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ERRATA.

Page 193, line 3 from top, for "declination" read "co-declination."
" 364, line 10 from bottom, read "one to two thousand."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. I.—*An account of the Meteor which burst over Weston in Connecticut, in December*, 1807, *and of the falling of stones on that occasion;* by Professors Silliman and Kingsley.*

On the 14th of December, 1807, about half past 6 o'clock, a.m., a meteor was seen moving through the atmosphere, with very great velocity, and was heard to explode over the town of Weston, in Connecticut, about 25 miles west of New Haven. Nathan Wheeler, Esq., of Weston, one of the justices of the court of common pleas for the county of Fairfield, a gentleman of great respectability, and of undoubted veracity, who seems to have been entirely uninfluenced by fear or imagination, was passing at the time through an inclosure adjoining his house, and had an opportunity of witnessing the whole phenomenon. From him the account of the appearance, progress, and explosion of the meteor, is principally derived. The morning was somewhat cloudy. The clouds were dispersed in unequal masses, being in some places thick and opaque, and in others fleecy and partially transparent. Numerous spots of unclouded sky were visible, and along the northern part of the horizon a space of ten or fifteen degrees was perfectly clear. The attention of Judge Wheeler was first drawn by a sudden flash of light, which illuminated every object. Looking up he discovered in the north a globe of fire, just then passing behind the cloud, which obscured, though it did not entirely hide the meteor. In this

* No detailed account of the Weston Meteor has ever appeared in this Journal. It has been thought proper therefore to reproduce the following account from the Memoirs of the Connecticut Academy of Arts and Sciences. This is introductory to some considerations upon the path of the body.

situation its appearance was distinct, and well defined, like that of the sun seen through a mist. It rose from the north, and proceeded in a direction nearly perpendicular to the horizon, but inclining, by a very small angle, to the west, and deviating a little from the plane of a great circle, but in pretty large curves, sometimes on one side of the plane, and sometimes on the other, but never making an angle with it of more than 4 or 5 degrees. Its apparent diameter was about one-half or two-thirds the apparent diameter of the full moon. Its progress was not so rapid as that of common meteors and shooting stars. When it passed behind the thinner clouds, it appeared brighter than before ; and when it passed the spots of clear sky, it flashed with a vivid light, yet not so intense as the lightning in a thunder storm, but rather like what is commonly called *heat lightning.*

Where it was not too much obscured by thick clouds, a waving conical train of paler light was seen to attend it, in length about 10 or 12 diameters of the body. In the clear sky a brisk scintillation was observed about the body of the meteor, like that of a burning firebrand carried against the wind.

It disappeared about 15 degrees short of the zenith, and about the same number of degrees west of the meridian. It did not vanish instantaneously, but grew, pretty rapidly, fainter and fainter, as a red hot cannon-ball would do, if cooling in the dark, only with much more rapidity.

There was no peculiar smell in the atmosphere, nor were any luminous masses seen to separate from the body. The whole period between its first appearance and total extinction, was estimated at about thirty seconds.

About thirty or forty seconds after this, three loud and distinct reports, like those of a four-pounder, near at hand, were heard. They succeeded each other with as much rapidity as was consistent with distinctness, and, altogether, did not occupy three seconds. Then followed a rapid succession of reports less loud, and running into each other, so as to produce a continued rumbling, like that of a cannon-ball rolling over a floor, sometimes louder, and at other times fainter : some compared it to the noise of a wagon, running rapidly down a long and stony hill ; or, to a volley of musketry, protracted into what is called, in military language, *a running fire.* This noise continued about as long as the body was in rising, and died away apparently in the direction from which the meteor came.

The accounts of others corresponded substantially with this. Time was differently estimated by different people. Some augmented the number of loud reports, and terror and imagination seem, in various instances, to have magnified every circumstance of the phenomenon.

The only thing which seemed of any importance beyond this statement, was derived from Mr. Elihu Staples, who said, that when the meteor disappeared, there were apparently three successive efforts or leaps of the fire-ball, which grew more dim at every throe, and disappeared with the last.

The meteor was seen as far south as New York ; and the explosion was heard, and a tremulous motion of the earth perceived, between forty and fifty miles north of Weston. From the various accounts which we have received of the appearance of this body at different places, we are inclined to believe, that the time between the disappearance and report, as estimated by Judge Wheeler, is too little, and that a minute is the least time which could have intervened. Taking this, therefore, for the time, and the apparent diameter of the body as only half that of the full moon, its real diameter could not be much less than 300 feet.*

We now proceed to detail the consequences which followed the explosion and apparent extinction of this luminary.

We allude to the fall of a number of masses of stone in several places, principally within the town of Weston. The places which had been well ascertained at the period of our investigation were six. The most remote were about 9 or 10 miles distant from each other, in a line differing little from the course of the meteor. It is therefore probable that the successive masses fell in this order, the most northerly first, and the most southerly last. We think we are able to point out three principal places where stones have fallen, corresponding with the three loud cannon-like reports, and with the three leaps of the meteor, observed by Mr. Staples. There were some circumstances common to all the cases. There was in every instance, immediately after the explosions had ceased, a loud whizzing or roaring noise in the air, observed at all the places, and so far as was ascertained, at the moment of the fall. It excited in some the idea of a tornado ; in others, of a large cannon-shot in rapid motion, and it filled all with astonishment and apprehension of some impending catastrophe. In every instance, immediately after this, was heard a sudden and abrupt noise, like that of a ponderous body

* From subsequent information it appears that this meteor was seen in the eastern part of Connecticut, in New Jersey, in the interior of the state of New York, and as high up, at least, as Rutland, in Vermont.

It was stated by Professor Day, in a discourse before the Connecticut Academy, that a gentleman who was riding in Colchester in Connecticut, which is about 50 miles east of Weston, saw this meteor distinctly ; it was passing within 15 or 20 degrees of the moon, and appeared to him to be about one-half as large as that luminary. It was justly remarked by Mr. Day that, if at this distance, it had this apparent diameter, its real diameter must have been 1200 or 1300 feet, or, about a quarter of a mile ; but, as the apparent diameter was not taken with an instrument, but by estimation, it was not supposed that this conclusion was perfectly exact. It is evident, at least, that the meteor must have been much higher, when it exploded, than was at first supposed.

striking the ground in its fall. Excepting one, the stones were more or less broken. The most important circumstances of the particular cases were as follows:

I. The most northerly fall was within the limits of Huntington, on the border of Weston, about 40 or 50 rods east of the great road from Bridgeport to Newtown, in a cross road, and contiguous to the house of Mr. Edwin Burr. Mr. Burr was standing in the road, in front of his house, when the stone fell. The noise produced by its collision with a rock of granite was very loud. Mr. Burr was within 50 feet, and immediately searched for the body, but it being still dark, he did not find it till half an hour after. By the fall, some of it was reduced to powder, and the rest of it was broken into very small fragments, which were thrown around to the distance of 20 or 30 feet. The rock was stained at the place of contact with a deep lead color. The largest fragment which remained did not exceed the size of a goose egg, and this Mr. Burr found to be still warm to his hand. There was reason to conclude from all the circumstances, that this stone must have weighed about twenty or twenty-five pounds.

Mr. Burr had a strong impression that another stone fell in an adjoining field, and it was confidently believed that a large mass had fallen into a neighboring swamp, but neither of these had been found. It is probable that the stone, whose fall has now been described, together with any other masses, which may have fallen at the same time, was thrown from the meteor at the first explosion.

II. The masses, projected at the second explosion, seem to have fallen principally at and in the vicinity of Mr. William Prince's in Weston, distant about five miles, in a southerly direction, from Mr. Burr's. Mr. Prince and family were still in bed, when *they heard a noise like the fall of a very heavy body, immediately after the explosions.* They formed various unsatisfactory conjectures concerning the cause, nor did even a fresh hole made through the turf in the door yard, about 25 feet from the house, lead to any conception of the real cause.

They had indeed formed a vague conjecture that the hole might have been made by lightning, but would probably have paid no further attention to the circumstance, had they not heard, in the course of the day, that stones had fallen that morning in other parts of the town. This induced them, towards evening, to search the hole in the yard, where they found a stone buried in the loose earth which had fallen in upon it. It was two feet from the surface, the hole was about twelve inches in diameter, and as the earth was soft and nearly free from stones, the mass had sustained little injury, only a few small fragments having been detached by the shock. The weight of this stone was about thirty-five pounds. From the descriptions which we have heard, it must have been a noble specimen, and men of science will not cease to deplore that so rare a treasure should have been immediately broken in pieces. All that remained unbroken of this mass, was a piece of twelve pounds weight, since purchased by Isaac Bronson, Esq., of Greenfield, with the liberal view of presenting it to some public institution.

Six days after, another mass was discovered, half a mile northwest from Mr. Prince's. The search was induced by the confident persuasion of the neighbors that they heard it fall near the spot, where it was actually found buried in the earth, weighing from seven to ten pounds. It was found by Gideon Hall and Isaac Fairchild. It was in small fragments, having fallen on a globular detached mass of gneiss rock, which it split in two, and by which it was itself shivered to pieces.

The same men informed us, that they suspected another stone had fallen in the vicinity, as the report had been distinctly heard, and could be referred to a particular region somewhat to the east. Returning to the place after an excursion of a few hours to another part of the town, we were gratified to find the conjecture verified, by the actual discovery of a mass of thirteen pounds weight, which had fallen half a mile to the northeast of Mr. Prince's. Having fallen in a ploughed field, without coming into contact with a rock, it was broken only into two principal pieces, one of which, possessing all the characters of the stone in a remarkable degree, we purchased ; for it had now become an article of sale.

Two miles southeast from Mr. Prince's, at the foot of Tashowa Hill, a fifth mass fell. Its fall was distinctly heard by Mr. Ephraim Porter and his family, who live within forty rods of the place, and in full view. They saw a smoke rise from the spot, as they did also from the hill, where they are positive that another stone struck, as they heard it distinctly. At the time of the fall, having never heard of any such thing, they supposed that lightning had struck the ground, but after three or four days, hearing of the stones which had been found in their vicinity, they were induced to search, and the result was the discovery of a mass of stone in the road, at the place where they supposed the lightning had struck. It penetrated the ground to the depth of two feet in the deepest place ; the hole was about twenty inches in diameter, and its margin was colored blue from the powder of the stone, struck off in its fall. It was broken into fragments of moderate size, and from the best calculations might have weighed 20 or 25 pounds.

The hole exhibited marks of much violence, the turf being very much torn, and thrown about to some distance.

We searched several hours for the stone, which was heard to fall on the hill, but without success. Since that time, however, it has been discovered. It is unbroken, and exactly corresponds in appearance with the other specimens. It weighs 36½ pounds.* It is probable that the five stones last described were all projected at the second explosion.

III. At the third explosion a mass of stone far exceeding the united weight of all we have hitherto described, fell in a field belonging to Mr. Elijah Seeley, and within thirty rods of his house. Mr. Seeley's is at the distance of about four miles from Mr. Prince's. Mr. Elihu Staples lives on the hill, at the bottom of which this body fell, and carefully observed the whole phenomenon.

After the last explosion, he says, a rending noise like that of a whirlwind passed along to the east of his house and immediately over his orchard, which is on the declivity of the hill. At the same instant a streak of light passed over the orchard in a large curve, and seemed to pierce the ground. A shock was felt, and a report heard like that of a heavy body falling to the earth ; but no conception being entertained of the real cause (for no one in this vicinity, with whom we have conversed, appeared to have ever heard of the fall of stones from the skies), it was supposed that lightning had struck the ground. Three or four hours after the event, Mr. Seely went into his field to look after his cattle. He found that some of them had leaped into the adjoining inclosure, and all exhibited strong indications of terror. Passing on, he was struck with surprise at seeing a spot of ground which he knew to have been recently turfed over, all torn up, and the earth looking fresh, as if from recent violence. Coming to the place, he found a great mass of fragments of a strange looking stone, and immediately called for his wife, who was second on the ground.

Here were exhibited the most striking proofs of violent collision. A ridge of micaceous schistus lying nearly even with the ground, and somewhat inclining like the hill to the southeast, was shivered to pieces, to a certain extent, by the impulse of the stone, which thus received a still more oblique direction, and forced itself into the earth to the depth of three feet, tearing a hole of five feet in length and four and a half feet in breadth, and throwing large masses of turf and fragments of

* It has been purchased by Mr. Gibbs, of Newport, Rhode-Island, who has thus enriched his splendid collection of minerals with the finest meteoric stone which is probably extant. This specimen abounds so much in iron, that it might almost be denominated an iron ore; some of the pieces of iron visible on the surface are more than an inch long.

stone and earth to the distance of 50 and 100 feet. Had there been no meteor, no explosions, and no witnesses of the light and shock, it would have been impossible for any person contemplating the scene to doubt, that a large and heavy body had really fallen from the skies with tremendous momentum.

From the best information which we could obtain of the quantity of fragments of this last stone, compared with its specific gravity, we concluded that its weight could not have fallen much short of 200 pounds. All the stones, when first found, were friable, being easily broken between the fingers; this was especially the case, where they had been buried in the moist earth; but by exposure to the air, they gradually hardened.

This stone was all in fragments, none of which exceeded the size of a man's fist, and was rapidly dispersed by numerous visitors, who carried it away at pleasure. Indeed we found it difficult to obtain a sufficient supply of specimens of the various stones, an object, which was at length accomplished, principally by importunity and purchase.

The specimens obtained from the different places are perfectly similar. The most superficial observer would instantly pronounce them portions of a common mass. Few of the specimens weigh one pound, most of them less than half a pound, and from that to the fraction of an ounce.

The piece lately found on Tashowa Hill is the largest with which we are acquainted. Mr. Bronson's is the next in size. The largest specimen in our possession weighs six pounds, and is very perfect in its characteristic marks. Of smaller pieces we have a good collection. They possess every variety of form, which might be supposed to arise from fracture with violent force. On many of them, and chiefly on the large specimens, may be distinctly perceived portions of the external part of the meteor. It is everywhere covered with a thin black crust, destitute of splendor, and bounded by portions of the large irregular curve, which seems to have enclosed the meteoric mass. This curve is far from being uniform. It is sometimes depressed with concavities, such as might be produced by pressing a soft and yielding substance. The surface of the crust feels harsh, like the prepared fish skin, or shagreen. It gives sparks with the steel. There are certain portions of the stone covered with the black crust, which appear not to have formed a part of the outside of the meteor; but to have received this coating in the interior parts, in consequence of fissures or cracks, produced probably by the intense heat, to which the body seems to have been subjected. These portions are very uneven, being full of little protuberances. The specific gravity of the stone is 3·6, water being one. The specific gravity of different pieces varies a little; this is the mean of three.

The color of the mass of the stone is mainly dark ash, or, more properly, a leaden color. It is interspersed with distinct masses, from the size of a pin's head to the diameter of one or two inches, which are almost white, resembling in many instances, the crystals of feldspar in some varieties of granite. The texture of the stone is granular and coarse, resembling some pieces of grit stone. It cannot be broken by the fingers, but gives a rough and irregular fracture with the hammer, to which it readily yields. On inspecting the mass, five distinct kinds of matter may be perceived by the eye.

1. The stone is thickly interspersed with black or grey globular masses, most of them spherical, but some are oblong. Some of them are of the size of a pigeon shot, and even of a pea, but generally they are much smaller. They can be detached by any pointed iron instrument, and leave a concavity in the stone. They are not attractable by the magnet, and can be broken by the hammer. If any of them appear to be affected by the magnet, it will be found to be owing to the adherence of a portion of metallic iron.

2. Masses of yellow pyrites may be observed. Some of them are of a brilliant golden color, and are readily distinguishable by the eye. Some are reddish and some are whitish. The pyrites appear most abundant in the light colored spots, where they exhibit very numerous and brilliant points, which are very conspicuous through a lens.

3. The whole stone is interspersed with malleable iron, alloyed with nickel. These masses of malleable iron are very various in size, from mere points to the diameter of half an inch. They may be made very conspicuous by drawing a file across the stone.

4. The lead-colored mass has been described already, and constitutes by far the greater part of the stone. After being wet and exposed to the air, the stone becomes covered with numerous reddish spots, which do not appear in a fresh fracture, and arise manifestly from the rusting of the iron.

5. There are a few instances of matter dispersed irregularly through the stone, which are considered as intermediate between pyrites and malleable iron. They are sometimes in masses apparently crystalline, but usually irregular. They are black, and commonly destitute of splendor, but exposed by a recent fracture, they appear like a glossy superficial coating. They are sometimes attractable by the magnet, and sometimes not.

ART. II.—*On the distillation of dense Hydrocarbons at high temperatures, technically termed "Cracking;"* by S. F. PECKHAM.

IN the American reprint of the Chemical News, for June of this year, an article appears "On Naphtha and Illuminating Oil from heavy California Tar," by Prof. B. Silliman, copied from the San Francisco Bulletin. In the September number of the same journal, an article appears "On the Distillation of Hydrocarbons," by Joseph Hirsch, Ph.D., in which the results obtained by Prof. S. are subjected to criticism, and certain statements made in reference to the subject of a most extraordinary character.

At the same time that Mr. Corning was engaged upon the experiments, the results of which form the subject of Prof. Silliman's paper, I was engaged upon experiments of a similar character for the Geological Survey of California, the results of which have not yet been published.* These results differed somewhat from those obtained by Prof. S., as also the method by which they were obtained. I shall, therefore, give a brief summary, both of method and results, and compare the conclusions to be derived from them with the statements made by Dr. Hirsch.

Those who first attempted the manufacture of commercial oils from crude California materials, when operating with the upright still in common use for the manufacture of Pennsylvania oils, encountered an apparently insurmountable obstacle, viz: a large proportion of the distillate was neither light nor heavy, neither burning oil nor lubricating oil, but an oil intermediate in density between the two, and therefore not merchantable. The difficulty was so far overcome by enclosing the stills in brick-work, heating them *entirely* by radiant heat, and distilling very slowly, that the amount of heavy lubricating oil was largely increased, and the "middlings" correspondingly diminished. The yield of illuminating oil, however, was very slightly increased, and it was for the purpose of securing a larger yield of that material, that my experiments were undertaken. I had at first intended to subject them to Mr. Downer's process of slow distillation in a high, upright still, the top of which was exposed to radiation. The small quantity of crude material at my command (5 gallons of each variety) rendered this operation exceedingly difficult to conduct suc-

* The volume of Reports of the Survey on "Economical Geology," containing these results, is now ready for the press, but its publication is delayed by the failure of the last California Legislature in making the necessary appropriation.

cessfully, and it was with extreme satisfaction that I saw at the time, in the October number of the Chemical News for 1866, an announcement that Mr. Young of Glasgow had obtained a patent for the manufacture of illuminating oils from heavy paraffine oils, by distillation under pressure. It was necessary that I should operate on a small quantity at a time, and also, that I should subject the four or five different samples which I had, to the same treatment, in order that I might compare the results, and judge of their relative value. For that purpose I contrived the apparatus described in the September number of this Journal for 1867, which, so far as I know, has but one fault, viz: the chamber of the valve is too small, and should be enlarged sufficiently to enable the pressure to be regulated by weights instead of by a spring.

With a pressure of between 30 and 40 pounds per square inch (the exact amount was not ascertained) the following results were obtained. See table, page 11.

No. I was an oil procured from one of the tunnels of the Hayward Petroleum Company, of a specific gravity of ·9023, yielding by distillation in a common still about 15 per cent of light oil, of a specific gravity of ·810, with about 40 to 50 per cent of "middlings," and 20 per cent of light lubricating oil.

No. II came from the celebrated Pico Spring, yielding the lightest oil of any natural outcrop in southern California. Its specific gravity was ·8932, and it yielded to treatment by the ordinary method only about 20 per cent of illuminating oil of the proper density.

No. III was from the Cañada Laga, of a specific gravity of ·9184, and yielding by the ordinary process only 3 per cent of illuminating oil.

No. IV was a sample of Maltha from the same spring as that operated upon by Prof. Silliman. Its specific gravity was ·978, and it yielded about 2·5 per cent of illuminating oil.

This table exhibits the results of actual experiment, not of theory, and while they differ from those obtained by Prof. Silliman, the difference is in degree and not in kind, and is without doubt due to the superiority of the apparatus used by myself, and to the higher degree of pressure employed. Both series of experiments confirm each other, and alike prove, that dense petroleums and a thick heavy tar—as thick as ordinary molasses—which yield practically little or no illuminating oil by ordinary treatment, by distillation under pressure are subjected to what is technically termed "cracking," and made to yield from 28 to 60 per cent of oil fit for burning, and rendered thereby nearly as valuable as the crude oils of Pennsylvania.

Percentage results of Distillation under Pressure.

Number	I.	II.	III.	IV.
Volatile impurity, consisting of air and water,	----	----	----	12·5
1st. Pressure Distillation of crude material,	91·—	87·66	82·86	72·20
Coke and loss at do.,	9·—	12·34	17·34	15·50
1st Fractionation of sp. gr. 43° B. = ·810,	42·—	56·72	40·33	16·70
Leaving Heavy Residue for re-distillation,	49·—	30·94	42·33	55·30
Which yielded, by 2d Pressure Distillation,	44·15	27·84	58·09	49·80
2d. Fractionation of sp. gr 43° B. = ·810,	12·25	6·96	9·52	12·40
Total crude Illuminating Oil,	54·25	62·68	49·85	29·10
Loss in treatment of do., $\frac{3}{100}$,	1·62	1·88	1·49	·90
Total yield of Refined Oil,	51·25	60·70	48·36	28·20
Total crude Lubricating Oil,	31·85	20·88	28·57	37·40
Loss in treatment of do., $\frac{3}{100}$,	·95	·62	·85	1·10
Total Refined Lubricating Oil,	30 90	20·26	27·72	36·30
Yield of Refined Illuminating Oil, sp. gr. 43° B.,	51·25	60·70	48·36	28·20
Yield of Refined Lubricating Oil, sp. gr. 23°–25° B.,	30·90	20·26	27·72	36·30
Loss in treatment,	2·61	2·50	2·34	2·00
Coke and loss in Distillation,	18·24	16·54	21·58	33·50
Yield of Illuminating Oil by ordinary process,	15·00	20·00	3·00	2·50

Dr. Hirsch criticises these experiments as not being executed under circumstances "parallel to the distillation on a large scale." He then states, that during the distillation of hydrocarbons on the large scale, "the process of 'cracking' always takes place in some degree," as "all hydrocarbons of high boiling points contained in such mixtures are, during distillation, exposed to varying degrees of temperature below their own boiling point, as long as those hydrocarbons of lesser gravity and lower boiling point have not been removed by distillation." He further states, that "it is this exposure to a lower degree of heat than corresponds to the distilling point of an oil of definite gravity, which comprises the operation of 'cracking.'" Again he states, that during slow distillation in the enormous stills now being introduced, "cracking" takes place without any "special efforts" to produce such a result, "while only a small portion distills over as paraffine oil, that being due to over-heating." He next states, that by rapid distillation of a small quantity, the different hydrocarbons which make up the petroleum come over unchanged, and that the difference between this last named distillation and the former, is the same as the one between distilling coal for the production of illuminating gas, and that for producing coal oil; the former producing a dense tar, being carried on in small low retorts, and the latter, in revolving retorts of large capacity. "In these the oily vapors are exposed to a cooler temperature than their own with every revolution of the retort, and are in this manner broken up into oils of lighter gravity."

He then gives a number of rules, the result of his own experience.

"1st. * * * the more the temperature of the actual boiling point of oil of definite gravity is above the temperature to which the same oil is raised, the greater is the quantity of light oil obtained.

"2d. The gravity of distillate resulting from reduction of temperature will be directly proportionate to said reduction. * * * In distillation, the temperature, therefore, should always be reduced to the boiling point of the oil of the specific gravity desired.

"3d. The difference between the temperatures of the two boiling points, viz: of the oil being subjected to distillation, and of the derived distillate, is in direct proportion to the height of the still employed, or to the facility for cooling the upper portions of the still.

"4th. The intensity of the process of 'cracking' is proportionate to the suddenness with which the oil vapors are condensed before leaving the still.

"5th. The difference in gravity between that of the oil distilled, and the desired distillate, is in direct proportion to the quantity of water produced in the process.

"These laws are the same with hydrocarbons distilled under the ordinary atmospheric pressure, as with those distilled in a vacuum, or under increased pressure."

It is very rarely that as many errors are included within as little space, and the entire discussion exhibits in a remarkable degree to what totally erroneous conclusions the results of close observation and experience may lead, when explained upon a false hypothesis.

The operation of "cracking," as conducted by Mr. Downer, consists in a slow distillation of oils of high specific gravity, and high boiling point, in a still furnished with a high dome, the outer surface of which is freely exposed to radiation. As distillation proceeds, those oils which are condensed at the temperature at which the dome is maintained, instead of passing into the worm and thence into the receiver, collect in drops upon the surface of the dome, and fall back upon the surface of the oil beneath, which has meantime become heated above their boiling points. Mr. Young distills the oils under pressure, thereby vaporizing them at a temperature above their normal boiling points.

It is therefore obvious, that the primary and essential condition of "cracking" is simply to subject the oils to a temperature above their boiling points, or in other words, to super-heat their vapors. It will be found that for oils of the same density, the higher the temperature to which they are raised, or at which they are distilled, the lighter will be the product; and that to produce an oil of given density, the heavier oil must be raised to a certain fixed temperature, the intensity of heat depending on the lightness of the oil required.

Now it is evident that Prof. Silliman could not subject five or ten gallons of Maltha to experiment *strictly* analogous to Mr. Downer's process, the two elements of time and large capacity of apparatus being practically unattainable when manipulating so small a quantity. He could, however, follow Mr. Young's process *strictly*, using from 10 to 15 pounds pressure per square inch. My own results were obtained by using from 30 to 40 pounds pressure per square inch, and operating upon only 1,500 cubic centimeters at a time.

Dr. Hirsch is correct in stating that during the distillation on the large scale, this process always obtains action in some degree, but his reasoning is utterly at fault. So, too, is his explanation of the fact, that "cracking" takes place in large stills without any special effort to secure such a result. The real explanation lies in the fact, that the upper portion of stills in ordinary use is generally exposed to atmospheric currents and radiation. With such an arrangement it is impossible, upon Mr. Downer's plan, to prevent more or less condensation upon the dome, and consequent "cracking," especially toward the end of the operation, in stills of the enormous capacity of 40,000 gallons, where all the conditions essential to his process are present. It was in stills set in this manner that the heavy California oils were first distilled, and in which they were "cracked" to an oil of medium density; but when the sides and domes of the stills were surrounded with brick-work, the vapors were no longer condensed, and they passed unchanged into the receiver.

Dr. Hirsch is again in error, in supposing that paraffine oils are produced by a high temperature. I am told that Mr. Downer has made illuminating oils, by "cracking" solid paraffine wax by means of his process. The paraffine lubricating oils of commerce are now most successfully produced from coals, by distilling the material in large kilns, in which combustion takes place at the upper surface, and the draft is conducted downward, insuring the expulsion of the volatile products at the very lowest temperature possible.

He is yet again in error, in the analogy which he assumes to exist between rapid and slow distillation of petroleum, and the distillation of coals, in small retorts to produce illuminating gas, and in revolving retorts to produce oil. Rapid distillation "cracks" the oil, because it necessitates increased temperature to force the vapors from the still. Such has been my experience repeatedly on both the large and small scale. Slow distillation yields the hydrocarbons *unchanged*, provided the vapors have ready egress from the still, because distillation is then carried on at the lowest temperature attainable. Small retorts are used for the manufacture of gas, in order that the coal may the sooner be raised to the red-heat, and the greatest possible "cracking" effect be experienced, while revolving retorts are used in the manufacture of oil, not that the charge may be repeatedly cooled, but in order that it may be uniformly heated, avoiding the necessity of over-heating the portion next the fire, in order that the upper portion may be heated sufficiently.

His first and second rules, when reduced to plain English, assert that "cracking" may be produced by refrigeration. Following their lead, in order to produce from paraffine wax the lightest member of the naphtha series isolated by Prof. Warren, and boiling at 0° C., the paraffine should be immersed in melting ice. According to these rules, the best method of producing illuminating gas from crude petroleum would be, to subject the oils to the action of a refrigerating mixture of solid carbonic acid and ether, instead of allowing them to drip upon red-hot coke.

His third rule is correct, as the lower the temperature at which the top of the still is maintained, the lower will be the boiling point of the liquid resulting from the condensation of the vapors that escape.

His fourth rule is too obscure in its signification to admit of criticism.

His fifth rule is of the most extraordinary character. Chemistry is not yet ready for the announcement of the transmutation of one element into another, and such transmutation must certainly take place if WATER *can* be produced by distillation of volatile HYDROCARBONS, with exclusion of OXYGEN. So, too, is it almost equally difficult to imagine how any general laws can be "the same" for two processes so diametrically opposed as distillation in a vacuum and under pressure.

I desire in this connection to note a few suggestions which have occurred to me in reference to this subject. In the last edition of Prof. Dana's Mineralogy (1868), he has classified the results obtained by Profs. Warren and Storer, and arranged the hydrocarbons isolated by them in three groups, viz: the Naphtha and Beta-naphtha series, and the Pittoleum group. The first two are isomeric, the last contains more carbon in proportion to its hydrogen. The members of the Pittoleum group at present isolated are doubtless the lower members of a large group, the higher members of which have very high boiling points; or perhaps there is still another group containing a still larger proportion of carbon. As the different members of these groups decrease in density, the proportion of hydrogen increases, and as they increase in density, the proportion of carbon increases. The process of "cracking" Pennsylvania oils, therefore, is simply subtraction of carbon; and it appears from the results of experiment and analysis, that each additional atom of carbon is held by a feebler affinity than the last, consequently the stability of the members increases as the proportion of carbon decreases. The lower the member is in the series, the stronger is the affinity of the hydrogen for the carbon, and consequently, the higher is the temperature required for the production of the member next below it. Thus it is that over-heating dense paraffine oils produces medium or illuminating oils; over-heating illuminating oils produces still lower and more volatile liquids; at a still higher temperature the products become gaseous, and at an excessively high temperature, light rather than heavy carburetted hydrogen gas is produced.

In the absence of actual demonstration by fractionation, I am led to believe from the behavior of California petroleums, that they do not contain either the Naphtha or Beta-naphtha series in appreciable quantity; nor do they contain the members of the Pittoleum group yet isolated in large proportion, but are doubtless made up of the higher members of that group, or a still more highly carbonized and unstable group not yet described, with which is mingled one or more nitro-hydrocarbons yet more easily decomposed. Be this fact or fancy, the appearance and physical properties of the refined pressure distillate from these oils, lead to the opinion that it is made up of the same members of which refined Pennsylvania petroleum is composed. The lightest oils existing in crude California petroleum change in a few weeks, after treatment, to a dirty yellow color, even when tightly corked and exposed only to the light. A bottle of refined pressure distillate in my possession has now been prepared nearly two years, yet its color has scarcely changed perceptibly.

By Prof. Warren's process of fractionation only a trace was eliminated from any of my samples of crude California oil under 150° C., yet in one instance, when my valve accidentally stuck so that the pressure was very considerably increased above 40 pounds, the vapors when they escaped passed through the worm uncondensed at 8° C., and melted the lead pipe at its point of connection with the retort; proving that as in the case of the heavy paraffine oils, decrease in the density of the distillate follows any considerable increase in the temperature of distillation.

I hope at some future day to be able to fractionate both the crude California petroleums, and the products of their distillation under pressure, and thus obtain some additional facts in reference to this interesting question.

Cambridge, Mass., Nov. 9th, 1868.

ART. III.—*On the Chromites of Magnesium;* by W. R. NICHOLS, student at the Massachusetts Institute of Technology.

THE extent of the power possessed by some of the hydrated sesquioxyds, notably by the hydrate of chromium, to hold magnesia and several other of the metallic protoxyds in insoluble combination, appears never to have been distinctly recognized by chemists. Fresenius, it is true, remarks* that sesquioxyd of chromium cannot be separated (quantitatively) by ammonia from the alkaline earths, since, even though carbonic acid be completely excluded, portions of the alkaline earths are thrown down in combination with the sesquioxyd of chromium. But his statement is far less forcible than the facts in the case demand.

Other chemists have fallen into the grave error of supposing that magnesium can be separated from chromium, with sufficient accuracy for the ordinary purposes of qualitative analysis, by mere addition of ammonia-water to throw down hydrate of chromium, after the solution to be analyzed has been mixed with chlorid of ammonium. Galloway† has developed a plan for separating the hydrates of iron, aluminum and chromium as a distinct group or class, by adding chlorid of ammonium and ammonia to the boiled filtrate from the precipitate produced by sulphuretted hydrogen in the ordinary course of an analysis. According to Galloway, this ammonia precipitate is collected by itself and examined for iron, aluminum and chromium; sulphid of ammonium is then added to the filtrate to throw down cobalt, nickel, zinc and manganese; carbonate of ammonium is next employed to precipitate barium, strontium and calcium, while the magnesium, if any be present, is finally detected by means of phosphate of sodium in the usual way.

As will appear, however, from the following experiments, it would be quite impossible in many cases to detect magnesium in this way in presence of chromium. From a solution con-

* Quantitative Chemical Analysis, 4th ed. London, 1865, p. 372.

† Manual of Qualitative Analysis, 4th ed. London, 1864, p. 84 et seq.

taining any considerable proportion of sesquioxyd of chromium as well as magnesium, the greater part or the whole of the latter will be thrown down, together with the chromium, on the addition of ammonia, even when the solution is highly charged with chlorid of ammonium.

Sulphid of ammonium, also, like ammonia, throws down from mixed solutions of chromium and magnesium a compound precipitate well-nigh insoluble in water and saline solutions. If no serious difficulty has been met with hitherto in detecting magnesium in presence of chromium by the ordinary method of qualitative analysis, it is doubtless owing to the fact that, in pursuing this method, some of the magnesium thrown down in combination with chromium will be encountered when the precipitate produced by sulphid of ammonium comes to be examined in regular course for (barium, strontium, calcium and) magnesium, which may have fallen down in combination with phosphoric, boracic, oxalic, or silicic acid, or in the form of fluorids; and this, even if nothing but chromium and magnesium are present.

As thrown down either by ammonia or by sulphid of ammonium, the compound precipitate of chromium and magnesium is nearly or quite insoluble in solutions of the fixed caustic alkalies, and even when the precipitate is dissolved in chlorhydric acid and the solution mixed with cold soda lye, all the magnesium and most of the chromium is re-precipitated. Since this behavior of the precipitate toward soda lye points clearly to the existence of chemical attraction or affinity between the magnesia and the chromic oxyd, I have devoted particular attention to the study of the precipitates thus produced by soda.

Equivalent quantities of chrome alum and Epsom salt were weighed out and dissolved in water with addition of chlorid of ammonium and the mixture was treated with ammonia-water in slight excess. It was found to make no difference with regard to the completeness of the precipitation whether the salts were dissolved together with addition of chlorid of ammonium, or separately, the chlorid of ammonium being added to the solution of sulphate of magnesium before mixing the two solutions.

After the addition of ammonia to the mixed solution of chrome alum and sulphate of magnesium, an attempt was made in each case to determine the amount of magnesium in the filtrate by precipitation with phosphate of sodium. Negative results were obtained for the most part in these trials, as will be seen below.

The precipitate thrown down by ammonia was dissolved in boiling dilute chlorhydric acid, the solution thoroughly cooled, and then mixed with an excess of a cold aqueous solution of caustic soda. The precipitate produced by the soda was filtered as rapidly as possible, washed somewhat with water, dried, ignited and weighed, while the filtrate was boiled to precipitate any chromium which had dissolved. The hydrate of chromium thrown down by boiling was collected on a filter, was washed, dried, ignited and weighed by itself.

I. 2·5098 grm. of chrome alum and 0·5240 grm. of Epsom salt were taken, so that the mixture contained 1·19 equivalent of Cr_2O_3 and 1 equivalent of MgO. No magnesium could be detected in the filtrate from the precipitate produced by ammonia in this mixture. The ammonia precipitate dissolved in chlorhydric acid, and treated with caustic soda in excess, gave up only 0·0103 grm. of Cr_2O_3 to the soda, while 0·3753 grm. of Cr_2O_3 went down again with the magnesium.

An analysis of the ignited soda precipitate gave the following results :—

		Taken.	Found.		Theory (MgO, Cr_2O_3).
(I.)	MgO	0·0847	0·0831	18·13	20·73
	Cr_2O_3	0·3843	0·3753	81·87	79·27
		0·4690	0·4584	100·00	100·00

II. 2·4979 grm. of chrome alum and 0·6150 grm. of Epsom salt were taken, so that the mixture contained 1 equivalent of Cr_2O_3 and 1 equivalent of MgO. Sulphid of ammonium was used as the precipitant in this instance, and 0·0105 grm. of MgO was found in the filtrate from it. When the precipitate was dissolved in chlorhydric acid and treated with soda lye, 0·0181 grm. of Cr_2O_3 went into solution in the soda.

III. 2·469 grm. of chrome alum and 0·302 grm. of Epsom salt were taken, so that the mixture contained 2·02 equivalents of chromic oxyd for 1 equivalent of magnesia. No magnesium could be detected in the filtrate from the ammonia precipitate. 0·0400 grm. of Cr_2O_3 was held dissolved by the soda, and 0·3393 grm. of Cr_2O_3 was thrown down again with the magnesia when the precipitate, after solution in chlorhydric acid, was treated with soda in excess.

An analysis of the ignited soda precipitate gave the following results :—

		Taken.	Found.		Theory ($MgO, 2Cr_2O_3$).
(III.)	MgO	0·0488	0·0492	12·66	11·56
	$2Cr_2O_3$	0·3780	0·3393	87·34	88·44
		0·4268	0·3885	100·00	100·00

IV. 2·0244 grm. of chrome alum and 0·2500 grm. of Epsom salt were taken, so that the mixture contained 2 equivalents of chromic oxyd for 1 equivalent of magnesia. A trace of magnesium was detected in the filtrate from the ammonia precipitate, and 0·0337 grm. of MgO was obtained from the soda precipitate. 0·1167 grm. of Cr_2O_3 was thrown down on boiling the filtrate from the soda precipitate.

V. 2·3967 grm. of chrome alum and 0·3005 grm. of Epsom salt were taken, so that the mixture contained 1·97 equivalent of Cr_2O_3 for 1 equivalent of MgO. No magnesium was detected in the filtrate from the ammonia precipitate. 0·0250 grm. of Cr_2O_3 was thrown down on boiling the filtrate from the soda precipitate.

VI. 1·0120 grm. of chrome alum and 0·5000 grm. of Epsom salt were taken, so that the magnesia should be in large excess, the proportion of magnesia to chromic oxyd in this case being as 2 to 1. 0·0077 grm. of MgO was obtained in the filtrate from the ammonia precipitate, and 0·0765 grm. of MgO in the soda precipitate. No chromium was found in the filtrate from the ammonia precipitate, while 0·1508 grm. was obtained from the soda precipitate. The results may be tabulated as follows:

		Taken.	Found in the soda precipitate.		Theory ($2MgO, Cr_2O_3$).
(VI.)	2MgO	0·0809	0·0765	33·66	34·34
	Cr_2O_3	0·1549	0·1508	66·34	65·66
		0·2358	0·2273	100·00	100·00

The soda precipitates, after having been ignited and weighed, were decomposed by fusion with carbonate of sodium and nitrate of potassium in the usual way.* The chromium was then determined as chromate of lead, and the magnesium in the form of pyrophosphate.

It was found to be impossible, in this way, to convert all the chromium into a soluble compound in one operation. On treating the fused mass with water, there remained undissolved, on every occasion, a certain quantity of a fine, whitish-yellow substance, which was so finely divided as to pass at first quite readily through the filter; it could be collected only by returning the filtrate to the filter several times. A second fusion with carbonate of sodium and nitrate of potassium was consequently resorted to in each case, in order to complete the decomposition. The yellow color of the fused mass and of the solutions obtained after these second fusions, showed clearly enough that, whatever the real composition of the unresolved powder may be, a certain proportion of chromium is always

* Fresenius. Quantitative Analysis, p. 372.

contained in it. It should be stated that considerable difficulty was encountered in filtering the chromite of magnesium precipitated by caustic soda, especially in those cases where a comparatively large proportion of chromium remained dissolved in the excess of soda, for hydrate of chromium is gradually deposited from such solutions even in the cold. Toward the close of the filtration a certain portion of the chromium, which was dissolved at first, is in this way precipitated upon the filter and added to the mass of chromite of magnesium.

The foregoing experiments were undertaken at the suggestion of Prof. F. H. Storer, by whom my attention was called to the subject in the autumn of 1867.

Boston, May, 1868.

Art. IV.—*Notices of papers in Physiological Chemistry*—No. II; by George F. Barker, M.D.

5. *On the formation of Sugar in the liver.*

(Continued from vol. xlvi, p. 390.)

(25.) On the 23d of March, 1857, Bernard announced to the Academy* the isolation of the glycogenic matter. His progress was hindered for a long time by the false notion that this substance was an albuminate; but at length he recognized the fact that it was the albuminoid ferment, not the glycogenic substance, which was altered by cooking, and that the latter could be separated from the ferment by solution in hot water. His process for its preparation is as follows: The liver from an animal fed entirely upon meat (though any liver may of course be used) is divided while still warm, into fine shreds, which are thrown into water in active ebullition, to coagulate the ferment. The mass is then bruised in a mortar, mixed with a small quantity of water, and boiled for $\frac{3}{4}$ of an hour. On straining and pressing out, an opaline liquid is obtained, to which is added 4 or 5 times its volume of strong alcohol; an abundant flocculent, slightly yellowish precipitate, is thrown down, which contains sugar, bile, and various nitrogenous bodies, as impurities. It is collected on a filter, washed with strong alcohol, and dried; it then forms a grayish, somewhat gummy mass of crude glycogenic matter. To purify it, it is boiled in a concentrated solution of potassic hydrate for half an hour, diluted, filtered, again precipitated by adding alcohol, collected on a filter, washed with alcohol, re-dissolved in water, neutralized exactly with acetic acid, again thrown down by alcohol, collected and dried. As thus prepared, the glycogenic matter

* C. R., xliv, 578.

resembles very closely hydrated starch; it is neutral, is without odor or taste, produces the sensation of starch on the tongue, and diffuses itself in water, making it strongly opaline. It has no characteristic appearance under the microscope. Iodine produces with it a color, varying from a dark violet-blue to a clear maroon-red. Heated with soda-lime, it gives, when pure, no ammonia, thus proving the absence of nitrogen. It does not reduce alkaline solutions of copper, does not ferment when mixed with yeast, is entirely insoluble in strong alcohol, and is precipitable from its solutions by basic plumbic acetate. It is changed into dextrin and then into sugar, by all those agents which convert ordinary starch into dextrin and sugar; prolonged ebullition with dilute mineral acids, and the action of diastase and analogous ferments, such as exist in the pancreatic juice, the saliva, and the blood, effect this change easily; the opaline character of the solution disappears, it is no longer colored by iodine, it reduces alkaline solutions of copper, and ferments with yeast, yielding alcohol and carbonic acid. The diastasic ferments produce this change in a few minutes at the temperature of the body. Torrefaction, and the partial action of ferments or of dilute acids, change the glycogenic matter into a substance which, like dextrin, is insoluble in strong alcohol, is soluble in water giving a transparent solution, is not colored by iodine, does not reduce copper-tests, does not ferment, and rotates to the right the polarized ray. From the above results, Bernard argues that the formation of sugar in animals should be divided into two parts: the 1st—entirely vital, since it takes place under the influence of life—consists in the production of the glycogenic matter in the living hepatic tissue; the 2d, entirely chemical, is the transformation of this into sugar by means of a ferment. This ferment was at first supposed to belong to the liver, but finding that blood effected the change in a marked degree, Bernard referred it to this fluid. During digestion, the circulation is much more active, and the transformation of the glycogenic substance is correspondingly great; in torpid animals like frogs, on the other hand, where the circulation is slow, the sugar disappears from the liver, while the glycogenic substance remains. If these animals be warmed, however, the sugar re-appears, and so on alternately. The condition of the nervous system also affects the transformation of the glycogenic matter. When the spinal cord is cut or wounded near the neck, below the origin of the phrenic nerves, the activity of the hepatic circulation is diminished, and after 4 or 5 hours there is no trace of sugar in the liver, though glycogenic matter is found there. On wounding the cerebro-spinal axis near the fourth ventricle, the phenomena produced are precisely the reverse; the abdominal circulation is accelerated, and the glycogenic matter is so largely transformed that it appears in the urine. The paper closes by comparing the liver to a seed; first, a vital action stores up starch in it; then a chemical one changes this starch into sugar.

(26.) On the 1st of June, SANSON announced to the Academy* the discovery of glycogenic matter in the spleen, the lungs, and the kidneys, as well as in venous, arterial, and portal blood.

(27.) At the next session, FIGUIER† communicated some experiments to prove that washing the liver by Bernard's method for forty minutes did not deprive it of sugar. He therefore pulped a liver and washed it until free from sugar; none re-appeared after 24 hours standing. He then washed a liver by Bernard's method for 1½ hours, and on dividing it into two parts, found that one at once reduced ·123 grams cuprous oxyd, while after 24 hours, the other half only produced ·102 grams. In a third experiment a horse's liver, proved to contain sugar, was washed through the portal vein for 2½ hours; on examination, no sugar was found in it, nor did it re-appear after 24 hours. Two other experiments resulted similarly. He expresses the opinion that Bernard's glycogenic matter is only albuminose altered by boiling with potash.

(28.) Some experiments on the conversion of the glycogenic matter into xyloidin, were presented to the Academy June 29,‡ by EUG. PELOUZE. One gram of the pure substance was mixed with concentrated nitric acid; in a few moments it dissolved, and the whole was then poured into water to precipitate the xyloidin. This, after drying, weighed 1·3 grams, the theoretical quantity. It was very combustible, taking fire at 180° C. When the glycogenic matter was treated with dilute nitric acid and heated, it was converted into oxalic acid. On combustion, it yielded carbon 39·8, hydrogen 6·1, oxygen 54·1 = 100, giving the formula $\text{Є}_6H_{12}\Theta_6$; while that of ordinary starch treated with potassic hydrate and dried at 100° C., has the composition $\text{Є}_{12}H_{22}\Theta_{11}$. Pelouze had examined other organs of the body, but had failed to find glycogenic matter in any but the liver. On treating the lungs of a calf by Bernard's method, a substance was obtained which at first sight resembled it; this substance, however, could not be changed into sugar and was probably modified albumin. Sanson's results are therefore incorrect.

(29.) In a paper presented at the same session,§ SANSON asserts that ptyalin converts the starch of the food into dextrin, which is absorbed by the abdominal venous system, carried

* C. R., xliv, 1159. † Ib., p. 1213. ‡ Ib., p. 1321 § Ib., p. 1323.

into the tissues by the general circulation, and converted into glucose. Animals nourished on meat, therefore, get dextrin from their food, a fact which explains the contradictory statements as to the existence of fermentable matter in the blood. Sanson shows that the action of the blood-ferment on dextrin produces sugar, by taking 500 grams arterial blood from the carotid of a horse, defibrinating it at once by whipping, and allowing it to stand 24 hours; at the end of this time it contains sugar and readily ferments. He maintains that the liver forms neither glycogenic matter nor sugar, but simply uses that carried to it in the blood; more sugar is found there than elsewhere, because the circulation is slower.

(30.) Sanson was immediately followed by BERNARD,* who asserted that this animal starch existed only in the liver. His experiments show that when rabbits are fed on carrots a large quantity of glycogenic matter is found in the liver, but none in any other organ. When the animal is fed on grain, the case is more complicated, as soluble starch colorable by iodine is then carried into the body. In a former memoir, it was stated that starchy matters did not pass into the blood as dextrin; but the process used was imperfect, and later researches have proved the presence of dextrin in both blood and muscles. Sanson has simply confirmed this fact. Two points were then noticed: I. The formation of glycogenic matter in the liver is constant, and is entirely independent of the external supply of dextrin or sugar. The blood of a rabbit fed for 3 or 4 days on wheat or oats contains dextrin; but this is not true with an animal fed on carrots. Moreover on changing the food of the first rabbit, the dextrin disappears. The glycogenic function, on the other hand, is never altered; it persists always, no matter how the food varies. The production of dextrin in the blood by certain aliments, is of course interesting; but only so as an independent fact. The presence of dextrin in the organs of horses fed on oats or other grain, has been confirmed at the Ecole Imperiale at Alfort, in connection with M. Bouley; but no conclusion as to the formation of sugar can be drawn from these accidental results. Nor has dextrin been detected in beef or mutton from the butcher's; which should not be the case were it present. II. A second proof of this function is the fact that it diminishes or disappears under the influence of many morbid conditions. Hence, the livers of hospital cadavers contain no sugar, and the livers of executed criminals must be used for its detection. The same fact is true of animal starch; whenever the animal is sick, none is formed, even when food is taken as usual. Experiments at the veterinary school

* C. R., xliv, 1325.

above mentioned, upon horses who though diseased yet ate well, showed no glycogenic matter in the liver, though sugar formed from the food was found there. A comparative experiment, witnessed by Prof. Schmidt of Dorpat, was made on two vigorous horses; one was healthy, but was to be killed because of an old paralysis of one fore leg, of traumatic origin; the other had had an acute inflammation of the foot-joint for 3 years, also from an injury. Both were fed on hay and oats and ate well, though the second animal had some fever. Three or four hours after eating they were killed. On dissection, digestion appeared to have taken place perfectly; the lacteals were full of chyle, and the urine was alkaline. The livers were removed and at once examined; that of the well horse furnished a strongly opaline decoction, very rich in glycogenic matter, while that of the arthritic animal gave a limpid solution containing not a trace. The flesh of both animals contained dextrin and sugar, from the food. It appears, therefore, that on the one hand, the formation of animal starch in the carnivora takes place upon food entirely free from carbohydrates, and on the other, that it ceases in the diseased herbivora, even with food rich in these substances. Beside the febrile state, all exhausting conditions, such as prolonged suffering, arrest this production; hence, healthy animals must be used for its demonstration. The paper closes with a new method for the preparation of the glycogenic matter. A filtered decoction of the healthy liver is mixed with glacial acetic acid in excess; a whitish precipitate of almost pure animal starch falls, while the albuminoid matters being soluble, remain dissolved. A diseased liver gives no precipitate; nor does the solution from any other organ. If a liver solution be divided into two parts, and acetic acid added to one, a heavy precipitate falls; the other, on the addition of a little saliva, becomes transparent and gives no precipitate. In this way, therefore, the presence of the glycogenic matter may be easily and rapidly recognized.

(31.) On the 27th of July, FIGUIER* asserted that the sugar which is contained in portal blood is one of the non-fermentable sugars. By a new process, he had isolated a sugar from the jugular and femoral veins of a dog, identical with that in portal blood; it reduced the copper-tests, was not precipitated by basic plumbic acetate, and was not fermentable until boiled with $\frac{1}{100}$ of sulphuric acid. When this sugar remains for a time in the system, it becomes fermentable; hence, kiestein-urine sugar, chyle sugar, and the sugar of eggs, are fermentable. Bernard's sugar, therefore, has its origin in the intestinal canal; and Figuier detected there a sweet substance,

* C. R., xlv, 132.

not precipitable by basic plumbic acetate and not fermentable. He enumerates three varieties of sugar: 1st, that of the intestines, not precipitable or fermentable, does not reduce copper-tests; 2d, that of the portal blood, which reduces copper-tests, and is not directly fermentable, but becomes so on boiling with acid; and 3d, that of the liver and chyle, which reduces copper and ferments directly. He asserts that by the action of a boiling solution of potash upon albumin, a sugar capable of reducing the copper-test is formed. Since as in animal fluids like milk, sugar and the albuminates occur together, they may be mutually dependent.

(32.) A paper by BONNET, confirming Bernard's results, was read at the same session.* Its conclusions are; 1st, there is no sugar in the portal blood of animals fed on meat, though the liver and hepatic blood contain it; 2d, sugar is formed in the liver posthumously; 3d, there is no sugar in the general circulation of animals on a meat diet; 4th, on starchy food after digestion is completed there is no sugar in portal blood; and 5th, if, as Figuier says, the liver actually converts the portal substance into sugar, this is a true function.

(33.) In a third paper read at this session,† SANSON maintains that glycogenic matter, being intermediate between starch and glucose, must vary in its properties according to the stage of conversion; and that the matter which he obtained from the tissues was true glycogenic matter, as, under the influence of diastase, it was converted into sugar, from the fermentation of which he had obtained several specimens of alcohol. Bernard has confirmed what Pelouze had denied, the existence of dextrin in the blood. If dextrin and salivary diastase both exist in the blood, what more is needed for the production of glucose?

(34.) In another paper, read Sept. 7th,‡ he says (1) Pelouze's conversion of the glycogenic matter into xyloidin does not prove it to be starch, since dextrin may be thus converted; (2) his analysis fails of proving it, the formula given being that of glucose; moreover, starch and dextrin being isomeric, analysis cannot decide between them; (3) acetic acid precipitates dextrin as well as starch; (4) the glycogenic matter of the liver is chemically dextrin, like that in the blood of other organs; (5) dextrin is found in the portal blood and in that of the general circulation of animals fed exclusively on meat; (6) the dextrin in meat is spontaneously converted into sugar, when the meat is cut fine, and exposed to the air for 48 hours; (7) no necessity exists for the assumption of a glycogenic function; (8) Bernard's experiments with carrots are unfortunate,

* C. R., xlv, 139. † Ib., p. 140. ‡ Ib., p. 343.

since these vegetables contain starch, from which came the dextrin which he found in the liver.

(35.) The following results of experiments by Coze were read also at the session of Sept. 7th:* (A) Under physiological conditions: 1st, the kind of death affects the quantity of sugar in the liver; the slower the death, the smaller being the quantity; 2d, the proportion of sugar in arterial blood is to that in the liver as 1:11. (B) Under pathological conditions: 1st, under the action of morphine hydrochlorate; (*a*) the sugar in the liver more than doubles, rising from 0·59 to 1·39; (*b*) the same is true of arterial blood, the amount increasing from 0·05 to 0·11; (*c*) the proportion of liver sugar to arterial sugar remains the same, 1 : 12, and hence pulmonary oxydation is not affected; (*d*) the increase of sugar under the influence of morphine is an argument against the employment of opium in treating diabetes, and explains the want of success met with in its use; (*e*) no sugar is found in the urine. 2d, under the action of tartar-emetic; (*a*) the liver sugar remains unaltered; (*b*) it is doubled in arterial blood, being 0·10 instead of 0·05; (*c*) the proportion between them is 1 : 6 instead of 1 : 11; (*d*) the combustion of sugar in the lungs is hindered,—thus accounting for the efficacy of this substance in pneumonia;—(*e*) no sugar appears in the urine. The action of tartar-emetic upon the production and the destruction of glucose is precisely the reverse of the action of morphine.

(36.) In a paper communicated to the Academy Oct. 19th,† Bonnet contends that the liver has a true glycogenic function, nothing analogous existing elsewhere in the animal economy; that the posthumous formation of sugar is established, the facts quoted to disprove it being inconclusive physiologically, since a liver-hash is not the normal liver; that the two sugars of Figuier, one in the intestinal canal, the other in the portal vein, have no existence; that even if a non-fermentable sugar be admitted to exist in the portal blood, the fact remains that the liver in converting this into a fermentable sugar, exercises a true glycogenic function; that the economy contains fatty, albuminoid and other matters, the eléments of which by being differently grouped molecule by molecule, under certain unknown influences, may yield glycogenic matter; but that this result, performed outside the body by chemical action, does not represent a physiological fact; that any substance serving as a basis of Sanson's opinions may be in vain sought for in portal blood; that dextrin is transformed by fuming nitric acid directly into oxalic acid, without forming xyloidin; that the formula of ordinary starch is not that of dextrin, anhydrous

* C. R., xlv, 345. † Ib., p. 573.

starch and dextrin being $C_{12}H_{18}O_9$, while glycogenic matter is $C_6H_{12}O_6$; that the latter resembles hydrated starch rather than dextrin, since like starch, it forms a paste with water and is colored blue by iodine, while dextrin does not form a paste and is not colored by iodine; that glycogenic matter when isolated, does not resemble dextrin; that Sanson is the only chemist who has found dextrin in the blood of the carnivora; that he (Bonnet) has not found it himself, and does not believe in its existence; that dextrin, which is so easily transformed by ptyalin, cannot be taken as food and enter the system without change; that chopped meat does not contain sugar after exposure to the air; that when food rich in sugar is taken, or when sugar is formed from starchy food by the diastase of the intestinal juices, it must be re-converted to dextrin, according to Sanson; that no substance capable of producing this change is known; and that even admitting that dextrin exists in portal blood, its conversion into sugar by the liver is a true glycogenic function.

(37.) Moos published a paper in 1858,* in which he shows that when the pneumogastric nerves of healthy dogs who excrete 0·7 grams of liver sugar for each half kilogram of bodily weight are cut, this amount of sugar constantly diminishes, reaching even 0·1 gram, but never entirely disappears. In rabbits, too, when this nerve is severed, the production of sugar is very slight. This result he ascribes partly to the important diminution in bodily weight and in all the secretions, including the formation of sugar, which takes place on dividing these nerves; and partly to the diminished or altogether suspended nutrition. Electrical irritation of the spinal cord of frogs produces increased urination with diabetes, in a very short time. On ligating the vessels of the liver, though the amount of urine continues large, the sugar disappears. When the spinal cord of these animals is divided, the sugar formation is entirely arrested in the course of fifteen to twenty hours.

(38.) In a paper published at the same time,† KEKULÉ recommends Bernard's method as the most convenient one for the preparation of glycogen from the liver. A half-hour's boiling with strong potassic hydrate is sufficient to deprive it entirely of nitrogen. The ash, which consists of lime-salts, may be entirely removed by repeated solution either in strong acetic or in cold dilute nitric acid, and precipitation with alcohol. Kekulé confirms the views of Bernard, Hensen, and Pelouze relative to the properties of glycogen. It is white, and completely amorphous, and is colored violet or reddish-

* Verh. Med. Heidelberg, Jan., 1858; Jahresb., 1858, 563.
† Ib.; Jahresb., p. 570.

brown by iodine. Its aqueous solution is opalescent, but becomes rapidly clear on boiling with dilute sulphuric acid; though prolonged boiling is necessary to convert it into sugar. His analysis yielded the formula $ϵ_6H_{10}Θ_5$. The liver of dogs contained on an average two per cent of glycogen.

(39.) In a communication by POISSEUILLE and J. LEFORT to the Academy,† they state that in some physiological researches made by one of them, certain results were obtained, which were inexplicable except upon the assumption that glucose is destroyed in the lungs. But this is admitting the glycogenic hypothesis; and since the radical objections made to it are enough to shake the strongest faith in it, they have undertaken, before giving their assent to it, to re-examine the whole subject *ab ovo*, seeking for glucose not only in the liver, but also in other organs of vertebrate animals. The livers of both fresh and salt water fish afforded, in some instances, from 0·484 to 1·5 per cent of sugar; but the intestines, spleens, roes, ovaries and muscles yielded none. In other cases, no sugar was detected anywhere. The liver of frogs gave 0·315 to 0·632 of sugar; the viscera and muscles none. Birds gave the same result as fishes; the liver contained from 0 to 2·164 per cent of sugar, the viscera none. With mammals, the liver of hares gave none, of roe-deer 1·092, of three rabbits 1 to 1·163, of three cats ·807 to 2·305, of two dormice in the hybernant state 0·624 per cent of sugar; the other viscera and the muscular tissue gave none. Since sugar is destroyed when left in contact with decomposing animal matter, the authors infer that when a liver affords no sugar, it is because of such a change. They state that they have found sugar in the juice of the horse-flesh upon which their dogs were fed, as also in mutton, veal, beef and pork, though in minute quantity, a few milligrams for 100 grams of meat. The following experiments were made to ascertain the origin of this sugar: (A.) Dog, weight 33 kilos. fasting for 60 hours; had been fed for a month with 3 to 4 kilos. horse-meat daily. The liver yielded 1·487 per cent glucose, the lymph of the thoracic duct 0·141, hepatic blood 0·821, portal, carotid, inferior vena caval blood, small intestines, mesenteric ganglions, none; blood of right ventricle, traces; of left ventricle, lungs, spleen, kidneys, brain, urine, muscle, none. None of these fluids contained dextrin; the same is true of the organs, except the liver,—the decoction of which was slightly reddened by iodine,—and the muscles, in which the coloration was more decided. Without much doubt the portal blood, though it contains no sugar, yet contains a substance which is transformed into sugar; but it

† C. R., xlvi, 565, March 22.

is in the liver that this change is effected. The sugar found in the lymph must come from the intestines or the liver; but since the former contain none, it evidently must be taken up by the lymphatics from the liver-tissue. (D.) Horse in full digestion; had eaten 10 liters of oats a day for the two previous days, and on the day of the experiment the same quantity at two different hours. The liver furnished 2·292, hepatic blood 1·128, chyle 0·222, lymph (from the vessels of the head and neck) 0·442, carotid blood 0·069, jugular blood above the ligature 0·050, portal blood above the ligature, 0·065, inferior vena cava below the ligature 0·057 per cent of sugar; mucus of the small intestine, the intestine itself, muscular tissue of the heart, locomotor muscles, traces; pancreas, doubtful; mesenteric ganglions, spleen, kidneys, brain, lungs and urine, none; synovia from the knee joint 0·142 per cent. As before, sugar exists in the liver, in hepatic blood, and in the contents of the thoracic duct, but now also in arterial and portal blood, the intestinal walls, etc. Moreover, since the quantity of sugar found in the lymph in these experiments is 0·166 per cent for the dog, and 0·442 for the horse, while the quantity found in a lymphatic of the mesentery of a cow by the author of intestinal glycogeny is only 0·186 per cent, the conclusion is easily reached that it is the lymph and not the chyle which furnishes the sugar, and hence, that this is not produced in the intestine. To sum up, the liver of fishes, reptiles, birds and mammals, examined immediately after death, always contains sugar, while its presence in other organs is accidental and temporary, due to special physiological conditions; consequently, "le foie seul forme du sucre."

(40.) In a subsequent paper,* these authors give some results,—obtained at Alfort,—of the examination of the intestinal chyle and the lymph of other parts of the body, of a bull killed while in full digestion. The chyle was collected from a large mesenteric vessel coming from the intestine; and 25 grams of lymph were obtained from a lymphatic vessel near the primitive carotid artery; from this artery itself 300 grams of blood were taken. These liquids were prepared the same day and examined on the next. In the arterial blood 0·073 per cent of sugar was found; in the chyle from the intestinal vessel, 0·123; in lymph taken from the neck, 0·266. The lymph, therefore, contains more sugar than the chyle; the reverse of this should be true, however, were glucose formed in the intestinal canal. It is clear, too, that during digestion all the sugar yielded by the liver is not destroyed in the lungs; but that a portion passes into the arterial blood and thus into

* C. R., xlvi, 677, Apr. 5, 1858.

all the organs of the body; whence it is at length absorbed by the lymphatics, and poured, partly into the right subclavian vein directly, partly into the left subclavian by means of the thoracic duct. The lymphatics thus supply sugar to the superior vena cava, as the hepatic veins do to the inferior. In the carnivora, this action is temporary, taking place only during digestion; in the herbivora, since they are always in digestion, it is permanent.

(41.) In a report to the Paris Academy of Medicine upon the formation of glycogen in the animal organism,* POGGIALE states as the result of his experiments, that the preparation of glycogen—which in its properties stands between starch and dextrin—by means of glacial acetic acid, is to be preferred to any other method. As contained in a concentrated decoction of the liver, or in muscular juice, this substance, after gently heating with saliva, passes readily into fermentation, with the evolution of carbonic acid. With animals fed exclusively on meat, glycogen is found only in the liver. It exists abundantly in the liver of the herbivora, but occurs in other organs only when the food is rich in amylaceous matters.

(42.) Dr. F. W. PAVY, in a communication to the Royal Society, June 17, 1858,† claims that the question under discussion is not whether sugar is found in the animal organism independently of saccharine food; for this he is ready to grant. But it is whether the sugar thus found *after death,* is really present during life, or is the result of a *post-mortem* change. As early as Feb., 1854, he had noticed that scarcely a trace of sugar existed in blood removed by catheterizing the right ventricle during life. Sometime afterward, on repeating this experiment, his attention was fixed upon this point, and an investigation was undertaken to elucidate it. As the result of more than 60 observations, he now asserts that the condition of the blood after death cannot be taken as an indication of its state in life. He finds that in blood drawn from the right ventricle of an animal in a tranquil state, hardly a trace of sugar can be detected; while in blood collected from a fine incision in this organ immediately after death, sugar is present. In one experiment barely a trace existed in the blood during life, but nearly 1 per cent was found in that collected after death. It was natural to pass next to the organ in which, as is alleged, sugar is produced. As Bernard has shown that a substance readily convertible into sugar exists in the liver, it was necessary first to search for some substance which, when introduced into this organ, would check this post-mortem transformation. Potassic hydrate was found to answer this

* J. pharm., III, xxxiv, 99; Jahresb., 1858, 569. † Proc. Roy. Soc., ix, 300.

purpose and to exert no injurious effect; instantly after death therefore, a strong potash solution was injected into the liver by the portal vein. In this liver no sugar was detected; though another liver not thus treated, gave in a short time the ordinary sugar-reaction. If one portion of a liver be thus injected, it shows no sugar, while a second portion not thus treated is found to be saccharine. Abstraction of heat from the liver, immediately after death, yields the same result. In one case, a dog was killed, a piece of the liver instantly sliced off and thrown into a freezing mixture of ice and salt; the absence of sugar in this portion was almost complete; while the rest of the organs, which remained a short time in the animal, afforded 2·96 per cent of sugar. Division of the spinal cord, as already noticed by Bernard, suspends the production of sugar. This fact Pavy explains by showing that the body is thus cooled down so low—to 70° F.—that the post-mortem change does not go on with sufficient rapidity to afford sugar enough for detection. After a longer time, however, the liver becomes strongly saccharine. This view is sustained by the fact that if the temperature of the animal after section of the spinal cord, be artificially maintained, the ordinary amount of sugar is found in the liver; and also by the farther fact that with rabbits whose coats are oiled, so that on exposure to cold their temperature falls, the same result takes place and sugar is no longer found. With frogs, the amount of sugar in the liver is dependent upon their temperature when killed; a fact noticed by both Pavy and Bernard about the same time. As already stated, Bernard explains this fact by the diminished activity of the abdominal circulation; Pavy considers that no sugar appears because the post-mortem change is prevented by this diminution of temperature. Hence, Bernard's name, "glycogenic matter," implies a conversion not physiologically true; Pavy proposes therefore to call this substance "Hepatine." He shows that the amount of hepatine is greater on a vegetable than an animal diet, that it increases when sugar is mixed with the food, and that it causes the liver to vary in relative weight. The liver of 11 dogs fed on meat weighed $\frac{1}{37}$ that of the animal, the percentage of hepatine being 6·97; that of five dogs on vegetable food, weighed $\frac{1}{15}$ that of the animal, and contained 17·23 per cent of hepatine; while that of four dogs fed on meat and given $\frac{1}{4}$ lb. cane sugar daily, was $\frac{2}{33}$ that of the animal, the hepatine being 14·5 per cent. What the destination of hepatine is, and by what means it resists transformation during life, are questions which the author leaves for future researches to determine. Possibly some analogy to the latter condition is found in the fact that saliva, which readily converts neutral hepatine into sugar, is entirely without action if it be either acid or alkaline. The trace of sugar (0·047 to 0·073 per cent) found in right ventricular blood, is caused, as Pavy supposes, by the escape of a small amount of hepatine from the liver; since any disturbance in the circulation, either by congestion or otherwise, causes this amount of sugar to increase. Moreover, on artificially introducing hepatine into the blood, this fluid becomes saccharine; and if sufficient be injected, strongly marked diabetic urine is voided. If an animal be killed, and the circulation be maintained by artificial respiration, the urine becomes strongly saccharine by the sugar produced by the post-mortem transformation of hepatine.

