinning in the same compound crystal which has been rarely observed in any species. The vertical axis of two of the single crystals are at right angles to each other, and that of the third cuts each of the other two at an angle of 60°. This latter fact explains the occurrence of this form.

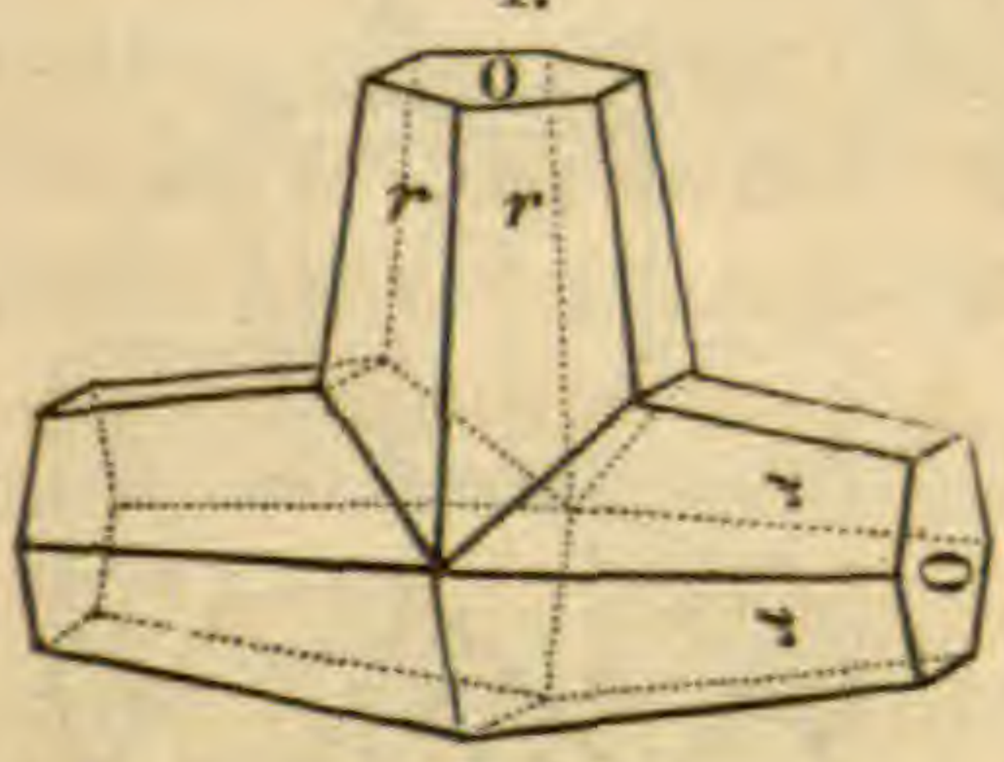
The crystals figured were from the cabinet of Professor Brush.

2. On a twin of Pyrrhotite.

The annexed figure (fig. 4) represents a remarkable crystal of pyrrhotite, which I have been enabled to examine through the kindness of Dr. Harrington of Montreal.

The crystal itself is somewhat more than three times the size of the cut, and is almost as symmetrically formed. The pyramidal planes (r) are uniformly deeply striated in a horizontal direction, parallel to the basal section; the faces are thus made quite uneven. Furthermore these same planes show a considerable number of minute longitudinal depressions, or etchings, parallel to the vertical axis.

4.



This is a prominent feature of all the pyramidal planes alike, thus confirming the accepted hexagonal nature of the species. The transverse crystal, as seen in the figure, is quite irregular, being made up of a group or bundle, of small crystals in parallel position; this is especially true of one of the extremities. These small crystals are some of them free from the striations alluded to and allow of exact measurement. The angle for $r \wedge r$ (basal) thus obtained is 163° ($O \wedge r = 98°30'$); this corresponds to the form $\frac{20}{3}$, which requires 162° 40′ ($O \wedge \frac{20}{3} = 98° 20'$). The twinning plane the pyramid 1, by which the vertical axes of the two individuals are brought nearly at right angles to each other, since $O \wedge 1 = 135° 8'$. This combination of three individual crystals is analogous to the penetration-twins of staurolite, described above.

Dr. Harrington has kindly furnished some notes upon the occurrence and chemical composition of this pyrrhotite, which he allows me to append here.

On the composition and mode of occurrence of the pyrrhotite from Elizabethtown, Ontario; by B. J. HARRINGTON, Ph.D.

The deposit from which the crystal of pyrrhotite was obtained is economically important as being the source from which considerable quantities of pyrite have been derived for the manufacture of sulphuric acid. It occurs on the nineteenth lot of the second concession of Elizabethtown, Ontario, in rocks belonging to the Laurentian system, but its true character has not yet been ascertained. To the mineralogist it is especially interesting on account of the association of minerals which it affords. The minerals number a dozen, and probably more, species, being pyrite, pyrrhotite, magnetite, quartz, talc, labradorite? phlogopite? a compact black mineral probably allied to hisingerite, calcite, siderite, apatite and cacoxenite.

Of these the pyrite and calcite occur in greatest abundance. The former is generally massive, but is sometimes well crystallized—the most common form being a combination of the cube and octahedron. Perfect octahedrons with the axes more than two inches in length have been obtained, and mammillary groupings of cubical crystals with rounded faces occasionally occur. According to the determinations of Hunt and Macfarlane (Geol. of Can., 1863, p. 506, and Can. Nat., 1st Ser., vol. vii, p. 194) the pyrite contains about half a per cent of oxide of cobalt.

Calcite forms the principal gangue in which the other minerals are embedded. It is mostly massive, but is also found on the walls of cavities in the form of obtuse rhombohedral crystals, often curiously modified. It ranges from opaque to transparent, and varies much in color, being white, gray, fawn-colored and sometimes red. The compact black mineral alluded to above also frequently forms the gangue of the pyrites and with it is occasionally associated a triclinic feldspar (probably labradorite) showing a beautiful play of colors. Magnetite is rather common and sometimes occurs in the form of small irregular grains scattered through the calcite. The mineral which I take to be cacoxenite—the occurrence of which in

Canada has not before been noted—is found in beautiful little yellow tufts on the walls of cavities in the calcite, the tufts being often so close together as to form a velvety coating. It is generally associated with pyrite.

Quartz, mica, apatite, talc and siderite were noticed, but they did not form important constituents of the deposit. Pyrrhotite was common in portions of the deposit worked several years ago, but has become less so as the mining has advanced. It is sometimes massive, but more frequently well crystallized. In general it is embedded in calcite, but it has also been found in steatite. The following is an analysis of a crystal:

Iron,	60·560
Copper,	·145
Manganese,	·060
Nickel,	·112
Cobalt,	·111
Sulphur,	39·020
Silica,	·036
	100·044

Hardness between 3½ and 4. Specific gravity 4·622. Readily attracted by the magnet and possessing polarity; the opposite poles seem to be situated not at the extremities of the crystals but along the sides.

A few months ago a crystal of the pyrrhotite was sent to Professor J. Lawrence Smith, who was anxious to compare its composition with that of troilite. The results of his analysis sent to me by him are as follows:

Iron,	59·88
Sulphur,	39·24
Silica (gangue rock),	1·01
	100·13
Specific gravity,	4·642

ART. L.—*Researches on the Solid Carbon Compounds in Meteorites;* by J. LAWRENCE SMITH, Louisville, Ky.

IN the study of Meteorites, it is well known that, of all the simple and compound substances met with in these bodies, the carbon has received the least study and investigation. This has arisen principally from the limited amount of material at the command of the chemist,—a fact to be regretted, since if any one element more than another demands attention, and excites wonder at the part it plays, either as an element or in its endless combinations with other substances, that element is carbon.

In its elementary condition we see it in crystals of exceeding hardness and brilliancy in the diamond, and also in irregular, nearly opaque masses that are not to be confounded with the diamond. Again, we have carbon in a soft, black, unctuous state, either in lustrous flaky crystals, or in fine-grained masses. It also occurs in the harsh and gritty form of coke, sometimes changed to an unctuous body approaching graphite in aspect, yet different physically as well as in some of its chemical relations. Deposits of anthracite furnish carbon in yet another form. Besides these, the results of decomposition of what are known as organic compounds give quite a list of different forms of carbon, made either by the incomplete combustion of hydrocarbons, or by passing through red-hot tubes the vapors of hydrocarbons, chloride of carbon, sulphide of carbon, etc., or by the decompositions of such substances as carbonic acid, carbides of boron, of iron, of manganese, etc.

These various forms of carbon have certain chemical differences, more or less marked, which differences have attracted the attention of chemists, although no one has studied them with much care or success except M. Berthelot, their investigation being difficult on account of the want of proper methods. M. Berthelot obtained his results by taking advantage of the singularly slow oxidizing action of a mixture of nitric acid and chlorate of potash on carbon, first pointed out by M. B. C. Brodie, in 1860,* in experiments on graphite, by which he produced for the first time what is known as graphitic oxide. He operated by this means on very many specimens of carbon, from the diamond to lamp-black, embracing a large variety of artificially prepared carbons, and discovered certainly six or eight more or less distinct chemical characteristics of these different carbons.† The physical differences of some of them are well known; among these differences none is more remarkable than that of their specific heats. Other bodies known as elements, as silicon and boron, oxygen, etc., take upon themselves different conditions called allotropic conditions,‡—a term applied to the isomeric conditions of simple bodies; but carbon differs from these, not only in exhibiting a most wonderful variety of allotropic conditions, but also in the phenomena coming under the head of isomerism, polymerism, and metamerism; so much

* Annalen der Chemie und Pharm., April, p. 6.

† The full detail of his researches is to be found in the Annales de Chimie et de Physique, IV, xix, 392, 1870 and xxx, 419, 1873.

‡ Notwithstanding the recent experiments of M. Weber, showing that under certain conditions, carbon, silicon, and boron are not exceptions to the law of Dulong and Petit, they still occupy a singular position in regard to specific heat.

so, that we are disposed to take this body away from the rank of a mere element, and call it a protean body that gives rise to substances of endless form and variety by combining with a very limited number of elements.

Additional interest attaches to carbon from the fact of its being regarded as belonging preëminently to the organic kingdom. In fact, some of the best observers and investigators assume that there is no such thing as mineral carbon among the rocks of our globe, and that wherever found, whether as diamond, graphite, or coal, it is a product derived from organic matter, in which it had first performed its part in the economy of nature.

A still more exciting interest has been felt in carbon since the new department of celestial chemistry has received the attention of scientists. And here we are not left for our knowledge of celestial carbon to the attenuated form of it which can be detected only by astronomical instruments; for masses of matter from other spheres reach our globe from time to time, bringing with them specimens of solid carbon for our investigation, and, at the same time, perplexing our minds with questions as to its mineral or organic origin, and as to the existence or not of life on other planets, and in other systems of planets.

Like the footprints of former life on the rock strata of our globe, these indications in what we call meteorites, however slight they may be, are not to be disregarded. While I do not wish to arrogate to myself any undue merit in the study of this subject, I must say that I believe that my methods published in 1855* set forth more prominently than it had been done before, the proper method of research for arriving at correct conclusions. It is clear that to attain positive results, the astronomer, physicist, mineralogist, and chemist must not run counter to one another in the use of the facts severally studied by them; and in all that I have done in this direction, it has been my effort to keep this in view.

In the present memoir, it is my object to develope new facts, and consider some points in connection with the carbon of meteorites.

1. *The Carbonaceous Meteorites.*

Certain well known meteorites, from among those whose fall has been observed, have been called, from their aspect, and from their containing a small amount of carbon, *carbonaceous* meteorites, although the small amount of carbon contained in them is not sufficient to account for their color. Perhaps the term *melanotic* meteorite would be a more appropriate one, to distinguish them from the stony and iron meteorites. There

* This Journal, II, xix, 153, 322.

are but four of them yet known, viz: that which fell at Alais in 1806, that at Kold-Bokeveldt in 1838, that at Kaba in 1857, and that at Orgueil in 1864. They contain, respectively, about 3, 2, 0·6, and 6 per cent of carbonaceous matter.

I would here remark that the Alais, Kold-Bokeveldt, and Orgueil are more closely allied to each other than to the Kaba meteorite. The predominating mineral constituents are about as follows:

	Alais, by Berz.	K-Bokt., by Harris.	Kaba, by Wöhler.	Orgueil, by Pisani.
Silica	31·22	30·80	34·24	26·08
Magnesia	22·21	22·20	22·39	17·00
Iron protoxide	29·03	29·94	26·20	29·60

If we now contrast these mineral constituents with those predominating in well-known meteoric stones, a most striking fact presents itself—one not commonly realized by those engaged in the study of these bodies. It is seen on comparing the above with the following tables:

	Chassigny.	Chateau Renard.	Harrison City.	Concord.	Danville.	Searsmont.
Silica	35·30	38·13	47·30	47·30	50·08	40·61
Magnesia	31·76	17·67	24·53	24·53	20·14	36·34
Iron protoxide	26·70	29·44	28·03	28·03	19·85	19·21

From these tabular statements, it will be seen that, deducting the small amount of carbon contained in the black meteorites, the mass of mineral matter constituting them is about the same, and corresponds thus with the so-called common type of meteoric stones; and hence the mineral matter to which these constituents belong must be the same in the two classes of meteorites, viz: olivines and pyroxenes, differing only in the more or less compact form of these minerals.

In the writings of some of the most astute observers of these bodies, we find little stress laid on these facts. Thus, M. Meunier, in a paper on the origin of meteorites, published in the Cosmos of December, 1869, expresses his amazement that I should speak of the circumscribed uniformity of the composition of meteorites as evidence of a circumscribed cosmical origin of these bodies, both with reference to the sphere or spheres whence they come, as well as their rock structure. He takes so opposite a view as to say (p. 9), "So far from the meteorites showing such a resemblance, we can establish between meteoric iron, olivine meteorites, aluminous meteorites, and carbonaceous meteorites, differences as great as between the most different terrestrial rocks." An assertion which would include all the ranges of rocks and sedimentary deposits from the basalt and granite to the cretaceous and tertiary deposits.

Let any one look at the above table, and say whether or not he sees so vast a difference in the mineral constituents of the different meteorites there enumerated; and yet they represent the two extremes of these bodies so far as their external properties are concerned. It is well known that three or four minerals represent the great mass of the constituents of every meteorite in various proportions, viz: nickeliferous iron, olivine, pyroxene, and anorthite, especially the first three; and the purely iron meteorites must be recognized as magnified masses of the metallic particles to be found in every stony meteorite, not excepting even the carbonaceous meteorites.*

My object, however, in this paper is not to discuss at length the general internal resemblances of these bodies, as I may have occasion to do it more fully at another time. I wish simply to note, that black and pulverulent as are the carbonaceous meteorites, they are not removed by their mineral constituents from the so-called common meteorites. I now pass on to show that even in their carbonaceous constituent they are strongly linked even to the iron meteorites.

2. *Graphite carbon in the Iron Meteorites.*—Ever since the internal structure of this class of meteorites has been examined by sections through the center of these compact metallic masses, nodular concretions have been noted in their interior, the most common of which consist of *troilite*, a protosulphide of iron, and filling ovoidal cavities. Sometimes these troilite concretions have a thin coating of a lighter colored mineral known as *schreibersite;* and this last is also found alone in concretionary masses which are usually angular or lamellar.

Less frequent concretions than either of the above, and even more remarkable, consist of carbon of the character of graphite: these, like the troilite, usually fill irregular ovoidal cavities, and are more or less contaminated with the latter mineral.

The most important of the meteoric irons containing these nodules, that have come under my immediate observation, are the Toluca, the Cranbourne, the DeKalb, and the Sevier: the last two have received my special study, the latter furnishing much the larger part of the material in my hands.

Character of the graphite nodules.— These concretions differ more or less in appearance, while their general character is the same. In this communication I call special attention to a large nodule taken from the very center of the Sevier iron, the largest that has come under my observation, and perhaps the largest known. It was detached from the iron entire and perfect in every respect. Its greatest length is 60 mm.; its dimensions in

* At present, the Orgueil and Rhoda meteorites are the only two in which no positive evidence of the presence of nickeliferous iron has been traced; in the Orgueil, however, we find nearly three per cent of oxides, nickel and cobalt, and the Rhoda has not been very critically examined.

the other direction vary from 20 to 35 mm. The weight before it was cut was 92 grams. Its form is that of an irregular dumb-bell, flattened on one side, and slightly nodular on the surface. Its color is plumbago-black, except at small places on the surface, where there is a little bronze-colored troilite. Its texture is remarkably close and compact, and it is cut readily by the saw except when the tool encounters particles of enclosed troilite. Its structure and powder is not unlike that of the close-textured graphite of Borrowdale in Cumberland, England, and quite unlike the scaly graphite such as that from Ceylon, or that found in certain cast irons.

Examined from the circumference to the center this nodule presents the following appearances: About one-fifth of the circumference of the section is made up of troilite with a thickness of one millimeter. The remainder of the section has all the aspect of graphite, except in a few spots. In the nodule there is a small mass of troilite not unlike in form the entire nodule; it is 10 mm. long by about 5 mm. wide; it is not continuous from its circumference to its center, but the center portion is cut off completely from the exterior portion by a thin belt of graphite one-half to three-quarters of a millimeter in thickness. Again on other parts of the surface small particles of troilite are to be seen.

The specific gravity of this graphite is 2·26 mm., as determined on a piece in which no troilite was visible to the eye, and after it was immersed in water and placed under the receiver of an air pump to abstract the air from its pores.

Chemical character of the graphitic nodule.—When pulverized and heated in a short glass tube from 100° to 150° C., water is given off which is doubtless water absorbed from the air by the graphite. If heated a little higher and then brought close to the nose, a slight empyreumatic odor is apparent; if heated still higher, there is a slight odor of sulphuretted hydrogen. If heated in the open air the carbon is burnt with difficulty, showing its true graphitic nature.

Treatment of the graphite by ether.—Very pure and concentrated ether was added to two grams of material in powder and rubbed up in a porcelain mortar; then poured into a small beaker; a little more ether was added and the two allowed to remain together for 12 or 18 hours, the vessel being covered to prevent evaporation. The ether was then filtered off from the graphite which was finally washed with a little ether. The ether was allowed to evaporate slowly in the uncovered beaker placed where the temperature was about 33° C. After the ether had evaporated, long colorless acicular crystals covered the sides of the vessel, and some shorter ones were in the bottom. There were also some rhomboidal crystals and rounded particles. The solid residue exhaled a peculiar odor of an aromatic character, somewhat alliaceous. The quantity of these crystals was small, not exceeding 15 milligrams from two grams of the graphite. Heated on a piece of platinum foil they fuse at about 120° C. Heated in a small tube closed at one end, they first melt and then volatilize, condensing in yellow drops that soon solidify leaving a carbonaceous residue. They are not soluble in alcohol, but very soluble in sulphide of carbon. Fuming nitric acid oxidizes the material, and gives, as one of the products, sulphuric acid. The quantity was too small to admit an ultimate analysis, but it was very evident that sulphur was the predominating constituent, the remainder being carbon and hydrogen. These three elements may be combined, forming a peculiar sulph-hydrocarbon, which in a previous note I called *celestialite*, or it may be sulphur containing a minute quantity of a hydrocarbon that gives the peculiar odor and determines the somewhat singular form of crystallization of the sulphur; for these acicular crystals may be only elongated rhombohedrons.

Be the compound what it may, it is a matter of chemical and astronomical interest that a solid graphite nodule thus encased in iron should contain a sulph-hydrocarbon, or free sulphur and a hydrocarbon.

The graphite powder, after treatment with ether, was then treated with bi-sulphide of carbon (which was re-distilled just before use) and after standing two or three hours was thrown on a filter; the filtrate was evaporated to dryness, and the residue was a yellow solid; in this instance, as in the last, the quantity was small. This, when heated in the open air on platinum foil to a red dull heat, first melts at about the temperature that sulphur melts, and finally the sulphur is burnt off, leaving a carbonaceous residue. When heated in a tube, it sublimes, leaving a black residue.

To all appearances this is the same substance, or mixture of substances, that was extracted by the ether, the ether not having exhausted the graphite in the first treatment.

The graphite nodules of the DeKalb and of the Cranbourne irons, on treatment with ether and sulphide of carbon, gave similar results. In the case of the Cranbourne graphite I had less than one hundred milligrams of the material to operate with, and I hardly hoped to obtain satisfactory results, but I did succeed, however, in obtaining such without the acicular crystals, for the whole residue was less than one milligram; but I had enough to recognize the peculiar odor, and also the minute quantity that could be scraped off the vessel in which the evaporation took place furnished the marked reaction by heat of volatilization in part and condensation of the same with a carbon residue. The Cranborne graphite requires more trituration with the ether than that from the Sevier meteorite as it is more flaky on being rubbed up.

Further remarks about this peculiar substance will be made a little farther on, when I come to speak of the same compound as obtained from the black or carbonaceous meteorites.

[To be continued.]

ART. LI.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXVIII.—*On the Oxidation product of Glycogen with Bromine, Silver Oxide and Water;* by R. H. CHITTENDEN, Ph.B., Assistant in Physiological Chemistry.

WHILE submitting an aqueous solution of glycogen to the action of bromine in an open vessel with the aid of heat, it was observed that the strong opacity of the fluid gradually disappeared, and that, after the removal of the free bromine by partial evaporation, a perfectly clear fluid remained which contained considerable combined bromine.

This reaction, indicating union between the glycogen and bromine, pointed to the possibility of the formation of an acid from the glycogen by oxidation, in a manner analogous to the formation of "dextronsäure" from dextrin, and "lactonsäure" from lactose, as described by Habermann,* Barth and Hlasiwetz.† The following experiments were undertaken to form, if possible, a corresponding acid from glycogen. The glycogen employed was prepared from the muscular tissue of *Pecten irradians*,‡ and was as pure as could be obtained. The process of oxidation was as follows: fifty grams of glycogen dried at 100° C., were dissolved in 300 c.c. of distilled water, and this solution transferred to a champagne flask fitted with a caoutchouc stopper, in which was a small stout glass tube drawn out to a point.§ Forty grams of bromine were then added and the stopper wired in. The flask was then heated in a water bath until the red vapors of bromine had entirely disappeared, which required about two hours' boiling. A heavy light yellow or white precipitate formed at first, which completely disappeared by the time the bromine had all been taken up. At the end of this first treatment the fluid was perfectly clear and of a pale yellow color. After cooling, the gases were allowed to escape, by breaking the end of the tube in the cork, and, being collected, were found to consist mainly of carbonic acid and bromoform. The stopper was then removed and 40 grams more bromine

* Annalen der Ch. u. Pharm., clxii, 297. † Ibid., cxxii, 96.
‡ This Journal, III, vol. x, p. 26. § Annalen der Ch. u. Pharm., clix, 315.

added. The flask was then closed and heated as before until the bromine had all been taken up, after which, on cooling, the gases were liberated and 40 grams more bromine added, and the flask treated as before. After this, 10 grams of bromine were added three times, so that 150 grams of bromine were employed in the oxidation of 50 grams of dried glycogen.

The fluid was then transferred to an evaporating dish and heated on a water-bath until somewhat concentrated. When cool the fluid was diluted with an equal volume of water, then mixed with freshly precipitated and thoroughly washed silver oxide until all bromine was removed from the fluid. After the silver bromide had completely settled, the fluid was filtered off and the silver contained in it precipitated by hydrogen sulphide. The silver sulphide was removed by filtration, and the filtrate upon partial evaporation left a yellowish-red fluid with strong acid taste and reaction. This was an impure solution of an acid which decomposed carbonates with avidity. In this manner 150 grams of dried glycogen were oxidized, in parts of 50, giving in all sufficient acid for the following experiments. Two methods of purification were employed. The first consisted in treating this impure solution of the acid with chemically pure animal charcoal, and precipitating the filtrate with an excess of alcohol, to remove inorganic salts derived from the glycogen, which the latter always contains in small quantity. This alcoholic filtrate is evaporated on the water-bath, when a moderately pure solution of the acid results.

The second and better method, however, is to treat the impure acid with pure calcium carbonate, on the water-bath, when a soluble calcium salt is obtained which is filtered off, and, after concentration, crystallizes out on standing several days. After washing the crystals with a little cold water, they are dissolved in a large quantity of hot water, and precipitated while still hot by basic lead acetate. This precipitate of a lead salt of the acid is washed with hot water, then emulsionized with water and decomposed by hydrogen sulphide. The lead sulphide is removed by filtration, the fluid evaporated, and then mixed with an excess of dilute alcohol. The precipitate, if any forms, is filtered off, and the filtrate on evaporation leaves the pure acid as a thick colorless syrup, which, after standing several months, shows as yet no signs of crystallization. A solution of the acid in water has an acid reaction on litmus; a strong acid taste; is not precipitated by alcohol, and dissolves freshly precipitated hydrated copper oxide to an azure-blue fluid, which remains blue when heated, but after long boiling shows strong reducing action.

Calcium Salt.—On treating an aqueous solution of the acid with calcium carbonate, on the water-bath, a violent evolution of carbonic acid takes place, and, after some time, a soluble calcium salt can be filtered off from the excess of calcium carbonate. If the solution is at all colored it can be purified by animal charcoal. When suitably concentrated, this solution on standing several days, changes into a mass of irregular white globules, which are aggregations of fine microscopic needles.

The air-dried salt, when heated to 100° C. loses only hygroscopic water, and, when heated above 100° C. turns brown, which would indicate decomposition. The salt after crystallization is difficultly soluble in cold water; readily soluble in hot water, and is precipitated from its aqueous solution by alcohol.

The analysis of the salt gave the formula: $C_6H_{11}Ca'O_7$.

I. 0·2097 grams of the salt dried at 100° C. gave ·2552 grams CO_2 and ·1015 grams H_2O.
II. 0·317 grams of the dried salt gave ·3851 grams CO_2 and ·1508 grams H_2O.
III. 0·3305 grams of the dried salt gave ·0429 grams CaO.
IV. 0·272 grams of the dried salt gave ·036 grams CaO.

	Calculated.	Found.			
		1.	2.	3.	4.
C_6	33·49	33·18	33·12	----	----
H_{11}	5·12	5·37	5·28	----	----
Ca'*	9·35	----	----	9·27	9·45
O_7	52·05	----	----	----	----

Barium Salt.—On treating a portion of the acid on the water-bath with barium carbonate, the latter salt is decomposed, and a soluble barium salt of the acid results. The salt thus formed does not crystallize readily from this solution. Alcohol is then added in excess to the fluid, when a heavy white precipitate forms, flocculent at first, but soon becoming gummy. This precipitate is washed with alcohol, and after drying has the appearance of a hard yellow gum. The gum-like mass is dissolved in water, filtered through animal charcoal, and evaporated to a small bulk, when, after standing a week, the fluid is converted into a mass of quite large, white, glassy prisms, which contain water of crystallization.

The analysis of the salt dried at 100° C. gave the formula: $C_6H_{11}Ba'O_7$. The air-dried salt gave the formula: $C_6H_{11}Ba'O_7+1\frac{1}{2}H_2O$.

I. 0·347 grams of the salt dried at 100° C. gave ·3488 grams CO_2 and ·1402 grams H_2O.
II. 0·2778 grams of the dried salt gave ·2793 grams CO_2 and ·1102 grams H_2O.
III. 0·498 grams of the dried salt gave ·1872 grams $BaCO_3$.
IV. 0·3117 grams of dried salt gave ·1175 grams $BaCO_3$.
V. 1·2257 grams of the air-dried salt gave by drying at 100° C. ·11542 grams H_2O.

* Ca' = 20.

	Calculated.	Found.				
		1.	2.	3.	4.	5.
C_6	27·32	27·41	27·41	----	----	----
H_{11}	4·17	4·48	4·40	----	----	----
Ba′*	25·99	----	----	26·14	26·21	----
O_7	42·54	----	---	----	----	----
$1\frac{1}{2}H_2O$	9·64	----	----	----	----	9·41

The crystals of this salt are very readily soluble in hot and cold water, but insoluble in alcohol.

The air-dried crystals when placed over concentrated sulphuric acid lose 6·42 per cent of water. The calculated amount for one molecule of water is 6·74 per cent. On drying the crystals at 100° C. the remaining one-half molecule is driven off. Heated at 120° C. the crystals turn brown and swell up.

Cadmium Salt.—On treating an aqueous solution of the acid with cadmium carbonate, on the water-bath, a soluble cadmium salt is obtained which does not crystallize. The salt is precipitated from its solution by three or four volumes of alcohol, then redissolved in water, filtered through animal charcoal, and reprecipitated by alcohol. It is thrown down from its solution as a flocculent precipitate which soon becomes gummy, and when hard yields on trituration a perfectly white powder.

The analysis of the salt dried at 100° C. gave the formula: $C_6H_{11}Cd'O_7$.

I. 0·3924 grams of the salt dried at 100° C. gave ·4105 grams CO_2 and ·1618 grams H_2O.
II. 0·3544 grams of the dried salt gave ·3702 grams CO_2 and ·1412 grams H_2O.
III. 0·344 grams of the dried salt gave ·0989 grams CdS.
IV. 0·3568 grams of the dried salt gave ·1023 grams CdS.

	Calculated.	Found.			
		1.	2.	3.	4.
C_6	28·68	28·52	28·48	----	----
H_{11}	4·38	4·58	4·42	----	----
Cd′†	22·31	----	----	22·36	22·29
O_7	44·70	----	----	----	----

Cobalt Salt.—On heating an aqueous solution of the acid with cobaltic carbonate, on the water-bath, a cherry-red solution of a cobalt salt of the acid, is obtained, which does not crystallize readily from the aqueous solution. On the addition of alcohol to a concentrated or only moderately dilute solution of this salt, a heavy pink colored precipitate forms which soon becomes gummy.

This precipitate, after being washed with alcohol and dried at 100° C. gave by analysis the formula: $C_6H_{11}Co'O_7$.

* Ba′=68·5. † Cd′=56.

I. 0·279 grams of the salt dried at 100° C. gave ·325 grams CO_2 and ·1213 grams H_2O.

II. 0·283 grams of the dried salt gave ·3325 grams Co_2 and ·1200 grams H_2O.

III. 0·2005 grams of the dried salt gave ·0268 grams Co.

	Calculated.	Found. 1.	2.	3.
C_6	32·07	31·76	32·04	----
H_{11}	4·89	4·83	4·71	----
Co′*	13·14	----	----	13·36
O_7	49·89	----	----	----

From a very dilute solution in water, the cobalt salt is precipitated by alcohol in the form of pink flocks. On allowing this precipitate to stand several weeks in the alcoholic fluid, it will be found to have changed its form, and under the microscope, will be seen to consist of fine needle-shaped crystals. These crystals dried at 100° C. gave by analysis the formula: $C_6H_{11}CoO_7+H_2O$.

I. 0·219 grams of the salt dried at 100° C. gave ·2372 grams CO_2 and ·108 grams H_2O.

II. 0·2992 grams of the dried salt gave ·3225 grams CO_2 and ·1484 grams H_2O.

III 0·2052 grams of the dried salt gave ·0254 grams Co.

	Calculated.	Found. 1.	2.	3.
C_6	29·69	29·53	29·40	----
H_{13}	5·36	5·47	5·51	----
Co′*	12·16	----	----	12·37
O_8	52·78	----	----	---

Manganese Salt—A solution of the acid treated, as in the preceding methods, with manganic carbonate, forms a soluble manganese salt which separates from the suitably concentrated fluid in masses of fine microscopic feather-like crystals. These crystals upon close examination are seen to be made up of radiating needles. When agitated in water they have a brilliant silky luster. They are slightly yellow, soluble in water, but insoluble in alcohol. The salt dried at 100° C. gave by analysis the formula: $C_6H_{11}MnO_7$.

I. 0·305 grams of the salt dried at 100° C. gave ·362 grams Co_2 and ·142 grams H_2O.

II. 0·353 grams of the dried salt gave ·062 grams Mn_3O_4·

	Calculated.	Found. 1.	2.
C_6	32·35	32·36	----
H_{11}	4·94	5·17	----
Mn†	12·35	----	12·65
O_7	50·33	----	----

* Co′=29·5. † Mn′=27·5.

Lead Salt.—On treating an aqueous solution of the calcium salt with basic lead acetate, best with the application of heat, a heavy white gelatinous precipitate is obtained, which after washing with hot water and drying at 100° C., yielded by analysis the formula: $C_6H_8Pb_2O_7$.

I. 0·280 grams of the salt dried at 100° C. gave ·115 grams CO_2 and ·0358 grams H_2O.
II. 0·3855 grams of the dried salt gave ·1645 grams CO_2 and ·0505 grams H_2O.
III. 0·436 grams of the dried salt gave ·3212 grams PbO.
IV. 0·1522 grams of the dried salt gave ·1122 grams PbO.

	Calculated.	Found.			
		1.	2.	3.	4.
C_6	11·88	11·20	11·63	----	----
H_8	1·32	1·42	1·45	----	----
Pb''_2	68·31	----	----	68·38	68·43
O_7	18·49	----	----	----	----

On adding a solution of neutral lead acetate to an aqueous solution of the acid, a white flocculent precipitate is obtained. A similar precipitate is obtained with basic lead acetate. These precipitates, after washing with water and drying at 100° C., gave by analysis the following results:

Precipitate produced by neutral lead acetate.		Precipitate produced by basic lead acetate.	
C	11·60	C	11·42
H	1·42	H	1·55
Pb	68·19	Pb	68·66
O	18·79	O	18·37

A silver salt was also obtained as a flocculent precipitate. This was not analyzed.

By a backward glance we see that in all the salts obtained, with the exception of the lead salt, the acid acts as a monobasic acid. In the case of the lead salt, however, four atoms of hydrogen are replaced by two atoms of the metal. Hlasiwetz in an article upon the basicity of "lactonsäure" and "gluconsäure,"* in which he shows that these acids are not only monobasic but also dibasic, gives a method whereby he obtained a dibasic barium salt from a monobasic calcium salt. On treating an aqueous solution of the monobasic calcium salt of this acid in the same manner, viz: with baryta water, and heating to boiling, a white flocculent precipitate is obtained, which after washing with hot water and drying at 100° C. gave by analysis the formula: $C_6H_{10}Ba''O_7$.

I. 0·282 grams of the salt dried at 100° C. gave ·2192 grams CO_2 and ·0905 grams H_2O.
II. 0·1585 grams of the dried salt gave ·1235 grams CO_2 and ·0505 grams H_2O.
III. 0·3255 grams of the dried salt gave ·192 grams $BaCO_3$.

* Annalen der Ch. u. Pharm., clviii, 253.

	Calculated.	Found. 1.	2.	3.
C_6	21·75	21·19	21·24	----
H_{10}	3·02	3·56	3·54	----
Ba″	41·38	----	----	41·01
O_7	33·83	----	----	----

The formation of this salt, together with the lead salt, shows that from this acid both monobasic and dibasic salts can be formed.

The formula of the acid is $C_6H_{12}O_7$ and in the oxidation of the glycogen we can assume that the following reactions take place: $C_6H_{10}O_5+H_2O+2Br=C_6H_{12}O_6Br_2$. Adding silver oxide to the bromine compound we have:

$$C_6H_{12}O_6Br_2+Ag_2O=C_6H_{12}O_7+2AgBr.$$

From analogy, it would seem proper to apply to this acid the name glycogen acid.

The preceding analyses and reactions show conclusively that by the action of bromine, water and silver oxide on glycogen, an acid is formed which bears the same relation to glycogen as "dextronsäure" to dextrin. On comparing this acid with the descriptions of "gluconsäure"* and "dextronsäure,"† we see that the glycogen acid differs from the two no more than the two differ from each other. There is also the same relationship existing between glycogen acid and the acid or acids obtained by the oxidation of amylum and paramylum‡ by Haberman, which latter show but few points of difference from "gluconsäure" and "dextronsäure."

February 26th, 1876.

ART. LII.—*On the existence or not of Horns in the Dinocerata;* by RICHARD OWEN. (Letter to the Editors of this Journal, dated London, Feb. 24, 1876.)

GENTLEMEN: Among the new forms of extinct Eocene mammals of America, for which science is indebted to Professor O. C. Marsh, those which he refers to "the new order *Dinocerata*" are the most singular.

The study of their characters, especially as described and illustrated in your Journal,§ has led me to submit a few remarks on the subject of "horns." These weapons in mammals, if formed or supported by bone, are either "autogenous" or "exogenous," either "epiphyses" or "apophyses;" terms which signify, in a word, either, that the horn is ossified from an independent center and

* Annalen der Ch. u. Pharm., clv, 120. † Ibid., clxii, 297. ‡ Ibid., clxxii, 11.
§ Vol. xi, February, 1876, p. 163.

afterwards coalesces with a cranial bone, or that it grows, as a process, from a cranial bone.

The giraffe yields an instance of the "autogenous" horn, and its skull, in either the recent or fossil state, shows the basal sutures. In other species the horns or horn-cores are "exogenous;" and such, in the absence of the sutural evidence, are the parts called "horn-cores" in the *Dinocerata.*

But before elevations or processes of cranial bones can be pronounced to be "horn-cores," the evidence of the horns they supported should be forthcoming. Paleontology, it is true, infers the existence of horns supported on bony bases, or "horn-cores," in extinct species in which such horns have perished. *Bos antiquus*, *Bison priscus*, *Sivatherium*, *Bramatherium*, are rightly referred to the "hollow-horned" group, and the two latter may seem more germain to the present question, seeing that the "horn-cores" are in two pairs. Such conclusion is based on the presence of foramina and ramified grooves upon the surface of the "cores," which are known to be the effects of the penetration and pressure of blood-vessels supplying the growth and renovation of the horny sheaths of such bony processes. The same evidence reveals the true nature of the horn-cores, which may be covered with skin instead of horn, such as are the horns of deer, from which when complete the skin is shed.

In the absence of such evidence the paleontologist infers that smooth unfurrowed protuberances or processes of cranial bones were covered, like the rest of the outer surface of the bones developing them, with persistent periosteum and skin, in the existing animal. He refrains from calling them "horn-cores," and from defining the extinct species manifesting them, as "horned," "four-horned," or "six-horned," *Dicerata*, *Tetracerata*, *Hexacerata ;* or, as in the case of the hornless herbivores of the Wyoming Eocene, *Dinocerata :* because such terms imply the possession by those extinct quadrupeds of weapons of which there has not, at present, been given any evidence.