(43.) On the 11th of July, 1859, Bernard presented to the Academy* a letter from C. Schmidt of Dorpat communicating the results of his examination of the portal and hepatic blood of three dogs, two of which were digesting meat, the other had fasted for two hours. The portal blood contained no sugar in either case; the hepatic nearly one per cent of the dry residue, for the first two dogs, and about ½ per cent for the third. Or more exactly, 0·93 and 0·99 for the dogs in digestion, 0·51 per cent for the fasting animal; thus confirming Lehmann's results.

(44.) In a paper read on the 1st of August,† Berthelot and de Luca give the results of a research to determine the nature of the glucose yielded by the transformation of Bernard's glycogenic matter. It has never been shown whether this sugar is identical with any of the known varieties, such as grape-sugar, malt-sugar, levulose, lactic glucose, etc., or whether it is a new one. Their glucose was obtained by the action of chlorhydric acid upon the glycogenic matter from the liver of a rabbit. They obtained in a well crystallized form, the compound of this glucose and sodic chlorid, and subjected it to a systematic examination. The compound forms bulky crystals, limpid and colorless, capable of reducing the copper-tests, and of fermenting with yeast. They are rhombohedrons of 78°, and their solution rotates to the right +47°, this rotatory power being considerably greater a few minutes after the crystals are dissolved, than subsequently. On analysis they gave 8·3 per cent chlorine, which corresponds to the formula $(\text{C}_6\text{H}_{12}\text{O}_6)_2\text{H}_2\text{O}+\text{NaCl}$. They agree in all their properties with those of the compound of grape-sugar and sodic chlorid, as first described by Peligot and Pasteur. It is therefore certain that hepatic glucose is identical with the ordinary glucose of grapes and of diabetes.

* C. R., xlix, 63. † Ib, p. 213.

(To be continued.)

ART. V.—*Derivative Hypothesis of Life and Species;* by Professor OWEN, F.R.S.*

§ 422. *Biological Questions of* 1830.—At the close of my studies at the Jardin des Plantes, Paris, in 1831, I returned strongly moved to lines of research bearing upon the then prevailing phases of thought on some general biological questions.

The great Master in whose dissecting-rooms, as well as in the public galleries of Comparative Anatomy, I was privileged to work, held that 'species were not permanent:' and taught this great and fruitful truth, not doubtfully or hypothetically, but as a fact established inductively on a wide and well-laid basis of observation, by which, indeed, among other acquisitions to science, Comparative Osteology had been created. Camper† and Hunter‡ suspected that species might be transitory; but Cuvier, in defining the characters of his *Anoplotherium* and *Palæotherium*, &c., proved the fact.

In this truly scientific labor the law of the subordination of the different organic characters to the condition of the whole animal was first appreciated, clearly enunciated, and its application shown to the reconstruction of lost species from fragmentary remains. The importance of this generalization may be paralleled with that of the principle of equivalents in chemical science.

Of the relation of past to present species, and the conditions of their succession, Cuvier had not an adequate basis for a decided opinion. Observation of changes in the relative position of land and sea suggested to him one condition of the advent of new species on an island or continent where old species had died out. This view he illustrates by a hypothetical case of such succession,§ but expressly states:—'Je ne prétends pas qu'il ait fallu une création nouvelle pour produire les espèces aujourd'hui existantes, je dis seulement qu'elles n'existoient pas dans les mêmes lieux, et qu'elles ont dû y venir d'ailleurs.'‖

Geoffroy Saint-Hilaire, whose discussions with his colleague in the 'Académie des Sciences' made its annals of 1830 meme-

* As there has been much discussion with regard to the relation of Prof. Owen's views on the origin of species to those of Dr. Darwin, we reprint this chapter (the 40th) from the forthcoming edition of his Anatomy of Vertebrates, from a pamphlet sent us by the author, omitting four of the longer notes.—EDS

† CCXCII''. ‡ CCXCIII'', and other authors cited in CXXXIX, p. xlv.

§ CXXXIX, tom. i, p. lxiii. ‖ Ib.

(Nos. denoting 'Works' in the 'Lists of Authors cited' in 'Anatomy of Vertebrates,' Vols. I, II, and III.)

rable, equally rejecting the idea of new creations,* opposed to Cuvier's inductive treatment of the question the following expression of belief:—Je ne doute pas que les animaux vivants aujourd'hui ne proviennent, par une suite de générations, et sans interruption, des animaux perdus du monde antédiluvien.'† But with regard to the demonstration of the proposition, of the truth of which he could not entertain a doubt, Geoffroy Saint-Hilaire expressly states:—'Je crois que les temps d'un savoir véritablement satisfaisant en géologie ne sont pas encore venus.'

The main collateral questions argued in these debates, to some of which I listened, and to all the reports and consequent pamphlets relating thereto devoted intense attention, appeared to me to be the following:—

Unity of Plan or Final purpose, as a governing condition of organic development?

Series of Species, uninterrupted or broken by intervals?

Extinction, cataclysmal or regulated?

Development, by epigenesis or evolution?

Primary life, by miracle or secondary law?

On returning home and resuming office with additional duties at the Royal College of Surgeons, I was guided in all my work with the hope or endeavor to gain inductive ground for conclusions on these great questions.

§ 423. *Homology or Teleology?*—Cuvier held the work of organization to be guided and governed by final purpose, or adaptation, expounding this principle under the terms 'conditions of existence' and 'correlations of structure.' Geoffroy denied the evidence of design, and protested against the deduction of a purpose as, e. g., from the coëxistence of a valve with a definite course of fluid: he contended for the principle which he called 'unité de composition,' as the law of organization. Most of his illustrations were open to the demonstration of inaccuracy, and his arguments to the refutation which they received from Cuvier in the debates in question: the logic, and, as it seemed, the facts, were on the side of teleology. The figurative language, moreover, in which contemporary anatomists had expressed their views of a principle akin to Geoffroy's was ill-calculated to enlist supporters. The expressions by which disciples of the school of Schelling illustrated, in the animal structures, the transcendental idea of 'the repetition of

* 'Or, cette proposition, déja contraire aux plus anciennes données historiques, répugne tout autant aux lumières de la raison naturelle qu'aux spéculations plus réfléchies des sciences physiques.'—CCLXXXVII'', p. 210.

† Also, more decisively:—'Les animaux perdus sont, par voie non interrompue de générations et de modifications successives, les ancêtres des animaux du monde actuel.'—CCLXXXVII'', p. 208.

the whole in every part,' operated disadvantageously to the calm inquiry into the prime question at issue. To Cuvier this language seemed little better than mystical jargon, and he alluded to it with transparent contempt.* When he did extend inferences from comparative anatomy beyond the adaptation of structure to function, Cuvier went not beyond a recognition of what I have since termed 'special homologies:'† and this lowest degree of correspondence he explained on the ground of the subserviency of such homologous parts to similar ends in different animals;‡ viewing them, in fact, in that relation which I express and contrast by the term 'analogies.'§ With Cuvier answerable parts occurred in the zoological scale because they had to perform similar functions.

Most of my fellow-students at the Garden of Plants, in 1830, and some subsequent fellow-laborers, Johannes Müller, Rud. Wagner, Milne-Edwards, Agassiz, implicitly accepted this explanation of the fact of answerable bones and other parts occurring in different species.

After the publication of the 'Memoir on the Pearly Nautilus,' and of those on Monotrematous and Marsupial generation, which subjects Cuvier had strongly recommended to my attention, the question of the condition or law of special homologies pressed itself upon me, more especially in connection with the task of arranging and cataloguing the osteological part of the Hunterian Museum.‖ As my observations and comparisons accumulated, with *pari passu* tests of observed phenomena of osteogeny, they enforced a reconsideration of Cuvier's conclusions to which I had previously yielded assent. To demonstrate the evidence of the community of organization, I found that the artifice of an archetype vertebrate animal was as essential as that of the archetype plant had been to Goethe in expressing analogous ideas; and as the like reference to an 'ideal type' must be to all who undertake to make intelligible the 'unity in variety' pervading any group of organisms.¶ From the demon-

* 'Quant à M. Oken, il déclare les pièces en question les parties écailleuses des temporaux, ou, selon son langage mystique, " la fourchette du membre supérieur de la tête."—Cet humérous de la tête de M. Oken devient pour M. Spix le pubis de cette même tête; ou, pour parler un langage intelligible, un des osselets de l'ouie, savoir le marteau.'—CXXXIX, tom. v, 2e partie, p. 85.

† CXL, p. 7.

‡ 'Ce n'est qu'un principe subordonné à un autre bien plus élevé et bien plus fécond, à celui des conditions d'existence, de la convenance des parties, de leur coördination pour le rôle que l'animal doit jouer dans la nature. Voilà le vrai principe philosophique d'où découlent la possibilité de certaines ressemblances.'—CCXCIV'', p. 9.

§ CXL, p. 7.

‖ XLIV.

¶ Such 'ideal type' must not be confounded with the so-called 'types' suppose to be exemplified by certain living species. Arguments against the latter vague and ill-defined ideas are of no weight against the former, and indicate a certain obtuseness of apprehension in the objector. See CCCXXVI'', p. 31.

stration of this principle, which I then satisfied myself was associated with and dominated by that of 'adaptation to purpose,' the step was plain—to me inevitable—to the conception of the operation of a secondary cause of the entire series of species, whether of plants, or vertebrates, or other groups of organisms, such cause being the servant of predetermining intelligent Will.*

But, besides 'derivation' or 'filiation,' another principle influencing organization became recognizable in the course of studies and researches on Invertebrate animals. To this principle, as more especially antagonistic to the theological idea, I gave the name of 'irrelative repetition;' sometimes also, as it prevailed most in plants and zoophytes, of 'vegetative repetition.'† The demonstrated constitution of the vertebrate endoskeleton, as a series of essentially similar segments, out of which, as corollary, came the power of enunciating not only 'special' but 'general' and 'serial' homologies, appeared to me to illustrate also the law of irrelative repetition. The recurrence of similar segments in the spinal column and of similar elements in a vertebral segment, struck me as analogous to the repetition of similar crystals as the result of polarizing force in the growth of an inorganic body.‡

Accordingly, these results of extensive, patient, and unbiassed inductive research—or, if there were a bias, it was toward Cuvier—swayed with me in rejecting the principle of direct or miraculous creation, and in recognizing a 'natural law or secondary cause' as operative in the production of species 'in orderly succession and progression.'§

§ 424. *Succession of Species, broken or linked?*—To the hypothesis that existing are modifications of extinct species Cuvier replied, that, in every mooted form of transmutation, the species were made to alter by small degrees, and that, therefore, traces of such gradual modifications were due from the fossil world:—'You ought,' he said, 'to be able to show, e. g., the intermediate forms between the Palæotherium and existing hoofed quadrupeds.'‖

The progress of Paleontology since 1830 has brought to light many missing links unknown to the founder of the science. My own share in the labor led me, after a few years' research,

* CXLI, (1849) p. 86.

† CCXLIX, p. 641 (1843;) and vol. i, Preface, p. ix.

‡ CXL, p. 171.

§ CXLI, loc. cit

‖ 'Cependant on peut leur répondre, dans leur propre système, que si les espèces ont changé par degrés, on devroit trouver des traces de ces modifications graduelles; qu'entre la palæotherium et les espèces d'aujourd'hui l'on devroit découvrir quelques formes intermédiaires, et que jusqu'à présent cela n'est point arrivé.'—CXXXIX, tom. i, p. lvii.

to discern what I believed, and still hold, to be a tendency to a more generalized or less specialized, organization as species recede in date of existence from the present time.* Even instances which to some have appeared to oppose the rule, really exemplify it. The little marsupial carnivore, e. g., of the Purbeck beds, *Plagiaulax* (p. 294, fig. 234,) retained the typical number of premolars (p. 1–4,) all of them being carnassials: the more modified pliocene *Thylacoleo* had them reduced to the last (p. 4, fig. 233.) So likewise in the latter placental *Carnivora*, the eocene form *Hyænodon*, fig. 266, had the typical number of teeth, the three true molars here showing the carnassial form: in the existing Hyæna and Felines the carnassials are reduced to, or concentrated in, a single molar. The oolitic *Phascolotherium*, with the typical marsupial number of teeth, shows less differentiation in their form than in modern Opossums and Dasyures: the oolitic *Amphitheria* and *Spalacotheria* manifest an earlier and more generalized type of dentition in the great number and similarity of character of their small molars. Both *Anoplotherium* and *Palæotherium*, with the majority of eocene placental Mammals, had the type-dentition of diphyodonts.†

The two notable examples of Cuvier's powers of restoration, viewed as Pachyderms, must have seemed widely different from any of the existing species of the order, and were so deemed. The *Anoplotherium* more especially, among its singular peculiarities, unexpectedly exemplified one dental character, previously known only in the human subject. These seeming anomalies, however, lost much of their import as evidence of insulated form, or special creation, when they came to be viewed by the light of the law of the 'more generalized character of extinct species.' Such law in its application to *Anoplotherium* also exemplifies the analogy between the earlier species of a class and the earlier stages of a fœtus. When, for example, the divided metapodials, the persistent upper incisors, and the hornless cranium of the Anoplothere were recognized as retentions of 'fœtal peculiarities' of existing ruminants,‡ that extinct species was seen to favor rather than oppose the idea of organization by secondary law.

* CCXLIX, Ed. 1843, pp. 129, 165; Ed. 1855, pp. 223, 332, 342. CLXXX, and XVII', pp. 1, 361, *passim*. Agassiz had been struck by indications of the same law in fossil fishes, and expressed it by the analogy of fœtal and mature structures (CCCXXIX'', (1844) p. xxvi), and this, in some degree, is true. The earlier forms of *Mammalia*, however, are not toothless, have rather an excess of teeth as compared with later and modern forms; but they exemplify, in the main, a more 'generalized' type.

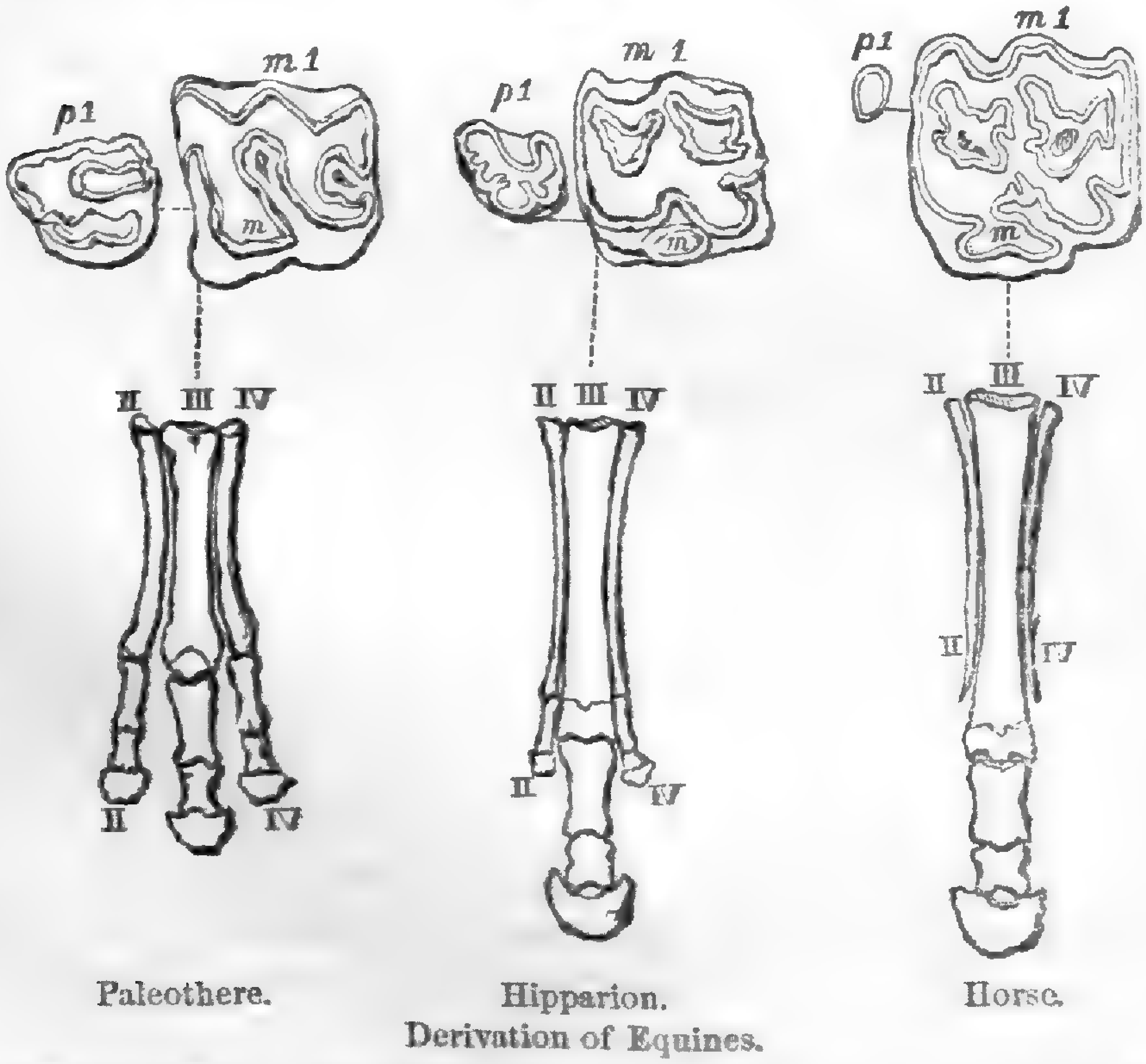
† v. p. 524, CLXXX, p. 361.

‡ CLXXX. p. 367.

The discovery of the remains of the *Hipparion** supplied one of the links required by Cuvier, between the *Palæotherium* and the Horse of the present day, and it is still more significant of the fact of filiation of species that the remains of such three-toed horses are found only in deposits of that tertiary period which intervene between the older palæotherian one and the newer strata in which the modern Horse first appears to have lost its lateral hooflets. These relations I illustrated in my Lectures on Fossil Mammalia at the School of Mines (1857) by the diagram, fig. 614.

Other evidences of gradation, in the case in question, have been brought to light. The molar series of the Horse includes six large complex grinders, individually recognizable by developmental characters as they are symbolized in fig. 280, p. 352. The representative of the first premolar is minute and soon shed. Its homologue in *Palæotherium* is functionally developed and retained, the type-dentition being adhered to.† In *Hipparion*, d_1 is succeeded by a p_1‡ smaller than in *Palæotherium*, but functional, with inflected folds of enamel on the grinding surface,

614.



Paleothere. Hipparion. Horse.

Derivation of Equines.

* cccIII″, tom. ii. p. 25 (1832). Another species was discovered in the Miocene at Eppelsheim—the '*Hippotherium*,' of Kaup; a third in deposits of similar age on the Sewalik Hills; a fourth, *Hipparion prostylum* Gv., at Vaucluse, in the southeast of France, in deposits 'peut-être plus récents que la molasse dans ces localités.'—cccxxx″, p 432.

† v. Pl. 35, figs. 4, 5, 6.

‡ cccII″, Pl. 19, figs. 1, 1a.

and permanent. It exemplifies a condition intermediate to that in *Palæotherium* and *Equus*. It is not that the jaws of the Horse are too short to hold the full complement of grinders; on the contrary they are relatively longer than in the Palæothere, being specially produced between the grinders and cutters: the first grinder might seem, indeed, to have been taken away in order to add to the space for the application of the 'bit.' The transitory and singularly small and simple denticle, fig. 614, p_1, compared with the large contiguous massive molar, m_1, in the Horse, exemplifies the rudiment of an ancestral structure, in the same degree as do the hoofless 'splint-bones,' ib., *Equus*, II, IV: just as the spurious hoofs dangling therefrom in *Hipparion*, ib. II, IV, are retained rudiments of the functionally developed lateral hoofs in the broader foot of *Palæotherium*, ib. II, IV.

Other missing links of this series of species have been supplied; as, e. g., by the *Paloplotherium** of the newer eocene of Hordwell, Hants., by the *Palæotherium aurelinense* from the 'molasse marine' of Orleans,† and by the *Palæotherium hippoïdes* of the lacustrine calcareous beds of Sansan, all which deposits are miocene, or are transitional between eocene and miocene. In the first-cited example, the swollen termination of the lobe of the molar, answering to *c*, *m*, fig. 268, remains longer as a detached column, *m*, fig. 269. In the two other Palæotherioids, the whole foot is longer and more slender, with a longer and thicker middle toe, than in the older eocene type-genus, whence the generic name *Anchitherium* applied to them by von Meyer.‡ It is interesting, also, to find that the transitional character is further marked by the smaller relative size of the first premolar, whereby *Anchitherium* intervenes, as in the modification of the feet, between the *Palæotherium* and *Hipparion*.

Thus amply and satisfactorily has been fulfilled Cuvier's requisition of 1821:—'Entre le palæotherium et les espèces d'aujourd'hui l'on devrait decouvrir quelques formes intermédiaries.' How, then, is the origin of these intermediate gradations to be interpreted? One may first remark, that as *Palæotherium*, *Paloplotherium*, *Anchitherium*, *Hipparion* and *Equus*, differ from each other in a greater degree than do the Horse, Zebra, and Ass, the difficulty of interbreeding would be greater, and the probability of fertility less, supposing those extinct

* This modification, as the *Palæotherium ovinum* Aymard, began to be shown at the upper eocene at Velay, e. g., ere *Palæotherium* proper had passed away. (Bulletin du Congrès Scientifique de France tenu à Puy, 1855.)

† Also in the upper eocene of the Basin of the Garonne, with *Acerotherium*.

‡ *Anchitherium* occurs, also, in the 'marine-molasse,' or lower miocene, of St. Genies, Languedoc.

genera to have co-existed. One cannot doubt, also, that every well-marked species of these genera paired within itself, and that they exemplified respectively the character of a 'group of individuals descended from common parents, or from such as resembled them as closely as they resembled each other.' They did not, however, exist as species, during the same periods of time, far less so 'from the beginning of things.' The single-hoofed Horse-family cannot be traced further back than the pliocene tertiary period: the tridactyle equine species have not been found in strata earlier than miocene, and disappear in the upper eocene: the heavier-bodied shorter-legged species with three functional hoofs to each foot belong to upper and middle eocenes. Furthermore, in the oldest eocene (London clay, super-cretaceous Conglomerates and Plastic clay at Meudon, Paris,) we get evidence of Ungulates (*Pliolophus*, *Hyracotherium*, *Coryphodon*,) in which the perisso- and artio-dactyle characters were less differentiated than in *Palæotherium* and *Anoplotherium*, affording additional significant evidence of progressive departure from generalized type. Thus, the succession in time accords with the gradational modifications by which *Palæotherium* is linked on to *Equus*.

With this additional knowledge the question, 'whether actual races may not be modifications of those ancient races which are exemplified by fossil remains?' presents itself under very different conditions from those under which it passed before the minds of Cuvier* and the Academicians of 1830. If the alternative—species by miracle or by law?—be applied to *Palæotherium*, *Paloplotherium*, *Anchitherium*, *Hipparion*, *Equus*, I accept the latter, without misgiving, and recognize such law as continuously operative throughout tertiary time.

In respect to its mode of operation, we may suppose Lamarck to say, 'as the surface of the earth consolidated, the larger and more produced mid-hoof of the old three-toed Pachyderms took a greater share in sustaining the animal's weight; and, more blood being required to meet the greater demand of the more active middle-toe, it grew; whilst the side-toes, losing their share of nourishment and becoming more and more withdrawn from use, shrank;' and so on, according to the hardening of the ground, until only the hidden rudiments of metapodials remained and one hoof became maximized for all the work. Mr. Darwin, I conceive, would modify this, like other Lamarckian instances, by saying that some individuals of *Palæotherium* happening to be born with a larger and longer middle-toe, and with shorter and smaller side-toes, such variety was better

* Pourquoi les races actuelles, me dirait-on, ne seraient-elles pas des modifications de ces races anciennes que l'on trouve parmi les fossiles?—CXXXIX, i, p. lvii.

adapted to prevailing altered conditions of the earth's surface than the parental form; and so on, until finally the extreme equine modifications of foot came to be 'naturally selected.' But the hypotheses of appetency and volition, as of natural selection, are less applicable, less intelligible, in connection with the changes in the structure and proportion of the molar series of teeth, which we have seen also to be gradational from *Palæotherium* to *Equus*, fig. 614.

Any modification of Geoffroy's 'ambient medium,' affecting the density of the soil might so far relate to the changes of limb-structure, as that a foot with a pair of small hoofs dangling by the sides of the large one, like those behind the cloven hoof of the ox, would cause the foot of the Hipparion, e. g., and *à fortiori* the broader based three-hoofed foot of the Palæothere, to sink less deeply into swampy soil, and be more easily withdrawn, than the more concentratively simplified and specialized foot of the Horse.*

Rhinoceroses and Zebras, however, tread together the arid plains of Africa in the present day: and the Horse has multiplied in that half of America where two or more kinds of Tapir still exist. That the continents of the eocene or miocene periods were less diversified in respect of swamp and sward, pampas or desert, than those of the pliocene period, has no support from observation or analogy.

Assuming, then, that *Palæotherium* did ultimately become *Equus*, I gain no conception of the operation of the effective force by personifying as 'Nature' the aggregate of beings which compose the universe, or the laws which govern these beings, by giving to my personification an attribute which can properly be predicated only of intelligence, and by saying, 'Nature has selected the mid-hoof and rejected the others.'

As some paragraphs in my 'Preface' have been misconceived,† I must further observe, to put my meaning beyond doubt, that, to say that *Palæotherium* has graduated into *Equus* by 'Natural Selection' is an explanation of the process of the same kind and value as that which has been proffered of the mystery of 'secretion.' For example, a particular mass of matter in a living animal takes certain elements out of the blood and rejects them as 'bile.' Attributes were given to the liver which can only be predicated of the whole animal: the 'appetency' of the liver, it was said, was for the elements of

* XVII′, p. 397.

† Referring to my 'Anatomy of Vertebrates,' in the fourth edition of the 'Origin of species by Natural Selection,' &c., the author asserts that 'he' (Professor Owen) 'at the same time admits that Natural Selection may have done something toward this end.' Mr. Darwin does not quote the passage or refer to the page on which he founds his assertion.—CCXIII″ (1866), Histor. Pref., p. xviii.

bile, and 'biliosity' or the 'hepatic sensation' guided the gland to their selection.*

Such figurative language, I need not say, explains absolutely nothing of the nature of bilification. One's surprise is that 'tropes' and 'personified acts' should not have died out, as explanatory devices, with the 'archeus faber,' the 'nisus formativus,' and other self-deceiving, world-beguiling simulacra of science, with the last century; and that a resuscitation should have had any success in the present. It is of interest, mainly, as illustrating the 'alternation of generations.'

What, then, are the facts on which any reasonable or intelligible conception may be formed of the mode of operation of the derivative law exemplified in the series linking on *Palæotherium* to *Equus?* A very significant one is the following:— A modern horse occasionally comes into the world with the supplementary ancestral hoofs. From Valerius Maximus,† who attributes the variety to *Bucephalus,* downward, such 'polydactyle' horses have been noted as monsters and marvels. In one of the latest examples,‡ the inner splint-bone, answering to the second metacarpal of the pentadactyle foot, supported phalanges and a terminal hoof, in position and proportion to the middle hoof, resembling the corresponding one in *Hipparion,* fig. 614, II.

In relation to actual horses such specimens figure as 'monstra per excessum;'§ but, in relation to miocene horses, they would be normal, and those of the present day would exemplify 'monstra per defectum.' The mother of a 'monstrous' tridactyle colt might repeat the anomaly and bring forth a tridactyle 'filly'; just as, at San Salvador, the parents of a family of six had two of the series born with defective brain and of dwarf size: they were 'male' and 'female;' and these strange little idiots are exhibited as 'Aztecs.' The pairing of the horses with the metapodials bearing, according to type, phalanges and hoofs, might restore the race of hipparions.

Now, the fact suggesting such possibility teaches that the change would be sudden and considerable: it opposes the idea that species are transmuted by minute and slow degrees. It

* CCCXXVIII″, vol. i, p. 268, and *passim.*

† 'Exemplorum memorabilium Libri novem, &c. (De rebus mirificis.)'

‡ CCCIV″, p. 55, Pl. 1.

§ Two such examples are described in LII, vol. ii, and one in CCCV″, p. 224, in which the left fore-foot had three subequal hoofs, and the right fore-foot two hoofs. But the application of an instructive and rightly discerned relation may be travestied and exaggerated: the two-tailed Lizard and the double-headed snake do not reproduce to view normal ancestral forms. The essentially single mid-toe (fig. 193, iii) of the horse, occasionally bifid and terminated by a pair of ill-shapen hoofs, lends no support to the idea of the digit (iii) being homologous with the so-called cloven hoof (really the digits iii and iv, ib.) of Ruminants. It is a malformation akin to that of the partially double digit of the Dorking fowl.

also shows that a species might originate independently of the operation of any external influence; that change of structure would precede that of use and habit; that appetency, impulse, ambient medium, fortuitous fitness of surrounding circumstances, or a personified 'selecting Nature,' would have had no share in the transmutative act.

There is, however, one relation which I cannot shut out, for I hold it as strongly as when I explained it, and endeavored to impress it upon the audience at my lectures of 1857: it is the fitness of the organization of the Horse and Ass for the needs of mankind, and the coincidence of the origin of Ungulates having equine modifications of the perissodactyle structure with the period immediately preceding, or coincident with, the earliest evidence of the Human Race.

Of all the quadrupedal servants of Man none have proved of more value to him, in peace or war, than the horse: none have coöperated with the advanced races more influentially in Man's destined mastery over the earth and its lower denizens. In all the modifications of the old palæotherian type to this end, the horse has acquired nobler proportions and higher faculties, more strength, more speed, with amenability to bit. No one can enter the 'saddling ground' at Epsom, before the start for the 'Derby,' without feeling that the glossy-coated, proudly-stepping creatures led out before him are the most perfect and beautiful of quadrupeds. As such, I believe the Horse to have been predestined and prepared for Man. It may be weakness; but if so, it is a glorious one, to discern, however dimly, across our finite prison-wall, evidence of the 'Divinity that shapes our ends,' abuse the means as we may.

Thus, at the acquisition of facts adequate to test the moot question of links between past and present species, as at the close of that other series of researches proving the 'skeleton of all Vertebrates, and even of Man, to be the harmonized sum of a series of essentially similar segments,'* I have been led to recognize species as exemplifying the continuous operation of natural law, or secondary cause; and that, not only successively, but progressively; from the first embodiment of the Vertebrate idea under its old Ichthyic vestment until it became arrayed in the glorious garb of the Human form.'†

* CXLI, p. 119.

† Ib., p. 86. Even in his partial quotation from my work of 1849, the author of CCXIII'' (4th Ed. 1866) might have seen ground for apologizing for his preposterous assertion, in 1859:—that 'Professor Owen maintained. often vehemently, the immutability of species' (p. 310), and for the question, as preposterous and unworthy: 'Does he really believe that at innumerable periods in the earth's history elemental atoms have been commanded suddenly to flash into living tissues?' (Ib., Ed. 1859, p. 483. In the Ed. of 1860, p. 111, the imputation is tacitly abandoned). The significance of the concluding paragraphs of CXLI was plain enough to BADEN POWELL, CCCXXXIII'', p. 401 (1855), and drew down on me the hard epithets with which Theology usually assails the inbringer of unwelcome light, CII', p. 61.

The series of observations on the Ungulate group of Mammals yields insight, as above explained, into the mode of operation of the secondary law; and gives evidence of the amount of geological time intervening between the introduction and disappearance of generic and subgeneric modifications. According to the analogy of the mammalian *Hipparion* and *Equus*, we may expect the corresponding precedent form of the Papuan of the well-wooded and richly fruited islands representing a departed tropical or subtropical continent, to be exemplified by fossils in formations not earlier than middle tertiary. All species coëxisting with the actual specific form of *Homo* will, with him, be immutable, or mutable only as he may be. To name such species, after comparing and determining their specific characters, will continue to be the Zoologist's staple task as long as his own specific intellectual character remains unchanged (Pref., p. XXXVI). To suppose that coëxisting differentiations and specializations, such as *Equus* and *Rhinoceros*, or either of these and *Tapirus*, which have diverged to generic distinctions from an antecedent common form, to be transmutable one into another, would be as unscientific, not to say absurd, as the idea, which has been bolstered up by so many questionable illustrations, and foisted upon poor 'working men,' of their derivation from a Gorilla!

§ 425. *Extinction, cataclysmal or regulated?*—If, in place of recognizing the series of the above-cited Perissodactyles as evidencing (preordained) departures from parental type, probably sudden and seemingly monstrous, but adapting the progeny inheriting such modifications to higher purposes, the theological notion be retained, and the species of Palæothere, Paloplothere, Anchithere, Hipparion, and Horse, be severally deemed due to remotely and successively repeated acts of direct creation, one is concomitantly led to suppose the successive going out of such species to have been as miraculous as their coming in. The destruction of one creation is the logical preordinance to a recurrence of 'genesis.' This nexus of ideas was too close not to have swayed with Cuvier: accordingly, in his famous 'Discours sur les Révolutions de la Surface du Globe,' we have a section of 'Preuves que ces Révolutions ont été nombreuses,'* and another section of 'Preuves que ces Révolutions ont été subites.'† Continued observations of Geologists, while establishing the fact of successive changes, have filled up the seeming chasms between such supposed 'revolutions,' as the discoveries of Paleontologists have supplied the links between the species held to have perished by the cataclysms. Each successive parcel of geological truth has tended to dissipate the belief in the un-

* CCCXX'', p. 5. † Ib., p. 8.

usually sudden and violent nature of the changes recognizable in the earth's surface. In specially directing my attention to this moot point, whilst engaged in investigations of fossil remains, and in the reconstruction of the species to which they belonged, I was, at length, led to recognize one cause of extinction as being due to defeat in the 'contest which as a living organized whole, the individual of each species had to maintain against the surrounding agencies which might militate against its existence.' (Pref., p. xxxiv.) This principle has received a large and most instructive accession of illustrations from the extensive knowledge and devoted labors of Charles Darwin: but he aims to apply it not only to the extinction but the origin of species.

Although I fail to recognize proof of the latter bearing of the 'battle of life,' the concurrence of so much evidence in favor of 'extinction by law' is, in like measure, corroborative of the truth of the ascription of the origin of species to a secondary cause.

§ 426. *How works the Derivative Law?*—The guesses made by those who have given the rein to the imaginative faculty in attempts to explain the mode of operation of the derivative law have mainly proved repellent to its study, and have raised the chief obstacles to its acceptance, by affording the most favorable opportunities of telling argument and caustic criticism to opponents of any recognition of such law in the abstract. Thus, De Maillet's conception of the conditions of transmutation invited Cuvier's crushing exposition of its absurdity, which fell with the full weight of his great anatomical knowledge. Lamarck gave occasion to many similar confutations, applied not always in good faith, and often by men without any anatomical or physiological qualifications for such criticism, to discredit veritable evidences of the operation of a secondary creative law. Subjoined, for example, is his hypothesis of the origin of the human species,* which, with similar illustrations from the web-footed, hoofed, and long-necked ruminant mammalia, have afforded topics of easy ridicule. So Lyell, asserting that, 'orangs had been tamed by the savages of Borneo, and made to climb lofty trees and bring down the fruit,'†

* 'Effectivement, si une race quelconque de *quadrumanes*, surtout la plus perfectionée d'entre elles, perdoit, par la nécessité des circonstances ou par quelqu'autre cause, l'habitude de grimper sur les arbres et d'en empoigner les branches avec les pieds, comme avec les mains, pour s'y accrocher; et si les individus de cette race, pendant une suite de générations, étoient forcés de ne servir de leurs pieds que pour marcher, et cessoient d'employer leurs mains commes des pieds; il n'est douteux, d'après les observations exposées dans le chapitre précédant, que les quadrumanes ne fussent à la fin transformés en *bimanes*, et que les pouces de leurs pieds ne cessassent d'être écartés des doigts, ces pieds ne leur servant plus qu'à marcher.'—CCXCVIII'', i, p. 349.

† CCC'', Ed. 1835, vol. ii, p. 463.

proceeds :—'It is for the Lamarckians to explain how it happens that these same savages of Borneo have not themselves acquired, by dint of longing, for many generations, for the power of climbing trees, the elongated arms of the orang, or even the prehensile tails of some American monkeys. Instead of being reduced to the necessity of subjugating stubborn and untractable brutes, we should naturally have anticipated "that their wants would have excited them to efforts, and that continued efforts would have given rise to new organs :" or, rather, to the reacquisition of organs, which in a manner irreconcilable with the principle of the "progressive" system, have grown obsolete in tribes of men which have such constant need of them.'*

An anatomist and physiologist competent to judge of the stable grounds of a derivative origin of species—unity of plan, geological epochs, successive species therein,—truly set forth by the great and philosophic naturalist, would have referred to him, bearing calmly and nobly an old age of blindness and poverty, in a more worthy spirit. From one destitute of qualifications for grappling with the difficulties of this profound genetic problem in physiology, silence would have been blameless. Vituperative condemnation by such a one of a given phase or an untenable ground of that problem is of no greater value than his extravagant commendation, with as little capacity for comprehending its weakness, of a subsequent attempt toward its solution.

Some of Lamarck's characteristic and assailable illustrations have indeed been adopted and further developed :—'Ceux des mammifères aquatiques qui contractèrent l'habitude de ne jamais sortir des eaux, et seulement de venir respirer à leur surface, donnèrent probablement lieu aux différens *Cetacées.* En effet, depuis l'énorme quantité de temps que ces animaux vivent dans le sein des mers, ne se servant jamais de leurs pieds postérieurs pour saisir les objets, ces pieds non employés ont tout-à-fait disparu, ainsi que leurs os, et même le bassin qui leur servoit de soutien et d'attache.† As a fact, however, so much of the pelvis has been preserved in *Cetacea* as serves to give origin to certain muscles of the genitals : and, in the mysticete whale, even a rudiment of the attached limb remains (vol. ii, fig. 159, $_{63-66}$). But beside the influence of habitual sojourn in water, Mr. Darwin adds another consideration to account for the enormous head in *Cetacea* :—'In North America the black bear was seen by Hearne swimming for hours with widely open mouth, thus catching, almost like a whale, insects in the water.‡ I see no difficulty in a race of bears

* Ib., p. 464. † CCXCVIII'', ii, p. 461. ‡ CCXIII'', p. 184, Ed. 1.

being rendered by Natural Selection, more and more aquatic in their structure and habits, with larger and larger mouths, till a creature was produced as monstrous as a whale.'*

The idea which Mr. Darwin persuades himself that he originated in addition to Lamarck's 'influence des circonstances sur les actions et les habitudes des animaux et de celle des actions et des habitudes de ces corps vivans, comme causes qui modifient leur organization et leurs parties' is most intelligibly illustrated in the paper in which he first communicated his views to the Linnæan Society. It is by 'an imaginary example from changes in progress on an island':—'Let the organization of a canine animal which preyed chiefly on rabbits, but sometimes on hares, become slightly plastic; let these same changes cause the number of rabbits very slowly to decrease, and the number of hares to increase: the effect of this would be that the fox or dog would be driven to try to catch more hares; his organization, however, being slightly plastic, those individuals with the lightest forms, longest limbs, and best eyesight, let the differences be ever so small, would be slightly favored, and would tend to live longer, and to survive during that time of the year when food was scarcest; they would also rear more young, which would tend to inherit those slight peculiarities. The less fleet ones would be rigidly destroyed. I can see no more reason to doubt that these causes in a thousand generations would produce a marked effect, and adapt the form of the fox or dog to the catching of hares instead of rabbits, than that greyhounds can be improved by selection and careful breeding.'† So Geoffroy Saint-Hilaire also wrote:—'Si ces modifications amènent des effets nuisibles, les animaux qui les éprouvent cessent d'exister, pour être remplacés par d'autres, avec des formes un peu changées, et changées à la convenance des nouvelles circonstances.'‡

The modifications on which Geoffroy Saint-Hilaire laid chief stress were those assumed to have affected the ambient medium, the mode of operation of which in the origin of species he thus exemplifies:—'Mon Mémoire, traitant de l'influence des milieux ambians pour modifier les formes animales, montre comment la quantité décroissante de l'oxygène, relativement aux autres composans de l'atmosphère, a pu forcer les surfaces cutanées des embryons, premier et principal siége des actes respiratoires, à s'ouvrir davantage, à gagner, dans une raison inverse du volume existant de l'oxygène, plus de profondeur, au moyen de plus larges anfractuosités dans le tissu cellulaire,

* This conclusion of the passage is omitted in later editions.
† CCCI, p. 49. But see the remarks on this in CLXXX, p. 434, and CII', p. 65.
‡ CCXCIX'', p. 79.

et à acquérir, par un accroissement dans l'intensité des effets, de plus en plus, le caractère d'ampoules et décidément de trachées, jusqua' à ce qu'enfin survienne dans le thorax une concentration des sinus respiratoires, et des arrangements de structure pour l'isolement des poches ou théâtres de respiration, appelés, suivant leurs qualités conditionnelles, *poumons* ou *branchies.*'—CCXCVII'', p. 82.

One should not be dealing fairly with this exposition of transmutative conditions if we were to take its terms in their literal or usual acceptation ; else, the obvious objection that embryos are shut out from the influence of the atmosphere until their lungs are prepared for it, at once suggests itself. I assume, therefore, that the term is used, metaphorically, to signify the low and early embryo-like forms of living things. But it may then be remarked that if speculation be permitted on possible changes in the constitution of the atmosphere of this planet, during past geological æons, it is more probable that the proportion of the carbonic acid has been reduced than that of the oxygen. The prevalence of remains of cold-blooded slow-breathers in palæozoic and older mesozoic strata has more than once suggested such relation to the 'ambient medium.' I repeat, however, that the sole consequence of vague generalities, or figurative impersonations, propounded to show how transmutation may go on, has been to prejudice calm and sound judgments against any acceptance of, or favor toward, the grounds of a belief in secondary creational law. I have elsewhere tested the ideas of Lamarck and Darwin as to the mode of transmutation, by reference to the species *Chiromys Madagascariensis* :* I will now apply them, together with Geoffroy's, to another and lower degree of life.

What spectacle can be more beautiful, striking and suggestive than that of the inhabitants of the calm expanse of water of an atoll, encircled by its vast ring of coral rock ! Leaving the bright-tinted Chœtodonts, the Scari with adamantine jaws, the Holothurians and other locomotive frequenters of the calcareous basin out of the question, and restricting the test to the species cemented or otherwise confined to its area : we may first ask :—

Were the elements of the coriaceous and of the softer contractile and secreting tissues of the coral-polyp suddenly combined and disposed so as to form the body-wall, inverted gastric-bag, produced tentacles, intermediate laminæ, generative plaits, vesicles and threads, with outer folds in arrangement and numbers such as to secrete the laminate calcareous polyp cell? Was the creature, so miraculously constituted, at the same

* CII'', pp. 64–66.

time endowed with generative faculties to multiply and reproduce its kind for all time; the creative act henceforth and thereafter being dispensed with? Accepting, with the theologian, this view, it must then be applied to each of the more or less closely allied species associated in the same coral workhouse. The origin of such species thus dates back to the beginning of life on the globe.* The first created coral-polyp included, potentially, the germs of its successors throughout all time.

Observation, however, shows that the species of existing *Anthozoa* cannot be traced very far back: those with a flexible, or with a branched, calcareous axis began only at the tertiary period; and, of the genera of eocene lamellate or stony corals, all the species are extinct, and have been superseded in their grand and useful operations by those now forming reefs and atolls. As we extend our researches back in time we find generic and family types of coral-polyps passing away: the prevalent pattern of stellate cups of rays of *six* or its multiples, has superseded a simpler pattern of *four* or its multiples. Of the *Cyathophyllidæ* of the palæozoic reefs which present a quadripartite character of their plaited polyp-cells, not one such species now exists, or has been observed in any formation later than lower green-sand. Moreover, the filling up of abandoned cells in the course of growth of the polypary becomes changed from a more complex to a more simple method, as we recede in time in pursuing our comparisons.†

With this generalized result of observation of reef-building polyps we return to the initial question in a frame of mind inevitably other than that in which the creation of a coral-island is pondered on by one ignorant of the geological history of the class engaged in its construction. Was direct creation, after the dying out of its result as a 'rugose coral,' repeated to constitute the succeeding and superseding 'tabulate coral'? Must we, also, invoke the miraculous power to initiate every distinct species of both *Rugosa* and *Tabulata*? These grand old groups have had their day and are utterly gone. When we endeavor to conceive or realize such mode of origin, not of them only, but of their manifold successors, the miracle, by the very multiplication of its manifestations, becomes incredible—inconsistent with any worthy conception of an all-seeing, all-provident Omnipotence! It is not above, but against, rea-

* I leave out of the question the subsequent lethal influence of the heavy and continuous rain added to the ocean in order to raise it above the highest mountains, according to the biblical flood.

† CLXXX, pp. 23–28.

son ; and I may assume the special primary creative hypothesis of the successive and coexisting species of *Anthozoa* to be not now held by the scientific naturalist.

Let us then test the propounded explanations of their origin by secondary law. That of 'appetency' subsides from the impotency of a coral-polyp to exercise volition. The weak point of Lamarck's creative machinery is its limited applicability, viz : to creatures high enough in the scale to be able to 'want to do something :' for the determined laws of the 'reflex function' in the physiology of the nervous system and the necessity of the superadded cerebral mass for true sensation rigorously fix the limits of volitional faculties.

We pass then to considerations of the 'ambient medium' and 'natural selection.' We have no evidence that the fabricators of the coral-reef of Wenlock-edge, or of those skirting the Cambrian slates and Devonshire 'killas,' or of those in the lofty limestone cliffs of Cheddar, worked in an ocean otherwise constituted than the present. What conceivable character of sea or of the air dissolved or diffused therein could have changed the loose aggregation of the individuals of composite *Rugosa* into the close combination, with intercommunicating pores, of those of the composite *Tabulata* ? Or what possible external influence could have transmuted the comparatively simple massive mode of growth or deposition of carbonate of lime common to both *Rugosa* and *Tabulata* into the light and complex character of the polyparies of most existing lamelliferous *Anthozoa* ? In the first mode the old polyp-cell is successively partitioned off from the one in occupation by floor after floor crossing the cavity : in the other, radiating vertical partitions alone occupy the deserted cell and extend uninterruptedly from its bottom or beginning to the superficial inhabited chamber. The quadripartite pattern of the plaited cup of the palæozoic coral has changed into the sexpartite disposition of the radiating lamellæ of the polyp-cells of tertiary and modern corals. But personifying the fact of such transmutations by the term 'natural selection' gives no more insight into the manner of the operations than we learn of that of the budding out of a new leg in a maimed newt, by being told that it is done by the 'nisus formativus' or by 'pangenesis' ! Even were there evidence of changes in the composition of the atmosphere, their 'modus operandi' in effecting such structural differences would not be more conceivable.

I do not believe that a sexpartite type of coral was miraculously created to supersede a quadripartite one. If the grounds are good for admitting the continuous operation of a secondary cause of the specific forms of Vertebrate life, *à fortiori* it is admissible in the lower sphere of Radiate life. It is consistent with facts that a quadripartite coral might bud out, or otherwise generate, a variety with a greater number of radiating laminæ. Some varieties, like those expressed by the modern generic terms *Porites, Millepora*, especially the *M. complanata*, with its strong vertical plates, were better adapted to bear the brunt of the breakers, and flourish in the surf, under the protection of the coating Nullipore. But to how small an exception is this relation applicable! Of the 120 kinds of coral enumerated by Ehrenberg in the Red Sea,* 100, at least, exist under the same conditions. The majority of species, originating in uncalled-for, unstimulated, unselected departures from parental structure, establish themselves and flourish independently of external influences. All classes of animals exemplify this independence: the Cetaceans, under an extraordinary and nicely graduated range of generic and specific modifications; and the same may be said of most Fishes.†

So, being unable to accept the volitional hypothesis, or that of impulse from within, or the selective force exerted by outward circumstances, I deem an innate tendency to deviate from parental type, operating through periods of adequate duration, to be the most probable nature, or way of operation, of the secondary law, whereby species have been derived one from the other.

It operates, and has operated, in the surface-zones where the chambered cephalopods floated, and at the depths where the brachiopods were anchored, as in the more defined theatre in which the various polyps of the coral reef display their diversities of color, size, shape, and structure, independently of outward influences. This tendency, moreover, is not exemplified in the ratio of the number, variety, or force of conceivable 'selective' surrounding influences, but is directly as the simplicity of the organism. In the *Foraminifera*, e. g., it is manifested in such degree that as many as fifteen genera defined by one given to—

> Intrigue with the specious chaos, and dispart
> Its most ambiguous atoms with sure art;
> Define their pettish limits, and estrange
> Their points of contact and swift counterchange,

have been found by his followers to be but varieties of a single type; and even this, too inconstant to come under the definition of a species given in p. 7. The departure from parental form, producing the beautiful varieties of perforate and imperforate Rhizopods, and which exemplify each group, respectively, under the Lagenine, Nummulinine, Globigerine, or

* cccxix″, p. 46. † xcix′, p. 44.

under the Gromiine, Milioline, and Lituoline types, has effected its ends independently of inner volitions or of outer selections. Certain incrusting forms seem by the presence of siliceous spicula to have been derived from sponges; but no explanation presents itself for such transitional changes, save the fact of anomalous, monstrous births—as these varieties, and the whole assemblage of alternate-generative phenomena, would be called 'in high life.'

According to my derivative hypothesis, a change takes place first in the structure of the animal, and this when sufficiently advanced, may lead to modifications of habits. But we have no evidence that the observed amount of change in *Porifera*, *Foraminifera* and *Anthozoa*, &c., has been attended with any change in the way or power in which they extract from their ambient medium, and precipitate, silex and carbonate of lime, or in the performance of any other vital function. As species rise in the scale, the concomitant change of structure can and does lead to change of habits. But species owe as little to the accidental concurrence of environing circumstances as Kosmos depends on a fortuitous concourse of atoms. A purposive route of development and change, of correlation and interdependence, manifesting intelligent Will, is as determinable in the succession of races as in the development and organization of the individual. Generations do not vary accidentally, in any and every direction; but in preordained, definite, and correlated courses.

If the survey of a series of siliceous polycystins and diatoms, of zoophytes, of brachiopods, of ammonites, excites pleasure by their beauty, and raises worship of the Power manifesting itself in such inconceivable and exhaustless variety, I accept the relation as one designed, and in His due time, fulfilled:—

> To doubt the fairness were to want an eye;
> To doubt the goodness were to want a heart!

'Derivation' holds that every species changes, in time, by virtue of inherent tendencies thereto. 'Natural Selection' holds that no such change can take place without the influence of altered external circumstances educing or selecting such change.

'Derivation' sees among the effects of the innate tendency to change, irrespective of altered surrounding circumstances, a manifestation of creative power in the variety and beauty of the results: and, in the ultimate forthcoming of a being susceptible of appreciating such beauty, evidence of the preordaining of such relation of power to the appreciation. 'Natural Selection' acknowledges that if ornament or beauty, in itself, should be a purpose in creation, it would be absolutely fatal to it as a hypothesis.

'Natural Selection' sees grandeur in the "view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one:"* 'Derivation' sees, therein, a narrow invocation of a special miracle and an unworthy limitation of creative power, the grandeur of which is manifested daily, hourly, in calling into life many forms, by conversion of physical and chemical into vital modes of force, under as many diversified conditions of the requisite elements to be so combined.

'Natural Selection' leaves the subsequent origin and succession of species to the fortuitous concurrence of outward conditions: 'Derivation' recognises a purpose in the defined and preordained course, due to innate capacity or power of change, by which nomogenously-created protozoa have risen to the higher forms of plants and animals.

The hypothesis of 'derivation' rests upon conclusions from four great series of inductively established facts, together with a probable result of facts of a fifth class: the hypothesis of 'natural selection' totters on the extension of a conjectural condition, explanatory of extinction to the origination of species, inapplicable in that extension to the majority of organisms, and not known or observed to apply to the origin of any species.

§ 427. *Epigenesis or Evolution?*—The derivative origin of species, then, being at present the most admissible one, and the retrospective survey of such species showing convergence, as time recedes, to more simplified or generalized organizations, analogous to von Baer's law of individual development, the result to which the suggested train of thought inevitably leads is very analogous in each instance. If to Kosmos or the mundane system has been allotted powers equivalent to the development of the several grades of life, may not the demonstrated series of conversions of force have also included that into the vital form?

In the last century, physiologists were divided as to the principle guiding the work of organic development.

The 'evolutionists' contended that the new being pre-existed in a complete state of formation needing only to be vivified by impregnation in order to commence the series of expansions, or disencasings, culminating in the independent individual.

The 'epigenesists' held that both the germ and its subsequent organs were built up of juxtaposed molecules according

* ccxiii", Ed. 1860, p. 490.

to the operation of a developmental force, or 'nisus formativus.'

Haller maintained the principle of 'evolution,' Buffon that of 'epigenesis.' Hunter, who surpassed all his contemporaries in observations on the formation of the chick, 'thought he could see both principles at work, together with a third.' However, as he limited the 'pre-existing entities' to 'the materia vitæ universalis' and the 'absorbent faculty,' he would now be classed with the 'epigenesists.' For, he reckoned among the parts newly built up, not evolved, 'the brain and heart, with their appendages, the nerves and vessels, and so on of all the other parts of the body which we do not find at first.'* His third principle is merely a modification of epigenesis, viz: 'change in form and action of pre-existing parts.'

At the present day the question may seem hardly worth the paper on which it is referred to.† Nevertheless, 'pre-existence of germs' and 'evolution' are logically inseparable from the idea of the origin of species by primary miraculously created individuals. Cuvier, therefore, maintained both, as firmly as did Haller.‡ It is, perhaps, one of the most remarkable instances of the degree in which a favorite theory may render us blind to facts which are opposed to our prepossessions. Hunter's demonstrations of the epigenetic development of the blastoderm and initial parts of the chick§ were not known to Cuvier; but the analogous ones of Wolff‖ he had studied. To the phenomena of the blood-lakes and their union in order to constitute the 'circulus vasculosus' of the vitellicle, Cuvier opposes the following remark:—'Mais il faut nécessairement admettre qu'il y avait une pré-existence de quelques chemins pour les pointes rouges; car en virtue de quelle force la figure veineuse serait-elle toujours composée des mêmes vaisseaux ayant la même direction? Comment ces vaisseaux aboutiraient-ils toujours au même point pour former un cœur? Tous ces phénomènes ne sont intelligibles qu'autant qu'on admet quelque pré-existence.'¶

Haller, who had made some good observations on embryonal development, confessed that there was a stage in that of the chick in which the 'intestinal canal was not visible;' he would

* XX, vol. v, p. xiv.

† The encasement or imboxing ('emboitement') of germs was deemed, a century or more ago, to receive support from the evolution of buds and other parts of plants, and from Swammerdam's discoveries in the chrysalis, not only of the parts which afterwards form the butterfly, as wings, antennæ, &c., but also of the eggs which were to be laid in that phase of life. Bonnet drew an inference in favor of the same view from his discovery of the numerous successive generations of *Aphides* which might be impregnated by a single copulation. (See, however, CXLII,' pp. 27, 39)

‡ XXVIII''.

‖ CCCVI''.

§ XX, vol. v, Pls. lxviii,–lxxviii.

¶ CCCVII'', tom. iv, p. 236.

not admit, however, that it was not formed, or that it did not pre-exist; but affirmed that it was too minute to be perceived: not until the head and limb-buds of the chick appeared, was the intestine visibly 'evolved.'*

To the beautiful demonstration of the steps in the successive building up and molding of the intestinal canal, out of the 'mucous layer' of the blastoderm, Cuvier objects :—'Mais quand il serait vrai que l'intestin se forme comme Wolff croyait l'avoir observé, il n'en résulterait aucune preuve en faveur de l'épigénèse; car le nombril, par lequel l'embryon tient à son placenta, est d'abord tout aussi large que l'animal lui-même ; c'est en enveloppant la portion du jaune qui doit rester dans l'intérieur, que la peau finit par rétrécir de plus en plus cette ouverture, qui primitivement n'en était pas une, et par la réduire à l'ombilic tel qu'on le voit dans le poulet ou dans l'enfant naissant.'†

Geoffroy contended that the dogma of 'pre-existence of germs' owed its origin to a metaphysical explanation of ill-observed phenomena. To admit that a germ included within itself all the forms, in miniature, which were afterward to be manifested, and to develop such theory by a matter so indefinable, was to multiply, at will, the most gratuitous suppositions.‡

His opponent's passages, above quoted, in defense of a doctrine now deemed by embryologists to be dead and buried, have hardly other than historical interest ; and I should not have recalled them, or their subject, were it not that ghosts of 'pre-existence' and 'evolution' still haunt some chambers of the physiological mansion, and even exercise, to many, perhaps an unsuspected sway over certain biological problems.

Although in the Debates of 1830, the question of 'Pre-existence of Germs,' was the sole one in which, as applied to Embryogeny, I held with Geoffroy Saint-Hilaire, I remained the thrall of that dogma in regard to the origin of single-celled organisms, whether in or out of body.§ Every result of formifaction I believed, with most physiologists, to be the genetic outcome of a pre-existing 'cell.' The first was due to miraculous interposition and suspension of ordinary laws ; it contained, potentially, all future possible cells. Cell-development exemplified evolution of pre-existing germs, the progeny of the primary cell. They propagated themselves by self-division, or by 'proliferation' of minute granules or atoms, which, when

* "Partes animalis non noviter formantur, sed transeunt ex statu obscuro in conspicuum."—XXVIII'', tom. viii, sectio 2da. p. 150–156. Also 'Mémoire II, sur la formation du Poulet,' p. 182.

† CCCVII'', tom. iv, p. 277.

‡ Anat. Philos., vol. ii, p. 280.

§ CCXLIX, CXLII.

properly nourished, again multiplied by self-division, and grew to the likeness of the parent-cells.

Those who still hold by this rag of 'pre-existence of germs,' call all organic corpuscles or granules 'cell-gemmules,' and maintain that they are transmitted, sometimes becoming developed, sometimes lying dormant from generation to generation, independent, automonous, pre-existing from their primal miraculous creation, as descendants, like all higher forms of life 'of that one form of "Natural Selection" into which life was first breathed.' Darwin grafts upon this modification of the old evolutional dogma* his provisional hypothesis of 'Pangenesis.' (CCCVIII)″.

In like manner the Evolutionists hold that every single-celled organism, torule, organic molecule, out of the body, arises from

* Studying under this belief the phenomena described in CXLII, I was led to regard all 'cells' or organic units concerned in development and repair as the progeny of the primary germ-cell in the ovarium of the mother, and to be in that sense 'derivative.' Save in the case of the hypothetical primordial created unit, such primary ovarian cell in the *Aphis* and all sexual organisms I regarded as impregnated. The derivative cells or organic units propagated themselves independently of direct sexual intercourse; but, that they should not be remotely or indirectly related to the act by which their seat, the developed organism, came to be,—in which organism, or its parthenogenetically propagated offspring, the 'cells' subsequently were formed,—was to me inconceivable on the then accepted hypothesis of 'pre-existence of germs' or 'omnis cellula e cellulâ.' Mr. Darwin, however, opposes to the above view the remark, "My gemmules" (=my germ-cells) "are supposed to be formed quite independently of sexual intercourse, by each separate cell or unit throughout the body." (CCCVIII″, ii, p. 375.) Yet, his provisional hypothesis of 'pangenesis' assumes that they ('cells,' 'cell-gemmules,' 'units') "are transmitted from the parents to the offspring" ib.). But how so (in sexual species), save as the progeny or outcome of the primary impregnated germ-cell in the mother, whence all subsequent development and cell-generation radiated? Take any case in CCCVIII″, which 'Pangenesis' is propounded to explain—and all the given instances of varieties, malformations, &c., are from sexual organisms—as e. g. 'when a stag is castrated the gemmules derived from the antlers of his progenitors quite fail to be developed.' (Ib., ii, p. 399): to each I should reply as to this case:—Such stag first existed as an impregnated unit in the oviducal ovum of the mother. By the 'spontaneous fission' or 'cleavage process' it must have existed as a mass of impregnated gemmules. Accepting the nonsense, that some of these gemmules were derived from the antlers of its parent, yet they are not less the progeny of the primary germ-cell which was formed within the ovarium of the female and was fertilized by the male. I fail, after every endeavor, to appreciate the 'fundamental difference' between Mr. Darwin's cell-hypothesis of 1868 and mine of 1849 (CXLII, p. 5—8). Both of them I now regard as fundamentally erroneous; in so far as they are absolutely based on 'pre-existence'—or 'omnis cellula,' &c. No doubt, many cells or organic units are derived from pre-existing cells (vol. i, p. 625): the phenomenon of the pale or granulated blood-cells which suggested to me, in 1838, the idea of the genetic mode of formation of the ordinary blood-discs, is a true phenomenon: but such mode of formation is subordinate to a wider law. Under given conditions matter in solution aggregates and shows form; if inorganic as 'crystal,' if organic as 'spherule': in the one the process is termed 'crystallization,' in the other 'formifaction.' If the large 'pale cell' was first filled by fluid holding organic matter in solution, the smaller granules or atoms it subsequently discharged might be the result of 'formifaction.' It is at least a more simple, and I believe truer, idea of their origin than that which ascribes such origin to a mysterious genetic act under the name of 'proliferation.'—(CCCVIII″, vol. ii, p. 374.)

a pre-existent germ; and that such germs abound in the air, in the waters, or wherever any forms of living matter may happen to make their appearance.

§ 428. *Nomogeny* or Thaumatogeny ?*† — The French Academy of Sciences was the field of discussion and debate, from 1861 to 1864, between the 'Evolutionists' holding the doctrine of primary life by miracle, and the 'Epigenesists,' who try to show that the phenomena are due to the operation of existing law. The analogy of the discussion between Pasteur and Pouchet, and that between Cuvier and Geoffroy, is curiously close. Beside the superiority in fact and argument, Pasteur, like Cuvier, had the advantage of subserving the prepossessions of the 'party of order' and the needs of theology. The justice of Jamin's summary,‡ awarding to the chemist the palm of superior care and skill both in devising and performing the experiments, and exposing the inferiority of the physiologist in polemical ability and coolness of argumentation, cannot be denied. Nevertheless, Pouchet is rapidly acquiring, in reference to the origin of monads, that position which Geoffroy Saint-Hilaire has taken in regard to the origin of species. It is a suggestive and instructive fact in the philosophy of mind and the history of progress.

Some rare instances, in every generation, are gifted with the faculty of discerning the light of truth through all obstruction: when its glimmer is of the feeblest their brain responsively vibrates through a barrier of beliefs, prepossessions, precise logic, across thickets of facts deemed to be rightly understood, athwart accepted 'laws' and principles, organized corps of the soldiers of science, public opinion, &c.; and these men never know when they are beaten and put out of court: happily, against all hindrance, they persist—'*'e pur si muove.*'

Pasteur by an ingeniously devised apparatus,§ collected atoms in the atmosphere, and described and figured them as examples of 'organized corpuscles,' 'globules,' or the 'germs' of living things, there floating.‖ In a solution of organic matter, otherwise unfit for the development of life, the addition of some of these germs was followed by the appearance, in abundance, of its simple forms.

To the conclusion that the monads were the consequence, not merely the sequence, of the 'ensemencement,' it can be objected that the atmospheric atoms figured¶ are not like the

* *νόμος*, law, *γένω*, root of *γίγνομαι*, to 'become,' or come into being.

† *θαῦμα*, miracle, *γένω*.

‡ CCCXXXIV″, pp. 442, 443.

§ CCCIX″, p. 25, Pl. I, fig. 1.

‖ Ib., Pl. I, figs. 2–9.

¶ Ib., "quelques corpuscles organisées."—p. 28, Pl. I, figs. 2, 3, 4:—"tout-à-fait semblables à des germes d'organismes inférieures."—p. 37. Of the various well marked forms of ova or germs of lower organisms, I know not any recognizable in the figures above cited.

observed formified corpuscles by which bacteriums have been seen to be built up; and, that the chemical treatment to which they had been subject, in their extraction from the atmosphere, would be likely to destroy the vitality of fecund germs, if any were present. To the alleged absence of any organisms in the experiments which were calculated to exclude extraneous germs, and to unfit the infusion for the development of any it might contain, the graver objection applies, that the microscopic power employed by Pasteur in their search was insufficient. Dr. Child,* in experiments which seem to be as exclusive as Pasteur's, does obtain bacteriums, discoverable, at first, by a power of 1,500 diameters, and, once so seen, afterward recognizable by a power of 750 diameters: whereas Pasteur, in his quest, did not avail himself of a power exceeding 350 diameters, and consequently failed to detect the evidence of 'nomogeny,' under conditions as decisive as can be hoped in an attempt to prove a negative. Against 'panspermism,' or the dogma that animalcules of infusions come, invariably and exclusively, from pre-existing germs falling from the air, Pouchet records the results of experiments, conclusive or satisfactory from their simplicity and ease of repetition, and freedom from need of minute, ambiguous, manipulatory precautions.†

A glass tube containing a filtered infusion is placed in the middle of a glass dish containing the same infusion: this stands in a wider dish of water in which a bell-glass is placed covering the vessels with the infusion. At the end of four or five days the tube-infusion has a thick film abounding with ciliate infusoria: the dish-infusion has a thin reticulate film containing only bacteriums and other small non-ciliate 'microzoaires.' It is 'difficult to see how the germs of the one kind of creatures should have entered or become developed in the one vessel and entirely different kinds in the other.'‡

I refer the reader to cccxii″ and cccxxxv″ for further analysis of the grounds of the disputants, and proceed to remark, that the illustrations of the process of development of a *Paramecium*§ so closely resemble those of the ovarian ovum in Fish or Mammal, that either fig. 555 or fig. 416, vol. i. of the present work serves as well as those given by Pouchet, to exemplify it. The proligerous pellicle, due to the resolution into molecules of the primarily formified bacteriums and vibrios of

* cccxii″. † cccx″, pp. 122, 135.