Professor Marsh, indeed, candidly admits in regard to the protuberances which suggested the generic name *Dinoceras*, that they "may possibly have been covered with thick skin and not with true horn."† But we have no evidence of the integument having been thicker, or other on the protuberances than on the cranial bones developing them.

It may be noted that the hornless exceptions in the group of existing herbivorous quadrupeds with true horn-cores and horns, the hornless *Moschidæ*, e. g., are furnished with other weapons of defense, a pair, namely, of long, edged, and sharp-pointed canines descending from the upper jaw.

The hornless *Dinoceras* was similarly armed, and Professor Marsh believes he has evidence of a sexual difference of size in those dental weapons, which would yield another analogy to the existing Musk-deer. But the dental and osteal characters of the

† Loc. cit., p. 164.

pentadactyle *Dinoceras* are consistently "perissodactyle." The truly remarkable peculiarity of its skull is the tendency of the outer wall of the bones to extend into ridges and bosses; and this not only in the cranium proper and upper jaw, but also in the lower jaw. If these bosses were legitimately interpretable as "horn-cores," we must give the animal a pair of horns descending from the under and forepart of the mandible to match the pair ascending from the maxilla. But the singular processes descending and diverging, as a pair, from the mandibular rami, show as marked an absence of any indication of their having been sheathed with horn as do the pairs of protuberances from the nasal, maxillary, and the frontal bones above.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Diplometer.*--M. LANDOLF has invented an instrument for measuring the diameter of objects without touching them and independently of their movements. A wedge-shaped piece of glass is cut in two along a plane perpendicular to the edge of the wedge, and joined together again after turning one piece 180°. Looking through the line of junction of the prisms, objects will appear double, because the prisms will deviate the rays in opposite directions. When the two images appear just in contact the doubling will be just equal to the diameter of the object. Hence knowing the distance we can compute the diameter, or *vice versa.* The prisms slide over a graduated rod so that the object being placed at one end they are moved until the two images are just in contact, when the distance furnishes a ready means of determining the diameter. In the instrument actually constructed a distance of 42 mms. corresponded to an overlapping of 1 mm. Consequently tenths of a millimeter were readily measured. Evidently motions of the object do not affect the measure since both images move together.—*Comptes Rendus*, lxxxii, 424.

[Numerous applications of this instrument will suggest themselves. In natural history the dimensions of various parts of animals or plants, whether large or small may be found, and in physics objects which cannot be touched, as bubbles, vibrating bodies, &c., may be quickly measured. By setting the prisms as an eye-glass this instrument would form a convenient substitute for a telescope, in measuring distances with a telemeter.] E. C. P.

2. *Specific Heat of Gases.*—M. WIEDEMANN has published in full his measurements of the specific heat of gases referred to in a recent number of this Journal (cviii, 465). His results are given in the following table in which the first column gives the name of the gas, and the second, third and fourth its specific heat under constant pressure at temperatures of 0°, 100° and 200°. The last

three columns give the corresponding specific heats under constant volume:

Name.	Constant Pressure.			Constant Volume.		
	0°	100°	200°	0°	100°	200°
Air,	·2389			·2389		
Hydrogen,	3·410			·2359		
Carbonic oxide,	·2426			·2346		
Carbonic acid,	·1952	·2169	·2387	·2985	·3316	·3650
Ethyl,	·3364	·4189	·5015	·3254	·4052	·4851
Nitrous oxide,	·1983	·2212	·2442	·3014	·3362	·3712
Ammonia,	·5009	·5317	·5629	·2952	·3134	·3318

—*Pogg. Ann.*, clvii, 1. E. C. P.

3. *Crooke's Radiometer.*—Mr. G. J. STONEY presents an explanation of the apparent repulsion produced by heat, according to the Kinetic theory of gases. Mr. Crookes has shown that the pressure produced on a blackened surface of two square inches by the light of a standard candle six inches distant would be ·001772 grains or somewhat less than ·01 milligram per square centimeter. Assuming that the pressure of the air in the interior is reduced to ·1 mm. there would still be something like a hundred million of millions of atoms in each cubic millimeter. These atoms will consist in part of oxygen and nitrogen from the air, of mercury and hydrocarbons, and probably in part of platinum, glass and other substances in a gaseous form.

The blackened surface will be heated by the candle more than the glass by an amount which may be assumed at ·1° and the air in contact with it will vary in temperature from that of the disk to that of the enclosing air. Were the air at its ordinary pressure the heated layer would be very thin, and may be estimated at about ·0005 mm., or about the wave-length of green light. With the small pressure here employed, however, the case is quite different, and the thickness of the layer would equal $·0005 \times (7600)^{1·33}$, or over a decimeter. The heated layer therefore extends to the wall of the surrounding vessel, and now a heat engine is formed by the particles of air which strike the disk with a velocity due to a temperature of perhaps 15°, and are repelled from it with a velocity due to its temperature of 15·1°. The resultant pressure on the disk may be readily computed and is found to be ·0115 milligram which agrees closely with ·01, as observed by Mr. Crookes. In other words a difference of temperature of ·1° C. is sufficient to account for the observed pressure.—*Phil. Mag.*, l, 177.

[The measurement here referred to, is described in *Engineering*, Feb. 18th, and was effected in the following manner: A torsion balance was constructed with a horizontal glass fiber and with a horizontal arm terminating in a cup at one end and in a disk of pith at the other. A small piece of iron weighing a hundredth of a grain was raised by a magnet and dropped into the cup. It was then found that the fiber must be turned through 100021° to bring the arm back to its original position. The light of a candle at a distance of 6 inches was next allowed to fall on the pith, when a torsion of 1775° was required to bring it back. This corresponds to ·001772 grains, or about an eighth of a grain per square foot. From this it would appear that the light of the sun would be equivalent to 32 grains per square foot or 57 tons per square mile. Mr. Crookes further applies this instrument as a photometer and suggests its application to observatories to determine the total amount of sunlight received during the year. The number of revolutions could be counted by attaching a magnet to the radiometer which should act on a magnetic needle moving a counter outside of the glass case. Similarly the power of the instrument might be transmitted through the glass without the usual loss by friction.

Numerous other articles appear on the same subject. Poggendorff and Neesen (Bibl. Univ., ccxvii, 84, and Phil. Mag., l, 251) publish independent series of experiments from which they conclude that the repulsion is due to convection currents. Several articles appear also in 'Nature,' on the radiometer; on p. 391, Mr. Crookes shows that the repulsion is inversely as the square of the distance and compares the effect of rays of various wave lengths. On page 324, Mr. Hutchinson states that a radiometer with mica vanes on metallic supports revolves more rapidly with dark heat than with light, but Mr. Crookes replies that pith should be used as the absorbing substance, since metals give erratic results.] E. C. P.

4. *The Gram Magneto-electric machine.*—M. TRESCA has made a careful measurement of the power required to drive a large and a small gram machine and compared the result with the light generated. A photometer disk was used, of which one portion was illuminated only by the electric light and an adjacent portion only by a carcel burner consuming 40 grams of oil per hour. Much trouble was experienced from the difference in color of the two lights, and the equality was best obtained by interposing two plates of glass, one of light green and the other of light pink. Owing to irregularities in the carbons the light continually underwent irregularities sensible only to the photometer. The light of the larger machine was placed 40 meters from the disk and the burner moved until the square of their distance should be as 1850:1, which was about the mean ratio of the two lights. When the two portions of the disk appeared equally bright the observer gave a signal and instantly the power and velocity were observed. The larger machine had a length of 80 cms., width 55 cms., and height 58·5 cms. The average number of turns per minute was 1274, and the work 576 killogrammeters or 7·68 horse-power. The light being 1850 burners, would equal ·415 of a horse-power per 100 burners, or ·31 kgms. per burner.

The smaller machine had a length of 65 cms., breadth of 41 cms., and height of 50·6 cms. It made 872 turns per minute, and gave a light of 302·4 burners. This required 211 kgms., or 2·8 horse power, equivalent to ·92 of a horse power per hundred burners, or ·69 kgms. per burner.

The consumption of oil to produce a light equal to that of the larger machine would be about 71 kgs. per hour or 194 cubic meters of gas. The cost of the oil would be therefore in Paris about a hundred times that of the electric light, or that of gas fifty times, to produce the same light. The comparison with the smaller machine would be less favorable. The carbons for the larger light had a cross section of 81 mms., and the ordinary consumption was a little over a centimeter in length per hour.—*Comptes Rendus*, lxxxii, 299. E. C. P.

5. *Effect of increase of temperature on the Index of Refraction;* by Professor T. P. MENDENHALL. Letter to the editors, dated Columbus, Ohio, April 10, 1876.—Dear Sirs: I have in progress an investigation of the effect of increase of temperature on the index of refraction, which has at this time yielded some results of considerable importance to spectroscopists. In 1858 Messrs. Gladstone and Dale announced as the conclusion of a research upon this question, that in every substance the refractive index diminishes as the temperature increases. I am satisfied that glass at least does not obey this law; that, on the contrary, with it the index increases with the temperature. In my experiments I have used equilateral glass prisms with indices of refraction of about 1·63. The change in the position of the D line has been observed with a parallel wire micrometer. That the effect was not due to a change in the angle of the prism during the process of cooling I satisfied myself—both by measurement of the angle when hot and when cold—and by receiving the image of the slit of the collimator reflected from both faces upon the cross hairs of two telescopes properly adjusted upon the instrument. No appreciable change in angle could be discovered. Numerous experiments agree well in fixing the "index of sensitiveness," but the quantitative results I have not fully worked out. I only wish at present to direct attention to the fact, that, in the use of a train of several glass prisms in a spectroscope, ordinary changes of temperature to which the instrument may be subjected will produce a very noticeable change in the position of the spectrum lines. In my own, of five large prisms, of an angle of 64°, the change in the position of the D line on removing the prisms from an open window, the temperature outside being about 32° F., to the room, at ordinary temperature was as much as 95 divisions of the micrometer screw head. With a smaller number of prisms the change was closely proportioned to that number. I wish to suggest that in this way may be found the cause of many discrepancies which occur in tables of wave-lengths, as furnished by different workers, in such cases as those in which the dispersion spectrum has been made use of, and the wave-length computed by interpolation. A great many such cases occur in Watt's Index of spectra. In an instrument of many prisms the observation of temperature will be a matter of vital importance in fixing the exact position of a line. I propose to pursue the investigation, especially in respect to the observation of lines more or less refrangible than the D, and also as to the effect of change of temperature upon other than glass prisms.

II. Geology and Mineralogy.

1. *Does the actual vegetation of the Globe furnish any general marks by which it could be recognized in all countries if it became fossil?*—This question is asked by Alph. DeCandolle in a brief article in the Archives des Sciences of Geneva for December, 1875. The question is answered in the negative, as was inevitable. For, as the author observes, the species of plants over the globe differ so widely with difference of locality that it would be exceedingly difficult, or rather, impossible, to draw the line between differences in species due to local distributions, and those due to successional relations. The difficulties, moreover, are greatly increased through the fact, well illustrated by Dr. Gray, that the vegetation of the northern hemisphere has widely changed place during even the Quaternary, and also more than once in earlier time. It hence follows, as DeCandolle urges, that any conclusions as to the succession or cotemporaneity of species in Europe could not be expected to be applicable to America or the other continents; and even the deposits of the several natural regions of a continent would not admit of being synchronized without great doubts over the conclusions. This special inference is not new to geologists; for they admit that with the best of evidence they cannot make out, except very uncertainly, the equivalency of the successive rocks of Europe and America.

But while this general proposition is well sustained, other questions are suggested by the author which appear to demand a reference to a wider range of facts than his paper considers.

Professor DeCandolle seems to regard all fossils as equally poor registers of geological age with plants. It is certain that fossil plants are a most unsatisfactory means of determining equivalency. *Marine* plants—in wonderful contrast with marine animals—have varied little through the geological ages; and hence if plants are used at all for chronological purposes we are confined, with hardly an exception, to the *terrestrial* species. But the terrestrial species, while much more diverse than the marine, include only a very limited series of distinct types, and floras have continued the same or similar through very long ages. Besides, *terrestrial* species, whether vegetable or animal, are more confined in their distribution through physical conditions than those of salt-water; and, further, they are far more poorly represented in the rocks than marine species. For these reasons, and because of the great doubts that come from migrations, the geologist makes little use of fossil plants except for the purpose of characterizing in a general way the floras of the grander divisions of geological time. In actual fact, geologists, in their subdivisions or identifications of formations, have relied almost solely on evidence from fossil animals, and especially marine animals; and if fossil plants are mentioned as the characteristics of a period or age, it has been, with rare exceptions, only after the question of the period or age has been decided by means of other evidence. Evidence from these other sources has its doubts, but it is not of so small value as that from plants. This comparative want of value is well illustrated by the present wide divergence between Paleophytologists and general Paleontologists with regard to the age of the plant-bearing beds of the Rocky Mountains, the Arctic regions, and Europe. An allusion to the uncertainties of Botanical evidence in the Rocky Mountain region may be found on page 149 of this volume.

But Prof. DeCandolle makes the evidence from plants of less value, we think, than is reasonable. He says: No one would dare to assert that during the progress of a given bed of Pennsylvania coal, there did not exist somewhere, perhaps far away, an elevated region less moist, on which Angiosperms were already in existence. The supposition is a forced one. For, in Cretaceous and Tertiary times, Angiosperms were the plants of *moist* lands, their leaves abounding in the coal-formations of those eras; and it is hence natural that they should have abounded in moist places also in the Carboniferous age, if in existence then along with the Acrogens and Gymnosperms.

In the Carboniferous period of North America, the peat-making marshes at times spread from Eastern Pennsylvania to Western Iowa and Arkansas, covering an area of more than 500,000 square miles; and, at the same time, there were dry hills or mountains along the borders of the marshes, in New York, New Jersey, Ohio, Wisconsin, Missouri, Arkansas, through all that long age. The Adirondacks were certainly in existence, and the Green Mountains, and the Highlands of New Jersey, and other ridges or mountains beyond the Mississippi. The area of those Carboniferous marshes with their surroundings was large enough, and varied enough in surface, to have borne a fair representation of the flora of that era of approximately uniform climate; and still the streams from the hills conveyed, so far as yet discovered, no leaves of Angiosperms to the marshes that bordered the hills. The Coal-measures of the Arctic bear similar testimony, whether there by migration or not, and so do those of Europe. Further, Permian, Triassic and Jurassic beds overlie the Coal formation both in America and Europe and have afforded no remains of Angiosperms. It is from facts like these that geologists have been led to infer that the flora of those lands during the Carboniferous age had characteristics distinguishing it very decidedly from that of other ages; and to deem it probable that the precursors of the Angiosperms existed then in a state unlike that of a Cretaceous or modern Angiosperm.

Prof. DeCandolle adds, in the same paragraph, that if fossil Angiospermous plants were found by geologists in any rock "that rock would be at once pronounced of the Cretaceous age," [or of later time]. In reply I only repeat that geologists are very generally convinced that the evidence from fossil plants is not to be trusted, and make the plants of whatever age the fossil *animals* present may indicate. The "Cretaceous" plants of the United States are the plants of beds which had previously been determined, through the animal fossils, to be Cretaceous; and, if geologists finally conclude that the flora of the Lignitic beds is all Cretaceous, it will be done on the ground of the animal relics, and in spite of what has been regarded as good botanical evidence.

While then there may be doubts over chronological conclusions from fossils of whatever kind, the geologist who surveys the whole field finds those doubts less weighty than they would naturally appear to one who looked at the subject from the botanical side alone. J. D. D.

2. *Report of the Geological Survey of Ohio.* Volume II. *Geology and Palæontology.*—Part. II, *Palæontology*, (or, as stated on the cover, Palæontology, Vol. II.) 436 pp. roy. 8vo, with over sixty plates. Columbus, Ohio, 1875.—This large volume contains, after a preface, by Dr. J. S. Newberry, the head of the survey, descriptions of Fossil Fishes, by Dr. Newberry, pp. 1–64; of Silurian Fossils, and of Crinoids from the Waverly group, by J. Hall and R. P. Whitfield, pp. 65–179; of Silurian and Devonian Corals, by H. A. Nicholson, pp. 181–268; of Invertebrate Carboniferous Fossils, by F. B. Meek, pp. 269–347; of Carboniferous Amphibians, by E. D. Cope, pp. 349–411; of Lower Carboniferous fossil plants, by E. B. Andrews, pp. 413–426. The paleontological work was thus in able hands, and covers a large number of species in each of its departments. The portions giving the most novel results are those of the Fishes and Amphibians, and the Lower Carboniferous plants.

Dr. Newberry describes the genus *Dinichthys* from new and magnificent specimens—including broad plates of the venter and back, fifteen inches to two feet in length, a mandible twenty-two inches long, a cranium almost complete, and other bones—and shows that it was closely related to *Coccosteus.* The large ventral pieces were five in number. The anterior end of the mandible was turned up so as to form a strong acute prominent tooth, which had a produced dentate margin in one species. The dentition resembles that of the living *Lepidosiren*; and Dr. Newberry refers the genus (along with *Coccosteus*, *Heterostius*, *Asterolepis*, *Pterichthys*, etc.), to the Lepidosiren group, or the Dipnoa, and agrees with Dr. Günther in placing the Dipnoans and Placoderms with the Ganoids. A species of Coccosteus, *C. occidentalis* Newb., is described from Ohio. Dr. Newberry describes "Conodonts" from the Waverly group, which he is inclined to refer to the Marsipobranchs. He also gives new species of *Cladodus*, *Polyrhizodus*, *Orodus*, *Ctenacanthus*, *Lystracanthus*, *Platyodus*, *Rhynchodus*, *Ctenodus*, *Dipterus*, and introduces the new genus of Ganoids, *Heliodus*, for species near those of *Dipterus*. Dr. Newberry, after remarking that the occurrence together of the spines *Ctenacanthus furcicarinatus*, the teeth *Orodus variabilis*, and certain dermal tubercles, show that they belong to the same species, remarks that impressions of the heterocercal tail, with the fins of the same species, have been observed—one of them six feet long; and that the lower lobe of the tail consists of rays that were distinctly ossified.

The Batrachians or Amphibians from the Carboniferous beds, and described by Prof. Cope, are referred by him to the order which he has named *Stegocephali;* an order including the *Labyrinthodonts* Owen, and also Owen's *Ganocephala*, and other species recently described which have been called *Microsaurs.* The species are partly lizard-like, with ribs and limbs (as in *Dendrerpeton*, *Hylonomus*, *Amphibamus*, *Colosteus*, *Archegosaurus*, etc.; and part very long and slender, snake-like, with limbs wanting, as in *Molgophis* Cope, which has ribs and probably no limbs, and *Phlegethontia* Cope, which is without both, and includes "true batrachian snakes,"—one imperfect specimen having 56 vertebræ. Prof. Cope also describes a few Amphibians which he refers to the *Proteida.*

The interesting Subcarboniferous plants brought to light and described by Professor E. B. Andrews, have been already noticed in the last volume of this Journal (p. 462) by the author. The turning out of so many new species of unusual forms in a region that had already been long explored is a fact of much geological interest.

The very numerous plates of this volume are well executed.

Dr. Newberry announces in his Preface that Volume III of the Paleontology will contain a general review of the fossil plants of Ohio, with descriptions of new species; a memoir by Prof. O. C. Marsh, on the *Dicotyles compressus*, and on the *Castoroides Ohioensis;* and notices of other Quarternary vertebrate remains, together with some invertebrate fossils yet undescribed. When completed, the series of Ohio reports will rank among the best State-survey publications that have appeared.

3. *Geological Survey of Alabama. Report of Progress for* 1875; by Eugene A. Smith, Ph.D., State Geologist. 8vo, 220 pp.—In this volume, a general outline of the Paleozoic formations of Alabama, with brief descriptions of the various beds, by Prof. Smith, is followed by a summary of the facts heretofore known concerning the Coal-fields of the State, prepared by Mr. T. H. Aldrich. The first systematic attempt at mining and shipping coal, in Alabama, is said to have been made in 1853, near the southwestern extremity of the Cahaba coal-field. Mr. Aldrich's paper includes a reprint of parts of an elaborate essay by R. P. Rothwell, published, two years since, in the Engineering and Mining Journal. The coal-series is said to contain ten or twelve veins [seams] of remarkable thickness, i. e., from two feet (average thickness of clean coal) upward, besides a number of smaller beds, several of which are from fifteen to eighteen inches in thickness. These ten or twelve workable beds are distributed in two series or groups, as we find in all our coal-fields, notably in West Virginia, Ohio and Pennsylvania. * * The *maximum available* thickness of coal as yet proved in any portion of the field will not exceed thirty or thirty-five feet; while, if we take the area of the

Cahaba field at 230 square miles, the *average* thickness of workable coal over the entire field would probably scarcely attain fifteen feet. This estimate, so much lower than we have been accustomed to see stated in reports and newspaper articles, is probably not very different from the thickness which the same method of estimating would give for any of our other bituminous coal fields. "The enormous thickness of the coal-bearing rocks in the Cahaba field, being estimated at over 5,000 feet, has no parallel in the Warrior coal-field." Record of four borings in the Warrior field show sections of from 400 to 600 feet of strata, including four, seven and eleven coal-horizons.

Prof. Lesquereux furnishes a list of 57 species of coal-plants, (of which 12 are named as new,) and remarks upon the very low position in the Coal-measure series to which they must theoretically be assigned, a few species, such as *Sternbergia*, *Lepidodendron Weltheimianum* and *Asterophyllites gracilis*, ranging down even into the Devonian. This corresponds with the suggestions already made, by several geologists, that the coal-measures of the Southern States are all very low in the series, the whole having been called "sub-conglomerate" by some writers. We should prefer, however, some more certain evidence on this point than has yet been produced. The surveys of Alabama, Georgia and Kentucky, now in progress, will leave but a short gap (in northern Tennessee) between the well-known fields of Pennsylvania and Ohio and the southern extremity of the system.

The body of the report is occupied with details of County-work, mostly in the Silurian areas of the State. Some analyses of ores are given, besides lists of elevations. There is also a valuable paper by A. R. Grote, on the cotton-worm (*Alëtia argillacea* Hübner) which is preliminary, the author states, to a more extended history of the worm. Mr. Grote writes from observations in Alabama on the habits of the worm, and also from a study of it elsewhere. He is an excellent entomologist, and if his reviews are continued in the survey, will add greatly, by his study of the insects injurious and beneficial, to the value of the State Reports.

We understand that the State appropriation, for the work thus reported on, is only $500 a year to cover the *travelling expenses* of the geologist during the vacations of the State University, in which institution he is a Professor. The volume may therefore properly be accounted a personal contribution to the cause of science.

4. *The Geological Record for* 1874.—An account of works on Geology, Mineralogy and Palæontology, published during the year. Edited by William Whitaker, B.A., F.G.S., of the Geological Survey of England. 398 pp. 8vo. London, 1875. (Taylor & Francis.)—Mr. Whitaker, the editor of the Geological Record, has had able co-workers, and has produced a volume which will be found of great value to all of all lands that are interested in the progress of geological science. The sub-editors are the following, all members of the Geological Society: W. Topley, G. A.

Lebour, F. Drew, and R. Etheridge, Jr., for Descriptive Geology; Professor A. H. Green, for Physical Geology; F. W. Rudler, for Mineralogy and Petrology; and L. C. Miall, Prof. H. A. Nicholson, and W. Carruthers, for Paleontology. The number of works and memoirs mentioned by title is very large, and, for much the larger part, short abstracts are given, which appear to have been carefully prepared. American publications are included, as well as those of other continents, and are judiciously treated. Some omissions we note, of papers in the Publications of Societies. This volume is to be the first of a series of Annual Records, and that for 1875 is already far advanced.

5. *Report on the Geology of a portion of Colorado examined in* 1873; by Prof. J. J. STEVENSON. 376 pp. 4to. Part IV of Lieut. Wheeler's Survey Report, vol. III. Published March 4, 1876.—Prof. Stevenson treats, in his report, of the general physical features of Colorado, of the various rock formations and mineral springs, and of the structure and age of the Rocky Mountain System and brings forward much that is of interest. Under this last head, the new conclusion is advanced that *there was an era of mountain-making in the Rocky Mountains at the close of the Carboniferous age*, synchronous with that in which the Appalachians were formed. The facts brought forward in its support appear to us to be too few and from too limited an area to establish fully its truth against the opposing statements of other Rocky Mountain investigators. If an epoch of mountain-making then occurred, it ought to be registered in an extensive series of obvious facts. We see in the Appalachians—in their breadth exceeding 100 miles, their length several hundreds, with upturnings everywhere—an example of an *individual* mountain-chain (i. e., one made in a single mountain-making operation); and also a display of the manifest evidences of disturbance which such an area should bear. We have, further, an illustration of the fact that such an "individual" cannot have narrow confines, because the crust of the earth has been—certainly since Silurian times—too thick to bend in a narrow trough or geosynclinal (the trough in which the deposits constituting the mountains were accumulated). We shall look with great interest for the results that may hereafter be published by other observers on this interesting question. J. D. D.

6. *Das Gebirge um Hallstatt.* Erster Theil; Die Mollusken-Faunen d. Zlambach und Hallstätter-Schichten. II Heft mit 38 Lith. Tafeln. Von EDMUND MOJSISOVICS von Mojsvár, Chef-Geologen d. k. k. geol. Reichsanstalt. 4to. Vienna, 1875.—This part of the great work on the peculiar fossil fauna of the renowned locality of Hallstatt is worthy of its predecessor previously noticed in this Journal. It contains the most complete series of figures and descriptions of the genus Arcestes yet published. Thirty-four plates are devoted to the illustration of this group and the specimens are, for the most part, very perfect. This has enabled the author to exhibit a very perfect series of a multitude of various forms all

having perfect apertures, and his plates are the most perfect demonstration, which we have yet seen, of the importance of the contours of the aperture in the classification of sub-groups. The author divides the genus Arcestes into several divisions according to the peculiarity of the sutures and gives complete tables of the geological and geographical distribution of the species. In fact the stratigraphical paleontology is treated in the most perfect manner. Any criticisms of the zoology of the work would be out of place and inapplicable, since it is essentially, as are all the later German paleontological memoirs on this and kindred subjects, paleontological geology; the differences of the animal remains being invariably the aim of all the researches, with the view of establishing data by which the different strata may be distinguished one from another, and the peculiarities of the faunæ noted. Two new genera are described, Didymites and Lobites. The latter being related to the true Arcestes in about the same way that Scaphites is to the typical Ammonites. Whether this will eventually hold or not is doubtful, since, as Quenstedt has shown in his master-work on the Jura, such forms are intimately connected with normal forms, sometimes not even specifically separable according to the generally accepted methods of classification. A. H.

7. *Rammelsberg: Handbuch der Mineral-Chemie.* 2d edition, 980 pp. 8vo. Leipzig, 1875.—The second edition of Rammelsberg's valuable work on mineral chemistry is an indispensable volume to all interested in the progress of mineralogy. The general plan adopted by the author is essentially that of the first edition (published in 1860), the most important changes being those which strict conformity throughout to the principles of the new chemistry has required. The first volume (136 pp.) contains the general treatment of the subject of mineral chemistry, with a detailed discussion of the principles of isomorphism and heteromorphism. The second volume (744 pp.) takes up the mineral species in order, giving under each the most of the analyses published, especially those of recent date. For each analysis the atomic ratios of the different elements have been calculated in full, and from them the formulas are deduced. The author has been so long an authority in mineral chemistry that his present conclusions on many disputed points, though occasionally appearing somewhat arbitrary, will have great weight with all. E. S. D.

8. *Einleitung in die Krystallberechnung*, von Carl Klein; *zweite Abtheilung*, pp. 209–393. Stuttgart, 1876.—The first part of Professor Klein's valuable work on crystallography was noticed in a recent number of this Journal, (III, xi, 68.) The present part includes a detailed description of the methods of calculation applicable to the monoclinic, triclinic, and hexagonal systems, the whole being characterized by the same clearness and thoroughness to which attention was before called. A chapter upon the drawing of crystals forms the conclusion of the work. E. S. D.

III. Botany and Zoology.

1. *Phænological observations in Giessen;* by H. Hoffmann. (*Phänologische Beobachtungen in Giessen*, von H. Hoffmann.)—We do not know any single English word for the kind of observations here recorded, and therefore we shall adopt that which our author has borrowed from Greek. This pamphlet of 32 pages gives the date when the leaf and flower-buds of over two hundred plants opened; the fall of the blossom; the ripening of the fruit, and the fall of the leaves. The observations in some cases extend through twenty years or more. The author has also noted the dates of the appearance of butterflies, birds, etc. The author submits his work as a contribution to the store of facts required by the student of vegetable climatology, but does not give any conclusions of his own except the following:

In the case of plants in a given locality, the average of a few years is very nearly the same as the average of many, e. g.:

The "first flowers" of the following species appeared on the days given below as means:

Average	of 8 years,	of 13 years.
Geranium sylvaticum,	20 May,	19 May.
Triticum vulgare,	"	"
Average	of 15 years,	of 21 years.
Helianthus annuus,	26 June,	25 June.
Average	of 9 years,	of 14 years.
Primula elatior,	25 March,	25 March.

In only one of the cases collated is there to be observed a difference of more than one day.

Prunus avium, average of 8 years,	....	23 April.
" 14 "	....	21 "
" 21 "	....	19 "

G. L. G.

2. *Bulletin of the Bussey Institution, Harvard University.* Part V. 1876.—This finishes the first volume, of 453 pages, and is therefore properly supplemented by a copious index. In articles No. 18, 19, and 20, Prof. Storer continues his valuable and practical chemical papers—the notice of which we leave to another hand. Papers No. 21 to 24 are by Prof. W. G. Farlow, as follows:

On a Disease of Olive and Orange Trees occurring in California in the spring and summer of 1875.—It proves to be the work of a fungus, *Fumago salicina*, which has been known and given trouble in Europe since the year 1829. In an excellent plate Prof. Farlow represents the stylospores, mycelium, pycnidia, and conidia.

On the American Grape-Vine Mildew.—It appears that the *Oidium Tuckeri*, which has been so disastrous to the vines of

Europe and Madeira, and which is "the conidial form of some species of the *Ascomycetes*, probably some *Erysiphe*," although supposed to have come from America, is not a common or conspicuous infestor of our native vines; but that the commonest, at least in New England, is *Perenospora viticola*, which is limited to the leaves and stems, and does not attack the fruit. Being often found on every leaf, of a vine, it would be expected to injure the grape crop. "Such, however, is not the case. The fungus does not attack the grapes themselves, nor does it, at least in New England, appear until about the first of August, and its withering effect upon the leaves is not very evident before September. As far as out-of-door grape culture in the northern States is concerned, we are inclined to believe that, practically, no harm is done by *Perenospora viticola*, but that, on the contrary, the fungus is really beneficial. Our native vines have a luxuriant growth of leaves; and the danger is that, in our short summers, the grapes may not be sufficiently exposed to the sun to ripen. But the *Perenospora* arrives, with us, at a time when the vine has attained its growth for the season, the important point being then to ripen up the grapes which are concealed by the foliage. By the shriveling of the leaves, the *Perenospora* enables the sun to reach the grapes without loss to the vines, as is shown by the fact that the vines continue to live on, year after year, without apparent injury." The botanical history, literature, and forms of this fungus are fully illustrated, two plates show the structure and fructification of this and some allied species; and a synopsis of the half-dozen species of *Perenospora* detected in the United States, and five of *Cystopus*, is appended.

List of the Fungi found in the vicinity of Boston. Confined to the species which have come under the author's own observation.

The Black Knot (of Plum and Cherry trees). This interesting and important memoir is illustrated by three beautiful plates, showing this disease in various stages, and the whole structure, development and fructification of *Sphoria mortosa* of Schucinitz, the fungus which produces this black knot, which so deforms and injures plum and cherry trees throughout the Northern States and Canada. The remedy is the knife or the axe. For prevention Dr. Farlow recommends the extirpation of choke cherry trees, upon which the pest largely breeds in the vicinity of Boston. Farther west it would all the more be necessary to destroy all the wild plum-trees (*Prunus Americana*), which are fearfully infested.

No. 25, the last paper of the volume, is Prof. Sargent's

Report of the Director of the Arnold Arboretum;—from which we learn that: "Probably over 100,000 ligneous plants have been raised during the nine months," and as many as 5,542 trees and shrubs have been presented to various establishments and individuals throughout the United States interested in agriculture," besides those sent to Kew and elsewhere abroad. A catalogue of the species raised is appended. A. G.

3. *Nymphœa flava* Leitner.—The plate of Audubon's great work which represents the American swan likewise represents the flower of a yellow *Nymphœa*, or true water-lily, under the above name. The foliage which accompanies it may be that of a *Nuphar*, but the flower is that of a *Nymphœa*. Leitner was a German botanist who explored southern Florida, and died or disappeared there, —if we rightly remember, was thought to have been killed by Indians. He doubtless met Audubon and gave him the name which he published on his plate. The species has properly been left unnoticed so long as the whole evidence of its existence rested upon Audubon's figure of a flower accompanied as it is with *Nuphar* foliage. But of late years we have heard of a yellow water-lily in Florida. In 1874, Dr. Edward Palmer sent us a specimen with foliage and flowers collected in Indian River, and certified to the yellow color. It has now been detected by Mrs. Treat, on the St. John's River, and living plants communicated to us, from which we may expect to see fresh blossoms. The growth is very different from that of *N. odorata*, the rhizoma being shorter, and thickly beset with salient blunt tubercles; and the plant propagates freely by stolons. A. G.

4. *Note on some of the Starfishes of the New England Coast;* by A. E. VERRILL.—In the Archives de Zoologie Experimentale et Générale, vol. iv, Nos. 2 and 3, 1875, M. Edmond Perrier has published a very useful and important paper entitled "*Révision de la Collection de Stellérides du Muséum d'Histoire Naturelle de Paris*," in which he has redescribed many of the types of Lamarck, J. E. Gray, Müller and Troschel, and others, as well as many new species, and has also added many remarks on various genera and species, as well as upon their classification, etc. At the present time I do not propose to discuss this memoir, as a whole, but wish to call attention to some errors into which the author has fallen concerning our common New England species, owing chiefly, doubtless, to his not having a sufficient number of well preserved specimens to form any clear ideas of their true specific characters and great variability.

Every naturalist who has occasion to collect and study any considerable number of living specimens of any of the larger species of *Asterias*, especially if from different localities or varying stations, must be deeply impressed by their extreme variability, not only in size and color, but in the form and relative length of the rays, character of the dorsal spines, number of pedicellariæ, etc. Moreover, if he has had occasion to preserve large numbers of specimens, both in alcohol and by drying, he must have observed the very different forms and appearances that specimens, quite similar when living, will assume, whether owing to the various states of contraction in which they die, or to the mode in which they are afterwards preserved. Thus similar living specimens may be killed and preserved so that one will have slender tapering rays; another, rays smaller in the middle and constricted at base; another, rays swollen at base and pointed at tip; some will die with swollen disk; others with contracted disk; some will have the rays collapsed and flattened; others will have them round and plump, or angular; some will have the spines erect; others, more imperfectly preserved, will have them flattened down and more or less detached. Moreover, the plates in some will be so closely drawn together by the contraction of the muscles of the skin as to give them a rigid character, while others, perfectly identical. if they die in a relaxed or inflated condition will have the plates separated by the looser integuments so as to give them an openly reticulated appearance, with wider naked spaces between the plates. Hence all such characters should be used with great caution.

It is, therefore, evident that any naturalist who would correctly limit the species in this group should at least have a very large number of specimens preserved, as well as possible, in various ways, and still better, when possible, he should collect large numbers of the living specimens and after studying them in life and making notes upon them he should preserve, and afterwards compare them with his notes. In this difficult group there are probably no species more variable and perplexing than those forms allied to *Asterias rubens* of Europe, and *A. vulgaris* and *A. Forbesii*, the common New England species. And yet in this very group M. Perrier attempts to decide the specific characters of our species, and to correct their synonymy after an examination of very few (sometimes only one), and often very badly preserved dry specimens (*A. pallidus*). And in doing this he relies on characters that are notoriously variable, and even upon those accidental features due to modes of preservation, as stated above.

As M. Perrier particularly refers (pp. 354–7) to my own views in regard to our native species, as expressed in several former papers,* and seems to think it strange that my conclusions in 1873 differed slightly from those held in 1866, I may be pardoned for stating that during the ten years that have elapsed since my first paper on the subject was published, these starfishes have been collected, studied, and preserved by me in very great numbers, and from hundreds of localities, during the various dredging expeditions that I have undertaken along our coast, some of which have been noticed in former volumes of this Journal. Therefore, having carefully examined many hundreds of specimens, in all conditions, and having taken ten years to consider the matter and to discuss it with others, I feel perfectly confident that M. Perrier has made at least five American "species" out of specimens that actually represent but two. These errors would have been more excusable had they not been made subsequently to my revision of the synonymy, for he might have supposed that my materials were far more ample than his own. The facts are as follows: Upon the coast of New England there are, as yet known,

* Proceedings Boston Society of Natural History, vol. x, p. 333, 1866; Report on the Invertebrata of Southern New England, Report of U. S. Commission of Fish and Fisheries, Part I, 1873 (published March, 1874).

but three species of *Asterias*, belonging to the littoral zone and shallow waters, though two or three other smaller species (*Leptasterias*) occur in deep water.* One of the shore species, *A. littoralis* (Stimpson), is a small species, rarely six inches in diameter, (belonging to the group, *Leptasterias*) found in the Bay of Fundy and northward to the Gulf of St. Lawrence, both between tides and in deep water, and although variable in form, size, color, etc., it has not yet led to much confusion. The two others are large and very common species—one southern and the other northern—but with their respective areas overlapping on the New England coast. One of these, *A. Forbesii* (Desor), extends from the Gulf of Mexico, to Casco Bay, Maine, and is the most common species on the southern coast of New England. The other, *A. vulgaris* (Stimp.), extends from Labrador (and probably farther north) to Long Island Sound, where it becomes rare; it is the most common species north of Cape Cod, and is very abundant on the coast of Maine and northward. Both species vary extremely in size, form, and color, even when living, and still more so when preserved by drying. The color of the faded dry specimen, which M. Perrier mentions as distinguishing "*A. Fabricii*" from "*A. berylinus*," is a perfectly worthless character, yet when living our two species can usually (but not always) be distinguished by the colors, for *A. Forbesii* (*berylinus*) is generally greenish, varying to orange and brown, with a bright orange madreporic plate; while *A. vulgaris* is usually reddish, purplish, or violet, varying to yellow and brown, but with a pale buff or cream-colored madreporic plate. Moreover, the colors and forms of each vary according to the sex, and the form varies according to the season, state of the ovaries, age, dilation with water, etc. The *forms* of the major pedicellariæ,† and of the adambulacral (or interambulacral) spines give the most available characters for distinguishing the two species under all circumstances, though the firmer skeleton of *A. Forbesii* is also an important and characteristic feature.