‡ cccxii″, p. 101: paraphrasing Pouchet:—'Si les œufs tombaient de l'atmosphère, comme le prétendent les panspermistes, il n'y aurait pas de raison au monde qui pût faire que, dans la même portion d'air, l'éprouvette en soit constamment remplie et la cuvette jamais. Celle-ci même, à cause de sa surface bien autrement étendue, devrait en récolter infiniment plus.'—cccx″, p. 136.

§ cccx″, Pl. II, figs. 1–5. and cccxi″, Pl. I, fig. 1..

infusions, answers to the molecular contents of the ovisac. In both instances the molecules or granules aggregate into groups forming spheroids more opake than the rest (as in fig. 555, A): as the aggregation and coalescence advances the sphere becomes more opake, more definite: then a clear line marks its inclusion within a membrane, analogous to a 'zona pellucida,' and proclaims its individualization (as in ib. B).

Next appears a clear nucleus, answering to the germinal vesicle (as in ib. C). Fission of the nucleus is followed by that of the monad, which may thus multiply itself within the primary envelope (*Chlamydomonas*, CCXLIX, fig. 29), like the cleavage-formation of the germ-mass: ciliary organs are acquired in both instances, rotating the germ-mass in the mammalian ovum, and extricating the monad from its proligerous bed; whereupon it revolves or darts along, a free animalcule, in the subjacent liquor of the infusion.

In neither instance is there any support, from observation, of the derivation of germ-mass or of monad by evolution out of a pre-existing cell: in both instances have the processes of epigenesis or building up *ab initio* been repeatedly seen and traced.*

In the case of the ciliate infusory the following are the primary or preliminary steps in the formation of the proligerous pellicle, or 'Burdach's mucous layer.' In the clear filtered infusion a slightly opalescent appearance precedes the formation of the thin superficial film. This consists of molecules of various sizes, the most minute testing the highest powers of the microscope. These molecules I attribute to the act of formifaction, which in reference to organic matter in solution corresponds with the crystalline aggregation of mineral matter in solution. Solution of organic matter, such as clear serum from a blister, inclosed in 'goldbeater's' skin or other close membrane, and inserted beneath the integument of a living Mammal—even distilled water which so placed obtains the elements of formifaction by endosmosis—shows its results in the form of granules, white blood-cells, pus-globules, &c. These experiments need repetition and modification mainly in reference to the objection that such 'leucocytes' might have wriggled their way, like *Amœbæ*, from without, through the close texture of the enclosing bag.† In the proligerous pellicle the larger molecules unite end to end, forming bacteriums, or less regularly into masses composing *Torulæ*: these send out parts which become jointed tubes, and may terminate in rows of sporules (*Penicil-*

* CCCX″, pp. 352–388. CCCXI″, pp. 133–253. CCCXII″, pp. 121–129. CCCXIII″, p. 1046. CCCXIV″, p 974. CCCXV″, p. 467: **Mantegazza spent sixteen consecutive hours in observing this genesis.**

† CCXVII″.

lium) or capsules of such (*Aspergillus*) The bacteriums may, by further union and confluence, form vibrios. There is much activity, allied in character to the Brunonian movements;* which, after a time, ceases, and the bacteriums, vibrios, &c., are decomposed to constitute the secondary series of molecules in and from which the development of the higher ciliate Infusory takes place. The formation of the proligerous pellicle or 'secondary histolytic mass of molecules'† by the primary developments and resolutions of the organic material, is analogous to the formation of the germ-mass, in ovo, by the successive spontaneous fissions, assimilations, and ultimate coalescence of the progeny of the original germinal cell.

To meet the inevitable question of 'Whence the first organic matter'? the Nomogenist is reduced to enumerate the existing elements into which the simplest living jelly (*Protogenes* of Hæckel) or sarcode (*Amœba*) is resolvable, and to contrast the degree of probability of such elements combining, under unknown conditions, as the first step in the resolution of other forces into vital force, with the degree of probability remaining, after the observations above recorded, of the interposition of a miraculous power associating those elements into living germs, or forms with powers of propagating their kind to all time, as the sole condition of their ubiquitous manifestation, in the absence of any secondary law thereto ordained.

In this, the last general summary of work which I am likely to find time to complete, the expression of belief on one or two points where proof is wanting may be condoned. The chance of its being a help, or encouragement to any younger, more vigorous mind, bent upon grappling with such problems, outweighs any anticipation of trouble consequent upon the avowal.

It seems to me, then, more consistent with the present phase of dynamical science and the observed gradations of living things, to suppose that sarcode or the 'protogenal' jelly-speck should be formable through concurrence of conditions favoring such combination of their elements and involving a change of force productive of their contractions and extensions, molecular attractions and repulsions—and that sarcode has so become, from the period when its irrelative repetitions resulted in the vast indefinite masses of 'eozoon,' exemplifying the earliest process of 'formifaction' or organic crystallization—than that all existing sarcodes or '*protogenes*' are the result of genetic descent from a germ or cell due to a primary act of miraculous interposition.

Some, accepting the latter alternative, teach that, while

* CCXVIII'', p. 470, in all organic molecules, living or dead.
† CCCXXXV'', p. 10.

generations of the first-created sarcode have descended to us unchanged from the period of the Laurentian limestone, other sarcodal offspring have developed and improved, or have been selected, into all higher forms of living beings. I prefer, however, while indulging in such speculations, to consider the various daily nomogeneously developed forms of protozoal or protistal jellies, sarcodes and single-celled organisms, to have been as many roots from which the higher grades have ramified, than that the origin of the whole organic creation is to be referred, as the Egyptian priests did that of the universe, to a single egg.

Amber or steel when magnetized seem to exercise 'selection;' they do not attract all substances alike. To the suitable ones at due distance they tend to move; but, through density of constitution, cannot outstretch thereto; so they draw the 'attracted' substance to themselves. If the amber be not rubbed, or the steel bar otherwise magnetized, they are 'dead' to such power. The movement of a free body to a magnet has always excited interest, often wonder, from its analogy to the self-motion so common and apparently peculiar to 'life.'

A speck of protogenal jelly or of sarcode, if alive shows analogous relations to certain substances; but the soft yielding tissue allows the part next the attractive matter to move thereto, and then by retraction to draw such matter into the sarcodal mass, which overspreads, dissolves, and assimilates it. We say that the *Protogenes* or *Amœba* has extended a 'pseudopod,' has seized its prey, has drawn it in, swallowed, and digested it. No 'organs,' however, are recognizable; neither muscle, mouth, nor stomach.

If the portion of iron attracted by the magnet became blended with the substance of its attractor, the analogy thereto of the act of the amæba would be, perhaps, closer, more just, than that other analogy which is expressed by terms borrowed from the procedure of higher organisms.

From certain knowledge of the homogeneous, by some termed 'unorganized,' texture of *Protogenes* and *Amœba*, we cannot predicate of their having sensation or exercising volition. Given 'life' and suitable organic substance at due distances, the act of making contact seems as inevitable, as independent of any volition of the amæba, as in the case of amber or steel, given 'magnetization' and attractable substances at due distance.

The term 'living,' in the one case, is correlative with the term 'magnetic' in the other. Devitalize the sarcode, unmagnetize the steel, and both cease to manifest their respective vital or magnetic phenomena. In that respect both are 'defunct.' Only the steel resists much longer the surrounding decomposing agencies.

A man perceives a ripe fruit: if he can and will, he stretches out his hand, plucks, brings to his mouth, masticates, swallows, and digests it.

The question then arises whether the difference between such series of actions in the man and the attractive and assimilative movements of the amæba, be less or greater, than the difference between these acts of the amæba and the attracting and retaining acts of the magnet.

More may be said on both questions than I have here space for; but when all is said, the question, I think, may be put with some confidence as to the quality of the ultimate reply and the affinity to truth, and liberty to accept it, in the equal respondent, viz: whether the amæbal phenomena are so much more different, or so essentially different, from the magnetic phenomena than they are from the mammalian phenomena, as to necessitate the invocation of a special miracle for their manifestation.

Magnetic phenomena are sufficiently wonderful, exemplifying, as they do, one of those subtle, interchangeable, may we not say 'immaterial,' modes of force which endows the metal with the power of attracting, selecting, and making to move, a substance extraneous to itself. It is analogically conceivable that the same CAUSE which has endowed His world with power convertible into magnetic, electric, thermotic and other forms or modes of force, has also added the conditions of conversion into the vital mode.

Nerve-force we know to be convertible into electric energy, and reciprocally: and from the electric force, so induced, magnetic and other modes have been derived (vol. i, p. 357). The direction, then, in which may be anticipated the replies to the ultimate question, will be toward an admission of the originating and vitalizing of the primary jelly-speck or sarcode granule, by the operation of a change of force forming part of the constitution of Kosmos; not contrary to its ordained laws, in the sense in which 'miracle' or the 'interposition of special creative act,' is rightly understood.

But from protozoa,* or protista, to plants and animals, the gradation is closer than from magnetized iron to vitalized sarcode. From reflex acts of the nervous system, animals rise to sentient and volitional ones.

* This is the better as well as older term: *ζῶον* being understood as 'life' generically, and before development has differentiated its manifestations into unambiguous 'vegetal' and 'animal' modes.

And with that ascent are associated brain-centers, progressively increasing in size and complexity. Arrest the development of the human brain at the point it has reached in the 'Aztec,' and the faculty of generalizing and giving expression to such generalizations is wanting. The Aztecs can articulate words, and apply the right noun to the thing, as e. g. 'bread,' 'chair;' but they cannot combine ideas into propositions, and say 'give me bread,' 'set me the chair.'

For such advance in intellectual acts more brain is essential. Compared with the normal state of brains in the brutes best endowed, so much more cerebral substance is required, and in such position, as to make the great and sudden rise, in the lowest grades of man, which is referred to in vol. iii, p. 144.

Thought relates to the 'brain' of man, as does electricity to the nervous 'battery' of the torpedo: both are forms of force, and the results of action of their respective organs.

Each sensation affects a cerebral fiber, and in so affecting it, gives it the faculty of repeating the action, wherein memory consists, and sensation in a dream.

A dog at the sight of a rabbit receives a sensation which induces a volition, and he barks with the excitement of the chase. He sleeps, and by suppressed barking and agitation of limbs reveals the fact that he dreams. Shall we obtain any further insight into the nature of the act or acts resulting in this sensation, memory, dreamy imagination, by saying that the perception of the rabbit reaches the 'soul' of the dog by the affection of its cerebral fibers? Is the 'soul' of the dog other than the personified sum of his psychological manifestations?

The 'sight' of the dog, is its faculty of vision, the 'soul' of a dog is its power of knowing what it sees and determining accordingly: it may approach the object with every manifestation of sentiments of gladness and submissive affection: it may rush upon it with every sign of rage: it may pursue it with every mark of excited ardor.

And these mental activities can only go on for a time: the waste thereby occasioned of fiber and of power calls for renovation, and this for repose, of the mental organ. In sleep the eyes close and sight goes; what then happens to the brain-fibers we cannot see nor tell: but the sum of action called 'soul' ceases. Deep sleep is utter unconsciousness to dog and man. The initial steps and partial resumptions of brain-action are 'dreams'; the awakening one issuing, often suddenly, in the full blaze of consciousness.

I am most averse to travel beyond my proper province; but a general physiological conclusion from the phenomena of the nervous system inevitably brings on collision with a dogmatic affirmation or definition of the cause of the highest class of those phenomena instilled as an article of religious faith into fellow-christians, and on which is based their mode of thought, affecting dearest hopes and highest aspirations. It must be repugnant to any good man's feelings to say aught that may unsettle such mode of thought, though he knows that what he has to impart lends truer and better support to both the faith and the hope.

If the hypothesis that an abstract entity produces psychological phenomena by playing upon the brain as a musician upon his instrument, producing bad music when the fibers or cords are out of tune, be rejected, and these phenomena be held to be the result of cerebral actions, an objection is made that the latter view is 'materialistic,' and adverse to the notion of an independent, indivisible, 'immaterial,' mental principle, or 'soul.'

What 'materialistic' means, in the mind of the objector, I nowhere find intelligibly laid down; but it is generally felt to be something objectionable, 'inconsistent with, or shaking the foundations of an article of faith,' as Stillingfleet would have said.

To this I repeat Locke's answer, that my faith in a future life and the resurrection of the dead, rests on the grounds of their being parts of a divine revelation.

If I mistake not, present knowledge of the way in which we derive ideas of an outer world, helps to a more intelligible conception of 'matter,' 'substance,' 'immateriality,' &c., than could be framed by patristic and mediæval theology. To make intelligible my own ideas in this subject, which the anticipated imputation draws from me, I would put a case and ask a question.

When Saul at Endor "perceived that it was Samuel,"* lines of force, as 'luminous undulations,' struck upon his retina. *Qu.* Were the centers whence they diverged to produce the idea of the dead Prophet 'material' or 'immaterial'?

Other lines of force, undulated in another manner, from centers producing the ideas of the dead man's speech:—"Why hast thou disquieted me, to bring me up?"† *Qu.* Were the centers radiating these acoustic lines of force material or spiritual?

Substitute the living for the dead Prophet, and it will be said that the points whence the rays of light converged to produce his image in the beholder are 'material,' because 'tangible;' in the case of the 'spirit of Samuel,' not. Had Saul stretched forth his hand to grasp the vision, it would have met

* 1 Sam. xxviii, 14. † 1 Sam. xxviii, 15.

no resistance. Let us, then, analyze the sensations from tangible lines of force. I stretch forth the sum of forces called 'hand,' and exercise part of them in a way and direction called 'pressure,' deriving the sense or idea of such act by my lines of force being opposed by other lines of force. To the extent to which my forces overcome the opposing forces, I have an idea of a something giving way; when my lines of force are overcome by the opposite lines of force, I have the idea of a hard or resisting surface. But all that I know, after ultimate analysis, is the meeting of opposite forces; of the centers respectively radiating such force I know nothing; and if I did or could know anything, I cannot conceive that I should get a clearer idea of 'touch,' than as a relation of certain lines of force acting from centers, which may as well be 'immaterial' as 'material,' for any intelligible notion I can frame of those verbal sounds.

If a blade of metal could move itself to and fro, in striving to cleave the space between excited electro-magnetic poles, and could tell us its sensations, they would be those of sawing its way through a substance like cheese; but there is no visible impediment: nor, were luminous undulations to vibrate from the hindrance, as from the plane of force resisting the pressing finger, would the hindrance be less 'immaterial.' Similarly, if lines of thought-force were visible, the 'ghost' would not on that account be more 'material.'

The ideas excited by the act of pressure are those of the 'exertion of force' and the 'resistance of force;' if these ideas be analyzed they include those of the direction of force in lines from centers or points. Further than this, my mind, or thinking faculty, cannot go; i. e., can have no clear ideas: I cannot feel that I know more about the matter by calling the 'centers of force,' 'material atoms' or 'immaterial' points, and am resigned to rest at a point beyond which Faraday* did not see his way.

Having evidence of the opposing force acting in lines from centers distinct from and outside of those volitional centers called 'ego,' the sensation is sufficient for my belief that it is due to the reaction of lines of force from outside-centers upon lines of force put into action from inside-centers. But I have no ground for calling the one 'material' and the other 'immaterial,' or either, or both. The same result has followed my attempts to analyze all sensations and volitions, i. e., I know of nothing outside myself of which I can have any clearer knowledge by calling it 'material,' than I have of that which

* CCCXXXVII'', p. 119.

originates force from within myself, by calling it an 'immaterial' entity, mental principle, or soul.

But, so it is; in the endeavor to clearly comprehend and explain the functions of the combination of forces called 'brain,' the physiologist is hindered and troubled by the views of the nature of those cerebral forces which the needs of dogmatic theology have imposed on mankind.

How long physiologists would have entertained the notion of a 'life,' or 'vital principle,' as a distinct entity, if freed from this baneful influence, may be questioned; but it can be truly affirmed that physiology has now established, and does accept, the truth of that statement of Locke,—'the life, whether of a material or immaterial substance, is not the substance itself, but an affection of it.'* Religion, pure and undefiled, can best answer, how far it is righteous or just to charge a neighbor with being unsound in his principles, who holds the term 'life' to be a sound expressing the sum of living phenomena; and who maintains these phenomena to be modes of force into which other forms of force have passed, from potential to active states, and reciprocally, through the agency of these sums or combinations of forces impressing the mind with the ideas signified by the terms 'monad,' 'moss,' 'plant,' or 'animal.'

If the physiologist rejects the theological sense of the term 'life,' without giving cause for the charge of unsoundness in religious principles, does he lay himself more open to the charge, by rejecting also the theologian's meaning of the term 'spirit,' of the term 'soul,' of the term 'mind,' and, we might add, of

* CCCXXXVI'', vol. i, p. 761. As the authority of a Physiologist and late President of the Royal Society may be cited for ascribing such vital phenomena to an invisible 'mental principle,' (a) I unwillingly refer to the remark by which Sir B. Brodie meets the obvious objection of the divisibility, without destruction, of acrite organisms:—'It is true that one of our most celebrated modern physiologists, from observing the multiplication of polypi by the mere division of the animal, has come to the conclusion that the mental principle, which to our conceptions presents itself as being so preëminently, above all other things in nature, one and indivisible, is nevertheless itself divisible, not less than the corporeal fabric with which it is appreciated.' (p. 115.) The reader, eager for new light and guidance toward truth, naturally here expects the facts and arguments exposing the weakness or fallacy of the inference deduced from the polyp-phenomena. The sole remark is a charge of that kind called '*argumentum ad hominem*.' 'But it is to be observed,' (proceeds Sir B. B.) 'that, great as is the authority of Müller generally, in questions of physiology, in the present instance he is not quite an unprejudiced witness, inclined as he is to the pantheistic theory,' &c. (p. 116) Now, the charge is untrue; and, were it otherwise, affects not the point in question. Johannes Müller was of the school of inductive physiologists, opposed to Oken and others of the school of Schelling. He would not accept even the 'vertebral theory of the skull,' or 'general homologies;' but adhered to the party of Cuvier. He lived and died a sincere member of the Roman Catholic Church. Brodie's notion of a 'mental principle' seems to be a combination of 'vital principle' and 'soul,' *πνεῦμα* and *ψυχή*.

(a) Brodie's Sir B., 'Psychological Enquiries,' 12mo. 1854, pp. 103, 115, 167.

'sin,' or 'death'? That is to say, arguments based upon scriptural expressions of thought-force may be drawn from the like personifications of the aberrations and cessation of such force. Both poets and painters have, in each case, endeavored to realize and give shape to the abstractions.

When doubting Thomas obeyed the Lord's command, his fingers met resistance below what seemed to him the surface of the side, and, entering the wound, were opposed by a 'force' exceeding the 'force' they exercised.* The resulting idea was that the 'matter' of our Lord was there, but wanting where the spear had penetrated; the fact was the opposition of a force by a force, and the sensation of that opposition. We know of nothing more 'material' than the 'centers of force.' Our ideas of things without as within the 'ego,' are the action and reaction of forces, as 'material' or 'immaterial,' as the ideas themselves.

In this view is avoided the alternative of 'idealism' with denial of an external world, or that of the personifying the sum of mental phenomena as an 'immaterial indestructible soul,' contradistinguished from other sums of forces which are as arbitrarily styled 'destructible matter.' Sleep, stimulants, drugs, disease, concur by their effects in testifying that the kinds and degrees of mental manifestations are the result of corresponding affections and changes of structure of the brain.

How the brain works in producing thought or soul, is as much a mystery in man as brutes—is as little known as the way in which ganglions and nerves produce the reflex phenomena simulating sensation and volition.

But it is a gain to be delivered from the necessity of speculating where the 'soul' wanders when thought and self-consciousness are suspended; or how it is to be disposed of until the 'resurrection of the body,' glorified or otherwise; of which reintegrated sum of forces 'soul' will then, as now, be a parcel. If the physiologist and pathologist had done no more than demonstrate 'the universal law of our being,'† which cuts away the foundations of 'purgatory,' or other limbo, from the feet of those who trade thereon,‡ which makes 'judgment' follow death, without consciousness of a moment's interval,§ they would deserve the gratitude of the Christian world.

* CCCXXXVI″, vol. i, p. 656. The whole of Locke's 'Second Reply' to Bishop Stillingfleet may be read, with profit, in relation to the undesigned testimony borne by physiology to the clear good sense and affinity for truth in the philosopher's remarks on the relation of the dogma of 'immateriality,' 'indestructibility' and 'separability' of soul, to a Christian's faith in the resurrection of the dead, as resting on the grounds of divine revelation.

† CCCXXXVII″, p. 306.

‡ Not to mention the kindred baser brood of 'Spiritualists and Spirit-Rappers.'

§ For the importance of this conviction to 'practice,' see CCCXXXVI″, vol. i, p. 156, § 63. 'In comparing present and future.'

ART. VI.—*On some phenomena of Binocular Vision;* by JOSEPH LECONTE, Prof. Chem. and Geol. in University of South Carolina.

I. *Adjustments of the eye.*

Two kinds of ocular adjustment take place in every voluntary act of sight, viz: (1) a proper convergence of the optic axes so that they shall meet on the object of sight, and (2), an adjustment of each eye so that the diverging pencil of rays which enters the pupil shall be brought to perfect focus, and therefore produce a perfect image, on the retina. The first or *binocular adjustment* is necessary for *single vision:* the second or focal adjustment is necessary for *distinct vision.* The first is distinctly sensible for all distances within 100 yards and perhaps for much greater distances: the second is scarcely if at all sensible for distances beyond two yards.

To the two adjustments mentioned above may be added a third, viz: *contraction of the pupil.* The design of the contraction of the pupil is probably to increase the clearness of definition of the retinal image by cutting off the most divergent rays from very near objects, and thus to decrease the spherical aberration which is not entirely corrected in the eye by the form of the lens. The pupil, however, also contracts involuntarily under the stimulus of strong light, without regard to distance. This must be carefully distinguished from the adjustive contraction, which is to some extent at least voluntary.

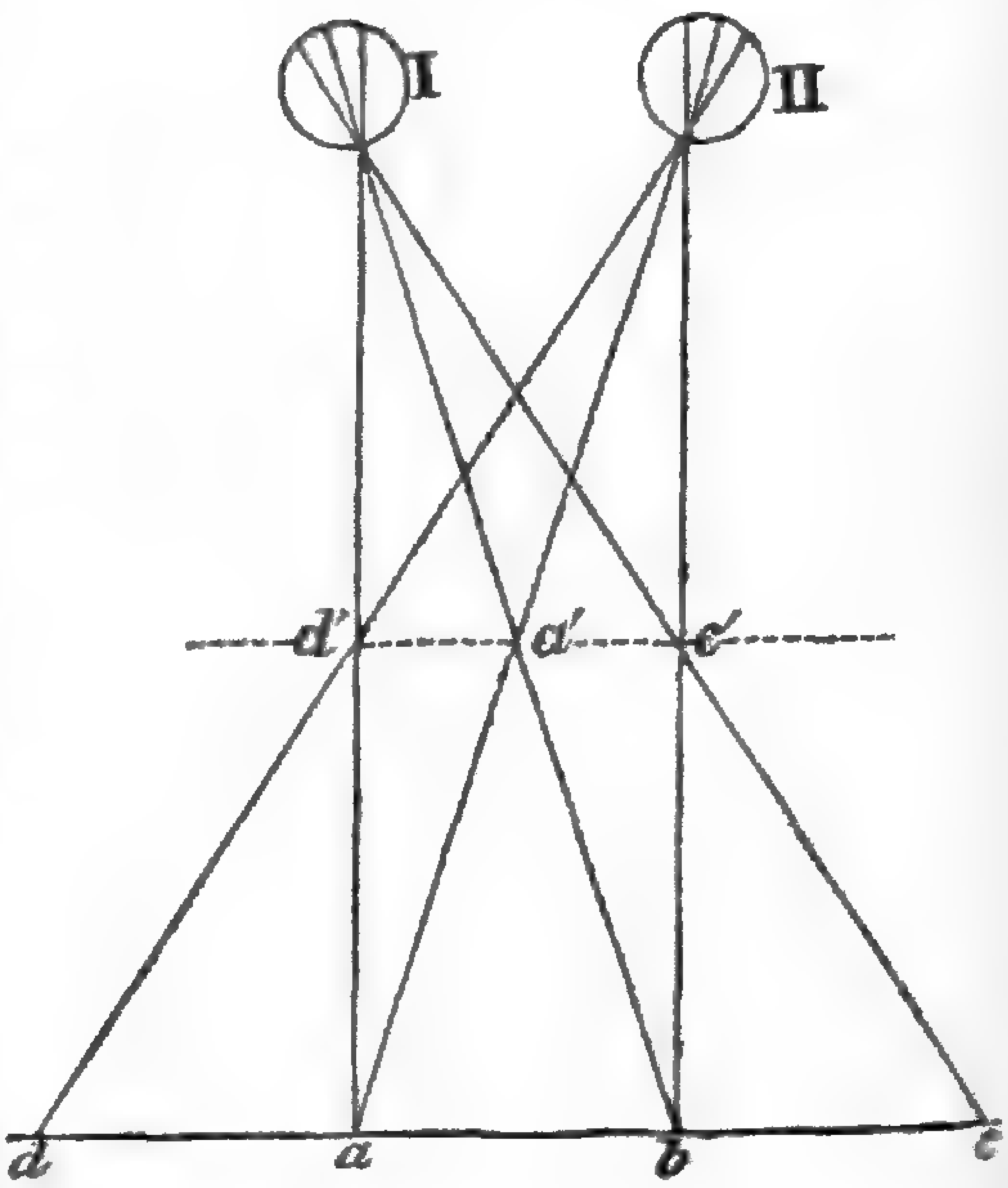
These three adjustments of the eye, viz: binocular or axial adjustment, focal adjustment and contraction of the pupil are associated in every voluntary act of sight. They are accomplished by one act of volition. They are so intimately associated that they cannot be voluntarily separated. It is usually impossible to converge the optic axes on any point, without at the same time adjusting the lens and contracting the pupil in a manner suitable for perfect vision at that distance. Such inseparably associated movements are called consensual movements.

The binocular adjustment is well understood; there is no difference of opinion as to its necessity nor the means by which it is accomplished. But in regard to the focal adjustment there has been much difference of opinion among the best physiologists and physicists. Some have denied altogether the necessity, and therefore the existence of any adjustment: attributing the phenomena which are usually explained by this means, to mere transference of attention from near to distant objects, or vice versa. The large majority of the best physicists and physiologists, however, have for a long time regarded focal adjustment as an optical necessity and therefore a fact; but the real nature of this adjustment and the means by which it is accomplished, has been a question in doubt. It has been attributed by some to the elongation of eye by the action of the recti muscles,—by others to the change in the convexity of the cornea—by others to the structure of the crystalline lens combined with contraction of the pupil—by others to the pulling forward of the crystalline lens by the ciliary muscle so as to elongate the chamber behind the lens, and by still others to the change of form of the lens by the action of the ciliary muscle. Recent very ingenious observations by Donders, Cramer and Helmholtz, upon the images of external objects made by reflection from the anterior surface of the crystalline lens, and the changes in form and size, which they undergo when the eye is adjusted for near objects, have definitely settled the question in favor of a *change in the curvature of the lens.* The mechanism by which this change is effected is not clearly known but it is probable that it is effected by the action of the ciliary muscle.

Before giving some experiments which bear upon the question of adjustment, I will state that my eyes are perfectly normal. In youth and early manhood the natural distance for distinct vision of small objects was eight inches, but with effort I could see perfectly distinctly at five inches. At the present time my natural distance for fine print is ten inches, though with effort I see distinctly at eight inches. Beyond this there is for me no limit of distinct vision. My eyes define the edge of the moon as perfectly as they do an object at the distance of ten inches. Moreover, by long practice I have acquired considerable and perhaps very unusual facility in making experiments on binocular vision and in analyzing my visual impressions. The following experiments which I have practised from boyhood, are interesting not only as a beautiful illustration of the laws of binocular vision, but I believe as throwing some light on the subject of adjustment and also upon the difficult subject of the *Horopter.*

If a plane surface checkered or otherwise figured *in regular pattern*, such as an oil floor-cloth, a tesselated pavement or a papered wall, be placed before the eyes at the distance of several feet, and the optic axes be then voluntarily converged (the eyes crossed) upon some point in space nearer than the surface, the figures will of course be all seen double. If now the convergence be steadily increased until two contiguous similar images, one belonging to the right eye and one to the left, are made to coincide perfectly, and the eyes be then held steadily in this position for some time, the patterned surface will be distinctly seen in exquisite miniature, not at its proper distance but between the real object and the eye, at a distance depending upon the interval between the centers of the contiguous similar figures of the pattern. If the pattern be very regular the illusion is complete—we actually seem to be looking at a real object. In this experiment the position of the eyes is such that, of two contiguous similar figures, the right eye is directed toward the left figure and the left eye toward the right figure; and the image is seen at the crossing of the visual lines. Thus if one eye be directed toward *a* (fig. 1) and the other toward *b*, a perfect image of these two figures will be seen at *a′*. So also *b* and *c* will be united and seen at *c′* and *a* and *d* at *d′*, and so on for all the figures of the pattern. The dotted line *d′ a′ c′* will be the position of the image surface. The image thus obtained may be a little indistinct at first, but it gradually grows perfectly clear. As soon as the image is distinctly seen and the outlines of the figures well defined, it may be retained without any difficulty; for we seem to be looking at a real object and therefore retain the necessary convergence of the optic axes with ease. The eyes may now be turned in every direction, viewing this extensive image surface precisely as if it were a real surface.

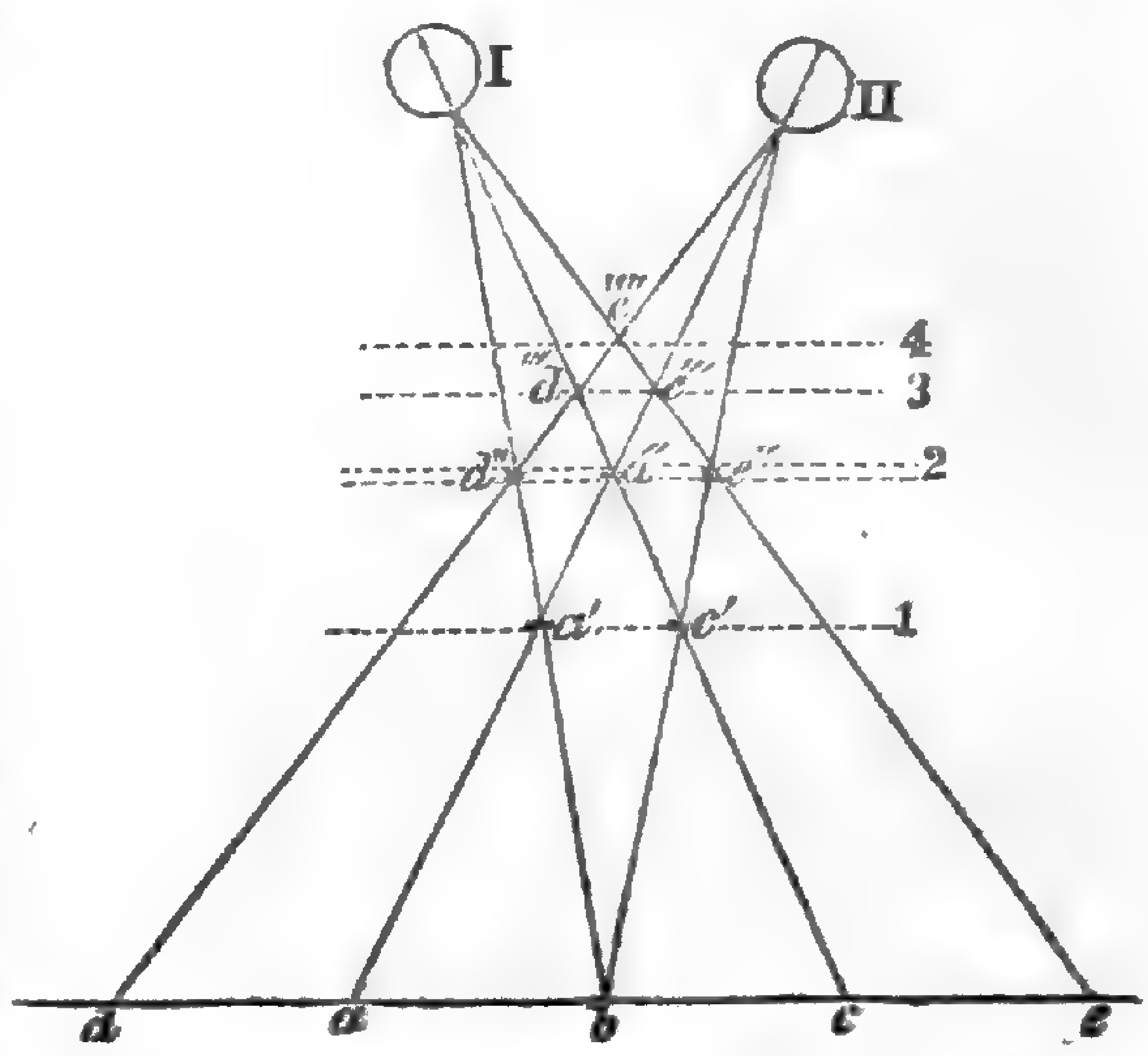
1.



If now while viewing the image in the last experiment we repeat upon it the same experiment, i. e., if by increasing the convergence of the optic axes we bring again the two contiguous figures in coincidence, a new image is formed between the last and the eye and is seen in still smaller miniature. In this case the position of the optic axes is such that the eyes crossing are directed not toward contiguous figures of the real object but to figures separated by an intervening one. Thus in the figure (fig. 2) *a* and *c* will be combined and seen at *a″*, *d* and *b* at *d″* and *b* and *e* at *e″*.

Upon this second image the same experiment may be repeated so as to make a third image still smaller and nearer the eye at d''' e''' and from the third even a fourth and still smaller image may be formed at e''''. The position of these successive planes are indicated by the dotted lines; but in this figure the position of the axes is only adapted to vision on plane No. 2. For the higher planes, the optic axes must converge still more. For the 4th plane Ie and IId will represent the visual lines. Standing erect and looking down upon the regularly checkered carpet on the floor of my room, the figures of which are 4½ inches from center to center, I can with the greatest ease bring out successively four distinct images one above the other, the nearest being but seven inches from my eyes, and the figures (which are two inches in diameter in the carpet) reduced to about ¼ inch in diameter. If while looking at the image on the 4th plane the convergence of the optic axes be suddenly relaxed the image drops and may be caught on No. 3. Again by relaxing the convergence it may be dropped and caught on successive planes until it falls to its natural position.

2.

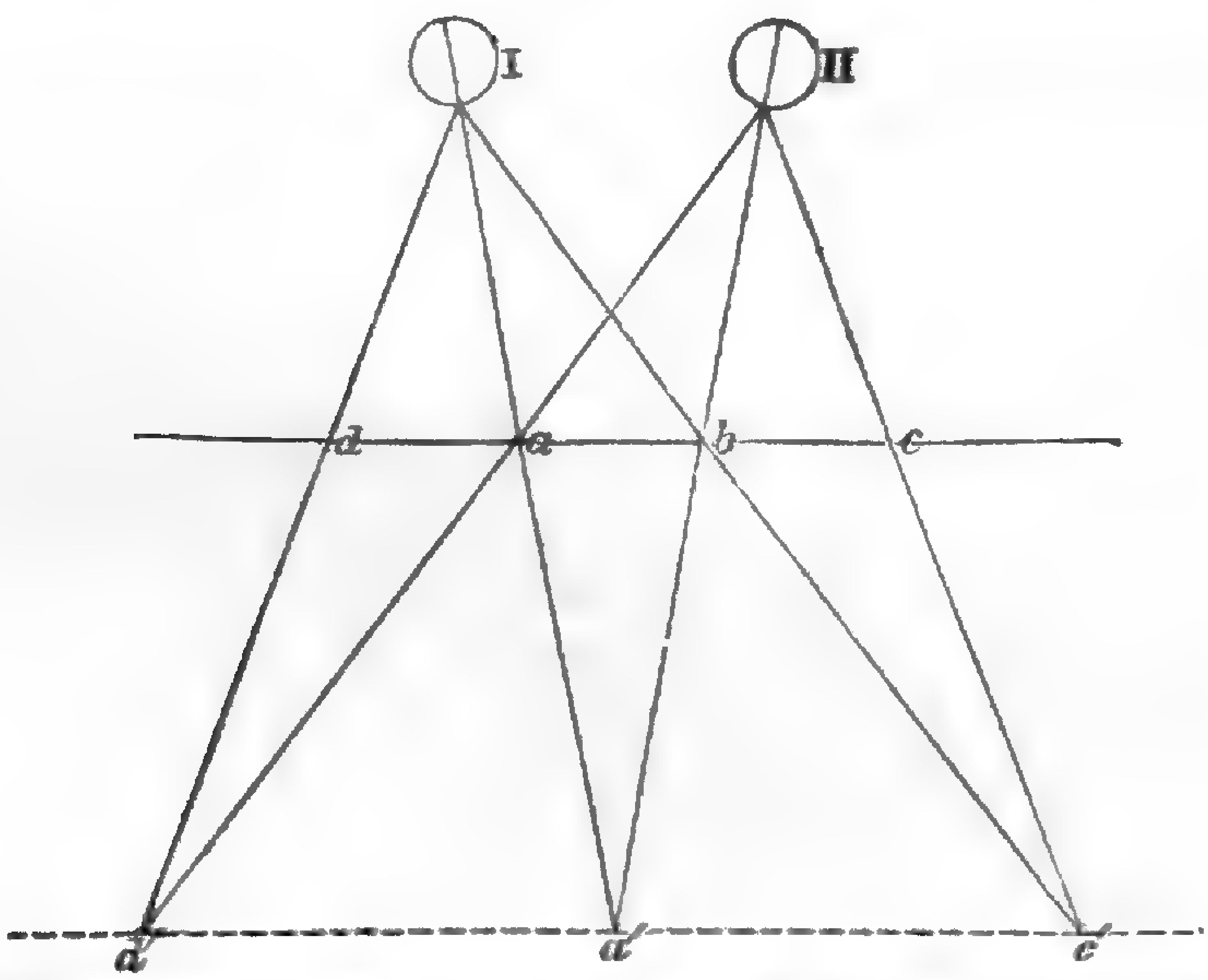


I have made similar experiments on a great variety of patterns of wall papering, oil-cloths, calicos, &c., with the same results. Of a regularly checked oil-cloth on my hall, the lozenge-shaped figures of which are 10·2 inches across, I make successively three perfectly distinct images, the nearest being but 4½ inches from the eyes.

Those who are not accustomed to experiments of this kind can probably most easily succeed as follows. If we look on the floor, and place the finger between the eye and the floor the finger will of course be seen double. Now move the finger up or down until we find a place in which the two images of the finger will exactly fall on contiguous figures of the pattern: the finger now indicates the position of the first plane. Now look steadily at the finger instead of the floor, until the image of the floor rises to it and becomes distinct, then withdraw the finger. To get the second plane, look again at the floor and raise the finger until its images fall upon figures separated from one another by an intervening figure and then look steadily at the finger. The other planes may be obtained in a similar manner. The position of the several planes may be also easily calculated ; the data being the inter-ocular line, the distance of the object and the interval from center to center of the figures. Both by measurement and calculation I determined the planes in the case of the carpet to be 21·5, 13·05, 9·37 and 7·3 inches respectively. In the case of the oil-cloth they were 11·8, 6·54 and 4·5 inches.

If the distance between the centers of contiguous figures be less than the inter-ocular line, then still other images may be seen *beyond* the real object and very much *enlarged.* The position of the eyes and the place of the image in this case also

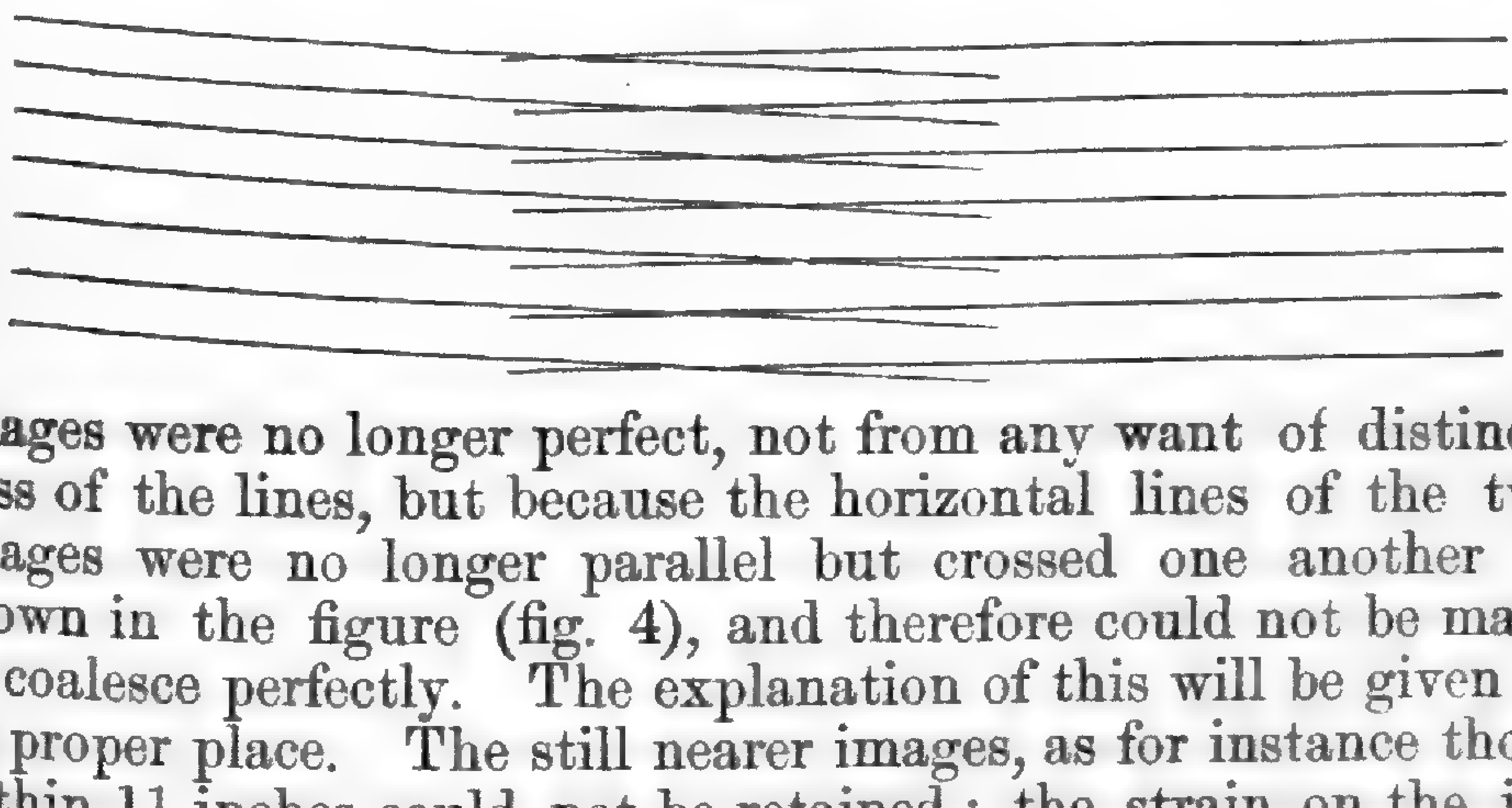
3.



is easily explained. If d a b c (fig. 3) be the plane of the real object, and the eyes I and II be directed toward contiguous figures a and b but *not crossed,* then the image of a and b will combine and be seen at a' the intersection of the visual lines. So also a and d will be seen at d' and b and c at c', and the dotted line will represent the position of the image plane. In order to make this image we must gaze *through and beyond* the pattern until we observe the double images come together and coincide, and then fix the eyes steadily. The enlarged image gradually becomes distinct.

This experiment is much more difficult than the preceding. The pattern should not be too small, otherwise the difficulty is very great. In former years I had often performed the experiment with perfect success, but the wall papering I had used for this purpose, had been destroyed and I found difficulty in again obtaining a suitable pattern. I therefore constructed a pattern by ruling black lines on a large sheet of paper so as to make perfectly equal squares $1\frac{1}{2}$ inches wide. With this simple diagram my success in all the preceding experiments was really marvellous. The colored patterns before used form far more beautiful images, but for scientific purposes the ruled diagram is far preferable. With this diagram standing upright before me at the distance of sixteen inches, I got with great ease seven successive images on this side of the object and one beyond. All the images on this side were defined, with great ease and *perfect distinctness* although the nearest, both by measurement and by calculation, was but three inches from my eyes, i. e., far within the limits of my distinct vision. With great effort I could obtain others still nearer. The nearest I actually retained and measured, was but $1\frac{1}{4}$ inches from the root of the nose, but I afterward found that there was no limit except the root of the nose itself. Within three inches, however, the

4.



images were no longer perfect, not from any want of distinctness of the lines, but because the horizontal lines of the two images were no longer parallel but crossed one another as shown in the figure (fig. 4), and therefore could not be made to coalesce perfectly. The explanation of this will be given in its proper place. The still nearer images, as for instance those within $1\frac{1}{4}$ inches could not be retained; the strain on the interior recti muscles of the eye was too great.

The image beyond the object is much more difficult to obtain with clearness, especially if the object be near the eyes. At the distance of two feet from the object, I obtained the image very clearly and without much difficulty, but on approaching to within ten or twelve inches it was only by patient trial for some time that it could be brought out with perfect distinctness. When the object was twelve inches from the eyes the image by calculation was found to be about thirty inches distant. By turning the diagram so that the diagonals were horizontal and similar points therefore more than two inches apart, the image was seen at the distance of about six feet. It had the exact appearance of a tesselated marble pavement made up of squares nine inches on a side.

In all these experiments the least irregularity in the pattern shows itself very conspicuously in the image, not by indistinctness of outline of the figures, but by apparent inequality in the plane of the image. Thus in the carpet it shows itself by an apparent wrinkle; in the lined diagram by some of the lines rising like black threads stretched above the general surface of the image. This phenomenon is a familiar one in stereoscopy, and is used for detecting the slighest difference in two apparently similar patterns; as for instance between a genuine and a forged bank bill.

I believe any one, and particularly any young person with good eyes, can with practice succeed in all the experiments detailed above. Several of my family have tried them with success. Yet in all cases it requires some practice to succeed well. I can, even yet, always detect some difficulty on first trial after an interval of a few days. But after several hours practice the illusion is so complete that it is almost impossible to dispel it. The image is so real that in attempting to recover the real object by relaxing the convergence of the optic axes, the doubling of the lines causes the eyes instinctively to return to their former position and thus to restore the image. I have sometimes been actually obliged to look away in order to recover the real object.

The experiments detailed above, have an important bearing on some points in the theory of vision. It is the universally accepted doctrine among physiologists that the axial and focal adjustments of the eye cannot be dissociated. Helmholtz speaking of the consensual movements of the eye says, "We cannot turn one eye up, and the other down; we cannot move both eyes at the same time outward; we are obliged to combine always a certain degree of accommodation of the eye to distance [focal adjustment], with a certain angle of convergence of the axes [axial adjustment]."* He proceeds, however, to give certain peculiar conditions under which the first two laws may be violated, but none in which the last is violated. For many years I regarded these experiments as confirming the ordinary doctrine. I had observed in my first experiments on the carpet that each successive plane became more and more indistinct. I accounted for this by supposing that both the optic axes and the lenses were adjusted for vision on the plane of the image, while the light diverged from the floor five feet distant. It seemed to me a crucial experiment proving the necessity of

* Helmholtz, Croonian lecture, Proc. Roy. Soc., April, 1864.

focal adjustment, and the inseparable association of it with axial adjustment. On re-commencing these experiments a few weeks ago, however, I was struck with the fact that the figures of the images were far more distinct than the real figures were when a small object was viewed in the position of the images. To test this point fairly I placed two bone buttons in similar positions and on similar spots on the pattern and then brought their images in coincidence. At first the united image was indistinct but gradually it became perfectly defined—every thread-hole as clear and distinct as it is possible to conceive. I succeeded, though with greater difficulty, in getting a perfectly distinct image of the buttons on all the planes. It was evident therefore that the indistinctness of the figures of the image on the higher planes, was not the result of the want of focal adjustment but of imperfection in the pattern. The subsequent experiments with the ruled diagram proved this beyond the possibility of doubt. The images in this case were obtained with much more ease and the lines were defined with the most perfect sharpness even when the image was brought nearly to the root of the nose.

In all cases, however, the image when first obtained was a little indistinct, and then gradually became clear. With unpracticed eyes this interval of indistinctness is considerable, but becomes shorter and shorter with practice, until it almost disappears. When the image once becomes clear, it remains so, but there is then a sense, while looking at the image, of gazing beyond it,—or rather perhaps, there is a difference between the image and a real object which we cannot account for, but which is not a difference of distinctness. There is evidently an unnatural condition of the eyes which produces strain and fatigue.

There is but one possible explanation of these phenomena, viz., that the *optic axes and the lenses are adjusted to entirely different distances.* The three adjustments of the eye, viz., the axial adjustment, the focal adjustment, and the contraction of the pupil, have been so associated through successive generations, and the association so confirmed and strengthened in each individual by constant practice from the earliest childhood, that a single act of volition accomplishes them all. Under ordinary circumstances they are so indissolubly associated that neither can be accomplished without the others. But the experiments described above prove that under certain circumstances *the first two,* at least, *may be completely dissociated.* In these experiments when the image is first obtained the optic axes, the lenses, and the pupil, are all consensually adjusted for vision at the distance of the image: and hence the image must be indistinct, for the rays diverge from an entirely different distance. But gradually the lenses adjust themselves to the actual divergence, i. e., for rays diverging from the real object, while the optic axes remain adjusted for the distance of the image. The difficulty experienced in dissociating these two adjustments causes the interval of indistinctness. The perception of the difference between the image and a real object is the sense of this dissociation. Consensual movements have been, perhaps, brought about by the necessities of single and distinct vision ; Helmholtz has shown* that other consensual movements may be dissociated when the necessities of single vision require it ; these experiments show that the consensual adjustments of the eye may be dissociated when the necessities of *distinct vision* require it.

I was now anxious to determine what part was taken by the pupil. Is the contraction of the pupil more intimately associated with the axial or the focal adjustment ? This question has been discussed by E. H. Weber, Cramer and Donders.† Weber believes it is directly associated with the axial adjustment, Cramer and Donders with the focal adjustment. To test this question, while I was obtaining the image and making it clear, an assistant standing behind and a little to one side observed my pupil reflected in a small mirror conveniently placed. After gazing intently at the real object until the pupil was steady, as soon as I converged the optic axes so as to obtain the image No. 1, the pupil was observed to contract decidedly, but as the image became clear *it again expanded to its original size.* Again at the moment of obtaining the 2d image the pupil contracted still more strongly, but as soon as the image became clear it again expanded, nearly, if not entirely to its original size. The same phenomena were observed for each of the images, only that in the nearest images when the convergence of the optic axes was extreme and the first contraction very great, the pupil did not return entirely to its original dimensions.

I then made similar experiments on the image beyond the real object. As before, I looked intently first on the real object at the distance of twelve inches until the pupil became steady. So soon as I gazed beyond the object the pupil of course expanded ; but as soon as the image became clear *it again contracted to nearly its original size.* In this last experiment the pupil is apt to be unsteady. This might have been expected; for as we have already said, it is much more difficult to obtain this image clear or to retain it when obtained.

There is no doubt of the fact, therefore, that *the contraction*

* Proc. Roy. Soc., April, 1864.

† Donders, accommodation and refraction of the eye, Trans., p. 574.

of the pupil is most intimately associated with the focal adjustment.

I believe that this principle of dissociation of consensual adjustments explains perfectly certain phenomena of the stereoscope. It is well known that many persons experience difficulty in seeing stereoscopic pictures distinctly even when the two pictures are brought into perfect coincidence: and I believe all persons experience some fatigue to the eyes in looking at stereoscopic pictures for a considerable length of time. I have often felt both the difficulty and the fatigue, though to a much less degree than most persons. The explanation of this difficulty is as follows. We judge of distance, as is well known by the axial adjustment. If then the two pictures are so taken that, in order to bring them together, the visual lines must meet at a certain distance, say fifty yards, then the picture will be seen at that distance and of course very much enlarged. But in order to see the picture clearly, the rays must come to the eye as if they diverged from the same distance; for the eyes are adjusted for that distance. To fulfil this condition lenses are always used; but it is obvious that a given pair of lenses are suitable for one distance only. For all other distances, or degrees of optic convergence, there must be some degree of dissociation of the two adjustments, and this is both difficult and fatiguing to most persons.

I have found that observations upon the images of the ruled diagram are a most delicate means of determining both the rotations of the eye, and the position of the Horopter. I hope in my next communication to take up this most difficult subject.

(To be continued.)

ART. VII.—*On the Geology of Lower Louisiana and the Rock-salt Deposit of Petite Anse;* by EUGENE W. HILGARD, PH.D., of Oxford, Miss. (Abstract.)

THE discovery in 1862, of a deposit of rock-salt on the coast of Louisiana, was a fact so unexpected to geologists, that at any other time a detailed investigation of its geological relations would quickly have followed the first announcement. The pressing necessities of the blockaded section soon caused its exploitation on the large scale, though in a very irregular manner; for a considerable period, these mines supplied the whole of the southwest. In November, 1865, Prof. Richard Owen made a brief examination of the locality, the results of which he published in the Transactions of the St. Louis Academy. A year later Dr. Charles A. Gössmann, under the auspices of the American Bureau of Mines, made an examination of the locality, mainly with a view to the exploitation of the deposit; his report, published by the Bureau, as well as the specimens which he courteously exhibited to me, confirm previous conjectures that the overlying strata were the equivalents of the formation I have described as the "Orange Sand" of Mississippi. I therefore gladly availed myself at the earliest possible moment, of the offer of the Smithsonian Institution to defray my expenses in making a detailed geological investigation of the region. The low stage of water prevailing at the time (December, 1867,) rendered it possible to observe to the best advantage the formations exhibited on the banks of the Mississippi; the examination of which, from Vicksburg to the Passes, was a needful preliminary step to the determination of the formations of the coast.

Having previously examined and described the sections exhibited at Vicksburg, Grand Gulf and Fort Adams,* I merely landed at some intermediate points to verify the conclusion previously reached, viz., that below Vicksburg, no marine formation crops out on the river banks, reports to the contrary notwithstanding; and that the profiles at Natchez, Rodney and other points are essentially similar to that at Fort Adams, where we find the strata of the (fresh-water) "Grand Gulf group" in a position nearly or quite horizontal; overlaid, first by the materials of the "Orange Sand," which in its turn is capped by the stratum of the "Loess" or Bluff formation, covered by a thin deposit of "Yellow Loam."

Facing southward from the "Blockhouse hill" at Fort Adams, we observe a wilderness of the characteristic sharp ridges of the Loess region, often fore-shortened into veritable peaks, elevated between 300 and 400 feet above the river. In this region, the Grand Gulf strata have been traced southward by Dr. George Little, the present State geologist of Mississippi, as far as the head waters of Thompson's Creek, northwest of Clinton, La.

The Orange Sand proper is visible, near the river, as far south as Jackson, La., but farther inland extends to a lower latitude. As for the Loess, it appears in full force and characteristically developed for some distance south of Fort Adams. But (according to Dr. Little's observations) these features become gradually modified as we advance southward. The Loess deposit thins out, its materials become poorer in lime and fossils, and assume more and more the character of a common fine grained "hardpan;" the transition being by insensible de-

* See Report on the Geology and Agriculture of Miss., 1860.

grees, while the two extremes are very obviously distinct. At the same time, the clayey substrata which, farther above appear only in patches (as at Nevitt's bluff, two miles above Natchez, as well as at the latter place itself) are seen more frequently and continuously, until, at Port Hudson, they become predominant.

The exposure at Port Hudson, previously examined in part by Bartram, Carpenter and Lyell, is about three miles in extent, from the mouth of Sandy Creek above the town, to Fontania Landing, $1\frac{1}{2}$ miles below. Its lower half is washed, and continually encroached upon, by the river ; its upper portion is now inland of an extensive sandbar. The strata are disposed horizontally or basin fashion, and vary a good deal both in thickness and materials, as shown in the subjoined profiles, situated about a mile apart ; the correspondence of strata is ascertained by actual tracing of the stratification lines.

Near Sawmill, Port Hudson.	No.	*Midway betw'n Port Hudson & Fontania.*
Yellow surface Loam, 4—6 ft.	6	Yellow Loam, sandy below; 8—10 ft.
Yellow Hardpan, ---------- 25 ft.	5	White and yellow hardpan, -----18 ft. Orange and yellow sand, sometimes ferruginous sandstone, irregularly stratified, ------------------8—15 ft.
Heavy greenish Clay, -------- 7 ft.	4	Heavy, greenish or bluish Clay, --7 ft.
Gravel, sand and clay in irregular bands, like river alluvium; with pebbles, driftwood, leaves, and Mastodon bones, ----------6 ft.	3	White indurate silt or hardpan, 18 ft.
Heavy, greenish or bluish, massy clay, similar to No. 4; 25 ft. visible.	2	Heavy green clay, with porous calcareous concretions above, ferruginous ones below; some sticks and impressions of leaves, -------------30 ft.
	1	Brown muck, White or blue clay, } with cypress stumps, 3–4ft.

At the stage of extreme low water prevailing at the time, the stump-stratum No. 1 was visible to the thickness of 10 ft. at its highest point ; showing several generations of stumps above one another, also the remnants of many successive falls of leaves and overflows. The wood is in a good state of preservation ; no prostrate trunks to be seen at present.

The main clay deposit, No. 2, varies but little in general character ; although very solid, its tendency to cleave into prismatic forms renders it very liable to "cave" into the river. The upper portion of the stratum, especially near its southern end, contains strings of calcareous nodules, on stratification lines eight to twelve inches apart. No fossils save rare impressions of leaves.

No. 3 is exceedingly variable. At the northern end of the outcrop, it is a narrow band of swamp deposit; at the first of the profiles given, it bears the character of a sandbar; lower down, it returns to that of a swamp deposit; still below, it is represented by a fine white silt, without a trace of vegetable remains. Lower down again, a lignitic layer appears at its base, with leaves and fruit of living species of lowland trees; while near Fontania, it is again a sandbar, with an abundance of prostrate trunks of driftwood, coarse sand and pebbles.

The green clay stratum No. 4 varies little, either in thickness or composition, and like the stump-stratum No. 1, forms a convenient level of reference.

The hardpan stratum No. 5 I conceive to be the more immediate representative of the Loess proper, with which it is connected by gradual transition, though at times greatly resembling some of the materials of the Orange Sand. It is void of fossils.

The present profile differs in many respects from those given by previous observers, which lay some distance farther west, where the river now flows. The strata are accordingly as variable in an east and west, as in a north and south direction, and with the exception of Nos. 1 and 2, are such as are now shown in ditches cut into the modern river-bottom deposits.

The stump-stratum No. 1, however, as appears from numerous data collected by myself or contained in Humphreys and Abbot's Report on the Mississippi river, exists at about the same level (i. e., near that of tide-water) not only over all the so-called Delta-plain of the Mississippi, but also higher up, perhaps as far as Memphis, and all along the gulf coast, at least from Mobile on the east to the Sabine river. Wherever circumstances allow, the overlying clay stratum No. 2, is also observed. These facts indicate the wide spread prevalence, during the epoch succeeding the drift, of quiet, shallow fresh-water lagoons and swamps of slightly varying elevation; through which the continental waters may for some time have found an outlet without a definite channel representing the Mississippi of to-day. The Port Hudson profile appears to be typical, its features being reproduced wherever denudation has not removed these deposits down to the level of the stump-stratum, as is mostly the case.

The Five Islands.

The chain of five islands rising partly from the sea, partly from the coast marsh, between the mouth of the Atchafalaya and Vermilion river, have been described by Mr. Thomassy,*

* Géologie pratique de la Louisiane; New Orleans, 1860.

who attributes their origin to "hydrothermal" or "volcanic" action. His descriptions are sufficiently faithful to show the general resemblance of their geological structure; so that after visiting the three middle members of the chain, viz: Côte Blanche, Weeks's Island, and Petite Anse, I have thought it superfluous to extend my examination to the two extreme ones, viz: Belle Isle, the promontory west of Atchafalaya Bay, and Miller's Island (or "Orange Grove,") overlooking the plains of the Vermilion. These elevations lie nearly in a straight line bearing N.W. by W. from Belle Isle.

Côte Blanche.

The next in order, affords on its sea-face a fine exposure of the lower members of the Port Hudson profile. At tide-level, we have the blue clay with cypress stumps, the tops of which are often surrounded by alternate layers of clay, muck and sometimes lignite. The overlying strata consist, partly of blue clay similar to No. 2 at Port Hudson, partly of various colored loams alternating with the former; and exhibiting the same calcareous or ferrugino-calcareous concretions along the stratification lines. At a few points, these calcareous concretions resolve themselves into distinct fossils, representing the fresh-water genera *Paludina, Melania, Unio, Anodonta* and *Cyclas,* in an indifferent state of preservation. The entire visible profile is about 50 feet high; the highest point of the island rises as high as 180 feet, but in its interior no exposures exist, so that the higher members of the series are not verifiable.

Weeks's Island.

This island, lying 6 miles N.W. by W. from Côte Blanche, has an area slightly greater, viz., 2,300 acres; it is nearly circular; maximum elevation 160 feet above tide water. Unlike Côte Blanche, it is traversed by deep ravines which exhibit the geological structure. In the central and highest portion, these gullies are bordered by steep slopes composed of the most characteristic materials of the Orange Sand group. On the exterior slopes, however, we find in a position inclined away from the center of the island, the lower strata of the Port Hudson profile—green or blue clay with calcareous concretions, and imperfect fresh-water shells. The blue clay stratum with cypress stumps is met with in ditching, and is also known to exist in the beds of the neighboring bayous, as well as in the surrounding marsh.

Petite Anse, or Avery's Island.

Petite Anse lies about 12 miles N.W. by W. of Weeks's Island, and in its general structure much resembles the latter, to which it is slightly inferior in size, and about equal in elevation; its highest point, "Prospect Hill," on the north side, being 160 feet above tide-level.

An elevated ridge connects Prospect Hill with another high point near the southern slope of the island; and near the west end a ridge, on which Judge Avery's house stands, falls off steeply toward the Bayou Petite Anse. These three points inclose the valley in which the salt deposit has been found and which opens southeastward into the marsh.

The topography of the island, as well as the history of the mine, have been ably given by Dr. Chas. A. Gössmann of Syracuse, in a report of the American Bureau of Mines.* Up to the time of his visit, all the pits and shafts had been sunk through detrital strata, washed down from the adjoining hills, and frequently inclosing the vestiges of both animal and human visits to the spot. Mastodon, buffalo and other bones; Indian hatchets, arrrow-heads and rush baskets, but above all an astonishing quantity of pottery fragments, have been extracted from the pits. The pots doubtless subserved the purpose of salt-boiling; human handiwork has, however, been found so close to the surface of the salt, as to render it likely that its existence in mass was once known, before the time when, in 1862, Mr. D. H. Avery struck the salt itself at the bottom of a salt water well.

The surface of the salt undulates considerably, so that borings commenced at different levels have repeatedly struck salt at nearly the same relative depth, the absolute level of the rock-salt surface varying from 32 ft. below to 1¾ ft. above tide level. The salt stratum has itself been penetrated to the depth of 38 feet, without any perceptible variation in quality; its "floor" being as yet unknown. Dr. Gössmann's observations and specimens proved to his and my satisfaction, the existence of the Orange Sand on the Island; but its relation to the rock-salt, and the age of the latter, remained undetermined.

Since then, another shaft has been sunk by Mr. Chouteau of St. Louis, with the assistance of Mr. Dudley Avery, to whom I am indebted for a record of the strata penetrated. This shaft was located at a higher level than any previously sunk, on a hillside where, not far off, the Orange Sand crops out *in situ*. After passing through these strata, the rock-salt was struck again, at a level several feet higher than on any former occasion.

* On the rock-salt deposit of Petite Anse, New York, 1867.

There can therefore be no doubt that the salt deposit is older than the Orange Sand, which here as at Weeks's Island, forms the nucleus of the mass on whose outer slopes, as well as its higher points, the strata of the Port Hudson profile reappear characteristically; with calcareous nodules, fresh water shells and aquatic plants identical with living species. Not only is the reference level of the cypress stump stratum the same as elsewhere, but the green clay band, No. 4 of the Port Hudson profile, is also there.

The stratigraphical disposition of these deposits is quite remarkable. They conform not to the *strata*, but measurably to the *outline* of the Orange Sand nucleus, roughly following its slopes and curvatures. At first sight therefore it seems as though a local upheaval had taken place, and hence arose, probably, the reports attributing a volcanic origin to these elevations, whose isolated position in the level coast region would naturally give rise to speculation as to their mode of formation. Indeed the extent to which these strata are sometimes seen to dip, rather staggers the observer; but the upheaval hypothesis does not explain the facts, unless we are content to assume a separate effort of the sort for every hillock on the islands.

There can be no doubt that subsidence subsequent to deposition has been the cause of the extravagant dips observed sometimes. Where the Port Hudson series is more immediately superimposed upon the Orange Sand nucleus, the dips are moderate, and such as may well be assumed as resulting from deposition on inclined surfaces. But when we see an apparently undisturbed clay-stratum moving down hill like a glacier, so as to overflow a deposit of loose stones, we need not go far to find the cause of extensive dislocation and subsidence.

Belle Isle and Miller's Island.

All the data I have been able to collect concerning the structure of these exterior islands, tend to confirm the probable supposition that, like the three interior ones, they consist of denuded nuclei of Orange Sand materials, upon which the Port Hudson series was afterwards deposited.