In A. Forbesii the major pedicellariæ are short, ovate, blunt-pointed, hardly longer than broad; the adambulacral spines are stout, obtuse, and in most cases many are more or less flattened, and grooved externally, at tip.

* It is proper that I should state that I have become satisfied that the species described by me as *A. Stimpsoni*, in 1866, was not well founded. The study of a far more extensive series of specimens has shown that the specimens thus named were somewhat peculiar small specimens of *A. vulgaris* (Stimp.), with which some young specimens of *A. littoralis* were also confounded, so that the characters given largely appertain to the young of *A. vulgaris*. It is probable that most of the specimens formerly distributed as *A. Stimpsoni*, were young of *A. vulgaris*, and such may have been those that M. Perrier says he has examined, though he considers it a good species.

† Dr. Stimpson, in 1862, (Proc. Boston Soc. Nat. Hist., vol. viii, p. 262, *note*) called special attention to the two peculiar forms of pedicellariæ, characteristic of this family, and to their importance in distinguishing the species. He termed the larger sessile ones, whether scattered over the surface or attached to the ventral spines, "*major pedicellariæ*," and the small pedunculate ones, borne in clusters on the spines, "*minor pedicellariæ*." In 1864–70 M. Perrier called the former, "*pédicellaires droits*," and the latter "*pédicellaires croisés*."

In A. vulgaris the major pedicellariæ are lanceolate, sharp-pointed, much longer than broad; the adambulacral spines are longer, more pointed, and seldom flattened.

The dorsal spines are variable in form and number in both species, but are usually more acute in the latter, though blunt and even clavate spines often occur on both species. The *number* of *minor* pedicellariæ on the spines, and of *major* ones on the back and also on the adambulacral spines, is extremely variable in both species. Yet these are the characters mainly relied upon by M. Perrier for distinguishing his supposed species. Three of the "species" recognized by him are evidently mere forms of *A. vulgaris.* These are *A. Fabricii* (Agassiz, MSS.), based on *one* dry specimen from Laborador; *A. pallidus* (Agassiz, MSS.) based mainly on six small dry specimens, sent, like the preceding, from the Museum of Comparative Zoology in 1864, and in a very bad state of preservation, the spines being mostly detached by partial decomposition during the drying of the specimen (which has misled M. Perrier, who imagined that they had been moveably articulated); and two large specimens that he refers to *A. vulgaris*, one from Beverly, Mass., and one in the British Museum. The latter was probably sent by Dr. Stimpson, who commonly used labels marked "Exploration of the East Coast of the United States" (not "west" coast, as M. Perrier gives the label), for his New England collections. I had given "*A. Fabricii*," after examining original specimens, as a synonym of *A. vulgaris* in my paper of 1866; and gave "*A. pallidus*" as an undoubted synonym in my Report of 1873–4. All the characters given by M. Perrier as distinctive are variable and partly accidental features that can be found, with all intermediate states, in any considerable collection of this species from a single locality. These manuscript names were given by Prof. L. Agassiz before he had made a very thorough study of the genus, but in a conversation, in 1871, while we were dredging in company in Vineyard Sound and obtaining both species in abundance, he fully agreed with me that there were *only two* large species of this group known on our coast, and he also positively identified the numerous good specimens of *A. vulgaris* with his *A. pallidus;* and likewise the *A. Forbesii* (or *arenicola*) with his *A. berylinus.* Dr. Wm. Stimpson in a conversation with me not long before his death, also agreed with these decisions. Had M. Perrier examined a good series of specimens he also would surely have found it impossible to have made the useless distinctions that he now proposes, based on such very insufficient material. In my Report of 1873–4 I stated that *A. Forbesii* and *A. arenicola* are "probably identical," the differences noticed (mainly in form and color) "being, perhaps, chiefly sexual," but not desiring to make premature changes, I left them under the two names, only because I had then no time to determine whether the differences are sexual, or properly varietal, or due to local or individual variations. Subsequent studies have satisfied me that the differences are mainly individual or casual and very inconstant, so that there is no longer any reason for distinguishing the two even as varieties, yet M. Perrier not only keeps them distinct, as two species, but names another slight variation as a distinct variety of *A. arenicola.*

There is now little doubt in my mind that *A. vulgaris* will prove to be identical with *A. violaceus* of Northern Europe, and that the latter may be, as many believe, a mere variety (or sexual form) of *A. rubens.* But M. Perrier considers these distinct species, though with some doubt as to *A. violaceus.*

That, in other cases, he has admitted, as valid species of *Asterias*, forms that are scarcely varietal is very probable, judging from his descriptions alone, for the distinctive characters that he gives are frequently those that are most apt to be variable. He has described a single dry Labrador specimen of *A. polaris* (from Dr. Packard's collection) as a new species under the name of *A. borealis.* But among a large number of fresh specimens observed by me at Anticosti Island, there were various forms intermediate between his specimen and what he regards as the typical *A. polaris* from Greenland. Moreover, the several Labrador specimens that I have examined, collected by Dr. Packard at the same time with the one now described as *A. borealis*, show great variations in the form of the spines, length of arms, and number of pedicellariæ,—characters that M. Perrier regards as distinctive in this case. Therefore there is good reason to believe that his *A. borealis* is only a form of *A. polaris*, to which American zoölogists have hitherto referred it.

Throughout the paper there are numerous typographical errors, many of them due to imperfect proof-reading, but others more important are due to careless references to the papers of other writers, especially those in English. "Contributions to the Zoology of Yale College" is scarcely a legitimate substitute for the "Contributions to Zoology from the Museum of Yale College," published in this Journal. The locality, Eastport, Maine, is once given as "East Port (Massachusetts,)" and once as "East Port, (Canada.)"

In one respect the nomenclature adopted, in some cases, by M. Perrier is very objectionable, for he attempts to restore some of the ante-Linnæan "names" of species used in 1733 by Linck, who was not, in any sense, a binomial writer, and whose polynomial (or accidentally binomial) phrases can have no claim to priority, as specific names, under the binomial system.

5. *Hæckel's theory* (*Allœogenesis*) *of the genetic connection between the Geryonidæ and Æginidæ.*—In the Proceedings of the Elliot Society for 1857, McCrady gave a very interesting account of the commensalism of the young brood of a Cunina and of Turritopsis. No notice was taken of this remarkable mode of development, McCrady's observations having been discredited by the later publication (1865) of a magnificently illustrated memoir on the "Rüssel-quallen" by Hæckel. The startling hypothesis of the genetic connection between the Geryonidæ and Æginidæ contained

in this memoir and called by Hæckel allœogenesis, has been ever since a stumbling block to all theories of genetic relationship among Medusæ.

Two short papers recently published, the one by Schulze (Nat. Wiss. Ver. f. Steiermark, 1875, p. 125), and the other by Uljauin (Archiv f. Naturg., 1875, p. 333), have, however, proved conclusively that Hæckel's theory, like many other of his vagaries, had no foundation of truth. It was based not merely on an incorrect interpretation of facts, but the facts themselves existed only in his imagination.

As, perhaps, with the exception of his monograph of the Radiolaria, no other memoir has contributed more than the one above quoted to give Hæckel the position he holds among zoölogists, we may be allowed to remind the Hæckelian school of naturalists that this same genetic connection has furnished the text for many a sermon from their high priest. Infallible himself, he has been unsparing in his condemnation of the ignorance and shallowness of his opponents. Proved now to be in the wrong, we expect therefore justice without mercy from this stern scientific critic, and look forward in the next number of the Jenaische Zeitschrift for a thorough castigation of Hæckel by Hæckel, showing up the absurdity of allœogenesis and all that hangs thereby.

ALEXANDER AGASSIZ.

6. *Animal Parasites and Messmates;* by P. J. Van Beneden. 274 pp. 12mo, with 83 illustrations. 1876. New York. (D. Appleton & Co.) The International Scientific Series.—The author of this work is an able Belgian zoologist, well versed in the subject of which he writes. The subject is treated in a popular style, and cannot fail to interest. The facts presented are some of the strangest that have been brought to light by recent investigations, and not the least marvelous are those relating to man's parasites.

7. *The Journal of Anatomy and Physiology*, conducted by G. M. Humphrey, M.D., F.R.S., Prof. Anat. Cambridge, Wm. Turner, M.B., Prof. Anat. Edinburgh, M. Foster, M.D., F.R.S., Prælector Physiol., Cambridge, and Wm. Rutherford, M.D., Prof. Inst. Med. Edinburgh. January, 1876, vol. x; part II, pp. 223–458, with 8 plates. Cambridge and London. (MacMillan & Co.)—The tenth volume of this able scientific quarterly commenced in October. The January number, recently issued, contains the following papers: On the anatomy of the lens, by Dr. Thin and J. C. Ewart (pl. ix); anatomy of the Lineidæ (Nemerteans), by Dr. McIntosh (pl. x–xiii); experiments on the biliary secretion of the dog, by Dr. Rutherford and M. Vignal; the transformations of the pulse-wave in the different arteries, by Dr. Galabin (pl. xiv); on the broncho-œsophageal and pleuro-œsophageal muscles, by Dr. Cunningham; the summation of electrical stimuli applied to the skin, by Dr. Stirling; development of Elasmobranch fishes, by F. M. Balfour (pl. xv, xvi); craniofacial apparatus of Pteromyzon, by Prof. Huxley (pl. xvii, xvii); secondary arches of the foot,

by S. M. Bradley; note on the placental area in the cat's uterus after delivery, by Prof. Turner; notices of books; report on the progress of anatomy, by Prof. Turner and D. J. Cunningham, M.B.

8. *Bulletin No.* 2, of the U. S. Geological Survey of the Territories, contains two papers by R. Ridgway, entitled Studies of the American Falconidæ, and Ornithology of Guadeloupe Island, illustrated by two plates.

III. Astronomy.

1. *Observations of the planet Jupiter.*—The Royal Astronomical Society of London have appointed a committee whose object shall be to endeavor to enlist observers in making drawings of the appearance of the planet Jupiter, and to obtain as extensive a series as possible of such drawings. The committee has issued a circular, and prepared, for the sake of uniformity and convenience, blank forms upon which the drawings can be made. Drawings and communications should be sent to the "Secretary of the Jupiter Committee," Royal Astronomical Society, Burlington House, London, W.

2. *Repertory of works in Pure and Mixed Mathematics.*—Doctors Kœnigsberger and Zeuner of Dresden, propose to collect, as far as possible, and publish "longer or shorter detailed reports written by the authors themselves upon their own books and treatises" in the mathematics. The reports will comprise articles on the entire field of mathematical research: Pure mathematics, and all the collateral branches of the mixed science, such as: astronomy and geodesy, mathematical physics, analytical and technical mathematics, all mathematical branches of engineering, mathematical statistics, etc.; besides which the editors intend to have the reports printed in the language in which they are sent to them by the author, taking it for granted that such reports in German, English, French, or Italian will be intelligible to scientific men of all countries.

They propose to begin with reports on all books and memoirs which have appeared since Jan. 1st, 1875. The "Repertory" will at first be issued in numbers, at irregular intervals. H. A. N.

IV. Miscellaneous Scientific Intelligence.

1. *The American Cyclopædia.* Vol. xv. SHO–TRO. D. Appleton & Co., New York. 1876.—The first volume of this edition of Appleton appeared in July, 1873. One volume more will complete the work, making in the aggregate nearly 15,000 pages of text. Among the articles in the fifteenth volume interesting to scientific readers we note particularly *steel*, by Dr. Droun; *silver*, by Dr. Raymond; *sound*, *spectrum* and *stereoscope*, by Professor Mayer; *sun*, by Professor Langley; *tides*, by Mr. Hilgard; *snow*, *storms* and *trade-winds*, by Cleveland Abbe; *telegraph*, by Professor Lovering; steam, steam-boiler, carriage, engine and navi-

gation, and strength of materials, by Professor Thurston. Numerous astronomical articles are furnished by R. A. Proctor, of London: e. g. spectrum analysis, sun (in part), &c. The medical and physiological articles are mainly by Dr. J. C. Dalton, while other well-known names add authority to the contents of this volume. We have in former notices called attention to the large amount of special work performed for this edition of Appleton's Cyclopedia by men of acknowledged ability and original research in the departments of which they treat. The foregoing list is in evidence that this character is fully maintained. The maps and wood-cuts are numerous and excellent in quality. B. S.

2. *Annual Report of the Light-House Board to the Secretary of the Treasury for the year* 1875. 136 pp. 8vo. Washington, 1875.—The executive members of the Light-House Board are Professor Henry, Commander J. G. Walker, U. S. N., Naval Secretary, and Major P. C. Hains, Engineer Secretary. The report for 1875 contains "an Account of the investigations of the Light-House Board relative to illuminating materials, by the Chairman of the Committee on Experiments." Lard-oil had been in former experiments found to be the best illuminating oil; and this conclusion is sustained as regards the large lamps by new comparisons of lard oil with the best mineral oil, though not as regards the smaller. On account of the increased expense of lard oil, the mineral oil will hereafter be introduced. There is also another of Professor Henry's valuable papers containing the "Investigations of the Board relative to sound in its applications to fog-signals," made under his direction in 1875.

3. *Meter-Diagram.*—Messrs. A. & T. W. STANLEY, of New Britain, Conn., well known as makers of accurate measures of length, levels, etc., have lately prepared a neat meter-diagram, printed on heavy paper faced with linen, with the scales and tables on both sides, and 4¼ inches wide. It gives in a complete manner the entire metric system, with the relations of feet and inches to the meter and its subdivisions, and in the appended tables the equivalents in denominations in use, with rules for conversion. Professor Newton says of it: "I know of no easier way by which anyone desirous of learning the system can do it, than by studying this scale, and keeping it in a place where he will frequently see it." It is put up in a paper case, and sold at the moderate price of three dollars per dozen, for use in schools, etc.

Handbook of Architectural Styles. Translated from the German of A. Rosengarten by W. Collett Sandars. 502 pp. 8vo, with 639 illustrations. 1876. New York. (D. Appleton & Co.)—An excellent, and profusely illustrated work.

A Short History of Natural Science and of the Progress of Discovery from the time of the Greeks to the Present Day, for the use of Schools and Young Persons; by Arabella B. Buckley. 488 pp. 12mo, with illustrations. 1876. New York. (D. Appleton & Co.)—An instructive work.

Physics of the Ether. By S. Tolver Preston. London. (E. and F. N. Spon.) 1875. 8vo, pp. 136.

Geological Survey of Pennsylvania. Report of Progress in the Clearfield and Jefferson District of the Bituminous Coal-field, by F. Platt, 296 pp. 8vo, with maps and sections.

OBITUARY.

A. R. Marvine.—Mr. Archibald R. Marvine, the geologist, died in Washington, March 2d, 1876. He was born at Auburn, N. Y., Sept. 26, 1848, and while a youth attended the military school at Sing Sing, and subsequently the School of Technology at Philadelphia. He then entered the Hooper Mining School of Harvard University, from which he graduated in 1870, when he was appointed instructor in the same school, a position which he held until July, 1871. He was one of the students who went with Professor Whitney, to make practical studies in geology and geography in the Park Mountains of Colorado, in 1869.

In the summer of 1870, Mr. Marvine was appointed assistant-geologist to attend the celebrated Santo Domingo Expedition, and on his return he prepared a brief report on the geology of the island, which was published, with the other reports relating to Santo Domingo affairs, by order of Congress.

In July, 1871, he received the appointment of astronomer to the Wheeler Expedition, in which capacity he served several months, while that work was in progress, and then continued as a member of the Expedition in the capacity of geologist. His report on the geology of a district of country through which he passed, embraced in southern Nevada, northwestern Arizona, and southern California, has lately been published by authority of Congress.

His next geological work was in the Keweenaw copper region on the shore of Lake Superior, under the direction of Professor Pumpelly, and his report was published by authority of the legislature of Michigan.

In March, 1873, he was given a position as geologist in the corps of the U. S. Geological and Geographical Survey of the Territories under Dr. Hayden. In this capacity he made a careful survey of a region embracing Middle Park and extending eastward across the Front Range to the foot hills. His published report on this work gives evidence of thorough preparation, great labor and much skill, in the collection of material, and ability in its use. After preparing his report on the Middle Park district, he returned to Colorado Territory for the purpose of extending his investigations into a region of country west of Middle Park on the headwaters of the Grand, White, and Yampa rivers, and entered into his labors with great vigor and enthusiasm. But a long summer of toil and privation in that wilderness of cañons, crags, and peaks, undermined his health, and shortly after his return, early in the winter of 1874–5, he was prostrated with an attack of rheumatic meningitis from which, after many weeks of suffering, he partially recovered, but was not again able to resume his work, and early in December last he relapsed into a condition that was soon found to be hopeless.

Mr. Marvine leaves behind a large circle of friends, among the working scientists of the country, who had learned to expect great and valuable results from his researches. G. K. G.

APPENDIX.

ART. LIII.—*On some Characters of the genus Coryphodon* Owen; by O. C. MARSH.

THE lower Eocene deposits of England and France have yielded a few remains of an interesting genus of ungulate mammals to which Owen, in 1846, gave the name *Coryphodon.** Hébert subsequently published a memoir on the subject, in which he figured and described the more important specimens found in France.† Although comparatively little is yet known in Europe of the structure or near affinities of these animals, the portions preserved are characteristic, and the genus is well marked. The geological horizon, also, is fully determined, viz: the London clay of England, and, in France, the base of the *Argile plastique.*

While in Wyoming with the Yale College exploring party in 1871, the writer had his attention called to a deposit at the base of the Eocene containing mammalian remains, and subsequently obtained a number of the fossils, mainly through the kindness of Wm. Cleburne, Esq., who sent other specimens to the Academy of Natural Sciences in Philadelphia. Some of the latter fossils were described by Prof. Cope as *Bathmodon radians* and *B. semicinctus;* and in the same paper another generic name, *Loxolophodon*, was proposed for the same lower molar named as the second species.‡ Since this time, other similar remains have been found in Utah and New Mexico, and their principal characters can now be determined.

An examination of an interesting series of these fossils now in the Yale Museum, including some portions of the same individuals described by Prof. Cope, clearly shows that they all belong to the genus *Coryphodon* of Owen. This is especially important, as the geological horizon of the remains is essentially the same in both countries, and the American specimens promise to clear up many doubtful points in regard to the animals themselves. One of the specimens in the Yale collection is a nearly perfect skull, representing an undescribed species

* British Fossil Mammals and Birds, p. 299.

† Annales des Sciences Naturelles, tome vi, p. 87, Plates III and IV, 1856.

‡ Proceedings American Philosophical Society, p. 420. 1872.

which may be called *Coryphodon hamatus.* It indicates an animal somewhat larger than a tapir. The more important characters derived from an examination of this cranium, and some other remains of the same genus, are as follows:

The skull is elongated, the facial portion being most produced. A basal line extending from the lower margin of the foramen magnum along the palate to the end of the premaxillaries is nearly straight. The zygomatic arches are much expanded, but the malar is comparatively slender, and joins the maxillary in front of the orbit. The latter is of moderate size, and confluent with the large temporal fossa. The general form of the skull is indicated in the cut given below, figure 2. The maxillaries are massive, and on the sides behind the canines are deeply indented, giving a marked constriction to this part of the skull. The lachrymal forms the anterior border of the orbit, and its foramen is inside the orbital margin. The nasals are quite slender in front, and broad posteriorly. The premaxillaries are expanded transversely, giving a wide anterior narial aperture. The anterior palatine foramina are small, (figure 2.) The posterior nares extend forward between the last upper molars. The dental formula is as follows:

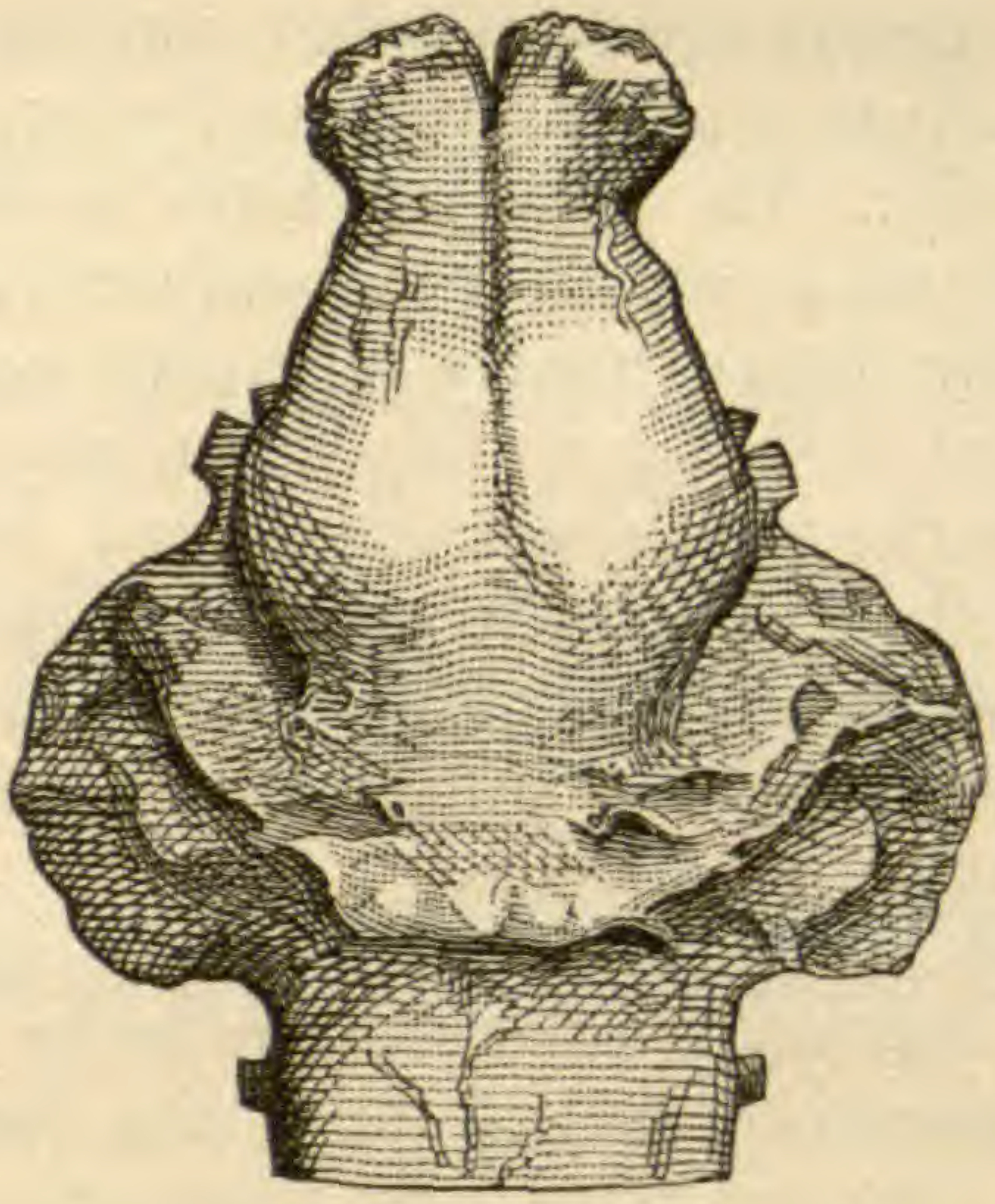
$$\text{Incisors } \frac{3}{3};\ \text{canines } \frac{1}{1};\ \text{premolars } \frac{4}{4};\ \text{molars } \frac{3}{3};\ \times 2 = 44.$$

The teeth agree in all generic characters with those figured as *Coryphodon* by Owen and Hébert. The occipital condyles are well separated, and there is a condylar foramen. Between the basisphenoid and the periotic, there is a large opening, partially due to the divergence downward of the inner faces of the latter bones. There is a strong paroccipital process, and a postglenoid process, which varies in size in different species. In *Coryphodon hamatus*, it is long, and curved forward, and to this the specific name refers. The skull as a whole presents strong Perissodactyle characters.

The brain cavity in *Coryphodon* is perhaps the most remarkable feature in the genus, and indicates that the brain itself was of a very inferior type. It was quite small, as in all Eocene mammals, but its most striking features were the small size of the hemispheres, and the large expanded cerebellum. The form and relative size of these are shown in the accompanying cuts, figures 1 and 2.

The olfactory lobes were large, and entirely in advance of the hemispheres. They were bounded in front by a well ossified cribriform plate, and partially separated by a vertical bony septum. The cerebral lobes were ovate in form, and very small, a transverse section exceeding but little that of the medullar opening. In shape and relative size, the hemispheres and olfac-

1.



2.

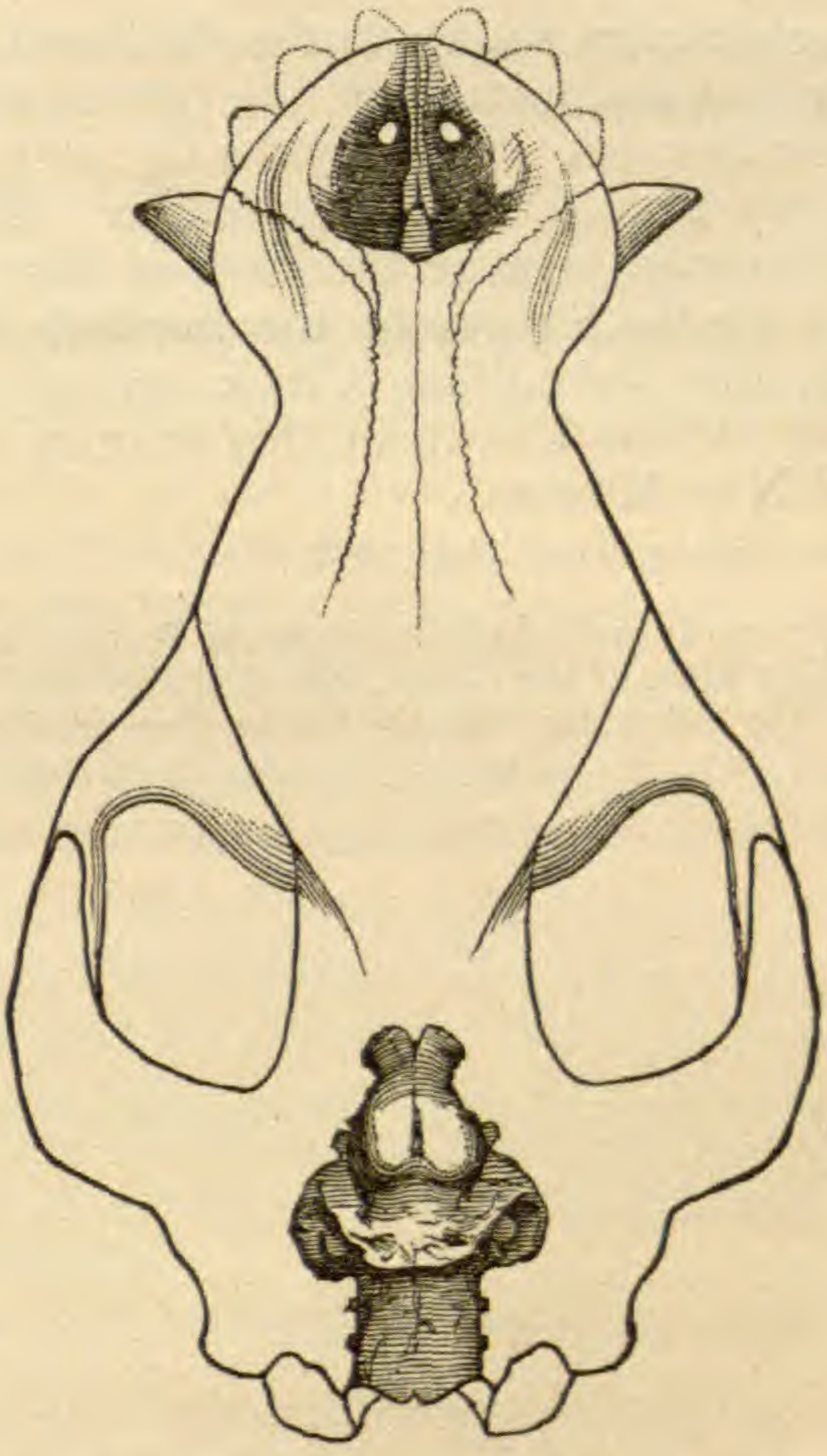


Figure 1.—Cast of brain-cavity of *Coryphodon hamatus* Marsh; top view. One-half natural size.

Figure 2.—Outline of skull and brain-cavity of *Coryphodon hamatus;* top view. About one-fifth natural size.

tory lobes of this genus are somewhat similar to those of *Dinoceras*. The cerebellum was proportionally large, and widely expanded transversely. Its peculiar form is shown in figure 1, which is drawn from a cast of the brain-cavity of *C. hamatus.* This portion of the brain nearly or quite equaled the hemispheres in size, thus differing widely from any known mammal. There is a well marked pituitary fossa, but no clinoid process. The foramina for the exit of the optic nerves are small, but for the others very large. The brain as a whole was very low in grade, and precisely such as might be expected in a mammal from the oldest tertiary deposits.

The skeleton of *Coryphodon* (*Bathmodon*) presents many features of interest, but only a few can now be mentioned. The limbs were comparativly short, and the femur has a third trochanter. The feet are especially interesting, as they present a primitive or generalized type. The manus and pes had each five short digits.*

The various characters shown in the skull and limbs of *Coryphodon* indicate that the animals of this genus were essentially five-toed Perissodactyles. They evidently represent a distinct family which may be called *Coryphodontidæ.* Their geological horizon in this country is near the base of the Eocene, in the deposits named by the Survey of the Fortieth Parallel, under Clarence King, the Vermillion Creek series. The remains of the family at present known in this country are from Utah, Wyoming, and New Mexico.

Yale College, New Haven, April 15th, 1876.

* Prof. Cope has recently published (Catalogue of Eocene Vertebrata from New Mexico, p. 28, 1875), a figure of the "Hind foot of *Bathmodon*," which is incorrect in several respects, the hallux, for example, having *three* phalanges!

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. LIV.—*Note on the Duplicity of the "1474" line in the Solar Spectrum*; by Professor C. A. YOUNG.

THE line "1474" is the one which is reversed in the spectrum of the Solar Corona, and coincides with one of the short lines in the spectrum of iron. In the iron spectrum it is brought out, however, only by the Leyden jar spark, and not by the electric arc between carbon points. As seen in the solar spectrum, with ordinary, or even very powerful, spectroscopes, it appears like a fine, hard, black line.

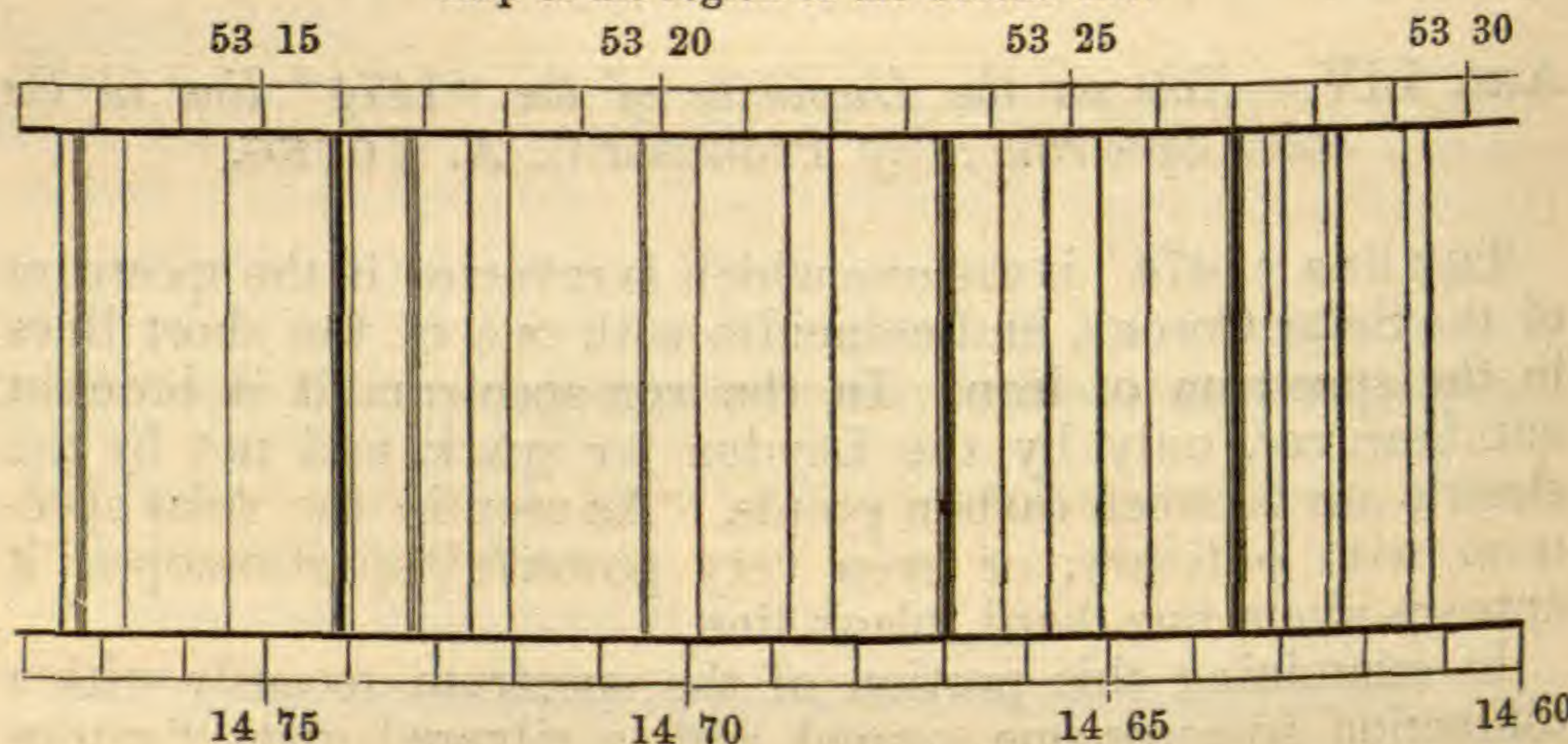
In examining this portion of the spectrum recently with a diffraction spectroscope, armed with a silvered glass "gitterplatte" of 8640 lines to the inch, for which I am indebted to the kindness of Mr. Rutherfurd, I find this line to be unmistakably double; the two components are separated by a distance of only about $\frac{1}{7}$ of a division of Ångstrom's scale, i. e. about $\frac{1}{40}$ of the distance of the D lines. The more refrangible component is heavier than the other and slightly winged or hazy at the edges, while the other is narrower and better defined. The more refrangible line is undoubtedly the real corona line, and the other belongs to the spectrum of iron, the close coincidence being merely accidental.

As long ago as 1870 I suspected the bright 1474, as seen on the limb of the sun, to be very slightly more refrangible than its dark analogue (the position of which with insufficient dispersive power would apparently correspond to the *mean* of the two components); and the suspicion has recurred from time to time on many occasions since then, while there has not been a single instance in which the bright line appeared to fall *below* the dark one. Still there has never seemed to be sufficient evidence to warrant a positive assertion. Examination with a speculum metal "gitterplatte" of 6480 lines to the inch, some two years ago, suggested the idea that the dark line might be closely double, but the definition of the grating was not sufficiently good to decide the question. With the new one, however, there remains no doubt. Another grating with 17280 lines to the inch, which is temporarily in my possession, shows it nearly, though not quite as well.

The accompanying map of this region of the spectrum gives a good idea of the appearance of things, and the amount of dispersive power obtained. The scale numbers at the top are those of Ångstrom, those at the bottom are Kirchoff's.

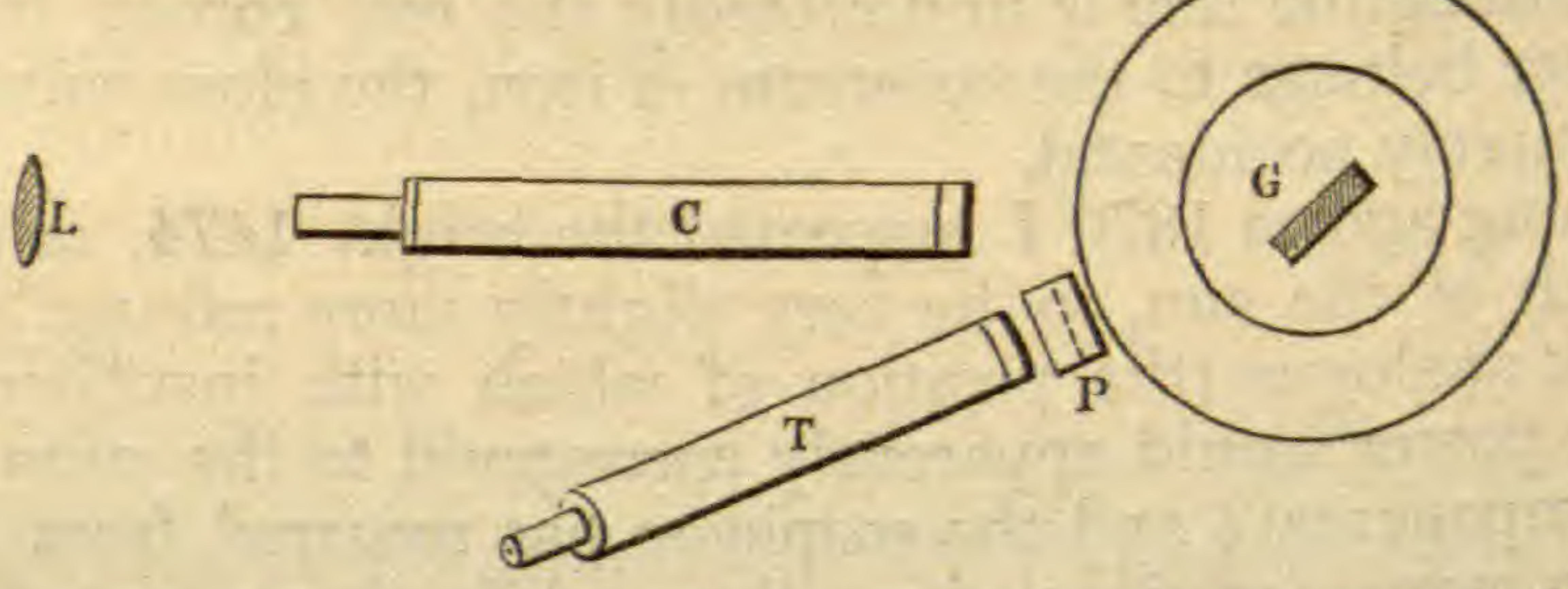
1.

Map of the region of the Corona-line.



The observations are best made in the spectrum of the 8th order (with the coarser grating—in the spectrum of the 5th order with the grating of 17280 lines to the inch) although the duplicity of the line is clearly visible in the spectra of the 6th, 7th, and 9th order, with proper precautions. The arrangement employed is that indicated in fig. 2. C is the collimator, the

2.



beam of light, thrown into the room by a heliostat, being concentrated by the lens L which forms an image of the sun on the slit. G is the "gitterplatte." P is a prism of 45°, with its refracting edge horizontal, and so placed as to bend upward the pencil of rays from G. T is the observing telescope, with the eye end elevated at an angle of about 35° so as to receive the rays from G after they pass the prism.

The spectra of the higher orders so overlap, that without the prism, or some analogous contrivance, it is impossible to observe in them any but a few of the strongest lines. By the prism these spectra are separated, one lying above the other; the red of the 6th order, for instance, falling below the yellow of the 7th, and this underneath the green of the 8th, while above this green lies the blue of the 9th order, and above that the extreme violet of the 10th. Thus the different spectra no longer interfere, and it is just as easy to observe the spectrum of the 8th order as that of the 1st, except that the former is fainter on account of the greater dispersion, and the obliquity of the grating, which narrows the transmitted pencil. A direct-vision prism in the eye-piece of the telescope answers the same purpose, but less perfectly. The same plan may have been used before. If so, however, I am not aware of it.

Hanover, N. H., April 19, 1876.

ART. LV.—*Contributions from the Sheffield Laboratory of Yale College.* No. XL.—*On a Lithia-bearing variety of Biotite;* by GEORGE W. HAWES.

THE feldspar quarries about Portland and Middletown in Connecticut have furnished many interesting minerals. The quarries are in the large granitic veins which intersect the gneiss and mica schist of the region. These veins, which have been described by various authors, are remarkable for the similarity of their mineral constituents, and the presence in several of rare elements, and it will be shown in this article that a lithia-bearing biotite is generally present. The feldspar is of two kinds, orthoclase and albite; considerable quartz is found, and with the feldspar it often forms beautiful specimens of graphic granite. Tourmaline, beryl, garnet, columbite, muscovite and biotite are common, and other species are found more rarely. Plates of muscovite and biotite united by their edges are of frequent occurrence, and sometimes one forms the center of a large crystal or plate of the other. The cleavage lines which are developed by striking the thin plates with a sharp point show that when the two species are thus united, there is a simple relationship between the axes of the muscovite and biotite, as long since found by Prof. G. Rose to be a general fact in the case of such combined crystals of mica. Some of the specimens examined by Rose came from granitic veins similar to those of Portland and Middletown. The cleavage lines begun in one mica often run some distance into the other without a change of direction, proving the complete continuity of the two.

The biotite of Portland is black, and possesses a high luster. In thin plates it is transparent, clear, and brown by transmitted light. It is optically uniaxial. Specific gravity 2·96. When heated before the blowpipe it imparts to the flame the characteristic carmine-red color of lithia, and it appears thus to differ from all the described varieties of the species. It afforded me, on analysis, the following results:

	I.	II.	Mean.
Silica	35·59	35·62	35·61
Alumina	19·99	20·08	20·03
Ferric oxide	·15	·11	·13
Ferrous oxide	21·83	21·86	21·85
Manganous oxide	1·19	1·18	1·19
Magnesia	5·26	5·20	5·23
Potash	9·61	9·76	9·69
Soda	·49	·55	·52
Lithia	·96	·94	·93
Titanic acid	1·45	1·47	1·46
Fluorine	·76	·76	·76
Chlorine	*tr.*	*tr.*	*tr.*
Water	1·84	1·90	1·87
	99·12	99·43	99·27

The ratio of the $\text{R} : \bar{\text{R}} : \text{Si}$ is $1 : 1 : 2$; and the analysis hence shows that the mica is an *iron* biotite, which has lithia replacing part of the potash, and which contains much more ferrous oxide and less magnesia than is common. It fuses before the blowpipe to a magnetic globule. Some specimens fuse with greater difficulty and give little color to the flame, indicating a transition toward the more ordinary variety. I would call attention to the fact that the iron of this mica oxidizes with great readiness both when heated and when exposed to the weather, and this shows how easy it would be to derive lepidomelane from it by alteration. This lithia-biotite seems to be widely distributed in granitic veins. Specimens from similar granitic veins in New Hampshire, Massachusetts and North Carolina were examined which imparted more or less of the lithia coloration to the flame, fused to magnetic globules, and which possessed the same physical appearance and the same association as the Portland biotite.

ART. LVI.—*Researches on the Solid Carbon Compounds in Meteorites;* by J. LAWRENCE SMITH, Louisville, Ky.

[Concluded from page 395.]

Action of strong nitric acid on the graphite.—Strong nitric acid, poured upon the powdered graphite that had been treated with ether and bi-sulphide of carbon acted vigorously upon the sulphide of iron mixed with it. and after digestion for some time in the acid and washing thoroughly with water, there remained 55 p. c. of the original matter, which consisted of carbon. This burnt in air with great difficulty, but very easily in oxygen, leaving a residue of one per cent of ash.

The nitric acid solution was analyzed and found to contain

Sulphur	35·05	Troilite.
Iron	62·21	
Cobalt	0·56	
Nickel	0·16	
Magnesia	0·30	
Silica	0·21	
	98·52	

It is a fact of some interest that in the sulphide of iron which occurs in meteoric irons (when these nodules are perfectly free from any adhering iron) the quantity of nickel and cobalt present is very minute, a most singular fact if we are to regard these nodules as the result of segregations from the mass of iron. And still further, while the nickel is very largly the predominant metal of the two in the iron, I have noted that the cobalt predominates over the nickel in the sulphide nodules; but I would not without further examination regard this as likely to be the rule in all cases.

Action of fuming nitric acid mixed with potash chlorate on the meteoric graphite.—The oxidation of graphite by this method is well known to chemists, it having been first pointed out, as I have stated, by Mr. Brodie in 1860, and subsequently by Berthelot in his elaborate memoirs published in the *Annales de Chemie et de Physique*, 4th series, volumes xix and xxx. The result is the formation of a substance which Brodie called graphitic acid and Berthelot graphitic oxide, although the compound invariably contains hydrogen as an essential element in its constitution. M. Berthelot made use of this reaction to study the different forms of carbon, finding that the results of the oxidation varied with the carbon from different sources, only those forms of carbon known as graphites proper furnishing the graphitic oxide. The same chemist studied this oxidizing action upon the graphite from the Cranbourne meteoric iron, and also upon the carbon from the Orgueil meteorite, and found that, of the two, only the graphite from the meteoric iron gave rise to the oxide.

The study of this subject I have pushed further, and have oxidized the graphites from the Sevier County and the De Kalb County meteorites, and have also re-examined that of the Cranbourne iron—having been very liberally furnished with a specimen by my friend, Prof. Maskelyne, of the British Museum. The carbonaceous matter from the Orgueil meteorite has also been subjected by me to the same reaction.

The details of conducting the process were identically those proposed by M. Berthelot in the memoirs already referred to, viz: To free the powdered graphite from sulphide of iron by first treating it with strong nitric acid, washing it thoroughly on a filter, drying it and mixing it with five times its weight of potash chlorate, then adding this mixture little by little to sufficient fuming nitric acid to moisten thoroughly the mass. In making the mixture, I place the nitric acid in a capsule and the latter in a little water with a piece of ice, thus avoiding any risk of explosion. The mixture, after standing a few hours is transferred to a ballon d'assais, and gently heated in a water bath at a temperature from 50° to 60° C. for several days. The result of this action upon the graphites of the Sevier County and DeKalb County meteoric irons was the formation of graphitic oxide, with all the characteristics of that furnished to Berthelot by the graphite from the Cranbourne iron, as well as to myself from this last graphite.

The conversion of the meteoric graphite into the oxide is more rapid than that of any terrestrial graphite with which I have experimented. The graphite soon changes from black to green, and finally, after two or three applications of the oxidizing agent, to a perfectly white substance. This, when filtered, washed, and dried under a bell glass with sulphuric acid, gives a yellow powder, somewhat adherent. If the oxidizing action of the nitric acid and potash chlorate be renewed several times on the same material, the oxide gradually diminishes in quantity, and if the process be stopped after the fourth or fifth treatment, the oxide is very gummy, adhering to the filter and preventing complete washing. When dried on the filter it adheres firmly, but can be detached by moistening the filter and rubbing off the paper with the finger, leaving tenacious flaky films.

The reaction and decomposition of the oxide obtained from the Sevier graphite is the same as that of the oxide from other sources.

My experiments on terrestrial graphites have been confined to the Siberian, Cumberland, and Ceylon varieties; and they show that the graphite under consideration differs from them in being more readily converted into oxide, it requiring only one-fifth to one-third of the time; and if the operation be continued by frequently renewing the oxidizing agents, the oxide first formed gradually diminishes in quantity, being more thoroughly altered, like some of those forms of carbon ranked as not properly graphites.

In fact, it appears that the meteoric graphites, when tested by this process, occupy a place between graphites proper and ordinary carbon, but much nearer to the graphites.

After completing my examination of the carbon nodules of the irons, my aim was to see what general deductions could be made with reference to the relations this graphitic material bore to the carbon found in the black meteoric stones. The material to operate with is, however, very rare; but I had in my collection enough for all necessary comparisons, though needing much more in order to obtain the peculiar products in sufficient quantities for chemical analysis.

The Alais meteorite.

Two grams of this meteorite were pulverized finely and treated with boiling water, which dissolved out a small amount of matter; which substance has been studied by others and it is not my object to recur to here.

The powder was then dried and treated with pure ether, in the same manner as the graphite from the Sevier iron, and the ether allowed to evaporate slowly at a moderate temperature, when the sides of the vessel became covered with acicular crystals, mixed with a few rhomboidal crystals. The residue had a peculiar odor, similar to that of the ether extract from the graphite of the Sevier iron, which odor it nearly lost in the same way, after several days exposure to the air. The form and appearance of the crystals are the same as of those obtained from that graphite; and a portion of the crystals detached and heated in a small tube gave the same character or reaction.

These crystals have already been studied by Prof. Roscoe, of Manchester, as carefully as could be done with the minute quantity at his disposal. My examination is perfectly in accordance with his, and there is no doubt that this product and that from the graphite must be of the same nature.

We must not forget to mention that Prof. Wöhler was the first to call attention to the hydrocarbon in these black meteorites when examining the one which fell at Kaba.

Orgueil meteorite.

This meteorite is one of the most interesting of all the known carbonaceous meteorites. And there are one or two points connected with it that do not appropriately belong to this paper, of which I will furnish a note before long. Through the liberality of Prof. Daubrée, and the Administration of the Garden of Plants, I have been furnished with the material on which my investigations have been made. This meteorite has, in most respects, been thoroughly examined by M. Cloez, and by M. Pisani, and their results given in the Comptes Rendus for 1864. The former chemist examined the carbonaceous matter as a whole, considering it to resemble humus; and this on drying at 110° gave him: Carbon 63·45, hydrogen 5·98, oxygen 30·75.

I have, as yet, done little toward the re-examination of this substance, which represents from four to six per cent of the entire meteorite, my examinations being made principally for those crystalline products, soluble in ether and bisulphide of carbon, of which I have found about one-half per cent in the meteorite.

The powdered meteorite was first treated with water and heated over a water-bath, and every thing soluble in that menstruum thoroughly washed out. The soluble portion, dried at 100° C., represents 8·65 per cent of the mass. After carefully drying the insoluble portion at 100° C., it was treated with ether in the same manner as the meteoric graphite. The ether was used in large excess, and allowed to remain for ten or twelve hours in contact with the material; the ether was filtered off, and the residue on the filter well washed with ether. The etherial solution was evaporated slowly, when the same acicular crystals made their appearance as in the case of the graphite, and numerous rhomboidal crystals were deposited in the bottom of the beaker. These appeared to be identical with those from the graphite. The action of heat on these crystals is the same as on those from the Sevier graphite.

The powdered meteorite exhausted by the water and ether was next treated by the bisulphide of carbon, when an additional quantity of soluble matter was obtained. On evaporating the bisulphide of carbon, a yellow mass remained having the aspect of sulphur. This, when heated, gave evidence of being sulphur mixed with some carbon compound. And to all appearance it was just like the substance obtained by similar treatment of the meteoric graphite.

The crystals in the upper part of the vessel from which the ether was evaporated being detached by scraping the sides of the vessel with a horn spatula, some bisulphide of carbon was poured upon the portions remaining attached to the vessel by which it was dissolved. The bisulphide of carbon was subsequently evaporated, when a residue was left consisting of a yellow solid surrounded by a dark brown semi-solid mass in minute quantity. This last is evidently a carbon combination not contaminated with sulphur, while the yellow mass is sulphur containing a small portion of the carbon compound.

I was enabled to obtain over 400 milligrams of these mixtures from about 50 grams of the meteorite, much the larger portion being sulphur. A few attempts were made to separate the sulphur from the carbon compound, but unsuccessfully; and I soon saw that by continuing my efforts, I should exhaust the small supply of material without reaching any useful result. So it was thought better to save what was left of the material as a specimen of it.

The other carbon meteorites I have not yet examined with regard to the points embraced in this report, but I hope to obtain sufficient material before long to allow of this being done, though I do not anticipate any different results from those that have been examined.

The nature of the hydrocarbon found in the meteoric graphite and carbonaceous meteorites.

That this substance belongs to the meteorites at the time of their fall, there can be no doubt; for in the carbonaceous meteorites there is nothing to enable us to account for its formation in the cabinets in which they have been placed after their fall. And in the case of the graphite nodules they were encased in the interior of an iron mass over twenty centimeters in diameter; and, furthermore, the powder operated with was taken from the interior of a compact nodule of graphite.

I have been strongly inclined to consider this as a hydrocarbon containing combined sulphur forming a sulph-hydrocarbon. In the absence of chemical evidence sustaining this view, I lay some stress on the peculiar odor of the ether extract, strengthened by a most singular property of the watery extract from the Orgueil meteorite, of which I will make a short statement, reserving for some future occasion any additional remarks.

If a small quantity of the powdered Orgueil meteorite, say two grams, be treated with water and heated for a short time over a water-bath, no peculiar odor will be observed, however carefully examined. Throw this on a filter and wash with water, then evaporate the filtrate to dryness over a water-bath, and during this time no odor will be observed. Allow the residue to cool, and still there is no odor. But now throw upon the residue a little water, say half to one cubic centimeter, move the capsule around to dissolve the mass, and then on bringing it near to the nose, a marked alliaceous odor will be perceived, sometimes so strong as to be disagreeable, reminding one of the odor of the oil of assafœtida. That it is produced by a sulphur compound chemists will be apt to admit, perhaps a minute quantity of sulphur compound not unlike the sulph-hydrate of ethylene $C^4H^6S^4$; and the needle-shaped crystals may not be far removed from the solid quintisulphide of ethylene, $C^4H^6S^5$, corresponding to sulphur 75·00, carbon 20·00, hydrogen 5·00. The crystals I scraped from the sides of the beaker—at the upper part—on which the ether solution of the Orgueil meteorite was evaporated to dryness, gave—sulphur 79·65, carbon 15·00, hydrogen 3·00.

In the above analysis the amount of sulphur is well determined; but the examination for carbon and hydrogen was made upon so small a quantity, that the results cannot be relied upon as very correct.

Roscoe burnt in dry oxygen ·008 grams of the residue from the Alais meteorite, and obtained ·010 grams of sulphurous acid, ·008 grams of carbonic acid, and ·003 grams of water, making sulphur 125 parts, carbon 54 parts, hydrogen 10.

As the above analysis was made with only eight milligrams, of course the results can be considered only as an approximation; but nevertheless, until we get better they must serve as our only guides.

I have not said anything about the gaseous carbon compounds found in meteorites, as these form a separate study from what is designed in this paper, and besides, Profs. Graham, Mallet, Wright, and others have already investigated their nature. Profs. Wright and Mallet are still engaged in this line of investigation.

Conclusions.

These then are some of the results of my experiments on the carbon of meteorites, and they are of great interest and importance. That we should find in the graphitic concretions from the interior of a solid mass of iron such substances as free sulphur and a hydrocarbon, simple, or combined with sulphur, having a marked odor, was certainly not to be expected, especially as we are almost forced to believe that the iron containing it must have been at some period in a state of fusion.*

The graphite nodules themselves are grand chemical and physical puzzles, as well as all the nodular concretions in meteoric irons; that they have resulted from a process of segregation is self-evident, but how marvelous the completeness of this segregation, for if we analyze the iron even within two or three millimeters of the concretions, only traces of the charac-

* In an article recently published by Dr. Mohr (Annalen der Chem. und Pharm., Dec. 1875, page 257), he advances the theory that meteoric iron and meteoric stones have been formed by the agency of water; his arguments are interesting, but far from being sufficient to cover all the facts in connection with meteorites.

teristic constituents of the nodules are here found. Then again, in the case of the troilite concretions, this sulphide has been separated from the mass of iron, and a phosphide of iron and nickel has been concreting along with it; and yet, there seems to be so great an incompatibility between these two minerals that they could not commingle, but the phosphuret was thrust, as it were, to the exterior of the nodule, there to form a thin covering to the sulphide, like the skin of an orange over the internal pulp.

Again, the graphitic concretions bear no resemblance to the scaly graphite found in the slag of iron furnaces and between the crystals of cast iron, either in structure or appearance; the fractured surface is more like that of the Borrowdale graphite, but the oxidizing action of the nitric acid and potash chlorate on this last differs somewhat from the action on the meteoric graphite. Many and varied have been the hypotheses formed in my mind to account for the formation and accumulation of this graphite, but I must admit that I have been forced at last to abandon them all, as none covers all the facts of the case. In appearance this graphite is more like the amorphous carbon that is separated from cast iron, but the oxidizing action of nitric acid and chlorate of potash at once points out their great difference as shown by Berthelot's experiments.* And although it differs in appearance from the scaly graphite of iron, the oxidation of the two are very similar. I am more inclined to adopt the suggestion of Berthelot, that it may be formed by the reaction of bisulphide of carbon upon incandescent iron, as this reaction is known to give rise to an amorphous graphite analogous to the one under consideration, and its association with sulphide of iron would lend some support to this hypothesis; and still further the presence of free sulphur and a carbon compound, either a hydrocarbon, or sulph-hydrocarbon, points also in that direction for a solution.

It is very clear from the present accumulated knowledge of the geological occurrences of graphite that we must abandon all attempt to account for its formation by any one series of reactions on the interior of our globe; for it is to be found in basaltic rocks, in the older crystalline rocks, and through all the series of rocks up to the recent Tertiary formations; and when we add to this the laboratory experiments of Berthelot that I have so frequently quoted, this view of the subject is strengthened. But on this point I may have something more to say in a paper on the Ovifak iron, and the graphite in the basalt in which this iron is found.

The carbon from the black meteorites, as the Orgueil, Alais, etc., I consider as having a similar origin to that found in the

* Annales de Chem. et de Physique, Fourth Series, xix, 425.

irons; for I have proved that they both contain similar crystalline products soluble in ether and sulphide of carbon, and while the carbonaceous matter reacts differently when treated with nitric acid and potash chlorate, this may arise from the difference of conditions under which the reaction took place that gave rise to it.

That the carbonaceous matter in the black meteorites is to be regarded as a kind of humus arising from organized matter is contrary to all we know about humus. For if we examine the mineral constituents of these meteorites, we find them to be a granular mass, with particles more or less impalpable, composed essentially of olivine and pyroxene, a most unpromising soil for so luxuriant a growth of vegetation as must have occurred to produce so abundant a percentage of carbonaceous matter as that found in the Orgueil meteorite. The action of caustic potash upon it is different from the action of that alkali upon what is commonly called humus; (although we must bear in mind that humus is not a well-defined substance; it being commonly regarded as vegetable matter that has not undergone complete decomposition into water and carbon, but by imperfect oxidation was converted into a varied mixture of carbon and certain organic compounds rich in carbon, some of them soluble in caustic alkalies). After the powdered Orgueil meteorite has been exhausted by water, ether, and sulphide of carbon, caustic potash or soda dissolves but an exceedingly minute trace of the carbonaceous matter. and even that trace may be a little hydrocarbon not extracted from the mass by the ether and sulphide of carbon. If a portion of the same be dried at 110° C., and then heated in a closed tube, water will not be given off until the temperature is elevated considerably. If the temperature be further increased, only a very slight odor is apparent; and this is another marked difference between it and humus. If heated on platinum foil, the carbonaceous matter burns off very readily with little or no odor, leaving an abundant residue. According to my experiments this combustible matter amounts to about 4·5 per cent of the entire meteorite.

It is not at all improbable that the carbonaceous matter of the black meteorites approaches in character the so-called hydrated carbon first pointed out by M. Eggert, but so clearly defined by MM. Schutzenberger and Bourgeois in a communication made to the Chemical Society of Paris in April, 1875, which was obtained from white cast iron by dissolving away the iron. But it is a question in my mind whether the carbon combination thus obtained from white iron is to be properly considered a hydrated carbon; that is to say, whether we are to consider the H^2O as united to the carbon in the same way as it is to metallic oxides to form what are known as hydrated oxides. If, however, it is to be considered as combined in a manner analogous to the H^2O, with ethyl to form alcohol, then there may be some plausibility in the hypothesis. For it will be remarked in referring to the actions of this hydrated carbon that it in no way resembles amorphous or ordinary carbon.

It is represented by MM. Schutzenberger and Bourgeois as follows: $C^{11}:3H^2O$=carbon 70·95, hydrogen 3·23, oxygen 25·80 per cent.

According to M. Cloez, the carbonaceous matter of the Orgueil meteorite, after being dried at 110°, was found to be composed of carbon 63·45, hydrogen 5·98, oxygen 30·75; and when we consider that some of this hydrogen belongs to the hydrocarbon now known to exist in that meteorite, the remainder of the hydrogen will approach near the proportion required to form water with the oxygen; and the quantity of carbon that may exist as a hydrate will be slightly diminished.

Attempts were made to separate completely all the mineral matter from the carbon, but I have failed to do so, after using fluorhydric acid alone, and in conjunction with nitric acid, also fluoride of sodium and sulphuric acid with a small amount of water, then treating the residue with cold nitric acid. There is no difficulty in getting rid of a great part of it, but in every instance the carbonaceous matter has been altered, however carefully the temperature was managed.

When this matter thus obtained is heated in a closed tube, after being dried at 110° C., it not only furnishes water at about 250° C., but gives out a very strong odor somewhat like that produced from certain bituminous coals, at one point resembling the disagreeable odor of an ignited cigar of a very inferior quality of tobacco.*

Viewed in the light of these experimental researches, the most reasonable conclusion is that this carbonaceous matter is not in any proper sense either carbon or humus, but a carbon compound analogous to the one just referred to.

Future researches upon these solid compounds, resembling in appearance amorphous carbon, such as hydrographitic oxide, pyrographitic oxide, carbon-hydrate, and similar compounds that may yet be discovered, will doubtless throw some light on the true nature of the carbonaceous compound of the black meteorites. So far as our knowledge now extends, its formation and its origin are wrapped in as much obscurity as the origin of the bodies in which it is found.

What we do know is that this carbonaceous matter occurs with the same minerals, viz., olivine and pyroxene, which are the predominating constituent materials of all stony meteorites;

* This odor will be found to belong to the hydrated carbon from cast iron, when heated in the same way.

also with the nickeliferous iron found in both the stoney and metallic meteorites; and furthermore, that this carbonaceous matter contains curious crystalline products soluble in ether and sulphide of carbon, which last have been traced in the graphite nodules in the interior of the metallic meteorites. Moreover in these graphite nodules we have found magnesia, which is so uniformly a constituent of the minerals of the stoney meteorites.

So far then as our present knowledge goes, we know of celestial carbon in three conditions, viz: in the *gaseous form* as detected by the spectroscope in the attenuated matter of comets; in meteorites in the *solid form*, impalpable in its nature and diffused in small quantities through pulverulent masses of mineral matter that come to the earth from celestial regions; also in the *solid form*, *but compact and hard*, resembling terrestrial graphite, and this is imbedded in metallic matter that comes from regions in space. But while we speak of these as forms of carbon, I think we should be careful in associating it in our minds with the element carbon as we understand it in its pure state whether crystallized or amorphous, for I cannot reconcile the carbon vapor detected in comets as simply that known as pure carbon in the form of an elastic vapor, nor are we to circumscribe ourselves with the notion that this cosmical carbon has an organic origin.

The researches embraced in this communication, while in many respects of a novel character, are imperfect from their very nature, both from lack of material for a thorough and complete study, as well as from the present imperfect methods of operating upon a minute quantity of the most interesting of the substances obtained.

I have therefore detailed as carefully as I could all the results as they have developed themselves, hoping that future opportunities may be afforded for continuing them, when new celestial messengers of the carbonaceous type shall visit our globe.

ART. LVII.—*Results of Experiments on Contact Resistance;* by Professor W. A. NORTON.

[Read before the National Academy of Sciences, April 21, 1876.]

THE experiments here referred to were undertaken with the view of determining the law of the diminution of the minute distance between two surfaces in contact, with the increase of the contact pressure; and its dependence on the extent, condition and nature of the surfaces in contact. Rectangular pieces of various substances $\frac{1}{8}$ inch in thickness, $\frac{1}{4}$ inch in width, and of suitable length for clamping were used in the experiments. The lower piece was clamped to a horizontal iron bar, which was firmly clamped to the vertical pillars of the testing machine used in my former experiments on deflection and set, and was also firmly propped directly beneath the point where the contact occurred. The other piece, $\frac{3}{4}$ inch in length, was keyed to the under surface of the lever used in the same experiments, at the farther end. The weights were placed on a scale pan resting above this on the lever, and vertically over the surfaces in contact. The depressions of this end of the lever were determined by means of a micrometer screw, which gave the equal elevations of the other end to within $\frac{1}{40000}$ of an inch. The firmness of the lower contact piece and its support was frequently tested by causing the weights to press directly upon it, without the intervention of the lever. The small thermal error of the apparatus was carefully determined and allowed for whenever any perceptible change of temperature occurred during any single series of experiments; but the precaution was taken to secure a nearly uniform temperature during the progress of the experiments. The weights employed, in the more precise determinations, ranged from 2 ounces to 24 ounces. The apparent surface of contact varied from $\frac{1}{32}$ of a square inch to a mere point. The touching surfaces were in some instances smooth, in others rough; and in the contact of plate glass with plate glass, highly polished. The decrement of contact distance was noted whenever a weight was put on, and the increment when the weight was removed, and in general the average of the two taken. By this means the thermal error, when the rise or fall of temperature was uniform, would be eliminated; as well as any error that might result from a change in the coefficient of the contact resistance, induced by the pressure and not passing off when the weight was removed. That errors from irregular variations of temperature, irregular variations of the coefficient of molecular resistance, and accidental causes, might be in a great degree eliminated, the mean of a considerable number of separate determinations was obtained in each case. A comparison of these means for sets of experiments differing in number, showed that the irregular and accidental errors were generally small. The initial pressure was the same in the different sets of experiments, and was very slight—being barely sufficient to secure a decided contact.

When a weight was applied the resulting diminution of the contact distance was generally greater than the increase that resulted from the removal of the weight. The reverse very rarely occurred; though the increment was sometimes equal to the decrement. It therefore generally happened that there was a slight contact set when the pressure was withdrawn. These facts show that the application of the contact pressure was generally attended with a diminution of the coefficient of molecular resistance at the surface of contact. When the pressures were renewed at short intervals, the contact set at first observed was generally maintained, and often increased.

The following table gives the diminutions of the contact distance obtained with the several weights, 2 oz., 4 oz.., 8 oz., 16 oz., and 24 oz. It is to be understood that the numerical determinations given in the table are the means of a number of individual determinations. It thus happens that the decimals are carried beyond the reliable reading of the apparatus. The mean results of different sets of experiments are given in two instances. The apparent surface of contact was about $\frac{1}{32}$ of a square inch, except in the case of the contact of a flat surface with a round surface of sharp curvature, in which the area of contact was too minute to be estimated.

	Iron on Iron.	Same.	Average.	Iron on iron,	Same.	Average.
	Surf. smooth.	Same.		Flat surface on round surf.	Same.	
	In.	In.	In.	In.	In.	In.
2 oz.	0·000170	0·000162	0·000166	0·000165	0·000162	0·000163
4 "	·000250	·000285	·000267	·000240	·000275	·000257
8 "	·000340	·000325	·000332	·000320	·000275	·000297
16 "	·000450	·000425	·000437	·000410	·000425	·000417
24 "		·000500	·000500			

	Iron on brass.	Brass on brass.	Brass on brass.	Plate glass on plate glass.	Gen'l average	Reliable average.
	Surf. smooth.	Surf. smooth.	Surf. rough.	Surf. polished		
	In.	In.	In.	In.	In.	In.
2 oz.	0·000167	0·000170	0·000175	0·000170	0·000169	0·00017
4 "	·000256	·000250	·000267	·000212	·000251	0·00025
8 "	·000335	·000256	·000250	·000294	·000294	0·00029
16 "	·000412	·000410	·000400	·000350	·000404	0·00040
24 "	·000500	·000500	·000500	·000475	·000493	0·00049

On examining this table it will be seen,

(1.) That the diminutions of contact distance were very nearly the same, whatever was the nature, or condition of the surfaces in contact.

(2.) That they were nearly independent of the extent of the surface in contact; since they were nearly the same when the surfaces touched in a mere point, as when the surface of contact had an extent of one-fourth of an inch by one-eighth of an inch.

(3.) That the diminution of contact distance for an increase of one ounce in the pressure, was nearly inversely proportional to the pressure. The fractions of an inch that would answer to this law are as follows: For 2 oz. 0·00017 in., for 4 oz. 0·00025 in., for 8 oz. 0·00033 in., for 16 oz. 0·00041 in., for 24 oz. 0·00046 in. These values differ but little from those given in the table as the reliable averages. The only material discrepancies occur in the results for 8 oz. and 24 oz. Now the table of results shows that in a few cases some cause was in operation to reduce the diminution of contact distance for 8 oz. to nearly the value observed for 5 oz. The same tendency was also often manifest in the individual experiments. If we reject the results for 8 oz. in these cases, that occur in the table, the average diminution of contact distance for a pressure of 8 oz., comes out 0·00032 in., and the discrepancy is reduced to 0·00001 in. Again the experimental result for the case of 24 oz. is 0·00003 in. larger than the law above stated calls for; but the individual micrometer readings were liable to this amount of error, and hence if the support had been depressed by this amount, by the 24 oz. weight, it would have escaped detection.

That the law of diminution of the contact distance which has been stated is very nearly, if not the exact law of Nature in the case, may also be inferred from the fact already stated, that the variation of contact distance is nearly if not entirely independent of the extent of the surface of contact. For if the contact area be diminished in any ratio, say 2 to 1, under the pressure of the same weight the pressure at each individual point of contact would be doubled, and the increment of pressure at each point, resulting from an additional weight of one ounce, would also be doubled. Now if we suppose the law, just referred to, to hold good for a given surface of contact, the diminution of contact distance at each point should be inversely proportional to the pressure on it, and therefore be half as great for the same increment of pressure there, as in the case of the larger area of contact; but in fact the additional pressure at a single point, resulting from an additional weight of one ounce, is doubled, and hence the diminution of distance should be the same as in the case of the larger area of contact.

We may conclude, therefore, that in the contact of surfaces, the force of molecular repulsion, in which the force of contact resistance consists, conforms in its variations very nearly, if not exactly, to the law that the decrement of the distance between the molecules, for the same small increment of pressure, is inversely proportional to the effective pressure by which the molecules are urged into closer proximity. If then we suppose the distance between the molecules to be denoted by x, and the effective molecular repulsion by r, and observe that x is a decreasing function of r, we may put $dx=-m\frac{dr}{r}$. This gives, by integration, $x=c-m\log r$; or $x=m\log\frac{n}{r}$, in which n is a new constant. It appears then that the curve of the effective molecular repulsion, which resists contact pressure, is the logarithmic curve.

This force of molecular contact repulsion cannot be identical with the effective repulsion in operation in the interior of bodies, when they suffer compression; for the same force of pressure produces a vastly greater diminution of molecular distance at the surface of contact than in the interior of bodies. Thus, in our experiments, a pressure equivalent to 30 lbs. to the square inch, diminished the contact distance by $\frac{1}{2500}$ of an inch. This pressure operating on an iron rod one inch in length would compress it $\frac{1}{8000000}$ of an inch. The distance between its individual molecules would be reduced $\frac{1}{8000000}$ part. This is immeasurably smaller than the observed diminution of contact distance; and therefore than the diminution of molecular distance at the point of contact, if the decrease of contact distance consisted simply in the closer approximation of the contiguous molecules of the two surfaces. It is not improbable, however, that it consists in part in a compression of a thin layer of molecules at the surface, having a comparatively small coefficient of elasticity. If such a layer have a thickness as great as $\frac{1}{100}$ of an inch, the compression it would receive from a pressure of 30 lbs. to the square inch, would still be 32000 times greater than a layer of the same thickness in the interior of a mass of iron would experience from the same pressure.

We must conclude, therefore, that the force of molecular contact repulsion has, for the same diminution of the distance between the molecules, an exceedingly feeble intensity in comparison with that of the internal molecular repulsion. It must operate then at greater molecular distances; and accordingly the range of its action must lie outside of that of the attraction of cohesion. In confirmation of this conclusion it may be stated that in none of the experiments was any evidence obtained of an attraction between the surfaces, operating outside of the contact distance.

It would seem, then, that the experiments discussed have served to establish the existence of an effective force of molecular repulsion, in operation at the surface of contact of bodies, whose sphere of action is external to the range of the attraction of cohesion for the same molecule, and which has a much feebler coefficient of intensity than the effective molecular repulsion exerted within the sphere of this attraction. They have also made known the law of variation of this force with the change of molecular distance, and shown that its coefficient of intensity is the same, or nearly the same, for the different substances used in the experiments.

ART. LVIII.—*On some Physical Observations of the Planet Saturn*; by L. TROUVELOT.*

DURING the last four years I have had many occasions to observe the planet Saturn, and to study its physical constitution under very favorable circumstances. My series of observations extends over more than a hundred nights, many of which were as good as could possibly be desired, both for the steadiness of the image, and for the amount of light.

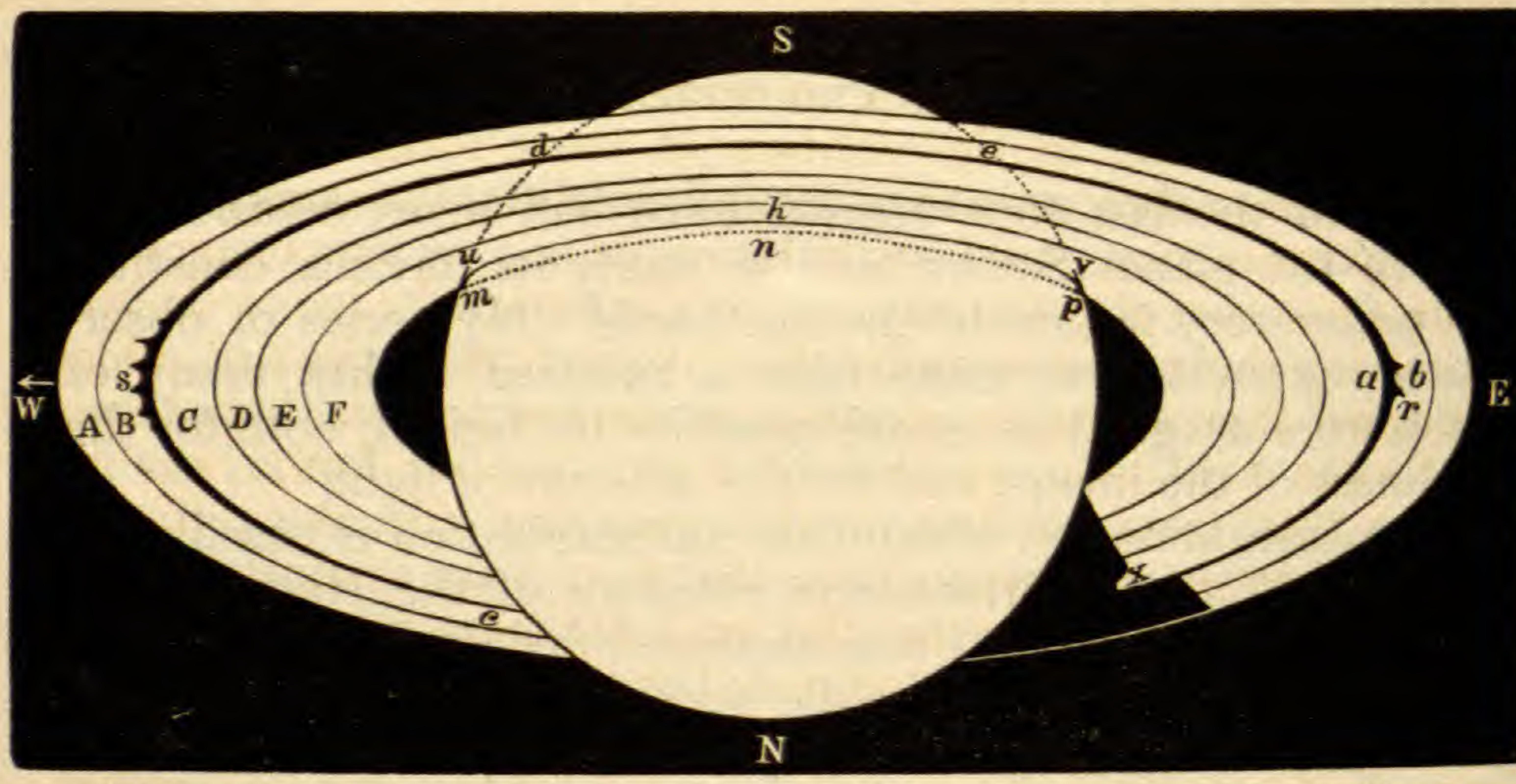
The observations on which this communication is based were made: 1. With the fifteen-inch refractor of the Harvard College Observatory, while I was employed by Professor Winlock in making the sketches for the series of the astronomical engravings published by him. By his kind permission I have availed myself of considerable of the data thus obtained. 2. With the twenty-six inch refractor of the Washington Observatory while it was still in the hands of Messrs. Alvan Clark & Sons. 3. With the six-and-one-quarter-inch refractor of my own Physical Observatory at Cambridge. During the past summer, I was honored with an invitation from Admiral C. H. Davis, Superintendent of the Naval Observatory, to visit Washington and make some sketches with the magnificent instrument of this establishment. I thus had an excellent opportunity to confirm all my previous observations. The powers used ranged, according to the amount of light and the steadiness of the atmosphere, from 140 to 700. On good nights, however, higher powers have been tried, but never with advantage, as the light lost by the use of high powers is generally of more importance for good vision than a superior enlargement with a reduced amount of light.