It seems likely that the same is true of a low ridge called Côte gêlée, in Lafayette parish, bearing N. or N.N.E. Thomassy places in the same category the Grand Côteau des Opelousas and the Avoyelles prairie.

Age of the Salt Deposit.

The Orange Sand strata so rarely approach the coast, that the deposits underlying them in the Coast region have scarcely been observed with certainty. Even the older strata underlying the blue stump clay have been observed at a few points only, viz: by the Delta Survey in the bed of the Mississippi river at Bonnet Carré and Carrollton, near New Orleans; at the latter city itself, in the boring of wells; at Salt Point, on Bayou Salé; and on the coast of Mississippi Sound.

The strata penetrated in the borings at New Orleans are considered by Sir Chas. Lyell as Delta deposits. But according to my examination, they are almost throughout demonstrably of marine origin, and while the species they contain are mostly (not all) now known to be living on the Gulf coast, yet the prevalence of species is very different from that now observed near the mouths of the Mississippi. In this respect, the fauna of these strata shows a great analogy to those described as Pliocene by Tuomey and Holmes, occurring on the Carolina coast.

It is most probable that the rock-salt of Petite Anse will be found when pierced, to be imbedded in the equivalents of the deposits penetrated at New Orleans and Bayou Salé, and of corresponding, probably early quaternary age, anterior to the drift or its southern representative, the Orange Sand.

Origin and extent of the salt deposit.

The absence of layers of the usual impurities of rock-salt, especially of gypsum, has induced Dr. Gössmann to suppose that it is not the result of the evaporation of sea-water, but owes its formation to crystallization from the purer brine of salt springs.

Our knowledge of the facts is still too limited to render a discussion of this point very profitable. In a very deep lagoon, withdrawn from the influx of the tides after the brine had acquired a considerable degree of concentration, all the gypsum might be found in a single bed at the bottom; upon it a large mass of pure salt, as in the present case; while the salts of the mother-waters would naturally have been washed away from the top. Or there might have been a succession of lagoons communicating with each only during high tides, and acting in a manner analogous to the process now practiced in salt-making on the sea-shore. The gypsum would then all have been deposited in the outer lagoons, while the inner ones would have acted as brine-pits, where pure salt alone could crystallize. Crystals of gypsum have repeatedly been found in shallow wells on the coast, beneath the "stump clay."

Upon any of the foregoing suppositions, calling into play a variety of circumstances not likely to be all simultaneously fulfilled, it does not seem probable that the rock-salt mass is very extensive horizontally, or that such masses should occur frequently in the coast region.

A mass of salt 144 acres in extent and 38 feet thick is, however, a handsome specimen, even if these dimensions should represent maxima. The great difficulty in mining it, heretofore, has been the influx of water through the gravelly strata overlying. But it has most probably been attacked, thus far, at its lowest surface level. Wherever elsewhere the Orange Sand formation prevails, it rests on a deeply denuded surface; and "hills within hills" are of very common occurrence. From the data thus far obtained it appears that the same is the case with the rock-salt mass, and that its surface roughly conforms to the hills and valleys now existing. Workings should be begun at higher levels; and it would not surprise me to learn that the auger had shown the mass to be accessible by level adits in lieu of shafts, on the hillsides. The interior of the solid mass once gained from a point secure from surface water, all difficulty would be at an end.

Geological History of the Lower Mississippi Valley.

It appears from the facts stated in the preceding pages, that after the termination of the epoch of that Eocene period, represented by the Vicksburg group of fossils, down to the Quaternary era, marine deposits ceased to be formed on the northern border of the basin now represented by the Gulf of Mexico.

I have acquired the certainty of the existence, over a large portion of northern Louisiana, of the "Grand Gulf" series of rocks. From specimens in the collection of the New Orleans Academy of Sciences, it appears that apart from the usual materials forming these beds in Mississippi, they assume in the Harrisonburg region the character of compact limestone, which in places is said to be fossiliferous, and would thus furnish the clue to the age of the Grand Gulf group, for which I have vainly sought in Mississippi. The problem is one of great interest, as it involves the question whether or not the Mexican gulf has, within comparatively modern times, been disconnected from the Atlantic ocean. The absence of the cauldron in which the Gulf Stream is concocted might have exerted climatic influences reaching beyond the American continent, and would explain many discrepancies between ancient and modern faunas on the shores of the Atlantic.

It appears that similar limestones, almost assuming the character of black marble, occur in St. Landry parish, near Opelousas. Whether the southern outline of the formation passes thence toward the Calcasieu region, where petroleum has been found, or whether it trends northwestward into the parishes of Sabine and Natchitoches, where limestone and sandstone ridges also exist, is a question still open. In the latter case, this outline would conform to the general shore lines of the great cretaceous and tertiary Mediterranean.

In Mississippi, the Grand Gulf series is mostly overlaid by the Orange Sand, deposited on a deeply eroded surface, and bearing itself the evidence of its formation by fresh water in a state of violent flow.* The southern outline of the main body of the Orange Sand runs southward of Opelousas, toward the mouth of the Sabine, whence, according to reliable information, a broad band of shingle extends toward Harrisonburg, Catahoula parish. This belt represents, probably, the most westerly bayou of the great Orange Sand Delta; while, as heretofore stated, the most easterly one extends from the neighborhood of Cairo along the western shore of the Tennessee river, down the valley of the Warrior toward the coast of Alabama. The middle and main pebble-stream evidently follows in general the course of the Mississippi river; but leaving it at the point where that river suffers its remarkable deflection eastward, we find the remnants of its ancient "bar" in the chain of the "Fire Islands," which lie directly across the shortest line by which the Mississippi could reach the Gulf, and no doubt have had their share in causing this deflection.

Both the size of the pebbles carried by this middle bayou, and their character proving transportation from high northern latitudes, show it to have been the main channel during the Orange Sand epoch. It is not surprising, therefore, that in the direction of its course the Orange Sand formation should extend farther south than anywhere else. The pebble-beds are now overlaid by fine sandy materials, proving a diminished velocity, owing, doubtless, to a general depression, but greater at the north than at the south.

While the lateral bayous descending through Louisiana and Alabama were closed at the end of the Orange Sand epoch, it is evident that the central channel continued open; inasmuch as the next succeeding deposit, viz: the Loess, lies in a trough-shaped depression of the Orange Sand materials, the line of contact being always conformable and devoid of any trace of atmospheric denudation. The perfect peroxydation of the materials of the Orange Sand would seem, nevertheless, to point to a certain period of exposure to atmospheric agencies, caused by a temporary diminution of the influx of northern waters, through the cessation of subsidence, perhaps.

During this epoch of quiet might have begun the formation of those extensive swamp and lagoon deposits, the lower mem-

* Am. Jour. Science, May, 1866; Miss. Rep. 1860, p. 26 and ff.

bers of the Port Hudson series, whose floor stratum, with its superimposed generations of cypress stumps, indicate a slow secular subsidence. The velocity of the latter seems gradually to have increased until the growth of old trees became impossible, and finally, in stratum No. 3 of the Port Hudson profile, we again meet the evidences of currents moving sand, pebbles, and drift-wood.

Then follows the Loess proper, a deposit utterly devoid, in Mississippi and Louisiana, of any evidences of fluviatile action —a uniform silt even in profiles of 80 feet, with scarcely a vestige of stratification, and none but terrestrial fossils.

The precise circumstances under which such a deposit could be formed, are perhaps a little obscure. There must have been such a depression of the whole country as to transform the immediate valley of the Mississippi, as far as Keokuk, as well as the valleys of the larger tributaries, into estuaries of the Gulf of Mexico, containing a mass of water too great to be sensibly affected by the variations now causing the annual overflows of those rivers (for otherwise the deposits must have shown lines of deposition), yet possessing a gentle flow above (since the materials of the bluff formation of Missouri and Indiana exhibit signs of fluviatile action); quite fresh in its upper portions (where fluviatile shells are found), but rendered unfit for the life of either a fresh or salt-water fauna by an admixture of sea-water, in its lower and almost stagnant portion, at tide level; and deriving its vestiges of animal life only from the "offscourings" of the adjoining unsubmerged lands.

Sir Chas. Lyell* inclines to consider the Loess as the product of "successive inundations of a great river," the absence of stratification from such deposits having, apparently, an analogue in the alluvial deposits of the Nile. But the case is far from being analogous; for the same phenomena are still observed in the modern deposits of the Nile, and are clearly attributable to the peculiarities of the hydrographic basin of that river; whereas, in the modern alluvium of the Mississippi, it is exceedingly difficult to find a uniform stratum two feet in thickness. The Nile mud is each year derived from the same rivers of Abyssinia, and equalized by intermixture and subsidence during at least 1,500 miles of its course. On the Mississippi, on the contrary, the deposits of different annual inundations are readily distinguished by the inhabitants for years afterward, according as the Illinois, the Missouri, Ohio, Arkansas or Red river happen to have furnished the main influx. The absence of any such differences from the Loess can only be explained on the assumption that the mass of

* Principles of Geology, 10th edition, p. 464.

water filling the channels was too great to be sensibly affected by such causes, the more so as the continental surface was sensibly diminished in consequence of a depression which, as far south as Fort Adams, cannot have been less than 400 feet, and on the coast not less than 200, but more probably the same as farther above.

The existence of the elevations on the Louisiana coast, above described, renders it necessary to assume that at the end of the period of depression—the "Champlain epoch"—the entire delta-plain (so-called) west of the Mississippi was covered by the deposits of the Orange Sand and Port Hudson series to an equal height; and that during the succeeding "Terrace epoch" of elevation, the veritable Mississippi—*our* Mississippi—swept away these deposits in excavating its present valley. At first it might sweep over or through the pebble ridge, but would finally turn to the direction of least resistance, leaving the "Five Islands" high and dry.

It would thus seem that, unlike other large rivers of the world which have from the outset added to the land by bringing down the materials to form their alluvial plain, the Mississippi has first formed by denudation the plain which it was subsequently to cover with its alluvial deposits to a comparatively inconsiderable depth. The western and southern limits of this denuding action would seem to be marked by the Grand Côteau des Opelousas, the Côte Gêlée and the Five Islands; and the materials swept away from this area doubtless contributed largely to form the foundations of the truly alluvial plain extending south and southeastward of lakes Maurepas, Pontchartrain and Borgne.

It is obvious how futile must be all attempts to estimate the age of the Mississippi river in absolute measure, by a comparison of the advance of its present delta into the Gulf, with the distance of its mouth from the divergence of bayous Plaquemine or Manchac. When the broad flood of the Terrace epoch contracted into the present Mississippi, that stream emptied into a sea rendered shallow by the deposition, within a comparatively short period, of a huge amount of material. Within such a sea its channel would be likely to change about, somewhat like those of the great rivers of China. *Now,* it is advancing into the deep water of the Gulf of Mexico, but at a very different rate, and by a very different process from that of simple alluvion. But the questions pertaining to this portion of the subject, together with the results of my observations in the Delta proper, I propose to discuss at a future time.

ART. VIII.—*Notes on the recent volcanic disturbances of Hawaii;* by Rev. TITUS COAN;* (from a letter to J. D. Dana, dated Sept. 1, 1868).

I HAVE recently returned from a tour of eighteen days in Puna and Kau, during which time I visited all the important points of interest, took measurements, and made careful observations. I take the liberty to give you a few facts and remarks from my notes.

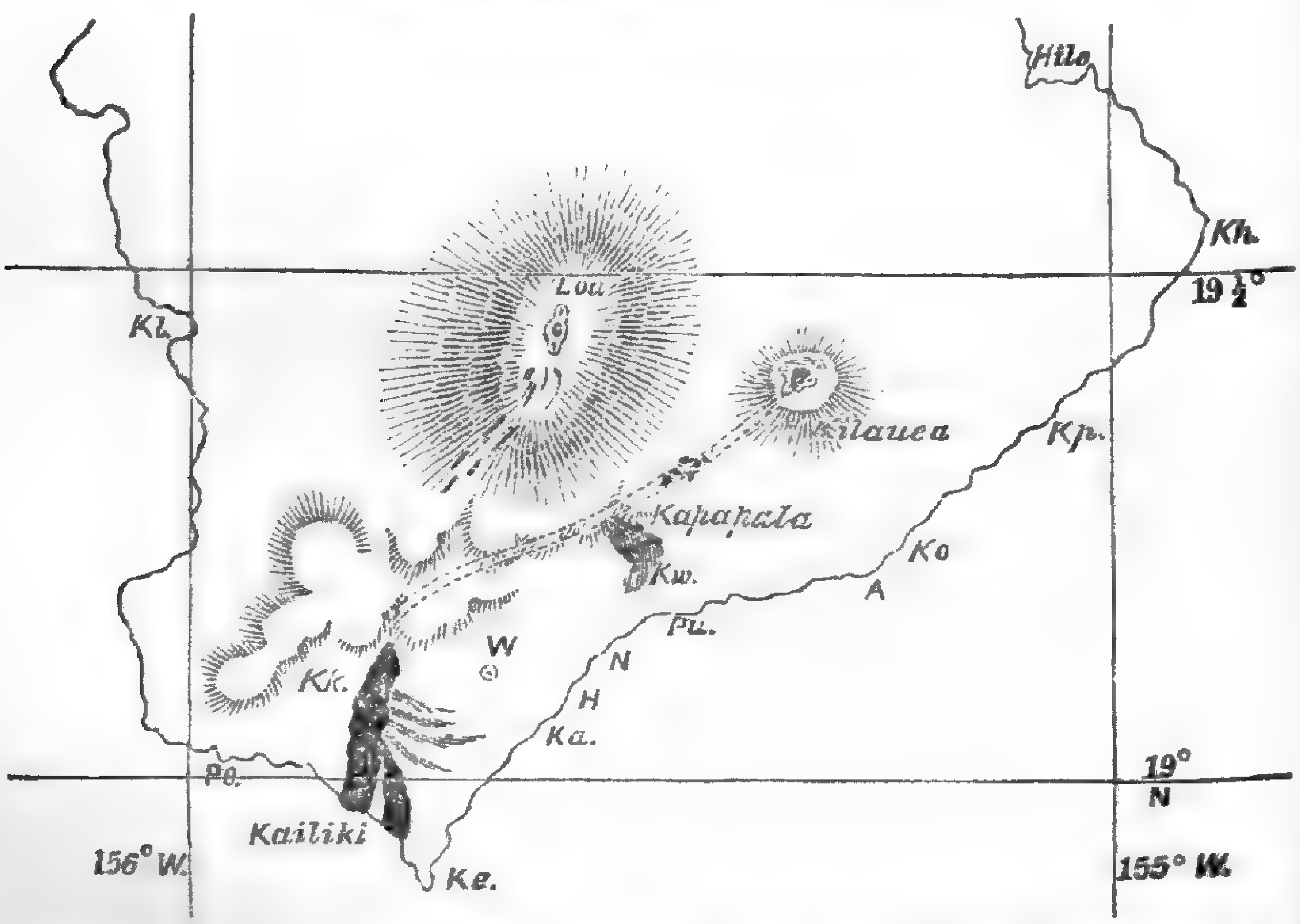
I left Hilo on the 4th of August and took the shore road through Puna. From Hilo to the east cape, Kapoho, the disturbances were not remarkable. Walls and rocks had been, more or less, shaken down, and fractures appeared, here and there, in the earth. From the east cape to the south point of the island, Kalae, the disturbance had been great. The whole coast line appears to have undergone a subsidence of unequal depth. At Keahialaka, about seven miles southwest of Kapoho, where there has been a small pool of brackish water, passed by a causeway of stones, the water now stands, at high tide, three to four feet deep, and spreads out among cocoa-nut groves where water was never seen before. At Kaimu, the sea has driven in a heavy beach of lava sand for two hundred feet into a beautiful young cocoa-nut grove, and spoiled a delightful lawn. At Kalapana, the sand beach has been forced into groves of Pandanus and cocoa-nut, where the trees now stand six to eight feet deep in sand, and the shore line has been pressed back the distance of 100 feet. The old stone church is nearly buried in sand, and the tide rises and falls within its walls. t formerly stood 200 feet from the water. The plain of Kala-

* Mr Coan also sends us the following by way of correction of our newspaper extracts in the July number of this Journal: "Article XIII of the Journal of Science for July has just come to hand. I regret that several important errors have been published and very widely circulated. But this could hardly be avoided in times of such terrible excitement, when exaggerated reports filled the air, and the honest eye and ear sometimes failed to report the exact truth. I am surprised to find myself reporting 2,500 to 2,600[1] head of cattle destroyed. Did I state that? If so, I stated just what was reported to me. The truth now appears to be, that on Kahuka, where 200 cattle were said to have been roasted in a holocaust, by a puff of Pele, only 35 perished, as I learn from the owner. And on the Kapapala ranch, one of the partners sets the number at 1,500, the other at 500, while some of the neighbors would reduce the loss to 200. But on a ranch of 5,000 cattle, spread over all the southeast and eastern flanks of Mauna Loa, it is impossible to tell the exact number destroyed by the landslide. The statements, also, that a vast lava stream went, under ground, into the sea at Punaluu, and that another broke out at Hiilea, were optical illusions of Mr. Lyman and many others. They spoke and wrote what, at a distance, appeared to them as facts, but which proved to be errors on careful inspection. So also the report, that the sea-waves rolled over the tops of cocoa-nut trees, sixty feet high, was an error by two-thirds.

[1] In my letter published in the Missionary Herald for July, you will see that I say 500 to 600 head of cattle, which is nearer the truth."

pana has sunk four to six feet, and many acres of once dry land are now covered with water three and four feet deep. Bathing coves, also, once having spaces of three, four and six feet between the water and the roof, are now full or nearly so. At Kealakomo, the salt works are all destroyed, and the spring of cold water among the rocks near the shore is sunk. At Apua, the most western village in Puna, all the houses, the crescent sand-beach, and the beautiful little canoe harbor are obliterated—the fishermen mourn. Water now stands where the village once stood. The same is true of Keauhou, the most eastern village of Kau. The subsidence there is seven feet by measurement upon cocoa-nut trees. This place has an ample and safe anchorage, and here was the station where large quantities of pulu were dried, pressed and shipped for market. The influx of the sea destroyed the buildings, but the proprietors have built again on higher rocks.

PART OF THE ISLAND OF HAWAII.



A, Apua. H, Honuapu. Ka, Kaaluala. Ke, Kalae. Kh, Kapoho. Kk, Kahuka. Kl, Kealakeakua. Ko, Kealakomo. Kp, Kalapana. Kw, Keaiwa. N, Ninole. Po, Pohue. Pu, Punaluu. W, Waiohinu.

In passing from Puna to Kau we took the high trail from Panau to Kapapala, a distance of about thirty miles. This whole route is a dreary wilderness, without a human habitation, a drop of water, or any thing to refresh man or beast. The track at first lies over *pahoehoe*, sand and *aa*, sprinkled with small trees and bushes; but the latter half of the way is upon open fields of lava and sand, and under a burning sun, with nothing to break his fierce rays. The road flanks Kilauea at a distance of nine miles on the south side. At one place we rode for miles on the verge of a precipice about 100 feet high, the wide region below us, on our right, giving evidence of a vast subsidence during some former age.

As soon as we left the region of trees and shrubs, and came into the sweep of the trade-winds, the sulphurous smoke from Kilauea filled the atmosphere, and was almost stifling to horses and riders. A dense cloud of gases and smoke spread over the country, and hung along the hills and over the plains, as far as the sea-shore at Punaluu. We now came upon the great lava fields of 1823, which are studded with conical tumuli, and more or less fractured with old fissures. But the late earthquakes, and the subterranean flow of the fires of Kilauea, have made strange work of the superincumbent strata. The old road is full of yawning cracks, and impassable. We were, therefore, obliged to deflect to the left and make a detour of a long distance from the track, in order to pass this terribly rent region. Our cavalcade numbered sixteen, all natives but myself, going to Keaiwa to attend our meeting of Association; and had it not been for an expert guide who marched us north, south, east and west; who zigzagged us, taught our horses to leap fissures, and who performed a score of necessary evolutions, we could not have passed that region of marvelous fractures, and of fissures from one to twenty-five feet wide. At last we were all crossed over. This done, I took the guide, and while the rest of the company went on, we turned due south, upon the *pahoehoe,* in search of a disputed outburst of fresh lavas which some affirmed and others denied had been deposited there. After an hour's search amidst this wild sea of ancient lavas, our guide, by mounting a ridge of *aa,* descried steam in the distance, and we were soon upon a patch of warm and steaming lava 1,000 feet long, 600 wide, and four to twenty-five feet deep, with an uneven, ropy and tumulated surface. This had been thrown up from an open fissure; below it, on the same line of rents, four more patches had been thrown out, of less areas. We now traveled to the west till we found the old trail and reached the ranch at Kapapala, where I spent the night. Here I examined the effects of the 2d of April. The yards, the plains and the hillsides were full of rents and seams; and in one place the earth opened and closed again, and in closing, one side of the rent surface lapped over the other, like the flap of an amputated limb. From Kapapala onward to Waiohinu the flanking hills and spurs of Mauna Loa were torn and scarred and striated and grooved by landslides of greater or less extent. Avalanches of rocks and patches of soil, with trees, shrubs and grass had been sent down from the steep hills everywhere.

Some of the steeps had the appearance of having been plowed in deep and broad furrows, leaving wide belts of green between them, which resembled rows of cane or hedge. The general trend of the fissures, from Kilauea to Kapapala, and onward to Kahuka, in western Kau, is southwest, running under the foot-hills and spurs of the mountain, as they rise, in an advancing and retreating line of verdant beauty and lofty grandeur, on the right of the traveler passing on the main road from eastern to western Kau.

Along this whole line we find the greatest disturbances. From the fiery jets which were thrown up along this line, on the *pahoehoe*, called *Nuku pili*, and from the awful shakings and rendings all along these hills, as also, from the testimony of multitudes, that they heard hissings under ground, and thumpings as of the striking of waves under a ship, we may conclude, that Kilauea sent off her burning messengers in this direction; and that her fires united with those of Mauna Loa and burst forth in fury at Kahuka. The testimony of many, that steam issued from several points of the fissures along this line, gives still greater probability to this view.

And this is the only probable theory to account for the exit of the vast molten floods which commenced to leave Kilauea, subterraneously, on the evening of the 2d of April, immediately after the great shock, causing a subsidence in the crater which continued until the 7th of April, when the vent opened at Kahuku, some forty miles to the southwest. As there is no proof that the fires of Kilauea went into the sea at any other point, and as there is positive evidence that they went fifteen miles, subterraneously, in this direction; and as the mountain and Kilauea were relieved, and their camp-fires extinguished at the same time, it is reasonable to infer, that they were in sympathy, acted in concert, and found final vent together at Kahuka.

While at Keaiwa I made a careful examination of what has been, erroneously, called "The Mud Eruption" and "The Mud Flow." I went entirely around it, crossed it in three places, and measured it. The length is just three miles, the breadth, in the center, half a mile, and at the head, where the cleavage took place, one mile. The depth is various, averaging about six feet on the grass plains, but deepening to twenty, thirty and forty feet in some of the gorges and depressions. It is nothing more or less than a landslide, having none of the characteristics of a mud eruption. You are aware, that the hills lying back of Kapapala and Keaiwa, and sweeping around to Waiohinu and beyond, are very steep, sending down lofty and beautiful spurs upon the grassy plains below, and buttressed by bold cones and headlands. Between these hills and the shore there is a gently inclined belt of land, four to six miles in width. On the steep highlands, above these precipitous hills, between Kapapala and Keaiwa, and in the forests of trees, ferns and jungle, the earth was terribly rent, on a line parallel to the shore, or northeast and southwest, and the face of the hills was shaken off, leaving a bold precipice, or wall, twenty to sixty feet high. The whole mass, below this wall,—earth, water, boulders, rocks, lavas long buried, trees, logs, etc.,—slid, rolled, pitched and tumbled down a steep incline—an upper terrace—until coming to a pali about 1,000 feet high, and on an angle varying from thirty to seventy degrees, it plunged down this fearful steep, and constantly gathering momentum, it rushed across the plains below by its own gravity, at the rate of more than a mile per minute. It was not mud, though there was much water in the caverns of these hills ; and where this became mixed with the soils in the descent, it formed mud of course, as earth and water mixed always do. But the mass was the superincumbent strata of the hills, as the earthquake shook them off ; and by sliding, rolling and plunging, under the force of gravity, all these materials were mixed up in one vast conglomerate mass. That this mass was not all mud is evident from the fact, that the whole atmosphere above and near it was filled with dry dust ; and that it was not exploded by steam or gases, appears probable, not to say certain, from the fact, that no steam was seen, and no heat was evolved. The whole mass was in a natural cold state. From reports, I had supposed the eruption to have been projected from the hills by some explosive force ; but a careful personal observation satisfies me, that this was an error, and that gravitation alone was the motor which propelled it, after the earthquake had rent and detached it from the parent hills. It now looks like a great plowed and harrowed field, and the taro, banana, fern and grasses are shooting up on its surface, while the natives are beginning to plant upon it.

From Keaiwa I went down to Punaluu, and thence along the shore to Honuapu. All is wreck and ruin along this coast. Churches, dwellings, yards, causeways, roads, fish-ponds, canoes, fish-nets, tackle, gear, furniture, and all things movable, were destroyed. In many places the road is sunk, or so obliterated with debris, that, without a guide, I could not have threaded my way along a shore and through villages once so familiar to me.

At Punaluu and Honuapu, I took correct measurements, on palms and on the ridges of *aa*, of the height of the earthquake waves of April 2d. The greatest height was twenty feet. The sand-beach at Punaluu was swept out to sea by the receding waves, and the beautiful pond of cold water filled with the ocean, and apparently blotted out forever. At length, however, the sea brought back her spoils and formed another sand barrier, more than 100 feet within the old line, leaving a few pools of water inside, and protecting them from the ocean surges. But the great, deep, cool and beautiful fish-pond is not there in its normal state.

The same is true at Honuapu and all along the coast.

I spent a Sabbath at Waiohinu, and surveyed the sad ruins of the place. Landslides, rents, scars and evidences of terrible disturbances were everywhere visible. A congregation of some 250 gathered in a beautiful kukui grove and listened with great attention to the word.

On Monday the 17th of August, I started, a little after sunrise, to visit the lava eruption of the 7th–10th of April, at Kahuka and vicinity. Capt. Brown, who keeps the Kahuka ranch, and who was driven out with much peril on the 7th of April, with his wife and six children, kindly offered to accompany me. Our course was southwest. After riding three or four miles, we crossed the first branch of the flow, a high ridge of *aa* some two miles long, and lying like a vast serpent upon the beautiful pasture lands of the people. Passing this, we rode about half a mile, and then crossed another stream of similar character and dimensions. These were each about 500 feet wide and fifteen to twenty-five feet deep. The next, or third, spur we crossed was near half a mile wide and some three miles long, from where it left the trunk stream. We soon crossed a fourth branch, which resembled numbers one and two. The general direction of these lateral streams was southeast, but varying of course. Their united breadth would be about one mile. The largest branch ran toward the landing at Kaalualu, and threatened the harbor. Having passed these four branches, we rode rapidly down the open and beautiful incline, to the great precipice at Kailiki, about one mile north of Cape Kalae, and close to the shore. Here, at a height of 400 feet, we had a grand bird's-eye view of the two main streams, which had flowed parallel with the precipice and with each other, for five miles on the old *pahoehoe* fields, 400 feet below this remarkable pali, which stands as a great rampart, forming the western line of the high plateau of grazing and cultivated fields extending to Waiohinu. This pali lies nearly north and south, extending from the south cape to the base of Mauna Loa. The cooled and shining streams which lay below us are each about half a mile wide, and are *pahoehoe*. They ran smoothly into the sea, say 1,000 feet apart, inclosing an island on three sides, about five miles long. The ocean line completes the island. On this narrow belt of land, three houses are standing near the shore, and here, also, Capt. Brown found and rescued some thirty head of cattle, after the flow had ceased. Several cattle died, and those which survived were terribly heated by the inclosing fires.

The features where the lavas entered the sea are like those at Nanawale in Puna, and like many other places. There was, probably, a small conical island formed a little distance from the shore, soon after the stream went into the water; but this soon became a cape, or headland, by the continuous pouring in and filling up between the cove and the shore. The coast line is not much extended, and the two sand coves are much smaller than those of Nanawale in 1840.

From the termini of these streams we rode up on the high precipice, having the two streams, with the long island, under us and in full view on the left, until we came to the great trunk, or parent stream, from which all the lateral branches had been sent off, the trunk somewhat resembling the palm of the hand, and the branches the spread fingers.

We visited the place where Capt. Brown's houses once stood, and where his young daughter was buried; and finally crossed over to the eastern margin of the stream, above all its lateral branches, and rode on the highlands which skirt its eastern margin for about a mile. The side hills on which we rode were covered with cinder and pumice, thrown up from the great seething river below; and all the trees and ferns were consumed or charred. At length we descended again upon the stream, which here appeared to be one and a half mile wide, and rode another half mile on its crackling surface. Here and there, there was a little heat, and white puffs were seen scattered, widely apart, on some parts of the flow.

At length we came to the point beyond which horses do not go. Here we left our beasts and proceeded one-fourth of a mile, over a horrid surface, and up a steep and shaggy precipice of fresh lavas, to the very head of the eruption. All this region shows marks of the raging volcanic fires. There is every evidence of the fury of the furnace, when it was in full blast. There may have been a great crater here, with many orifices and fiery blow-holes; but if so, they are all filled and obliterated. What we now see is a great fissure, or rent, running irregularly from the head of the eruption for two and a half or three miles, in a southerly direction. This I traced distinctly with the eye, from my high position on the great ridge where the rending and disgorgement commenced. This was in a forest, but the fires have cleared out the thicket, so that the view is clear for four miles down the stream, taking in its entire breadth. In some places the fissure yawns twenty to thirty feet, in others it is ten, six, four and two feet. Near the head I crossed it without difficulty, and I followed it for some distance, and looked down its fearful jaws. It is hung with stalactites, and is still hot at some points. I found alum sulphur, sulphate of lime, and Glauber salts in small quantities. None of the salts are abundant, and the products and general character of the eruption are similar to our former eruptions.

The whole length of the stream does not exceed ten miles—probably it is less—and I would give the average width, by uniting all the branches, one and a half miles. The depth varies from two feet to fifty, and more. The general course is south, or nearly so.

I employed two men to measure across, but they were stopped, about midway, by an impassable fissure. This I much regretted, as time would not allow me to wait for another trial. I, however, left a request for another effort and hope to hear of success.

I returned via Kilauea, and made examinations of the great crater and its surroundings. All around the upper rim the earth is terribly rent, and immense avalanches of rocks have been detached from the walls and sent thundering down to regions below.

Before the earthquake of the 2d of April, the lavas of Kilauea burst up vertically in the bottom of Little Kilauea, and spread over the old deposit of 1832. On the night of the earthquake the fires of Pele began to be extinguished and her lavas to escape through underground passages. Consequently, a subsidence has taken place in all the central portion of the crater. The outer circumference of the pit remains intact, a well defined "Black Ledge" of 100 to 600 feet wide. The central area, or the great plateau which has been lifted up from year to year by the forces below, and without disturbing the surface, has sagged gently down, carrying with it its botanical garden of ferns and ohele bushes undisturbed. The subsidence is about 300 feet in the center, forming a great basin, or caldron, with its sides or angles of 30° to 60°. The ferns are still growing in the bottom of this basin, 300 feet below their position on the first of April.

In going to the south lake we first cross the "Black Ledge," then descend into the great basin, cross its bottom, and then rise an incline of 20° on the south side, for half a mile, when we come to Old Halemaumau. But how changed! It is a pit 500 feet deep, about 3,000 feet in diameter on its upper rim, and 1,500 on the bottom. It is nearly circular. Its walls are, in some places, perpendicular, in others, overhanging, and in others, on an angle of 40°, but everywhere jagged and threatening. When I was there a little light, or faint blush, was seen at night, but no fire was visible during the day; but there was much smoke.

My daughter H. has prepared the outlines of a map of Hawaii, on which you will find some marks which may help you as to localities, courses, etc. We have no really correct map of the island.

I see Mr. Brigham's map has several important errors in the location of places, and in the situation and comparative sizes of lava-flows. The eruption of 1855 was immensely greater than that of 1859, but his map makes the latter much the larger.

The amount of matter discharged by our recent eruption is small compared with that of many former eruptions. The eruption was short and fierce. So rapid was the rush of the rivers of lava, that cattle, grazing on the plains, were surrounded before they were aware of danger. Some were consumed; but I saw several green islets, of two and three acres, where ten or twenty head were inclosed for days and afterward rescued. Houses were, also, surrounded by the lavas and left unscathed, and a great ridge of burning *aa* would come within twenty-five or thirty feet of a house, pass by it, cool as high as the ridge of the house, and not burn it. I visited a family of four on an island formed by the igneous flood, where they were inclosed for ten days on an area of about one acre. The burning stream came within twenty feet of their house, on one side, and yet the house stands, and they all remain in it. I asked them what they did and how they felt during those days of "fiery trial." They replied, that when they found themselves surrounded and all hope of retreat cut off, they gave themselves up to God, and continued in prayer. Many cases of escape from the fire and from the sea were marvelous, and seem like miracles.

Ere this reaches you, you will have heard of the strange tidal phenomena which occurred around our group on the 14th, 15th and 16th of August. Without any apparent cause the sea rose and fell three to six feet once in ten, fifteen, and twenty minutes, for three days. I was at Keaiwa at the time and only heard the reports. These reports vary as given by different individuals and in different localities.

These rapid and long-continued oscillations of the sea are a puzzle to us, and we wait to hear from the coast, or from some other regions, to decide whether the cause was near or remote—

whether the result of submarine eruptions, or the effect of great disturbances in distant parts.

Our shakes continue up to this time, but they are not severe.

Number of houses buried by landslide,	10
" " deaths " "	31
Houses destroyed by igneous eruption,	37
" " " influx of sea,	108
Deaths " " " "	46
Houses " " earthquake,	46

All the above disasters were in Kau.

Hilo suffered not a little in buildings and other works.

On the 8th of August Hilo and Puna were visited by a most awful and protracted thunder-storm. It commenced a little after noon and continued until midnight. The clouds rested on the earth and the whole atmosphere was surcharged with electricity. The air was like hot steam, and white streams of lightning were constantly flashing out and playing along the ground, the report coming with the flash and seeming to make the earth tremble. I was in Puna at the time, and the natives were greatly alarmed. One man went out of doors and returned immediately, saying, that the lightning looked like white hens, running on the ground around him. This electrical storm must have been excited by the volcanic action, as thunder-storms in summer are as rare with us as rain in Egypt or on the coast of Peru.

The map I send does not profess to be exactly correct, but it is sufficiently so to give you a general idea of localities.

A sharp earthquake occcurred on the 6th inst.

Art. IX.—*Geographical Notices;* by D. C. Gilman.

I. Notes on China, by Rev. W. A. P. Martin, D.D.

A recent number of the Journal of the North China Branch of the Royal Asiatic Society, contains an account of an overland journey from Peking to Shanghai, made in 1866, by Rev. W. A. P. Martin, D.D. This distinguished Chinese scholar, has been for many years a missionary of the American Presbyterian Board, and is now by appointment of the Imperial authorities of China, one of the Professors in the newly established University of Peking. The article referred to discusses four topics; I, the imperial road leading south from Peking; II, the present condition of the Jews in Honan; III, the navigation of the Yellow River; and IV, the central section of the Grand Canal.

From this communication we gather such facts as are of most interest to scientific readers.

1. *The Si-Shan Hills.*

Passing south from Peking to Kai-fung, the writer thus refers to the metallic deposits of the Si-Shan hills, which meeting him outside the gates of Peking run parallel to his course for nearly four hundred miles. Their highest peaks were "crowned with snow (in February and March) and glittering like a thousand gilded domes, their rugged sides resembling the wave-worn shore of a long retired ocean."

"Silver, they certainly do contain, but the mines of Shan-si, whether from defective engineering or other causes, are no longer remunerative, and have ceased to be worked. Of gold we have no notice; but the "black diamond" is found there in rich deposits, and along with it an abundance of iron, the most precious of all metals. Iron founderies are in operation in at least two districts—one near Peking and the other in *Hooh-lu-hien*, about two hundred miles to the south. As we passed the latter place, we met a vast number of carts conveying its productions to all parts of the province. These ranged from kitchen utensils up to salt boilers, five or six feet in diameter. They appeared to be well executed and the metal of good quality.

"Of coal deposits, there seems to be a continuous chain, extending from the verge of the Mongolian plateau to the banks of the Yellow River. In the vicinity of Peking there are beds of both bituminous and anthracite, but at other points, I met only with the latter variety. With the exception of places near the Hwangho, it is transported mainly by land carriage—near Peking on the backs of camels, further south on mules, donkeys and wheel-barrows. The consequence is, that while at some points, it is cheap and abundant, at intermediate places it becomes so costly that the people are obliged to burn reeds and millet-stalks or glean a scanty supply of fuel from their stubble fields.

"Here then, on the line of this imperial road, and along the base of this range of hills is the track for *the first grand trunk railway in the Chinese empire.* Not only would it find close at hand iron for its rails and coal for its motive power; but the carriage of coal and iron to all the cities on the line including Peking and Tientsin would constitute one of the richest sources of its revenue. With Ta-ku for one terminus and Kai-fung for the other, it would pass through the capital of the empire, through two provincial capitals, six *fu* cities, and an indefinite number of *chows* and *hiens*.

"Between these places the amount of local travel is immense. At some points I estimated the number of vehicles passing in the course of a day at two hundred, employing from four to five hundred mules; while caravans of pilgrims mounted on camels were flocking to the shrines of Shan-si as the Hindoos do to those of Benares. The supposed railway would soon supersede these slow and painful modes of locomotion."

2. *Recent Changes in the course of the Hwangho.*

Dr. Martin crossed the Hwangho, the lesser one of the great twin rivers of China, at three points;—first near Kai-fung, where it still continues in its old channel; again at Tsing-kiang-pu, where he walked dry shod over the place where Lord Amherst's junks offered incense to secure a favorable passage, and third near Tung-ping-chau, where the river was hastening in its new course toward the Gulf of Chili. He thus describes the wonderful change which has thrown *five hundred miles of sea coast* between its present and its former embouchure.

"According to the best information I was able to collect, the breach that opened the new channel occurred near *E-fung-hien*, thirty or forty miles to the east of K'ai-fung-fu. From that point, washing the city of K'au-ching, it flows north passing under the walls of Ts'au-chou-fu, as far as Fan-hien, where it spreads into a lagoon some thirty *li* in width. I passed near this place, and should have crossed the river here but for the ice that had formed on the lagoon. Turning in an easterly direction it intersects the Grand Canal at *Chang-ch'iu-chen.* It was at *Li-lan-k'iau*, a little beyond this place that I crossed it—it had there diverged from the canal to the distance of fifteen *li*. A stone bridge that gave name to the locality, and which in former years sufficed to carry passengers over a small tributary of the Ta-ts'ing, was lying in ruins, the advent of the Hwang-ho having tossed it aside with little ceremony. From this point, it not only usurps the bed of the Ta-ts'ing but obliterates its very name—the natives everywhere speaking of that startling phenomenon, the 'Coming of the Yellow Waters.'

"As to the cause of this phenomenon, we are left very much to conjecture. Superstition discovered a mysterious relation between the outbreak of the Taiping rebellion and the behavior of the unruly stream in refusing to pay tribute to the Eastern ocean, bursting over all bounds and pouring its waters into what the natives call the Northern Sea. They view it only in the light of a portent; but the alleged relation is not to be set aside as altogether imaginary. Dr. D. J. Macgowan, who in a communication to the *North-China Herald* first drew public attention to the remarkable change in the Yellow river, quotes authority to show that 'in the latter part of 1852 the people of *Hwai-ngan* found the river fordable, and in the spring of the ensuing year travellers crossed it dry shod.' Mr. Wade in the Parliamentary Blue Book for 1859–1860 cites a Chinese document to the effect that by an inundation in 1855 'the north bank of the river in Honan was carried away and the river ceased to flow.' Now it was just between these dates that the Rebel invasions of the northern provinces took place; and what more natural supposition can we make than that the *Ho-tuh* or Superintendent of the River-works, who has under him a force of 64,000 men, on a *quasi* military footing, should have found other employment for his 'navvies' on the approach of the enemy, and neglected the river at a critical juncture? He may even have employed the impetuous stream as means for checking their advance. A rumor became current at the time, that many rebels had been drowned in consequence of the river breaking its banks. That this outbreak was the result of neglect occasioned by the rebel panic is highly probable; but the military use of the river which I have just hinted at is not without a precedent. The Chinese are as well acquainted with this method of extinguishing an enemy as were the heroes of the Dutch Republic. Not to speak of other instances, K'ai-fung-fu has in this way been subjected to at least three destructive inundations. Once by the forces of *Ts'in* for the purpose of dislodging the prince of *Wei* who held his court in what was then called *Ta-liang*; once by the Mongols in their conflict with the Sung dynasty, and again by a general of the Ming's with a view of destroying a body of rebels who were laying siege to the city. The whole population of the city fell victims to the miserable stratagem, and Chinese historians charge the cruel act to the rebel *Li-tse-ching*; but we prefer the contemporary testimony of Jesuit missionaries.

"It is not perhaps generally known that the Yellow river in that immense departure from its late channel, which excites the astonishment of the present age, is only returning to a long forsaken pathway. The highlands of Shantung rise like an island from the level of the great plain, and it appears from Chinese records that the restless river in finding its way to the sea has oscillated with something like periodic regularity from one side to the other of this promontory, and at two epochs flowed with a divided current converting it into an immense delta. These vagaries are minutely traced in a hydrographical work published under the patronage of K'ang-he. From this we learn the curious fact that the river divided its waters between the two principal channels, and insulated the highlands of Shantung for a period of 146 years, and that it was not till the reign of the Mongols about 500 hundred years ago, that it become settled in its southern bed. The writer concludes with the expression of an earnest desire that the troublesome stream may be induced to return to its northern course. After the lapse of two centuries his wish is now gratified."

The reader who is interested in studying the recorded changes in the Hwangho delta (which are probably without a parallel in the history of physical geography) will find a very instructive series of illustrative diagrams, eleven in number, appended to Professor Raphael Pumpelly's Observations on the Geology of China, Mongolia and Japan, published in 1866 by the Smithsonian Institution. (Wash., 162 pp. 4to.) The text of Mr. Pumpelly gives a synopsis of the Chinese history respecting these remarkable alterations in the river bed. Prior to these recent changes the subject had been ably discussed by M. Edouard Biot in the *Journal Asiatique* [iii], tome 14, p. 152 [iv], tome 1, 453, and [v] tome 4, 408. Mr. Pumpelly's and Mr. Bickmore's narrative of observations on the Delta were printed in this Journal for March, 1868.

3. *The Great Imperial Canal.*

Dr. Martin thus continues: "The canal, where I crossed it, to the south of *Tung-ch'ang-fu,* was nearly dry, and I had not been able to learn whether it was in a working condition above *Ts'ing-kiang-p'u.* From Kiuhfan to this place, it was accordingly my intention to proceed by land; but my cart driver taking alarm at rumors of rebels refused to advance, and I was compelled to seek for some other mode of prosecuting my journey. The canal was suggested, and I made my way in that direction slowly, painfully toiling on, now on foot, now on a wheel-barrow, anon mounted on one of the imperial post horses, or seated in a mandarin carriage. At length ascending a hill I saw the *Weis-han* lake spreading its silvery expanse at my feet. Embosoming an archipelago of green islands, and stretching far away among the hills—to my eye the scene was too pleasing to be real. I distrusted my senses and thought it a *mirage* such as often before had cheated my hopes with the opposition of lake and stream; and when my guide assured me that it was no deceptive show, I gave way to transports not unlike those in which the Greeks indulged, when escaping from the heart of Persia they caught a distant view of the waters of the Euxine.

"Taking passage at the foot of the lake I glided gently down with the current, and reached *Chin-kian-fu,* a distance of 900 *li* in less than a week.

"Through this portion of its course the canal deserves the appellation of 'Grand.' For the first half, extending to *Ts'ing-kiang-p'u* it varies from eighty to two hundred feet in width. Seething and foaming as it rushes from the lake and rolling on with a strong current through the whole of this distance it has more the appearance a natural river than of a canal. Near Ts'ing-kiang it parts with enough of its waters to form a navigable stream which enters the sea at *Hai-chau.* Beyond this point, where it intersects the old bed of the Yellow river, its waters are drawn off by innumerable sluices to irrigate the rice grounds, until about forty feet in breadth and four in depth. Recruited, however, by a timely supply from the *Kav-yu* lake, it recovers much of its former strength, and flows on to the Yang-tsze-kiang with a velocity that makes toilsome work for the trackers.

"To what extent the canal may be practicable for steam navigation is a question not without interest; and my mind had been occupied with it for some days, when I happily had the opportunity of seeing it subjected to the test of experiment. Just off the city of Kau-yu, where the canal reaches its minimum depth, I met the Hyson, a well known tug-boat from Shanghai, towing a flotilla of war junks. She would be able to reach the city of *Ts'ing-kiang-p'u,* but not to go beyond on account of the locks or water-gates, some of which are only twelve feet in width.

"As the canal now is, propellers of three feet draught and ten feet beam, making up in length what they lack in other dimensions, might drive a profitable trade between *Chin-kiang* and *Tsi-ning-chau* a distance of 1,200 *li*; but the utility of the canal would be greatly enhanced by adding a lock or two in the shallower portions and increasing the breadth of those that now exist so as to admit the passage of larger vessels. A little engineering at its point of intersection with the new course of the Yellow River would supply an abundance of water to a portion that is dry, making its facilities for junk navigation equal to those of its best days; and it would then be possible for steamers of the class that were lately employed in penetrating to the silk districts of the interior, to make inland voyages from Shanghai nearly to the gates of Peking."

II. Arctic Explorations.

1. *European Expeditions.*

The German North Polar expedition, in which Dr. A. Petermann has been so deeply interested, has returned without having achieved the highest results. The party came back to Bremen early in October last, and Capt. Koldewey has already published an outline of his summer work. Some interesting observations were made on the northern coast of Spitzbergen, in Henlopen Straits, and on the southern shore of North-east land. The snow-covered peaks of Gillis land were seen by telescope from the deck of the "Germania." The farthest point reached was 81° 5′ N. lat., in long. 16° E., on the evening of Sept. 14, when progress was stopped by pack ice in every direction but the south. No doubt the observations of the party will add many interesting details to our knowledge of the North Atlantic, and the leaders are already desirous of making a new voyage in a better vessel propelled by steam.

So much enthusiasm was shown by Dr. Petermann in projecting this expedition, and in arranging for its departure, that the scientific world will share in this disappointment, and will wish for him ample means to renew and carry forward his Arctic researches.

The expedition projected by M. Lambert under the auspices of the Emperor of the French and a committee of learned men, is to enter the Polar Seas by Behring's Straits. A subscription of 600,000 francs is called for as the material support of this undertaking.

The Swedish Arctic Expedition, according to a letter from Prof. Nordenskiöld (dated at Kobbe Bay, on the northwest coast of Spitzbergen, Sept. 16), went as far north as 81° 9′, where ice stopped it at the end of August.* A week later the sea was clear, and from one of the highest peaks of Parry Island (80° 40′ N.E. of "North East Land") traces only of ice further northward could be seen. The exploring steamer then made again for the north, whether to pass a winter in the ice or not is uncertain. The coal ship returned to Sweden with the collections already made.

2. *Renewed recommendation of Smith's Sound, by Dr. I. I. Hayes.*

Meanwhile, Dr. I. I. Hayes (whose zeal in the prosecution of Arctic researches has never languished, though his plans for another expedition were interrupted by the late civil war) has delivered a lecture before the American Geographical Society of New York (Nov. 12, 1868), in which he proposes the renewal of explorations of the North Polar regions, and makes a statement of the grounds on which he bases his expectations of a successful enterprise. He is very clear and forcible in the advocacy of what may be termed "the American route" northward by the way of Smith's Sound. This is the course (as our readers will remember) by which he discovered in 1854 and revisited in 1861, *the most northern land* yet known on the

* Later reports say that the highest latitude attained in open sea was 82° 40′ N.

globe, and it is the course which he had proposed to follow in 1862 if a third opportunity of exploration were offered to him. While he does not now expressly pledge himself to a fresh expedition, he says that he is no less earnest than formerly for the opportunity to conduct a party over the to him familiar course, and "to try conclusions with his old foe, the Smith's Sound Ice." Whether the American Geographical Society, which is already, through Mr. Henry Grinnell and others, so honorably identified with the progress of Arctic research, will render new aid for these proposed researches remains an open question.

In connection with a review of the history of Arctic discovery, Dr. Hayes examines briefly the comparative advantages of the four proposed entrance routes, namely: 1. *Smith's Sound*, which he prefers; 2. *Behring's Straits*, advocated by Gen. T. L. Kane, and Capt. S. Bent, U. S. N., and adopted also by the French expedition under M. Lambert; 3. Between Spitzbergen and Greenland, the route proposed by Dr. Petermann; and 4. Between Spitzbergen and Nova Zembla, which Dr. Hayes regards as least promising of all.

Two considerations are urged in preference of Smith's Sound: land as a base of operation, and the opportunity to colonize a party of hunters and natives as a permanent support.

The eloquent terms in which Dr. Hayes pleads for a renewal of northern discoveries, and the intrepid spirit which animates him, are well fitted to awaken new zeal in the public and to enlist the support of men of wealth. Dr. Hayes's address has been printed in a pamphlet form by the Society to which it was delivered. (N. Y., 1868, pp. 44.)

3. *Capt. Long's Discovery of "Wrangell's Land"* (70° 46′ N. lat. and 178° 30′ E. long.). *His opinion of the Behring's Strait route.*

Capt. Thomas Long, of the bark "Nile," who has been well known in years past as a most successful whaling master, published rather more than a year ago a letter addressed to Mr. H. M. Whitney, the editor of the *Pacific Commercial Advertiser*, dated Nov. 5, 1867, giving an account of his discovery of land hitherto unknown in the Arctic Ocean, and Capt. G. W. Raynor, of the ship "Reindeer," at the same time published his notes of observations of land in the same vicinity. Both writers refer also to the testimony of Capt. Bliven who, while cruising near *Herald* Island (71° 20′ N. lat., 175° W. long.), eight miles southeast of Wrangell's Land, saw the mountain ranges extending northwest as far as the eye could reach. As the position of this land is attracting much attention, we place on record the essential portions of the two letters.

Capt. Long writes :—

HONOLULU, November 7, 1867.

During my cruise in the Arctic Ocean this season, I saw land not laid down on any charts that I have seen. The land was first seen from the bark Nile on the evening of the 14th of August, and the next day at 9½ o'clock A. M.; the ship was eighteen miles distant from the west point of the land. I had good observations this day, and made the west point to be in lat. 70° 46′ N., and long. 178° 30′ E.

The lower parts of the land were entirely free from snow, and had a green appearance, as if covered with vegetation. There was broken ice between the ship and land; but as there were no indications of whales, I did not feel justified in endeavoring to work through it and reach the shore, which I think could have been done without much danger.

We sailed to the eastward along the land during the 15th and 16th, and in some places approached it as near as fifteen miles. On the 16th the weather was very clear and pleasant, and we had a good view of the middle and eastern portion of the land. Near the center, or about in the longitude of 180°, there is a mountain which has the appearance of a distinct volcano. By approximate measurement I found it to be 2,480 feet high. I had excellent observations on the 16th, and made the southeastern cape, which I have named Cape Hawaii, to be in lat. 70° 40′ N., and long. 158° 51′ W. It is impossible to tell how far this land extends northward; but as far as the eye could reach, we could see ranges of mountains until they were lost in the distance, and I learn from Capt. Bliven, of the bark Nautilus, that he saw land northwest of Herald Island as far north as 72°.

Capt. Raynor writes :—

HONOLULU, November 1, 1867.

I send a short account of a large tract of land lying in the midst of the Arctic Ocean, hitherto but little known. This island has heretofore been considered to be two islands, one of which has been marked on the English charts as Plover Island, which is laid down to the W.S.W. of Herald Island. The other is simply marked "extensive land with high peaks." On my last cruise, I sailed along the south and east side of this island for a considerable distance three different times, and once cruised along the entire shore, and by what I considered reliable observations, made the extreme southwest cape to lie in N. lat. 70° 50′, and E. long. 178° 15′. The southeast cape I found to lie N. lat. 71° 10′, and W. long. 176° 40′. The south coast appears to be nearly straight, with high rugged cliffs and entirely barren. The northeast coast I have not examined to any extent, but it appears to run from the southeast cape in a northwesterly direction for about fifteen or twenty miles, and then turns to the north and northeast. I learned from Capt. Bliven that he traced it much further north, and has seen others who traced it to the north of lat. 72°. I think there is no doubt that it extends much further to the north, and that there is another island lying to the east of it, say in long. 170° W., and to the northwest of Point Barrow, with a passage between it and the land I have just described. My reason for thinking so is this: We always find ice to the south of the known land, further to the south than we do the eastward of it. The current runs to the northwest from one to three knots an hour. In the longitude of 170° W. we always find the ice barrier from fifty to eighty miles further south than we do between that and Herald Island, and there is always a strong current setting to the northwest between those localities, unless prevented by strong northerly gales (for in such shoal water as the Arctic Ocean the currents are changed easily by the winds), which would indicate that there is a passage in that direction where the waters pass between two bodies of land that holds the ice, the one known, the other unknown.

I would add that the southwest cape of this island, described above, lies twenty-five miles distant from the Asiatic or Siberian coast.

More recently, Capt. Long has announced this discovery in a letter to the Geographical Society of Paris, which is printed in their journal for Sept., 1868. He has also written for the *Pacific Commercial* a letter strongly advocating the Behring's Strait route for Arctic discovery. For copies of these very interesting letters of Capt. Long we are indebted to a merchant in New London, Conn., Henry R. Bond, Esq., who has watched very closely the progress of all the Polar discoveries. He informs us that the summer of 1868 has been what is termed "an icy season" in the Arctic Ocean, and that the whaling fleet has probably been unable to go as far north as in 1867, which was a peculiarly open season.

In view of all that is said at home and abroad on this subject, we copy the statement of Capt. Long in respect to the Behring's Strait route for Arctic research.

The *route* I would recommend as the best would be to follow the Asiatic shore from Behring's Strait as far as Cape Kekurnai or Cape Schelagskoi. The ice melts earliest near the shore, and the melting of the snow upon the land forming innumerable streams of water, impels the ice from the shore, leaving an open lane of water near the shore, through which a ship can pass without difficulty, especially when assisted by steam in calms and adverse winds. After passing Cape Jakan, there being no land to the north, the ice is driven from the shore by these streams and scattered in fragments in the open sea seen by Wrangell, with sufficient openings for the safe navigation of a ship. In the month of August, 1867, the bark *Nile* passed over a position within ten miles of the point where Wrangell saw the open sea in March. From some point between Cape Kekurnai and Cape Schelagskoi the course would be from north to northwest, as the ice would permit, until north of the Läächow Islands, when the effects of the current from the rivers of Northern Asia would be sensibly felt. From thence a course directly toward the North Pole or Spitzbergen, as would appear most feasible, should be pursued.

After getting to the north of the Läächow Islands, should a vessel be obstructed by ice, the current, though not as strong as that found north of Spitzbergen and in Baffin's Bay, *would eventually carry the vessel through one of these channels into the Atlantic.* In the event of any disaster to the vessel, the chances for the preservation of the lives of those on board are much greater than by the route east from Behring's Strait, as from the river Kolyma to the westward, Russian settlements are found near the mouths of all the rivers, where assistance can be procured.

Another route by which this voyage can be accomplished is to follow the shore from Behring's Strait to the mouth of the river Lena, and from thence directly north beyond Cape Sievero Vostoschni; from thence to the westward toward Spitzbergen. After passing the mouth of the Lena, a vessel would receive assistance from the current of this river and the other rivers between the 105th and 140th meridians.

The effect of those large rivers in impelling the ice from the land was seen by Franklin in his expedition from Great Bear Lake in 1826 down the Makenzie river, and along the shore toward Point Barrow. In this expedition, he reached the longitude of 149° W., with but little impediment from ice on the 15th of August. At this point he determined to return. His associate, Dr. Richardson, proceeded to the eastward with another party as far as the Coppermine river, without any difficulty. Franklin says that the natives informed him that, from the top of the hills at the mouth of the Makenzie river, no ice was to be seen for two months of the year, i. e., in August and September, showing the powerful influence of these rivers upon the ice. It was the current from this river and its thermal influence which enabled McClure to reach Banks's Land, and had there been other large rivers to the eastward, with no land to obstruct their discharge northward, would have enabled him to make the passage between one ocean and the other.

The months of August and September are, I think, the best months for explorations along the shores of the Arctic Ocean. American whalers have passed to the eastward of Point Barrow, and taken whales as late as the 15th of September, seeing no ice to the northward except in the immediate vicinity of Point Barrow. Whales have also been taken as late as the 12th of October in lat. 71° N.

Deshnew, it is certain, in 1648, sailed from the mouth of the Kolyma along the coast of Asia, and passed through Behring's Strait to the Anadyr river. The account of this voyage, though vague and uncertain in regard to its details, yet established the fact of the separation of the continents of Asia and America.

Shaularow, Billings, and others attempted exploration along this coast, but were unsuccessful, and some of them perished in their attempts. When we consider the scanty facilities and rude structure of their vessels, we cannot wonder at their failure.

With our modern improvements in the structure of vessels and appliances for propelling them, what to the navigators of 200 years ago appeared possible, should to us appear and be of easy accomplishment.

That a vessel properly fitted for the purpose can winter in safety at almost any point along the shore, is proved by the experience of Capt. Collinson, in the *Enterprise*, who twice wintered eastward of Point Barrow, once at Camden Bay, where there is no protection from the north, except the ice which may be grounded seaward from the vessel.

That the passage from the Pacific to the Atlantic Ocean will be accomplished by one of the routes which I have indicated, I have as much faith in as I have in any uncertain event in the future, and much more than I had fifteen years ago, in the success of the Atlantic Telegraph.

In conclusion, I submit these remarks to the public, and while deprecating criticism on any verbal inaccuracies, would invite discussion in regard to the views advanced, or the feasibility of the routes proposed. Although this route will be of no great importance to commerce as a transit from one ocean to the other, yet could the passage along the coast as far as the mouth of the Lena be successfully made every year, (which I think probable,) it would be of great benefit in developing the resources of Northern Siberia. Yours truly, THOS. LONG.

Honolulu, January 15, 1868.

4. *Hall's Search for Sir John Franklin.*

The newspapers have recently published some apparently fresh reports respecting Capt. Hall's search for Sir John Franklin. As some of these reports were quite inaccurate and have led to confusion, we have obtained the following trustworthy information from a correspondent in New London, Henry R. Bond, Esq., already referred to as a merchant interested in every thing which pertains to Arctic discovery.

These reports were brought by Dr. Goold, steward of the whaling brig "Isabella," which arrived from Cumberland Inlet in September last. The Isabella was in Hudson's Bay in August, 1867. This is the date of Hall's letters to Mr. Henry Grinnell of New York, which were received and published more than a year ago; so that the Isabella's report contains no new information.

No whaling vessels remained in Hudson's Bay last winter. Two have gone there this summer, and will probably pass the present winter there. It is not likely that there will be any further news from Hall until such vessels return next August.

"The whaling master," continues our correspondent, "who last saw Hall, has just left my office. He reports that the latter intended if his expedition was successful to go down to York Factory, one of the Hudson's Bay Company's posts, in the summer of 1868, and sail thence to England if opportunity offered. If he had done so, it is time to have heard of his arrival in England. Hall had a party of five men with him all armed, and he proposed to fight his way to King William's Land if the natives opposed him."

III. Report of Hon. A. Campbell on the Northwest Boundary.

Some time since we called attention to the interesting maps of the regions adjacent to the disputed boundary of the United States and British possessions in the Northwest, published by Archibald Campbell, Esq., U. S. Boundary Commisioner. His Report, which has since been printed (Washington, 1868, 270 pp. 8vo), contains much geographical information, especially respecting San Juan and the neighboring islands in dispute (pp. 128–142). The document and the map together are a very important contribution to our knowledge of a region in respect to which we were nearly plunged into war. We make a single extract descriptive of the San Juan island, regretting that we have not room for a more complete synopsis of this important document.

San Juan Island is the most western of the Haro group, and has an area of about fifty-four square miles. Its greatest length is about fourteen and a half miles; its general shape being very irregular, the width varies at different localities; its widest part is about six and a half miles. Low ranges of hills trend along its eastern and western shores, those on the western side being the highest, Mount San Juan, in this range, having an elevation of about one thousand feet. These ranges slope out toward the north, and there are no elevations of any consequence on the northern shore of the island. Between these hill ranges, near the center of the island, lies a basin-like country, gently undulating in its character. There are extensive prairies in several localities, and from the south end of the island to within a short distance of its northern extremity flocks can feed on green grass almost throughout the year. The greatest amount of arable land is found within the southern third of the island.

Bellevue prairie, situated on the lower end of the island, is about two miles long by half a mile wide. Oak prairie, which takes its name from the groves of oak scattered over it, containing about one thousand acres, is bounded on the north and west by the hills along the west shore that extend across the island at its greatest width. Some of these hills are grassy to their summits, while others are more or less timbered. Immediately north and west of these hills lies a beautiful valley, stretching toward the north end of the island. The southern end of this valley contains several hundred acres of meadow land, but on the north it is heavily timbered. The land contained in it is all apparently fertile, and around it the hills are covered with a luxuriant growth of grass. This valley lies immediately opposite to Henry Island and adjacent to good harbors.

The northern end of the island contains much good land, now covered by a heavy forest, but when divested of this it can be brought into profitable cultivation. In this region there is a grove of large cedars very valuable for lumber. One-third of the entire area of this island, or about twelve thousand acres, is well adapted to cultivation, and nearly all the remainder to pastoral purposes. The soil of the arable portions is excellent, with the exception of Bellevue prairie, which is somewhat gravelly.

Upon this island are at least four beautiful lakes. From some of these flow rivulets of sufficient size and force to produce good water power: but as yet there are no inducements for the erection of mills, as the lumber of the adjacent shores of Puget Sound is superior to that of the island, the latter having all more or less suffered from frequent conflagrations; but in a few years more, when the husbandman shall begin to receive returns for his labors in rich crops of grain, some of these sites may be selected for erecting mills to prepare the produce for distant markets.

A circumstance of great importance, in connection with this island, is the existance upon it of extensive deposits of limestone. It is to be found near the southern end, in the vicinity of the Hudson's Bay Company's station. On the western shore, near the base of Mount San Juan, immense masses raised up into perpendicular walls are seen at several localities, covering an area of many acres. The northeastern corner of the island is composed of an extensive ledge of the same material. A very small island, (O'Neal's) lying close to the northeast end of San Juan Island, containing only a few acres, is composed almost entirely of limestone. Tested by acid and burning, it proved to be of a superior quality. It exists in sufficient quantities not only for lime, but might be profitably quarried for building stone. The value of these deposits can better be appreciated from the fact that up to the time of the discovery of limestone on this island it was not known to exist at any point on Pugent Sound, within United States territory, and for building purposes it was necessary to procure all the lime used, from California or Vancouver's Island.

In the vicinity of the southern end of the island are, perhaps, the best fishing grounds on Puget Sound. Great quantities of halibut, codfish, and salmon, are taken by the numerous tribes of Indians who, at the proper season, resort to this vicinity for the purpose of fishing The Hudson's Bay Company were formerly in the habit of putting up at this place from two to three thousand barrels of salmon alone, which were bought from the natives. Persons supplied with the proper appliances for carrying on a fishery would find it a very profitable vocation.

At the southern end of the island there is a large bay, known as

Ontario Roads, where vessels are well protected from the prevailing storms of this region. The water near the shore is not deep, and should it ever be desirable to build a wharf at this point it would require one several thousand feet in length to reach three fathoms at low water. The entrace to this roadstead from the Straits of Fuca is through a very narrow but deep channel, known as Little Belt Passage, separating this island from Lopez Island. It is a very convenient and favorite resort for vessels escaping from storms which often in winter season, very suddenly arise on Fuca Straits. On the northwestern end of the island are several bays, well protected by Henry Island, forming good harbors for vessels of light draught.

IV. Prof. Cook's new Map of New Jersey.

We have received from Prof. Geo. H. Cook, of Rutgers College, a reduced copy of the new Geological map of New Jersey. The geographical information is compiled and drawn by G. M. Hopkins, C. E., from data furnished by the State Geological Survey, the U. S. Coast Survey, and other official and private sources. The coasts, the river courses, the civil boundaries, the roads, and other horizontal features of the country are elaborately delineated. The elevations or vertical features are not so distinctly brought out. As the reproduction of a larger map by a new photographic process, the map is exceedingly interesting to cartographers. The copies we have seen are printed in a single tint; others colored geologically are probably published.

V. C. C. Parry's Report on the Physical Geography of the Kansas Pacific Railway Route. (35th Parallel.)

Dr. C. C. Parry, Geologist of the Survey, has published a preliminary report (Philadelphia, 20 pp., 1868) on the route from the Rio Grande to the Pacific Ocean, of the Kansas Pacific R. R. We make the following extract.

All the features of "the Plains" are changed on reaching the great barrier constituting the eastern outlier of the Rocky mountain range: instead of uniform slopes, the elevations exhibit abrupt rocky declivities; the valleys are cut deeply through the rocky strata, forming chasms and prolonged cañons; the sheltered recesses, irregular character of soil, and more abundant moisture, favor the growth of trees and shrubbery, while the variety of rock exposures, including volcanic, metamorphic and sedimentary, serve still farther to vary the general features of the country.

On crossing this first range of mountains near the line of the 35th parallel, at an elevation of about 7000 feet above the sea, we descend into the valley of the Rio Grande, 2000 feet lower, and from this point westward to the Colorado, a distance of 575 miles, the principal drainage is to the south, the intervening water-sheds between the different valleys, presenting ridges of moderate elevation, as exhibited in the general profile of the surveys.

The region under special consideration, comprises three very distinct sections, which may be briefly referred to in regular order, proceeding westward from the Rio Grande.

1st. From the Rio Grande to the Colorado of the West.

2d. From the Colorado to the summit of Tehachapa Pass in the Sierra Nevada.

3d. The western slope of the Sierra Nevada, descending into the Tulare valley, and thence over the Californian coast range or down the Tulare and San Joaquim valleys to the Pacific.