Numerous observers, among whom are such eminent astronomers as Sir William and Sir John Herschel, Otto Struve, Dawes, Bond, etc., have made careful studies of this planet; and it is not, therefore, to be expected that very important discoveries remain to be made by later observers. As I have had the opportunity of observing with the same instrument many of the celestial objects previously studied with so much success by Professor George P. Bond, it gives me the greatest pleasure to express my admiration for the accuracy and fidelity of his observations.

The following diagram, representing the outlines of Saturn and its rings, will facilitate my explanations, and give clearness to the subject:—

* Read before the Amer. Acad. of Arts and Sciences by William A. Rogers, Dec. 14, 1875.

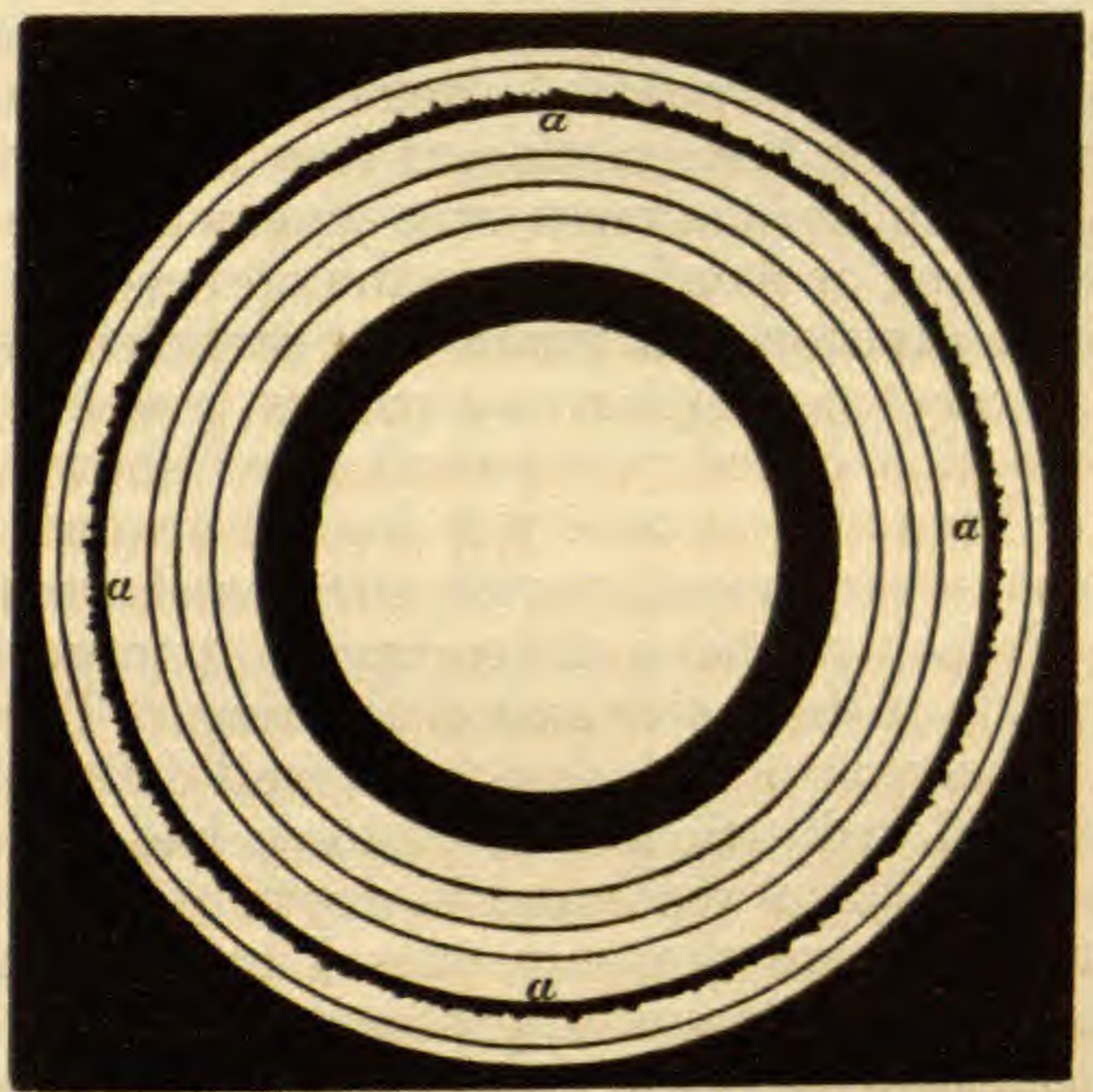
1.



When looking at the rings, attention is at once attracted to a conspicuous dark line, apparently concentric with the outer margin of the rings, and boldly surrounding the planet, and adorning it by its sharp contrast. This dark line is known as "the principal division of the rings," and is shown at *a*, fig. 1. Owing to the effect of perspective, it always appears widest at the two extremities of its major axis, on that portion called "the ansæ," as there only, it is seen without foreshortening. I have carefully compared the intensity of this dark line with the sky outside of the rings, and inside of the ansæ; and I have always found it to be slightly lighter. All my observations also agree in showing this line as appearing a little narrower on the side farther from the observer, at *c*, fig. 1, than it appears on the opposite side, at *d*. This phenomenon could readily be explained by supposing that the outside margin of the ring *C* is on a plane higher than the ring *B*, and may, consequently, conceal a narrow portion of the dark line. The assumption of such an hypothesis seems to be fully supported by the observations, as will be shown hereafter. It is furthermore to be remarked, that the outside margin of the ring *C* has always appeared to me to be more sharply defined on that part of the ellipse farther from the observer than on the side nearest. The case is the same for the outer border of the ring *A*, which appears sharper on its northern than on its southern side. In both cases, the northern portion of the ellipse is limited by the matter composing the surface of the rings on their flat and illuminated side; while for the southern portion it is seen a little edgeways, and this may account for the vagueness of its outlines on this side.

Soon after the beginning of my observations, in October, 1872, my attention was called to a singular appearance not heretofore noticed, as far as I am aware. Two small, dark, angular forms, *r*, fig. 1, were seen near the summit of the principal division of the rings on the following side, and apparently projected upon the ring *B*. After an interval of three hours, no sensible change could be detected in the position of these forms; and on the following day they were seen occupying about the same position. This phenomenon could easily be explained by supposing there were some sort of protuberances on the external edge of the ring *C*, casting their shadow under the oblique rays of the sun, which occupied then a proper position to answer to this hypothesis. But some days later, another of these singular forms was observed 180° from the first, on the preceding side, at *s*. This at once overthrew the supposition that they were shadows cast by protuberances existing on the ring *C;* since in this case the shadows would have been projected opposite the sun on the ring *C*, and not on the ring *B*. Since that time, I have rarely observed the planet without seeing some of these singular appear-

2.



ances, either on one side or the other, but generally on both sides. The number of these dark forms is variable. One, two, three, four, and even five, have been seen at the same moment, and on the same side. Though these forms are variable, and appear and disappear, I have never been able to detect in one night any change of position which could be ascribed to the rotation of the rings.

The most plausible explanation of the phenomenon which I can conceive is, that the inner margin of the ring *B*, which forms the outer limit of the principal division, is irregular, jagged, and deeply indented, as shown at *a*, fig. 2, which represents Saturn as it would appear to an observer placed above one of its poles.

As Bond speaks of the principal division of the rings as "not being perfectly elliptical," and as in one instance he has suspected that it "was narrower in some places," it is to be inferred that he had some faint glimpses of the phenomenon which I have observed, and which possibly may be more conspicuous now than twenty years ago.

But the fact that this phenomenon has not been observed earlier does not necessarily prove that it had no existence before; as it is well known, by those who have had experience with the telescope, that one may look for a long while at a celestial object, and miss perceiving what he will readily see when once he is told where to look, and what to look for. Seeing what is new and unsuspected is quite different from seeing what has been observed before.

Though no noticeable changes in the position of the dark angular forms could be observed in the course of two or three hours, it does not follow that the system of rings does not rotate upon an axis, as theory indicates; since the supposed indentations seen on the ansæ would be placed in the most unfavorable positions for showing their motion, if they have any, because it would be accomplished almost in a line with the visual ray, either approaching or receding from the observer.

Next to this division, but much less conspicuous, and to be seen only on very good nights, is a narrow, grayish, and somewhat diffused line, called "the pencil line," shown at *b*, fig. 1. I have never been able to trace this line all around the planet, as it diminishes very rapidly with the foreshortening, and is soon lost. Probably I have never traced it more than 30° or 40° on each side of the major axis of the rings. The pencil line has never appeared to me black and well defined, but rather grayish and diffused. Sometimes I have had the impression that it was irregular in width and in depth of tint.

These two lines are the only ones I have observed, which could, with a certain amount of probability, be said to be a separation of the rings; though they might just as well be depressions, or dark belts, especially the outer one. But the fact that they have been observed on both surfaces north and south, apparently corresponding in position, is in favor of their being real separations of the rings. Though I have repeatedly endeavored to see the planet through the principal division between *d* and *e*, fig. 1, I have never seen the faintest traces of it; and I am not aware that others have been more successful.

If the principal division of the rings is, in fact, what it is said to be,—viz: a space free from matter, and entirely disconnecting the rings *B* and *C*,—I do not see why the planet has never been seen through it. If the planet could be seen through that space, the dark line forming the principal division would be invisible from *d* to *e*, as the bright light of the planet would shine through in its place, and be undistinguishable from that of the rings. It may be objected that the invisibility of the planet through the principal division is due to the thickness of the ring *C*; but in this case, why should the black sky be seen, if the planet is invisible?

Besides the two dark gaps or divisions of which I have just spoken, the rings are subdivided by concentric zones or belts, which reflect light of different hues and intensity. Though only three of these belts are conspicuous, I have found by careful examination that there are six which I can always recognize whenever the illumination is good, and the image steady. These zones are represented on the diagram, fig. 1, at *A*, *B*, *C*, *D*, *E*, *F*. On several occasions, I have had a pretty distinct impression of seeing the whole surface, from *C* to *E* inclusive, grooved, as it were, by numerous narrow concentric belts. These impressions may have been illusory, as they were almost instantaneous; but I have since learned by experience, that, after all, rapid impressions are not so much to be discarded, as, quite often, even more fugitive impressions have proved in the end to be real. A striking instance in my own experience may be worth recording. This summer I made a study of the Horseshoe Nebula in Sagittarius with my 6¼-inch refractor. During the course of my observations, I was much annoyed by what appeared to me as faint ghost-like reticulated shadows projected upon the nebula. I at first thought I had left the reticule of squares ruled on glass in the eye-piece; but having convinced myself that this was not so, and the same appearance again presenting itself, I wiped my eye, but with no better result. As I experienced the same thing on other nights, I paid no more attention to it, thinking the trouble was in my sight. Some time afterwards, while in Washington, I had an opportunity of studying the same nebula with the great twenty-six inch refractor of the Naval Observatory. I was not a little surprised to see that the ghost-like reticule which I wanted so much to rub out of my eye while at home, was caused by dark channels in the nebula itself, which is divided on the preceding side by bright luminous patches, separated by dark intervals.

In order of brightness, the zones or belts composing the system of rings run as follows: *C*, *D*, *B*, *E*, *A*, *F*; *C* being by far the brightest, and *F* by far the darkest. The zones *A* and *B* have a bluish cast, or light slate color; *C* is of a bright luminous white; *D* is slightly grayish; *E* is a little darker; while *F*, which is very dark, is tinged with bluish purple.

A is separated from *B* by the pencil line; *B* from *C* by the principal division; while the others do not show any separation whatever, and are only limited by the contrast of their different colors and shades, and seem to be in immediate contact. However, the different zones do not terminate abruptly where they come in contact, but seem somewhat blended into each other. This is especially the case between *E* and *F*. Though at that point the contrast between the two internal rings is very great, yet it is impossible to see any line of division, so much do they mingle at their point of contact.

On good nights, I have often observed on that part of the rings *A*, *B*, and *C*, seen on the ansæ, an unmistakable mottled or cloudy appearance such as is represented on Plate 1. This appearance was always more characteristic and better seen on the ring *C*, especially near its outer margin, close to the principal division. It would seem, as has been already remarked, that the ring *C* is on a higher level than that of the rest of the rings, and that the cloudy appearances observed there form by their accumulation some kind of protuberances of different heights and breadths. The bright spots resembling satellites, so often observed by Bond in 1848, when the plane of the rings was parallel with that of the ecliptic, were probably caused by the crests of some protuberances similar to those now seen on the ansæ. The form of the shadow thrown by the planet on the rings on Nov. 30, 1874, as shown at *x*, fig. 1, seems also to agree with this hypothesis. The curious and deep indentation of the shadow at *x*, in that part where it it is projected on the outer border of the ring *C*, is perfectly explained on the supposition that this part of the ring is on a higher level. The same shadow, as it appeared projected on the rings *B* and *A*, also clearly indicates that the plane of these zones is on a lower level.

In order to find the shape of the surface of the rings from the observation of the form of the shadow thrown by the planet, I have experimented on a miniature representation of Saturn, illuminated by a lamp occupying the position of the sun, while my eye occupied the position of the earth. By successive trials in altering the shape of the miniature rings, I have soon found what must be the form of the rings in order to give to the shadow the same appearance which had been observed on the planet; and the result agrees with the explanation already given.

From the form of the shadow as it has appeared at different times during the last four years, and from the experiment just mentioned, it seems pretty clear to me, that, from the inner margin of the dusky ring *F*, the thickness gradually increases until it reaches the extreme border of the ring *C*, where it gently decreases, as indicated by the rounding of the shadow at this point; after which it sinks perpendicularly down, until it comes even with the general level of the rings *B* and *A*. The slightly curved appearance of the shadow of the planet during the present year, with its concavity turned towards its globe, also supports this hypothesis.

Though in general, the level of the ring *C* is always higher than that of the rest of the system, it does not seem, however, to be uniform and permanent, but varies, either by the rotation of the rings upon an axis, or by some local changes in the cloud-forms themselves; as in several instances I have observed quite rapid and striking changes taking place during the course of one evening in the indentation of the shadow shown at *x*, fig. 1. Sometimes the indentation appeared to increase, indicating a higher level; and sometimes to decrease, indicating a lower level.

That the thickness of the rings is increasing from the interior margin of the dusky ring to the outer border of the bright ring *C*, seems to be corroborated by the phenomena which I have observed on the dusky ring, and of which I shall speak presently.

On all favorable occasions, I have made careful searches on the dusky ring for the divisions suspected by Bond; but I have never had the faintest glimpses of them. The dusky ring appears to me to be continuous, though it is certainly not of the same thickness throughout. Whatever may be the material of which this ring is composed, it is quite rarefied; and it becomes more and more so as it approaches its inner margin. There, it seems to be composed of discrete particles, each of which reflects the light separately; and by applying high powers to telescopes of large aperture, I have had the impression that the supposed particles were more widely separated by the increase of magnifying power. I do not pretend to have seen distinct and isolated particles in the dusky ring; but by instants my impressions have been so decided, that it seemed as if only a little more favorable conditions were required to enable me to see separate corpuscles of matter. The appearance was somewhat like fine particles of dust floating in a ray of light traversing a dark chamber.

The inner border of the dusky ring, notwithstanding its dark appearance, is sharply defined on the dark sky within the ansæ; but it loses this sharpness of outline in that part which is seen projected upon the disk of the planet. There it appears very diffused and ill-defined.

The inner border of the dusky ring, as seen within the ansæ, forms a part of a perfect ellipse concentric with the other rings; but these graceful curves are remarkably and quite abruptly distorted where they enter upon the disk of the planet at *m* and *p*, fig. 1. At these points, they are seen turning up rapidly, describing a short curve; after which they continue parallel with the curves of the other rings until they meet at *h*. If the ellipse described within the ansæ should cross the planet without any deflection, it would be seen along the dotted line, fig.

1, and pass through *n*; while, on the contrary, it is seen above at *h*.

I was surprised, at first, by this singular phenomenon; but I at last satisfied myself with the following explanation: If we conceive the dusky ring to be made up either of vapors or of numerous small independent solid bodies, and, moreover, if we conceive the thickness of this ring as increasing from its interior margin to its outer limit, we shall have an easy explanation of the observed phenomena. When the matter composing this ring, whether solid or gaseous, is seen projected upon the disk of the planet brilliantly illuminated, it will be lost, and will individually disappear, absorbed by the irradiation of the bright light surrounding it, and it will remain visible only at that part where it forms a stratum thick enough to overpower the effect of irradiation.

The fact that the distortion of the inner margin of the dusky ring is not abrupt at *m* and *p*, where it enters upon the disk, but is gradual, seems to prove that the planet is less luminous on its border than elsewhere, providing the above explanation holds good; and this may be owing to the absorption caused by an atmosphere surrounding the planet.

Bond has represented the limb of the globe of Saturn as seen through the whole width of the dusky ring. In this he agrees with all previous observers. All the drawings of Saturn represent the limb of this planet as plainly and equally visible throughout the dusky ring, becoming invisible only where it enters under the internal margin of the ring *E*. In Bond's memoir, it is positively stated that Mr. Tuttle saw the limb of the planet through the whole width of the dusky ring. If these observations are correct,—as without doubt they are,—the solid particles, vapors or gases, composing this ring, must have undergone some changes of position since Bond's time; as by using the same instrument, and even one of almost double the aperture, I have not been able to confirm these observations.

During the last four years, I have never been able to see the limb of the planet Saturn under the dusky ring, beyond the middle of its width. As it enters under it at *m* and *p*, it remains quite distinct for a short distance; but, as it advances farther in, it diminishes gradually; and it entirely vanishes at about the middle, at *u* and *v*; as if the matter composing the dusky ring was more dense or thicker towards its outer border. This observation has been so carefully made, and so many times repeated, the phenomenon has been so distinctly seen, that there is not the least doubt in my mind as to its reality. Therefore it seems pretty certain that changes have lately taken place in the distribution of the matter composing the dusky ring.

As already shown, the substance composing the dusky ring does not seem to be uniformly distributed; but seems moreover to be agglomerated here and there into denser masses, which I have often recognized upon that part of the dusky ring crossing the planet between *u* and *v*. These supposed agglomerations appeared as dark masses, intercepting the light of the planet. This phenomenon could not be attributed to dark markings on the planet, seen through the dusky ring; since there are no markings so dark and so small on Saturn. Neither could they be produced by the dark bands sometimes surrounding the globe of Saturn, as some traces would have been detected on the edge of the dusky ring, since these bands are usually wider than the transparent part of the dusky ring.

Of the planet itself I have little to say. It has certainly a mottled or cloudy appearance, like Jupiter. The clouds of Saturn are more finely divided, like certain forms of the cirri clouds of our own atmosphere. The cloudy appearance of Saturn, of course, is not so easily seen as that of Jupiter. It always requires a good steady night to see it.

I have never seen the planet striped with a large number of parallel bands, such as some observers have described. Three or four form the extreme limit. Nor have I seen the bands so conspicuously marked, so regular, so distinct in outline, and so dark; the equatorial band being always by far the most conspicuous, while the others were barely perceptible. The equatorial belt has always appeared to me to be slightly tinged with a delicate carmine red, very much like the equatorial belt of Jupiter; only the pink color of the former is much fainter. In no instance could I compare the color of this band to "brick red," as it is commonly described.

Like the equatorial belt of Jupiter, that of Saturn is variable in width, and changes its form as well as its position. It is usually composed of two grayish, irregular bands, forming its limits north and south, between which are seen flocculent pinkish cloud-forms.

The general color of the planet differs from that of the rings, in being of a slight warm brown in which there is a yellowish tinge. The contrast of color with the rings is better seen by the use of very high powers.

To conclude: my observations show,—

I. That the inner margin of the ring *B*, limiting the outer border of the principal division has shown on the ansæ some singular dark angular forms; which may be attributed to an irregular and jagged conformation of the inner border of the ring *B*, either permanent or temporary.

II. That the surface of the rings *A*, *B*, and *C*, has shown a mottled or cloudy appearance on the ansæ during the last four years.

III. That the thickness of the system of rings is increasing from the inner margin of the dusky ring to the outer border of the ring *C*, as proved by the form of the shadow of the planet thrown upon the rings.

IV. That the cloud-forms seen near the outer border of the ring *C* attain different heights, and change their relative position, either by the rotation of the rings upon an axis, or by some local cause; as indicated by the rapid changes in the indentation of the shadow of the planet.

V. That the inner portion of the dusky ring disappears in the light of the planet at that part which is projected upon its disk.

VI. That the planet is less luminous near its limb than in the more central parts, the light diminishing gradually in approaching the border.

VII. That the dusky ring is not transparent throughout, contrary to all the observations made hitherto; and that it grows more dense as it recedes from the planet; so that, at about the middle of its width, the limb of the planet ceases entirely to be seen through it.

VIII. And, finally, that the matter composing the dusky ring is agglomerated here and there into small masses, which almost totally prevent the light of the planet from reaching the eye of the observer.

Cambridge, Dec. 1, 1875.

ART. LIX.—*Curve of Eccentricity of the Earth's orbit;* R. W. McFARLAND.

THE diagram on the following page gives the curve of eccentricity as found in Croll's work, and also in Stockwell's. The former was computed with a view of explaining the climate of past time; the latter, in examining the moon's mean motion. It will be seen that the curves have the same general form, differing in the value of the ordinates, more than in the position of the maximum and minimum points. I have recomputed the values by Le Verrier's old formulæ, and find Croll's figures correct in most cases, and not in error to the amount of ·001 except in one instance. My work extends over 1,100,000 years, and is for points nearer together than Croll's. Stockwell's is doubtless nearer correct than the other, but there is substantial agreement. The two curves for the last 40,000 years, and for the next 30,000, are almost identical; so nearly is this the case, that only one line is drawn in the figure for those periods.

Curve of eccentricity of the Earth's Orbit. The upper line Croll's; the lower, Stockwell's.

The following table gives the difference between perihelion and aphelion, the sun's mean distance being 92,000,000 miles.

Before 1850. years.	Croll. miles.	Stockwell. miles.
175,000	8,400,000	6,750,000
160,000	6,700,000	5,150,000
140,000	6,360,000	4,910,000
120,000	7,920,000	6,590,000
100,000	8,700,000	7,400,000
80,000	7,320,000	6,290,000

At 40,000 the difference disappears.

Ohio Agricultural and Mechanical College, Columbus, April, 1876.

ART. LX.—*On a Bolide of January 31st, that passed over Kentucky;* by J. LAWRENCE SMITH.

ON the afternoon of the 31st of January, at five and one-half o'clock, while crossing one of the streets of Louisville, my attention was suddenly arrested by a magnificent meteor crossing the heavens. I first saw it at 60° above the horizon, and it disappeared to my view behind the houses at an elevation of about 20°. It was pear-shaped, and very bright, and remained in view for two or three seconds. Its apparent size was about one-sixth that of the disk of the moon. It did not separate while under my observation, nor did I hear any noise. On asking, through the medium of the public prints, for the observations of others, I received some fifteen communications, from an area one hundred and twenty miles in diameter.

To a number of the observers an explosion was visible, producing several flashes of light, of different colors. This occurred about ten degrees above the horizon. On exploding, all the fragments disappeared instantly except the largest, which also disappeared before reaching the horizon. Some observers saw sparks flying off from the ball, and a short stream of light behind it. One or two think they heard a whizzing noise and at the time of bursting heard the explosion. All agree in stating that the direction was from northwest to southeast. Nothing has yet been heard of any fragments having been collected. My opinion is that it fell about the range of the Cumberland Mountains in Kentucky, or in the northeast of Tennessee.

This is the third bolide in three consecutive years that it has been my good fortune to witness, in their passage over Louisville, but the fragments of none of them have been obtained. They all were passing from the northward to southward.

ART. LXI.—*Notes on the Sensitiveness of Silver Bromide to the Green Rays as modified by the Presence of other Substances;* by M. CAREY LEA.

SEVERAL investigations made at different times during the past years on the sensitiveness of silver bromide to rays of different refrangibilities, led me to the conclusions: 1st, that its sensitiveness to the different rays could be distinctly modified (increased or diminished) by the presence of various bodies, colored and colorless; 2d, that no relation could be traced between the color of the modifying body and the refrangibilities of the rays to which the sensitiveness was modified. During the past winter, I have carefully re-examined the question, which is an important one both in its theoretical and practical relations, and have found my conclusions in all respects confirmed. And I have during the past winter been occupied with a single portion of the subject, namely, the action of the green rays, as a special study.

In this investigation I have pursued the same general method as before; that is, I have used colored glass whose transmitted rays have been carefully studied with the spectroscope. There is no doubt in my mind that this method of examination is capable of giving results as valuable as those obtained by the use of the spectrum. It may be said in fact that the subject requires for its full elucidation, the use of both methods. To those who may imagine that the results of the exposure to the spectrum are the more reliable, it may be interesting to have specified the weak points of that method.

The *relative strength* of the impression produced by different portions of the solar spectrum on a sensitive surface will always depend upon the intensity of the light employed and the length of exposure. It has already been proved that silver iodide and bromide are sensitive to every part of the spectrum. It follows that any and every part of the spectrum may be photographed upon plain iodide or bromide if only the exposure is sufficiently prolonged. The longer the exposure, the stronger will be the impression produced by the less refrangible rays as compared with the more. Also, it is known that by a system of masking, Prof. John Draper has succeeded in photographing the whole spectrum at once; that is to say, he was able to hold back the action of the more refrangible rays until that of the less was sufficiently strong. The masking was of course done with red or yellow media. Now, when pigments of these colors are extended over sensitive films, what is this but a sort of masking, which retards the action of the more refrangible rays while freely permitting that of the less? Even supposing these pigments to be chemically inert they would check the action of the blue end of the spectrum and render it possible by a longer exposure to obtain both together. Even a longer exposure would not be absolutely essential, a more powerful or a longer development may take its place, being rendered possible by the diminished impression of the blue end.

If, then, it is alleged that by coloring a film of silver bromide with *red* pigments, the sensitiveness to *yellow* rays is increased, we are at once moved to reply that such a result is no proof of a chemical or photochemical action exerted by the red pigment; that precisely the same result might be expected if the red substance were chemically inert, or if it were extended over a glass surface and simply interposed in the path of the rays, between the prism and the film, without even coming into contact with the latter. Many results that have been published are liable to this fatal objection. It has been proposed to modify the form of experiment by applying the color to the front of the plate and exposing on the back, through the glass. But even this does not remove the difficulty. The collodion film containing the silver bromide is exceedingly thin and when moistened, very porous and absorbent. Any soluble color applied, dissolved in water or alcohol, instantly penetrates it through and through and even moistens the glass under it. And all the colors mentioned by other experimenters as having been subjected to this experiment, are soluble. It is on these results that theories have been based, and they are all susceptible of full and complete explanation in the manner just mentioned.

In matters of photographic experiment such as these now under discussion, there are always three distinct factors: The sensitiveness of the matter, the force of the impression (depending upon the intensity of the light and the length of exposure), and the development. This makes the investigation difficult and deceptive. If we take two identical sensitive films, and submit them to the same exposure, we may get quite different results by varying the development, or with an identical film and development, by varying the exposure. The amount of error and deception liable to be introduced in this way is known to none but those made familiar with it by experience, and consequently for accurate results, these sources of error must be eliminated. There is but one way of doing this: a film must be taken, one portion of it must be washed over with the substance whose action is to be studied, and then the two portions, the plain and that which has been treated, must be simultaneously exposed and simultaneously developed. Both portions must receive the same rays, of equal intensity and for an equal time, the development must be made by a bath applied to both portions equally and for an equal time. If these conditions are not maintained, the result will be deceptive. The *intensity* of light must be the same and the *duration* of exposure, because the *relative* effect of the different rays will always be proportionate to them. The same plate exposed to the same image for a double time, or for an equal time to an image of double intensity, will give a final result in which the relative strengths are totally different. The relative strength of different parts of the image is also largely modified by the development. I am therefore justified in affirming that no strictly comparative trial can be made except the two images, on the colored and uncolored films, be received on the same plate, simultaneously and for the same time, and be simultaneously and equally developed.

These conditions have never yet been fulfilled in the case of spectral observations. By the use of colored glass they may be maintained with absolute exactness. And when colored media can be obtained which exclude all but a given band of consecutive rays, the effect of colored substances added to the film of silver bromide, in modifying its sensitiveness to this band of rays, may be accurately fixed. Having previously, with the aid of the spectroscope, determined the exact character of this band, we are enabled to speak very decisively of the action of the rays of which it is composed.

In the present investigation I have limited myself to a single question: Does there exist any red substance which is capable of increasing the sensitiveness of silver bromide to the green rays?

For the purpose of this examination, I used three thicknesses of very dark green glass, the limiting wave-lengths of which had been many times measured with closely corresponding results; the widest variation was two minutes of arc, which when it is considered how gradually the band fades out at its borders, and how very faint the illumination is at its extreme limits, is fully as close an approximation as could be expected. The extreme limits of the band, measured to the limit of visibility, were $\lambda 497$ and $\lambda 581$. But the extremities showed an illumination too feeble to have any effectual result. Cutting off the very faint light, the band was reduced to $\lambda 517$ on one side, and $\lambda 569$ on the other, and the rays between these limits may be taken as those whose results were observed.

In order to register the effects produced, a glass negative of a suitable character was placed under these three glasses, and under the negative, a glass carrying the film to parts of which the coloring matters had been applied. Complete contact was obtained by a pressure frame, and the exposure was made directly to the sun. With a good sunlight, in winter, the exposure was about 45 seconds, corresponding to perhaps 20 or less of summer light. The development was in all cases the alkaline, viz: pyrogallol, and ammonium carbonate controlled with potassium bromide.

As already said, the main object of the research was to arrive at a solution of the question whether any red pigment could be found which would enhance sensibility to the green rays. The following were tried:

Ammonium hæmateate.	Murexide.
Santaline.	Aurine.
Coralline.	Carminic acid.
Rosaniline.	Naphthaline red.
Ferric sulphocyanide.	

In addition to these substances of well-established composition, some pigments were tried whose commercial names are:

Cardinal red.	Rouge ponceau.
Saffranine.	Bordeaux claret.
Cerise.	

With the single exception of coralline, not one of these substances produced the slightest increase of sensitiveness to the green rays.

It was my intention, in the case of finding any red pigments which increased the sensitiveness to green light, to make a careful study of their absorption spectra by means of the spectroscope, but as none such were found except coralline, its spectrum only was examined.

The power of coralline, however, to increase the sensitiveness of AgBr, to green light, cannot be considered as any function of its color, for two most excellent reasons.

1. Coralline exhibits a *still more marked tendency to increase the sensitiveness of AgBr to the red ray than to the green.* This action on the red ray was observed and published by me in March, 1875, and completely disproves of the theory of any special action of coralline upon the green ray. There appears to be a heightening of sensibility to the less refrangible end of the spectrum, rather than to any particular ray.

2. The action on green light is not a function of the color of coralline, because *it is easy to destroy that action without destroying the color.* This singular result is accomplished in the following way.

Coralline appears to be the ammonia salt of a yellow acid. If we place a drop or two of a weak acid, acetic or gallic, in a capsule, and add a few drops of alcoholic solution of coralline, the deep red color of the solution passes to a clear yellow. With the addition of more coralline solution, the color reäppears with its full brilliancy. But the solution, if too large a proportion of coralline to the acid has not been added, is found to have wholly lost its power of exalting sensitiveness to the green rays, although the amount of color applied be made the same in both cases.

Absorption Spectrum of Coralline.—With a moderately strong solution and a narrow slit, the transmitted band is confined to the red rays. As dilution increases, the band widens, passes the D lines, and transmits all the yellow rays. In all cases the band is continuous, and shows neither intervals nor a second maximum.

Coralline, then, forms no exception to the general rule, as above deduced from the examination of fourteen substances of strong red coloration.

Action of Colorless, or nearly Colorless, Substances on the Sensitiveness to Green Rays.

The following substances examined, gave an increase of sensitiveness:

Potassic arsenite.	Morphia acetate.
Argentic arsenite.	Tincture of capsicum.
Salicine.	Ammonium valerate.
Codeia.	Caffeine ?

It appears, therefore, that it is not among the colored, but the colorless substances, that we must look for those capable of enhancing sensitiveness to green light. While not a single red substance could be found that possessed that property, no less than eight colorless substances exhibited it.

The following substances neither increased nor diminished the sensitiveness:

Ammonium hippurate.	Phloridzin.
" mucate.	Parabanic acid.
" malate.	Tincture of aloes.
Plumbic arsenite.	Potassic formate.

It was a little uncertain whether two of these substances, phloridzin and potassic formate, did not give a slight increase of sensitiveness.

The following colorless substances distinctly diminished the sensitiveness to green light:

Brucia.	Gentianine.
Strychnia.	Podophylline.
Narcotine.	Aconitine.
Daturine (tinct. of stramonium.)	Asparagine.
Acid ammonium urate.	Berberine.
Piperine.	

Finding, therefore, no red substance capable as such of increasing the sensitiveness of AgBr to green light, and, on the other hand, many colorless substances which have that effect, I am entirely confirmed in the opinion originally expressed in the pages of this Journal, that there exists no relation between the color of a substance and that of the rays to which it increases the sensitiveness of silver bromide.

Philadelphia, March 13, 1876.

ART. LX.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXIX.—*On the Chemical Composition of Durangite;* by GEORGE J. BRUSH.

IN an article* on this rare mineral, published in 1869, I expressed the hope to make further examination of its chemical composition whenever sufficient material could be obtained for this purpose. Several years elapsed before any new discoveries of the mineral in Durango were made. I am again indebted to Mr. Henry G. Hanks of San Francisco for a new supply of the crystals obtained in recent explorations. These crystals are much smaller than those previously examined, being from one to three millimeters in diameter, and they are of a darker shade of color. The former were loose detached crystals, while these are associated with, and in some cases attached to, rolled fragments of crystallized hematite and cassiterite. The density of the small dark colored crystals is 4·07, while that of the purest of the bright colored crystals before described is 3·937. In all other physical characters there is a perfect correspondence between the two varieties.

The chemical examination of the dark colored small crystals has been undertaken, at my request, by my assistant Mr. George W. Hawes, first to estimate the amount of fluorine in the mineral, which in two determinations he found to be 7·67 and 7·49 per cent, and Mr. Hawes has also placed at my disposal for this article a complete analysis of this variety of the mineral. The fluorine was determined directly by Wöhler's method as modified by Fresenius.† To determine the arsenic acid, and the bases, the mineral was decomposed by sulphuric acid, and the arsenic weighed as sulphide: the alumina, iron and manganese obtained in the analysis were carefully examined to ascertain their purity. The soda and lithia were

* This Journal, II, xlviii, 179.

† Fresenius' Quantitative Analysis (Johnson's edition), p. 406.

weighed as sulphates and then converted into chlorides and separated by ether and alcohol.

The results of the analysis are as follows:

	I.	II.
Arsenic acid,	53·11	....
Alumina,	17·19	....
Ferric oxide,	9·23	....
Manganic oxide,	2·08	....
Soda,	13·06	....
Lithia,	0·65	....
Fluorine,	7·67	7·49
	102·99	

The percentage of fluorine, 7·67, corresponds to 3·23 per cent of oxygen, which, being subtracted, the analysis foots up 99·76, and calculating the percentages of the elements we have the following:

	As	Al	Fe	Mn	Na	Li	O	Fl	
	34·63	9·18	6·50	1·45	9·69	0·31	30·33	7·67	= 99·76
Atomic ratio,	·462	·335	·116	·026	·421	·044	1·894	·404	
	·462	·477			·465		2·298		

Multiplying this ratio by the quantivalence of the elements we find that the ratio of $\dot{\mathrm{R}} : \bar{\mathrm{R}} : \dot{\mathrm{A}}\mathrm{s}$ is 1 : 3 : 5 with 9(O, Fl.)

The formula may therefore be written $(\mathrm{Na, Li})^2$ $(\bar{\mathrm{A}}\mathrm{l}, \bar{\mathrm{F}}\mathrm{e}, \bar{\mathrm{M}}\mathrm{n})$ $\mathrm{As}^2(\mathrm{O}, \mathrm{Fl}_2)^9$

The ratio of oxygen to fluorine in the mineral is as 9·4 : 1.