I. *From the Rio Grande to the Colorado of the West*, the country presents the character of a vast upland, crossed by a succession of mountain ridges, and basin-shaped valleys, interrupted by the product of recent volcanic eruptions in the form of extinct craters, cones, and streams of lava, which have overflowed and buried up the lower sedimentary rocks. The principal mountain axes exhibit a granitic nucleus, which at certain points is exposed to view in irregular mountain ranges, trending northwest and southeast, and constituting the general frame-work of the country, as exhibited in the Sierra Madre, the Mogollon range and the Pinaleno mountains of Central Arizona. Intermediate to these is the great table-land or *mesa* formation of Western New Mexico and Eastern Arizona, comprising the sedimentary strata of Triassic and Cretaceous rocks, which spread out into broad uplands, abruptly terminated by steep mural declivities, bounding valleys of erosion, or presenting isolated butes and fantastically castellated rocks, that serve to give a peculiar aspect to the scenery The principal foci of extinct volcanic action are represented by the elevated cones of San Mateo, and San Francisco, attaining an elevation of over 12,000 feet above the sea, whose alpine slopes, reaching above the timber line, present in their covering of snow, the only wintery feature pertaining to this latitude.

It is in the eastern section of this district that we meet with the most populous and flourishing of the interesting tribes known as Pueblo Indians; here they secure not only defensive positions for their towns on the tabled summits of isolated hills, but also fertile valleys adjoining, suited to their rude agriculture, and a wide scope of grazing country, limited only by the necessity of protection from the thievish and roving Navajo and Apache.

What is known as the Navajo country, extending still further to the west and north, comprises a similar character of broken country with fertile valleys, grassy slopes, and deeply sheltered cañons, especially adapted to their mode of life as nomadic and at the same time partially agricultural; still, better suited, however, to the wants of an energetic civilized community, who can properly appreciate the advantages of a healthful climate, combined with a useful variety of soil, and that picturesque beauty of scenery, which adds such a charm to rural life.

In passing to the valley of the Colorado, we descend by a succession of irregular mountain ranges and basin valleys, becoming more arid as they reach a lower elevation, and finally passing into the valley of the Colorado, characterized by its bare mountain ranges, desert uplands, and broad alluvial bottoms, supporting their peculiar semi-tropical vegetation.

II. *From the Colorado to the summit of Tehachapa Pass in the Sierra Nevada.*—After leaving the valley of the Colorado, and crossing the first range of mountains, bounding the valley on the west, we come upon that peculiar section of country properly characterized as the southern continuation of the Great Basin. In all its external features of isolated mountain ridges, separated by stretches of desert plain, and valleys with intermittent flowing streams, terminating in saline or fresh water lakes or sinks; in its numerous dry water-courses or *washes*, which convey the product of summer rains, in sudden floods to the lower plains, it is an exact counterpart of the settled mining district of Nevada, characterized, however, by milder winters and greater summer heats. It is also much narrower in its eastern and western extension, the entire desert tract being comprised within little over 200 miles, the valley of the Mojave occupying in its easterly course more than half of the distance.

On reaching the eastern slope of the Sierra Nevada, here much reduced in elevation, and affording a number of practical railroad passes, we reach a well-watered country with frequent springs and small water-courses flowing from the adjoining mountains, and toward the dividing ridge, good grazing and agricultural lands are interspersed with groves of timber, presenting all the desirable features of an inhabitable country.

III. *The western slope of the Sierra Nevada, descending into the Tulare valley, and thence across the coast range of California, or down the Tulare and San Joaquim valleys to the Pacific.*—This section of country comprising the southern extension of the great valley of California, is too well known to require any detailed description.

Further information respecting this region is given under seven heads, viz: Climate, Supply of Water, Vegetation, Adaptation to Agriculture, Minerals, and Facilities for railroad connections. The recapitulation is as follows.

The general comparative advantages of this trans-continental route along the 35th parallel may be thus briefly summed up:

A salubrious climate favorable to health and activity, accessible to the moist southerly currents, while at the same time protected from the severe northern blasts, receiving along the higher elevations precipitation of rain and snow sufficient to favor the growth of natural forests and upland grasses, without forming any obstruction to winter travel.

A pleasant variety of atmospheric temperature, connected with differences of elevation or exposure in closely adjoining districts, which can be selected to suit the requirements of the season, or the particular taste of individuals.

An agricultural capacity that in its proper development can be made ample to supply the prospective wants of this region, and in the production of fruits and garden vegetables, can afford the delicacies that enter into the essential wants of civilized communities.

A pastoral region unequalled in the extent or quality of its grasses, which in adjoining districts keeps up a constant supply of nutricious fodder through the year, requiring only the light labor of herding to secure the remunerative returns of this branch of industry.

A mining region yet undeveloped but sufficiently known to be characterized as second to none on the continent in the extent and variety of its mineral products, only waiting for the facilities of railroad transportation to invite and retain permanent capital and industrious labor.

A location of route which presents the special advantages of a main trunk line in being naturally connected with adjoining rich districts that will thus seek an outlet by branch roads to central commercial points.

VI. Early American Maps. Discovery of an Important Manuscript of Hakluyt.

Rev. Dr. L. Woods, lately President of Bowdoin College, in a recent visit to Europe, secured through Mr. J. G. Kohl reduced copies of thirty-two early maps relating to the discoveries on the northeastern coast of America, during the first half of the sixteenth century. They are to be reproduced in the forth-coming volume of the collections of the Maine Historical Society, probably in the course of January, 1869.

He has also had the good fortune to discover among the manuscripts of Sir Thos. Philips, at Cheltenham, a manuscript of Richard Hakluyt, drawn up in 1584 by request of Sir Walter Raleigh, and giving an account of the results of the "Western Discoveries, lately attempted." Dr. Woods was allowed to bring home with him a copy of this curious manuscript, made with scrupulous care, and this will be printed in a second volume soon after the first. We shall make further comment on these interesting volumes when they appear from the press.

VII. The Bahamas Hurricane of 1866.

The destructive hurricane which passed over the Bahamas in October, 1866, disabling many American steamers,* furnished a remarkable illustration of the theory of cyclones often

* Among them the "Evening Star" foundered.

expounded in this Journal by the late Mr. William C. Redfield. His son, Mr. John H. Redfield of Philadelphia, has drawn a diagram, illustrating the direction of the winds at noon on the first day of October, availing himself not only of the marine reports of our newspapers, but especially of the abstract of observations officially reported by His Exc. R. W. Rawson, Governor of the Bahamas. This diagram is so striking that we reproduce it with the following extract from Mr. Redfield's letter to Governor Rawson. It originally appeared in connection with the latter's Report, recently printed as a pamphlet at Nassau, N. P. The general course of the hurricane through the Atlantic is indicated on another map by Capt. W. H. Stuart. The cyclone was five days in progress from the Eastern Bahamas, to the latitude of Cape Hatteras.

Philadelphia, March 23, 1868.

* * * * It was this consideration that suggested to me to employ your abundant data for mapping out the actual winds of the storm, in the different portions of its area, at a definite period of time, and thus coördinating the facts of the case. This mode of analysis had been a favorite one with my father (Wm. C. Redfield, of New York), and was employed most elaborately, and with signal success in his Memoir upon the Cuba Hurricane of Oct., 1844. (See Am. Jour. Sci., II, vol. i, ii, Jan. to Nov., 1846.) Some embarrassment, which I experienced from an occasional want of more definite information from some of the points of observation as to the precise direction of the wind at the hour selected, has been removed by the kindness of Capt. W. H. Stuart, Deputy Inspector of Lighthouses, Bahamas, who, at your suggestion, has sent me all needed facts in the table subjoined. From this table the accompanying small chart is constructed, showing the directions of the wind at *noon* of Oct. 1, at 34 different points in various portions of the storm area. In regard to this chart, I have few annotations to make, preferring that it should tell its own story.

The center of the hurricane, at the hour named, was evidently but a few miles southeastward from Shroud Cay, the wind at that Cay being from N.E. until 1 P. M., when there was a lull of an hour, followed by the gale from S.W. At the points 4, 5, 7, 8, 9 and 10, which lie beyond, or just within, the retreating limb of the cyclone, we find, as might be expected, that the storm's violence was abating, while at points 31 to 35, just covered by the advancing limb, the tempest was increasing gradually as its vortex was approaching.

Observation No. 18 (Sch. *Violin*), seems somewhat anomalous in its character, and as no log was kept, and the crew could neither read nor write, it is not too much to suppose, either that they reported the wind too much to the S. of W., or else that the vessel's position was really farther to the eastward. [It is believed that the master and crew only guessed their position.—R. W. R.]

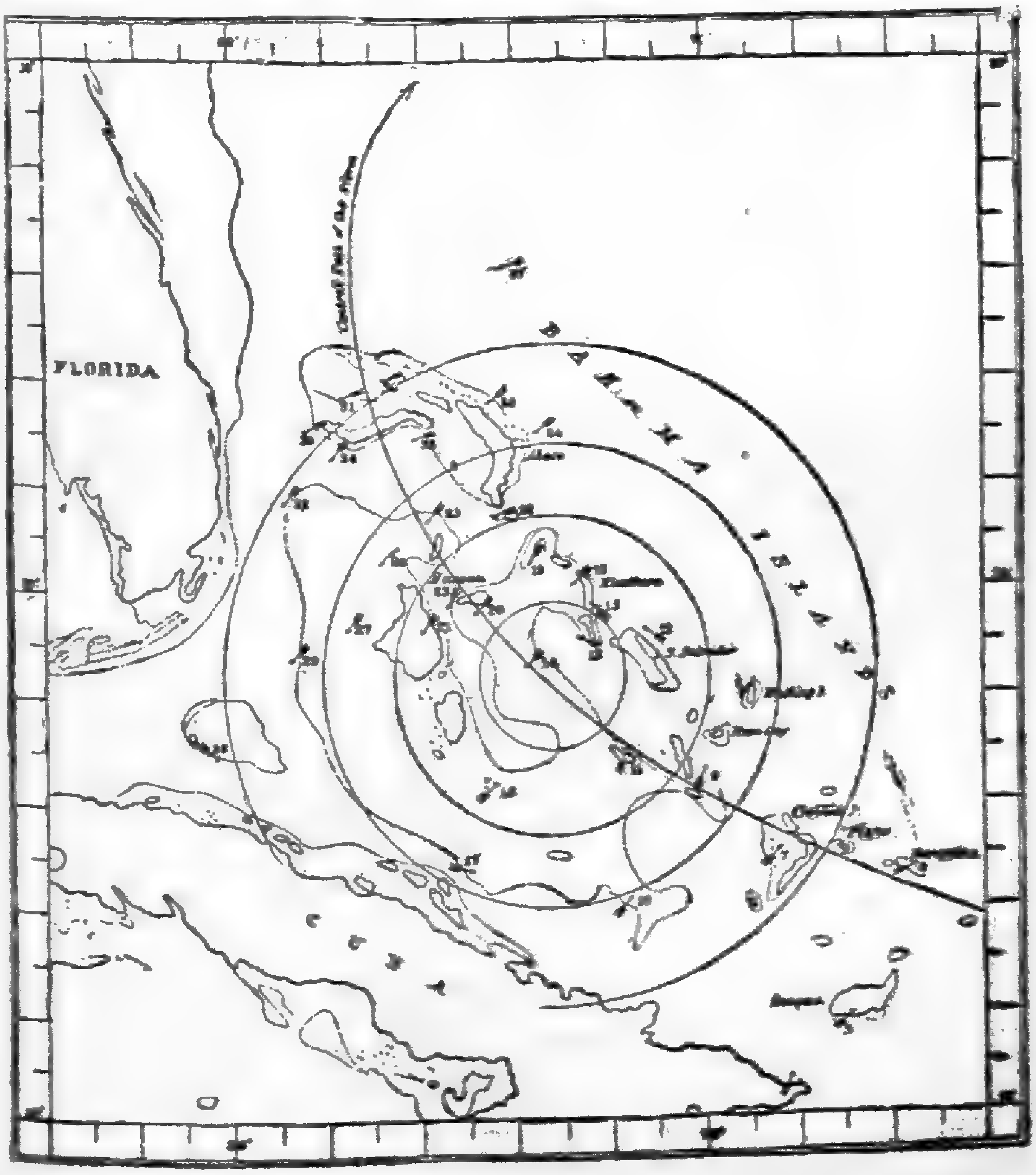
The line of progress of the axis of this storm, after leaving Turks Island, seems to have been almost identical with that of the Antigua Hurricane of Aug. 22, 1848, which also passed over the Bahamas, beginning its easterly recurvature also about latitude 28° or 29°, and pursuing its northeasterly track at least to long. 35° and lat. 45°, occupying twelve days in its journey.

The numbers upon the little chart refer to the places named in your Abstract of Observations, as well as to those of Capt. Stuart's table.

With great respect, I have the honor to remain,
Your obdt. servant, JOHN H. REDFIELD.

BAHAMAS HURRICANE OF OCTOBER, 1866,
Showing directions of wind at noon, Oct. 1.
FROM OBSERVATIONS COLLECTED BY GOV. RAWSON, W. RAWSON, C. B., AND CAPT W. H. STUART, BAHAMAS.

By JOHN H. REDFIELD, Philadelphia.



ART. X.—*Meteors of November 14th*, 1868.

ON the morning of Saturday the 14th of November, 1868, there was another recurrence of the November star shower. The following are the accounts of the numbers and appearances of the meteors as reported from different places.

1. *At New Haven.*—The writer, with Mr. O. Harger, and several students in the College, watched from the top of the tower of Graduates Hall. We commenced counting as the clock struck twelve. There were at first seven or eight in the party, but within ten minutes this number increased to about twelve who were counting. Toward morning the number diminished to seven or eight. To each person was assigned a direction to which he was to look. From our station we had an unobstructed view of the heavens.

The counting was aloud to avoid duplication ; each meteor, whether seen by one or by several persons being counted once and once only. Such at least was our aim, and probably few were lost at first. But after a time as the meteors became more abundant it was evident that many were lost in the counting. For, when two or three came nearly or quite at the same instant, only one might be added to the number. We were then by no means certain that we were making a fair census. I directed each one, therefore, to count silently during intervals which I carefully limited by the watch. The numbers reported by each observer during these intervals were entered upon the record. Immediately upon the close of an interval the counting in concert was resumed.

In the first table given below there are in the first column the intervals of observation during which we were counting in concert ; in the second column the lengths of those intervals; in the third column the numbers actually counted in the interval; in the fourth the average number per minute; in the fifth the number of observers; and in the sixth the total number per hour.

In computing these *hourly* numbers, allowance is made for the omitted intervals by assuming for the rate per minute the half sum of the rates of the periods just preceding and following. In the four hours and forty-two minutes the numbers counted amounted to 5573. For the omitted intervals the above rule gives 1786, making a total of 7359 in the six hours.

In the second table are the numbers seen by each observer in the corresponding specified intervals. The average numbers per minute for each observer are given in the final column.

TABLE I.—*Showing the numbers of meteors seen at New Haven in successive intervals, on the morning of Nov.* 14*th*, 1868, *by the whole party observing.*

Interval of observation.				Duration.	Number.	No. per min.	No. of obs.	Hourly No.
From	12h 0m	to	12h 15m	15	141	9·6	10	
"	15	"	30	15	179	11·9	12	
"	30	"	40	10	134	13·4	12	
"	40	"	45	5	72	14·4	12	
"	45	"	50	5	87	17·4	12	
"	50	"	55	5	65	13·0	12	757
"	55	"	1h 0m	5	79	15·8	12	
"	1h 0m	"	5	5	79	15·8	12	
"	5	"	10	5	89	17·8	12	
"	10	"	15	5	71	14·2	12	
"	15	"	20	5	85	17·0	12	
"	20	"	25	5	83	16·6	12	
"	25	"	30	5	93	18·6	12	
"	30	"	35	5	84	16·8	12	
"	35	"	40	5	101	20·2	12	
"	40	"	45	5	116	23·2	12	
"	45	"	48	3	66	22·0	11	
"	50	"	55	5	95	19·0	11	
"	55	"	2h 0m	5	129	25·8	11	1132
"	2h 0m	"	2	2	39	19·5	11	
"	3	"	6	3	50	16·7	11	
"	6	"	10	4	101	25·2	11	
"	10	"	15	5	109	21·5	11	
"	15	"	20	5	111	22·2	11	
"	20	"	23	3	73	24·3	11	
"	25	"	30	5	114	22·8	11	
"	30	"	33	3	84	28·0	11	
"	35	"	40	5	118	23·6	11	
"	40	"	45	5	108	21·6	11	
"	45	"	52	7	149	21·3	11	
"	52	"	55	3	68	22·7	11	
"	55	"	3h 0¼m	5¼	128	24·4	11	1369
"	3h 0¼m	"	3	2¾	56	20·4	11	
"	5	"	11	6	115	19·2	11	
"	11	"	12	1	21	21·0	11	
"	13	"	18	5	110	22·0	11	
"	20	"	23	3	68	22·7	11	
"	23	"	28	5	103	20 6	11	
"	28	"	36	8	160	20·0	11	
"	38	"	41	3	78	26·0	8	
"	43	"	46½	3½	102	29·1	8	
"	48	"	50½	2½	59	23·6	8	
"	53	"	55½	2½	49	19·6	8	
"	58	"	4h 0½m	2½	59	23·6	8	1335
"	4h 3m	"	8	5	108	21·6	8	
"	8	"	10½	2½	41	16·4	8	
"	13	"	15½	2½	50	20·0	8	
"	20½	"	24	3½	93	26·6	8	
"	28	"	33	5	102	20·4	7	
"	33	"	34	1	20	20·0	7	
"	38	"	43	5	84	16·8	8	
"	48½	"	53	4½	131	29·1	8	1364
"	58	"	5h 3m	5	150	30·0	8	
"	5h 7m	"	13	6	193	32·2	8	
"	13	"	18	5	214	42·8	8	
"	23	"	25½	2½	74	29·6	8	
"	30½	"	33½	3	61	20·3	8	
"	38	"	43	5	97	19·4	7	
"	46	"	48	2	27	13·5	7	
"	48	"	54½	6½	48	7·4	7	1402

Total in 4h 42m, 5573; in six hours, 7359.

TABLE II.—*Showing the numbers of meteors seen in New Haven in the intervals of time named, on the morning of Nov. 14th, 1868, by single observers.*

Interval of observation.	Duration of interval.	Harger.	Dudley.	Lanman.	Williams.	Slocum.	Chapin.	Todd.	Perry.	Sweet.	Maynard.	Nead.	Clark.	Av. no. per minute.
1h 48m to 1h 49m	1m	7	9		7	7	5	5	10		13	5	12	8.0
2h 2m " 2h 5m	1	2	1	2	0	5	6	5	6	6		4	7	4·0
23⅓ " 25	1⅔	11	13	10	13	10	13	14	16	11	16	16		7·8
33 " 35	2	19	21	14	10	14		12	24	9	23	18		8·2
3h 3m " 3 5m	2	11	11	12	6	12		15	13	15	12			5·9
12 " 13	1	9	6	8	7	4		4	8	0	4			5·6
18 " 20	2	12	21	20	19	24		19	22	21	24			10·1
36 " 38	2	14	20	14		17	7	18	19	14				7·7
41 " 43	2	22	25	19		22	32	34	23	11				11·7
46½ " 48	1½	12	8	10	14	13	17	12	18	5				8·1
50½ " 53	2½	17	13	17	22	18	23	17	20	18				7·3
55½ " 58	2½	20	22	16	28	16	16	27	21	18				8·2
4h 0½m " 4h 3m	2½	17	15	13	17	10	16	17	22	14				6·3
10½ " 13	2½	14	7	11	17	12	9	19		8				4·8
15½ " 20½	5	31		46	36	38	43	36	52	29				7·8
24 " 28	4	39	25	30	28	33	42	32	35	25				8·0
34 " 38	4	44	21		44	32	43	44	47					9·8
43 " 48½	5½	52	49	56	50	45	70	48	64					9·9
53 " 58	5	53	29	35	49	43	42	59	43					8·8
5h 3m " 5h 7m	4	45	40	35	44	48	49	54	34					10·9
18 " 23	5		30	37	54	58	66	60	83					11·1
25½ " 30½	5	38	38	46	47	48	78	52	65					10·3
33½ " 38	4½	31	9	35	36	23	15	48	27					6·2
43 " 46	3	19	12	4	6	5	12	18						3·6
54½ " 6h 0¾m	6¼	21	19	21	16	18	7	25						2·7

The short duration of the shower seems to imply that the radiant is very narrow perpendicularly to the ecliptic. Early in the morning hours it seemed that this area must extend parallel to the ecliptic nearly up to ε Leonis. But its length in that direction had then to be determined by flights that were nearly parallel to the horizon. The eye cannot easily make allowance for the curvature of the arc of a great circle in carrying backward the line of such a track. I feel sure that the tendency to make such tracks parallel to the horizon is so strong that in a careful location of the radiant, we must reject nearly all those in which the meteor first appears several degrees from the sickle. Yet after all allowances I believe that some paths in these early hours would when traced backward pass near ε Leonis. After the radiant had reached an altitude of 30 or 40 degrees, there were very few tracks, if any, which traced back would not cut across the line joining γ Leonis with the sixth magnitude star (230 Piazzi, 3423 B. A. C.) in the center of the bend of the sickle, and between these stars.

But there were many tracks which when extended backward cut this line at large angles and near either extremity of it. This implies that the radiant was not much shorter than the distance between them. The latitude of the radiant I estimated as about that of γ Leonis, or $8\frac{1}{2}°$. To determine this latitude only meteors starting from near the radiant and moving nearly parallel to the ecliptic were noticed.

If the radiant was a point, any two well observed flights would determine its place. But in consequence of its considerable length, only flights nearly parallel to, or nearly perpendicular to, the ecliptic are convenient for determining its latitude, and its limits in longitude.

The prevailing tint of the trains was green, or bluish green. Mr. Harger counted 60 unconformable meteors during the six hours. These were strikingly unlike the conformable ones, giving usually the impression of a harder nucleus, and leaving no train.

Many trains were visible for several minutes, and one remained for 44 minutes. They usually floated to the northward.

The sky was beautifully clear, and moonless. There was no abatement in the numbers of the meteors until dawn. Throughout the shower the proportion of faint meteors was very small. As a consequence, the most fruitful regions of the sky were nearer the horizon than in August showers. During the forenoon of the 14th, I watched clear portions of the sky for a short time, but saw no indications of the meteors.

2. *At Bowdoin College, Brunswick, Me.*, communicated by Prof. C. G. Rockwood, Jr.—In the count of meteors, I was assisted by Mr. J. P. Gross and a number of students.

The number varied from thirteen to sixteen, being at no time less than thirteen, nor more than sixteen ; the place selected was on the College grounds, where we had a view of the sky on all sides nearly down to the horizon. The night was perfectly clear, the sky being free from clouds.

The display began before midnight, but the party was arranged and the formal count began at 51^m after midnight.

After 1 o'clock the numbers were recorded (by myself) at intervals of five minutes, the numbers seen being given below :—

	1h to 2h		2h to 3h		3h to 4h		4h to 5h		5h to 6h	
m m	Total.	Unconf.	Total.	Unconf.	Total.	Unconf.	Total.	Unconf.	Total.	Unconf.
0— 5	67	1	91	4	106	0	123	2	149	0
5—10	68	2	89	4	109	2	132	2	165	4
10—15	75	3	86	3	105	0	110	2	150	0
15—20	90	8	108	3	99	0	124	1		
20—25	84	4	104	4	106	1	131	1		
25—30	91	5	107	3	104	0	135	1		
30—35	75	2	108	2	108	4	134	0		
35—40	91	0	84	2	118	3	126	1		
40—45	90	2	96	3	107	6	123	0		
45—50	92	7	95	4	140	3	115	0		
50—55	110	5	82	0	123	3	142	0		
50—60	107	6	114	2	111	3	150	2		
Total,	1040	45	1164	34	1336	25	1545	12	464	4

The whole number counted in $4^h 24^m$ was 5670, of which number 121 were reported as not conformable to the radiant in Leo. But as the observers were all without previous experience in such work but little reliance can be placed on the number of unconformable meteors recorded.

Almost all the brighter meteors left trains of various lengths, and frequently several trains would be visible upon the sky at one time. These presented the usual appearance of clouds of luminous smoke, which gradually changed their form, and floated toward the north. Many meteors were so brilliant as to cast a light on surrounding objects, which was plainly visible even when the meteor itself was hidden from view by some intervening object. Very many were brighter than Venus, which was visible in the east during the latter part of the watch.

3. *At Boston, Mass.*—Mr. T. W. Tuttle, from a window looking north, saw as follows:

From	3^h 7^m	to	3^h 22^m	92	meteors.	From	5^h 0^m	to	5^h 4^m	20	meteors.
"	22	"	43	52	"	"	4	"	8	17	"
"	43	"	55	34	"	"	8	"	14	29	"
"	55	"	4^h 4	26	"	"	14	"	20	31	"
"	4^h 33	"	43	47	"	"	20	"	27	25	"
"	43	"	46	20	"	"	27	"	30	12	"
"	46	"	54	20	"	"	30	"	38	10	"
"	55	"	5^h 0	20	"	Total in 2^h 2^m 455					

4. *At New Bedford, Mass.*—Mr. R. Taber reports the following numbers for three observers:

From	10^h 0^m	to	10^h 30^m	19	meteors.	From	1^h 30^m	to	2^h 0^m	237	meteors.
"	11 0	"	11 45	52	"	"	2 0	"	2 30	101	"
"	11 45	"	12 0	53	"	"	2 20	"	2 0	212	"
"	12 25	"	1 0	170	"	"	3 0	"	2 30	271	"
"	1 0	"	1 30	215	"						

For one observer:

From	3^h 30^m	to	4^h 0^m	190	meteors.	From	4^h 30^m	to	5^h	155	meteors.
"	4 0	"	4 30	159	"	"	5 0	"	6	207	"

The flights were in general unusually bright, leaving in many cases long trails, which remained visible sometimes two, and four, and in one instance eight minutes; gathering apparently in knots and waves, with an apparent motion of its parts and curving like a trail of smoke in a light wind. One of these examined by the telescope, showed plainly a difference in the intensity of its light, being much more luminous on the knots, so to speak; yet not enough to dim the intensity of the third magnitude star which shone through it.

5. *At Williamstown, Mass.*—Messrs. Benjamin J. Gilman, W. D. Granger and F. B. Wilder, saw

between	11^h 35^m	and	12^h 35^m	450	meteors.
"	12 35	"	1 20	250	"
"	3 0	"	4 0	950	"
"	4 0	"	5 0	830	"

Total in 3^h 45^m 2480 by three observers.

6. *At Stamford, Conn.*—Mr. E. A. Fuertes was occupied principally in locating the tracks of the meteors upon the chart. He estimates the number seen by him at more than a thousand. The sky was magnificent, the belts of Jupiter appearing with a brownish red color which he has only seen in remarkably fine nights with his glass, of six feet focus and $4\frac{1}{2}$ inches aperture. He thinks that in the earlier part of the evening the meteors were green, and gradually changed to blue as the night advanced.

7. *At Poughkeepsie, N. Y.*—Miss Mitchell reports from five observers 3,766 meteors. The hour most fruitful was that from two to three o'clock, in which 900 were counted. The most fruitful minute was from $2^h\ 24^m$ to $2^h\ 25^m$. The other hours from 12^h to $5\frac{1}{2}^h$ were much alike. Flashes of light for which they could not account by any meteor above the horizon were frequent, and Miss Mitchell was confident that the evening of Nov. 13th was lighter than common for a moonless night without aurora.

8. *At Palisades, N. Y.*—Mr. W. S. Gilman, Jr., gives the following summary of the numbers of meteors seen by himself and Mr. Thomas P. Gilman. The latter was constantly counting, but the former was occupied principally in noticing the peculiarities of the more remarkable ones, and mapping the paths upon the chart. He judged that the numbers were 25 or 30 per cent of the true number visible. A few meteors of great beauty were seen before $11\frac{1}{2}$ o'clock.

Time.	Number.	Barometer.	Therm.	Sky.	Wind.
$11\frac{1}{2}^h$ to 12^h	32	30·105	37°	Clear.	W.
12 " 1	182	·110	38	"	Light.
1 " 2	294	·102	38	"	Very still.
2 " 3	304	·100	32	"	Still.
3 " 4	290	·095	31	"	Still.
4 " 5	238	·080	32	Slight haze in Leo.	Fresher; west.
5^h " $5\frac{1}{2}$	122	·077	30	"	Light N.W. wind.

"I could distinctly, and with the utmost ease, distinguish the little companion of Sirius in my 4-inch glass with a power of only 40, at 3^h A. M., which shows remarkable clearness and steadiness of atmosphere. I noticed blue trains at first, afterward more greenish ones. The radiant point seemed to me to be near η Leonis this year, say R. A. 152°, N. Dec. 18°."

Mr. Gilman has furnished many valuable observations upon individual meteors and trains, which will be of special value when compared with those made at other places upon the same bodies.

9. *At Philadelpha.*—Mr. Pliny E. Chase counted from a window, between 1^h and 2^h A M., 155 meteors; between 2^h and 3^h, 163; between 3^h and 4^h, 206; between 4^h and 5^h, 221; and between 5^h and $5^h\ 28^m$, 105. The successive hundreds

were counted in 35, 47, 32, 22, 39, 27, 25, and 26 minutes, and the remaining fifty in 15 minutes. Among the number seen were 15 couplets and 1 triplet. Only 25, or 3 per cent, were nonconformable.

10. *At Haverford, Pa.*—Prof. S. J. Gummere furnishes a report of observations made at Haverford College under his direction, though he could himself watch only from a window. The report is by Mr. E. B. Taylor.

Counting was begun at 11^h 34^m, and the time of finishing the even hundreds, and the numbers per minute, are as below:

Time.	Entire no.	No. pr m.	Time.	Entire no.	No. pr m.	Time.	Entire no.	No. pr m.
12h 4m 0s	200	6·7	2h 39m 26s	1900	12·1	4h 38m 14s	3500	12·1
34 0	400	6·7	46 40	2000	13·8	43 0	3700	41·9
46 3	500	8·3	53 3	2100	15·7	46 38	3800	27·5
58 18	600	8·2	3h 9 0	2300	12·5	50 31	3900	25·7
1h 17 0	800	10·7	17 34	2400	11·7	53 38	4000	32·1
34 13	1000	11·6	30 55	2600	15·0	55 27	4100	55·0
41 55	1100	13·0	45 26	2800	13·8	59 26	4200	25·1
47 13	1200	18·9	51 23	2900	16.8	5h 25 33	4700	19·1
55 25	1300	12·2	58 13	3000	14·6	30 55	4800	18·6
2h 1 25	1400	16·7	4h 7 2	3100	11·3	35 45	4900	20·7
8 57	1500	13·3	14 29	3200	13·5	40 17	5000	22·1
23 25	1700	13·8	22 1	3300	13·3			
31 12	1800	12·8	29 58	3400	12·6			

Several hundred more were counted, but the times were not observed.

11. *At Magnetic Observatory, Toronto, Canada.*—Communicated by the director, Mr. G. T. Kingston. Nearly 3000 were counted between 10^h 45^m and 18^h 0^m of Nov. 13th, Toronto astronomical time. With the exception of about one per cent their courses were from the constellation Leo.

Owing partly to the remarkably favorable state of the sky during most of the night many of the meteors appeared very large and brilliant, some exceeding Sirius in apparent magnitude, and often exhibiting a variety of colors. Most of them were followed by trains which often left tracks that continued visible from two to four minutes. Two observers were constantly watching excepting from 10^h 45^m to 11^h 0^m and from 11^h 50^m to 12^h 10^m, when only one was engaged. From 12^h 10^m to 17^h 0^m a third observer was frequently though not constantly assisting.

The annexed table shows the number seen at different points of the night together with the corresponding state of the sky.

	Time. h m — h m	Number counted.	State of the sky.
Nov. 13.	10 45—12 0	173	Very clear.
	12 0 13 0	320	"
	13 0 14 0	583	"
	14 0 15 0	489	Occasional very light haze.
	15 0 16 0	375	" "
	16 0 17 0	572	Haze increasing.
	17 0 18 0	365	Clouds 0·4, and very hazy.
	Total,	2886	

12. *At Marathon, N. Y.*—Mr. Lewis Swift, after half past one o'clock, counted 896 meteors, all but five from Leo. At a little after three o'clock a train in Cancer was visible for several minutes which floated to the *north.*

13. *At Bloomington, Ind.*—Prof. T. A. Wylie reports observations by some of the students of the Ind. State University. He had made no arrangements himself for observing as he had watched during the whole of the preceding night, judging, with good reason, that there was more probability of a display on the morning of the 13th.

The whole number observed by the students was 2,500. The numbers increased gradually from 11 o'clock to 3 o'clock, when it reached 20 per minute, at which rate it continued until a quarter before four. The sky then became hazy, and by $4^h\ 40^m$ quite cloudy. At four minutes past five they began regular counting with these results.

From	$5^h\ 4^m$	to	$5^h\ 7^m$	50	meteors.	
"	7	"	10	50	"	
"	10	"	15	56	"	
"	15	"	18	50	"	
"	18	"	22	50	"	
"	22	"	27	50	"	
"	27	"	30	55	"	Clear.
"	30	"	35	50	"	
"	35	"	39	50	"	
"	39	"	43	50	"	
"	43	"	47	59	"	Hazy.
"	47	"	51	50	"	
"	51	"	59	50	"	Cloudy near horizon.
"	59	"	6^h 11	50	"	

The train of one meteor at $5^h\ 25^m$ remained ten minutes in sight, moving slowly *eastward.* The meteor passed through the bowl of the great dipper.

Prof. Kirkwood and Mr. Maxwell thought they saw one meteor in the forenoon of the 13th, and suspected two or three others. They were looking from a shaded place, in the direction of the radiant. Prof. Kirkwood, however, in view of the fact that no considerable fall seems to have commenced until several hours later, thinks it quite probable that there was some optical deception.

14. *At Vevay, Ind.*—Mr. Charles G. Boerner reports the following numbers seen by himself and two assistants in successive intervals, beginning at $1^h\ 4^m$ and ending at $5^h\ 37^m$:

From 1h 4m to 3h.			From 3h to 4h 37 m.			From 4h 37m to 5 37m.		
In 30m	17	meteors.	In 15^m	138	meteors.	In 13^m	161	meteors.
30	90	"	15	114	"	10	156	"
11	48	"	15	162	"	16	147	"
15	116	"	15	119	"	15	102	"
15	93	"	18	111	"	6	25	"
15	153	"	19	174	"	—	—	
						Total in $4^h\ 37^m$,	1926	"

15. *At Des Moines, Iowa.*—Mr. J. E. Hendricks counted in an hour and a quarter, from four o'clock, 250 meteors, only one being nonconformable.

16. *At Manhattan, Kansas.*—Prof. B. F. Mudge with one assistant counted 833 from half past four to half past five o'clock. The time per hundred varied from seven to nine minutes, the last hundred being counted in eight minutes.

As Prof. Mudge was $1^h\ 35^m$ west of New Haven (long. 96° 40′), and as this number for two observers corresponds to about 1,700 for eight observers, his numbers indicate that there was no diminution, but rather an increase in the intensity of the display just after dawn at New Haven.

17. The above abstract of observations is confined principally to the numbers seen on the morning of Nov. 14th. The most palpable peculiarities of the display are its uniform intensity through several hours, and its appearance twelve or eighteen hours later than might have been expected.

We are indebted to Commodore Sands, Prof. Rockwood, Mr. W. S. Gilman, Miss Mitchell, Mr. Fuertes, Mr. Tuttle, Prof. Gummere, Mr. Swift, Mr. Wm. C. Taylor of Philadelphia, Prof. Twining, and others, for valuable observations upon particular meteors remarkable for brilliancy or duration of train. These, together with a few observations on other nights, and some general considerations respecting the display, will furnish matter for a continuation of this article in another number of the Journal.

Meanwhile we respectfully request any persons within two hundred miles of New Haven, who may have observations upon particular meteors, to communicate them to us, or exchange them with ours.

Especially desirable would be any additional observations upon the remarkable meteor which passed about 80 miles north of Philadelphia, and disappeared at an altitude of about 50 miles at a point over Schuylkill Co., in Pennsylvania, at $1^h\ 16^m$, New Haven time. The portions of its train floated in different directions, and continued visible nearly or quite three-fourths of an hour. It was seen and its place noticed, at Williamstown, New Haven, Poughkeepsie, Palisades, Haverford, and other places.

For several communications we are indebted to the courtesy of Prof. Henry, Secretary of the Smithsonian Institution.

H. A. N.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *Spectral analysis of the protuberances observed during a total eclipse of the sun.*—The total eclipse of the 18th of August, 1868, attracted an even more than usual degree of attention. It was observed at Wah-Tonne on the peninsula of Malacca by Rayet; at Guntoor by Janssen; at Beejapoor by Capt. Haig, R. E.; at Barram Point on the Island of Borneo by Hennessy; and at Jamkhandi by Lieuts. Herschel and Campbell. The observations made by means of the spectroscope were particularly interesting and important, and agree upon the whole remarkably well. Rayet was provided with a silvered glass reflecting telescope, the reflector twenty centimeters in diameter, and a direct vision spectroscope with three prisms. When the image of a long protuberance on the eastern border of the sun was brought upon the slit of the spectroscope, a spectrum was seen with nine brilliant lines; of these, four corresponded respectively to B, D, E and F; two corresponded to lines in *b*; two to lines in the group G, and there was one unknown line. The western portion of the sun's margin gave the same lines except that only one violet line was visible. With respect to these observations we may remark that according to the other observers Rayet must have mistaken C for B, and also that the line which he identifies with D is really more refrangible, being separated from it by an interval too large to admit of a doubt as to the want of coincidence. In his letter to the Académie des Sciences Janssen gives no details of the spectral lines observed by him, but communicates the important result that the spectra of the protuberances may be seen at all times, so that it is not necessary to wait for an eclipse. His conclusion is that the protuberances are immense gaseous masses, chiefly composed of hydrogen. Though the merit of having first observed this most important fact belongs undoubtedly to Janssen, it appears that in 1866 Mr. Norman Lockyer suggested that the spectra of the protuberances might be seen by examining the margin of the sun's disc, but did not succeed in observing these spectra until the 20th of October of the present year, or two months after the observation had been made by Janssen. On the 20th of October Mr. Lockyer observed the spectrum of a protuberance, and found it to consist of three lines,—one exactly corresponding with C, one very nearly with F, and one eight or nine degrees of Kirchhoff's scale more refrangible than D. There was no ray at B or at *b*; the region near G was not examined. Observations recently made in Cambridge by Prof. Winlock with the 15-inch equatorial and with a spectroscope with two flint glass prisms of 60°, give the same results. Mr. Lockyer also found that the three bright rays extended to a small distance upon the surface of the sun, and that their lengths were different, the red being the shortest. In a very

interesting and suggestive paper "On the physical constitution of the Sun and Stars," communicated to the Royal Society, May 15th, 1867, Mr. G. J. Stoney remarks that he has several times thought that he observed a bright line somewhat more refrangible than D in examining the sun with a spectroscope composed of two flint glass prisms, as well as two other lines, one between 1025·5 and 1027·7 of Kirchhoff's scale, and another between the two lines D. During the summer of the present year Mr. Stoney saw several faint bright lines in other parts of the spectrum, one coinciding with or very near Kirchhoff's copper line of wave length 522·3. There can be no doubt that the line seen near D was one of those noted by Mr. Lockyer.—*Rayet in Comptes Rendus*, lxvii, 759; *Janssen in Comptes Rendus*, lxvii, 838; *Lockyer in Comptes Rendus*, lxvii, 904; *Stoney in Proceedings of Royal Society*, xvii, p. 1. W. G.

2. *On the magnetism of chemical combinations.*—WIEDEMANN, to whom we owe most of our precise knowledge of the magnetism of compounds, has published a new series of observations of much interest. These all confirm the law already announced by the author that in salts of similar constitution containing the same metal, the magnetism referred to the equivalent of the compound, or the atomic magnetism, is sensibly constant. The author's new results may be stated as follows:

(1.) Observations made by the method used in his former investigations show that the same relations exist for the haloid and oxysalts of cerium, didymium and oxyd of copper (CuO). Taking the unit previously established, we have for the atomic magnetism of aqueous solutions of the salts above mentioned the following numbers:

Sulphate of didymium, - - -	104·4
Nitrate " - - - -	104·2
Chlorid " - - - -	105·2
Acetate " - - - -	105·7
Nitrate of cerium (CeO), - -	48·7
Chlorid of cerium (CeCl), - - -	47·6
Sulphate of copper, - - -	49·5
Nitrate of copper, - - - - -	50·7
Chlorid of copper, (CuCl), - -	48·9
Bromid of copper, (CuBr), - - -	47·7
Acetate " - - -	48·0

(2.) We obtain approximately the same values for the solid salts, especially when they contain water of crystallization. For anhydrous salts the atomic magnetism is in general a little less than for the hydrated salts. This diminution is particularly sensible for the salts of copper and nickel.

(3.) Experiments prove that two diamagnetic elements—as for example, copper and bromine—may give a magnetic compound.

(4.) The author has examined the magnetism after double decomposition of two saline solutions of magnetism M_1 and M_2 known before mixture. After reaction the magnetism is sensibly equal to the sum M_1+M_2. In the greater number of cases the formation of a precipitate exerts no disturbing influence.

(5.) The atomic magnetism of hydrated oxyds is a little higher or lower than that of the corresponding salts.

(6.) The magnetism of precipitated hydrate of sesquioxyd of iron increases rapidly, beginning at the moment of precipitation. The author attributes this fact to the colloidal state of the hydrate at the first moment of precipitation. The differences presented by the atomic magnetism of acetate of sesquioxyd of iron doubtless arise from the same cause. These differences are not remarked in the solution of hydrate of sesquioxyd of chromium, in caustic potash, and in that of oxyd of nickel in ammonia.

(7.) The magnetism of the anhydrous oxyds of the magnetic metals is notably less than that of the saline combinations of these oxyds.

(8.) With the exception of sulphate of manganese, the magnetism of the sulphates of the magnetic metals is very feeble.

(9.) The magnetism of the cyanids of nickel and cobalt disappears almost completely when the cyanids are dissolved in cyanid of potassium, which would not happen if this solution be considered as a double salt.—*Comptes Rendus*, lxvii, 833, *from Monatsbericht der Königl. Preuss. Akad. for July*, 1868. W. G.

3. *On a new series of chemical reactions produced by light.*—Tyndall has communicated to the Royal Society the results of experiments made by subjecting the vapors of volatile liquids to the action of concentrated solar or electric light. The author's paper is too full of detail to admit of an abstract, but we shall give a few of the more salient results. A tube 2·8 feet long and 2·5 inches internal diameter is closed at both ends by glass plates. It may be connected with an air pump and with a series of tubes used for the purification of air. A number of test tubes were converted into Wolfe's bottles by means of corks and tubes. Each test tube was filled partly with the liquid to be examined and introduced into the path of the purified air. When the experimental tube was exhausted and the air then allowed to bubble through the liquid, a mixture of air and vapor entered the experimental tube together, and were then submitted to the action of light. At one end of the experimental tube was placed an electric lamp transmitting an intense beam of light through the tube parallel to its axis. When the vapor of amylic nitrite was allowed to enter the tube in the dark and the beam of light was then sent through the tube, the tube appeared for an instant optically empty; then a sudden shower of liquid spherules was precipitated on the beam. On repeating this experiment with a condensed beam of light forming a cone eight inches long, the cone which was at first invisible flashed suddenly like a luminous spear. The rapidity of the condensing action diminished with the density of the light. The same effects were produced when oxygen or hydrogen were

employed as carriers; when the head of the beam was sifted out through a plate of alum, or when the beam was used without sifting. That the amylic nitrite undergoes decomposition is proved by the formation of brown fumes of nitrous acid. Sunlight produces similar effects. The author proves in the next place that the decomposition is effected by the more refrangible rays of light, and that liquid amylic nitrite is most potent in arresting the rays which affect its vapor. This seems to show that the absorption takes place in the atoms and not in the molecules. The author anticipates wide, if not entire, generality for the fact that a liquid and its vapor absorb the same rays. When the tube is filled with a rare and well mixed vapor, the electric light develops a blue color, which may be pure and deep or milky according to the intensity of the light. The author connects this result with those of Brücke on the colors of the sky. Various other liquids were tried with success. In many cases the condensed vapors formed extremely beautiful and regularly shaped clouds, the particles rotating around the axis of the tube, or round other axes. The most beautiful forms appear to have been those produced by iodhydric acid.—*Proc. of the Royal Society*, xvii, p. 92. W. G.

4. *On the carbonaceous matter of meteorites.*—The analyses of Wöhler and Cloez have shown that certain meteorites contain substances composed of carbon, oxygen and hydrogen, and resembling the last residues of organic substances of terrestrial origin. Berthelot has applied to the carbonaceous matter of the meteorite of Orgueil the method of hydrogenation employed by him with so much success, and has succeeded in producing a notable quantity of hydrocarbons of the formenic series ($C_{2n}H_{2n+2}$) comparable with the oils of petroleum. This result forms a new analogy between the carbonaceous matter of meteorites and substances of organic origin found upon the surface of the earth.—*Comptes Rendus*, lxvii, 849. W. G.

Prof. Brush has called my attention to Mr. Henry's paper, with which I was not acquainted at the time of the publication of my own process for the estimation of manganese as pyrophosphate. That full justice may be done, Mr. Henry's paper is by my request republished in full.—W. Gibbs.

5. *On the separation of Nickel and Cobalt from Manganese*; by T. H. Henry, Esq., F.R.S.—The methods given by the best authorities for the separation of nickel and cobalt from manganese are either inconvenient or inaccurate. The only method which affords exact results is that of Ebelmen, in which the sulphids formed at a high temperature are acted upon by dilute hydrochloric acid; but it is both tedious and unpleasant to pass sulphuretted hydrogen over the oxyds contained in a porcelain tray in a porcelain tube at a red heat; and the modification suggested by Wöhler, of converting the oxyds into sulphids by fusing them with sulphur and carbonate of soda, still furnishes the sulphid of nickel in such state that it is slightly acted upon by very dilute hydrochloric acid.

I have obtained very accurate results by a process differing altogether from those described, but as simple as any.

When chlorid of ammonium and ammonia are added to a warm solution of sulphate of manganese or chlorid of manganese, and afterward phosphoric acid till the precipitation ceases, the whole of the manganese is precipitated; and after filtration no precipitate or even cloudiness is produced by the addition of sulphid of ammonium to the solution. The salt formed is, according to Otto, NH_4O $2MnO$, PO_5+2HO, and after ignition $2MnO+PO_5$. When a solution of chlorid or sulphate of nickel is treated in a similar manner, no precipitate occurs, even on standing a few days in a vessel lightly covered, when sufficient chlorid of ammonium and free ammonia are present.

The following example will show the mode by which I operate.

12·6 grs. of pure sulphate of manganese, $MnOSO_3+4HO$, were gently ignited, and weighed 8·73 grs. =4·11 MnO. 6 grs. of oxyd of nickel, containing a trace of silica, were ignited and weighed then 5·63 grs.; these were dissolved together in hydrochloric acid and water, and the solution diluted; it was made acid with hydrochloric acid, phosphoric acid was added, and the whole heated until nearly boiling: when ammonia was added in excess, a white bulky precipitate was produced which rapidly contracted in volume and became crystalline; after standing twelve hours it was filtered and washed with a solution of chlorid of ammonium and ammonia (the chlorid of ammonium is absolutely necessary). The precipitate was perfectly white, and remained so after ignition, when it weighed 8·67 grs. = 4.33 MnO; it was decomposed and examined, it did not contain a trace of nickel.

The ammoniacal solution was treated by sulphuretted hydrogen, and the sulphid converted into oxyd by roasting with a little carbonate of ammonia. It weighed 5·57 grs.

	Taken.	Obtained.
MnO - - - - -	4·11	4·33
NiO - - - - -	5·63	5·57

I probably drove off a little sulphuric acid on igniting the sulphate of manganese. I have obtained more accurate results with the manganese since.

In operating with cobalt and manganese in the same manner, I obtained a slight excess of manganese, and the salt was slightly pink, but on repeating the operation the separation was complete. I took 3·22 grs. of oxyd of manganese, and obtained 3·15 grs. free from cobalt.—*Phil. Mag.*, IV, xvi, 197.

II. MINERALOGY AND GEOLOGY.

1. *Note on the supposed Fossil Footmarks in Kansas;* by C. H. Hitchcock. (Communicated for this Journal.)—In this Journal, II, vol. xli, p. 174, is a description by Prof. B. F. Mudge of Kansas, of a slab containing four impressions referred to *Leptodactylous* birds. The rock was conjectured to belong to the Lias, and the specimen was obtained from the top of a bluff one hundred and twenty-five feet above the level of Republican river, in the central part of the State. The slab was much weathered, and careful search brought no other instance of the impressions to light.

It was my fortune to examine this specimen at Manhattan the past summer, but during the absence of Prof. Mudge. It had been suggested by eminent authority that these impressions were probably the marks of exogenous leaves. Little credence was given to this view, inasmuch as Prof. Mudge had favored us years before with a careful drawing of the slab; yet we were not prepared for the disappointment produced by the first glance at the slab. No two of the impressions were alike, and they had been obviously chipped out of the stone by the aborigines.

By reference to the description, it may be said that impression A is very much narrower than B, and less skillfully excavated. The edges are rough. Between C and D there is great dissimilarity. The middle toe of the first is very much longer than either of what might be called the middle toe of D, the latter having four toes. The divarication of the toes in A does not correspond with that in B, nor that of C with D, as ought to be the case if these footmarks had been made by the stepping of an animal.

The physical character of the impressions shows them to be unlike ichnites; for the excavation has a sharp edge, while the surface slopes gradually from the top of the layer to the bottom of the furrows in all genuine footmarks.

It is not the first time that aboriginal markings have been mistaken for animal remains. In the Hitchcock Ichnological Museum, Case No. 31, are numerous examples from Westmoreland county, Pennsylvania, of the same character. The roughness of the marks is more obvious than in the Kansas specimens, while the imitations of the tracks of dogs, birds, and other animals had received long generic and specific names. (See this Journal, II, vol. i, p. 268, 1846.) I have also heard of several cases not described in any scientific communication, but mentioned by friends. Hence, it appears to have been a habit of the aborigines to engrave upon the rocks imitations of the feet of animals, whether undesignedly or not does not appear.

The rock resembles some portions of the Connecticut River Sandstone, and it may be that genuine impressions will yet be discovered in Kansas. Prof. Mudge has manifested great enthusiasm in collecting specimens in the State of his adoption, and we anticipate the appearance of valuable papers from his pen.

In this connection it may be worthy of remembrance that in 1866 I found a single impression of the *Cheirotherium* on the Delaware river in Pennsylvania, a few miles below Easton, and that in the following year I found on the New Jersey side an impression of the *Grallator gracilis*, and one or two small reptilian ichnites. These are all Triassic.

26 Pine st., New York, Nov. 2d, 1868.

2. *On Ivigtite;* by G. Hagemann.—Over a year ago I made some examinations of a mineral occurring in the cryolite of Greenland; but as my analyses were too imperfect to determine therefrom a new mineral species, I did not publish them. Mr. Theo. D. Rand has, however, recently published a still more incomplete analysis (see this Journal, II, xlvi, November, 1868) of the same mineral, which he has named *ivigtite*, and I think it therefore proper also to make my examinations known, as they may throw some more light on this species.

Besides the occurrence already mentioned by Mr. Rand, ivigtite is found in small veins passing through the cryolite without reference to its cleavage. It has then a gold-yellow or pale-green color, and forms radiated plates, which seem as elastic as mica. I have also found it forming single stars of radiated plates in the solid cryolite. Sometimes the veins thicken, and the ivigtite appears then as an olive-green solid mass, often quite transparent.

It does not fuse before the blowpipe (Mr. Rand has probably got his sample mixed with cryolite, as he has found it to melt readily). It dissolves in borax with an iron and silica reaction. When heated in a closed tube it affords water with an acid reaction. Analyses I. and III. were made by fusion with carbonate of soda; II. was decomposed with fluohydric acid.

	I (yellow crystals).	II.	III (green mass).	Mean.
Silica,	40·00	----	42·82	41·41
Sesquioxyd of iron,	37·92	39·02	13·06	39·01
Alumina,			27·03	
Soda,	----	10·27	----	10·27
Potash,	----	1·05	----	1·05
Water,	3·06	----	3·93	3·49
Fluorine,	trace	----	trace	trace
Loss,	----	----	----	4·77
				100·00

Adding the alkalies to analysis III, we have

$\dddot{Si}$ 42·82, $\overline{\dddot{Fe}}$ 13·06, $\overline{\dddot{Al}}$ 27·03, $\dot{Na}$ 10·27, $\dot{K}$ 1·05, $\dot{H}$ 3·93, F *tr.* = 98·16.

This would give nearly the ratio of a unisilicate, 1 : 6 : 8 : 1, and the formula:

$$3(\overline{\dddot{Al}}_2, \overline{\dddot{Fe}}_2)\dddot{Si}_2 + (\dot{Na}, \dot{K})\dddot{Si} + \dot{H}O.$$

The great difference between Mr. Rand's and my analyses can perhaps be accounted for by Mr. Rand's having had a considerable quantity of cryolite in his sample. The cryolite would increase the soda and decrease the alumina and silica. This would perhaps account also by its fluorine for a loss of 11·68 per cent in Mr. Rand's analysis. Of my samples, I. was not entirely free from cryolite; II. was more so, and III. I should judge almost entirely.

3. *Notes on the Chemical Geology of the Gold-fields of California;* by J. Arthur Phillips. Communicated by Prof. A. C. Ramsay. (Proceedings Royal Soc., xvi, 294.) (Abstract.)

Rocks of the Gold-Regions of California.—The great sedimentary metallic belt of California lies on the western slope of the Sierra Nevada, beginning in the neighborhood of Tejou Pass, and extending through the state to its northern limit. In consequence, however, of various local circumstances, different portions of this band are of very unequal importance as gold-producing districts.

The slates of the auriferous belt have been shown by Professor Whitney to belong, for a great extent, to the Jurassic period, although the occurrence of numerous Triassic fossils in the gold-bearing rocks of Plumas county and elsewhere renders it more than probable that no inconsiderable portion of the slates in the heart of the gold region are of that age.

The rock constituting the principal mass of the Sierra Nevada is a granite containing only a small proportion of quartz, and in which but one species of feldspar (oligoclase) is generally found.

Lying between the band of metamorphic slates and the great central mass of granite forming the more elevated portions of the chain, are found various crystalline rocks, such as syenites, diorites, and porphyries.

Quartz Veins.—The matrix or gangue of the auriferous veins of California is invariably quartz, which is generally crystalline in its structure, or partially vitreous and semi-transparent. In the majority of cases the quartz constituting an auriferous veinstone is ribboned in such a way as to form a succession of layers parallel with the walls of the lode itself; and some one or more of these laminæ are not unfrequently far more productive than all the others. In some instances these parallel bands are separated from each other by a thin layer of quartz, slightly differing, either in color or structure, from that forming the seams themselves; or they may be only distinguished by a difference of color of two adjoining members of the series.

In many cases, however, laminæ of the enclosing slates divide the vein into distinct bands; and in such instances it will be observed that the thickness of the interposed fragments of slate is sometimes not greater than that of a sheet of the thinnest paper. Cavities or druses containing crystals of quartz occur in all the auriferous veins of the country; and a certain amount of crystallization may also not unfrequently be remarked along the lines of junction of the several bands of which a vein is composed. In addition to ordinary quartz, in a more or less crystalline form, amorphous hydrated silica, or semiopal, and chalcedony are occasionally met with; in some instances the opal is interfoliated between layers of true quartz, and is sufficiently auriferous to repay the expenses of treatment.

The metallic minerals enclosed in the gangue of auriferous veins are ordinary iron pyrites, blende, and galena, and less frequently, arsenical pyrites, magnetic and copper pyrites, and cinnabar. These sulphids invariably contain gold; and veins in which some one or more of them does not occur in considerable amounts, are not regularly and lastingly productive.

Near the surface the iron pyrites and other sulphids become decomposed by the action of air and the percolation of meteoric water through the mass, staining the quartz of a red or brown color, and leaving the gold in a free state. Under such circumstances numerous cubical moulds of iron pyrites are found in the veinstone; and although this mineral has been entirely removed by chemical action, the cavities left contain finely divided gold, obviously liberated by the decomposition of pyrites.

Beneath the line of natural drainage of the country the sulphids remain undecomposed; but if rock containing pyrites be placed in nitric acid the sulphid becomes dissolved, and finely divided, crystalline, or filiform gold will partially occupy the resulting cavities.

In one of the detrital beds in the vicinity of the village of Volcano in the county of Amador, and elsewhere, distinctly marked quartz veins may be observed cutting through the gravel, and evidently formed by the action of water holding silica in solution.

Attention has also been recently directed to bands of auriferous slate found in the copper-bearing belt west of the main gold-belt of the State, and in the foot hills of the Sierra. In this locality the gold, instead of being obtained from a well defined vein, chiefly composed of ordinary quartz, is inclosed in a band of siliceous slaty rock, extending northwest and southeast, and dipping in conformity with the other strata of the district.

The number of fluid-cavities contained in the veinstones of the auriferous lodes of California is seen under the microscope to be exceedingly limited; and in order to obtain sections affording good examples, even of small size, it is necessary to select such bands as may be more than ordinarily crystalline, or to operate on thin fragments of crystals sometimes found lining the interior of drusy cavities. In the more opaque and generally most auriferous portions of veins, the cavities are numerous but exceedingly small, and are often so opaque, apparently rendered so by being internally coated with a lining of clay, that no vacuities can be distinguished.

Out of more than fifty sections of veinstone examined, only some six or seven were found to contain fluid-cavities of sufficient size to admit of any attempt at accurate measurement by means of ordinary appliances; but in all cases there appeared to be considerable differences in the relative dimensions of the vacuities and the enclosing cavities, and the temperatures at which they severally became filled were consequently ascertained by direct experiment. In every instance they were found to require very different degrees of heat to become full, since in the same specimens some of the vacuities disappeared at 180° Fahr., others filled at temperatures slightly above that of boiling water, whilst many, though much reduced in size, remained perfectly visible at 365° Fahr.

Alluvial Deposits.—Although a very large amount of the gold annually obtained was no doubt originally derived from auriferous veins, not more than about one-third of the precious metal collected is procured directly from that source. The larger proportion of the gold now brought into the market is derived from alluvial diggings, in which it is separated by washing from the clay, sand, and gravel with which it is associated.

This gold-bearing drift belongs to at least two distinct geological epochs, both comparatively modern—although the latter period is distinctly separated from the earlier, its materials being chiefly derived from the disintegration and redistribution of the older deposits.

In California the more ancient deposits or "deep placers" are referable to a river-system different from that which now exists, flowing at a higher level, and frequently nearly at right angles to the direction of the main valleys of the present period.

The deep placers are in many localities covered by a thick capping of lava; and the eruptive matter covering them often occurs in the form of basaltic columns, beneath which are found the layers of sand, gravel, and boulders with which the gold is associated. The wood which occurs in these gravel-beds is either beautifully silicified, or is replaced by iron pyrites.

In the more clayey strata of these deposits leaf beds and impressions of leaves are not unfrequently found; and an examination of these made by Dr. Newberry authorizes the conclusion that the auriferous deposits lying beneath the lava are of tertiary age, and that they probably belong to the later Pliocene epoch. Water-worn gold is disseminated throughout the whole mass of these deposits, not, however, with uniformity, but always in greater abundance near the bottom, and more particularly in direct contact with the "bed rock," which is invariably grooved and worn by the action of water.

The materials of which these deep placers are composed are frequently consolidated into a sort of hard concrete, by being firmly bound together by crystalline iron pyrites; and sometimes this cementing material consists either of carbonate of lime or silica. The silica is rarely met with in a crystalline form; but near Kenebeck Hill a cavity, resulting from the junction of several pebbles, was found completely lined with well-defined crystals of quartz. These did not show, under the microscope, the usual fluid-cavities of quartz of the ordinary quartz veins of the country.

Where the cementing material of the conglomerate chiefly consists of pyrites, the enclosed trunks of trees are usually replaced by that mineral, although of two pieces of wood lying in close proximity to each other, one may have become silicified, whilst the other is replaced by iron pyrites.

The assay of several specimens of the cementing pyrites showed that it invariably contained a certain but very variable amount of gold. In order to ascertain whether this exists in the form of water-worn grains mechanically enclosed within the sulphid, or in the form of spongy, crystalline, and filamentary particles, similar to those met with in the pyrites of auriferous veins, various samples were dissolved in nitric acid, and the the residues afterward subjected to microscopical examination. In this way granules of the precious metal, which had evidently been worn by the action of water, were detected, whilst others appeared not to have been subjected to such attrition. Mr. Ulrich states that in the gold-drifts of Australia, pyrites is often found replacing roots and drift-wood, and that samples have, on assay, yielded from a few penny-weights to several ounces of gold per ton.

Hot Springs.—Hot and boiling springs are exceedingly numerous throughout California; and considerable accumulations of sulphur, together with evidences of extensive solfatara action, are met with in different sections of the State.

The most remarkable instance on the Pacific coast of the actual growth, on a large scale, and at the present time, of mineral veins, is probably that afforded by the boiling springs in Steamboat Valley, about seven miles northwest of the great Comstock silver vein in the State of Nevada.

These springs are situated at a height of about 5000 feet above the level of the sea, at the foot of the eastern declivity of the Sierra Nevada. The rock in this locality presents several straight and parallel fissures, either giving out heated water or simply ejecting steam. The first group of crevices comprises five longitudinal springs extending in a straight line, and parallel to each other, for a distance of above 3000 feet. These fissures are partially filled by a siliceous incrustation, which is being constantly deposited on the sides, whilst a longitudinal central crevice allows of the escape of boiling water or steam. On the most eastern of these lines of fracture are five active centers of eruption, from which boiling water is sometimes ejected by the force of steam to a height of from eight to ten feet. These waters are alkaline, and contain, in addition to carbonate of soda, the sulphate of that base, together with chlorid of sodium. There is also everywhere an escape of carbonic acid, whilst from some places sulphuretted hydrogen is also evolved. These products, on arriving at the surface, give rise to the deposition of sulphur, silica, and anhydrous oxyd of iron. The silica and oxyd of iron form semi-crystalline bands parallel with the walls of the fissures; and spongy deposits accumulate around some of the points of most active emergence.

At a considerable distance to the west of those above described, a fissure having the same origin is observed; but this is no longer traversed by currents of hot water, although it still gives off steam and carbonic acid at various points throughout its extent. At its northern extremity a central fissure still remains open; but in other localities it is, for the most part, obstructed by siliceous concretions. This siliceous rock is metalliferous, and, in addition to oxyd of iron and manganese, contains iron and copper pyrites. M. Laur states that he also discovered metallic gold in this deposit.

The rock enclosing the veins of Steamboat springs is granite, which in their vicinity is much decomposed, being often reduced to a cavernous skeleton of silica containing a few scales of mica.

Alkaline Lakes.—In that portion of California lying on the east of the Sierra Nevada are Mono Lake and Owen's Lake, both considerable sheets of water, highly impregnated with alkaline salts. Owen's Lake lies in lat. 36° 20″ south, long. 118° west from Greenwich, and is about twenty miles in length and eight in width.

The waters of this lake have a specific gravity of 1·076, and contain 7128·24 grs. of solid matter per gallon. The salts held in solution are chiefly carbonate and sulphate of soda, with chlorid of sodium; but potash, silica, and phosphoric acid are also present.

The incrustations, which at certain seasons of the year are found to the extent of many hundreds of tons, consist of a white spongy efflorescence, and are, as will be seen from the results of the analysis given in the paper, chiefly composed of carbonate of soda, mixed with a little chlorid of sodium and sulphate of soda.

General deductions.—The author remarks that, in the present state of our knowledge, the results of a careful examination of the gold-regions of the Pacific coast would appear to lead to the following conclusions:—

a. Quartz veins have generally been produced by the slow deposition from aqueous solutions of silica on the surfaces of the enclosing fissures.

b. From the general parallelism with its walls of the planes of any fragments of the enclosing rock which may have become imbedded in a vein, it is to be inferred that they were mechanically removed by the growth of the several layers to which they adhered, and that a subsequent deposition of quartz took place between them and the rock from which they had become detached. In this way were introduced the masses of rock known as "horses."

c. The formation of quartz veins is due to hydrothermal agencies, of which evidences are still to be found in the hot springs and recent metalliferous veins met with in various parts of the Pacific coast.

d. From the variable temperatures at which the vacuities in their fluid-cavities become filled, it may be inferred that they are the result of an intermittent action, and that the fissures were sometimes traversed by currents of hot water, whilst at others they gave off aqueous vapor or gaseous exhalations. This is precisely what is now taking place at Steamboat springs, where the formation of a vein is in progress, and from which currents of boiling water are often poured forth, whilst at other times the fissures give off currents of steam and heated gases only.

e. That gold may be deposited from the same solutions which give rise to the formation of the enclosing quartz, appears evident from the presence of that metal in pyrites enclosed in siliceous incrustations, as well as from the fact of large quantities of gold having been found in the interior of the stems of trees, which in deep diggings are often converted into pyrites.

f. The constant presence of iron pyrites in auriferous veins, and when so occuring its invariably containing a certain amount of gold, suggests the probability of this sulphid being in some way necessarily connected with the solvent by which the precious metal was held in solution. It has been shown that finely divided gold is soluble in the sesquichlorid of iron and, more sparingly, in the sesquisulphate of that metal. It is also well known that iron pyrites sometimes results from the action of reducing agents on the sulphates of that metal. If therefore sulphate of iron, in a solution containing gold, should become transformed by the action of a reducing agent into pyrites, the gold at the same time being reduced to the metallic state, would probably be found enclosed in the resulting crystals of that mineral.

g. The silica and other substances forming the cementing material of the ancient auriferous river-beds have probably been slowly deposited at a low temperature.

The connection existing between the decomposition of granite by the agency of boiling springs, the existence of alkaline plains, and the formation of lakes containing various salts of soda and potash, is too obvious to require comment.

4. *Gold in Rhinebeck, Dutchess county, New York.*—Reports on the auriferous quartz veins of Rhinebeck, have recently been published by Prof. J. G. Pohlé and Prof. John Torrey. According to the former, there are four veins dividing the talco-argillaceous schist in the direction of the cleavage. The quartz is cellular, and brown, with oxyd of iron arising from the decomposition of pyrites; but below it is more compact, and the pyrites is unaltered. Analysis of the rock afforded Prof. Pohlé 963 to 975 thousandths of gold, the rest almost solely silver. Prof. Pohlé adds: "The geological formations on this property are of the Lower Palæozoic age; the Potsdam periods of it are well represented by the metamorphic slates and the calciferous sandstone, but no fossils have thus far been found. The slates (argillaceous mainly) form a characteristic feature of the whole Appalachian range, and the presence of gold here, supplies the link heretofore wanting, in the chain of the Appalachian gold fields; for there can be no doubt that this is a part of the grand belt, taking its rise on the St. Lawrence river and scattering its auriferous treasure for thirteen hundred miles, along the southeastern border of North America, terminating in Alabama.