This is a confirmation of the conclusion drawn by me from the analysis of the lighter colored crystals described in the original paper.*

The mean of my two analyses gave:

Arsenic acid,	54·16
Alumina,	20·35
Ferric oxide,	4·92
Manganic oxide,	1·43
Soda,	11·76
Lithia,	0·75
Fluorine,	undetermined

The variety examined by Mr. Hawes contains less alumina, and considerably more iron, which accounts for its darker color and slightly higher specific gravity. His results prove the mineral to be an arseniate analogous in chemical composition to amblygonite as suggested in my previous paper.

* Loc. cit.

ART. LXIII.—*The Geological Survey of Brazil. First Preliminary Report made to the Counsellor Thomaz José Coelho de Almeida, Minister and Secretary of State for Agriculture, etc.;* by CH. FRED. HARTT, Chief of the Geological Commission of the Empire of Brazil. Rio de Janeiro. 1876.*

ON the first of May, last year [1875] I had the honor of being appointed chief of the Commission charged with the undertaking of a geological survey of the Empire. At the same time Dr. Elias Pacheco Jordao was chosen assistant, and Messrs. Orville A. Derby and Richard Rathbun were appointed assistant geologists. The government having given me authority, I engaged, as the photographer of the Commission, Senhor Marc Ferrez, photographer of the Royal Navy.

While awaiting instructions, and with the permission of his Excellency the Minister of Agriculture, I went with Commander E. P. Wilson to the district of Sao Gonçalo, in the southwestern part of the province of Minas Geraes, for the purpose of inspecting the auriferous tract pertaining to a grant in which the latter was interested. I examined with care the region between Bella Vista, on the Don Pedro II railroad and Sao Gonçalo, including the gold mines of that locality and in the vicinity of the city of Campanha, verifying the occurrence of gold in three distinct deposits.

First, it is found in gneiss of what appears to be the upper part of the Archæan series (Laurentian), the metal being distributed in more or less irregular veins, apparently conforming in direction with the stratification of the rocks. Throughout this section the gneiss is very much decomposed, remaining, however, *in situ,* the decomposition at times extending to a depth of 30 meters or more. While in the solid gangue the proportion of gold is not great enough to pay the cost of working, it has been extracted with profit in many places from the decomposed portion of these same lodes. It is quite probable that this softened rock contains proportionally more gold than the solid gneiss, owing to the accumulation of the contained

* Translated and arranged for publication in this Journal by Theo. B. Comstock, Ass't Professor, in charge of Geological Department, Cornell University, Ithaca, N. Y. This document has not yet been published in Brazil. A copy of the original manuscript in the Portuguese language has been kindly furnished to the translator, with full liberty of action. The report has been shortened by the omission of not a little concerning the details of the Survey, which can have no special interest to the readers of this Journal. The mode of arrangement and construction of sentences differs somewhat from the original, but mainly in cases in which conciseness is gained without the sacrifice of perspicuity. Statements of fact or opinion will be found to correspond as closely as possible with the ideas of the author. Explanatory words within brackets are added by the translator. Words enclosed in parentheses occur thus in the original.—[T. B. C.]

metal of the lower portion in this part of the vein. Action of this nature was first demonstrated by Dr. James E. Mills in the auriferous district of Rio Grande do Sul and has been confirmed by the observations of Viscount de Barbaçena in his mineralogical studies of the auriferous gneiss of Minas Geraes.

Secondly, the gold occurs in the lower part of a deposit of red earth, which covers the whole surface of this region. This is a mechanical mixture of the ingredients of the decomposed gneiss, the local origin of which is not perfectly evident. All parts of the earth are not equally rich, and the gold is probably distributed in some relation to the outcrops, as has been shown by Dr. Mills in the eastern part of the province.

Thirdly, some gold is found in the alluvial deposits of the river-valleys, and in the vicinity of the ancient excavations of Sao Gonçalo and Campanha; the abundant "waste" also contains a considerable quantity of the metal.

Notwithstanding the small amount of gold and the fact that the richer deposits have already been extensively washed, this section appears to be worthy of scientific study, and, probably, by the application of methods similar to those employed in the California *placers*, the region may yet become productive. To this end are required the detailed exploration of this part of the country, the limitation of claims and the introduction of a cheap method of extraction. A survey of the character mentioned will probably be soon made.

Last year, Dr. Mills spent about six months in the study of the auriferous region of Minas Geraes, where he succeeded in discovering and applying the law of the distribution of gold, especially of its occurrence in the series of rocks characterized by the presence of *itacolumite*,—a law extremely important in its bearing upon the future development of the resources of that province. This gentleman has promised to give me a complete report, in which he will not only enunciate and illustrate this law, but will also include all the scientific results of his interesting explorations.

For several years past I have been engaged in the systematic examination of the border of the Brazilian plateau for the purpose of discovering and critically studying the different geological formations, which are there better developed and more fossiliferous than in the interior, being convinced that I could thus most quickly obtain a sure basis for the study of the general geology of the country. Having received instructions for continuing those studies by beginning in the northern provinces and proceeding southward, I judged it best to inaugurate the labors of the Commission by the exploration of the coast of Pernambuco. On the 10th of July [1875] I left Rio for that province, accompanied by Drs. Jordão and Freitas, and Senhor

Ferrez. Opening a provisional laboratory [in the City of Pernambuco], I commenced the examination of the vicinity, soon discovering limestone beds of the Cretaceous formation containing many species of fossils. The explorations were continued northward as far as Catuáma. At Maria Farinha, Cretaceous rocks were found so rich in fossils that several weeks were spent in carefully studying the formation, making a map of the locality, determining accurately the position and sequence of the strata, and collecting enormous quantities of fossils, among which are very many new species. Other collections were obtained from the vicinity of Olinda, from Iguarassú, from the island of Itamaracá, and at Catuáma.

I studied with especial care the limestones which are used for lime-making, analyses of which will be given in my report, and I am also preparing for the same report a chapter on the manufacture of lime and the construction of calcining furnaces.

The examination of the coast was then continued as far as Santo Agostinho, including a reconnoissance along the line of the railroad to Una, with excursions from several points upon both sides. The geology of this portion of the country is, however, extremely monotonous and of little interest.

At the same time I examined carefully the reefs along the coast,—whose geological features are of the highest importance to the country. As I have already indicated to your Excellency, these are of two classes: 1. *Coral Reefs*, composed of calcareous material derived from the débris of certain species of Polyps, Acalephs, and calcareous plants, and 2. *Consolidated Praias* [Beaches], made up of compacted siliceous sand. The reef of Pernambuco is a representative of the latter class This I examined very minutely, and, with the aid of Drs. Jordao and Freitas, made a map of a portion of it. Senhor Ferrez, under my direction, procured a fine series of photographs exhibiting its structure and appearance.

In the study of these reefs a magnificent collection of corals was obtained, including hundreds of large specimens, with many new species. Along with these, and with very little trouble, we gathered large numbers of marine animals, such as fishes, Echinoderms, Crustaceans, etc., etc., among which there is an extraordinary variety of unknown forms.

In September I went with the members of the Commission to make a reconnoissance along the Sao Francisco River to a point a little above the rapids of Paulo Affonso. We ascended the river in a sailing-boat as far as Piranhas, the limit of navigation. At that place, thanks to Senhor Ventura, we found prompt conveyance to the falls, where we remained eight days. Senhor Ferrez took a series of views of the most characteristic points. This was an extremely laborious task, and I cannot but commend him for the firmness with which he struggled against great difficulties and for the good results that he obtained. As the rocks of the *cachoeira* [rapids] are excessively black and the water is white, it was very difficult to photograph both at the same time. On this account I thought it better to make several separate photographic studies of parts of the *cachoeira* which could be mounted as one picture. I hope that I shall be able, by means of the geological, topographical and photographic material collected, to present clear and exact ideas of this wonder of Brazil.

From this place a trip was made to the Serra* de Maria Farinha, distant about five *leguas*.† From the summit of this ridge, at an altitude of two thousand feet or more, may be seen a very large portion of the provinces of Sergipe and Alagoas, with parts of the provinces of Bahia and Pernambuco, and thus a very accurate idea of the topography of the included area may be obtained. All this portion of the country is composed of gneiss and related rocks, forming a plateau less than one thousand feet in height, and nearly level or with very slight undulations. As the region is arid, there has been scarcely any decomposition of the rocks, and the effects of water action are not very evident, owing to the absence of rain during the greater part of the year. There is no vegetable mould, but the rock is covered with a layer of sand a few inches in depth. The vegetation is such as is peculiar to arid plains—gigantic cactuses, the *xique-xique*, *faxeiro*, *mandacarú*, etc., being abundant. The rivers run in shallow channels, and are completely dry during a great part of the year, as are also the small lagoons which abound on the plain just as upon its continuation in the interior of Bahia.

From point to point peaks or short *serras* rise abruptly from the plain, like islands in the sea, seldom attaining an altitude greater than twenty-five hundred feet. In the provinces of Sergipe and Alagoas the serras are commonly composed of gneiss or some other rock of the same series. Between Piranhas and the rapids, however, small ridges of sandstone are encountered, and in the provinces of Bahia and Pernambuco, in the neighborhood of the Sao Francisco River, there are large serras and high *chapadas* [table lands] composed of the same rock. Such are the serras of Tacaratú. Long ago this sandstone covered the whole of this region; but by the action of the sea during the elevation of the Brazilian Plateau, and afterward by the action of the pluvial waters, these beds were denuded over a large portion of the country, leaving only the isolated fragments now forming the table-topped hills which abound in the valley of the

* The term *serra*, as used in Brazil, signifies a rocky ridge, usually more or less serrated in outline, although even table-topped hills are often thus designated.—[T. B. C.]

† The Portuguese *legua*, or league, is equivalent to nearly 1·6 English miles.—[T. B. C.]

Sao Francisco. This river once ran over these beds at a height of one thousand feet, more or less, above its present level, excavating its channel in the sandstone until it had cut through to the gneiss. Above the rapids the river now courses almost over the surface of the plain, its channel being but slightly indented; but upon arriving at the rapids it is suddenly precipitated by a series of magnificent waterfalls through an exceedingly narrow gorge, the depth of which immediately below the rapids is from 80 to 90 meters. This gorge, or cañon, with its walls nearly or quite vertical, extends as far as Piranhas, the river in this portion being much broken by rapids. Below Piranhas the river valley still maintains its cañon-like character, although its width is increased.

Like Niagara, these falls are slowly retroceding, but in a different manner. * * * * *

At the Cachoeira de Paulo Affonso the water passes over gneissic rocks. These do not here suffer decomposition or disintegration by frost, but they are much eroded by transported sand and stones, which produce extraordinary effects, by excavating innumerable pot-holes that soon communicate and allow masses of the rock to fall away. In my final report, the region of the cachoeira will be very carefully described, as well as the geology and physical geography of the lower Sao Francisco.

We returned to Pernambuco at the end of six weeks, and a few days later the bulk of the collections, filling sixty large boxes, were sent to Rio de Janeiro.

Dr. Jordao becoming ill at this juncture, I permitted him to return to the capital [Rio.] As my two assistants, Messrs. Derby and Rathbun, had not yet arrived, and beginning to feel the need of help, I called upon my former assistant, Mr. John Caspar Branner, who, being then in Rio, came at once to my aid, performing very valuable services. He is still employed by the Commission.

After shipping the collections, I returned to Maria Farinha, with Dr. Freitas and Mr. Branner, in order to finish the study of the Cretaceous beds and to examine the coral reefs. In ten days we returned again to Recife [another name for the city of Pernambuco] with a large vessel loaded with fossils, etc. Having been informed that your Excellency desired to exhibit some of the results of the Commission at the National Exposition [held at Rio Janeiro, January, 1876] the new collections were packed in seventy more cases, and I returned with them to the capital in December, accompanied by Senhor Ferrez. * *

While at Pernambuco, I procured a large barque from Mr. Frederico Soares, of Maria Farinha, and sent Dr. Freitas and Mr. Branner to explore the geology of the coast and its reefs as far as Parahyba do Norte,—a trip which was made with good results.

Afterward, Dr. Freitas returned to Iguarassú, where he obtained a fine collection of Cretaceous fossils, including many specimens of shark's teeth and of the teeth of an enormous reptile which is unknown to me. Mr. Branner then continued his study of the coast south of the Cape of Santo Agostinho, examining the reefs, from which he has sent me an interesting collection of corals and other objects, embracing many new species. He also sent photographs of a number of important points. In the beginning of February [1876], Messrs. Freitas and Branner, after sending me forty additional boxes of specimens, went to Aracaju, in the province of Sergipe, to study the Cretaceous deposits. They have already [March 5] made a large collection of fossils, especially of *Ammonites*, *Ceratites*, *Natica*, *Janira*, etc., with numerous forms not before obtained from Brazil. * *

Upon arriving at Rio de Janeiro, a building was hired, the collections were unpacked, and I began to arrange a series of specimens for the Exposition, Dr. Mills assisting me in the work. About the middle of the month [December, 1875] Messrs. Derby and Rathbun arrived, bringing new collections, including a typical series of the Carboniferous and Devonian fossils of the Amazonas and some North American fossils to compare with them. These gentlemen at once aided me in the preparations for the Exposition, and while this lasted they took charge of the other collections. In the meantime Senhor Ferrez mounted for the Exposition a collection of one hundred photographic views and prepared two albums containing a nearly complete series of the photographs taken for the Commission. As it was possible to expose but a small portion of the collections, there were exhibited only a series of the Cretaceous rocks and fossils, with another series illustrating the structure of the reefs, and the collections brought by Messrs. Derby and Rathbun. * *

On the 1st of February, Messrs. Derby, Rathbun and Ferrez were sent to Bahia, to commence the study of the geology of the vicinity and to continue the examination of the reefs. They established themselves at the station of Plataforma, and have to this date [March 5] been engaged in tracing the limits of the areas occupied by the different formations at the northern part of the Bahia de Todos os Santos [Bay of All Saints], in collecting Cretaceous fossils, and in the study of the reefs. Already they have obtained a large quantity of fossils,—teeth, bones and scales of reptiles and fishes, together with Mollusks and Crustaceans. In connection with the study of the recent formations and the reefs, they have also made a splendid collection of marine animals.

Two years ago, Mr. Herbert H. Smith, one of my assistants on the Morgan Expedition of 1870, was engaged in scientific studies in the province of Pará [on the Lower Amazonas], in some measure under my direction. Since that time he has gathered a very extensive collection of insects, numbering, as he assures me, many thousands of species. This collection is now owned by Mr. Smith, and it is of so great value that I feel it my duty to call the attention of your Excellency to the matter, suggesting that it may be advisable to take steps toward securing at least a complete set of the species for the National Museum.

In accordance with the request of your Excellency, I accepted the Directorship of the geological section of the National Museum. After inaugurating the plan and arranging the work for my assistant during my absence, I propose to leave the city to continue my studies in the north.

My prospective journey is as follows: to leave Bahia for the diamond fields, and after examining these to proceed as far as the Rio Sao Francisco, to a point not yet determined. I intend to visit the watershed between the Saö Francisco and Tocantins rivers and to determine its character, afterward descending the Sao Francisco to the vicinity of the rapids of Paulo Affonso. From thence I expect to make a trip to the Serra de Araripe, and then to visit the locality of fossil fishes to examine the Serra de Ybiapaba, striking the sea coast probably at Ceará.

After making this expedition and reviewing the work of the assistants, who will be left along the coast in the provinces of Bahia, Sergipe and Alagoas, I ought to have a somewhat clear idea of the general geology of the northeastern portion of the empire, a region concerning which we have now very little information. This journey will probably occupy about four months. At the close of the exploration I propose to return to Rio de Janeiro with the members of the Commission to arrange and describe the collections and to prepare a report *in extenso* upon all the results obtained by the Commission with descriptions and engravings of a large number of Brazilian fossils.

Addendum by the Translator.

A letter received from Professor Hartt, bearing date, Rio de Janeiro, March 25, 1876, gives some particulars concerning the work accomplished since the writing of the foregoing report. He states that Messrs. Derby and Rathbun have met with most excellent results at Bahia, while Dr. Freitas and Mr. Branner have made yet another very large collection of Cretaceous fossils from the Maröim region. Mr. H. H. Smith had also done characteristic work upon the Amazonas, amassing extended and valuable collections. Professor Hartt himself was about ready to start for the interior.

By the same mail a letter was received from Mr. O. A. Derby, dated Santo Amaro, near Bahia, March 26, 1876. He writes: "We have found some rich deposits of Cretaceous fishes and reptiles in this vicinity and are working them up with care. I do not know as yet what we have, but am certain of several species of Crocodiles and think we have *Iguanodon.* * * * Branner reports quantities of fine Cretaceous fossils from the Province of Sergipe, where he is now." * * * * * * *

"We are now engaged in an exploration of the bay [Bahia de Todos os Santos] and are in a little town four hours by steam from the city [Bahia]. The geology of this vicinity is mainly Cretaceous and Tertiary and quite interesting, though one should have a dozen pairs of eyes to study it. Sections are hard to find and when found more difficult to understand properly. The rock is much decomposed and about a dozen different things present almost the same appearance when in a state of decomposition."

"I have visited a diamond locality. * * * They occur in gravel which is either late Tertiary or modern."

A collection of Brazilian auriferous ores, Professor Hartt writes, will be exhibited at the Centennial Exposition in Philadelphia. The friends of the Professor will be pleased to learn that he has removed with his family to the mountains back of Rio, where all are safe from the ravages of the yellow fever.

Letters for the members of the Commission should be sent enclosed to Major O. C. James, Secretario do Commissao Geologica, Caixa no Correio No. 126, Rio de Janeiro, Brazil, to whom all business connected with the survey may be entrusted. Questions which can be answered by the undersigned will receive due attention. THEO. B. COMSTOCK.

Ithaca, N. Y., May 5, 1876.

ART. LXIV.—*Notice of the Meteoric Stone of Waconda, Mitchel County, Kansas;* by CHARLES UPHAM SHEPARD, Sr., Massachusetts Professor of Natural History in Amherst College.

FOR my first knowledge of the meteorite of Waconda, Kansas, I am indebted to Dr. George W. Chapman of that place. This gentleman wrote me on the subject during my absence from the country the past summer, and it was not until lately that the correspondence was renewed which has led to the information now afforded. From his letters and a few grams of the stone transmitted by post, I am enabled to give the following partial description,—reserving a fuller account until my return to the north, when I expect to receive the large mass from which the fragments sent were detached.

The stone was found two years since, lying above ground, in the grass, upon the slope of a ravine, at the distance of two miles from the village of Waconda. Many pieces were then broken from the mass, leaving about one-half, whose present weight is fifty-eight pounds, half of which is still covered by the original crust. The specimens transmitted show a freshness equal to that of any newly fallen stone, although they came from the immediate surface,—a circumstance obviously pointing to the recentness of the fall. I have known instances where stones exposed only a few weeks to the weather have suffered a decided oxidation, whereas no such change seems to have occurred in this case.

Its cohesion is about that of the average among meteoric stones. In this respect, as well as shade of color, it corresponds very nearly to the Searsport (Me.) stone, of May 21, 1871; but in structure it differs by being less oolitic. Indeed, it is only obscurely so at all,—the individuals that are distinct being rather granular, often with well-marked angles, some of which suggest the species augite; others, those of forsterite (variety boltonite). There is considerable amorphous whitish matter interposed among the grains (in which they may be said to be imbedded), which is doubtless a mixture of minerals, and may consist of chladnite with some one or more of the feldspars. The chamasite (nickelic iron) is present in thickly scattered, very minute, rounded, lustrous grains, requiring for the most part the use of a lens for their discovery; while the troilite (magnetic pyrites) is now and then seen in considerable grains, or ovoidal aggregations of imperfect crystals. The crust is rather thicker than usual, of a dull iron-black color, with a slight tinge of brown, and much crumpled or reticulated. The specific gravity of a fragment weighing 4·35 grams (of which two-fifths were covered by crust) is 3·810; that of a fragment without crust, weighing 3·57 grams, is 3·58.

By mechanical analysis the stone gave 5·66 per cent of chamasite, and 1·34 per cent of troilite. The earthy portion was rather more than one-half decomposed by aqua regia, the soluble portion, after the separation of the silica, giving magnesia, and protoxide of iron (with a little lime) in the usual proportions of chrysolite. The matter not attacked by acids probably belongs to augite, some feldspathic species, and chladnite.

There exists a rumor that a second stone has been found, twelve miles distant from the first; but it lacks confirmation.

It is very likely that the exact date of this meteoric fall will yet be ascertained, inasmuch as it must have taken place very lately, and, without doubt, was attended with a notable report.

Charleston, S. Car., March 16, 1876.

Art. LXV.—*Paleozoic subdivisions on the 40th Parallel;* by Clarence King.

The geological exploration of the fortieth parallel has covered an east and west section of the Cordilleras from the 104th to the 120th meridian, or from the east base of the Rocky Mountains to the eastern boundary of California, along the fortieth and forty-first parallels. The belt of territory under examination is a little over one hundred miles from north to south. Over the greater part of this area bodies of Paleozoic rocks are observed at intervals. A considerable study of these more or less detached exposures, together with the final determination of a large collection of molluscan fossils, has rendered it possible for us to correlate the various members of the series, and construct with considerable precision a complete Paleozoic section. It is the object of this paper to announce the stratigraphical divisions established in the field, and their relation to the Paleozoic subdivisions as established in New York and in the Mississippi basin.

It may be well to remark that along our eastern boundary, in the region of the Rocky Mountains, the entire Paleozoic series—including Coal-measure beds and strata bearing Potsdam fossils—are embraced within a section from 900 to 1200 feet, the whole entirely conformable and resting discordantly upon an Archæan foundation. In passing westward the series rapidly expands from 1,000 to 32,000 feet. Lithologically, divisions which were lost in the narrow Rocky Mountain Paleozoic zone are established with great volume and persistency over wide areas in Utah and middle Nevada. Finally, in the neighborhood of Battle Mountain, at longitude about 117° 25′, an Archæan land-mass rose to the west of the Paleozoic ocean, interrupting farther continuance in that direction.

The region of the Rocky Mountains represented Archæan islands and shallows, around and over which the sparing sediments were deposited, while toward the westward the general contour of the Paleozoic ocean deepened over a broad basin, which probably continued to a great depth quite against the western shore in longitude 117° 30′. To the southward, from the well-known observations of Dr. Newberry and others, it is evident that this Paleozoic sea very perceptibly shallowed. The northern configuration of the bottom and the depth of the Paleozoic series in at present unknown. It is a striking fact, that wherever, within the limits of this exploration, exposures are made,—and they are very frequent,—from the Primordial to the summit of the Coal-measures, there is never the slightest unconformity between the various members of the series. The key to the subdivision of the whole Paleozoic is obtained in the Wahsatch Range, where I have observed a single section of about 30,000 feet of conformable rocks extending from the Permo-carboniferous strata, conformably underlying the red sandstones of the Trias, down to low exposures of the Cambrian.

Ignoring such minor subdivisions as we find to be very variable and local, and describing only such as are observed to be persistent and widespread, I will note in their order from the base of the Cambrian upward the important stratigraphical subdivisions, with their position in the New York scheme.

The lowest member of the series consists of a group which rests non-conformably upon the Archæan, and consists of three prominent members: the lowest is a series of siliceous schists and argillites, best exposed at the mouth of Big Cottonwood Cañon, in the Wahsatch Range, and having a total thickness of from 800 to 1,000 feet; over this is a series of quartzite and quartzofeldsitic strata, having limited beds of slate interspersed through it and containing near the top some dark micaceous zones, the whole reaching in Cottonwood Cañon a thickness of over 12,000 feet: the uppermost member is a narrow zone of variable argillites, calcareous shales, and thin, slightly siliceous limestones having in the Wahsatch an extreme thickness of seventy-five feet. The shaley zone and the accompanying slates carry fossils of well-defined Primordial types, but the quartzite and the deep-lying slates have not yet yielded any organic forms. We have therefore in the Wahsatch a series of 12,000 feet, of which the thin summit member carries Primordial fossils, and the vast underlying series is thus far barren. Comparing the quartzites and argillites with those of the Cambrian section in Wales, the likeness is too great to pass unnoticed, and in view of the enormous developments of these low-lying rocks, as compared with the Silurian lying above the Primordial horizon, I have determined to draw a line at the upper limits of the Primordial period to include the uppermost members of the Potsdam epoch, and to consider the whole underlying conformable series as Cambrian down to the point of their non-conformity with the Archæan. In the extreme east of our work, in the region of the Rocky Mountains, the Cambrian formation is of variable thickness and nowhere reaches an exposure of over 100 feet. In middle Nevada the uppermost zone of the Cambrian, equivalent to the calcareous and argillaceous shales of the Wahsatch, is an immense body of dark limestones at least 3,000 feet in thickness carrying Primordial fossils throughout; the downward continuation of the series being there entirely hidden by the overlying Quaternary desert. The fossils obtained by our survey from the Cambrian series are as follows:

Lingulepis Mæra, n. sp.
" ? minuta, n. sp.
Obolella discoidea, n. sp.
" sp ?
Kutorgina minutissima, n. sp.
Paradoxides ? Nevadensis *Meek.*
Conocephalites (Ptychoparia) Kingi *Meek.*
Conocephalites (Pterocephalus) laticeps, n. sp.
Crepicephalus (Loganellus) Anytus, n. sp.
" " Haguei, n. sp.
" " granulosus, n. sp.
" " maculosus, n. sp.
" " nitidus, n. sp.
Crepicephalus (Loganellus) simulator, n. sp.
" (Loganellus) unisulcatus, n. sp.
" (Bathyurus ?) angulatus, n. sp.
Chariocephalus tumifrons, n. sp.
Ptychaspis pustulosus, n. sp.
Dicellocephalus bilobatus, n. sp.
" flabellifer, n. sp.
" multicinctus, n. sp.
Agnostus communis, n. sp.
neon, n. sp.
" prolongus, n. sp.
" tumidosus, n. sp.

Conformably overlying the summit shales of the Cambrian is a body of limestone, which in the Wahsatch has a maximum development of 2,000 feet, thinning out along the southern part of our belt of country in the region of the Little Cottonwood cañon of the Wahsatch to 1,000 feet. This group, to which we have given the name of the Ute Limestone, has thus far yielded only fossils of the Quebec group; but none have been obtained from its immediate summit or base. In western Nevada the calcareous shales of the Potsdam and the limestone of the Quebec have enormously thickened, and the whole body of Silurian and calcareous upper Primordial represent from 4,000 to 5,000 feet of continuous limestone, in which were found fossils of the Lower Helderberg, Niagara, Quebec and Primordial. In the Wahsatch it is certain that the Ute limestone is, with the exception of possibly a few thin members at the top and at the extreme bottom constituting an insignificant fraction of the whole zone, altogether of Quebec; while in the middle of Nevada, in the region of White Pine, Robert's Peak and the Piñon Mountains, more than half of the heavy body of limestone is Primordial, a very limited amount is Quebec, and a very large portion of the upper part of the series—exact horizons being yet undetermined—is occupied by the Niagara and Lower Helderberg. The Quebec is represented by the following fossils:

Lingulepis Ella, n. sp.
" or Lingula, sp. ?
Obolella, sp ?
Kutorgina, sp. undeterminable.
Orthis Pogonipensis, n. sp.
Leptæna melita, n. sp.
Strophomena nemia, n. sp.
Porambonites obscurus, n. sp.
Rhynchonella, sp. ? fragments only.
Ophileta complanata, var nana *Meek.*
Euomphalus (Raphistoma) rotuliformis *Meek.*
Euomphalus (Raphistoma) trochiscus *Meek.*
Raphistoma acuta, n. sp.
Maclurea minima, n. sp.
Cyrtolites sinuatus, n. sp.
Fusispira compacta, n. sp.
Conocephalites subcoronatus, n. sp.
Crepicephalus (Loganellus) quadrans, n. sp.
Dicellocephalus gothicus, n. sp.
" quadriceps, n. sp.
" Wahsatchensis, n. sp.
Bathyurus Pogonipensis, n. sp.
Ceraurus ? sp.
Ogygia paraboloidalis, n. sp.
" producta, n. sp.

The Niagara furnishes the following:

Cladopora sp. (resembles C. seriata *Hall*).
Orthis (resembling O. hybrida, *Dal.*, but of larger size.
Atrypa reticularis *L.*
Atrypa resembles *A.* nodostriata. *Hall.*
Illaenus, sp. undet.

The following fossils from the Lower Helderberg horizon were found:

Favosites Helderbergia *Hall.*
Diphyphyllum, n. sp.?
Campophyllum (impressions only).
Crinoidal columns.
Small branching Bryozoans, too indistinct for generic determination.
Crania, sp. undet.
Orthis multistriata *Hall.*
" new sp., (resemb. young of O. oblata *Hall*).
Strophodonta punctulifera *Con.?* fragments only.
Spirifera Vanuxemi *Hall.*
Trematospira?
Collospira, new sp. (allied to C. imbricata *Hall*).
Atrypa reticularis *L.*
Rhynchonella, sp. undet.
Pentamerus galeatus *Dal.* (frag. only).
Cryptonella, sp.? (fragments only).

Directly overlying the Ute limestone is a quartzite which is distinctly developed in the Wahsatch Mountains, varying from 1,000 to 1,600 feet thick. It is usually white or pale green, very fine-grained and slightly schistose toward the top, and contains occasional zones of conglomerate consisting of remarkably smooth quartz pebbles in a fine siliceous matrix. From its characteristic development in Ogden cañon we have called it the Ogden quartzite. It is again met with in western Nevada, where it has a thickness of 700 or 800 feet. This body of siliceous material is sometimes altogether wanting, its place being taken by limestone, the Ute and Wahsatch limestones forming one continuous body, siliceous impurities marking the horizon of the Ogden. In the Wahsatch the Ogden quartzite is overlaid by a limestone of very great thickness, to which we have given the name of the Wahsatch limestone, and whose lowermost fossils in the Wahsatch are Upper Helderberg. In western Nevada also the Ogden quartzite is seen between the upper and lower Helderberg horizons. We have included it provisionally within the Devonian age, considering it the probable equivalent of the Schoharie and Cauda-galli grits.

Next above the Ogden quartzite, as just mentioned, lies the Wahsatch limestone, a body reaching 7,000 feet in thickness in the Wahsatch and over 8,000 in middle Nevada. Although varying slightly in the purity of the material and constantly in its physical aspect, it is nevertheless a single limestone series. The lower 1,200 or 1,400 feet are embraced within the Devonian, and characterized by fossils of the Upper Helderberg and Chemung group, and in a single instance a group which would seem to have the facies of both the Upper and Lower Helderberg. If in the lowermost members there is a mingling of Silurian forms, as may possibly hereafter be proven, it will then be necessary to move the Silurian line higher, so as to include the Ogden quartzite; but the present evidence would seem to restrict the Lower Helderberg to the region below the Ogden quartzite.

From the horizon of the Upper Helderberg were obtained the following:

Alveolites multiseptatus *Meek.*
Cladopora prolifica *H. & W.*
Acervularia pentagona *Goldf.*, *Meek.*
Smithia Hennahii *Lowrd.*, *Meek.*
Diphyphyllum fasciculum *Meek.*
Ptychophyllum ? infundibulum *Meek.*
Naticopsis, sp. undet.
Orthoceras Kingii *Meek.*

From the upper members of the Devonian, ranging from the Upper Helderberg to the Chemung inclusive, there were obtained:

Favosites polymorpha *Goldf.*, *Meek.*
Syringopora Macleuri ? *Bill.*
Smithia Hennahii *Lowrd.*, *Meek.*
Cyathophyllum Palmeri *Meek.*
Strophodonta canace *H. & W.*
Productus subaculeatus *Murch.*
Spirifera alba-pinensis, n. sp.
" argentaria *Meek*, (very closely allied to S. zigzag *Hall*).
Spirifera Engelmani *Meek.*
Atrypa reticularis *L.*
Rhynchonella Emmonsi, n. sp.
Pentamerus, sp ?
Cryptonella, sp.?=Rensellaeria ? sp. *Meek.*
Paracyclas peroccidens, n. sp.
Pterinea, sp.?
Pleurotomaria, sp. undet.
Isonema, sp. ?
Bellerophon neleus, n. sp.
Orthoceras, sp. ?

In a single instance, namely, that at White Pine, the Chemung is overlaid by black shales, the probable equivalent of the Genesee group, from which are collected the following:

Leiorhynchus quadricostatus *Hall*= Rhynch. (*Leiorhynchus*) papyraceus *Meek.*
Aviculopecten catactus *Meek.*
Nuculites triangulatus, n. sp.
Linulicardia fragosa = Posidonomya fragosa *Meek.*

From our present knowledge it would seem that the lower 1,200 or 1,400 feet of the Wahsatch limestone is strictly Devonian. The Genesee and the Chemung are followed by beds carrying forms having a close resemblance to the Waverly group, but which are considered by Messrs. Hall and Whitfield as closely allied to the Upper Devonian. They consist of the following species:

Michelinia sp.?
Streptorhynchus equivalvis *Hall.*
" inflatus *H. & W.*
Strophomena rhomboidalis *Whal.*
Chonetes Loganensis, n. sp.
Productus, sp.? (fragments only).
Spirifera centronata *Winch.*
" alba-pinensis, n. sp.
Athyris Claytoni, n. sp.
Athyris planosulcata ? *Phillips.*
Rhynchonella pustulosa? *White.*
Terebratula Utah, n. sp.
Euomphalus (Straparollus) Utahensis, n. sp.
Euomphalus latus *var.* laxus *White.*
" (Straparollus) Ophirensis n. sp.
Proetus peroccidens, n. sp.
" Loganensis, n. sp.

The thickness of the Waverly series is not definitely known, since there is quite a gap of barren limestone between it and the next fossiliferous zone. Not far, however, above the Waverly, especially as shown in the Oquirrh Range and White Pine, occur fossils of the true Sub-carboniferous, such as:

Zaphrentis eccentrica *Meek.*
Fenestella, sp. ?
Polypora, sp. ?
Glauconome, sp. ?
Orthis resupinata *Martin ?*
Productus lævicostatus *White ?*
" semireticulatus *Mart.*
" elegans *N. & P. ?*
Productus Flemingi, *var.* Burlingtonensis *Hall.*
Spirifera striata *Mart.*
" setigera *Hall.*
" Keokuk *Hall.*
" sp.? resembles S. imbrex *Hall.*
Athyris subquadrata *Hall.*

From the evidence in the Oquirrh Range it would seem that the Sub-carboniferous and Waverly together cannot be less than 1,000 feet thick. Through the remainder of the Wahsatch limestone, up to its very summit—a thickness of at least 4,000 feet above the Sub-carboniferous—are found at intervals beds carrying distinct Coal-measure forms. This immense body of limestone therefore, represents 4,000 to 4,500 feet of Coal-measures, 1,000 to 1,200 feet of Sub-carboniferous and Waverly, and 1,000 to 1,400 feet of Devonian, all these figures varying slightly according to the general expansion or contraction of the Wahsatch limestone as a whole in different localities.

Next in the series above the Wahsatch limestones occurs a very remarkable bed of siliceous material, which we have named the Weber quartzite from a typical occurrence in the Weber cañon of the Wahsatch Range. Here, conformably overlying the limestone, is a body of quartzite about 6,000 feet in thickness, having a few red sandstones at the base, and occasional limited fine beds of shale interspersed at three or four different horizons through the body, and varied to a considerable extent by thin sheets of conglomerate and rounded quartz pebbles. It has never in this locality yielded any fossils; but its reference to the middle of the Coal-measures is rendered absolutely certain by the collection of great numbers of different Coal-measure fossils from the Wahsatch limestone below and from an overlying body of limestone to be described later. In the cañon of the Weber, this Weber quartzite has a minimum thickness. In the Oquirrh it has been estimated to be 9,000 or 10,000 feet; and unless we have made some errors in the assignments of horizons in western Nevada, it there reaches an even larger figure. To this member of the series we have referred the great body of sandstones with intercalated shales and conglomerates which form the body of the Uinta Range, and there display a thickness of certainly over 10,000 feet, and according to Major Powell, a much greater thickness. The evidence on which this is referred will be detailed in the forthcoming geological report of this exploration. The Weber quartzite is exceedingly variable in its thickness and mechanical condition. For the most part it represents a true quartzite, but here and there at various localities it is less altered and is merely a series of coarse granular sandstones. At several places in the Wahsatch this body of quartzite is exposed between the two Coal-measure limestones, where there can be no doubt of its true stratigraphical relations.

Conformably overlying this is a body of about 2,000 to 2,500 feet of limestones, chert-beds, calcareous and argillaceous shales, and some beds of calcareous sandstones and arenaceous limestones, the whole constituting a very variable series, and carrying from the bottom to the top distinct Coal-measure forms. In middle and eastern Nevada the shales and arenaceous beds are wanting, and the whole series is a continuous body of limestone.

In the broken and dislocated exposures of the desert country of Nevada there are many outcrops of limestone disconnected from other formations and only referable by their fossils to the Coal-measures. In such cases it is sometimes impossible to determine whether the body should be strictly referred to the Upper Coal-measures or to the Coal-measure part of the Wahsatch limestone. In consequence of this uncertainty, it is impossible at present to say what species are common to both Coal-measure limestones and to group those which are restricted to the two different horizons. From localities where the data is complete, it is evident that both limestones have many species in common. The combined list of the two is too extensive to be published here but will be found in full in the Paleozoic chapters of our reports.

Overlying the true Upper Coal-measures is a variable body of argillaceous and calcareous shales and mud rocks, with limited beds of limestone and sandstone, containing many ripple marks and unquestionably a deposit of very shallow water. It is composed altogether of fine silted material and contains forms which have been referred unhesitatingly by Meek, and Hall and Whitfield to the Permo-Carboniferous. This series is extremely variable and reaches a maximum of 500 feet. While through the Upper Coal-measures there is more or less evidence in the country east of the Wahsatch of a progressive shallowing, there is a decided difference between the Coal-measure proper and the Permo-Carboniferous. The two are apparently quite conformable, yet at the same time a very great change of condition has taken place and it is possible that subsequent study will show a slight discordance of position. If so, the extent of the disturbance of the pre-Permian members has been very slight east of the Wahsatch, while to the west of that range the Permian is wanting.

The following are some of the characteristic fossils:

Aviculopecten curtocardinales, n. sp.
" McCoyi *Meek.*
" sp., *Meek*, (Pal. Up. Mo., plate 2, fig. 10).
Aviculopecten occidaneus *Meek.*
" parvulus, sp. n.
" sp.? resembling Pecten Clevelandicus *Swallow.*

Aviculopecten Weberensis, sp. n.
Eumicrotis Hawni *M. & H.*
" sp. undet.
Myalina permiana *Meek.*
Myacites Weberensis *Meek.*
" aviculoides *Meek.*
" inconspicuus *Meek.*
Schizodus, sp., =S. ovata *Meek.*

From the immense thickness developed in central Utah, the Paleozoic series, there 30,000 feet thick, thins toward the east until as before stated, in the region of the Laramie Hills, it is compressed into 1,200 feet. From the observations of Newberry, and the later accounts of Gilbert, Powell and Marvine, it is clear that it also shallows toward the south, and the observations of the Carboniferous in California would indicate thinning in that direction.

The Archæan body spoken of in western Nevada may or may not have had a continental significance. It would seem, however, from the relations of the Carboniferous in the Blue Mountains of Oregon and Bass' Range in California, that, if a continental mass, it possessed deep westward bays in which the Paleozoic sediments were deposited. It is, however, probable that the Archæan body was only a mountainous region of no very great east and west development, and that the Paleozoic sediments were deposited around it to the north and south.

While as yet no non-conformity has been observed in the whole series from the base of the Cambrian up, there is in middle Nevada an evidence of shallow water and the accumulation of plant-bearing earthy coal beds in the upper part of the Wahsatch limestone. When the detailed stratigraphy to the south of our field comes to be worked out, it is possible that a local uplift will be found near the close of the deposition of the Wahsatch limestone. But otherwise throughout the whole extent we have no indication of a non-conformity. On the contrary, there seems to have been a continuous undisturbed deposition varying between siliceous and calcareous sediments in which the lines of these two types of material have been sharply drawn in a deep oceanic basin over the greater part of the area of Utah and Nevada, while toward the shallow shore in the region of the Rocky Mountains the deposit was more irregularly mixed. Aside from the intimation of a local shallowing at the close of the Wahsatch limestone in western Nevada, the evidences are all of deep-water deposits till near the close of the Upper Coal-measure series, when ripple-marked shales make their appearance, and the Permian depositions thereafter seem all to be of a shoal-water character.

ART. LXVI.—*A Nebula-photometer;* by E. C. PICKERING. (From a letter to the editors, dated Boston, April 5, 1876).

AN examination of the article in the May number of this Journal on the changes on the Nebula M. 17 shows the desirability of accurate photometric measurements of these bodies. I wish therefore to make known the following nebula-photometer in

Page 482, line 5 from top, for Powell read *Howell.*

hopes that some one having the use of a telescope of sufficient size may undertake such measurements. A plate ruled with squares is inserted in the eye-piece of an equatorial telescope with a minute circle of collodion near the center as in the photometer of Dove. It is illuminated in front by the nebula, and behind by a plate of glass inclined at an angle of 45° which reflects the light of a lamp placed on one side of the eye-piece. The light may be varied by two crossed Nicol's prisms, by passing the light through a slit whose width may be varied and measured, or in other ways. To measure the brightness of a nebula the various portions are brought in succession into the center of the field and the light varied until the spot disappears. The exact position of each point is found by observing the various positions of any star in the field with regard to the squares. The real motion of the photometer is thus found from the apparent motion of the star. A contour map may then be constructed showing the brightness of the various portions, and would soon show any marked changes in the light of the various parts. The light of the adjacent sky must be similarly measured and subtracted from all the readings. The brightness may be compared with that of any star by throwing the latter out of focus until its disk attains a given size, and a star photometer is thus obtained. Observations on a comet, with contours showing its brightness on various days, would be both interesting and valuable. The brightness of different portions of the moon could be measured by slightly modifying this photometer. By using a very low power the light of an aurora, of the zodiacal light or of different portions of the sky could be similarly measured. For very faint objects it might be better to insert a diaphragm in the eye-piece having an aperture but little larger than the collodion film, thus giving a dark background. Positions could then be determined by the finder or by moving the entire eye-piece by micrometer screws.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Hydrocellulose.*—In some of the processes in the arts in which cellulose is used, as in paper-making, this substance undergoes a transformation by which it is rendered friable. Aimé Girard has investigated the matter, and finds that this change is owing to the assumption of a molecule of water by the cellulose to form a new body of the composition, $C_{12}H_{22}O_{11}$, to which he gives the name of hydrocellulose. To prepare it, some form of purified cellulose, such as carded cotton, is placed in sulphuric acid of 45° B. in the cold for twelve hours. It is then well washed, pressed and dried. After it is dry, its fibrous character is destroyed by pressure; rubbing between the fingers converting it into a white

powder. Girard explains in this way the brittleness of certain papers bleached with chloride of lime.—*C. R.*, lxxxi, 1105, Dec. 1875.

G. F. B.

2. *On the Decomposition of Stearic Acid by distillation under Pressure.*—Under the direction of Professor Thorpe, JOHNSTON has submitted stearic acid to distillation under pressure, with a view to determine the decomposition products. A copper tube was employed for this purpose, bent twice at right angles. At the second bend from the end, which served as the retort, was an elongation of the tube serving as a receiver. The end of the tube was provided with a stopcock to allow the gases to escape. The stearic acid was heated, allowed to distill over, then run back into the retort, again distilled over, and so on until the acid was completely decomposed. The liquid products were collected and examined. They proved to be hydrocarbons of the marsh gas and olefine series exclusively. The gases resulting were similar, with the addition of water vapor and carbon dioxide.—*J. Chem. Soc.*, II, xiv, 8, Jan., 1876.

G. F. B.

3. *On Liquid Carbon Dioxide in mineral cavities.*—On heating a microscopic slide of quartz containing fluid cavities only to a moderate temperature, HARTLEY was surprised to find that the liquid, previously perfectly visible under the microscope, had disappeared. On cooling, the liquid reappeared accompanied by a sort of flickering movement within the cavity. Experiments on fluid cavities in various minerals made by Brewster in 1823, showed that the liquids all disappeared below 88° F., that their expansion between 50° and 80° F. was 32 times that of water, and their index of refraction 1·2946 in topaz and 1·2106 in amethyst. From these results Simmler, and later, Sorby and Butler concluded that the liquid must be carbon dioxide. The author sought carefully to determine the critical point of the liquid, which he did by immersing the slide in water of known temperature, removing, wiping hastily, and examining. As a result, it appeared that the critical point lay between 30·75° and 31° C., that point for pure carbon dioxide having been fixed by Andrews at 30·92° C. In further corroboration of this view is the fact that when water was also present in the same cavity, the other liquid floated on it; the density of carbon dioxide being 0·83 at 0° and 0·6 at 30°. Moreover, Geissler has shown the presence of this gas in quartz by its spectrum in a vacuum tube in which the quartz was broken. In explanation of the formation of these fluid cavities, the author supposes the silica in hot solution to have come in contact with a limestone under pressure, setting free carbon dioxide which being enclosed in the crystal cavities along with water would on cooling condense to a liquid.—*J. Chem. Soc.*, xxix, 137, February, 1876.

G. F. B.

4. *Decomposition of Alcohol by Aluminum and its Iodide.*—GLADSTONE and TRIBE have continued their researches on the action of aluminum in presence of its haloid salts upon organic

bodies. Alcohol for example, which may be boiled for hours with metallic aluminum without action, evolves hydrogen at once when aluminum iodide is added to the mixture, and in amonnt precisely equal to that theoretically obtainable from the aluminum. The residue of the reaction consisted of aluminum ethylate mixed with some iodo-ethylate. The former body distilled over above 275° C., but suffered partial decomposition. Under diminished pressure, a yellowish white solid collected in the receiver, which fused at 115° and boiled about the boiling point of mercury. Analysis showed it to be aluminum ethylate. The same action was observed with amyl alcohol, and with bromide in place of iodide.—*J. Chem. Soc.*, clviii, 158, Feb., 1876. G. F. B.

5. *New Method for producing Condensed Hydrocarbons.*—WATSON SMITH, having observed that naphthalene passed through a red hot tube, loses hydrogen and produces iso-dinaphthyl, sought to increase the yield of this substance by heating the naphthalene vapor with that of some volatile and easily reducible metallic chloride, the chlorine of which should help to remove the hydrogen. A mixture of naphthalene and antimonous chloride vapors passed through a red hot tube filled with pumice, gave an abundant yield of iso-dinaphthyl. The reaction is—

$$(C_{10}H_8)_6+(SbCl_3)_2=Sb_2+(HCl)_6+\begin{pmatrix}C_{10}H_7\\ \vdots \\ C_{10}H_7\end{pmatrix}_3$$

The author thinks the reaction general.—*Ber. Berl. Chem. Ges.*, ix, 467, April, 1876. G. F. B.

6. *On Manganese Boride and on the Function of Manganese in Iron Metallurgy.*—TROOST and HAUTEFEUILLE have produced a definite manganese boride simply by heating boric acid in a carbon crucible with manganese carbide. Small dark violet crystals were obtained which afforded on analysis the formula MnBo, containing 27 per cent of boron. When free from an excess of manganese, it dissolves in acids, disengaging hydrogen. Water is not decomposed by it at 100°. Mercuric chloride when moist transforms it at once into manganese chloride, boric, and chlorhydric acids. In this reaction, each gram evolves 1697 calories; while its elements taken free evolve 4184; the difference 2487 calories represents the heat set free by the manganese and the boron in combining. Hence the compound is an energetic one. Iron borides too are stable, unlike the iron carbides and silicides. The authors conclude from their researches that the important part which manganese plays in the metallurgy of iron is due (1) to the formation of compounds which evolve in their production a greater amount of heat than that set free by the corresponding compounds of iron; and (2) to the facility with which these compounds form slags, since in oxidizing they evolve more heat than those which contain the same quantity of iron; especially when, as is the case, they exist in presence of a large excess of metal.—*C. R.*, lxxxi, 1263, Dec., 1875. G. F. B.

7. *On the Occurrence of Platinum, Palladium, and Selenium in Silver coins.*—In a letter to Wöhler, Rössler, of the Frankfort parting office, gives some facts of interest relative to the work done in that establishment. During the last year over 400,000 pounds of silver and 5,000 pounds of gold were parted. The silver is purified by crystallization as sulphate and subsequent reduction to the metallic state by iron turnings. The gold is precipitated from its solution in aqua regia by ferrous chloride and melted in gas furnaces; being obtained 1000 fine in this way. Fine silver, especially that obtained from old coins, contains gold averaging about one-thousandth. It also contains both platinum and palladium, the latter sometimes in so large a quantity that its solution in nitric acid is dark yellow. The silver from Commern and Mechernich in the Eifel showed 0·0058 per cent platinum and 0·0053 per cent palladium. In the last year, the office has obtained from the 500,000 pounds of crude silver worked over, twelve pounds of platinum and two pounds of palladium. To obtain these metals, the solution, from which the gold has been precipitated by ferrous chloride, is reduced again by iron turnings, whereby all the other metals present are precipitated as a black sediment. This is freed from copper by the iron chloride, the residue is dissolved in aqua regia, the traces of remaining gold are removed, the platinum is thrown down by ammonium chloride and the palladium by ammonia and hydrochloric acid. In this way selenium was discovered in this deposit. Since then the sediment is fused with soda and charcoal before treating it with aqua regia; several pounds of selenium a year being obtained from this source. The selenium forms an interesting compound with palladium, which is obtained in hard brilliant plates when the regulus obtained as above is dissolved. These plates are not soluble in nitric acid, nor, when platinum is present, in aqua regia; but on ignition they evolve selenium and are then soluble. They are composed of equal atoms of palladium and selenium and resemble the iridosmine scales very closely, being isomorphous with them.—*Liebig's Ann.*, clxxx, 240, Feb., 1876. G. F. B.

8. *On the Conversion of Olefines into the corresponding Alcohols.*—The considerable similarity between the heptylene obtained from pentamethyl-ethol and the terpenes led Boutlerow to attempt the direct synthesis of the alcohol from the olefine by direct union with water, just as the hydrate of terpin is formed. The heptylene was sealed in a tube with water containing a little nitric acid and alcohol. After a few weeks the heptylene had disappeared and had been replaced by the characteristic crystals of pentamethyl-ethol. Liquid isobutylene was then subjected to a similar treatment and with a similar result; trimethyl-carbinol was produced. Sulphuric acid was found to act upon isobutylene in the same way. This olefine, sealed in a tube with double its volume of a mixture of equal parts concentrated sulphuric acid and water, disappeared in the course of two days, and yielded

trimethylcarbinol. Pseudobutylene, isomeric with the former, suffers a similar change but much more slowly.—*Liebig's Ann.*, clxxx, 245, Feb., 1876. G. F. B.

9. *On the Trimethylbenzols of Coal tar Oil and their Separation from each other.*—JACOBSEN has examined very carefully the trimethylbenzols obtainable from coal tar and has shown that only two, mesitylene and pseudocumol are present therein, the third one which theory points out as possible, not existing in the oil at all. Pseudocumol forms only a single sulpho-acid with sulphuric acid, the salt supposed formerly by the author to be isocumolsulphate of barium being a well characterized double salt of mesitylene-sulphate and pseudo-cumol-sulphate. The two trimethylbenzols found separated readily by converting them into the amides of the sulpho-acids, by obtaining first the chlorides by the action of phosphoric chloride and then the amide by the action of ammonia upon this. By crystallization from alcohol, the two substances, mesitylene-sulphamide and pseudocumolsulphamide, are easily and completely separated, the former being far more soluble. On treating the pure amides with hydrochloric acid, the hydrocarbons were regenerated.—*Ber. Berl. Chem. Ges.*, ix, 256, Feb., 1876. G. F. B.

10. *On the Detection of Phloroglucin and Nitrites.*—When very dilute solutions of phloroglucin and of toluidine or aniline nitrate are mixed and a few drops of a solution of potassium nitrite is added, the liquid, at first clear, becomes turbid and brownish-yellow, then orange, and deposits a cinnabar-red precipitate. WESELSKY, who discovered this reaction, proposes it as a test for phloroglucin and nitrites. One c.c. of a solution, containing 0·0005 gram phloroglucin was mixed with one c.c. of a solution of toluidine nitrate saturated at ordinary temperatures, diluted to 50 c.c. with water and treated with one c.c. of a solution containing 0·001 gram potassium nitrite. In 15 minutes the solution became yellow and in three hours the cinnabar precipitate was obtained. Similar reactions are obtained with aqueous solutions of maclurin and catechin, and decoctions of fustic and hops, in place of phloroglucin.—*Ber. Berl. Chem. Ges.*, ix, 216, Feb., 1876. G. F. B.

11. *On the Succinic acid obtained from Active Tartaric acid.*—Pasteur announced some time ago the existence of an optically active succinic acid, and Kekulé showed that the succinic acid derived from active malic acid was itself inactive. BREMER and VAN'T HOFF, deeming the existence of such an active succinic acid extremely improbable, since its molecule contains no asymmetrical carbon atom, have examined the acid which is produced along with dextro-malic acid, by the reduction of dextro-tartaric acid. The acid in question proved to be absolutely inactive and to agree in all respects with ordinary succinic acid. Hence we have: (1) Dextro-tartaric acid, $\underline{C}O_2H.\underline{C}(OH).CH(OH).CO_2H$, containing two asymmetrical carbon atoms and rotating the polarized plane $(\alpha)=6°·6$; (2) Dextro-malic acid, $CO_2H.\underline{C}H(OH).CH_2.$

CO_2H, containing one asymmetrical carbon atom and rotating $(\alpha)=3^{\circ}{\cdot}3$; and (3) Succinic acid $CO_2H \,.\, CH_2 \,.\, CH_2 \,.\, CO_2H$, containing no asymmetrical carbon atom, and being inactive.—*Ber. Berl. Chem. Ges.*, ix, 215, Feb., 1876. G. F. B.

12. *On the Re-conversion of Paroxybenzoic acid into Salicylic acid.*—Neutral potassium salicylate, as Ost has shown, when heated to 210°–220°, splits up into potassium paraoxybenzoate, phenol, and carbon dioxide; while the sodium salt affords no paraoxybenzoate. KUPFERBERG has succeeded in effecting the inverse reaction and in converting the paraoxybenzoate again into salicylate. 39·5 grams sodium paraoxybenzoate, heated to 280° to 295° for six hours in a current of carbon dioxide gas, yielded 7 grams of salicylic acid; or 56 per cent of the acid present. Salicylic acid is converted into paraoxybenzoate by heating the neutral potassium salt to 220°; paraoxybenzoic acid is converted into salicylic by heating the sodium salt to 290°.—*J. pr. Ch.*, II, xiii, 103, Feb., 1876. G. F. B.

13. *On a new Hydro-oxy-benzoic Acid.*—In the hope of obtaining trimesinic acid—an acid containing three carboxyl groups—EMMERLING and OPPENHEIM oxidized oxyuivitinic acid by means of potassium manganate. The product, precipitated by hydrochloric or sulphuric acid, is purified by crystallization from hot water. It appears in thin colorless needles, fusing at 274°·5, and giving a yellow precipitate with ferric chloride. Analysis gave it the formula $C_7H_8O_3$, the barium salt being $(C_7H_7O_3)_2Ba, (H_2O)_2$ and the silver salt $C_7H_7AgO_3$. Fusion with potash separates water and converts it into benzoic acid.

$$C_7H_8O_3=C_7H_6O_2+H_2O.$$

—*Ber. Berl. Chem. Ges.*, ix, 326, March, 1876. G. F. B.

14. *On Vicin, a Constituent of Vicia sativa.*—Some time ago, RITTHAUSEN described a crystallized highly nitrogenous substance obtained from the seeds of the vetch. He now shows that it is a new body, and has the composition $C_8H_{16}N_3O_6$. He assigns to it the name vicin. A kilogram of seeds yielded 3·2 grams of vicin.—*Ber. Berl. Chem. Ges.*, ix, 301, March, 1876. G. F. B.

14. *Acoustic Attractions.*—M. DVORAK has examined the attractions and repulsions of small pendulums hung near sonorous bodies. A square of paper or a piece of cork is hung by a silken thread, and held near a wooden rod vibrating slowly. Varying the positions of the pendulum it is sometimes attracted and sometimes repelled. These motions seem to be due to the air-currents approaching or receding from the rod, and the motions of the cork served to determine approximately the directions. These results were verified by the motions of a flame and the indications of a very sensitive water manometer. The air thrust aside by the vibrating rod escapes laterally, repelling light bodies. This is replaced by air forming counter-currents toward the rod, producing the effect of attraction. When the amplitude of the vibrations is small, the rod acts like the prongs of a tuning fork, and attraction takes place in every direction.

In front of the opening of a tube of Kundt, is placed a second open tube, giving the same sound as the first, and suspended by two threads. Making the first tube resound loudly, the second tube is strongly repelled. The same effect is obtained if the second tube gives one of the harmonies of the first. Placing two tubes facing each other opposite the tube of Kundt and perpendicular to its axis, they tend to approach each other. With a very sensitive manometer it appears that in a column of air in a state of permanent vibration, the air at the nodes has an excess of pressure. This accounts for the heaping up of water in the loops of a tube of Kundt. It is explained by admitting that the amplitude of the vibrations cannot be neglected compared with their length. It follows that there ought to be a continuous motion of the air from a node to a loop. This might be proved by filling the resonant box of a tuning fork with the fumes of chloride of ammonium and seeing if they are thrown out when the fork is set in vibration.

If a bell is filled with water and a drop of oil allowed to fall on it, the circular film becomes quadrangular when the bell is sounded. The water-currents start from the nodes and accumulate at the loops. A disk of glass is attached to the end of a rod vibrating longitudinally. If a glass drop is hung opposite the disk it will be repelled at the center and attracted around the periphery. There are then, as with air, currents outward at the center, and counter currents inward along the edges.—*Journ. de Phys.*, v, 122.

E. C. P.

15. *Correlation of Forces.*—M. Grove describes a convenient apparatus for showing the relations between heat, electricity, and mechanical force. The arrangement is as follows:

Two of Clamond's thermo-electric generators are connected for quantity and put in communication with a gram machine, in such a way as to set this in motion. In the circuit is inserted a sort of electric lamp, in which a platinum wire, placed in the center of a small globe (which protects it from agitation of the air) can be raised to incandescence. The only difficulty of the experiment consists in so regulating the length and diameter of the platinum wire, that it may be raised to a red heat while the thermo-electric current retains sufficient intensity to drive the gram machine.

A circuit entirely metallic is thus obtained, with which the following transformations can be effected;

(1.) The gram machine being excluded from the circuit, a portion of the heat, transformed into electricity by the thermopile, reappears as heat in the wire.

(2.) The wire being excluded from the circuit and the machine introduced, a portion of the heat, transformed into electricity in the pile, reappears as work in the machine.

(3.) The wire and machine being included in the circuit, a part of the heat, transformed in the pile into electricity, produces heat in the wire and work in the motor. If we then stop the machine, the incandescence of the wire is increased. The machine being liberated, on the other hand, starts, and the wire cools as its motion increases. The expenditure of heat needed to develop an increasing quantity of mechanical work is thus rendered sensible to the eye.

(4.) Turning the machine in the direction of the rotation produced by the current, a velocity may be reached such that the incandescence shall completely disappear.

(5.) Turning the machine in the opposite direction, there is considerable resistance, and the wire rapidly grows hotter, and is soon fused.

Thus, in the metallic circuit under consideration, the circulation of a given quantity of energy may appear exteriorly in the form of heat, or, as work. If, by an exterior force, we introduce into the circuit an additional quantity of work, the increase of the quantity of energy put in circulation is rendered visible by the incandescence of the wire; any communication outward from the circuit, of a certain quantity of energy which circulates in it, appears, on the other hand, in diminution, or even disappearance, of the incandescence.—*Journ. de Phys.*, iv, 359; *Nature*, xiii, 434. E. C. P.

15. *Change of Volume of Electric Conductors.*—HERR EXNER has measured the change in length of a conductor through which an electric current is passing, by a method free from the error caused by the expansion due to the heat generated by the current. Two pieces of the same wire of nearly equal lengths were hung one over the other, and so connected with a battery that the current might be passed through either. The lower wire passed through a glass which might be filled with water if desired. The elongation was measured by resting the end of the wire on a lever carrying a mirror whose deflection was read by a telescope and scale. The current being passed successively through the two wires a different deflection was obtained in each case, but these were rendered equal by inserting an additional resistance in circuit with that wire whose elongation was greater. The tube was now filled with water so as to carry off the heat generated in the lower wire as rapidly as possible. It was found that the galvanic expansion was only 1·2 to 2·2 per cent of the heat expansion; and no connection was recognizable with the nature of the metal employed. If it be considered that these values, of course, can only by an upper limit, it will follow from the smallness of the effect obtained that there is no sufficient ground for the hypothesis of a special expansion power of the galvanic current. There can hardly be any doubt that the slight expansion which the water-enclosed wire still shows is simply and alone due to the heat remaining in it.—*Nature*, xiii, 452. E. C. P.

16. *Proper Motion of the Stars.*—P. SECCHI points out a new source of error in the measurement of the proper motion of the stars by the displacement of the lines of the spectrum. The author tabulates a number of the observations made by Huggins, Vogel and himself, and those at Greenwich Observatory, and shows there is considerable contradiction in the results.

The question arises whether there may not be some systematic error in the manner of observing or in the instruments. Comparing the dark line F of Sirius with the hydrogen line Hβ of a Geissler tube, he got always the same result — a shortening of the Sirius line occurs (contrary to Huggins) when the telescope was carried along by the clock-work, and the assistant was at the seeker to keep it on a fixed point corresponding to the slit of the spectroscope; but if the clock-work stopped, or the assistant deranged the position of the star, the light line was displaced and came into coincidence with the star line. Dispensing with clock-work the line was found to be on one side or the other, according as the star was looked at on one side or the other of the axis of the telescope. A change was also noticed on turning the spectroscope 180° on its axis. No attempt is made to explain these phenomena, but they are pointed out as possible sources of illusion. — *Comptes Rendus*, lxxxiii, 761, 812; *Nature*, xiii, 480. E. C. P.

II. Geology and Mineralogy.

1. *Paleozoic fossils from a limestone associated with the Serpentine formation* (*Zone of the Pietre verdi*) *of Chaberton* (*Alpi Cozie*).—Prof. B. Gastaldi, in the Bulletin of the R. Comitato Geol. d'Italia for 1875, p. 346, has published a paper on discoveries made by G. Michelotti, a letter from whom is published in the paper. An accompanying geological section, by Michelotti, gives the stratification of the upper part of Mt. Chaberton: No. 1, the dolomitic limestone; 2, anthracitic sandstone of a red color, with black beds containing lamellar hematite, etc.; 3, quartzyte, with beds of gypsum; 4, talcose "calcischist," of a greenish color. Michelotti states that in an amphitheater under the summit of the mountain, bounded by lofty walls of dolomitic limestone, regularly stratified, presenting splendid examples of folding, he found in fragments of the limestone, among some detritus of serpentine, various limestone blocks that were fossiliferous. The fossils were not as well preserved as could be desired, owing apparently to incipient alteration, but they enabled him to distinguish the following genera: *Syringopora*, near *S. abdita*, (a fine species with the long branches one-sixth to one-third of an inch in diameter, and one-eighth to one-fourth of an inch apart, according to the figure); *Halysites;* a branching coral supposed to be a *Favosites;* a joint of a stem of an *Actinocrinus;* a shell of an Ostracoid, referred to "*Cythereis;*" and a sponge, *Lithospongia.* The species indicate that the limestone is probably of the age of the later Upper Silurian, or the earlier Devonian.

Prof. Gastaldi remarks that the limestone beds are superimposed directly on the serpentine, euphotide, and variolite of the region, in some places with a rather sudden transition from one to the other; but also that at other localities the more recent beds of the *pietre verdi* zone, that is, the "*Calcischist*" containing beds of limestone which afford fossils, seem to pass into the *pietra verdi* by a gradual transition. He supposes, however, that the *pietra verdi* may still be pre-Paleozoic. Yet he offers no evidence to show that it is not merely older Paleozoic. He regards the question one requiring special study, and, in closing, expresses the hope that it may speedily be settled by new discoveries.

The limestone of Mt. Chaberton is stated to be probably equivalent with that of Montaldo Dora, of Lessola near Ivrea, of Rivara, and of Levone; it also occurs at Susa, at the Piccolo Moncenisio, at Seguret, along the French frontier between Frejus and Chaberton, at Balmas, at Rognosa, at Chinivert, and at other places.

The following number of the same Bulletin contains a paper by C. DE STEFANI, sustaining the ground that the serpentine beds of the Apuan Alps *overlie* the Middle and Lower Eocene; that those of several localities in Tuscany are between Cretaceous strata or Eocene; and that those of Elba, Gorgona, Gichio, Jano, and perhaps those of Montecristo and Cape Argentario, are older than the Lower Lias but newer than the Carboniferous.

2. *Eozoon Canadense not organic.*—This is the conclusion of Mr. Otto Hahn after geological and zoological investigations, an account of which is published in the Naturwissenchaftliche Jahreshefte for 1876 of Wurtemburg, and a translation in the Annals and Magazine of Natural History for April. He says, "By my investigation it is established that there is no gigantic foraminifer in serpentine limestone;" "that the most essential characters of the foraminifera, the chambers and the test are not there, but that we have to do with pure rock-formations such as occur every where in serpentine;" that "there is no rock which is so certainly the result of metamorphism, and can be derived from so many minerals, as serpentine;" that he has investigated an immense number of serpentines and always found that they are products of metamorphism." One of the masses of Eozoon which Mr. Hahn examined was from Canada, and bore Dr. Carpenter's label.

3. *Exploration of Lake Titicaca;* A. AGASSIZ and S. W. GARMAN, (Bull. Mus. Comp. Zool., iii, 274).—The Paleozoic fossils collected by these authors about Lake Titicaca are described by Mr. O. A. Derby. Nine are Carboniferous and all but one, *Euomphalus antiquus*, are represented by the same or closely allied species in the United States and Brazil. No Subcarboniferous fossils were met with; but Devonian were found close along side of the Carboniferous at the island of Coati two or three miles from the Lake. The Carboniferous formation extends in a general northwestern direction, and the beds are tilted, often at a high angle. According to Mr. Orrego the formation extends as far north as Callyoma; Prof. J. Orton found, in the same line, Carboniferous fossils at the headwaters of the Amazonas (Pichis R.) and states that Prof. Raimondi, of Lima, has traced the rocks to a height of 14,000 feet, on the Apurimac, between the Pichis and the Cuzco valley.

4. *Note "On the Youngest Huronian Rocks South of Lake Superior;"* by ROLAND IRVING.—In a paper with the above title published in the March number of this Journal, Mr. T. B. Brooks, by an accidental misquotation, makes me responsible for a rock composed of a strange medley of minerals. He says that I mention "these rocks as being coarsely crystalline aggregates 'chiefly of labradorite and orthoclase feldspar, hornblende and some variety of pyroxene.'" I wrote,* "Nearly all of them can, however, be included in two or three general kinds, labradorite, orthoclase felspar, hornblende and some variety of pyroxene seeming to be the chief ingredients." In this I meant to mention the main ingredients of the different kinds, not to say that all of these minerals occur in one rock. I am inclined, with my present knowledge, to follow Mr. Brooks in referring to the Huronian the belt of rocks in Northern Wisconsin, to which the above quotation alludes, as I followed him before in referring them to the Copper Series.† I cannot agree with him in designating the rocks as "granitoid," as, so far as my knowledge goes, they are chiefly rocks of a low degree of silication consisting mainly of labradorite and pyroxene. The general run of the rock in the country occupied by this belt west of Bad River is a dark colored coarsely crystalline mixture of the above minerals, accompanied by hypersthene, magnetite and mica as accessories, as ascertained recently by Mr. Chas. E. Wright from a microscopic examination made by him for me. The "granitoid" rocks which Mr. Brooks has seen occur as patches among these dark colored diabases and allied rocks. In the former only have I noticed orthoclase and hornblende.

University of Wisconsin, April 27th, 1876.

5. *Gigantic fossil bird from the Eocene of New Mexico;* Prof. COPE. (Proc. Acad. N. Sci. Philad., 1876.)—This bird was related both to the Cursores (Struthionidæ and Dinornis) and to Gastornis of the Paris basin. Its size was twice that of the ostrich. Prof. Cope names it *Diatryma gigantea.*

6. *Richmond Infusorial Stratum.*—Mr. Charles Stodder in a paper on the Richmond Infusorial stratum, first described by Professor W. B. Rogers in his Virginia Report of 1840, (this Journal, xlv, 313, 1843), states that Mr. R. B. Tolles examined the stratum as it is exposed in a ravine on the west side of Shockoe Hill, near Richmond, and obtained specimens at the depths, 5, 7½, 10, 11, and 14 feet below the top of the bank, and and also from the north side 40 feet below the top, from a bed which was apparently a continuation of the 14-feet bed, the hill being higher on the north side. The lower layer contains 50 to 80 per cent of organic forms, the uppermost about 20 per cent. The species below this top layer vary but little; while in that they are partly different in species, and the frustules are less broken.

* See paper "On the Age of the Copper-Bearing Rocks of Lake Superior, this Journal, July, 1874.

† Pumpelly and Brooks, this Journal. vol. iii, 1872.

The species of Diatoms peculiar to it are: *Coscinodiscus perforatus*, *Aulacodiscus crux*, *Eupodiscus Rogersii*, and *Mastagonia actinoptychus*. Mr. Stodder gives a list of the species afforded by the several beds.—*Proc. Boston Soc. Nat. Hist.*, xviii, 206, 1875.

7. *Carboniferous Articulates.*—Mr. S. H. Scudder has described (Canadian Naturalist, April, 1876) a fossil larve from a Carboniferous shale near Sydney, Cape Breton, which he refers to a genus near Libellula, and names provisionally *Libellula Carbonaria;* and also, accompanying it, part of a wing of a Cockroach, which he names *Blattina sepulta.*

Mr. Scudder has also published a supplement to his paper on Carboniferous Myriapods (noticed in this Journal, III, vi, 225) in the Memoirs of the Boston Society of Natural History for 1875, giving figures of the specimens there described.

Mr. H. Woodward has described a new fossil Scorpion, from the British Coal-measures, at Sandwell Park and Skegby Collieries, naming it *Eoscorpius Anglicus;* also a gigantic Orthopterous Insect, from Scotland, which he calls *Lithomantis carbonarius.*—*Q. J. Geol. Soc.*, xxxii, 57, 60, 1876.

8. *Note on the Uinta and Wahsatch Ranges: A Correction.*—In vol. III, chapter 7, of the reports of the United States Geological Exploration of the Fortieth Parallel, in mentioning the Wahsatch and Uinta ranges, I stated that the date of their uplift was at the close of the Jurassic period. The chapter was written in 1869, after a brief visit to the two ranges and before the final determinations of horizons and fossils were effected. In 1870–'71–'72 a more careful examination revealed the fact that the more important uplift of these two ranges occurred at the close of the Cretaceous, and not at the close of the Jurassic period. The error had arisen from the extremely close resemblance of certain conglomerates of the lower Eocene with those of the lowest horizon of the Cretaceous. In the Uinta range it was in reality the Eocene conglomerates, and not those of the lower Cretaceous, which we had observed resting unconformably upon the upper shales of the Jurassic. Continued delay in the publication of the geological volumes of our series induces me now to make this correction. In the case of the Uinta there is clearly no non-conformity, from the lowest exposures of the Carboniferous to the highest Cretaceous horizon, while the lowest Eocene rests upon the Cretaceous with distinct non-conformity.

In the Wahsatch the evidence is far more complicated. While the post-Cretaceous disturbance clearly had its profound effect on the range, there are also many facts which confirm our belief that the close of the Jurassic also marked a period of orographical activity, as it did in the Sierra Nevada. CLARENCE KING.

9. *The trilobite, Ceraurus pleurexanthemus, of Trenton Falls, New York.*—Mr. C. D. WALCOTT has given an account of the mode of occurrence of this species and of the characters of the under surface of the dorsal shell, in vol. xi of the Annals of the

Lyceum of Natural History of New York, pp. 155 and 159. He states that the specimens in some layers of the limestone are very numerous: 326 entire specimens were counted in a space measuring 30 feet by 40; and, of these, all but 8 lay with the back downward, the position which exuviæ of the concave form in Ceraurus would take. Their length varies from $\frac{3}{16}$ of an inch to 2 inches. The separated heads are found in immense numbers, and the surface of the rocks is sometimes nearly covered with them.

10. *Glacial phenomena in Jefferson Co., New York.*—According to T. G. B. Lloyd, (Q. J. Geol. Soc., xxxii, 76), the glacial scratches between Philadelphia on the southeast of Theresa and Redwood on the north, (distant ten miles), have a southwest direction, coinciding with the longer axis of most of the lakes between Theresa and Redwood, and with the general course of Indian River. The course is the same as that observed by Emmons near Watertown. Well characterized *roches moutonnées*, with their steepest side facing southwest, occur near Theresa. Mr. Lloyd also describes a pot-hole in the Laurentian granite which is 29 feet deep and 7 to 10 feet in diameter.

11. *Origin of the Porphyry of Marblehead, Mass.*—Mr. T. T. Bouvé, in the Proceedings of the Boston Society of Natural History for January 19, 1876 (xviii, 217), discusses the origin of the red porphyry and red felsyte rock of the vicinity of Marblehead, and proves it to be of metamorphic origin. The felsyte has long been known to contain disseminated grains or fragments of quartz.

He states that in 1862 he observed that the felsyte, near Hingham, was in part pebbly and slaty, and graduated into a conglomerate, and had evidently been derived from the alteration of the conglomerate. His observations since have confirmed this conclusion. [A fine series of specimens was exhibited, illustrating the transitions.]

Mr. Bouvé stated further that he was disposed to include among the rocks having the same origin some, at least, of the underlying syenytes. The succession of rocks as given by others, is—(1) conglomerate; (2) compact feldspar or felsyte, gradually passing into porphyry; (3) porphyry, gradually passing into a rock between porphyry and syenyte; (4) syenyte; and this relation of the beds Mr. Bouvé observes, is itself probable evidence that the causes that led to the changes in the higher portions of the series affected all, though in varying degrees.

The reading of Mr. Bouvé's paper was followed by remarks by Mr. Hyatt, sustaining the view that had been presented respecting the origin of the porphyry. Among his facts he stated that at one point on the ocean side of Marblehead Neck, the variegated conglomerate is altered to compact light-colored felsyte in one direction, and in another to a true deeply colored porphyry, containing distinct crystals of feldspar; and that the included pebble may sometimes be traced until it becomes, without any change of form, a mere spot in the light felsyte matrix, the interior last losing its original characteristics.

12. *Annual Report for* 1874 *of the U. S. Geological and Geographical Survey of the Territories;* F. V. HAYDEN, U. S. Geologist in charge. Conducted under the authority of the Secretary of the Interior. 8vo, with numerous illustrations (88 full paged) and maps. Washington, 1876.—During the year 1874, the explorations of the expedition, under the general charge of Dr. Hayden, were carried on in Colorado, with Denver as headquarters. In the introductory remarks—a letter to the Secretary of the Interior—Dr. Hayden states that the series of older metamorphic rocks, of probable Archæan age, have "alone afforded the precious metals and minerals of Colorado." The volume contains a Report by Dr. HAYDEN on the Lignitic series, the geology of the eastern base of the Front Range, glacial phenomena, and on the Elk Mountains (40 pages); a Report on the geology of the Northwestern portion of the Elk Range by W. H. HOLMES (14 pp.); Report on the features and geology of the valleys of the Eagle, Grand and Gunnison Rivers, and on the detailed features of the formations of the district by Dr. A. C. PEALE (106 pp.); Report on the Geology, etc., of the San Juan division by F. M. ENDLICH (62 pp.); valuable Reports by L. LESQUEREUX on the Tertiary flora of the Lignitic beds, and on the Cretaceous flora of North America, with descriptions of new species (94 pp. and eight plates); Report of W. H. JACKSON, on Ancient Ruins in Southwestern Colorado (13 pp.); besides also a short zoological report by E. INGERSOLL; Geographical and topographical Reports of HENRY GANNETT, S. B. LADD, A. D. WILSON, F. RHODA; also a Report on the superficial deposits of Nebraska, by S. AUGHEY.