The discovery of gold in this part of the Appalachian gold fields is due to the scientific attainments and perseverance of E. G. Freligh, M.D."

Dr. Torrey assayed a quantity of the quartz procured by him at the mine, it being "a fair average of the lode," and amounting to several hundred pounds, and states as his results, that of three assays the average yield of gold per ton of ore (2000 lbs.) was $118.49. In no case was the proportion of gold less than $100 per ton.

III. BOTANY.

1. *Botanical Necrology for* 1868.—The most distinguished name upon the list is that of

George A. Walker-Arnott, Professor of Botany in the University at Glasgow, who died on the 17th of June last, in the 70th year of his age. He was born in Edinburgh, Feb. 6, 1799, educated at the celebrated High School of that city, and at the University, where he took high rank as a scholar, especially in the mathematics,—publishing two papers in Tilloch's Philosophical Magazine in 1817 and 1818, while yet a student in arts—and then, turning to law studies, he was called to the bar, as a member of the Faculty of Advocates in the year 1821. He hardly entered, however, upon the duties of his profession, his taste for natural history having been early developed under the lectures of Prof. Jameson and of Mr. Stewart,—the latter a well-known teacher of botany at that time, and his patrimonial estate of Arlary in Kinross-shire sufficing for his support, so that he could devote himself to botany, as he did, with unsurpassed ardor and success. His earliest botanical paper, upon some Brazilian Mosses, was written in French and published in a journal at Paris, in 1823. In 1826 and 1827 he contributed to the Edinburgh New Philosophical Journal a lively narrative of a botanical tour to the South of France and the Pyrenees. He resided for some time at Montpellier, and in Paris, examining the principal herbaria there, also that of DeCandolle at Geneva, and in 1828 the herbaria at St. Petersburgh. In 1831 he married and established himself with his collections at Arlary, where he resided until in 1845, he accepted the professorship of Botany in the University of Glasgow. It was during these fourteen years that the vast amount of scientific work he was able to accomplish was mainly done. He wrote the article *Botany* in the seventh edition of the Encyclopedia Brittanica,—the best treatise of the kind of its day in the English language, and one of the most influential. In conjunction with his early friend, Sir Wm. Hooker, he wrote the Contributions to the Flora of South America, &c., which form a long series of articles in the Botanical Miscellany, Journal of Botany and other similar periodical or serial publications edited by Sir William. He took a similar part in the Botany of Beechey's Voyage; in connection with Dr. Wight he brought out the first volume of the Prodromus Floræ Peninsulæ Indiæ Orientalis; and made numerous contributions to various periodicals.

Up to 1845 or somewhat later Dr. Arnott was one of the foremost botanists of the time, one of the most zealous and sagacious, versed alike in European and exotic botany. But upon assuming the duties of his chair at Glasgow he appears soon to have abandoned the field in which he had won the highest honors, and in which much more was justly expected. He assumed, however, the joint authorship of Hooker's British Flora, taking, we believe, the whole charge and responsibility of the later editions. As he began with Mosses, so for the last fifteen or twenty years of his life, he devoted himself principally to the *Diatomaceæ*, bringing to their investigation all the ardor of his nature and the keenest powers of observation combined with indomitable patience and unwearied care. So that he became in this department of microscopical research one of the highest authorities, and amassed one of the richest collections extant. As a professor he was greatly esteemed and respected, although he may be thought to have come almost too late in life to the professor's chair. In his later years he was much withdrawn from general botanical intercourse; but his surviving correspondents and friends on this side of the ocean cherish very pleasant memories of him.

Nathaniel Bagshaw Ward, Fellow of the Royal and Linnæan Societies, after whom, as its inventor, the Wardian case is named, died, at the ripe age of 77 years, on the 4th of June last. He was born in the east end of London, where his father was a medical practitioner of repute, and where for the greater part of his busy and most useful life he laboriously devoted himself to the same profession. About twenty years ago he exchanged the smoke-charged atmosphere and dingy dwellings of Wellclose Square for the pleasant and airy suburb of Clapham Rise, but still actively engaged almost to the last in professional practice and in his various official duties, mainly in connection with the Apothecaries' Society, filling in succession nearly all its important offices. The renovation and even the maintenance of the celebrated Apothecaries' Garden at Chelsea—the oldest botanical establishment of the country—is probably mainly due to his counsels and exertions. We cannot here enter into the interesting history of the invention of the now familiar Wardian case,—a discovery which grew out of Mr. Ward's persistent endeavors to cultivate the plants he delighted in under the smoke and soot of the dingiest part of London, and which resulted in providing for the poor as well as the rich denizens of the smoky towns of the Old World the inexpensive but invaluable luxury or comfort of being surrounded at all seasons with growing plants and fresh flowers. Nor is the invention less applicable to house-culture, especially of Ferns, under the clearer and purer air of our own country rendered arid by the cold of winter, as hundreds could testify who have enjoyed the benefit, perhaps without knowing even the name of their benefactor. Equally important is the application of the Wardian case to the conveyance of living plants between distant countries. The writer well remembers the first case of growing plants sent to New York thirty-five years ago, which arrived as fresh and healthy as when they left London; and the transmission was quite as successful between England and Australia, when the voyage, confined to sailing ships, was far longer than now. So useful has this contrivance proved to be in this respect, that the Director of Kew Gardens "feels safe in saying that a large proportion of the most valuable economic and other tropical plants now cultivated in England would, but for these cases, not yet have been introduced." The earliest published account of the Wardian case was given by Mr. Ward in the form of a letter to his near friend, the late Sir William Hooker, and was printed in the Companion to the Botanical Magazine for May, 1836. His volume, "On the Growth of Plants in closely-glazed Cases" appeared in the year 1842, and a second edition, considerably enlarged and suitably illustrated, was published a few years later. These were, we believe, Mr. Ward's only scientific publications, excepting reports of communications to various societies with which he was connected, several of them relating to a subject near to his heart, the improvement of the dwellings of the poor in England, and the amelioration in other respects, of their hard condition. A most enthusiastic and in some departments a learned botanist, his contributions to his favorite avocation were not in the form of authorship, to which he seemed averse: a man "given to hospitality" indeed, but as unpretending as it was cordial and unlimited. The coming generation will hardly appreciate the extent of the influence he exerted and the strength of the attachment he inspired so widely among the cultivators of natural science, nor understand, perhaps, how it could be said of him, and without exaggeration, that "for very many years his hospitable house, first in Wellclose Square, and latterly at Clapham Rise, was the most frequented metropolitan resort of naturalists from all quarters of the globe of any since Sir Joseph Banks' day." But while any survive of those who have had the privilege of knowing him personally, or in the friendly correspondence he delighted in, Mr. Ward will be remembered as "one of the gentlest, kindest, and purest," and in the highest sense one of the best of men.

Edward Pœppig, Professor of Zoology in the University of Leipsic, died on the 4th of September, at the age of 70 years. His name is familiar to botanists on account of the collections he made in Cuba, Chili, Peru, and the upper part of the Amazon; which were in part published in Pœppig and Endlicher's Nova Genera et Species Plantarum, etc. Soon after his return from his long journey of exploration, he took the chair of zoology at Leipsic, and consequently abandoned botany.

We hear of the decease of Dr. Schnitzlein in Germany, and of the venerable François Delessert, the head of the house of Delessert since the death of his brother Benjamin, the life-long friend of DeCandolle, and the possessor of the botanical museum and rich library so well known to and so freely at the service of botanists.

A. G.

To these names must be added that of HORACE MANN, assistant in the "Gray Herbarium" at Cambridge. When Dr. Gray sailed for Europe, in September last, Mr. Mann was in failing health, and probably the above necrological notices were written by Dr. Gray about the time of his assistant's death, which took place Nov. 11.

Mr. Mann's botanical fame will rest chiefly upon the large collections which he made in the Sandwich Islands in connection with Mr. W. T. Brigham, and upon the subsequent "Enumeration of Hawaiian Plants" published in the Proceedings of the American Acad. of Arts and Sciences, for Sept. 11, 1866.

In botany itself his name is happily commemorated by the striking genus *Hesperomannia* (Gray), a Mutisiaceous Composite which he discovered in the little island of Lanai of the Hawaiian group. D. C. E.

2. *Index to the Native and Scientific names of Indian and other Eastern Economic Plants and Objects, originally prepared under the authority of the Secretary of State for India*, by J. FORBES WATSON, M.D., etc., etc. London: Trübner & Co. 1868. —A large, beautifully printed volume, of 637 oblong 4to pages, in double columns, the native names belonging to 30 oriental and several European languages, followed by the recognized scientific name, embodying the result of much erudition and immense labor. It will be useful to botanists on the one hand, to philologists on the other, and not less so to commercial men who have occasion to seek for information concerning Indian and other eastern plants and vegetable products, information otherwise oftentimes very difficult to find. A. G.

3. *Synopsis Filicum; or a Synopsis of all known Ferns, including the Osmundaceæ, Schizæaceæ, Marattiaceæ and Ophioglossaceæ (chiefly derived from the Kew Herbarium).* Accompanied by figures representing the essential characters of each genus; by the late Sir WILLIAM JACKSON HOOKER, K.H., D.C.L., F.R.S., A.S., and L.S., Director of the Royal Gardens at Kew, and JOHN GILBERT BAKER, F.L.S., Assistant Curator of the Kew Herbarium. London: Robert Hardwicke, 1865–1868. 8vo, pp. 482, tt. 9.—With the concluding part of the "Species Filicum" its author announced that he was preparing, should life and health be spared to accomplish it, this Synopsis, to be a supplementary volume to the work then just completed, reviewing all the recognized sections, genera and species, giving them brief characters, with general habitats, and with references for synonyms, more full localities, figures, etc., to the pages of the "Species," adding all needful corrections and recently discovered species, and finally embracing the sub-orders named above. The full accomplishment of this purpose he was not permitted to see, for "whilst the sheet which terminates at page 48 was passing through the press, Sir W. Hooker's long career of botanical authorship was somewhat unexpectedly terminated by his death." The manuscript notes already prepared and the annotated copy of the "Species Filicum" were entrusted by Dr. Hooker to Mr. J. G. Baker, an ardent Botanist, already

well known for his volume on the "Botany, Geology, Climate, and Physical Geography of North Yorkshire," and for his "Supplement to Baines' Flora of Yorkshire," with the request that the Synopsis should be carried out to its completion in strict accordance with the original plan.

Mr. Baker has performed his task faithfully and well, and with commendable diligence, for he has thoroughly studied the Ferns, has made himself familiar with the plan of the work already begun, and has brought it to a well-ordered conclusion, all within two years after the death of his venerable predecessor in pteridological science. This book is now the only one which gives a complete view of all known Ferns, and although almost any other writer on the subject would have admitted a considerably larger number of species, it will be to all fern-lovers a most useful work, and in accordance with its classification and nomenclature most collections of Ferns will conveniently be arranged. The number of species admitted and described is 2235. But very many names of doubtfully distinct or imperfectly known species are given, with a few words of diagnosis, along with the recognized species with which they are most closely allied. Of the genera not reached in the "Species Filicum" it may be well to note in this place that *Osmunda* has 6 admitted species, *Todea* has 4, *Schizœa* 6, *Aneimia*, or as it is here written with Swartz' own mistaken orthography, *Anemia*, 26, *Mohria* 1, *Trochopteris* 1, *Lygodium* 16, *Angiopteris* 1, *Marattia* 7, *Danæa* 11, *Kaulfussia* 1, *Ophioglossum* 9, six of which it is quite possible that Sir W. J. Hooker would have united with *O. vulgatum*, *Helminthostachys* 1, and *Botrychium* 6. Of the species familiar to N. American botanists the *Osmundas*, our *Schizœa*, our *Lygodium* and our *Ophioglossum* are left as we know them, *Botrychium simplex* is admitted, *B. lanceolatum* regarded, and probably with justice, as a variety of *B. rutaceum*, *B. lunarioides* is referred to the earlier known *B. ternatnm*, as had already been done by Milde, and *B. Virginianum* is made to include the East Indian *B. lanuginosum*, commonly a larger and more hairy plant. The name is here *virginianum* as written by Swartz, and before him by Linnæus. The name *Virginicum*, commonly received in N. America, seems to have originated with Michaux, from whom it was copied first by Willdenow, and then by all N. American botanists to this day. A few pages of "addenda et corrigenda," and a full index of species and synonyms conclude the work.

D. C. E.

4. *Plantæ Wrightianæ Cubenses.*—Mr. Wright's last distribution of his Cuban collections was made in 1865, or late in 1864. Since then he has been exploring the botany of western Cuba, and has again made very large collections, many of the species being new to the series, for the numbers have ascended from about 3,500 to 3,963. The distribution into sets is now made, all the former sets which remained unsold being incorporated into the new distribution, so that the fullest set contains 2,253 species, and the thirteenth has 637. The larger sets must contain by far the greater

part of the flora of Cuba, and the smaller ones will be of great value to amateur botanists in showing the character of tropical plants. A catalogue of names will be published in due time, especially since it is understood that Mr. Wright has now in preparation his "opus magnum" of a Flora Cubensis.

Persons wishing to purchase these plants may address Charles Wright, Esq., Wethersfield, Conn., or Prof. D. C. Eaton, New Haven, Conn. The price per century is fixed at the low rate of ten dollars in currency. D. C. E.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Flow of the Great Lakes. Interesting Experiments.*—The Detroit *Post*, in an article on the various methods that have been tried from time to time to solve the mystery of the supply and outflow of the Great Lakes, gives an account of a new and successful apparatus just completed, for the purpose of measuring accurately the velocity of the currents in their tidal flow into and out of the lakes. The *Post* says:

"It is now two years since the newspapers of the West began to discuss whether the great lakes are fed by subaqueous springs, or have hidden outlets. The party who favored the theory of subaqueous springs asserted that more water flowed out the St. Lawrence than could be poured in by all the sources of supply known to exist, while the upholders of the idea of hidden outlets contended that evaporation and the visible outflow could not account for all the water which the lakes received and distributed.

"General W. F. Reynolds, Superintendent of the Lake Survey, determined to give this subject such consideration as, in the West, could only be afforded by the engineers employed on that work, and, accordingly, for the past two summers, observations have been made in the St. Mary's, St. Clair, Detroit, Niagara and St. Lawrence rivers for the purpose of ascertaining the exact amount of outflow of the lakes. The river-guaging has, from the start, been entrusted to Assistant D. Farrand Henry, of Detroit, and the apparatus used is one of his own invention. This apparatus is so much more delicate and accurate than any previously tried that the results are of great value.

"To calculate the amount of outflow of any stream, it is necessary to have the area of the body of water, and its mean velocity, at any point. These two quantities multiplied together give the discharge. The first is easily obtained by making frequent soundings across the stream on a known line. The second is more difficult. The only practical methods heretofore in use, for the determination of the velocity are, first by the time of passage of floats past a known line; second, by the difference in the height at which water will stand in two tubes, one of which is bent toward the current at the bottom and the other is straight; and, third, by water mills, as they are termed, which consist of float wheels exposed to the current, the number of revolutions being recorded by a system of decimal gears or telltale. Of these methods the first is the only one which has been used in deep water."

Mr. Henry was dissatisfied with these methods, and devised a "Telegraphic Current Meter," which he has used with perfect success during the past season.

"This meter consists of a propeller, or float wheel, which has on its hub an eccentric, and on the axle an ivory lever, which has one end kept on the eccentric by a light spring, while into the other end a hole is drilled, meeting another hole, drilled at an angle with it, near the center of the bottom side. Into these holes a platinum wire is forced, so that the lever rests on the point of the wire coming out of the center hole. Under this point a small platinum plate is fastened to the axle. The other end of the wire is connected by a hinge joint to a long copper wire, which is fastened to the axle, but insulated from it. At the rear end of the axle are two vanes, at right angles to each other, sufficiently large to keep the wheel in the thread of the current. The whole is suspended by a yoke which has two small eyes on its side.

"The method of using the meter is as follows: A boat being anchored in the stream at the point where the current is to be tested, a weight with a copper wire attached is let down from the stern. The upper end of this wire is fastened to a spring pole, which takes up most of the motion of the boat. This wire is passed through the eyes on the side of the yoke in the meter, a measured cord is fastened to a swivel ring in the upper, and a weight to one in the lower end of the yoke. The meter may now be lowered to any depth, sliding down the anchored wire, the upper end of this wire and of that fastened to the platinum point, being connected with a battery in the boat; then at every revolution of the wheel the circuit will be opened and closed by the eccentric, raising the ivory lever, and thus breaking the connection between the platinum point and plate. If now a Morse's paper register be placed in the circuit, at every revolution of the wheel a dot will be made on the moving paper, and thus the number of revolutions in any given time can be ascertained.

"The observations in the rivers were taken on a known line, one hundred feet apart, and at each five feet of depth. One of the first things noticed was the irregularity of the beat of the counter, showing that the current pulsated.

"The pulsations are not regular, the common maximums being from one-half to one and a half minutes apart, with every five or ten minutes a greater increase or decrease. They are least in the maximum current, and increase toward the bottom and sides of the stream.

"The maximum velocity of the current was found to be at or a little below the surface, and the velocity at the bottom is probably not over two-thirds the maximum.

"The following approximate velocities and discharges of the different rivers is taken from the computations of the work last year. The quantities for the Detroit River are accurately computed:

RIVER.	Maximum velocity. Feet per second.	Miles per hour.	Mean velocity. Feet per second.	Miles per hour.	Disch'ge cubic ft. per second.
St. Marie's	1·921	1·30	0·967	0·66	90,783
St. Clair	4·544	3·09	3·514	2·39	233,726
Detroit	4·800	2·71	3·000	2·04	236,000
Niagara	3·370	2·32	2·258	1·54	242,494
St. Lawrence	1·462	1·00	0·954	0·65	319,943

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *How Crops Grow. A Treatise on the Chemical Composition, Structure, and Life of the Plant, for all Students of Agriculture.* With numerous illustrations and tables of analyses; by SAMUEL W. JOHNSON, M.A., Professor of Analytical and Agricultural Chemistry, in the Sheffield Scientific School of Yale College. New York, [1868] Orange Judd & Co.—The appearance of Professor Johnson's volume is well timed. Every American teacher who has attempted hitherto to inform his students of the bearings of chemical or physiological science, upon the art and practice of agriculture has keenly felt the want of text-books fitted to enforce his precepts and make good his own omissions and defects. It has been plain that, in the lack of such books, few of the schools of agriculture which are now coming into operation in this country, could even attempt to teach their students in accordance with the hopes and demands of the communities in which they have been established. It is consequently a matter of no slight importance when an acknowledged master applies himself to supply the want.

The mere allusion to some of Professor Johnson's earlier essays, such as those on Soil Analyses,* and Agricultural Science,† will recall to the minds of many of our readers the pleasure and satisfaction with which those admirable papers were first received and will secure a hearty welcome for the new work. Before an audience so familiar with the learning and good sense of the author and with the clearness of his style, it would be useless to dwell upon these characteristics, or to say more of them than that they are all conspicuous in the present work. But there are other features peculiar to the book just published, which should be particularly mentioned. It is in the first place, a well-considered and thorough-going attempt to impart knowledge by the inductive and experimental method; as such, it supplies a deficiency that has long existed in English literature.

"For those who have not enjoyed the advantages of the schools, the author has sought to unfold his subjects by such regular and simple steps, that any one may easily master them. It has also been attempted to adapt the work in form and contents to the wants of the class-room by a strictly systematic arrangement of topics, and by division of the matter into convenient paragraphs."—p. 5.

But besides charging the mind of the student with valuable information in a thorough and logical way, the intention is so to impress upon him the manner and methods by which questions are

* This Journal, xxxii, 233. † Ibid., xxviii, 71.

asked of inanimate things, that he may himself become in some sort an investigator of truth.

In these days when a belief in the power of evolving all things from the depths of consciousness is so strong among men, there is something truly refreshing in the spirit of the following paragraphs. Strange as the assertion may seem, there is many a "modern" scientist who dares not enunciate some of these simple truths.

"The *art* of Agriculture consists in certain practices and operations which have gradually grown out of an observation and imitation of the best efforts of nature, or have been hit upon accidentally. The *science* of agriculture is the rational theory and exposition of the successful art."

"Strictly considered, the art and science of agriculture are of equal age, and have grown together from the earliest times. Those who first cultivated the soil by digging, planting, manuring and irrigating, had their sufficient reason for every step. In all cases, thought goes before work, and the intelligent workman always has a theory upon which his practice is planned. No farm was ever conducted without physiology, chemistry and physics, any more than an aqueduct or a railway was ever built without mathematics and mechanics. Every successful farmer is, to some extent a scientific man. Let him throw away the knowledge of facts and the knowledge of principles which constitute his science, and he has lost the elements of his success. The farmer without his reasons, his theory, his science, can have no plan; and these wanting, agriculture would be as complete a failure with him as it would be with a man of mere science, destitute of manual, financial and executive skill" (p. 17.) * * * * "It is the boast of some who affect to glory in the sufficiency of practice and decry theory, that the former is based upon experience, which is the only safe guide. But this is a one-sided view of the matter. Theory is also based upon experience, if it be truly scientific. The vagarizing of an ignorant and undisciplined mind is not theory. Theory in the good and proper sense, is always a deduction from facts, the best deduction of which the stock of facts in our possession admits. It is the interpretation of facts. It is the expression of the ideas which facts awaken when submitted to a fertile imagination and well balanced judgment. A scientific theory is intended for the nearest possible approach to the truth. Theory is confessedly imperfect, because our knowledge of facts is imcomplete, our mental insight weak and our judgment fallible. But the scientific theory which is formed by the contributions of a multitude of earnest thinkers and workers, among whom are likely to be the most gifted intellects and most skillful hands, is, in these days, to a great extent worthy of the divine truth in nature, of which it is the completest human conception and expression."

"Science employs, in effecting its progress, essentially the same methods that are used by merely practical men. Its success is commonly more rapid and brilliant, because its instruments of observation are finer and more skillfully handled; because it experiments more industriously and variedly, thus commanding a wider and more fruitful experience, because it usually brings a more cultivated imagination and a more disciplined judgment to bear upon its work."—(p. 22).

"It has been sought to present the subject inductively, to collate and compare, as far as possible, all the facts, and so to describe and discuss the methods of investigation that the conclusions given shall not rest on any individual authority, but that the student may be able to judge himself of their validity and importance. In many cases fulness of detail has been employed, from a conviction that an acquaintance with the sources of information and with the processes by which a problem is attacked and truth arrived at, is a necessary part of the education of those who are hereafter to be of service in the advancement of agriculture. * * * The subject should be taught so thoroughly that the learner may comprehend at once the deficiencies and the possibilities of our knowledge. Thus we may hope that a company of capable investigators may be raised up, from whose efforts the science and the art may receive new and continual impulses."—(p. iv).

Whether the hope expressed in the last sentence is likely to be realized in the near future is a debatable question which can only be answered by experience. In any event Professor Johnson is to be congratulated in having thus distinctly set forth a noble conception. He may take comfort also in the reflection that he has striven faithfully in behalf of the idea. It will be no fault of his if the times prove inauspicious. The aim is manifestly a high and legitimate aim. When the present rage for burdening the Colleges and Schools of Agriculture with experimental farms, conservatories of plants and collections of costly implements shall have gone by, as it has gone by in Europe, and the principles upon which agricultural art depends come to be taught in a rational and methodical way then the student will have time and temper for the mental discipline and sober reflection so essential for the acquirement of habits of thought and investigation. Then the seed now sown will doubtless bring forth good fruit. It will be strange indeed if the general acceptance of correct notions of teaching agriculture be not greatly hastened by the work before us.

The present volume, though complete in itself and printed as an independent work, is in reality but one of a series of volumes by the same author to be published hereafter. In the second volume now in press, entitled "How Crops Feed," the food of plants and the relations of the atmosphere and the soil to the plant, will be discussed in detail. A third volume, upon Cultivation; or the Improvement of the Soil and of Crops by Tillage and Manures, is promised at an early day; while a fourth work on Stock Feeding and Dairy Produce, considered from the point of view of chemical and physiological science, may finish the series.

It is a bright omen for the future of our country when in aid of a great educational revival such as now animates the land there are produced within its limits books so sensible, so judicious and so learned as this first volume of the promised series unquestionably is. It is a safe prediction that the reputation of the book will not be confined to American soil. F. H. S.

2. *Outlines of Comparative Anatomy and Medical Zoology;* by Harrison Allen, M.D. Philadelphia, J. B. Lippincott & Co. 8vo. 1869.—In the first part of this work the author has given a very concise compendium of the Comparative Anatomy of all classes of Animals. The definitions and explanations are usually clear and accurate, although brief, and the arrangement of subjects very convenient.

The second part is devoted to Medical Zoology, and contains detailed descriptions of all animals immediately connected with medical science, whether on account of their medicinal products, venomous powers, or parasitism. The portion relating to parasitic worms will be found particularly useful.

It is scarcely to be expected that a work like this and covering so wide a field would be entirely free from errors or imperfect statements, yet in this there are fewer instances of these than in most works upon the same subject. Most of those noted are of minor importance: such as the statement on page 16 that *Alcyonium* has six tentacles (correctly stated on page 14, however); on page 25 the distinction between the "skeletons" of *Pennatula* and *Renilla*, and the statement that the valves of Brachiopoda are anterior and posterior; on page 106 where, apparently, the winter-buds (statoblasts) of Polyzoa are regarded as eggs. The division of Radiata into Echinodermata and Cœlenterata (p. 15), although sustained by good authorities, is objectionable, since the latter can be regarded only as an artificial group, at times convenient, perhaps, like the term Invertebrata.

As a whole the book is a very useful one, and will supply a want long felt in this country by teachers and students, as well as by physicians and others interested in Comparative Anatomy. In no other work upon the subject can so many important facts and clear explanations of structure be found in so small a compass. V.

3. *A List of the Birds of New England;* by ELLIOTT COUES, M.D. From Proceedings of the Essex Institute, Salem, Mass. Vol. v. 1868.—During several years past much attention has been given to the distribution of American birds by many ornithologists, and a number of local catalogues have been published. In the present work the author has given a complete and very useful summary of all the important information of this kind that we at present possess with reference to New England Birds. V.

4. *Synopsis of the Birds of South Carolina;* by ELLIOTT COUES, M.D. From the Proceedings of the Boston Society of Natural History. 1868.—An interesting contribution to our knowledge of the distribution of birds in the Southern States. The season of occurrence and relative abundance is given for each of the 294 species. V.

5. *The Butterflies of North America.* Part 2. By WM. H. EDWARDS. Philadelphia, August, 1868. 4to, with five colored plates.—In this number there are descriptions and beautiful illustrations of *Argynnis Callipe*, *A. Hesperis*, *Colias Alexandra*, *C. Helena*, *C. Christina*, *C. Behrii* and *Apatura Alicia*, the latter a new species from New Orleans. V.

6. *Extra Digits;* by BURT G. WILDER, M.D. From Publications of the Mass. Medical Society. Vol. ii, no. 3. Boston, 1868. —Many curious statistics concerning the occurrence of supernumerary fingers and toes, obtained from 152 cases, are given in this memoir, and their significance and interest well discussed. The author solicits additional information and appends a list of questions to be answered in recording new cases of Extra Digits. V.

7. *The Natural Wealth of California, &c.;* by TITUS TEY CRONISE. pp. 696. 1868. San Francisco: H. H. Bancroft & Co.—This volume, which has been some time on our table, is a digest of interesting facts relating to the early history, geography, topography and scenery; climate; agriculture and commercial products; geology, zoology and botany; mineralogy, mines and mining processes, internal communications, immigration, population, and educational institutions. Each County is separately treated. Less

statistical and dry than Hittell's work, Mr. Cronise has made his volume more interesting to the general reader. He has had the advantage of the coöperation of several persons well versed in different departments, as well as a long and intimate familiarity with the commercial and mining resources of the State. Now that the Pacific Railroad promises to place California within reach of a summer excursion, it is important that its resources and wonders should be more familiarly known than hitherto by people on the Atlantic border, and to this end the present volume is timely and well adapted.

8. *Outlines of Physiology, Human and Comparative;* by John Marshall, F.R.S., Professor of Surgery in University College, London; Surgeon to the University College Hospital. With additions, by Francis G. Smith, M.D., Prof. of Institutes of Medicine in the University of Pennsylvania. 8vo, pp. 1026. 1868. Philadelphia: Henry C. Lea.—This is an elaborate and carefully prepared digest of human and comparative physiology, designed for the use of general readers, but more especially serviceable to the student of medicine. Its style is concise, clear and scholarly, its order perspicuous and exact, and its range of topics extended. The author and his American Editor have been careful to bring to the illustration of the subject, the important discoveries of modern science in the various cognate departments of investigation. This is especially visible in the variety of interesting information derived from the departments of chemistry and physics. The great amount and variety of matter contained in the work, is strikingly illustrated by turning over the copious Index, covering twenty-four closely printed pages in double columns.

9. *Reliquiæ Aquitanicæ.*—(H. Baillière, London; Baillière Brothers, New York).—Part v, of this handsomely illustrated work, in quarto, on the relics of man and of associated animals in the caves of Southern France, continues the account of the geology of the Vezère, and contains also a chapter "on the similarity of some implements found in the caves of Dordogne, to some used by the North American Indians," together with observations on the "Germani" of the Roman period, and on the range of the Reindeer. Beside plates of stone arrow-heads and chippings, there are views of the region of the Vezère in Dordogne.

10. *Geological Survey of Illinois;* A. H. Worthen, Director. Vol. III, Geology and Palæontology, v, and 574 pp. large 8vo, with map and sketches, and 20 plates of fossils. 1868. Published by the authority of the Legislature of Illinois.—This volume of the Geological Survey of Illinois has just been issued by the Director, Prof. Worthen, and deserves a much more extended notice than our present space will permit. Part I. (pp. 1 to 287) is devoted to Geology, and in the opening chapter Prof. Worthen gives an able exposition of the Coal measures and Carboniferous system of Illinois, and their relations to the same formation in some of the neighboring States. The subsequent chapters, by Prof. Worthen and his field-assistants, Messrs. Engelmann, Free-

man and Bannister, relate more especially to the geology of various counties of the State, and contain much interesting local and general information.

Part II, (pp. 291–574,) is a valuable contribution to American Paleontology, and reflects much credit on its authors, Messrs F. B. Meek, and the Director of the survey. Many new species of paleozoic fossils are here described, and among them, the interesting Articulates from the Coal-measures will especially attract the attention of paleontologists. An important Supplement, by Mr. S. H. Scudder, containing descriptions of fossil insects from the Coal measures of the State, and another by Dr. J. V. Z. Blaney, giving the results of chemical analyses of various limestones, complete the work, which forms a worthy companion volume to those already issued.

Proceedings Acad. Nat. Sci., Philadelphia, No. 2, March, April, May, 1868.—p. 92, Remarks on fragments of a large Enaliosaurian; *E. D. Cope.*—p. 93, Remarks on the New Species of Osmerus; *T. Norris.*—p. 94, Unionidæ from Lake Nicaragua; *I. Lea.*—p. 96, Reptilia, and Batrachia obtained by the Orton Expedition to Equador and the upper Amazon, with notes on other species, and a Second Supplement on some New Raniformia of the Old World; *E. D. Cope.*—p. 140, Sexual Law in Acerdesycarpum; *T. Meehan.*—p. 142, On a New Mineral in Cryolite; *T. D. Rand.*—p. 143, Description of Sixteen New Species of Unio of the United States; *I. Lea.*—p. 145, Notes on some Singular Forms of Chinese species of Unio; *I. Lea.*—p. 147, Remains of a New Ophidian from the Green Sand of New Jersey; *E. D. Cope.*—p. 148, List of Birds collected at Laredo, Texas; *H. B. Butcher.*—p. 150, Descriptions of four New Species of Exotic Unionidiæ; *I. Lea.*—p. 151, Description of Twenty-six New Species of Melanidæ of the United States; *I. Lea.*

Proceedings Boston Soc. Nat. Hist. Vol. XII.—p. 81, Description and History of a New Species of Erirhinus; *F. G. Sanborn.*—p. 82, Eruption of Mauna Loa; *W. T. Brigham.*—p. 83, Projection of After-pictures; *B. J. Jeffries.*—p. 84, Analysis of Petrosilex; *C. T. Jackson*—p. 85, Birds of Iowa and Illinois; *J. Allen.*—p. 86, The Hairy Men of Yesso; *W. P. Blake.*—p. 87, Migratory Grasshoppers of the United States; *S. H. Scudder.*—p. 88, Apatite at Perth, Canada West; *C. T. Jackson.*—p. 90, Larva and Pupa-case of Microdon globosus; *F. G. Sanborn.*—p. 91, Comparison of Young Larvæ of Insects; *L. Trouvelot.*—p. 92, Analogy between Lima-codes and some Hymenoptera; *L. Trouvelot*—p. 94, The Thaumatrope; *B. J. Jeffries.*—p. 96, New Mineral Locality, Auburn, Me.; *L. Hills.*—p. 97, Metamorphosis of Siredon into Amblystoma; *O. C. Marsh.*—p. 99, Reproduction of lost limbs in the Walking-stick; *S. H. Scudder.*—p. 100, On a Thread Worm infesting the Brain of the Snake-bird; *J. Wyman.*—p. 104, Synopsis of the Birds of South Carolina; *E. Coues.*—p. 128, On the Nature of the Movements involved in the Changes of Level of Shore Lines; *N. S. Shaler.*—p. 136, Deep Dredging between Cuba and Florida; *Pourtales.*—Disappearance of the Cane from the central part of the Ohio valley; *N. S. Shaler.*—p. 138, Nephrite from Turkistan, *R. von Schlagintweit.*—p. 139, Hodotermes Japonicus; *H. Hagen*—A Century of Orthoptera, Decade I, Gryllides; *S. H. Scudder.*—p. 143, Natural History of Alaska; *W. H. Dall.*—p. 145, Absence of Glacial Action in the valley of Youkon River; *N. S. Shaler.*—p. 152, Obituary of Horace Mann; *W. T. Brigham.*—p. 157, Nautilus pompilius in Alcohol, from the Moluccas; *A. S. Bickmore.*—p. 158, Results of Mann's study of the Hawaiian Flora; *W. T. Brigham.*

THE

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[SECOND SERIES.]

ART. XI.—*On some phenomena of Binocular Vision;* by JOSEPH LECONTE, Prof. Chem. and Geol. in University of South Carolina.

[Continued from page 77.]

II. *Rotation of the eye on the optic axis.*

NEARLY all the experiments described in this paper had already been made and the results obtained, when my attention was called to Helmholtz's Croonian lecture "On the normal motions of the eye in relation to binocular vision."* From this lecture I received some useful hints, as to the best method of experimenting on this subject, which have been of great service to me, and have made my results much more satisfactory, without, however, materially modifying them. As these results differ very greatly and fundamentally from those of Helmholtz, I repeated the experiments daily for many weeks, modifying them in every conceivable way, to avoid the possibility of error. I am perfectly sure, therefore, that the results are true for my own eyes, and as far as I have been able to have them verified, they are true also for most other normal eyes. Unfortunately, however, the difficulty of verification for other eyes is very great. Many of these experiments which I find perfectly easy are almost impossible for most persons.

Helmholtz's lecture, I suppose, is the most authoritative statement which we have of the present condition of science on

* Proc. Roy. Soc., April, 1864, vol. xiii, p. 186.

the subjects of the motions of the eye and of the Horopter. It seems to be an abstract of more extended researches which I have not seen. On this account it is obscure in some parts; yet I think I cannot be mistaken in his general results. In order to make myself clear, whether in discussing Helmholtz's results or in describing my own experiments, I find it necessary to define the terms I shall most frequently use. The position of the eye when the optic axes are parallel and at right angles to the vertical line of the face, as when with head erect we look at a point on a distant horizon, is called by Helmholtz the *primary direction of the eye*, and the visual line in this case is the *primary direction of the visual line*. All other directions are called *secondary directions*. A plane which passes through the visual line is called a *meridian plane of the eye*, and the intersection of such a plane with the retina we will call a *meridian of the eye*. The *vertical line of demarkation* is that meridian of the eye upon which the image of an apparently vertical line falls when we look directly at the line, and which therefore divides the retina into two equal halves containing corresponding points in the two eyes. The *horizontal line of demarkation* is that meridian of the eye upon which under similar circumstances the image of an apparently horizontal line falls. The plane which passes through the two visual lines we will call the *visual plane*, and that visual plane which is at right angles to the line of the face the *primary visual plane*. The line joining the root of the nose and the point of sight, and which therefore bisects the angle of optic convergence, we will call the *median line of sight*.

Now Helmholtz gives as the law controlling all the movements of the eye the following, viz: that when the eye turns from its primary to any secondary position, *it turns "on a fixed axis which is normal both to the primary and to the secondary visual line."* In other words, the eye may turn on any axis at right angles to the optic axis, but *does not rotate about the optic axis*. Again he states that "vertical and horizontal lines keep their vertical or horizontal position in the field of vision when the eye is moved from its primary direction vertically or horizontally." This law had been previously stated by Listing but without proof; Helmholtz claims to have established it by experiment. His method is very ingenious. It is well known that if we look for some time at a bright object, and then turn the eye upon a comparatively obscure field, a spectrum having the form of the object will be seen. As such spectra are the result of a temporary modification of the retina itself, they must follow the motions of the eye with the greatest exactness. If therefore the bright object be a *line*, then if there be any rotation of the eye on the optic axis, in turning the eye in various directions the linear spectrum ought to incline to one side or the other. Suppose, then, the object be a bright red vertical line on a gray wall at the exact height of the eye: Helmholtz finds that on gazing at the bright line with one eye, taking care that the eye shall have its primary direction, and then turning the eye in a horizontal plane to the right or left, *the spectrum retains perfectly its verticality.* "I found," he says, "the results of these experiments in complete agreement with the law of Listing." For the ingenious device of Helmholtz for getting the primary position of the eye we must refer the reader to his lecture. I have tried Helmholtz's experiments with similar results. Nevertheless, I believe it may be demonstrated that though rotation of the eye does not take place under the circumstances of these experiments, yet it does so under other circumstances not touched by them; and that in a manner which deeply affects the question of the Horopter. The law of Listing is doubtless true or nearly true when the eyes move together parallel to each other, but is far from being true in strong convergence. The experiments which follow prove beyond a doubt that in my own case and in most other cases tried, *the eyes in convergence rotate on the optic axes outward, and that the amount of rotation increases with the degree of convergence.* Meissner* has attempted to determine experimentally the position of the Horopter, and from the position thus determined he *infers* the rotation of the eyes; my experiments prove *directly* the rotation of the eyes, and from this as well as from direct experiment I hope to establish the position of the Horopter.

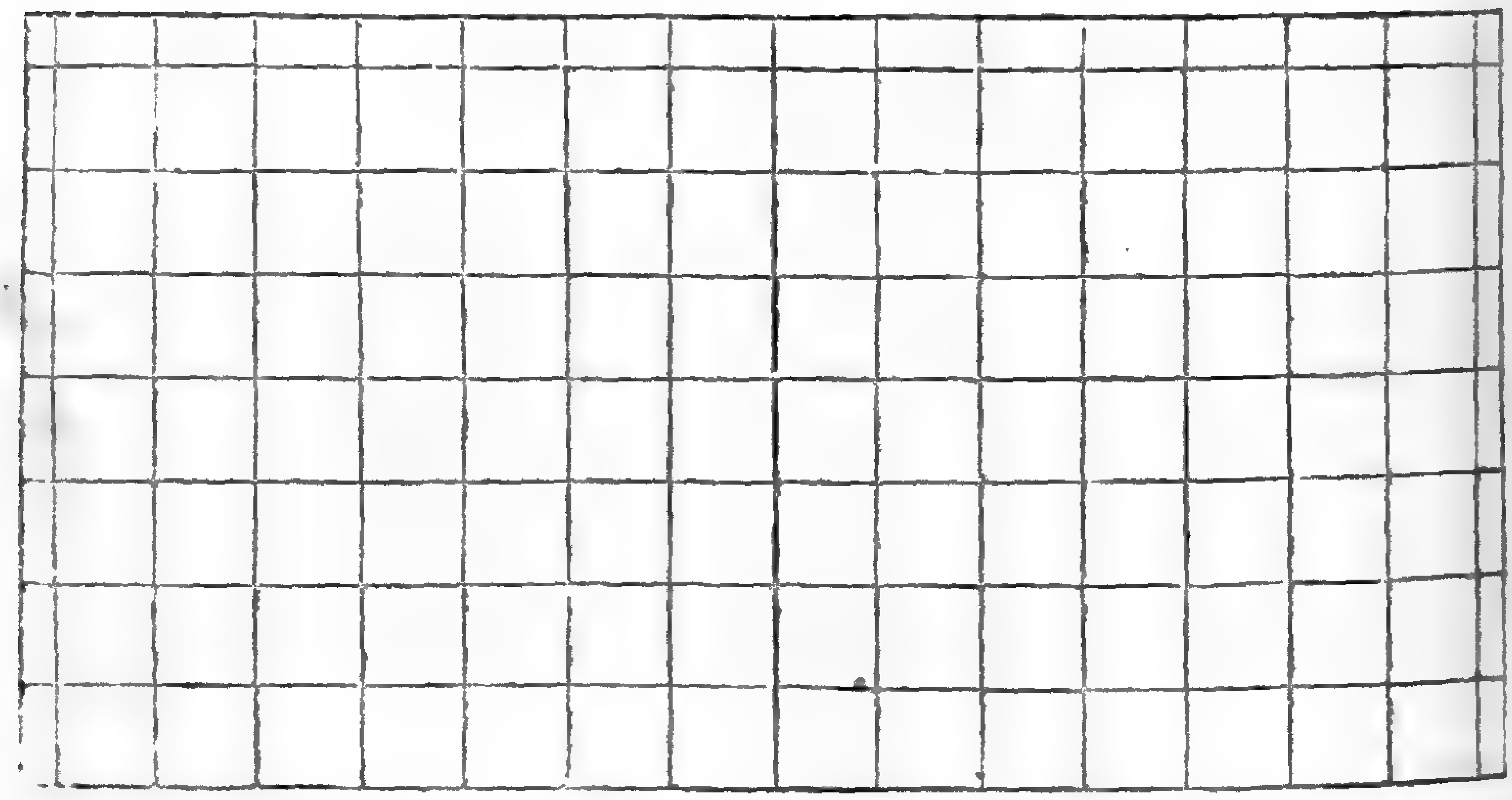
Helmholtz, it is true, admits some degree of rotation of the eye on the optic axis, particularly when the eye makes wide excursions in the field of view; but that he does not regard this as sufficient to interfere seriously with the law of Listing is evident from the form of the Horopter which he deduces. Moreover, according to Helmholtz, these slight rotations are controlled by the law of Donders, viz: "*that the eye returns always into the same position when the visual line is brought into the same direction.*" He regards this law as rigorously exact. "Every position of the visual line," he says, "is connected with a determined and constant degree of rotation." But the experiments about to be described prove that under certain circumstances the law of Donders, too, is far from being true.

We have already stated (p. 73) that when the squares of the ruled diagram (fig. 5) are combined by converging the

* Bib. Un. Archiv. des Scien., II, vol. iii, p. 160.

optic axes, if the amount of convergence be great, the horizontal lines of the two images are distinctly observed to cross each

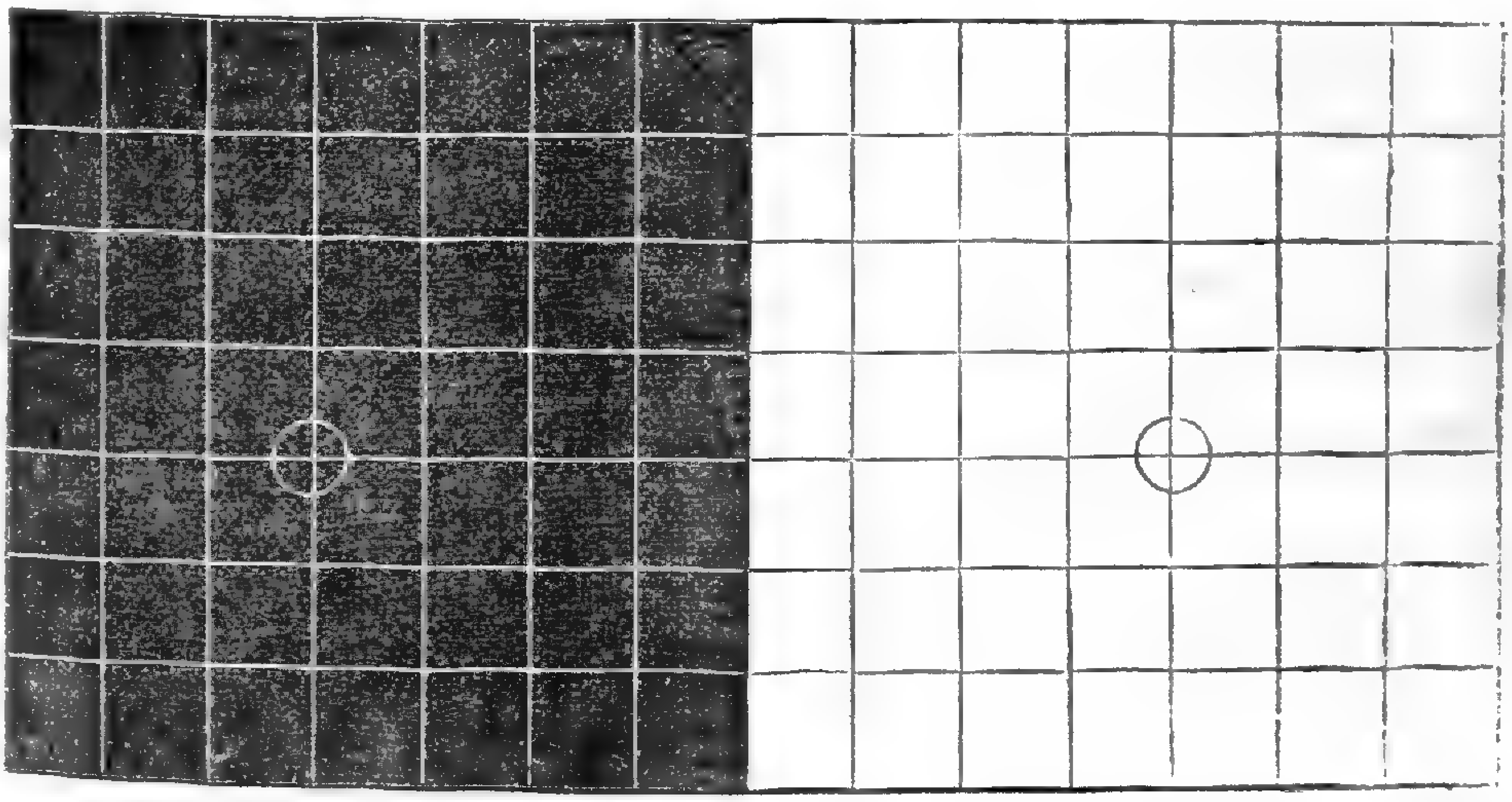
5.



other at small angle. After my attention was once directed to this fact, I could see slight crossing of the horizontals for every degree of convergence, but the verticals seemed to coalesce perfectly. By placing, however, both the diagram and the head perfectly perpendicular, looking straight forward at a point exactly at the same height as the eyes, the visual plane therefore in the primary position, and then slowly increasing or decreasing the convergence of the optic axes, so that the vertical lines of the two images *passed slowly over one another,* it was plainly seen that the verticals of the two images were not parallel, but crossed each other at small angle.

This my original diagram, however, is not well adapted to experiments on this subject for two reasons: 1. It is difficult to distinguish the image of one eye from that of the other. 2. It is difficult to control perfectly the convergence of the eyes. When the vertical lines approach each other, they, as it were, leap and cling together as a single line, even though they really cross at considerable angle; the really crossing lines, by a well known law of stereoscopic combination, being seen as a single line inclined to the visual plane. I therefore constructed a similar diagram, one-half of which consisted of black lines on a white ground and the other half of white lines on a black ground. It is convenient also to have two small circles, one on each half and similarly situated (fig. 6). If I place such a diagram perfectly perpendicularly before me, with the head perfectly erect and the eyes at precisely the same height as the small circles, and then stereoscopically combine the circles by crossing the eyes, I distinctly see the white and black lines, both vertical and horizontal, crossing one another at small angle, as if the images of both eyes had rotated on

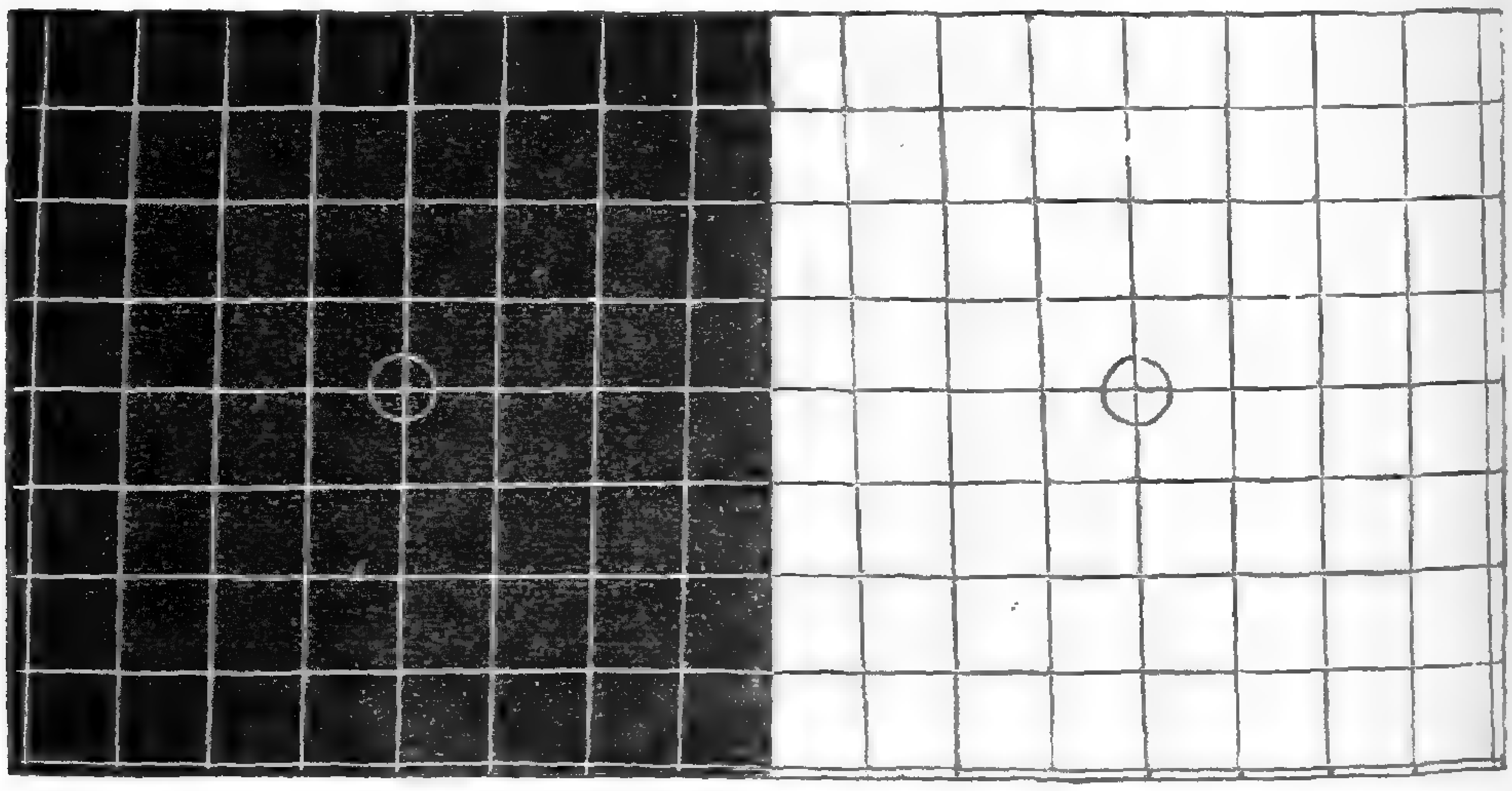
6.



the visual line in opposite directions. This angle of crossing increases as the plane of the diagram is brought nearer and decreases as the diagram is carried farther from the eyes. Or these different angles of crossing may be obtained without moving the diagram or the head, by converging the eyes more and more and causing the white and black vertical lines to pass successively over each other. This is more easily done if there are several small circles on each half, similarly situated but at different distances from each other. In this diagram, the lines being of different colors do not stereoscopically combine easily—they do not cling together as in the other case. Their approach toward or recession from one another and the angle which they make with one another may be marked with the utmost exactness. Nor is there any danger of confounding the two images. For since the eyes are crossed we know that the *white lines* belong to the *right eye* and the *black lines* to the *left eye*, we can therefore determine the direction in which each image rotates. I find always that the black lines or the *image of the left* eye rotates to the right ↷ and the white lines or the *image of the right* eye rotates to the left ↶. Now as the image always moves in a direction contrary to the motion of the eye (differing in this respect from spectra), this indicates *a rotation of both eyes on the optic axes outward* ↶↷.

To test this question still farther I constructed another diagram with the horizontal lines continuous across but the verticals not perfectly vertical, the upper ends of those of the right half inclining to the left and those of the left half to the right by about 1° 20′ (fig. 7). On bringing the circles together I found that at a certain distance of the diagram—but only at a

7.



certain distance depending upon the interval between the circles—the *verticals coalesced perfectly*; the horizontals, however, as might be expected, still crossed at a small angle, and in the same direction as before, viz: the whites or right eye image thus and the blacks or left eye image thus , indicating in this case also rotation of each eye outward. *Beyond* the proper distance the verticals approach but do not attain parallelism; *within* the proper distance they cross in a direction contrary to that in the diagram. When the circles are ten inches apart, the proper distance is nearly three feet and the image therefore about seven inches from the eyes.

Helmholtz has a diagram similar in all respects to my own except turned upside down, in which, he states, both verticals and horizontals coincide perfectly when the circles are combined. Our own figure (fig. 7) turned upside down will answer for Prof. Helmholtz's. We quote his own words: "The horizontal lines are parts of the same straight line; the vertical lines are not perfectly vertical. The upper end of those of the right figure are inclined to the *right* and those of the left figure to the left by about $1\frac{1}{4}$ degrees." But his experience differs from our own in a most unaccountable manner. He says: "Now combine the two sides stereoscopically, *either by squinting or by a stereoscope*, and you will see that the white lines of the one coincide with the black lines of the other, as soon as the centers of both figures coincide, although the vertical lines of the two figures are not parallel to each other." He accounts for this, not by rotation of the eyes but by *the* *principle of the difference between real and apparent verticality.* The ignorance of this principle he believes has vitiated the results of all previous observers. He illustrates this principle thus: "When you draw on paper a horizontal line, and another line crossing it exactly at right angles, the right superior angle will appear to your right eye too great and to your left eye too small; the other angles show corresponding deviations. To have an apparently right angle, you must make the vertical line incline by an angle of about $1\frac{1}{4}$ degrees for it to appear really vertical. We must distinguish therefore between the *really* vertical lines and the *apparently* vertical lines in the field of view." "Now look alternately with the right and the left eye at these figures (fig. 7 turned upside down). You will find that the angles of the right figure appear to the right eye equal to right angles, and those of the left figure so appear to the left eye; but the angles of the left figure appear to the right eye to deviate much from a right angle, as also do those of the right figure to the left eye." Prof. Helmholtz therefore believes that the perfect stereoscopic coincidence of the vertical lines of his diagram is the result of this principle. "Therefore," he says, "not the really vertical meridians of the two fields correspond as has been hitherto supposed, but the apparently vertical meridians. On the contrary, the horizontal meridians really correspond at least for normal eyes which are not fatigued."

On this principle Prof. Helmholtz builds his whole theory of the Horopter. But that this principle cannot account for the phenomena he observes I think can be proved. In the first place, I find that if there be any distinction between real and apparent verticality for my eyes, the difference is too small to be detected by the simple observation of lines drawn at right angles with each other. For my own eyes really vertical lines are also apparently vertical, and lines inclined $1\frac{1}{4}$ from verticality are not at all apparently vertical. I have tried several other normal eyes with the same result. But leaving this aside: in the second place, it is by no means indifferent whether the two halves be combined by a "*stereoscope or by squinting.*" If they are combined by a stereoscope as stereoscopes are usually constructed, the right half is looked at by the right eye and the left half by the left eye, so that the point of sight and the plane of combination is *beyond the diagram;* coincidence in this case, therefore, would be a true illustration of Prof. Helmholtz's principle. But if they are combined by squinting, the eyes are crossed, and therefore ***the right eye is looking at the left half and the left eye at the right half*** of the diagram, and therefore in Prof. Helmholtz's own words, the verticals should "deviate much from a right angle," viz: $2\frac{1}{2}$ degrees. I have tried many eyes and I have yet found *none* in which the coincidence of the verticals of Prof. Helmholtz's diagram was perfect when combined by means of a stereoscope, i. e., beyond the diagram; but I have found *one* person to whom the coincidence seemed to be perfect when the combination was made by squinting.

It is evident, then, that Prof. Helmholtz's principle cannot explain the stereoscopic coincidence by squinting in his own experiment. I myself believe that if the coincidence takes place only by squinting (as in the case mentioned above), it can only be explained by rotation of the eyes *inward*. It is true that in this case the horizontals ought to cross also; but Prof. Helmholtz himself admits that such is sometimes the fact, but attributes it to fatigue. "After keeping the eyes," he says, "a long time looking at a near object, as in reading or writing, I have found that the horizontal lines crossed each other; but they became parallel again when I had looked for some time at a distant object."

On reading Prof. Helmholtz's lecture and finding his results so different from my own, I immediately tried his figure by squinting, but found the verticals cross one another at an inclination much greater than in the diagram itself, while the horizontals also crossed but at a less angle. On turning the figure upside down, however, the verticals coincided perfectly when the proper distance was obtained, though the horizontals crossed as before. All these phenomena are easily explained by rotation of the eyes *outward*. To test the question still more thoroughly, I then constructed other diagrams in which both verticals and horizontals were inclined so as to make an angle of $1\frac{1}{4}$ degrees with the true vertical and the true horizontal (fig. 8) and therefore perfect squares with one another. At the proper distance, when the small circles were brought together, *the coincidence of both verticals and horizontals seemed to be perfect*. When the plane of the diagram was too near or too far, all the lines crossed, in the one case in one direction and in the other case in the other direction. I then constructed still other diagrams, in which the inclination of the lines with the true vertical and the true horizontal were 40 minutes, $2\frac{1}{2}$ degrees and 5 degrees. In all cases I brought the lines in coincidence, but of course by different degrees of convergence. In the last the optic convergence necessary was extreme, and the strain on the eyes considerable; but in the other cases there was not the slightest difficulty or strain. Recollecting, however, that Prof. Helmholtz supposed that the change of position of the horizontals might be the result of fatigue, I tried repeatedly after long rest but always the phenomena were precisely the same. In the diagram in which the

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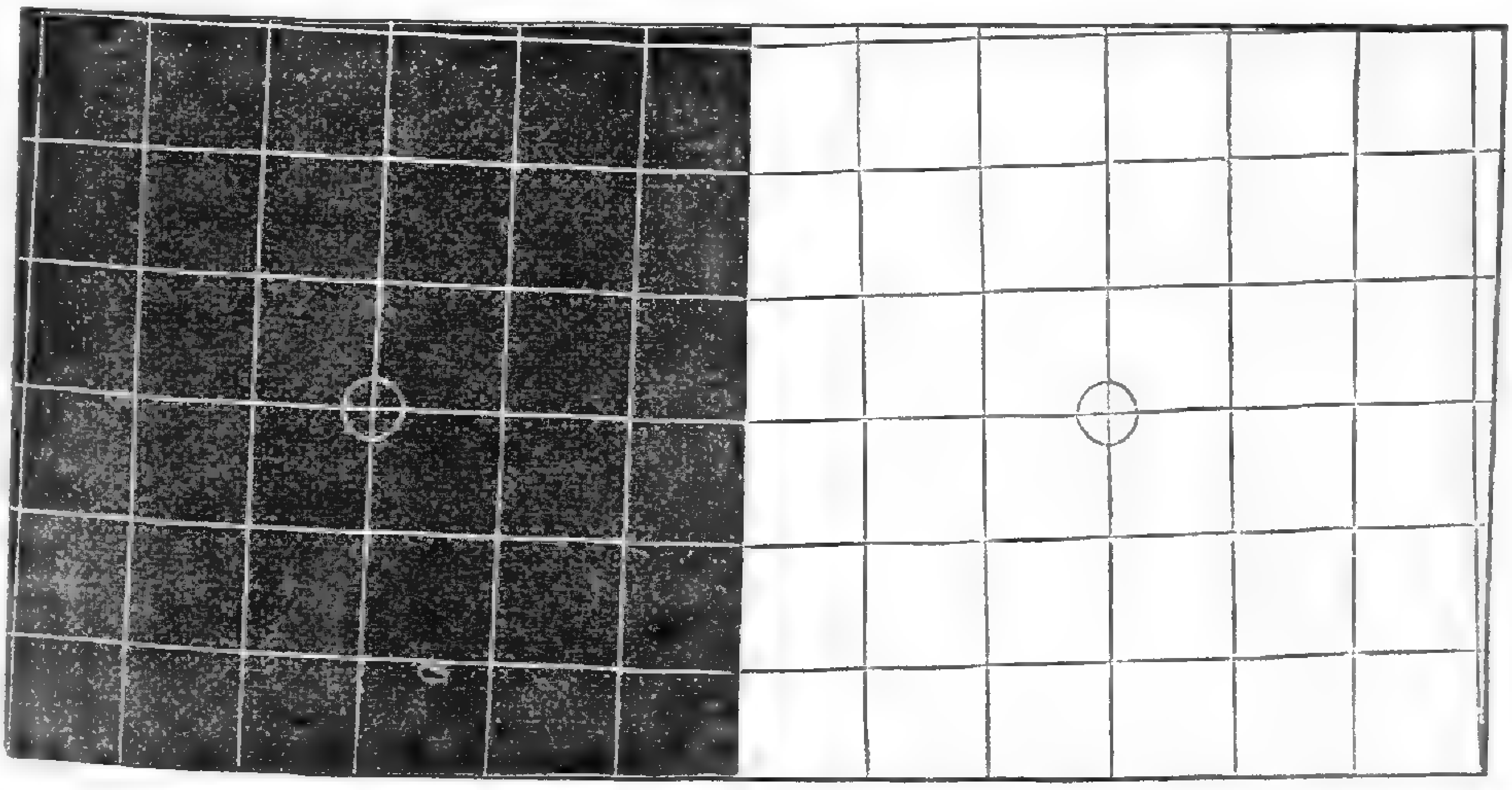

inclination of the lines was five degrees I observed, however, that a *greater degree of convergence was necessary to bring the horizontals in coincidence than to bring the verticals into coincidence.* The difference in the distance of the diagram in the two cases was about two inches and the difference in the distance of the point of sight was about one-half inch. I cannot explain this except by supposing that the form of the optic globe was changed by the excessive action of the muscles.

I can conceive of no possible source of fallacy in these experiments. From long practice they have become almost as easy to me as any ordinary act of vision. They do not now fatigue my eyes in the slightest degree. I see the lines of the two images, which I bring together just as plainly as if they were black and white threads. While watching them I control their motions almost as perfectly as if I was sliding with my hands two frames with white and black threads stretched across them. There is not the shadow of a doubt, therefore, that in my own case the eyes in convergence rotate slightly outward, and that the amount of rotation increases with the degree of convergence.

I next proceeded to determine the amount of rotation for different distances of the point of sight. In the diagram in which the inclination of the lines was 5 degrees, the distance of the image was only 2 to $2\frac{1}{2}$ inches; for the lines inclined $2\frac{1}{2}$ degrees, the distance of the image was 4 inches; for lines inclined $1\frac{1}{4}$ degrees, the distance was 7 inches; and for 40 minutes, the distance was about 12 to 14 inches. I am able, by great strain, to obliterate or nearly obliterate the common field of view of the two eyes. In this case of course the eyes both look at the root of the nose. In this extreme convergence I find that lines coincide which make with each other an angle of 22 degrees or 11 degrees with the vertical. This would seem, therefore, the extreme rotation for my eyes. The distance of the image in this case is nearly at the root of the nose.

If, however, in extreme convergence, rotation on the optic axes takes place to the extent of 11°, this rotation ought to be detectable by means of ocular spectra or even by direct observation of the eye itself. I determined to try these also. My method of experimenting with ocular spectra is as follows: Standing in a somewhat obscure room, I gaze with the left eye (the other being shut) at a vertical crevice in a closed window until a distinct spectrum is obtained. Placing myself now opposite a vertical line on the wall of the room, with my right side toward the wall, I turn my head until my left eye II (fig. 9), looking across the root of my nose *n*, can see the vertical

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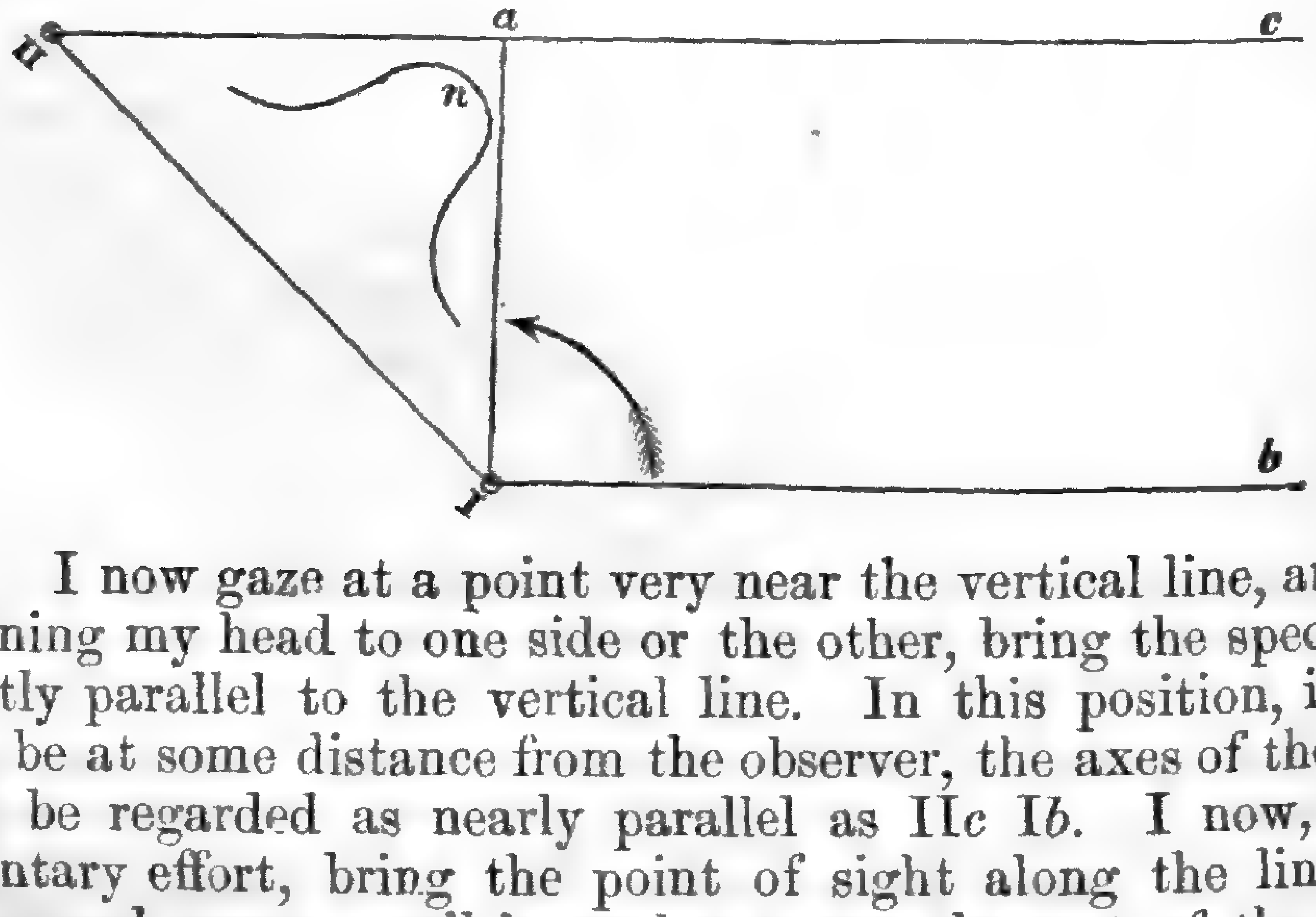
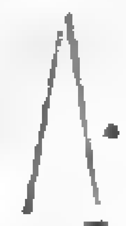


line. I now gaze at a point very near the vertical line, and by inclining my head to one side or the other, bring the spectrum exactly parallel to the vertical line. In this position, if the wall be at some distance from the observer, the axes of the eyes may be regarded as nearly parallel as II*c* I*b*. I now, by a voluntary effort, bring the point of sight along the line II*c* nearer and nearer, until it reaches *a* near the root of the nose. In doing so the spectrum is always seen to incline to the left thus \. On relaxing the convergence and looking again at the wall, the spectrum retains its inclined position for an appreciable time and then gradually recovers its original verticality. In similar experiments with the right eye the spectrum is always seen to incline to the right thus /.

I next tried direct observation of the eye itself. As I could not find any one with the necessary control over the eyes, I was compelled to make myself the subject of this observation. While therefore, with the right eye shut, I gaze with the left eye across the root of the nose on vacancy, or on a distant object as in the figure (fig. 9), an observer conveniently placed near the visual line carefully examines the iris of my eye so as to recognize the position of the radiating lines. When now, without changing the position of the visual line of the left eye, I turn the right eye inward, as in the previous experiment, until the point of sight is at *a*, the globe of the left eye is distinctly seen to rotate outward. I got four different persons to make this observation upon my eye, and the testimony of all was the same.

I had proceeded thus far in my experiments when I was led to reflect farther upon the phenomena presented by the diagram in which the lines were highly inclined. In this diagram, it will be remembered, the verticals were combined with more facility than the horizontals. I now repeated all my experiments with more care and with especial reference to this point. As I expected I found the same true for all the diagrams, but the difference was so small that it had escaped detection. This led me to suspect that there might be some truth in Prof. Helmholtz's principle of real and apparent vertical. I therefore constructed many other diagrams to test this point. I constructed first a diagram exactly like figure 6, except that the circles were the same distance apart as my eyes, viz: $2\frac{1}{2}$ inches. On placing this diagram before me and gazing on vacancy, the eyes therefore in their primary position, the circles were brought together. In this experiment the *verticals came together parallel.* I sometimes thought there was a scarcely perceptible inclination in the direction required by Helmholtz's principle, viz: thus . If any such inclination really existed, it could not have been more than $10'$ for each line with the vertical, or $20'$ with one another; for this angle I can distinctly detect under these circumstances. I next constructed a diagram like Prof. Helmholtz's, except that the outward inclination of the verticals was only $40'$ instead of $1\frac{1}{4}°$. On combining the two halves of this diagram by means of a stereoscope, there really seemed to be perfect coincidence of both verticals and horizontals; but I soon found by trying several, that stereoscopes differ much in this respect. I therefore discarded them as unreliable. On combining the same diagram with the naked eye in the manner of a stereoscope, i. e., beyond the plane of the diagram, the verticals coincided perfectly when the point of sight was about twelve inches distant, but the horizontals very perceptibly crossed, though certainly, I think, at an angle less than $40'$—it seemed about $20'$. On combining the two halves by squinting (of course turning the diagram upside down), I found the result precisely the same when the point of sight was at the same distance, viz: twelve inches. In the next diagram which I constructed the verticals inclined 1¼ degrees and the horizontals 50 minutes, the difference being therefore 25 minutes. In this case both seemed to combine perfectly when the point of sight was distant 7½ inches. The next diagram tried had the verticals inclined 5° and the horizontals 3° 45′, the difference being 1¼ degrees. In this case both verticals and horizontals combined perfectly at the distance of 2·2 inches. I then tried one in which the verticals inclined 10°. In this case I could not make perfect coincidence of both verticals and horizontals until the difference of inclination was made as great as 5°. The diagram used is shown reduced in the figure (fig. 10). The point of sight in this

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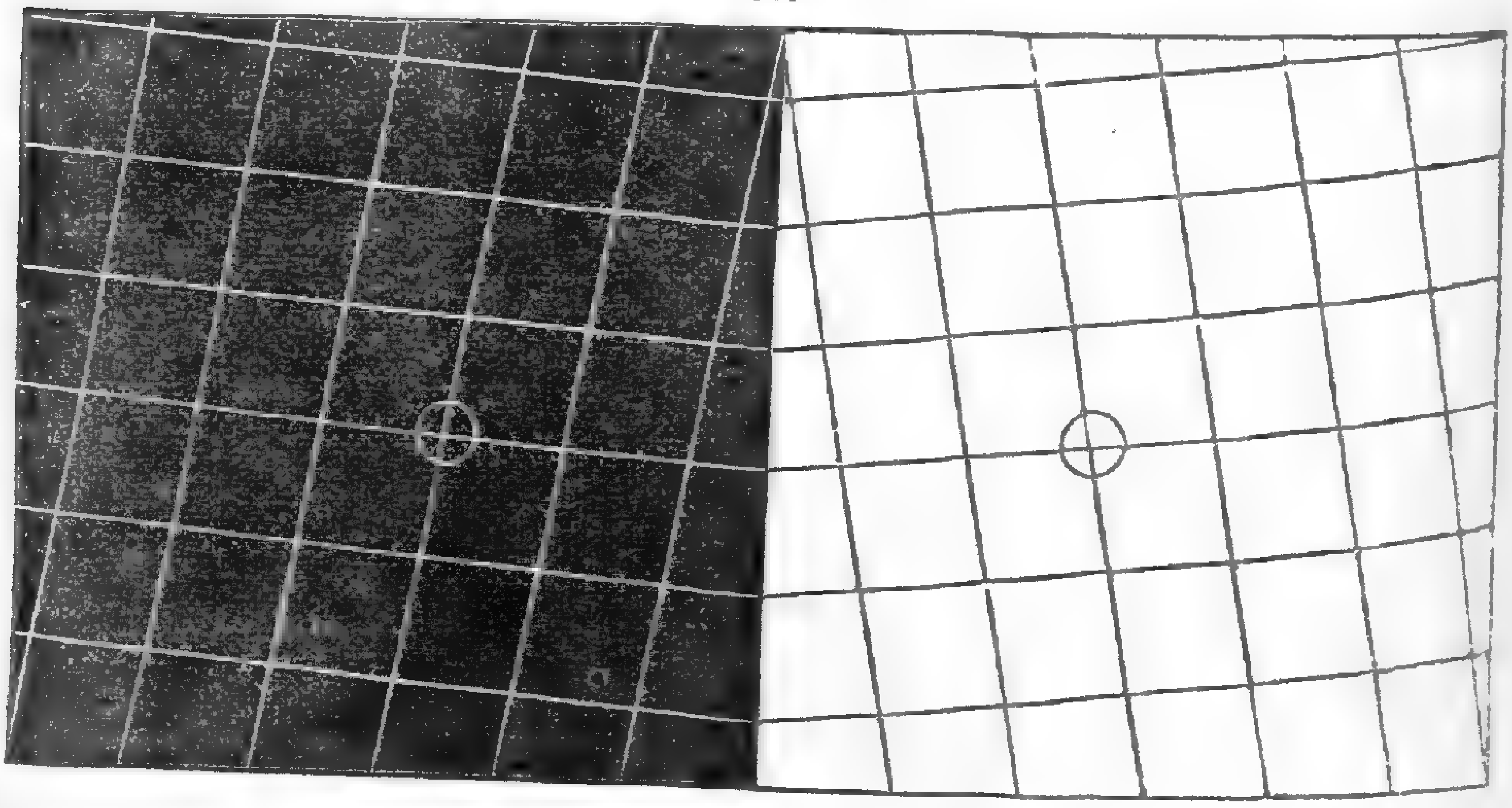


experiment was only 1¼ inch from the line joining the optic centers, or about one-quarter inch from the root of the nose.

I attribute these phenomena to a slight distortion of the ocular globe under the action of the oblique muscles—a distortion which increases with the degree of optic convergence. We will refer to this again.

In all the experiments described above the greatest care was taken that the visual plane should be in the primary direction, i. e., at right angles to the line of the face, and especially that the median line of sight should be at right angles to the plane of the diagram. I now wished to try the effect of turning the visual plane upward and downward. Meissner, from his experiments on the position of the Horopter, had arrived at the conclusion that the rotation of the eye was zero, whatever be the degree of convergence, when the visual plane was inclined downward 45° from its primary position, and that the rotation increased as the plane was elevated toward the eyebrows. I was anxious to test this result. The plane of the diagram still remaining vertical I now turned the face upward (taking care, however, that the eyes should still be on an exact level with the circles of the diagram), until the eyes looked in the direction of the point of the nose. In this position, on stereoscopically combining the small circles, the lines, both vertical and horizontal, in all cases *maintained their true position :* i. e., in the diagram with parallel lines (fig. 6), the coincidence of all the lines was perfect, in the diagram with inclined verticals (fig. 7), the horizontals coalesced perfectly and the verticals crossed at their true angle of inclination, while in the diagram with the verticals and horizontals both inclined (fig. 8), both the verticals and horizontals crossed at their true angle of inclination. I tried the same experiment for various distances and therefore various degrees of optic convergence, but always with the same result. *There is,* therefore, *no rotation* of my eyes *when the plane of vision is inclined* 45° *downward.* In continuing the inclination still farther downward I observed a decided rotation of the eyes in the contrary direction, i. e., *inward.* As the eyes are raised from the position, 45° downward, the rotation increases until the visual plane is again in its primary direction. When the visual plane is raised above this, however, I do not find the rotation to increase as stated by Meissner, except in cases of extreme convergence, but rather to decrease again, although it does not again become zero.* In strong convergence, however, as for instance, when the point of sight is less than seven inches distant, the rotation continues to increase as stated by Meissner.

In all these experiments, in order to detect the true rotation, it is absolutely necessary that the median line of sight should be exactly at right angles with the plane of the diagram. The least error in this respect will cause *perspective convergence* of the parallel verticals, or increase or decrease of the angle of inclination of the inclined verticals. With the diagram three feet distant, if my eyes look one inch above or below their true level, on combining the two halves of the diagram I can detect the perspective convergence, upward or downward, with the greatest ease. In all cases, also, but particularly when the convergence is very strong, it is necessary to fix the attention on that horizontal which passes through the small circle, for those above and below converge by perspective.

* More recent experiments, just concluded, have convinced me that in my own eyes, if the convergence is very slight, the outward rotation *does* reach zero and may even be converted into an *inward* rotation. The reason is, that when my eyes are parallel or nearly so, elevation of the visual plane causes *inward* rotation. In some other eyes, however, I have found that elevation of the visual plane when the eyes are parallel causes outward rotation as stated by Meissner. In these cases, therefore, Meissner's results on this point are entirely true.

In these experiments the size of the diagrams is of little importance. I have used them of every size from 5 by 10 inches to 15 by 30 inches.

My next desire was to determine how far these results were general for normal eyes. The great difficulty was to find any one who was able to repeat the experiments. Nevertheless, I have found four young persons with normal eyes who, with some practice, have succeeded in all except the most difficult of them. *Their results agreed perfectly with my own.* In a fifth case, however, in which great difficulty was experienced and the results were uncertain, I was led to believe that the eyes in convergence rotated *inward.* It is not improbable, therefore, that normal eyes differ in this respect.

We believe, therefore, that we are justified in the conclusion that when the eye is in its primary position and therefore passive, the vertical line of demarkation coincides with the vertical meridian and the horizontal line of demarkation with the horizontal meridian of the eye, and therefore these two lines of demarkation are at right angles to each other. But as soon as the eyes begin to converge, the oblique muscles (particularly the inferior oblique) begin to act, rotating the eye on the optic axis and slightly distorting its form; so that the vertical line of demarkation is now not only no longer coincident with the vertical meridian but also no longer at right angles to the horizontal line of demarkation. Both the rotation and the change in the relation of the two lines of demarkation increases with the degree of optic convergence. It is possible that the frequent action of the muscles distorting the globe of the eye may leave some permanent impress upon the form of the globe, so that even in a passive state the vertical line of demarkation does not coincide perfectly with the vertical meridian. If so, then to that extent Helmholtz's principle of real and apparent vertical in the primary position of the eye would be true. Or to express it differently: we have seen that the inclination of the vertical upon the horizontal line of demarkation decreases as the point of sight recedes; at $1\frac{1}{4}$ inches it is 5°, at 2·2 inches it is $1\frac{1}{4}$°, at 7·5 inches it is 25′, and at 12 inches 20′. It is possible that even when the point of sight recedes to infinite distance and the horizontal line of demarkation becomes coincident with the horizontal meridian, the vertical line of demarkation may still make a small angle with the vertical meridian. If so, this angle is the difference between the real and apparent vertical spoken of by Prof. Helmholtz. We do not yet admit this as probable, however, for the natural position in which all lines at all distances combine when the visual plane is inclined 45° downward seems inconsistent with this idea.

The decrease of the rotation of the eye when the visual plane is inclined downward and its increase when the visual plane is inclined upward, seems to be the result of the relative power of the two oblique muscles. Ordinarily the inferior oblique is the stronger and the rotation is therefore outward, but as the visual plane is inclined downward, the action of the two become more and more nearly equal, until at 45° they balance each other and there is no rotation. Below 45° the action of the superior oblique predominates and the eye therefore rotates inward. In turning the visual plane upward and converging strongly, the action of the inferior oblique predominates more and more.

It will be observed that the rotation of the eye which we have demonstrated necessitates, in optic convergence, a difference between the real and apparent vertical; but our views differ entirely from those of Prof. Helmholtz's in the following respects: 1. Prof. Helmholtz admits only a difference between real and apparent *vertical*; we have shown a difference between the real and apparent horizontal as well as the real and apparent vertical. 2. Prof. Helmholtz's difference is a constant one, viz, $1\frac{1}{4}°$; ours varies from 11° to 20′ and probably to zero. 3. According to Prof. Helmholtz the relation of the apparent vertical to the apparent horizontal is a constant one, viz: an angle of about $88\frac{3}{4}°$; our experiments prove that this relation varies to the extent of 5°.

It is certain, therefore, that the law of Listing is far from being true in strong convergence. Evidently the reason is that in convergence muscles are used which are not used in simply turning the eyes from side to side, as in the experiments used by Helmholtz to prove this law (p. 155). That different muscles are used in strong convergence is easily shown as follows: It is easy to turn either eye inward until it looks in the direction of the root of the nose, provided the other eye moves parallel with it, i. e., outward; but it is almost impossible to turn both eyes at the same time so as to look at this point. Great strain is experienced in producing convergence even much short of this. The eyes are turned from side to side, parallel to each other, by means of the interior and exterior recti muscles, while in convergence the oblique muscles are also used. For this reason Prof. Helmholtz's experiments on spectra do not apply to convergence.

The law of Donders is equally untrue for strong convergence. This law asserts that the position of the eye is rigorously constant for every position of the visual line. But in the experiment represented by figure 9 the eye II, *although the direction of its visual line is unchanged, rotates on its axis* when the visual line of the other eye is turned from the direction I*b* to the direction I*a*.

The reason is, that as I. turns toward *a* the oblique muscles in both eyes begin to act. It is probable that the action of the oblique muscles and therefore the rotation of the eye, is *consensual with the two adjustments and with the contraction of the pupil;* and it is well known that under the circumstances represented by the figure, the pupil of the eye II would contract also, although the direction of the visual line is unchanged.

III. *The Horopter.*

If we look intently at any point the visual lines converge and meet at that point. Its image is therefore impressed on exactly corresponding points of the two retinæ, viz: on the central spot of each. A small object at this point is therefore seen single. We have called this point the *point of sight.* All objects beyond or on this side of the point of sight are seen double, for their images do not fall on corresponding points of the two retinæ. But objects above or below, or to one side or the other of the point of sight, may possibly be seen single also. *The sum of all the points which are seen single,* while the point of sight remains unchanged, *is called the Horopter.* Or it may be expressed differently thus: Each eye projects its retinal images outward into space and therefore has its own field of view crowded with its own images. When we look at any object, we bring the two external images of that object in coincidence at the point of sight. Now the point of sight, together with all other corresponding points of the two fields of view which coalesce at that moment, constitute the Horopter. Of course the image of all points lying in the Horopter fall on *corresponding points* of the retina.

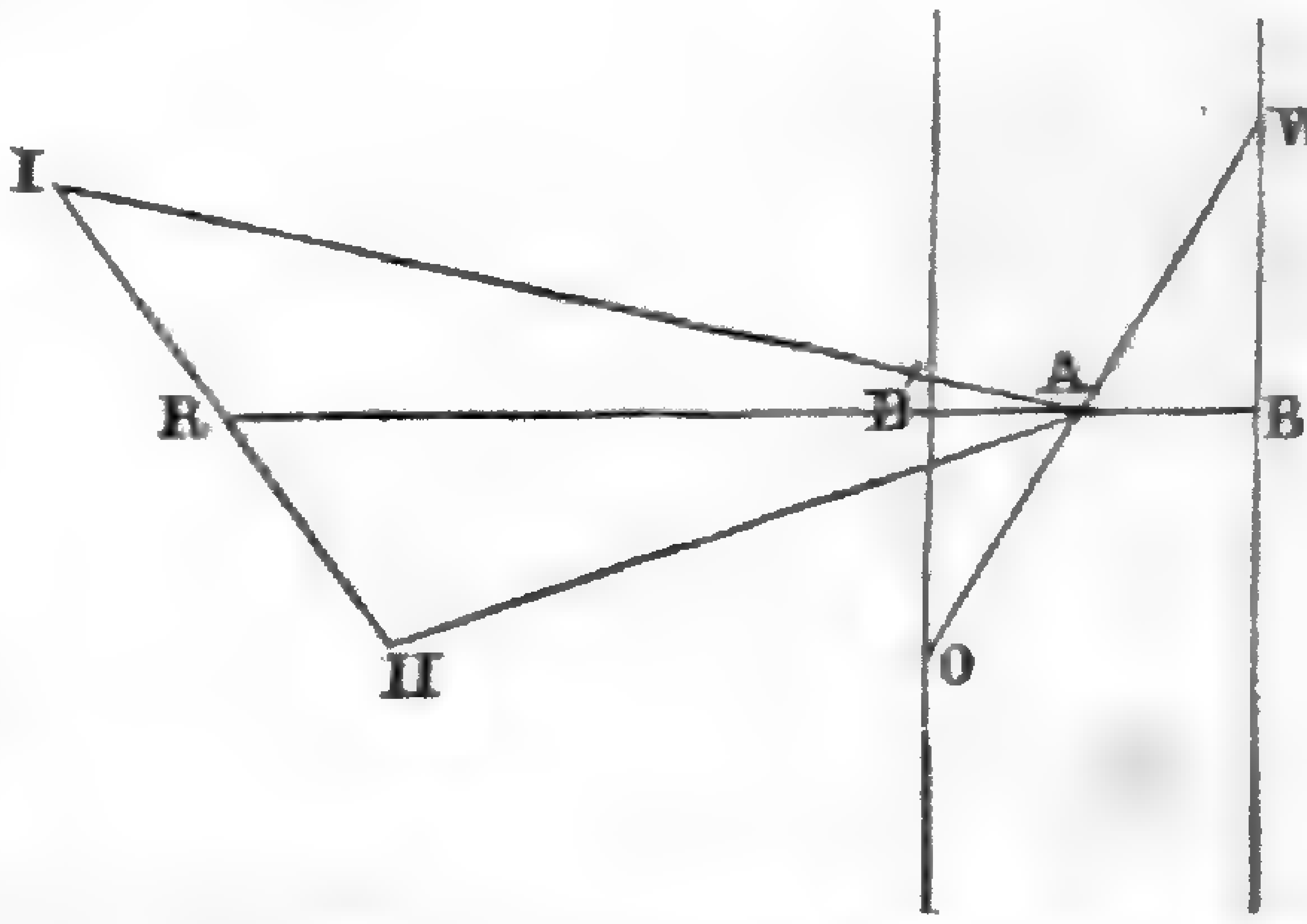
Is the Horopter a surface or is it a line? In either case what is its form and position? These questions have tasked the ingenuity of physicists, mathematicians and physiologists. If the position of identical points of the retinæ under all circumstances was known, then the question of the form of the Horopter would become a purely mathematical one. But the position of identical points evidently depends upon the laws of ocular motion. It is evident, therefore, that it is only on an experimental basis that a true theory of the Horopter can be constructed; and yet the experimental investigation as usually conducted is very unsatisfactory, on account of the indistinctness of vision when the object is at any considerable distance from the point of sight in any direction.

The most diverse views have, therefore, been held as to the nature and form of the Horopter. Aguilonius, the inventor of the name, believed it to be a plane passing through the point of sight and perpendicular to the median line of sight. Others have believed it to be the surface of a sphere passing through the point of sight and the optic centers; others, a torus formed by the revolution of a circle passing through the point of sight and the optic centers, on a line joining the optic centers. The subject has been investigated with great acuteness by P. Prévost, A. Prévost, J. Muller, G. Meissner, E. Claparède,* and lastly by Helmholtz.† A. Prévost determines in it, as he supposes, a circle passing through the optic centers and the point of sight, which he calls the "*horopteric circle,*" and a straight line passing through the point of sight at right angles to the visual plane, which he calls the "*horopteric vertical.*"

Until the investigations of Meissner almost all attempts to determine the form of the Horopter have been by mathematical calculations, based upon the doctrine of identical points and assuming the law of Listing. Meissner attempts the same question experimentally. We condense the following account of his admirable investigations from Claparède's memoir of this subject‡ already referred to:

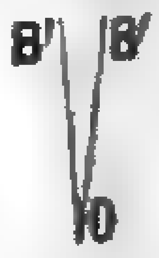
Let R (fig. 11) be an observer and I II his two eyes, A the point of sight, B an object beyond and B′ an object nearer than the point of sight, but all in the same line, joining the root of the nose and the point of sight. Of course, both B and B′ will be seen double. If, now, while the sight is still fixed upon A, B be elevated, its two images, according to Meissner, will approach, until at some point, W, they coalesce.

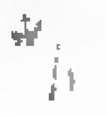
11.



If, on the contrary, B be depressed, its images separate more and more. If, now, B′ be elevated, its images separate, but if it be depressed, its images approach and coalesce at O. The line WAO is, therefore, the Horopter or line of single vision. It is not at right angles but inclined to the plane of vision. Again, according to Meissner, if instead of points we have vertical lines like threads WB and OB′ (fig. 11), then OB′ will

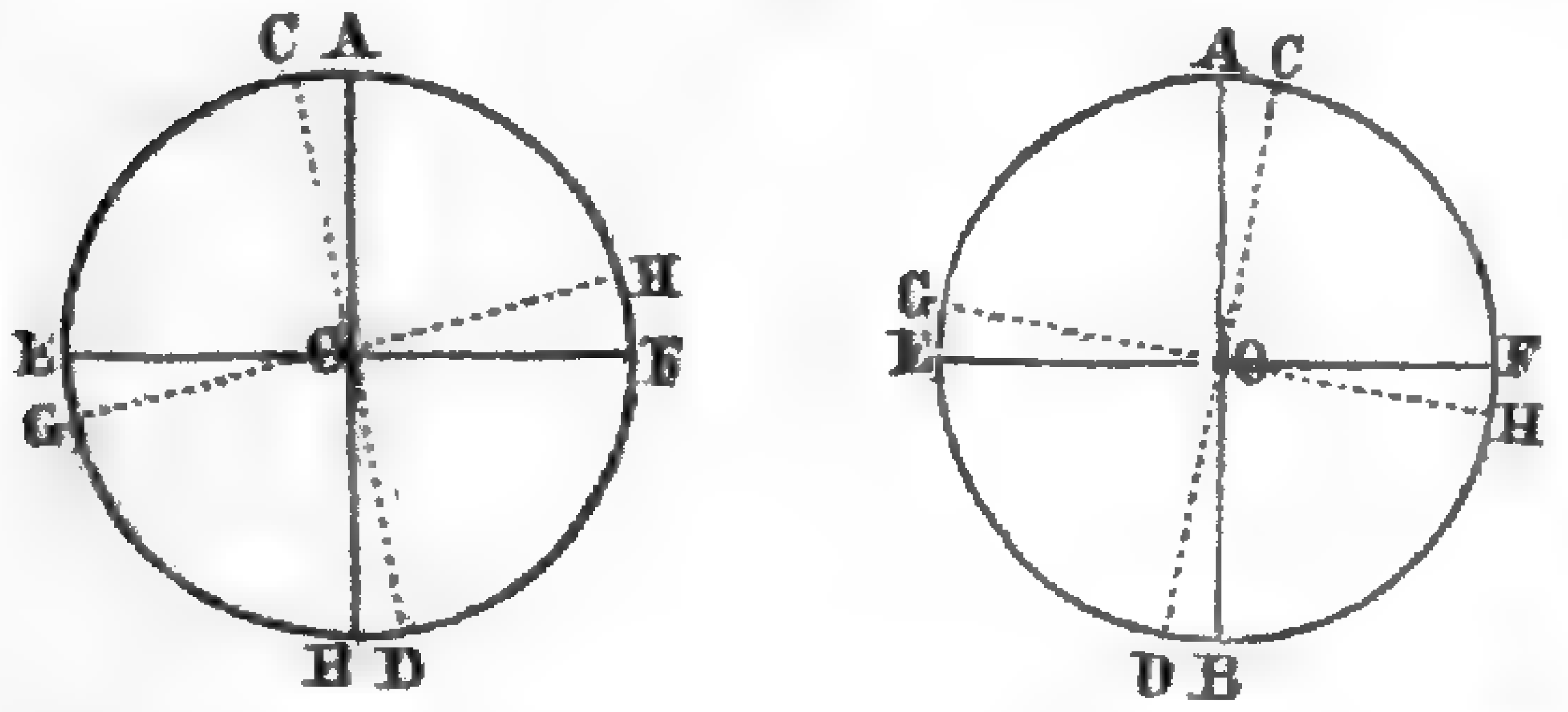
* Bib. Un. Archiv. des Scien., II, vol. iii, p. 138 and 225.
† Proc. Roy. Soc., Apr., 1864. ‡ Bib. Un. Arch. des Scien., vol. iii, p. 138.

double at B′, the images being crossed, and approach one another and meet at O, in other words, will appear thus: 

while BW will also double at B but not cross (i. e., each image will have the same name as the eye), and will be seen to converge and meet at W thus  . Lastly, if the vertical line pass through the point of sight A, the images will cross one another like an X.

Meissner accounts for these phenomena by supposing that, in converging the optic axis, the eyes rotate on the optic axis outward so that the vertical lines of demarkation CD no longer coincide perfectly with the vertical meridians AB (fig. 12,) as they do when the eyes are in the primary direction (the axis parallel) but cross them at a small angle. In the primary direction of the eye the image of a vertical line according to Meissner falls on the vertical line of demarkation CD in both eyes (for these lines then coincide with the vertical meridian) and is therefore seen single.

12.



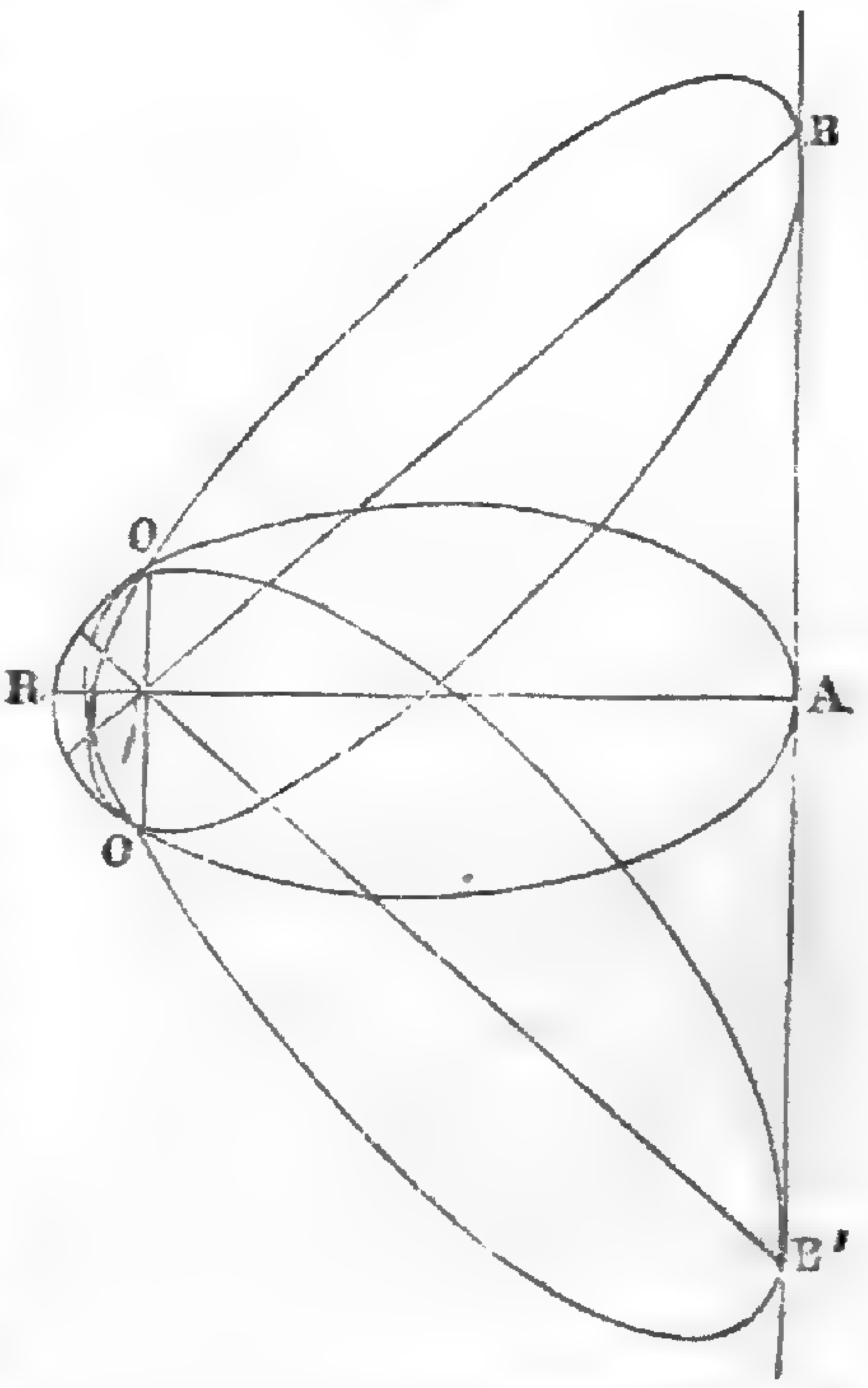
But if the eyes rotate on the optic axes outward then the image of a vertical line still falling on the vertical meridian must cross the line of demarkation in opposite directions in the two eyes and therefore cannot be seen single except at the point of sight, the image of which corresponds to the central point O of the retina of each eye. In order that the image of a line shall fall on the line of demarkation in both eyes and thus be seen single, it must be inclined at a certain angle with the vertical, the lower end being nearer and the upper end farther away. It is moreover evident, upon a little reflection, that when the eye rotates the Horopter cannot be a plane nor a surface of any kind, for objects right and left of the horopteric line must all be doubled by displacement of the horizontal line of demarkation GH (fig. 12,) which therefore no longer coincides with the horizontal meridian EF.

From various experiments made at different distances and with different degrees of inclination of the visual plane upward and downward, Meissner concludes: 1. That looking straight forward at an infinite distance the Horopter is a plane at right angles to the visual lines. 2. That for all other distances, the visual plane remaining the same, the Horopter is a straight line passing through the point of sight and increasing in inclination to the visual plane, as the convergence of the optic axes increases. 3. That in turning the visual plane downward, the inclination of the horopteric line with that plane becomes less and less, until at 45° downward it becomes perpendicular and therefore the Horopter again expands into a plane at right angles to the median line of sight. 4. That in raising the visual plane upward toward the eyebrows, the inclination of the Horopter to the visual plane increases.

We have given Meissner's investigations more in detail, because by entirely different methods we have confirmed almost all of them.

Claparède by similar experiments fails to confirm the conclusions of Meissner and therefore rejects them. He concludes, partly from his own experiments and partly from calculation, that "the Horopter is a surface of such a form that it contains a straight line perpendicular to the plane of vision and passing through the point of sight, and that every plane passing through the optic centers makes by intersection of this surface, the circumference of a circle." In other words, he believes that the horopter is a surface which contains the horopteric vertical BAB′ (fig. 13) and the horopteric circle OAO of Prévost, and that in addition the surface is further characterized by the fact that, while the point of sight remains at A, the intersection with it of every plane passing through the optic centers OO upward or downward as OBO and OB′O is also a circle. It is evident that as these circles would increase in size upward and downward the Horopter according to Claparède, must be a surface of singular and complex form.

13.



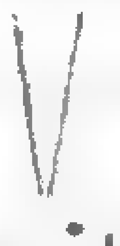
Finally, Helmholtz arrives at results entirely different from all previous observers. He sums up his conclusions as follows:

"When the point of convergence is situated in the middle [vertical] plane of the head, the Horopter is composed of a straight line drawn through the point of convergence [direction not stated but evidently not at right angles to the visual plane, for see below the sentence marked[a]] and a conic section passing through the optic centers and intersecting the straight line."

"When the point of convergence is in the plane which contains the primary visual lines [primary visual plane], the Horopter is a circle going through that point and the optic centers [Prévost's horopteric circle] and a straight line intersecting the circle [where and in what direction not stated]."

"When the point of convergence is situated as well in the middle plane of the head as in the primary visual plane, the Horopter is the circle just described [Prévost's horopteric circle] and a straight line going through that point [direction not stated]."

"There is but one case in which the Horopter is really a plane, viz: when the point of convergence is in the middle plane of the head and at an infinite distance. Then the Horopter is a plane *parallel* to the visual plane and *beneath* it, at a certain distance which depends upon the angle between the *really* and *apparently vertical* meridians, but which is nearly as great as the distance of the feet of the observer from his eyes when he is standing. Therefore, when we look at a point on the horizon, the *Horopter is the ground on which we stand.* When we look at the ground on which we stand at any point equally distant from both eyes, the Horopter is not a plane, but the straight line which is one of its parts *coincides completely with the horizontal plane on which we stand.*"

These conclusions of Helmholtz are the result of refined mathematical calculations *based entirely upon the supposed constant difference between the real and apparent vertical.* If this principle be true for all normal eyes, then it is probable that Helmholtz's conclusions in regard to the form and position of the Horopter are also true for those cases in which the point of sight is at considerable distance and in which, therefore, the rotation of the eye is very small. I am not able to test all of Prof. Helmholtz's conclusions by calculations based upon this principle, but I easily see that the position of the Horopter lying along the ground is the necessary consequence of a difference of $1\frac{1}{4}$ degrees between the real and apparent vertical when the eyes are in their primary direction. For if a line be drawn from each pupil downward, making an angle of $2\frac{1}{2}°$ with each other or of $1\frac{1}{4}°$ with the vertical, they would intersect each other at the distance of about five feet below the eyes or about the feet of the observer standing erect. Now if these two lines be placed thus  before the observer whose eyes are in the primary direction, it is plain that their stereoscopic combination would be a line lying along the ground to infinite distance. If the difference between the real and apparent vertical be less than $1\frac{1}{4}°$, then the distance below the eyes of the horopteric plane would be greater. We have already shown that if there be any such difference in our own eyes, it cannot be more than 10′; in this case the horopteric plane would be at least 35–40 feet below the eyes. But Prof. Helmholtz takes no account of rotation of the eyes on the optic axes, which greatly affects the form and position of the Horopter when the point of sight is near; and we believe that it is only when the point of sight is near, that the form and position of the Horopter is of any practical importance in vision, for it is only then that the doubling of images lying out of the Horopter is perceptible.

It has been with much hesitation that I have ventured to criticise the conclusions of so distinguished a physicist. My ability to do so, if well founded, I attribute entirely to a facility in the use of the eyes such as I have never seen equalled in the case of any other person.

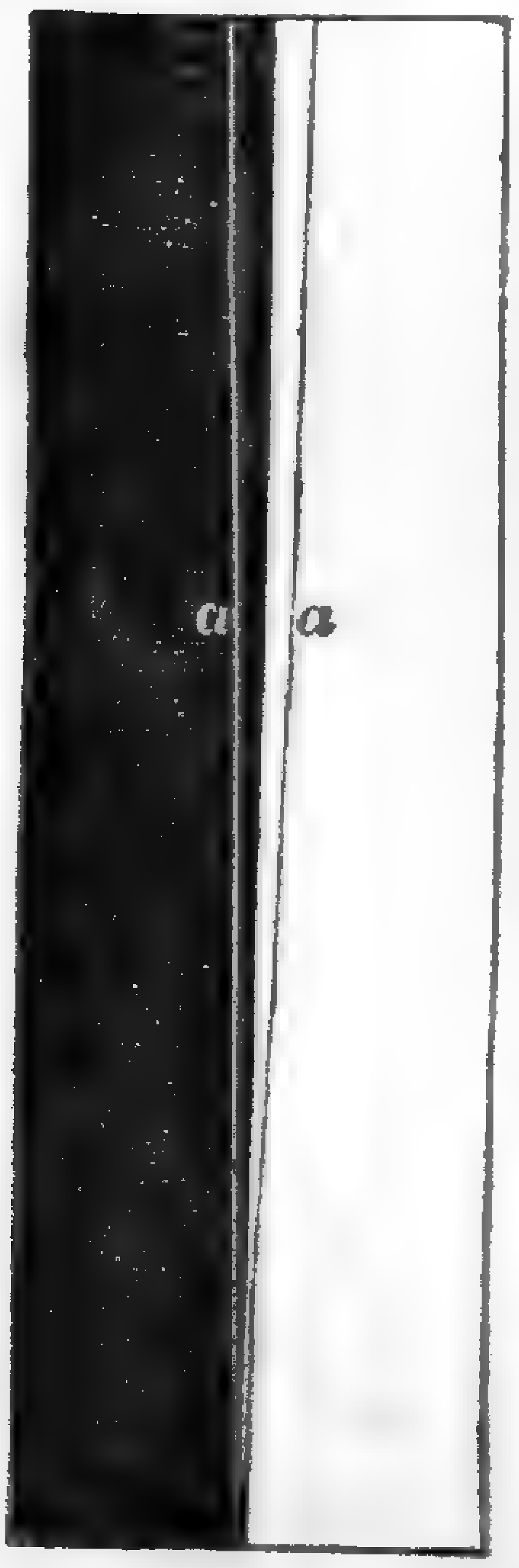
Although, I believe, Meissner has arrived at truer results than any one who has yet written on this subject; yet I think his method very unsatisfactory. I have wondered at the skill and patienee which could attain such true results by such imperfect methods. I have tried Meissner's experiments without any satisfactory results, and I confess I commenced these experiments with the conviction that his theory was untenable; but contrary to my expectation his views have been in a great measure confirmed. The difficulty with Meissner's method and in fact with all previous experimental methods, as already stated, is the indistinctness of objects at any considerable distance from the point of sight in any direction. In Meissner's experiment with the three points, B′, A and B (fig. 11), in lowering B′ or elevating B, the indistinctness was so great that I could not tell with certainty whether the images approached each other or not; and in his second experiment with the thread, the obstinate disposition on the part of the eye to see single by stereoscopic combination even when the images cross, interferes seriously with the certainty of the result. But in my experiments, by virtue of the complete dissociation of the axial and focal adjustments, the lines are seen perfectly clearly; and by making them pass each other slowly, their relation to each other may be observed with great exactness.

I will now state my own results in regard to the Horopter.

It is evident that if, in convergence, the eyes rotate on the optic axes as my experiments prove, then in this state of the eyes the Horopter cannot be a surface, but a line; and this line cannot be vertical but inclined to the visual plane. Perhaps this requires farther explanation. If the eyes in a state of convergence be fixed on a vertical line, then if the eyes rotate the line must be doubled except at the point of sight. This doubling is the result of *horizontal* displacement of the two images in opposite directions, and therefore the two images may be brought together by bringing the doubled portion of the vertical line nearer or carrying it farther away. This is done in inclining the line as in fig. 11. But all points to the right and left of the horopteric line are also doubled by rotation, but this doubling is the result of *vertical displacement* of the images; now vertical displacement cannot be remedied by increasing or decreasing the distance, because *the eyes are separated horizontally.* Therefore no form of surface can satisfy the conditions of single vision right and left of the horopteric line. The restriction of the Horopter to a straight line and the inclination of that line to the visual plane are therefore necessary results of rotation on the optic axes. But I have also proved this by direct experiment.

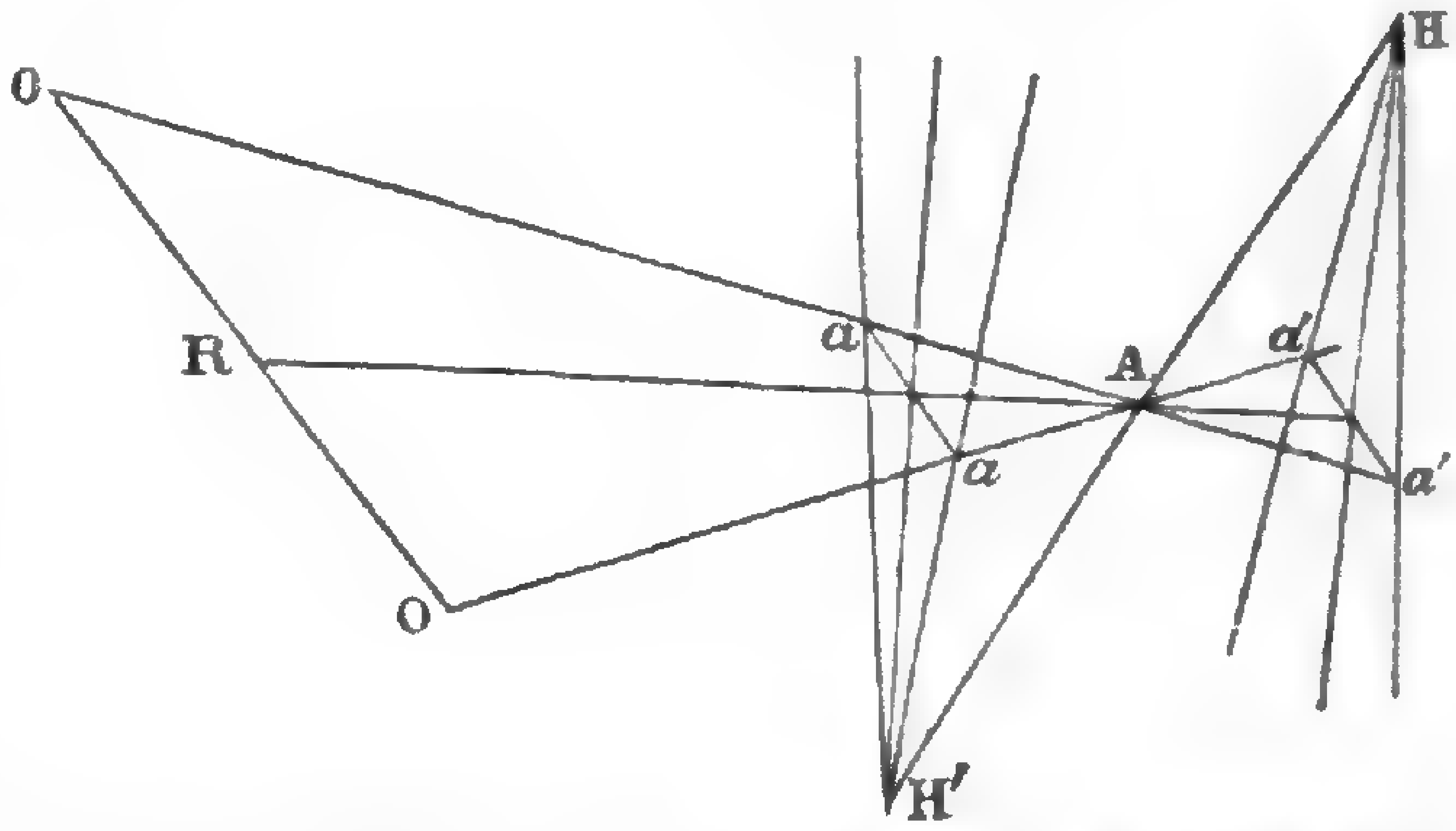
If two lines, one white on black and the other black on white (fig. 14) be drawn at an angle of $1\frac{1}{4}$ degrees with the vertical and therefore $2\frac{1}{2}$ degrees with each other, then by bringing my eyes so near to them at any point *aa* (taking care that the median line of sight shall be perpendicular to the plane of the lines) that the visual lines without crossing shall meet beyond the diagram at the distance of seven inches from the eyes, the two lines are brought in perfect coincidence. If on the contrary the same figure be turned upside down and the eyes be placed a little farther than seven inches, so that the two points *aa* are brought together by crossing the optic axes at the distance of seven inches, then also the lines are brought in perfect coincidence. The accompanying figure (fig. 15) in which OO are the eyes, A the point of sight, *a*H, *a*H and *a*′H′, *a*′H′ are the lines in the two positions, will explain how the stereoscopic combination takes place in each case. The line HAH is the Horopter. This experiment is difficult to perform satisfactorily. When the lines come together it is difficult to determine whether there is real coincidence or not. I have observed, however, that when the coincidence is not perfect the white and black lines seem to run spirally round each other. The best plan is to observe them at the moment of coming together or of separating. I feel quite confident of the reliability of the conclusions reached.

14.



I made many calculations based upon these experiments and on the previous experiments on rotation of the eye, to determine the inclination of the horopteric line for different degrees of convergence, i. e., for different distances of the point of sight. The results of these calculations were not entirely satisfactory. I had expected from Meissner's results that there would be found a progressive increase as the distance decreased. But I could not be sure from my calculations of any increase or decrease with distance. For all distances the inclination seemed to come out about seven degrees—in some a little less, in some a little more. Beyond three inches there seems to be a slight progressive increase rather than decrease; within three inches the action of the eyes was irregular.

15.



I then adopted another method. I used the diagram of parallel lines (fig. 6,) and inclined it at an angle of exactly 7° from the perpendicular in the supposed direction of the Horopter, and at the distance of 3 feet. In this position the verticals of course all converge by perspective. I then brought together successively the lines 3 inches apart, then those 6 inches apart, then those 9 inches, 12 inches, 15 inches, 18 inches and so on even to the last, which were 30 inches apart: *in each case the lines seemed to come together parallel* or at least the divergence, if any, was so small that I could not be sure about it. Now in this experiment the point of sight varied from 16½ inches to only 2·8 inches in distance, and yet the inclination of the horopteric line seemed to be nearly the same for all, viz: 7°. If there was any difference at all it seemed to be in favor of *greater inclination at greater distance.* This result which I arrived at, though doubtfully by experiment alone, would be the necessary result of any residual difference between the real and apparent vertical, or in other words, any residual inclination of the vertical upon the horizontal line of demarkation of the eye in its primary position, such as Helmholtz maintains and as I have supposed possible. Still it by no means proves the existence of this residual difference.

It must not be supposed, however, because the lines 3 inches, 6 inches, 9 inches, 12 inches, &c., apart, are all brought into coincidence at the same or nearly the same inclination, that therefore the amount of rotation of the eye is the same for all. The perspective convergence of the lines, of course increases with their distance apart, and therefore the rotation of the eye necessary to bring them successively into coincidence increases also. It is quite possible that the rotation should increase with the optic convergence, and yet the inclination of the horopteric line remain constant or even decrease with the convergence. Whether the inclination of the horopteric line increases or decreases with distance would depend upon the law of increase of rotation with increasing convergence. If it increases with distance then it is possible that when we look at the ground before us the Horopter may be a line lying along the ground, as maintained by Helmholtz.

I next tried the same experiments with the eyes inclined downward 45°. The *lines do not change at all their natural perspective convergence.* In all the experiments made with eyes in this position the inclination of the lines in the image was the same as in the object. I conclude therefore that in this position of the eyes the Horopter is at right-angles to the plane of vision, and since there is no rotation of the eye the Horopter in this position expands *into a surface.* Below this inclination *the Horopter again becomes a line but inclined now the other way, i. e., the upper end toward the observer.* In turning the eyes upward toward the eyebrows, I have found the rotation, except in cases of strong convergence, less than looking straight forward. I conclude therefore that in this position the horopteric line inclines less to the visual plane than it does when the visual plane is in its primary direction.*

The points in which my experiments do not confirm Meissner are—1, the increasing inclination of the horopteric line with increasing convergence. 2, the increasing rotation of the eye as well as inclination of the horopteric line under all circumstances in turning the eye upward. Again, I believe that Meissner is also wrong in supposing that the Horopter is a *plane* when the eyes are depressed 45°. In this position it is a *surface* but not a plane. It is clear that the images of points situated to the right and left of the point of sight and in the same plane with it cannot fall on corresponding points of the two retinæ. As to the form of this surface, I feel myself unequal to the task of its mathematical investigation, and its experimental investigation presents, I believe, insuperable difficulties.

We have seen that the eye in convergence rotates on the optic axis. The question naturally occurs, is this rotation to be regarded in the light of an imperfection of the instrument (of which there are several examples in the structure and mechanism of the eye,) and should the law of Listing be regarded as

* As stated in note on p. 165, eyes certainly differ in this respect. In my own, if convergence be small, the outward rotation decreases with the elevation of the visual plane, becomes zero, and even is converted into an *inward* rotation; the inclination of the Horopter, therefore, decreases, *becomes perpendicular and even inclines the other way.* In some other eyes the outward rotation increases whatever be the convergence; in this case, of course, the inclination of the Horopter increases as stated by Meissner.

the ideal of ocular motion, though an ideal seldom or never realized in nature; or does the rotation of the eye subserve some useful purpose in vision? I believe there is no doubt that the latter view is the correct one, for there seems to be special muscles which are adapted for this rotation and the action of these muscles is consensual with the adjustments of the eye and the contraction of the pupil. This purpose I explain as follows. A general view of objects in an extended field is absolutely necessary to animal life in its highest phases, but an equal distinctness of all objects in this field would only distract the attention. Therefore the eye is so constructed and moved, as to restrict as much as possible both *distinct* vision and *single* vision. Thus as in *monocular vision* the more elaborate structure of the *central spot* of the retina restricts distinct vision to the visual line, and the *focal adjustment* still farther restricts it to a single point in that line; so also in *binocular* vision, *axial adjustment* restricts single vision to the Horopter, while rotation restricts the Horopter to a single line.

Conclusions.

The most important conclusions arrived at in this paper may be briefly summed up as follows.

1. The axial and focal adjustments of the eye are not so inseparably associated as is generally supposed; but on the contrary when distinctness of vision requires it they may be completely dissociated.*

2. In this dissociation, the contraction of the pupil associates itself with the focal in preference to the axial adjustment.

3. In optic convergence there is a rotation of both eyes on optic axes *outward*; and this rotation increases with the degree of convergence.

4. In inclining the visual plane downward the rotation of the eyes for the same degree of convergence decreases, until when the visual plane is inclined 45° downward the rotation becomes zero for all degrees of convergence. Below the inclination of 45° the rotation is *inward*. In turning the eyes upward, except in cases of strong convergence, the rotation also decreases slightly but does not reach zero;† in strong convergence it increases as stated by Meissner.

5. Besides the rotation produced by optic convergence there is also a decided inclination of the vertical line of demarkation upon the horizontal line of demarkation, which increases with the degree of convergence. This change in the relation of these two lines is probably the result of distortion of the ocular globe.

* While these pages were passing through the press, I discovered that in this conclusion I had been anticipated by Donders and others. All previous experiments, however, were made by means of glasses. Mine were made with the naked eye.

† See this statement modified in note on p. 165.

6. As a necessary consequence of the rotation of the eyes, for all degrees of convergence in the primary visual plane, the Horopter is a *line* inclined to the visual plane, the lower end nearer the observer ; but whether the inclination increases or decreases with distance I have not been able to determine with certainty. It probably increases with distance.

7. In inclining the visual plane below the primary position, the inclination of the horopteric line becomes less and less until when the visual plane is lowered 45° the horopteric line becomes perpendicular to that plane and at the same time expands into a surface. Below 45° the Horopter again becomes a line, but now inclined in the contrary direction, i. e., the *upper* end nearer the observer.

8. In inclining the visual plane upward or toward the brows, if the optic convergence be strong, the inclination of the horopteric line increases, but if the optic convergence be small it decreases but does not reach zero or become perpendicular.*

9. In looking downward 45° for all distances the Horopter is a *surface* passing through the point of sight and perpendicular to the median line of sight, but the form of the surface I have not attempted to determine. In looking straight forward at infinite distance the Horopter is also a *surface* passing through the point of sight, but the inclination of this surface I am unable to determine.

10. It is possible that in some eyes which would be considered normal there is, in convergence, a rotation of the eyes *inward*, probably from greater power in the *superior* oblique. In such cases the position of the Horopter would be different.

Columbia, S. C., Nov. 16, 1868.

ART. XII.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. 6.—*On a new Salt containing Tin, Cæsium and Chlorine ;* by S. P. SHARPLES, S.B., Assistant in the department of Chemistry.

SOME time during the month of November, 1868, Dr. Gibbs called my attention to the fact that when a solution of the chlorids of tin was added to one containing the chlorids of sodium, potassium, lithium, cæsium and rubidium, together with free chlorhydric acid, a heavy white crystalline precipitate was formed. Upon examination I found that this substance consisted almost entirely of a salt containing cæsium ; this salt was so pure even from one precipitation that it gave the violet flame characteristic of the metal, and upon examination with the

* See this statement modified in note on p. 176.

spectroscope showed only faint traces of the other metals of the alkaline group.

The crude salt was dissolved in a large quantity of water to which some chlorhydric acid was added and the solution evaporated until crystals began to form on the surface of the liquid. It was then allowed to cool and the resulting crystals filtered off, washed with chlorhydric acid and dried. They were then found to be entirely free from even traces of the other alkalies. Two separate portions of the crystals were weighed out and treated with dilute nitric acid, which on boiling precipitated the tin completely as SnO_2; this was filtered off and the chlorine determined as chlorid of silver. In the first analysis ·4105 gram of the salt gave ·1004 grams of SnO_2 and ·5844 grams of AgCl. In the second, ·4767 grams gave ·1169 grams of SnO_2 and ·6727 grams of AgCl or in per cent—

	I.	II.	Calculated.
Cl	35·22	34·91	35·84
Sn	18·12	18·18	19·49
Cs by dif.	46·66	46·91	44·67

This it will be seen corresponds very nearly to the formula $SnCs_2Cl_6$, so that the salt is analogous to the well known platinum salt $PtCs_2Cl_6$. Like this it crystallizes in the regular system.

In order to ascertain whether the salt might be made use of for detecting cæsium in minerals, three or four grams of Hebron lepidolite were fused with about their own weight of carbonate of soda and treated with HCl to remove the silica. To the strongly acid liquid a solution containing stannic chlorid was added; a slight turbidity was at once produced; upon standing a few hours a white precipitate settled to the bottom. The supernatant, liquid was then poured off, the precipitate washed with HCl and examined before the spectroscope; it was found to consist almost entirely of the cæsium salt.

The new salt will not serve for a quantitative separation of cæsium as it is slightly soluble even in strong chlorhydric acid. If the stannic chlorid is added to a neutral solution containing CsCl no precipitate is formed, but upon adding to the solution about its own volume of strong chlorhydric acid the salt is at once thrown down. Alcohol does not seem to have any influence upon the precipitation.

The double salts of the other alkaline metals with tin seem to be perfectly soluble in chlorhydric acid, rubidium perhaps the least so, but even this is not insoluble enough to interfere with the complete separation of the cæsium. As a method of preparing the salts of cæsium in a state of purity the precipitation of the metal in the form of chloro-stannate appears to present great advantages.

Cambridge, Dec. 31st, 1868.

Art. XIII.—*Upon the Atomic Volumes of Liquids;* by Frank Wigglesworth Clarke, S.B.

If we divide the weight of a given bulk of any liquid at 0° by the weight of an equal bulk of its vapor at the same temperature, it is plain that the quotient will represent the number of volumes of vapor formed by one volume of the liquid, supposing both to exist at the above named temperature.

These quotients I term the *vapor volumes* of liquids. It is true that but few liquids can form vapor at 0°, and therefore these vapor volumes are fictitious quantities: yet, nevertheless, real or imaginary, exceedingly interesting results may be obtained by comparing them.

Of the relations existing between the vapor volumes of different liquids, I shall have but little to say in this paper, except in so far as they have been instrumental in determining atomic volumes. However, I will state as briefly as possible the leading results I have obtained by their comparison, but, as I am still at work upon the subject, I shall not enter into details.

In any homologous series of liquid compounds, the first member of the series possesses a higher vapor volume than the second, the second higher than the third, and so on indefinitely. I have yet found no exception to this rule, although I have calculated the vapor volumes of more than 400 different liquids. Furthermore, the difference between the vapor volumes of the first and second members of a series is greater than the difference between those of the second and third, and this again, greater than the difference between the third and fourth, and so on. The only exceptions I have found to this rule lie among compounds which stand so high in their respective series that the differences between their vapor volumes are very small, and consequently a very slight error in determining the specific gravities of the liquids is enough to account for the trifling variations from the rule. Even these exceptions are rare.

The first of these rules may be carried still farther. If we calculate the number of volumes of vapor actually formed at the boiling point by one volume of liquid at 0°, we shall find that the first member of a series forms more vapor than the second, the second more than the third, and so on as far as I have had data from which to calculate.

Hence it seems that the usual increase in the boiling points as we ascend in a series, is not sufficient to counterbalance the decrease in the vapor volumes. Since, however, the differences between the vapor volumes are constantly diminishing as we ascend in any series, it seems almost certain that there must be a point at which the increase in the boiling points will overcome the decrease in the vapor volumes, and therefore the amounts of vapor formed by the members of the series at their boiling points must begin to increase with every step upward. I have as yet found no such point, however, in any series upon which I have calculated.

In any series of substitution compounds, as a rule, the vapor volumes diminish as the atomic weights increase.

Chlorid of arsenic has a lower vapor volume than the fluorid. Bromids have lower vapor volumes than the corresponding chlorids, and iodids still lower than bromids. Sulphids, also, have lower vapor volumes than the corresponding oxyds.

Last of all I give the rule upon which most of my work upon atomic volumes is based. Whenever two compounds have equal vapor volumes, their atomic volumes also are equal, or nearly so, and, as a general rule, the greater the vapor volume the less the atomic volume, and *vice versa.* There is not an actual inverse ratio between the vapor volumes of liquids and their atomic volumes although a casual glance at the numbers would seem to suggest one. It must be remembered that the vapor volumes are calculated from the specific gravities of liquids at 0°, while the atomic volumes are referred to the boiling points, and therefore an exact inverse ratio would be very improbable.

Although as yet I have found no relations between the vapor volumes of liquids which would enable me to calculate their specific gravities at 0° from their composition, still the relations which I have found seem to me to indicate decidedly the existence of definite relations between the atomic volumes of liquids, their boiling points, and their rates of expansion.

We now come to the subject named at the head of this paper,—the atomic volumes of liquids. In 1855 Kopp published several articles upon this subject.* In them he described a method of calculating the atomic volumes of liquid compounds, showing that the atomic volume of any compound equalled the sum of the atomic volumes of the elements composing it, just as its atomic weight equalled the sum of the atomic weights of its constituent parts. For a large number of compounds he actually determined the atomic volumes, and thence deduced those of the elements contained in them; but, as he employed the old atomic weights, the numbers he gave for oxygen, sulphur, and carbon, have since been doubled. With this correction the list stands as follows. Hydrogen 5·5, chlorine

* "Annalen der Chemie und Pharmacie," xcvi, 153 and 303, xcvii, 374, xcviii, 367.

22·8, bromine 27·8, iodine 37·5. Oxygen, in the radical 12·2, out of the radical 7·8. Sulphur, out of the radical 22·6, in the radical 28·6. Nitrogen, in compounds of the ammonia type 2·3, in hyponitric acid 8·6, and in cyanogen 17. Carbon 11. Besides these he also determined experimentally the atomic volumes of the following compounds, some of which are amended to suit the new atomic weights.

PCl_3 93·9, PBr_3 108·6, $AsCl_3$ 94·8, $SbCl_3$ 100·7, $SbBr_3$ 116·8, $SiCl_4$ 122·1, $SiBr_4$ 144·2, $SnCl_4$ 131·4, $TiCl_4$ 126·0.

Kopp suggested at the time, that probably phosphorus and arsenic had equal atomic volumes, and added to them silicon also, whose chlorid, regarded as $SiCl_3$, gave the number 91·6. Silicon now, however, stands apart. Tin and titanium also he regarded as having equal atomic volumes, but changing the atomic weight of silicon has brought that element near these two, so that the atomic volume of titanium agrees better with that of silicon than with that of tin.

As I shall show hereafter, tin stands by itself, having an atomic volume different from either silicon or titanium. Moreover, the fact that the atomic weights of silicon and titanium are nearer together than those of titanium and tin, goes to show that if titanium be classed with either, it should be with silicon.

As I previously stated, whenever two liquids have equal vapor volumes their atomic volumes also are equal, or nearly so. For instance, the various isomeric ethers formed by the homologues of formic acid with the methyl series of hydrocarbons, (ethyl acetate, formate, &c.,) have both equal atomic volumes and vapor volumes, and so on with all strictly isomeric bodies. Again, to cite an example of liquids diverse in their natures, benzol, butyronitrile, and bromid of ethylene, all have the vapor volume 257. Their atomic volumes, calculated by Kopp's method, are respectively 99, 99·5, and 99·6. I could cite many other examples, but it is not necessary. There are exceptions, however, though they are not common.

Suppose now we wish to determine the atomic volume of any element in its liquid compounds,—boron, for example. The terbromid of boron has the vapor volume 239. Acetic anhydrid has the vapor volume 238. Therefore the atomic volumes of these two compounds must be nearly equal. The atomic volume of the anhydrid is 109·2, calculated by Kopp's process. Regarding this as also the atomic volume of the bromid, and subtracting from it that of the three atoms of bromine, we have left as the atomic volume of boron, the number 25·8. Triethyl boron, $C_6H_{15}B$, has the vapor volume 157, equal to that of œnanthic acid. The calculated atomic volume of the acid is 174. This, then, is also the atomic volume of triethyl boron, and, subtracting from it the atomic volume of C_6H_{15}, we have left for that of boron the number 25·5, closely agreeing with the result obtained from the bromid. But we do not get such close agreements in all cases, and therefore in order to obtain accurate results, we must compare the numbers obtained from several of the liquid compounds of the element in question, and regard the average of them all as nearest correct. Before going farther in this direction, however, let us compare the vapor volumes of a number of similar compounds of boron, phosphorus, and arsenic.

The chlorids of these three elements have respectively the vapor volumes 257, 262, and 252. The bromids of boron and phosphorus have respectively 239 and 241. Triethyl boron, triethyl phosphine, and triethyl arsine have the vapor volumes 157, 154, and 159, and triethyl phosphate and triethyl arsenate have 132 and 130.

Kopp, from the chlorids, found the atomic volumes of phosphorus and arsenic to be probably equal. The comparison of these vapor volumes confirms this view, and adds boron as also possessing the same atomic volume as phosphorus and arsenic.

To make this still more certain I have calculated the atomic volumes of these elements from the vapor volumes of their compounds, in the manner already described.

For boron I have made calculations from eleven compounds, —the chlorid, bromid, triethyl boron, trimethyl, triethyl, triamyl, and monamyl borates, ethyl diamyl, amyl diethyl, and methyl diethyl borates, and tetraphenyl diborate. In these compounds I obtained respectively as the atomic volume of boron in its liquid compounds, the numbers 30·9, 25·8, 25·5, 26·0, 24·1, 26·1, 19·7, 25·3, 31·9, 24·1, and 19·9. The average is 25·4.

Although there are very great variations between these different numbers, it will be seen hereafter that the averages obtained by this method agree closely with the numbers found by actual experiment.

The atomic volume of phosphorus I have calculated in a similar manner in eleven of its liquid compounds, exclusive of the chlorid and bromid. I include Kopp's numbers for these last, however, for the sake of completeness in making up the average.

The list of compounds then stands as follows. The chlorid, bromid, oxychlorid, oxybromid and oxybromochlorid of phosphorus, triethyl phosphine, triethyl phosphite, diamyl phosphoric acid, triethyl phosphate, tetrethyl pyrophosphate, and the ethyl, butyl, and amyl chlorophosphites. In these, giving

Kopp's numbers for the first two, the atomic volume of phosphorus is found respectively as follows: 25·5, 25·2, 26·2, 25·8, 22·4, 25·5, 26·1, 23·2, 29·3, 32·3, 27·1, 27·1, and 19·7. The average of these, 25·8, agrees very closely with Kopp's numbers, and varies only 0·4 from the average obtained for boron.