No mention is made of Mr. James T. Gardner and his party, who had charge of the Topographical department of the Survey.

Dr. Hayden, in his remarks on the Lignitic series, sustains the views stated in a notice of a paper of his on p. 148 of this volume. He observes that the formations which have been recognized along the eastern front in Colorado above the Archæan are the Silurian, Carboniferous, Triassic (?), Jurassic, Cretaceous and Tertiary. The beds of the Lower Silurian occur along the Black Hills, Big Horn and Wind River Mountains, and near Colorado Springs and Cañon City, but none have been found by the survey for the 200 miles between Fort Laramie and Colorado Springs.

Dr. Peale's excellent report presents a large series of facts with reference to the various geological formations in his district from the Archæan to the Quaternary, and including the igneous rocks. On Eagle River, and between it and Grand Rivers, beds of sandstone and limestone variously colored, and in the upper parts gypsiferous, afforded the plants *Calamites Suckovii*, *C. gigas*, *Stigmaria ficoides;* and Lesquereux concludes, since *C. gigas* has not been found below the Permian, that the beds are probably *Permian*. From the lower part of the series, Dr. Hayden has reported species of *Productus*, *Spirifer*, and from the upper, an *Orbicula*. Dr. Peale calls the beds *Permo-Carboniferous*. Mr. Marvine observed the beds passing down into the Carboniferous.

The question as to the age of the Lignitic beds is well discussed by Dr. Peale; and in the course of this discussion a full tabular list is given of all papers on the subject hitherto published, their places of publication, and the views they present, the several regions of the beds being taken up in succession.

The conclusions are:

That the Lignitic beds of Coalville and Bear River are undoubtedly *Cretaceous:* whether the Evanston should be included is left doubtful.

That the Judith beds are *Cretaceous*, and have their equivalent along the eastern edge of the Mountains (Front Range) below the Lignitic or Fort Union group, and also in Wyoming, and are, either, part of No. 5 (Fox Hills Group) of the Cretaceous, or, a group to be called No. 6.

That the coal (which is partly anthracite) of Rock Creek, Slate Creek, Anthracite Creek and Ohio Creek, is probably all of Cretaceous age. The coal of some beds is excellent, two analyses giving 88·2 and 91·9 p. c. of carbon. The anthracitic character is owing to a trachytic eruption.

That the Fort Union group (at Fort Union, Fort Clark, and under the White River beds, on the North fork of the Platte River, above Fort Laramie and west of Wind R. Mts., also on Grand River, Nebraska and farther north) and the Bitter Creek series (including beds of Black Buttes, Hallville, Medicine Bow, Carbon, Point of Rocks, the Rock Spring series and Washakie Station) are, although both afford Dinosaurian remains, *Lower Eocene.*

That the Lignitic beds east of the mountains in Colorado are the equivalent of the Fort Union group of the Upper Missouri, and are *Eocene;* "also that the lower part of the group, at least at the locality 200 miles east of Greeley, is the equivalent of a part of the Lignitic strata of Wyoming." The Lignitic beds near Golden, Denver, Colorado Springs, Cañon City, Raton Hills, are placed in a table with those of the Fort Union group; but are not afterwards remarked upon.

13. *Age of Angiospermous plants referred to the Cretaceous.*—In my notice of a paper by DeCandolle, on pp. 447–449 of this volume, I remark that the "Cretaceous plants of the United States are the plants of beds which had previously been determined, through the animal fossils, to be Cretaceous." This statement needs, as I find, some modification. It is a fact that the plant-bearing beds of the Lower Cretaceous of New Jersey and the Rocky Mountain region have been referred to the Cretaceous for stratigraphical reasons; and those of New Jersey on this ground, long before the plants were found. But the *chief* evidence in favor of this reference in the Rocky Mountain region was, as I learn from Dr. F. V. Hayden, who has been prominent in collecting the facts, the *existence of Angiospermous leaves*, the animal fossils

having been found in the layers just above those containing the leaves instead of those below. Further we have to admit that the stratigraphical evidence is far from demonstrating in either region that the plant-bearing beds are not Upper Jurassic. De-Candolle's charge is hence not far from right, and should be set aside, if possible, by further observations. Looking over Dr. Hayden's Report for 1874 (noticed above) I find that the Lignitic of the Dakota group (Lowest Cretaceous) is stated by Dr. Peale to have been observed at the mouth of the Gunnison to be *underlaid* by beds which contain a Cretaceous *Scaphite*, and Mr. Peale also mentions that Dr. Newberry speaks of the Cretaceous *Gryphea Pitcheri* being associated with the lower Lignitic beds of the same period. These appear to be pertinent facts. But more are needed. J. D. D.

14. *Swiss Paleontological Society.*—This society was founded in 1874, upon the plan of the Paleontographical Society, for the purpose of publishing the paleontological works of its members, especially those concerning Switzerland, and also of continuing, in a slightly different form, Pictet's *Matériaux pour la Paléontologie Suisse.* The volume for 1875 has just been distributed. It includes the second part of a monograph of *Pholadomya*, with 14 plates, by C. Mœsch; descriptions of Jurassic fossils from Savoy, and remarks upon their vertical distribution, with 7 plates, by E. Favre; further contributions toward distinguishing the Horses of the Quaternary, with 3 plates, by E. Rütimeyer; description of a lower jaw of *Dinotherium Bavaricum*, with 1 plate, by Is. Bachmann; description of Tertiary Echinoderms of Switzerland, with 8 plates, by P. de Loriol. The volume for 1874 contained the first part of Mœsch's monograph of *Pholadomya*, with 26 plates, and a description of fossil plants from Sumatra, with 3 plates, by O. Heer. The society announces, as in preparation, several papers upon fossil Mollusks, Crinoids, Echinoids, Nummulites, Ammonites, Turtles, Stags, etc. The work deserves better support than it has yet received. Only six American names are upon the list of members. The annual subscription is 25 francs, payable in advance to Prof. Eugene Renevier, Lausanne, Canton de Vaud, Switzerland.

15. *Geological Survey of New Jersey.*—Annual Report of the State Geologist, Prof. G. H. Cook, for the year 1875. 42 pp. 8vo. Trenton, N. J., 1875.—This report is mainly economical in its facts. It is accompanied by a large map showing the triangulations of the U. S. Coast Survey, including the primary stations selected in 1875.

16. *Eocene Corals of Italy.*—The memoir of Prof. d'Archiardi, of Pisa, on the Eocene corals of Friuli, has been issued as a separate work. It contains 100 pages of text, in 8vo, (describing a large number of species,) and 16 beautiful lithographic plates. The deposits of Friuli, as described by Prof. Taramelli, are a marly limestone containing echinoderms, and, below this, beds of

different kinds containing the fossil corals. The species are more than 120 in number and many of them are published as new. They show that the Italian seas in Eocene time were within the limits of the coral-reef seas.

17. *Physikalische Krystallographie und Einleitung in die krystallographische Kenntniss der wichtigeren Substanzen;* von P. GROTH. 528 pp. 8vo. Leipzig, 1876.—Professor Groth of Strassbourg has done excellent service for the science of mineralogy by putting in the hands of students a clear and comprehensive work upon Physical Crystallography. He takes up first the general subject of wave-movements and the undulatory theory of light, and by a series of careful descriptions, aided by excellent illustrations, makes the whole subject very intelligible without the use of mathematical formulæ. The fundamental laws of light are then explained, and, as following from them, the various optical properties of crystallized minerals; the whole being treated in a thorough and comprehensive manner. The properties of crystals in their relation to heat, electricity, and magnetism, are also fully described. The second part of the work embraces a discussion of the forms of crystals, based especially upon the general laws of symmetry which characterize the different systems. The illustrations throughout the work are of a high degree of excellence: this is especially true of the colored plates at the end of the volume. A special chapter is devoted to the description of the various instruments employed in optical researches, and a considerable number of detailed examples are given. E. S. D.

III. ZOOLOGY.

1. *Recent Corals from Tilibiche, Peru, nearly* 3000 *feet above the sea-level.*—Professor A. AGASSIZ, in his recent South American tour, found a coral limestone at Tilibiche, 2,900 to 3,000 feet in elevation, about 20 miles in a straight line from the Pacific. The ravine where it occurs is about 450 feet below the general level of the nitrate basin of Peru. Two species of corals, modern in aspect, are described by L. F. Pourtalès, both new species, *Isophyllia duplicata*, and *Convexastræa ? Peruviana*, and besides these a Millepore was observed near *M. alcicornis.* Professor Agassiz concludes that the Pacific, within comparatively recent times, extended through gaps in the Coast Range and made an internal sea, which stood at a height of not less than 2,900 feet, and probably much above this, as the sea must have played an important part in the deposition of the salt and the nitrates of the nitrate beds; and consequently, that there has been an elevation since the formation of the coral reefs, of not less than 2,900 feet. The presence of other extensive saline basins at a height of 7,000 feet seem to make the submergence still greater. The existence of eight species of *Allorchestes* (Amphipod Crustaceans), a salt-water genus, in Lake

Titicaca, is stated to suggest the presence of the sea, at no very distant period, at a height of 12,500 feet.—*Bull. Comp. Zool.*, iii.

The facts have a special interest from the fact that there are now no coral reefs on the South American Coast south of Cape Blanco, near the equator, owing to the cold oceanic currents of the coast. The Coast Range would have been a protection against those currents in the era of the Tilibiche coral reefs.

2. *Caspian Sea.*—The zoology of the Caspian Sea has recently been studied by Mr. Oscar Grimm, with important results. He has found in this great half-salt lake 120 animal species, and states that the whole number existing there must exceed 150 species. His discoveries include 6 new species of fish, (a *Gobius* and five *Benthophili*), 20 species of Mollusks, (*Rissoa dimidiata*, *Hydrobia Caspia*, *H. spica*, *H. stagnalis* with two varieties, *Eulima conus*, *Neritina liturata*, *Lithoglyphus Caspius*, *Bythinia Eichwaldi*, *Planorbis Eichwaldi*, sp. n., *Cardium edule* and var. *rusticum*, *C. Caspium*, *C. crassum*, *C. Trigonoides*, *Adacna vitrea*, *A. edentula*, *A. plicata*, *A. læviuscula*, *Dreissena polymorpha*, *D. Caspia*, *D. rostriformis*, and some other terrestrial and fluviatile Mollusca), a Bryozoan (*Bowerbankia densa* Farre, in which the colonial nervous system may be admirably seen), and about 35 species of Crustacea, among which we find the family Gammaridæ in particular represented by colossal forms and *Idothea entomon* in considerable quantities. Then there are 20 species of worms (*Sabellides octocirrata*), numerous Turbellaria, two sponges (*Reniera flava*, sp. n., or perhaps a variety of *R. alba* O. Schm., and another *Reniera* in the larval state), and, lastly, 13 Protozoa, among which are 6 new species.

The most interesting gatherings were made at 108 fathoms. At one haul the dredge brought up 350 specimens of Gammarids, belonging to 4 or 5 species, 150 specimens of *Idotea entomon*, 50 *Mysids* of colossal dimensions, 6 fishes, a multitude of large specimens of *Hydrobia Caspia*, *Dreissena rostriformis*, and enough more of zoological specimens to make four times this number.

Among the author's conclusions are the following. These species common to different seas, show the affinities of the Caspian Sea to the Aral Lake, the Black Sea, and the Arctic Ocean; but the affinities with the glacial sea seem to be more recent than those with the Black Sea; for in the latter certain species, such as the seals, *Coregonus leucichthys*, and others which are common to the Caspian and glacial seas, are wanting. We may suppose that in the Tertiary epoch there existed in Europe and in the neighboring parts of Asia a vast closed basin of fresh water. By an upheaval of the crust of the earth, due to the action of internal forces which still make themselves felt energetically in the region of the Caspian, this was separated into some smaller basins, which are the existing Black Sea and the Aralo-Caspian basin. The latter in its turn was afterward divided, just as we still see, into two small salt lakes separate from the Caspian. At the same time the water of the glacial sea penetrated into the basin of the Caspian, which still had a slight connexion with the Black Sea, so that only a small number of animals could arrive there from the glacial sea. Hence we find that the primitive forms of the Caspian are fresh-water animals (*e. g. Dreissena polymorpha*), and then that the emigrants from the glacial sea which reached it are marine animals for the most part inhabiting great depths. Hence, also, we recognize that the Caspian in its fauna presents more affinities with the glacial than with the Black Sea, which, again, has become richer in animals under the influence of the Mediterranean.

The Caspian has not only received species from the glacial sea, but has also furnished it with some—as, for example, a species of sturgeon, which seems to be *Acipenser ruthenus*, and lives in the rivers of Siberia. I regard the Sturgeons as belonging to the ancient Aralo-Caspian basin, and as having emigrated, as has been said, into the glacial sea, and perhaps even to America, where, as is well known, the nearest relatives of the *Scaphirhynchi* of the Aral exist. On the other hand we may presume that the place of origin of the Acipenseridæ was the Indian Ocean, and that they were derived from the Selachia, with which, especially when young, they have many points in common (*e. g.* their teeth).—*Zeitschr. wiss. Zool.*, xxv, 322, 1875, condensed from *Ann. Mag. Nat. Hist.*, IV, xvii, 176, Feb. 1876.

IV. Astronomy.

1. *Il passaggio di Venere sul Sole, osservato a Muddapur nel Bengala*; Relazione di P. Tacchini. Palermo, 1875.—The party of observers under P. Tacchini were provided with five telescopes two of which had spectroscopes. In this volume we have the results of their observations. P. Tacchini concludes, that the spectroscope can be employed to advantage in transits; that the solar diameter is smaller in the spectroscope than in an ordinary telescope; that the atmosphere of Venus so appears in the spectroscope as to show that it has a large quantity of vapor like the earth's atmosphere.

2. *Planets recently discovered.*—In the August number of the Journal for 1875, p. 158, was given a table of the recently discovered small planets. We here continue it, repeating some of the planets whose elements were then not well determined. In that table the names Siwa and Polana were interchanged, also the value of φ for Aethra should have been 22° instead of 2°. The elements below so far as (147) are obtained from the Berlin Astronomische Jahrbuch for 1878.

The name of (139), the planet discovered by Prof. Watson while at Peking, is Jue-wa, written in Chinese by two characters, but in western languages to be written without a hyphen, as in the table.

PLANETS RECENTLY DISCOVERED.

No.	Name.	Time of Discovery.	Discoverer.	Mean dist.	Angle of Eccent.	Incl.	Long. of node.	Long. per.
					° ′	° ′	° ′	° ′
136	Austria,	Mar. 18, 1874.	Palisa.	2·2870	4 52	9 33	186 10	316 32
137	Meliboea,	Apr. 21, "	Palisa.	3·1334	12 2	13 46	204 18	310 20
138	Tolosa,	May 19, "	Perrotin.	2·4437	9 6	3 14	54 55	311 23
139	Juewa,	Oct. 10, "	Watson.	2·8140	2 57	8 19	358 37	115 32
140	Siwa,	Oct. 13, "	Palisa.	2·7316	12 29	3 12	107 2	300 33
141	Lumen,	Jan. 13, 1875.	Paul Henry.	2·7095	12 54	11 33	319 3	22 38
142	Polana,	Jan. 28, "	Palisa.	2·3872	6 3	2 18	292 36	227 23
143	Adria,	Feb. 23, "	Palisa.	2·7525	2 49	11 32	333 45	223 20
144	Vibilia,	June 3, "	Peters.	2·6501	13 29	4 52	76 50	8 21
145	Adeona,	June 3, "	Peters.	2·6939	12 17	14 24	77 43	118 8
146	Lucina,	June 8, "	Borelly.	2·7077	3 51	12 42	84 22	237 43
147	Protogeneia	July 10, "	Schulhof.	3·1254	1 42	1 57	252 29	84 43
148	Gallia,	Aug. 7, "	Pros. Henry.	2·7687	10 44	25 18	145 8	36 13
149		Sept. 21, "	Perrotin.					
150		Oct. 18, "	Watson.	2·9807	7 30	2 9	207 33	352 45
151	Abundantia	Nov. 1, "	Palisa.	2·5841	5 44	7 52	40 2	215 57
152	Atala,	Nov. 2, "	Paul Henry.	3·1320	4 43	12 10	41 29	80 0
153	Hilda,	Nov. 2, "	Palisa.	3·9504	9 23	7 51	228 20	284 42
154	Bertha,	Nov. 4, "	Pros. Henry.	3·2285	5 45	20 49	37 36	168 41
155		Nov. 8, "	Palisa.					
156	Xantippe,	Nov. 22, "	Palisa.	3·0375	15 17	7 29	246 11	155 57
157	Dejanira,	Dec. 1. "	Borelly.	2·5857	12 42	11 50	62 25	109 12
158	Koronis,	Jan. 4, 1876.	Knorre.	2·9901	16 59	1 23	282 49	355 10
159	Aemilia,	Jan. 26, "	Paul Henry.					
160	Una,	Feb. 20, "	Peters.	2·7334	3 28	3 51	9 18	56 49
161		Apr. 16, "	Watson.					
162		Apr. 21, "	Pros. Henry.					
163		Apr. 26, "	Perrotin.					

H. A. N.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Atti della Reale Accademia dei Lincei.* Serie 2, vols. 1, 2, 3. 4to, 1873–1876. Roma, 1875, 1876.—The "Accademia dei Lincei" held its first meeting near the beginning of the seventeenth century, and hence it is the oldest of existing scientific academies.* It has had, however, an intermittent existence, owing to the stifling conditions about it.

Founded by Federico Cesi, of the Roman nobility, in 1603, avowedly to promote the progress of truth, its members, and especially their leader, encountered almost immediate persecution; and in 1606 they were forced to suspend their sessions in order to escape greater evils. The Academy was at work again in 1609, and soon after, Galileo (then 45 years old), Baptista Porta and Colonna were among its members. The Academy adopted as its insignia the figure of a lynx—the piercing sight attributed from

* It was preceded in Italy, by the Academia Secretorum Naturæ, which was established by Baptista Porta, in 1560, and suppressed after a few years of existence by Pope Paul III.

ancient time to the lynx making it a fit emblem of the searching spirit of Science, of which Galileo was already an illustrious exponent. The members were "Lincei." They required that a candidate for membership should not be younger than 22 years or older than 30; and he must be devoted to the sciences of experiment or observation. Discussions in jurisprudence, modern history, theology, politics and poetry were excluded, because not properly within their range. The Academy had, in part, in conformity to the spirit of the place and times, the form of a religious order; for St. John the Evangelist was recognized as its protector, and the members were required to recite daily "l'officio della Beata Vergine." This, however, was not enough to save the "Lincei" from suspicion.

In 1630, Federico Cesi, the founder and patron of the Academy, died, to the grief and great loss of its members. The Academy had already come into disfavor, because Federico in 1625 had denied that the sky was solid, and Galileo had asserted that the earth moved; and, soon after Federico's decease, its sessions were for the *second* time, suspended. Some private meetings were occasionally held, under the protection of Cardinal Barberini, nephew of the pope, Urban VIII, at the house of Cassiano del Pozzo, where there was a rich museum. But in 1651, on the death of this Cardinal, the suspension became complete, and the Academy was for the second time dead; and so it remained for nearly a century.

Its revivification—the *second*—did not take place until 1740, when the learned and liberal Pope Benedict XIV, Lambertini, reinstated it and gave it the name of the "Accademia de' nuovi Lincei." But in 1758, on the death of this pope, it became for the *third* time extinct.

In 1795, a society of young men which had held meetings for scientific purposes since 1786 at the "Collegeis Umbro-Fuccioli" and which embraced among its members Feliciano Scarpellini, took the form of an academy, called the "Accademia Umbro-Fuccioli," which was in effect a revival—the *third*—of the old Academy. Owing to the political disturbances of 1801, the Academy was again suppressed. But in July, Pius VII, a patron of science and art, succeeded to the papal chair, and the meetings were resumed, Scarpellini having the full confidence and support of the pope. This was the *fourth* return to active life. It took at first the title of "Accademia Gaetani;" but in 1802 it adopted that of "Accademia de' nuovi Lincei," and in 1804, returned to the original name, dropping the *nuovi*.

The Academy continued in activity for forty years, sustained largely through the influence and labors of Scarpellini. With Scarpellini's death, in 1840, it became once more defunct, having been closed by Gregory XVI, against earnest solicitations. This pope finally gone, Pius IX succeeded; and being eminently liberal in his views when he took the pontificate, the Academy, a year afterward, in 1847, was reëstablished for the *fifth* time, the Pope giving it the modified title "Accademia Pontificia de' nuovi Lincei."*

Finally, Rome having emerged from the dynasty of the popes, and become the Capitol of United Italy, under Victor Emmanuel, an extraordinary session was held in January, 1874, for the revision of the constitution. Among the changes, there was, first, the restoration of the original name "Accademia dei Lincei" with the addition of the prefix "Reale," recognizing the new government under which it existed. Next, the Academy was divided into two sections; one, of Physical, Mathematical and Natural Science, and the other of Moral, Historical and Philological Science; the former to consist of *forty* National Associates, *ten* Foreign Associates, and *sixty* Correspondents; and the latter of the same, excepting that the number of National Associates was made *thirty*. Eight foreign associates of the first section have been since elected; three residing in Great Britain, three in Germany, one in France, and one in the United States.

Of the *Atti* of the Academy, a second series was commenced in 1873, and three volumes have been published; vol. I. for 1873–1874; II. for 1874–1875, and III. for 1875–1876. They contain papers on Mathematical or Mathematico-physical subjects, by Volpicelli, Bataglini, Betti, Dini, Conti, Favero, Ascoli, Valentino, Cremona, Menabrea, Tonelli; in Solar Astronomy and Spectroscopy, by Respighi and Volpicelli; in Electricity by Govi, Ricco, and Volpicelli; in Chemistry by Cannizaro and Paternò; in Zoology, by Cadet, Maggiorani, Boll, Colasanti, De Sanctis, Moriggia, and Todaro, (the paper of the last an elaborate discussion of the anatomy of the Salpa with 10 4to plates; in Paleontology by Gastaldi, Capellini, Meneghini and G. Ponzi; on Volcanoes of Lazio, by G. Ponzi; on some Fungi Uredinei, by C. Bagnis Calor; in Meteorology, at the observatory of Campidoglio, by L. Respighi, and on the climate of Rome, by R. Paveto; Archæological discoveries about Rome by R. Canevari and others. Many of the articles are illustrated by plates.

Signor Cav. Quintino Sella is now President of the Academy.

2. *Statistics of Mines and Mining in the States and Territories west of the Rocky Mountains;* being the Seventh Annual Report of Rossiter W. Raymond, U. S. Commissioner of Mining Statistics. 540 pp. 8vo. Washington, 1875.—This volume brings the subject down to the close of the year 1874. The condition of mining industry in California, Nevada, Idaho, Oregon, Montana, Utah, Colorado and Arizona, is given in about 400 pages, forming Part I, full of interesting details and accurate information gleaned with care from many sources, and states with great condensation often in tabular form, and arranged for easy reference. Part II is devoted to metallurgical processes—its Progress in the west

* The preceding historical facts are taken from an address to the Academy delivered in March, 1848, by Volpicelli, on the occasion of this last reinstatement. Only two preliminary meetings had been held in 1847.

during 1874; the distillation of zinc-silver alloy; silver-lead smelting at the Winnamuck Smelting Works; the Germania Refining and desilverization works, Utah; the construction and operation of a slag hearth; the Rocky Mountain Coal and Coke, condensed from a paper by Mr. Blodgett Britton; separation of gray copper ore from barytes; the Patchen process. Part III. treats of the geology of the Sierra Nevada in its relation to vein mining, by Amos Bowman, and his detailed observations will be read with profit by all who are interested in the genesis of veins however they may differ from some of the theoretical views of the author. This is followed by a history of the relative values of gold and silver, by R. W. Raymond, and another on improvements in mining and milling machinery in the Pacific States, by William P. Blake, with a description of the remarkable Silver Mill of the Consolidated Virginia on the "Big Bonanza" of the Comstock Lode in Nevada, and miscellaneous statistics.

We regret to state that unless some new action is had on the part of the General Government this is the last of the valuable series of Reports on the mineral resources of the United States which will appear.

3. *The National Academy of Sciences* held its regular annual meeting on April 18 to 21, at Washington City. The following are the titles of the papers read:

On the precise determination of the number of vibrations of tuning-forks, and on the effect of temperature and of amplitude of vibration on the vibratory periods of forks; by A. M. Mayer.

On a method of exploring the acoustic condition of the atmosphere, leading to the invention of an instrument for determining the direction of a source of sound; by A. M. Mayer.

On the sensations produced by concurrent sounds, and by sounds quickly succeeding one another; by A. M. Mayer.

On the theory of simultaneous ignition of many mines; by H. L. Abbot.

On Maxima and Minima in Algebra, by the late Gen'l D. P. Woodbury, with a biographical notice of the author; by J. G. Barnard.

The Character of the Eocene Fauna of New Mexico; by E. D. Cope.

Contributions to Meteorology; by E. Loomis.

A conjectural restoration of a pueblo of the Mound Builders; by L. H. Morgan.

The Geological evidence on the question of the cause of the cold of the Ice Period; by J. S. Newberry.

On the history of the Problem of the Tangencies; by B. Alvord.

On the theory of Magic Squares; by F. A. P. Barnard.

On the progress of a Magnetic Survey of the United States, at the charge of the Bache Fund of the Academy; by J. E. Hilgard.

Results of Experiments on Contact-resistance; by Wm. A. Norton.

On the imperfections of the present system of Chemical Nomenclature; by R. E. Rogers.

The Age of Mountains as determined by degradation; by J. W. Powell.

Biographical Memoir of Joseph Winlock, late Director of the Harvard Observatory, Member of the Academy; by Joseph Lovering.

On the Geological and Physical structure of the Black Hills; by Henry Newton.

4. *Memoir of Caroline Herschel*, by Mrs. John Herschel, with portraits. New York: D. Appleton & Co. 1876. 12mo, pp. 355.—The scientific life of Caroline Herschel as an observer and investigator in astronomy closed in 1822 with the death of her illustrious brother, William Herschel, with whom she was so

closely identified as to make the two lives almost one. She was already 72 years of age when she returned to Hanover, as she then believed soon to die. Her memoir is intensely individual. During her twenty-six years of exile in Hanover (1822 to 1848) from all those most loved in England, she maintained an active correspondence with her nephew, Sir John F. W. Herschel and his wife, and exchanged frequent letters with most of the eminent astronomers of the time in Europe. These letters, as well as parts of her diary and personal recollections, are excellent reading. We find in them much to compensate us for the want of productiveness, which Miss Herschel constantly laments as the great mistake of her life in not continuing in England; for if she had remained there this correspondence, so full of vitality and varied interest, would not have existed. While she was "minding the heavens" with her beloved "Sweeper," as of old, she would have found no time to record her recollections of her fifty years activity in her brother's service, to honor whom she was so willing to obscure her own real merits. But she will not be forgotten while scientific literature endures. We are led to hope that from the materials accumulated by her assiduity and other sources in the possession of the family of Sir John Herschel, we may yet have a satisfactory biography of Sir William Herschel—a work still wanting. B. S.

5. *The depth of the Pacific, and the nature of its bed.*—We take the following facts from a recent report by Prof. Wyville Thomson.

Between Hawaii and Tahiti, the depth, with one exception of 1,525 fathoms, ranged between 2,000 and 3,000 fathoms and has a mean of 2,600; the bottom, except near the islands, mainly red clay, with much oxide of manganese in small concretions, and many foraminifers; and over two patches, there were siliceous shells of Radolarians, making a "Radolarian ooze." The fauna of the bottom was very meager.

Between Tahiti and Valparaiso (reached on the 19th of October), 5,000 miles in distance, the course taken was southward to latitude 40° S., and then on that parallel to Valparaiso. The mean depth was 2,139 fathoms; the bottom was of red clay with nodules of manganese, with Globigerina ooze in the shallower parts. Life was very sparse, except between Juan Vernandez and Valparaiso, where, although the depth was 2,225 fathoms, it was abundant; the bottom was a bluish mud with very little manganese.

Notices of the following works are deferred to another number.

Reliquiæ Aquitanicæ, Part xvii, the closing part of the work. Williams & Norgate, London.

Geological Survey of Pennsylvania. Historical sketch of Geological explorations and other States, by J. P. Lesley. pp. 200 and xxvi, 8vo.

Mines and Mineral Statistics of N. S. Wales, 1875. 246 pp. with maps and sections.

Revue de Géologie pour les Années, 1873 et 1874, par M. Delesse et M. de Lapparent. Paris, 1876.

Geology for Students and General Readers. Part I, Physical Geography, by A. H. Green, M.A., F.G.S. 552 pp. 8vo. London. (Daldy, Isbister & Co.)

Second Annual Report of the Geological and Agricultural Survey of Texas, by S. B. Buckley, A.M., Ph.D., State Geologist. 96 pp. 8vo. Houston, Texas, 1876.

APPENDIX.

ART. LXVII.—*Notice of a new Sub-order of Pterosauria;* by Professor O. C. MARSH.

THE first Pterodactyle discovered in this country was found by the writer, in 1870, in the Upper Cretaceous of Kansas; and during the next year two other species were obtained in the same region.* These three species were referred provisionally by the writer to the genus *Pterodactylus* of Cuvier, with which the remains then described essentially agreed. An examination of the large series of specimens of this order now in the Yale Museum, shows, however, that some of these fossils possess characters widely different from all forms known in the old world, and indicate a new and highly interesting type. The distinctive feature in this group is the *absence of teeth*, and hence the order may be called *Pteranodontia*, and the family *Pteranodontidæ*, from the typical genus described below.

Pteranodon, gen. nov.

This genus is readily distinguished from any Pterodactyles hitherto described by the cranial characters, which are well shown in a nearly perfect skull, and portions of others, in the Yale Museum. The cranium preserved is very large, and the facial portion greatly elongated. There is a high sagittal crest, which projects backward some distance beyond the occipital condyle. The latter is directed backward, and somewhat downward. The quadrate is long, and inclined well forward. The orbits are large, as are also the antorbital and nasal apertures. The maxillary bones are closely coossified with the premaxillary, and the whole forms a long, slender beak, which, in the specimens examined, tapers gradually to the pointed apex. There are no teeth, or sockets for teeth, in any part of the upper jaws, and the premaxillary shows some indications of having been encased in a horny covering. The lower jaws, also, are long and pointed in front, and entirely edentulous. The rami are closely united by a symphysis which extends from the apex to beyond the posterior extremity of the dentary bone, thus resembling the mandible of *Rhynchops* and some other birds. In several other respects, the jaws in this genus are more like those of birds than of any known reptiles.

The vertebræ in the present genus are similar to those in European Pterosaurians, and the altas and axis are united.

* This Journal, vol. i, p. 472, 1871; vol. iii, pp. 241 and 374, 1872.

There are four phalanges in the wing finger, and the metacarpal that supports it is longer than one half the ante-brachium. In one specimen, which probably belongs to this genus, there are four slender bones, apparently all metacarpals, which are pointed above, and do not reach the carpus. Another specimen, which is described below, and probably belongs to this genus, has five vertebræ in the sacrum.

The nearly complete skull mentioned above may be regarded as the type of the genus *Pteranodon.* Its principal measurements are as follows:

Length from occipital crest to end of premaxillary about 30 inches, or	760· mm.
Transverse diameter of occipital condyle,	8·4
Distance from occipital condyle to distal end of quadrate,	105·
Length of lower jaw about 23 inches, or	584·
Greatest depth,	62·2
Depth at articulation for quadrate,	23·2

The species represented by this specimen is well marked, and may be called *Pteranodon longiceps.* It is somewhat larger than *P. occidentalis* Marsh, which apparently has more slender jaws. The Yale collection contains portions of a skull indicating a much larger species, which is probably *P. ingens* Marsh. If this skull was of the same proportions as that just described, its length would be no less than four feet!

One of the smallest American species yet found is represented in the Yale Museum by several bones of the wing, a number of vertebræ, and the nearly complete pelvis. The wing-bones preserved are elongated, and very slender. The pelvis is unusually small, and there are five vetebræ in the sacrum. The last of the series indicates that the tail was short. The following are the principal dimensions of this specimen:

Length of ulna,	187· mm.
Length of metacarpal of wing finger,	300·
Antero-posterior diameter of outer condyle at distal end,	15·
Transverse diameter of shaft, above condyles,	13.
Length of first phalanx of wing finger,	347.
Extent of five vertebræ of sacrum,	57.

This species, which may be called *Pteranodon gracilis*, was about two-thirds the size of *P. velox* Marsh. It probably measured about ten feet between the tips of the expanded wings.

All the specimens here mentioned are from the Upper Cretaceous of Western Kansas. It is an interesting fact that the localities and geological horizon of these specialized, toothless, Pterodactyles are precisely the same as those of the *Odontornithes*, or birds with teeth, and the two doubtless lived together in the same region.

Yale College, New Haven, May 15, 1876.

Pteranodon comptus, sp. nov.

The smallest Pterodactyle known from American strata is indicated by portions of three skeletons in the Yale Museum. Among these remains are two distal ends of the characteristic metacarpal of the wing finger, other portions of the wing bones, and two sacral vertebræ. The large metacarpal is very slender, and elongated, and its outer distal condyle has its superior margin elevated above the shaft, and terminated proximally in a point. The ulna is comparatively large, and the proximal carpal has an oval air cavity on its radial side. The sacral vertebræ have their centra short, and medially constricted.

The principal measurements of the remains of this species are as follows:

Greatest diameter of ulna at distal end,	15· mm·
Transverse diameter of proximal carpal,	17·
Antero-posterior diameter of outer distal condyle of wing metacarpal,	12·8
Longitudinal extent of condyle,	11·6
Transverse diameter of shaft above condyle,	11·5
Length of medial sacral vertebra,	9·0
Transverse diameter of centrum,	8·4

The above specimens are all from the Upper Cretaceous of Western Kansas.

Yale College, May 22, 1876.

Art. LXVIII.—*Notice of new Odontornithes*; by Professor O. C. Marsh.

Among the remains of Cretaceous Birds in the Yale Museum, are specimens which indicate two undescribed species of special interest. Both were of gigantic size, and clearly belong to the *Hesperornithidæ*, although quite distinct from *Hesperornis regalis* Marsh, the type of the group.* The more important characters of each, so far as now known, are given in the following description:

Lestornis crassipes, gen. et sp. nov.

The present genus is very nearly related to *Hesperornis*, but the sternum has five pits on each side for the attachment of ribs, and essentially no posterior emarginations. The tarso-metatarsal bones present a distinctive feature which may prove of generic value. On the inner side of the upper half of each, there is a large tuberosity, somewhat similar to the ossified

* This Journal, vol. iii, p. 360, 1872, and vol. x, p. 403, 1875.

support of a rudimentary spur. The teeth and the vertebræ in this genus resemble closely those of *Hesperornis.*

The present species is based upon the greater part of a skeleton, including portions of the skull. These remains indicate a huge swimming bird, fully six feet in length, from the apex of the bill to the end of the toes, or somewhat larger than *Hesperornis regalis.* The femur and the tibia resemble those in some modern diving birds, but the toes are shorter and stouter. The large rugosity on the metatarsal bone is a striking character, which may possibly be an indication of sex. The sutures uniting the three metatarsals are well marked.

The principal dimensions of this specimen are as follows:

Length of sternum,	197· mm.
Width in front,	162·
Extent of five articulations for ribs,	54·
Length of femur,	103·
Transverse diameter of distal end,	51·
Transverse diameter of tibia at distal end,	33·
Length of tarso-metatarsal,	135·
Transverse diameter of proximal end,	35·5
Transverse diameter through protuberance,	30·5
Length of first phalanx of fourth toe,	39·5

The known remains of this species are from the Upper Cretaceous deposits of Western Kansas.

Hesperornis gracilis, sp. nov.

A second species of *Hesperornis*, somewhat smaller than *H. regalis*, and of more slender proportions, is indicated by a few remains in the Yale collection. The most characteristic of these specimens is a left tarso-metatarsal, which in general form is very similar to the corresponding bone in *H. regalis*, but is much less robust. The following are its principal measurements:—

Length (approximate) of tarso-metatarsal,	130· mm.
Transverse diameter of proximal end,	29·
Antero-posterior diameter,	17·
Distance from proximal end to articulation for hallux,	78·
Transverse diameter of shaft at articulation for hallux,	15·1
Antero-posterior diameter,	18·
Antero-posterior diameter of distal condyle of third metatarsal,	14·
Transverse diameter,	7·

This species, also, is from the Upper Cretaceous of Western Kansas.

Yale College, New Haven, May 18, 1876.

Ichthyornis victor, sp. nov.

In addition to the remains described above, the Yale Museum contains a number of specimens which indicate a new species, apparently, of the genus *Ichthyornis*. The best preserved of these fossils consist of characteristic portions of the humerus, coracoid and scapula, all parts of one skeleton. They pertain to a bird somewhat larger than a pigeon, and about one-third larger than *Ichthyornis dispar* Marsh, the type of the order *Odontotormæ*.* The humerus has the radial crest greatly expanded, indicating strong power of flight. The radial condyle of the distal end is larger than that for the ulna. The coracoid is stout, and at its scapular articulation has a nearly round, deep pit, into which was inserted a corresponding tubercle of the scapula, forming a strong support for the humerus. Above this tubercle, the scapula has an anterior angle, sharp and pointed.

The remains preserved of this species have the following dimensions:—

Greatest diameter of distal end of humerus,	12·5 mm.
Longitudinal extent of radial condyle,	6·
Diameter of shaft just above condyle,	10·
Greatest diameter of coracoid at articulation with scapula,	6·5
Diameter of articular pit,	3·
Extent of coracoid above top of pit,	8·
Vertical diameter of scapula at articulation,	8·
Transverse diameter,	3·

The known remains of this species are from the same region and geological horizon as those above described.

Yale College, May 22, 1876.

* American Naturalist, Vol. IX, p. 630, Dec., 1875. This name was substituted for *Ichthyornithes*, which proved to be preoccupied.

INDEX TO VOLUME XI.*

* The Index contains the general heads BOTANY, GEOLOGY, MINERALOGY, ZOOLOGY, and under each the titles of Articles referring thereto are collected.

H

I

T

U

V

W

Y

Z