The atomic volume of arsenic I have deduced from the vapor volumes of three of its compounds exclusive of the chlorid. In the chlorid the element has the atomic volume 26·4, (Kopp) and in triethyl arsine, triethyl arsenite, and triethyl arsenate, I obtain the numbers 25·5, 26·7, and 29·2. The average of all four numbers is 26·9.

If now, regarding boron, phosphorus, and arsenic, as possessing the same atomic volume in their liquid compounds, we take the average of the numbers obtained from the twenty-eight compounds in which that atomic volume has been determined, we get the number 25·8. To these three elements we can probably add vanadium, which Roscoe has shown belongs in the same group. There is but one liquid compound of this metal for which I had data to calculate from,—the oxychlorid, Roscoe's "vanadyl trichlorid," $VOCl_3$. Calculating its vapor volume, and thence the atomic volume of vanadium, I obtained the number 27.4.

It will be seen that a number of compounds of boron, phosphorus, and arsenic gave higher results than this, and therefore, for a single compound, this number seems close enough to that found for the other three elements, to be classed with them. This view is somewhat strengthened by the fact that the atomic weight of vanadium is intermediate between those of phosphorus and arsenic.

For antimony, as already stated, Kopp determined the atomic volumes of the chlorid and bromid. Deducing the atomic volume of antimony from these, we get the numbers 32·3 and 33·4. In addition to these I have determined from their vapor volumes the atomic volumes of triethyl and triamyl stibine, and the chlorid and bromid of triethyl stibine. From these I obtain respectively as the atomic volume of antimony, the numbers 33·3, 32·1, 32·9, and 35·9. Adding in these with the numbers from the chlorid and bromid of the metal, we get 33·3 as the average.

In the case of bismuth there is but one liquid compound for which I had the necessary data. Triethyl bismuthine has the vapor volume 138, while triethyl stibine has the number 141. These are so near together that it seems probable that bismuth in its liquid compounds has the same atomic volume as antimony. But more data are needed to decide this point definitely.

In the case of silicon, thanks to the labors of Friedel, Crafts, and Ladenburg, materials were more abundant. Apart from the chlorid and bromid, whose atomic volumes were determined by Kopp, I have calculated the vapor volumes of sixteen liquid compounds of silicon, and thence the atomic volume of silicon itself. These compounds are tetramethyl, tetrethyl, and tetra-amyl silicates, diethyl, diethyl dimethyl, triethyl methyl, triamyl ethyl, diamyl diethyl, and trimethyl ethyl silicates, hexmethyl and hexethyl disilicates, ethylsilicic monochlorhydrin, dichlorhydrin, and trichlorhydrin and methylsilicic monochlorhydrin and dichlorhydrin. In these compounds I obtain respectively as the atomic volume of silicon, the numbers 31·0, 32·8, 33·0 30·3, 32·8, 32·2, 38·8, 35·2, 30·9, 30·3, 32·8, 36·3, 32·0, 39·0, 29·9, and 34·6. Taking also the numbers given by Kopp for the chlorid and bromid of silicon, amending them to suit the new notation and new atomic weight of silicon, and thence deducing the atomic volume desired, we get the numbers 33·0 and 30·9. Taking the average of these eighteen numbers we obtain 33·1 as the atomic volume of silicon in its liquid compounds. The atomic volume of titanium, as deduced from that of the chlorid, as determined by Kopp, is 34·8. Further investigation will probably show its atomic volume to be equal with that of silicon.

The atomic volume of the chlorid of tin, as determined by Kopp, and since doubled to suit the new notation, is 131·4. This gave for tin the number 40·2. The vapor volume of the same compound gave as the atomic volume of tin, the number 40·1. I have also calculated the vapor volume of the following nine compounds containing this metal. Stanntetrethyl, stanndimethyl diethyl, stanndiethyl, stannethyl trimethyl, the chlorid, bromid, and iodid of stanntriethyl, and the iodids of stanntrimethyl and stanndimethyl. From the vapor volumes of these liquids I have obtained respectively as the atomic volume of tin the numbers 46·5, 42·0, 39·3, 44·0, 37·7, 42·1, 41·0, 41·8, and 44·0. Including the number deduced from the chlorid of tin, we get as the average 41·8, the atomic volume of tin in its liquid compounds.

In the case of zinc there were but three liquid compounds for which I was able to calculate the vapor volumes. These were zinc ethyl, zinc methyl, and zinc amyl. The atomic volume of zinc, deduced from their vapor volumes, I obtained respectively as 23·2, 24·2, and 23·2. The average is 23·6.

Of liquids containing selenium I have the vapor volumes of but two,—the oxychlorid, $SeOCl_2$, and monohydrated selenic acid. The latter of these, however, has never been obtained free from an excess of water, the strongest containing only about 97 per cent of monohydrate; and therefore its atomic volume, as deduced from its vapor volume, is undoubtedly a trifle too low. Be that as it may, however, in these two compounds I obtained as the atomic volume of selenium the numbers 24·7 and 21·8, the mean being 23·2. This is only 0·6 greater than the number given by Kopp as the lower atomic volume of sulphur, and, therefore, taking into account that sulphur and selenium in the solid condition have equal atomic volumes, it seems almost certain that the same equality holds true in their liquid compounds.

I determined the vapor volumes of two lead compounds, lead tetrethyl and lead triethyl, but, to my great surprise I obtained the same number for both. This is so anomalous that I am inclined to think either that there is an error in the numbers published as the specific gravities of these liquids, or else that their vapor densities do not follow the usual law. At all events I could get nothing reliable from them.

From the vapor volume of chlorochromic acid I determined its atomic volume, and thence that of chromium in liquid compounds, as 43·6. The vapor volume of the fluorid of arsenic gave me the means of ascertaining the atomic volume of fluorine, for which I obtained the number 10. But as each of these was determined from only one compound of the element, I place no great reliance upon either, regarding them merely as possible approximations to the truth.

In order to test more thoroughly this process of determining atomic volumes by means of vapor volumes, not being content with the coincidence of my numbers with those of Kopp in the cases of phosphorus, arsenic, antimony, silicon and tin, I calculated the atomic volumes of chlorine, bromine, and iodine by the same method. For chlorine, in an average of forty-one compounds, I obtained the number 22·9, Kopp's determination giving 22·8. Bromine, in an average of fourteen compounds, gave me 28·0, Kopp's number being 27·8; and iodine, calculated from nine of its liquid compounds gave the number 38·5, that of Kopp being 37·5. To this determination of the atomic volume of iodine I shall refer again hereafter.

In regard to phosphorus, arsenic, antimony, silicon, and tin, my determinations might be regarded by some as labor lost, after Kopp's examination of the chlorids and bromids. But, very frequently, the atomic volume of a single compound as calculated by Kopp's method, varies considerably from that actually found. Therefore, in order to determine accurately the atomic volume of any element in its liquid compounds it is necessary to ascertain the atomic volumes of a number of those compounds.

The examination of one, or even two or three compounds of any element may give a very different result from that which would be obtained from a larger number of determinations. For instance, Kopp's earlier experiments upon a small number of liquids gave him the number 4·7 as the atomic volume of carbon (C=6), but, upon examining a larger number of compounds he obtained the number 5·5. Doubling these to agree with the modern atomic weight of carbon, and we have a difference of 1·6. Therefore my results in the cases of the elements above named are useful as confirmatory of Kopp's. My determinations for boron, bismuth, vanadium, selenium, and zinc, however, are entirely new.

The atomic volume of mercury at its boiling point I have calculated directly from the metal itself, by the data furnished by Regnault. According to this chemist, the specific gravity of mercury at 0° is 13·5959, its boiling point is 350°, and one volume of the metal at 0° becomes at 350°, 1·065743 volumes. From these data I find the specific gravity of mercury at its boiling point to be 12·7572, and dividing the atomic weight by this number I get 15·68 as the atomic volume.

It will be seen here that I assume that mercury when free has the same atomic volume as when combined. This appears true for bromine, ammonia, cyanogen, and hyponitric acid, according to Kopp's determinations, and therefore it seems allowable to regard mercury as following the same rule. Possibly this metal may have more than one atomic volume, like oxygen, nitrogen, or sulphur; in that case 15·68 is probably either one of them or their mean. If all elements possessing but one atomic volume in their liquid compounds follow after bromine, then we can calculate approximately their specific gravities at their boiling points. That of hydrogen would be 0·1818, and that of carbon 1·0909. This, however, is a mere matter of curiosity at present.

Having now the atomic volumes of twenty elements in their liquid compounds at their boiling points, we may proceed to compare the numbers, and see if there are any definite relations between them. Of the relations found between the atomic volumes of compounds by Schröder and Kopp I shall have nothing to say, since more important relations between the elements seem to exist.

Hydrogen, having in liquids the atomic volume 5·5, is most readily substituted, atom for atom, by chlorine, bromine, iodine, and hyponitric acid. These have respectively, according to Kopp, the atomic volumes 22·8, 27·8 37·5, and 33. The last of these is an exact multiple by a whole number of that of hydrogen, and the second varies but 0·3, the first by

0·8, and the third by 1·0 from multiples of the same. Now since Kopp does not claim rigid accuracy for his numbers, perhaps we may be justified in altering these to multiples of 5·5. Then for chlorine, bromine, and iodine we shall have the atomic volumes 22, 27·5, and 38·5. The last of these, it will be seen, is the same number which I deduced from the vapor volumes of nine compounds of iodine. The lower number was deduced from only three compounds,—the iodids of methyl, ethyl, and amyl, whose atomic volumes were determined by actual experiment. The atomic volume of iodid of methyl, according to Kopp, is from 65·4 to 68·3, that of iodid of ethyl from 85·9 to 86·4, and that of the amyl compound from 152·5 to 158·8. If now we calculate the atomic volumes of these three compounds, we shall find that if we regard iodine as having the atomic volume 37·5, we shall get a better agreement with the numbers found for the second of these compounds than if we ascribe to this element the value 38·5; but in the first and third cases, although the lower value for iodine agrees best with the lower of the two numbers between which the atomic volume of each compound varies, the value 38·5 agrees more nearly with them than between those numbers. In other words, we shall get a closer agreement with the results of experiment, if we ascribe to iodine the atomic volume 38·5, than if we give it the lower number obtained by Kopp.

The atomic volumes of seventeen organic liquids containing chlorine, have been determined by Kopp. When two values are given for one liquid I take the mean between them, and then I find upon calculating their atomic volumes, that the number 22 for chlorine agrees best with the determinations in seven cases, while Kopp's number affords a closer agreement with the numbers found for the remaining ten.

For bromine, out of five compounds whose atomic volumes have been actually determined, Kopp's number coincides best with the results found for four, while the altered number is nearer to the value obtained for the remaining one. Yet the alteration of Kopp's numbers is least in the case of bromine. Let us now examine some other groups of elements in a similar manner.

Since hyponitric acid, replacing as it does, hydrogen atom for atom, possesses an atomic volume an exact multiple of that of hydrogen, let us take this compound as our starting point for the nitrogen group. According to Kopp, nitrogen has three atomic volumes; in ammonia 2·3, in hyponitric acid 8·6, and in cyanogen, 17·0.

If now we alter the first to 2·15, and the third to 17·2, leaving the second (our starting point) as it is, then the second becomes exactly four times the first, and the third just twice the second. In other words, the higher atomic volumes of this element become multiples by whole numbers of the lower. This coincidence seems too remarkable to be accidental. For boron, phosphorus, vanadium, and arsenic, the number 25·8 was found as the atomic volume. This is exactly three times 8·6, our starting point for this group. The number obtained for antimony, 33·3, it will be seen lacks 1·1 of a multiple of 8·6. But of the six compounds of antimony from which I calculated, two contain chlorine, and two bromine.

In my calculation I employed Kopp's values for those elements. If, however, the altered numbers are the true atomic volumes of chlorine and bromine, then we must re-calculate the atomic volume of antimony. Doing this, using the altered values for chlorine and bromine, we obtain as the atomic volume of antimony, the number 34·2. If we take the atomic volumes actually found for the chlorid and bromid of antimony, and from them determine the value of antimony, using the new numbers for chlorine and bromine, we obtain a mean of 34·5. 34·4 is just four times 8·6 ! This seems to lend additional strength to the idea that the chlorine group of elements have atomic volumes which are multiples by whole numbers of that of hydrogen.

If, however, we re-calculate the atomic volume of antimony on the basis of new values for chlorine and bromine (iodine also, whenever necessary), we must do the same for boron and the elements classed with it. Doing this, we obtain the number 26·4, a variation of only 0·6 from the multiple of 8 6 previously found.

In making these corrections it must be borne in mind that whenever the atomic volume of an element is deduced from the vapor volume of a compound, if that vapor volume is compared with that of a compound containing either chlorine, bromine, or iodine, then the atomic volume of the latter must itself be corrected, before deducing from it that of the element in question.

Passing now to the oxygen group, we have two atomic volumes given by Kopp for oxygen, 7·8 and 12·2. Between these two I have yet found no relation. But mercury, as calculated from the metal, has the atomic volume 15·68. 15·6 is just twice 7·8. Zinc, having according to my calculation the atomic volume 23·6, exceeds by only 0·2 a multiple of 7·8 by 3. For sulphur, Kopp gave the numbers 22·6 and 28·6. Between these two I have found no relation, but the lower one varies only 0·8 from three times 7·8. For selenium I obtained the number 23·2, only 0·2 less than the same multiple of 7·8. Hence it seems very probable that the true lower atomic volume for sulphur and selenium is 23·4.

I have found no relation, however, between the higher atomic volumes of oxygen and sulphur.

One more group remains, that of tetratomic elements. Of these, carbon, as determined by Kopp, has the atomic volume 11. For silicon I obtained the number 33·1, almost exactly three times 11. Altering this, as in the case of antimony, to agree with the altered atomic volumes for chlorine, bromine, and iodine, we get 33·7, still near enough to 33 to be regarded as following the usual rule.

In the case of tin we meet the first and only obstacle to this rule in the list of elements whose atomic volumes have been determined. For this metal I obtained by means of the vapor volumes of ten of its compounds, the atomic volume 41·8. Corrected for chlorine, bromine, and iodine, it stands 41·5, the nearest multiple of 11 to this being 44. Therefore either tin is an exception to the generality of cases, or else my determination of its atomic volume is incorrect.

For the chlorid of tin, the atomic volume found was 131·4. If we regard chlorine as possessing the atomic volume 22, then tin in this compound has the value 43·4. Also from the vapor volume of stanntrimethylethyl I obtained the number 44·0 as the atomic volume of tin. Therefore it is not unlikely that a more accurate investigation will decide the atomic volume of this metal in its liquid compounds to be 44, but for the present it must remain in doubt.

This comparison of the atomic volumes of these elements in their liquid compounds at their boiling points, makes it therefore extremely probable that the atomic volume of every element in the liquid condition is a multiple by a whole number of that of the element typifying its group. That is, the atomic volumes of monatomic elements are multiples of 5·5, those of diatomic elements, of 7·8, those of triads, of 8·6, and lastly those of tetrads, multiples of 11, and consequently also of 5·5. Tin may be an exception, but the only one in twenty elements. Whether this rule be absolutely true or not, however, it will be seen to be very near the truth, since in only one case, that of iodine, have I altered the number found by so high a quantity as 1·0, and that in many cases, a change of only from 0·1 to 0·3 was necessary.

Furthermore, the numbers 5·5, 7·8, and 8·6 are not numbers for which we would expect to get exact multiples, unless some definite law accounted for it. Coincidences in two or three cases might be ascribed to accident, but in nineteen cases, all probability is against such an idea. I should not venture to alter any of Kopp's numbers, had he claimed rigid accuracy for them, and the variations between his own earlier and later results for carbon, seem to render such alterations as I have made, more justifiable. His later numbers for oxygen, hydrogen, and carbon, having been deduced by comparing the atomic volumes of forty-five compounds containing them, it will be seen I have not changed at all. His numbers for chlorine, bromine, iodine, and sulphur, however, which I have altered the most, were obtained from a comparatively small number of compounds containing them.

If my alterations be accepted, and also my new determinations, then the list of the atomic volumes of elements in their liquid compounds at their boiling points will stand as follows. Hydrogen 5·5, chlorine 22·0, bromine 27·5, iodine 38·5, oxygen 7·8 and 12·2, sulphur 23·4 and 28·6, selenium 23·4, mercury 15·6, zinc 23·4, nitrogen 2·15, 8·6, and 17·2, boron, phosphorus, vanadium, and arsenic 25·8, antimony, and possibly also bismuth 34·4, carbon 11·0, silicon, and titanium 33·0, tin, doubtful, either 41·5 or 44·0, probably the latter.

Note.—For the benefit of any who may wish to consult the authorities upon the subject of atomic volumes, I will state that, apart from Kopp's original articles, previously referred to, the best summaries I have been able to find are in the following works. "Watts' Dictionary," vol. 1, article "Atomic Volume." Kekulé's "Lehrbuch der Organischen Chemie," vol. 1, and Buff, Kopp, and Zamminer's "Lehrbuch der Physikalischen und Theoretischen Chemie."

ART. XIV.—*On the Occultator;* by Prof. LEWIS R. GIBBES, Prof. Astronomy, &c., in College of Charleston, Charleston, S. C.

IN the years 1848–1854, I was much engaged in observing occultations of fixed stars by the moon, and as a means of obtaining the approximate times of disappearance and reappearance with less labor than by calculation, I devised and constructed, in 1849 or 1850, an instrument for that purpose, to which I gave no special name. This instrument is still in my possession, but not in use, as certain parts, presently to be mentioned, have deteriorated with the lapse of time.

The Rev. Thomas Hill, of Cambridge, Mass., has published in the Nov. number of this Journal, a description and figure of an instrument for the same purpose, invented by him in 1842, and called by him the occultator. As the two instruments have the same end in view, there is a general agreement in plan, but the details differ. Mine is founded on the well known method of orthographic projection usually adopted in projecting eclipses and occultations, and I have published no description of it, nor do I propose doing so at the present time; but I wish to mention now, the devices I adopted to overcome certain difficulties which present themselves in both instruments, and the following explanations will be sufficiently intelligible, without a figure, to those familiar with the subject.

Mr. Hill finds the desired projection, on the plane of the instrument, of any point above that plane, as the extremity of a steel rod, by means of a silk thread stretched on a brass bow, set on a triangular base, and made normal to the chart or plane of the instrument, by screws in the base. I effect the same end very simply and readily by the following expedient, based on the optical principle that an incident ray and the corresponding reflected one *coincide when they are normal to the reflecting surface.* A bit of a good glass mirror with parallel surfaces, and a few inches square, having two lines traced on it with a writing diamond perpendicular to each other, is placed on the plane of projection, and moved under the given point, representing the place of observation, until by the eye placed above that point and close to it its image is seen to coincide with the intersection of the lines on the glass. Evidently, this intersection is then the projection required on the plane of the mirror, and the corresponding point on the plane of the instrument is easily determined, if desired, by the use of the intersecting lines as lines of reference. In my instrument there is no necessity for determining this point on the plane of the instrument, as the time of disappearance is found by moving the point representing the place of observation, and the disc representing the moon, to successive corresponding positions belonging to successive minutes of time, until *the image of the point is occulted by the disc;* the time of reappearance is determined in like manner. Instead of the pointed extremity of the rod, may be used a minute perforation in a thin sheet of metal or of pasteboard, through which may be viewed its image in the mirrors. If instead of either of these, were used a polished bead of glass or metal, smaller than the pupil of the eye, the light reflected from it would be seen in the mirror as a star-like point, which by the principles of the projection employed, would be the representatives of the star to be occulted, and by the interposition of the disc representing the moon, would actually disappear, and again reappear, as in an occultation. The use of the minute perforation gives the best results, as the eye cannot then fail to be in the proper position above the point indicating the place of observation.

I use the same method for adjusting in position a plane which in my instrument represents the plane of the parallel of latitude of the place of observation; this revolves about an axis, to which ought to be parallel the plane of the instrument or the chart, and this parallelism I test by finding, by reflection, the projections of a certain point in three or more widely distant inclined positions of the plane; these projections ought to lie in a right line. This plane ought also to form with the plane of the instrument, or plane of projection, an angle equal to the complement of the declination of the star, and any line in it, perpendicular to the axis of rotation, being taken as radius, will have its projection equal to the sine of the declination. Having a scale of equal parts, for sines, drawn on the plane of the instrument, corresponding to a certain line in the movable plane as radius, I take from a table of natural sines the proper length for the given declination, and placing the intersection of the cross lines on the mirror, to coincide with the proper point in the scale for sines, I rotate the plane about its axis, until the image of the extremity of the radius adopted coincides with the center of the cross; the plane will then have the proper angle, without requiring an arc divided into degrees and parts as in Mr. Hill's instrument.

Mr. Hill mentions several methods of adjusting the varying relation between the moon's hourly motion and semi-diameter, and decides finally upon a fixed permanent scale, in inches and parts, for hourly motion, and calculates the corresponding values of semi-diameter and parallax, in order to avoid the labor of dividing in every case the moon's hourly motion along its path into minutes of time. I adopt a permanent scale of ten inches for the earth's radius, (which fixes moon's semi-diameter also) compute the value on this scale for the hourly motion of moon in her orbit, from the horizontal parallax and hourly motions in R. A. and Dec., for each occultation, and adjust the instrument for this varying quantity, by using a scale of equal parts for minutes of time, *laid down on an extensible sheet.* This sheet is of caoutchouc, and *the scale is made variable by extending the sheet*, until sixty minutes or one hour on the scale corresponds in length with the computed value of the hourly motion of the moon. Two such scales might be used if the uniformity of the rate of extension of the sheet could not be trusted throughout the required extent of the variation of hourly motion in different parts of the orbit. These scales, made of the vulcanized caoutchouc obtainable at the date of the construction of the instrument, have now lost their elasticity and must be replaced before I can again use the instrument.

These expedients are simple, and I make them known, as the adoption of one or both may add to the efficiency of Mr. Hill's instrument, or similar instruments. I am not aware that any one had proposed them at the time I applied them to my instrument. I have heard that Mr. Adie of Edinburgh has proposed something similar to the last to thermometer scales I believe, but I do not know the date of his proposal, nor whether it has been adopted in the scales of any instruments.

College of Charleston, Charleston, S. C., 19th Dec., 1868.

ART. XV.—*On the wave lengths of the Spectral Lines of the Elements;* by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University. Read before the National Academy of Sciences, Aug. 16, 1867.

IN a memoir published in the Philosophical Transactions for 1864, Mr. Huggins has given for a particular scale the relative positions of a number of spectral lines. The scale selected was purely arbitrary. The number of elements examined was twenty-eight, and as the measurements were made with much accuracy it seemed to be desirable to extract from them all the information which they were capable of giving. For this purpose I have endeavored to determine the wave length of each line with as much precision as the present state of science permits, and in this manner to form tables which might enable me to determine whether the spectral lines are distributed according to definite laws, and if so, whether they can be considered as particular cases of the general principle of interferences.

The materials at my disposal were as follows: First, measurements by Angström of the wave lengths of 37 lines identified with particular elements and expressed in ten-millionths of a Paris inch. These were reduced to millionths of a millimeter by multiplying them by the constant 27·07. Second, measurements by Ditscheiner of 107 wave lengths, those of Fraunhofer's lines A, B, H and H′ not being available. The entire number of spectral lines measured upon his scale by Mr. Huggins is about 1000, distributed among the elements as follows:

Oxygen	32	Thallium	16	Mercury	24
Hydrogen	1	Silver	18	Cobalt	75
Nitrogen	74	Tellurium	44	Arsenic	31
Sodium	9	Tin	18	Lead	22
Potassium	15	Iron	102	Zinc	28
Lithium	3	Cadmium	15	Chromium	65
Calcium	50	Antimony	67	Osmium	18
Barium	31	Gold	23	Palladium	43
Strontium	71	Bismuth	44	Platinum	20
Manganese	46				

With respect to Ditscheiner's measurements I may here state that I have reduced them as in my memoir on the construction of a normal map of the solar spectrum.* That is, I have taken the wave length of the more refrangible line of D, as 589·43, as determined by Angström, instead of 588·8, which is the

* This Journal, II, vol. xliii, p. 1, Jan., 1867.

value found by Fraunhofer. The reduced values as thus obtained correspond very closely with those of Angström, as I have shown in the paper referred to, excepting in the part of the spectrum between C and D.

With these materials it first became necessary to identify a sufficient number of lines upon Mr. Huggins's scale with lines the wave lengths of which had been measured. This proved to be a task of extraordinary difficulty and a severe tax upon my time and patience. Mr. Huggins's scale cannot in any part be superposed upon that of Kirchhoff. The identification of a few strongly marked lines, like those which Fraunhofer selected, is of course easy, but these do not suffice as data for interpolation. By very careful and laborious comparisons of the two scales; by observing the coincidences or close approximations of lines produced by different elements; by occasional graphical constructions, and by employing the numerical results obtained in my two papers on wave lengths already published, I at last succeeded in obtaining data covering all of Mr. Huggins's scale between 589·5, or C, and 4671, which lies about half way between G and H. Of the correctness of the identification of the lines I shall be able I think to furnish satisfactory proof.

In selecting the wave lengths to be used in interpolation, I have given the preference to the measurements of Angström. But in the part of the spectrum between C and D, these were not sufficiently numerous to be of service. For this portion I have employed exclusively the values given by Ditscheiner, reduced in the manner already pointed out. In other parts of the scale it has also been necessary occasionally to use Ditscheiner's measurements, but in all these portions the difference between the results of Angström and of Ditscheiner rarely amounts to a unit in the first decimal place.

The number of lines upon Mr. Huggins's scale identified with lines the wave lengths of which have been measured, amounts to forty-five. For the purpose of interpolation these were divided into nine groups. In table I, I have brought together for convenience both the data employed and the results obtained.

In this table the column H gives the scale-number of the line; λ the wave length as observed, λ' the wave length as calculated by the formula obtained, Δ the difference between the observed and calculated wave-lengths, and ε the probable error for each group of data. The wave-lengths marked † are those of Ditscheiner; the others are given by Angström.

TABLE I.

H		λ	λ′	Δ	ε
589·5	†	656·65	656·56	+0·09	
612	†	652·07	652·25	−0·18	
623	†	650·03	650·14	−0·11	
709	†	634·23	634·03	+0·20	
772	†	623·59	623·39	+0·20	
795	†	619·60	619·90	−0·30	
813	†	617·48	617·39	+0·09	
818	†	616·71	616·71	0·00	± 0·18
813	†	617·48	617·42	+0·06	
818	†	616·71	616·76	−0·05	
843	†	612·75	612·81	−0·06	
856	†	610·80	610·73	+0·07	
939	†	598·12	598·13	−0·01	
1000	†	590·07	590·06	+0·01	
1005	†	589·43	589·45	−0·02	± 0·04
1000		590·04	590·04	0·00	
1005		589·43	589·45	−0·02	
1236		561·99	562·00	−0·01	
1247		560·70	560·65	+0·05	
1251		560·26	560·22	+0·04	
1274		557·65	557·53	+0·12	
1413	†	543·37	543·30	+0·07	± 0·06
1413	†	543·37	543·44	−0·07	
1422		542·83	542·91	−0·08	
1445	†	540·95	541·09	−0·14	
1481	†	537·55	537·42	+0·13	
1532		533·16	533·18	−0·02	
1545		532·00	532·01	−0·01	
1582		528·73	528·69	+0·04	
1600		527·32	527·25	+0·07	± 0·07

H		λ	λ′	Δ	ε
1600		527·32	527·34	−0·02	
1645		523·69	523·71	−0·02	
1708		518·79	518·79	0·00	
1723		517·66	517·65	+0·01	
2036	†	496·11	496·05	+0·06	
2092		492·23	492·31	−0·08	
2147		489·50	489·45	+0·05	
2200		486·52	486·52	0·00	±0·04
2147		489·50	489·51	−0·01	
2172	†	488·18	488·11	+0·07	
2200		486·52	486·59	−0·07	
2236	†	484·63	484·64	−0·01	
2315	†	480·56	480·55	+0·01	±0·04
2236	†	484·63	484·91	−0·28	
2315	†	480·56	480·26	+0·30	
2785		457·75	457·74	+0·01	
3272		440·81	440·83	+0·02	
3341		438·63	438·67	−0·04	
3597		431·03	431·04	−0·01	±0·18
3272		440·81	440·84	−0·03	
3341		438·63	438·69	−0·06	
3532		432·78	432·81	−0·03	
3597		431·03	431·00	+0·03	
3728		427·45	427·46	−0·01	
3773		426·26	426·28	−0·02	±0·04
3597		431·03	430·97	+0·06	
3728		427·45	427·45	0·00	
3773		426·26	426·29	−0·03	
3812		425·21	425·29	−0·08	
3909		422·94	422·88	+0·06	
4267		414·71	414·66	+0·05	
4633		407·45	407·39	+0·06	
4671		406·59	406·61	−0·12	±0·06

The method of interpolation employed was that first given by Cauchy* and afterward reduced to a practical form by M. Yvon Villarceau, in the Connaissance des Temps for 1852. As in my recent reduction† of Kirchhoff's scale, I employed only expressions of the form

$$\lambda = a + bh + ch^2 + dh^3 \ \&c.$$

these being found to give an approximation within the limits of the probable errors of observation.

Table II, gives the values of the constants, a, b, c, and d, as deduced from the data given in Table I.

In this table H is the initial and H′ the terminal point of Mr. Huggins's scale in each group of data employed for discussion. For easy use in calculation it is more convenient to transfer the initial and terminal points, so as to make each parabolic curve

* Moigno. Leçons de Calcul Differentiel et de Calcul Integral. Tome Ier, 513.

† This Journal, II, May, 1868, vol. xlv, p. 1.

begin and end with a multiple of ten upon the scale. This has been done in Table III.*

TABLE II.

H	H′	*a*	*b*	*c*	*d*
589·5	818	656·5589	−19·0571	−0·4300	+0·4984
800	1018	619·5488	−15·6660	−0·3960	+0·4264
1000	1413	589·9750	−12·5440	+0 2369	+0·0165
1413	1600	543·5600	− 7·8810	−0·8653	+0·2469
1600	2200	527·3240	− 8·1670	+0·2276	------
2147	2315	489·5078	− 5·6093	+0·1622	------
2230	3597	485·2950	− 6·1092	+0·2398	−0·0061
3272	3773	440·8098	− 3·3028	+0·1019	−0·0043
3597	4671	430·9556	− 2·7232	+0·0433	------

TABLE III.

H	H′	*a*	*b*	*c*	*d*
590	800	656·4637	−19·0614	−0 4225	+0·4984
800	1000	619·5488	−15·6660	−0·3960	+0·4264
1000	1410	589·9750	−12·5440	+0·2369	+0·0165
1410	1600	543·7956	− 7·8284	−0·8875	+0·2469
1600	2200	527·3240	− 8·1670	+0·2276	------
2200	2310	486·5804	− 5·4374	+0·1622	------
2310	3270	480·5580	− 5·7372	+0·2252	−0·0061
3270	3770	440·8759	− 3·3069	+0·1022	+0·0043
3770	4670	426·3739	− 2·5734	+0·0433	------

From these tables it will be seen that in three out of the nine groups parabolas of the second order are sufficient.

If the results obtained in the discussion of Mr. Huggins's scale be compared with those given in my paper on the measurement of wave-lengths by the method of comparison in which Kirchhoff's scale is subjected to a similar treatment, the greater regularity of the former will be at once remarked. Thus in Kirchhoff's scale in two instances, parabolas of the 5th order are required. In two cases a straight line represents the observations best, the probable error being however unusually large. In the case of Mr. Huggins's scale, on the other hand, the probable error for each group appears to be much within the limits of the errors of observation. In the 6th and 7th groups of data discussed in the present paper, the intervals between the scale numbers are larger than I could have wished. Yet the differences between calculation and observation are not large, the curves here approaching a straight line. The greater regularity or relatively lower order of the curves in the case of Mr. Huggins's scale arises doubtless from the fact that the prisms employed were, during the whole series of observations, in a constant position, which was not the case with Kirchhoff's apparatus.

* In using the constants in Table III for computing wave-lengths the value of *h* is to be found by subtracting the initial number in column 1 from the scale number and dividing the remainder by 100.

By means of Table III the wave lengths of all the lines upon Mr. Huggins's scale between C and scale-number 4671 have been computed. These are given in Table IV. I have however not followed the order adopted by Mr. Huggins, but have put near each other as far as possible those elements which form members of natural groups.

TABLE IV.*

NITROGEN.

H		λ
629·5	2	649·08
807	2	618·45 h.d
959	1	595·33
967	6	594·26
975	4	593·20
978	1	592·80
1100	5	577·53
1118	1	575·37
1135	2	573·37
1150	2	571·64
1171	1	569·24
1177	8	568·55
1180	1	568·21
1187	7	567·43
1294	5	555·56
1302	5	554·71
1310	1-5	553·85
1314 5	5	553·37
1319	2	552·91
1349	1-2	549·78
1366	1	548·04
1383	5	546·33
1394	7	545·43
1502	2	535·81
1516	2	534·57
1537	2	532·69
1713		518·39 h
1718		518·01 h
1721		517·78 h
1860	5	507·64
1900	3	504·88
1929	7	502·93
1941	5	502·12
1951	5	501·46
1956	5	501·14
1960·5	10	500·84
1967	10	500·42
1978	5	499·71
1990	3	498·94
2079	5	493·43
2140	5	489·86
2168	5	488·28
2192	3	486·96
2205	5	486·18
2221	3	485·30
2305	2	480·84
2336	1	479·08
2350	7	478·29
2642	2	463·78 d
2669	3	462·60
2689	1	461·74
2707	1	460·97
2722	1-5	460·35
2738	1-5	459·68
2856		454·96 h
2904 }		453·15 h
2978 }		450·46 h
3009	1	449·37
3011	1	449·31
3056		447·76 h
3144		444·83 d
3174 }		443·86 h
3219 }		442·43 h
3292		440·11 h
3456	2	434·99 n
3863		423·99 h
3991		420·90 h
4145		417·34 h
4263		414·75 n
4330		413·34 h
4473		410·45 n
4505	1	409·82 n

HYDROGEN.

H		λ
589·5		656·76 h

OXYGEN.

H		λ
807	2	618·45 h.d
1678	5	521·10
1699	5	519·47
1737	3	516·57
2043	5	495·61
2060	7	494·58
2089	5	492·83
2119	5	491·07
2145	5	489·58
2181	5	487·56
2213	3	485·74
2502	7	470·33 n
2512	7	469·84 n
2563	5	467·38 n
2597	5	465·82 n
2626	2	464·50
2642	2	463·78 d
2748	1	459·27
2766	1	458·54
3086		446·75 h
3238	1	445·03 n
3241	1	441·75 n
3395	1	436·85 n
3456	2	435·00 n
3560	1	431·95 n.d
3710		427·87 h
4059	1	419·29
4087	1	418·65
4232		415·43
4395	2	412·01
4615	1	407·82 n
4639	1	407·31 n

POTASSIUM.

H		λ
727	5	630·85 s
763	5	624·81 s
840	1-5	613·25 s
1049	1	583·78 n
1069	5	581·79 n
1073	1 5	580·80 n
1328	1	551·96 n
2260	2	483·18 n
3328	2	438·96 n
3591	1-5	431·16 n
3762	2	426·00 n
4082	3	418·77 n

SODIUM.

H		λ
818	1-5	616·72 s
821	1	616·56 s
1000	8	590·04 s
1005	8	589·43 s
1169	2	569·46 s
1174	1	568·90 s
1746	1	515·90 n
1753	5	515·37 n
1991		498·87 h

THALLIUM.

H		λ
596	4	655·32 s
768	1	624·01 h
921	2	600·76 s
960	6	595·20 s
1055	2	583·02 s
1099	5	577·65 n
1356	1	549·07 s
1505	9	535·54 s
1747	4	515·82 n
1851	4	508·27 n
1885	1	505·91 s
1999	2	498·36 h
2146	2	495·44 s
2379	2	476·70 n
2437	4	473·61 n
4443	3	411·16 n

LITHIUM.

H		λ
856	3	610·73 s
2013	2	497·48 s
2732	5	459·93 n

SILVER.

H		λ
690	2	637·48 s
762	2	624·96 s
899	5	604·06 s
943	5	597·58 s
1031·2	2	586·03 s
1207	5	565·19 n
1223	2	563·42 n
1227	7	563·00 n
1240	3	561·46 s
1257	7	559·57 s
1276·3	5	557·46 s

* In this table the small numerals after the scale numbers give the estimated intensities of the spectral lines upon an ascending scale. The letters s, n, h and d have the following significations; s, a sharply defined line, narrow when the slit is narrow; n, a line with indistinct edges even when the slit is narrow; h, a hazy line not resolvable into finer lines; d, double but too close for measurement.

TABLE IV (continued).

H		λ	
1286·5	3	556·36	s
1372	2	547·44	s
1380	7	546·63	s
1421	3	542·81	s
1435	3	541·64	s
1446		540·72	h
1675	5	521·34	n

GOLD.

H		λ	
643	5	646·32	s
659	5	643·28	s
727	5	630·85	s
734	5	629·63	s
747	3	627 43	s
951	5	596·45	s
956	5	595·75	s
981	7	592·41	s
1011	1	588·62	n
1025	2	586·82	s
1045	4	584·38	s
1081·5	5	579·76	s
1109	1	576·44	s
1199	5	566·07	s
1266	5	558·59	n
1647	5	523 55	s
1869	5	506·87	s
2291	5	481·51	s
2326	4	479·64	s
3026	2-5	448·78	s

ARSENIC.

H		λ	
672	1	640·83	n
707	1	634·37	n
759	1	625·45	n
812	6	617·67	n
833	1	614·35	n
850	3	611·67	n
870	2	608·53	n
908·8	3	602·59	n
1042	1	584·66	n
1090	5	578·73	n
1203	7	565·63	n
1231	5	562 54	n
1257	5	559·58	n
1291	6	558·88	n
1348	6	549·88	n
1443	1	540·97	n
1465	1	539·08	n
1529	6	533·41	n
1577	1	529·20	n
1648	4	523·47	n
1737	1	517·63	n
1814	4	510·89	n
1993	2	498·75	n
2153	1	489·12	n
2450	1	472·95	n

H		λ	
2859	1	454·84	h
2897	1	453·40	h
3006	1-5	449·48	h
3097	2-5	446·38	h
3381	1	437·34	n
3497	1	433·77	n

ANTIMONY.

H		λ	
614	1	651·92	n
620	1	650·72	n
640	1	646·89	n
679	2	639·51	h
719	1	632·24	n
729	2	630·49	n
739	2	628·77	n
765	3	624·49	n
787	1	621·09	h
796	5	619·77	h
819	2	616·56	n
837	7	613·70	n
871	7	607·45	n
889	3	605·59	n
921·1	8	600·75	n
937·5	2	598·72	n
981·5	1	592·35	n
988·5	3	591·45	n
1000·5	3	589·96	n
1041	5	584·78	s
1057	5	582·77	s
1081	1	579·82	n
1145	5	572·22	n
1158	1	570·72	n
1189	5	567·26	n
1207	1	565·19	h
1214	4	564·41	n
1220	1	563·75	n
1279	4	557·18	n
1383	3	546 33	n
1457		539·77	h
1471	3	538·55	n
1501		535·90	
1636	3	524·42	h
1661	1	522·44	s
1715	3	518·24	h
1765	3	514·47	h
1803	3	511·64	h
1849	1	508·41	s
1900	2	504·88	h
1919	2	503 60	h
2051	3	495·13	n
2171	3	488·11	n
2251	2	483·67	n
2339	2	478·90	h
2377	2	476·81	h
2397	2	475·74	n
2440	2	473·46	n
2488	3	471·03	n

H		λ	
2529	3	469·01	n
2687	1	461·83	h
2740	1	459·60	h
2763	2	458·66	n
2977	2	450·51	n
3115	1	445·79	n
3359	1	437·97	h
3446	4	435·29	n
3756	3	426·68	n
3819	1	425·08	n
4043	1-5	419·67	n

BISMUTH.

H		λ	
621	4	650·53	s
837·3	5	613·72	n
884	5	606·36	n
887	5	605·95	s
899	1	604·06	s
939	1	598·14	h
943	1	597·58	h
1026	2	586·68	n
1059	1	582·53	n
1143	6	572·36	n
1197	3	566·31	n
1293	1	555·67	s
1305	5	554·39	s
1395	1	545·13	h
1453	1	540·12	h
1495	1	536·43	h
1598	7	527·48	n
1675	10	522·12	n
1685	1	520·56	n
1759	7	514·92	n
1787	6	513·88	n
1834	1	509·46	n
1851·3	5	508·41	n
1979	1	499·52	h
2015		497·36	h
2105	5	491·89	n
2119	1	491·07	n
2317	1	480·15	n
2408	1	475 14	n
2467	5	472·09	n
2502	1	470·33	n
2837	5	455·68	n
3060	1	447·62	n
3315		439·39	n
3481	3	434·24	n
3519	2	433·13	n
3619	5	430·31	n
3778	5	426·11	n
4378	4	412·85	n
4603	3	407·97	n

CALCIUM.

H		λ	
622	5	650·34	s
625	1-5	649·77	s
637		647·47	s
642	2	646·51	s
649		645·17	s
655		644·03	s
699	2	635·83	s
709	2	634·03	s
723	2	631·55	s
813	1	617·51	s
818	4	616·67	s
843	3	612·78	s
859	2	611·06	s
863	2	609·63	s
882		606·67	h
921·2	2	600·63	s
934		598·87	h
1031	3-5	586·05	s
1247	5	560·63	s
1249	5	560·39	s
1252	1-5	560·13	s
1256	2-5	559·69	s
1258·5	5	559·41	s
1260	4	559·25	s
1265	7	558·59	s
1335	7	551·23	s
1506	1	535·45	s
1599·5	4	527·36	s
1605	3	526·93	s
1609	5	526·61	s
1612	1-5	526·36	s
1702	1	519·24	s
1907	7	504·40	s
1935	3	502·52	s
2172	7	493·85	s
2777	5	458·10	s
2784	3	457·82	n
2792	2	457·50	s
3124	3	445·49	s
3181	3	443·64	s
3212	2	442·66	s
3561	3	431·94	s
3602·5	2-5	439·79	s
3617	4	430·36	s
3628	2-5	430·08	s
3665	3	429·06	s
3692	2-5	428·36	s
3909	6	422·86	s

BARIUM.

H		λ	
608	1	653·02	s
621·5	7	650·44	s
645	1	645·54	s
704	1	634·83	s
847	2	612·15	s
879	2	607·13	s

TABLE IV (continued).

H		λ	
908	1-5	602·70	s
925	1-5	600·18	s
943	1-5	597·58	s
993	5	590·89	s
1005	1	589·43	n
1034	6	585·67	s
1057	1	582·77	s
1096	1	578·00	s
1119	1	575·26	s
1308	4	554·06	s
1327	1	552·06	s
1351	5	549·58	s
2075	9	493·67	n
2133	4	490·23	n
2459	5	472·49	s
2535	5	468·71	s
2856	9	454·96	s
2931	3	452 23	n
4127	2	417·74	n
4332	5	413·30	n

STRONTIUM.

H		λ	
595	2	655·51	s
619	2	650·91	s
655		644·03	h
669	4	641·39	s
681	1	639·15	s
684	1	638·59	s
692	5	637·12	s
703	2	635·21	s
705·5	5	634·66	s
723		631·55	h
745	2	627·77	s
760		625·29	h
777		622·61	h
807	5	618·45	s
856	1	610·73	s
924		600·32	h
941	1	597·86	h
945	3	597·30	s
1061	5	582·29	s
1102	5	577·29	n
1203	1	565·64	n
1227	1 5	562·98	st
1268	2	558·37	s
1301	2	554·81	s
1305	2-5	554·39	s
1311	3	553·74	s
1324	3	552·38	s
1341	3	550·61	s
1349	7	549·78	h
1359	3	548·75	s
1365	4	548·15	s
1397	2-5	544·93	s
1425	2	542·48	s
1467	1-5	538·91	h
1617	3	525·95	s
1638	2	524·26	s
1651	1-5	523·23	s
1656	1-5	522·83	s
1659	1-5	522·60	s
1665	1	522·14	s
1745	1	515·98	s
1817	5	510·68	
2021	1	496·98	s
2029	2	496·48	s
2060		494·58	h
2145	1	489·57	s
2176	1-5	487·84	s
2180	1-5	487·62	s
2185	1	487·35	s
2213	1	485·74	s
2254	1-5	483 51	s
2291	1-7	481·51	s
2343	1-5	478·69	s
2410	5	475·04	s
2427	1	474·14	s
2469	1	471·98	s
2726	5	460·18	n
3169	1	444·02	s
3389	5	437·03	n
3409	5	436·41	n
3489	1	434·01	n
3553	1	432·16	h
3604	5	430·73	n
3952	5	421·83	n
4181	3	416·58	n
4599	3	408·05	n

IRON.

H		λ	
623	1	650·15	s
641·5	5	646·61	s
667	5	641·77	s
673	1	640·64	s
674	5	640·45	n
683	5	638·88	s
696·2	5	636·35	s
709	7	634 02	s
719	5	632·24	s
726	5	631·02	s
758	5	625·62	s
763	5	624·81	s
772	1	623·38	s
795	1	619·92	s
829	2	614·98	s
852	5	611·36	s
869·5	5	608·60	s
909	5	602·55	s
937	5	598·43	s
953	5	596·17	s
995	2	590·64	s
1011	2	588·62	s
1030	2	586·18	s
1090	5	578·73	s
1225	5	563·20	s
1236	1-5	561·93	s
1247	5	560·69	s
1251	2	560·24	s
1261	2	559·14	s
1274	7	557·71	s
1276	2	557·49	s
1338	7	550 91	s.d
1383	5	546·33	s
1391	7	545·53	s
1400	7	544·64	s
1413	5	543·37	s
1419	7	542·97	s
1421·5	7	542·78	s
1434	5	541·73	s
1438	5	541·39	s
1445	7	540·80	s
1446	7	540·71	s
1456	7	539·86	s
1459·5	2	539·55	s
1467	7	539·00	s
1481	1	537·67	s.d
1485·3	5	537·28	s
1486	2	537·24	s
1486 5	2	537·18	s
1488	2	537·05	s
1532	2	533·14	s
1537	7	532·69	s
1541	5	532·34	s
1545	5	531·98	s
1560	2	530·67	s
1574	2	529·45	s
1582	7	528·77	s.d
1593	5	527 84	s
1599	7	527·40	s
1600	7	527·33	s
1603	5	527·09	s
1608	5	526·68	s
1613	3	526·28	s
1621	3	525·63	s
1632	3	524·74	s
1645	1	523·70	s
1653	1	523·09	s
1662	5	522·36	s d
1691	3	520·09	s
1696	3	519·70	s
1698	5	519·55	s
1713	7	518·39	s
1728	1	517·25	s
1731	5	517·03	s
1753	3	515·38	s
1767	7	514·33	s
1775	5	513·74	s
1821		510·40	h
1940·5	1	509·01	s
2036	1-5	496·05	s
2092	1	492·65	s
2098	7	492·30	s.d
2147	1	489·47	s
2781	5	458·02	s
3272	1-5	440·81	s
3341	2	438·55	s
3532	1-5	432·75	s
3597	1-5	430·93	s
3610	1-5	430·55	s
3623	5	430·21	s
3645	5	429·61	s
3728	2	427·40	s
3773	1-5	426·24	s
3812	1-5	425·26	s
4009	1	420·46	s
4019	1	420·23	s
4221	1	415·70	s
4267	1	414·67	s
4323	1	413·49	s
4633	1	407·41	s
4671	1	406·73	s

MANGANESE.

H		λ	
704	2	634·93	s
837	}	613·72	} h
843	}	612·78	}
909	3-5	602·55	s
913	2-5	601·96	s
915·5	2	601·66	s
1289	1	556·00	s
1329	1-5	551·85	n
1376	1	547·04	s
1413	7	543·37	s
1428	1	542·22	s
1438	7	541·39	s
1443	7	540·97	s
1452	2	540·20	s
1456	2	539·86	s
1473·5	3	538·34	s
1505·5	5	535·50	s
1515	4	534·65	s
1559	}	530 58	} h
1571	}	529·79	}
1617	5	525·96	s
2267		482 80	s
2343	5	478 69	s
2379	6	476·70	s
2385	5	476·38	s
2401	5	475·62	s
2433	1	473 82	s
2456	1	472 65	s
2492	7	470·83	h
2987	7	450·14	s
2999	1	450·07	s
3021	1	448·96	s
3054	1	447·83	s
3097	1	446·38	s
3102	1	446·22	s
3114	1	445·82	s

TABLE IV (continued).

H		λ	
3120	1	445·62	s
3131·5	3	445·31	s
3133	2	445·19	s
3141	1	444·99	s
3180	1	443·67	s
3242	1	441·72	s
3691	1	428·44	s
3749	1	426·85	s
3782	1	426·11	s
3870	2	423·84	s

COBALT.

H		λ	
645	1	645·94	n.d
701	5	635·47	s
731	2	630·75	s
745	5	627·77	s
763	2	624·81	s
844		612·62	h
865	7	609·32	s
891	7	605·29	s
921	1	600·76	s
923	2	600·47	s
931	5	599·30	s
937	5	598·45	s
985	2	591·90	s
1039	2	585·04	s
1043	2	584·53	s
1207	5	565·19	s
1217	5	564·08	s
1257	7	559·58	s
1361	1	548·55	s
1401	1-5	544·54	
1470	5	538·64	s
1483	1	537·50	s
1491	1	536·79	s
1496	2	536·34	s
1500·5	2	535·75	s
1501·5	2	535·66	s
1508	2	535·27	s
1514	4	534·74	s
1525	5	533·77	n.d
1534	5	532·96	s
1539	3	532·52	s
1543	2	532·17	s
1549	5	531·64	s
1573	2	529·54	s
1579	2	529·02	s
1584	2	528·60	s
1586	1-5	528·43	s
1591	5	528·01	s
1602	3	527·17	s
1604	3	527·01	s
1617	5	525·96	s
1619	3	525·79	s
1622	5	525·55	s
1626	3	525·23	s
1642	5	523·94	s
1650	5	523·31	s
1670	1-5	521·73	s
1685	5	520·56	s
1699	5	519·46	s
1707	5	518·85	s
1743	5	516·13	s
1756	5	515·15	s
1781		513·30	n
1813		510·96	h
1857	5	507·85	h
1876	5	506·52	s
1887	3	505·77	s
1925	5	503·19	s
2021	5	496·98	s
2186	3	487·29	s
2236	3	484·47	s
2286	2	481·78	s
2325	1	479·63	s
2409	5	475·09	s
2438	5	473·56	s
2471	5	471·98	s
2550	5	468·01	s
2785	2	457·78	n
2823	1	456·22	n
2862	3	454·73	s
2910	3	452·92	s
4388	3	412·15	n
4394	3	412·07	n
4437	1	411·16	n
4523	1	409·48	n

CHROMIUM.

H		λ	
621	5	650·53	n
640	5	646·89	n
654	1	644·22	n
817	1	616·86	n
843	1-5	612·77	n
856	7	610·73	n
1081	1	579·82	h
1087	1	579·10	h
1090	1	578·73	h
1212	1	564·63	s
1242	1	561·23	s
1439	1-5	542·14	s
1507·5	7	535·31	s
1510	1	535·10	s
1532	1	533·14	s
1567	1	530·07	d
1594	7	527·75	s
1605·5	1	526·93	s
1607	1	526·77	s
1619	5	525·79	s
1626	5	525·23	s
1640	5	524·10	s
1657	7	522·76	s
1677	4	521·18	s
1680	3	520·95	s
1681·5	3	520·87	s
1749	2	515·68	n
1815	7	510·82	n
2097	1	492·35	s
2156	1	488·95	s
2175	7	487·89	s
2181	3	487·57	s
2198	5	486·63	s
2257	5	483·35	s
2266	1-5	482·86	s
2336	7	479·07	s
2400	1	475·57	s
2406	5	475·25	s
2435	7	473·73	s
2452	7	472·85	s
2474	1	471·72	s
2619	1	464·81	s
2627	7	464·46	s
2632	1-5	464·21	s.d
2663	5	462·86	s
2701	7	461·31	s
2740	7	459·60	s
2768	7	458·45	s
2840	7	455·56	s
2871	7	454·38	s
2887	7	453·78	s
2899	7	453·33	s
2914	1	452·78	s
2927	1	452·30	s
3007	1	449·45	s
3444	2	435·35	s
3465	1	434·72	s
3473	1	434·48	s
3489	1	434·01	s
3663	3	429·13	s
3719	3	427·64	s
3797	3	425·64	s
3905	1	422·96	s
3951	1	421·90	n

CADMIUM.

H		λ	
639	2	647·08	n
656	8	643·84	s
889·5	5	605·22	n
918	1	601·22	n
953	1	596·17	n
986	1	591·77	n
1473	10	538·38	n
1517	10	534·47	n
1556	1	531·01	n
1747	1	515·83	s
1843	10	508·83	s
2315	8	480·27	s

H		λ	
2562	6	467·43	s
3239	4	441·81	s

ZINC.

H		λ	
696	7	637·37	s
782	1	621·84	s
855	5	610·89	n
895	1	604·69	s
909	5	602·55	n
1001	2	589·90	s
1062	5	582·16	s
1110	5	576·33	s
1122	6	574·90	n
1269	5	558·26	n
1283	1	557·75	n
1519	5	534·30	n
1626	1	525·23	n
1645	5	523·70	n
1743	5	516·13	n
1790	5	512·64	n
1797	5	512·13	n
1845	5	508·69	n
1859	1	507·71	n
1893	5	505·35	n
2016	5	497·29	n
2091	8	492·71	n
2110	8	491·60	n
2191	5	487·02	n
2294	7	481·35	s
2469	6	471·98	s
2559	4	467·58	s

LEAD.

H		λ	
898	1	604·22	s
924	2	600·33	n
1000	1	590·02	n
1015	2	588·14	n
1031·1	4	586·05	s
1055	5	583·03	n
1094	1	578·25	n
1240	9	561·46	
1279	5	557·18	n
1479	7	537·85	n
1593·3	5	527·84	n
1685	1	520·57	s
1698	1	519·54	n
1735	5	516·72	n
1900·3	2	504·86	n
2384	3	476·44	n
3329	7	438·93	n
3730	2	427·35	n
3831	6	424·83	n

TABLE IV (continued).

MERCURY.

H		λ	
685	1	638·41	n
697	1	636·20	n
826	7	615·46	n
863	2	609·63	n
1008	1·5	589·01	n
1019	5	587·59	n
1060	3	582·41	n
1074		580·68	h
1083·5	8	579·51	n
1100·5	8	576·93	n
1177	2	568·55	n
1252	1	560·13	n
1385	10	546·13	n
1421	1·5	542·81	n
1487	5	537·15	n
1583	5	528·69	n
1662	3	522·37	h
1777	5	513·60	h
2033	1	496·24	h
2101	5	492·12	h
2263	5	483·02	n
3421	8	436·05	n

TELLURIUM.

H		λ	
657·3	7	643·59	s
693	1	636·93	s
703	5	635·11	n
735	2	629·46	s
765·3	3	624·44	n
774	3	623·07	s
894	2	604·84	s
917	1	601·37	s
927	5	599·90	n
945·3	6	597·26	s
971·5	1	593·66	s
1030·3	5	586·14	s
1035	2	585·54	s
1111	7	576·21	n
1122	1	574·90	s
1151	7	571·53	n
1204	6	565·53	n
1230	1	562·65	s

H		λ	
1270	4	558·15	s
1357	4	548·96	s
1366	4	548·05	s
1396	4	545·03	s
1438·3	4	541·40	s
1485	4	537·32	s
1548·2	3	531·69	s
1562	2	530·50	s
1658	5	522·68	n
1773	1	513·89	n
1909	5	504·27	n
2191	1	487·01	h
2245	1	484·00	h
2341	5	478·80	h
2497		470·58	h
2595	1	465·91	n
2613	1	465·09	n
2730	1	460·01	n
2739	1	459·64	n
2875		454·23	h
3051	1	447·93	h
3435	5	435·62	n
3619	2	430·32	n
3779		426·09	h

PALLADIUM.

H		λ	
685	5	638·40	s
762	2	624·96	s
837	2	613·72	s
995 }		590·64 }	h
1005 }		589·43 }	h
1023	3	587·07	s
1031·5		586·00	h
1056·7		582·90	s
1068		581·42	h
1084		579·46	h
1127	3	574·31	s
1129	1	574·08	s
1185	5	567·65	s
1199	5	566·08	s
1212	5	564·63	s
1219 }		563·86 }	h
1233 }		562·29 }	h

H		λ	
1240·5	5	561·40	s
1248	5	560·57	s
1259·2	7	558·24	s.d
1281	1·5	556·96	s
1299	1	555·03	s
1303	1·5	554·60	n
1331	1·5	551·65	n
1380	1·5	546·63	s
1412	5	543·51	s
1456	2	539·86	s
1492	5	536·71	s
1511	5	535·01	s
1548	5	531·73	s
1569	3	529·88	s
1617	5	525·96	s
1622	5	525·55	s
1642	1·5	523·94	s
1674	7	521·42	s
1735	2	516·72	s
1798	1·3	512·05	s
1807	1	511·40	s
1873	5	506·73	s
2175	1·5	487·89	s
2279·5	2	482·12	s
3065	7	447·45	s
3963	4	421·57	s

TIN.

H		λ	
648	7	645·27	s
1076	10	580·43	n
1219	2	563·86	s
1260	8	559·25	n
1284	8	557·63	n
1484	1	537·40	s
1506	2	535·45	n
1520	1	534·21	h
1524	4	533·85	n
1576	1	529·29	n
1657	2	522·75	n
1821	3	510·40	n
2205	3	486·17	n
2777	2	458·10	n
2931	4	452·23	n

OSMIUM.

H		λ	
641	1	646·70	s
741	7	628·43	s
929	7	599·59	s
1029	1	581·31	s
1093	5	578·37	s
1141	1	572·69	s
1264	1	558.91	s
1322	2	552·60	s
1405	5	544·14	s
1432	1·5	541·89	s
1683	5	520·72	s
1859	5	507·71	s
2861	7	454·76	s
3225	2	442·24	s
3421	1	436·05	s
3583	1	431·33	s
3645	1	429·61	s
3773	3	426·24	s

PLATINUM.

H		λ	
689	5	637·63	s
913		601·95	h
939		598·14	h
950	5	596·59	s
958	5	595·47	s
1041	5	584·78	s
1045	5	584·28	s
1073	7	580·80	d
1367	7	547·95	s
1459	5	539·59	s
1484	5	537·41	s
1561	2	530·76	s
1653	1	523·08	s
1689	3	520·25	s
1879	1	506·32	s
2857	1	454·92	s
2936	3	451·97	s
2999	7	449·72	s
3156	1	444·45	s
3525	1	432·95	s

I have stated above that the principal difficulty with which I have had to contend was the identification of a sufficient number of scale lines with lines of which the wave lengths had been measured either by Angström or by Ditscheiner. Upon the correctness of this identification the value of my work depends. For the purpose of verification, before proceeding to determine parabolic formulas for different parts of the scale, I carefully compared Kirchhoff's and Huggins's scales with each other for their whole lengths, selecting the lines about whose correspondance there could be no doubt. When the formulas had been obtained, I calculated the wave lengths for all these lines upon both scales. The results of this comparison are given in Table V.

TABLE. V.

K	H	Kλ	Hλ	Δ	E
717·5	619	650·68	650·91	−0·23	Sr
718·7	621·5	650·24	650·44	−0·20	Ba
720·1	625	649·88	649·77	+0·11	Ca
729	637	647·60	647·47	+0·13	Ca
731.7	642	646·93	646·51	+0·42	Ca
736·9	649	645·60	645·17	+0·43	Ca
740·9	658	644·60	644·03	+0·57	Ca
753·8	669	641·38	641·39	−0·01	Sr
756·9	673	640·60	640·64	−0·04	Fe
771·8	696	636·99	637·37	−0.38	Zn
831	772	623·52	623·38	+0·14	Fe
849·7	795	619·60	619·92	−0·32	Fe
860·2	813	617·47	617·52	−0·05	Ca
863·9	818	616·71	616·71	0·00	Ca
880·9	837	613·50	613·73	−0·23	Sb
884·9	843	612·94	612·78	+0·16	Ca
890	847	611·75	612·15	−0·40	Ba
894·9	856	610·80	610·73	+0·07	Ca
958·8	939	598·22	598·14	−0·08	Pt
961·5	943	597·05	597·58	−0·53	Ba
1002·8	1000	590·07	590·02	+0·05	Na
1006·8	1005	589·43	589·42	+0·01	Na
1029·3	1031	585·87	586·05	−0·18	Ca
1031·8	1034	585·53	585·67	−0·14	Ba
1043	1045	583·95	584·28	−0·33	Au
10[illegible]0	1057	582·91	582·79	+0·12	Ba
1158	1177	568·34	568·56	−0·22	Hg
1200·6	1225	562·95	563·20	−0·25	Fe
1207·3	1236	561·96	562·00	−0·04	Fe
1217·8	1247	560·75	560·62	+0·13	Fe.Ca
1219·2	1249	560·46	560·46	0·00	Ca
1231·3	1261	559·12	559·14	−0·02	Fe
1235	1265	558·65	558·70	−0·02	Ca
1245·6	1276	557·38	557·49	−0·11	Fe
1274·7	1311	553·92	553·75	+0·17	Sr
1301	1341	550·79	550·61	+0·18	Sr
1337	1383	546·72	546·33	+0·39	Fe
1343·5	1391	545·93	545·53	+0·45	Fe
1352·7	1400	544·94	544·64	+0·30	Fe
1385·7	1439	541·33	541·30	+0·03	Cr
1410·5	1467	538·77	538·90	−0·13	Fe
1421·5	1481	537·80	537·67	+0·17	Fe
1430·1	1491	536·62	536·79	−0·17	Co
1440·2	1501·5	535·61	535·86	−0·25	Co
1443·5	1506	535·25	535·45	−0·20	Ca
1506.3	1582	528·77	528·77	0·00	Fe
1523·7	1599·5	527·42	527·37	+0·05	Fe.Ca
1539	1617	526·00	525·96	+0·04	Sr
1566·5	1642	523·87	523·94	−0·07	Co
1575	1651	523·29	523·22	+0·07	Sr
1577·5	1656	523·08	522·83	+0·25	Sr
1582	1659	522·74	522·60	+0·14	Sr
1596	1670	521·63	521·73	−0·10	Co
1601·5	1677	521·31	521·18	+0·12	Cr
1627·2	1702	519·25	519·24	+0·01	Ca
1650·3	1728	517·56	517·26	+0·30	Fe
1769·5	1843	508·77	508·83	−0·06	Cd
1832·8	1907	504·65	504·39	+0·26	Ca
1867·1	1940·5	502·25	501·86	−0·39	Fe
1961	2036	496·11	496·05	+0·06	Fe
1989·5	2075	493·78	493·67	+0·11	Ba
2058	2172	488·40	488·06	+0·34	Ca

TABLE V (continued).

K	H	Kλ	Hλ	Δ	E
2071·3	2186	487·19	487·29	−0·10	Co
2103·3	2236	484·66	484·48	+0·18	Co
2132·3	2286	482·00	481·80	+0·20	Co
2148·5	2315	480·56	480·27	+0·29	Cd
2157·4	2325	479·76	479·70	+0·06	Co
2626·5	3156	444·61	444·45	+0·16	Pt
2668·5	3239	441·90	441·75	+0·15	Cd
2721·6	3341	438·75	438·55	+0·20	Fe
2822·3	3532	432·82	432·75	+0·07	Fe
2834·2	3561	431·77	431·92	−0·15	Ca
2854·7	3597	431·03	430·92	+0·11	Fe
2864·7	3617	430·79	430·36	+0·43	Ca
2869·7	3628	430·21	430·05	+0·36	Ca

In this table K represents the scale number upon Kirchhoff's scale, H that upon Mr. Huggins's scale, Kλ and Hλ the corresponding wave lengths, and Δ their difference. An examination of this table will show, I think, upon the whole, a very satisfactory correspondence. For the portion of Mr. Huggins's scale beyond G I have been obliged to rely upon my unassisted judgment for identification, as Kirchhoff's scale does not extend much farther than this point. This is therefore probably the least reliable portion of my work, though as yet I have seen no reason to doubt its accuracy. For the portion of the scale between A and C we possess very few measurements of wave lengths, and I have therefore postponed its discussion until more ample materials should be at hand. To form a correct estimate of the degree of accuracy to be obtained by interpolation in cases like the one before us, it is desirable to compare with each other the measurements of wave lengths made by different observers. To facilitate this comparison as much as possible, I have brought together in tables VI and VII all the measurements of wave lengths which I have been able to find, with the exception of a few isolated cases.

This table contains measurements by Van der Willigen,* Angström† and Ditscheiner,‡ with columns of differences for convenience of reference. Column first contains the numbers denoting the lines measured by Van der Willigen; column second, the corresponding numbers on Kirchhoff's scale; column third, Angström's measurements, reduced to millionths of a millimeter; column fourth, the measurements of Van der Willigen; column fifth, Ditscheiner's measurements, reduced for the value of D_2, given by Angström; column sixth, Ditscheiner's measurements, as given in his second paper,§ in which the absolute value of the interval between two successive

* Archives du Museè Teyler, vol. i, p. 1. † Pogg. Ann., cxxiii, p. 489.
‡ Sitzungsberichte der k. k. Akad. der Wissenschaften, Bd. l, 1864.
§ Band lii. 289.

NOTE.—I have not been able to find in any public library in this country a copy of the Annales Scientifiques de l'ecole normale supérieure à Paris, vol. iv, containing an extended memoir on wave lengths by Mascart.

Table VI.

V.D.W.	K.	Ang. λ.	V.D.W.λ.	Ditsch.λ1.	Ditsch.λ2.	A.and V.D.W.	A.andD.1.	A.andD.2.	V.D.W. and D.1.
4a	593	687·49	687·48	687·81	688·33	+0·01	−0·32	−0·84	−0·33
5	694	656·76	656·56	656·77	657·11	+0·20	−0·01	−0·35	−0·21
6	711·5	-----	651·94	652·09	652·58	---	---	---	−0·15
7	719·5	-----	649·77	650·05	650·54	---	---	---	−0·28
9	850	619·17	619·45	619·72	620·09	−0·28	−0·55	−0·92	−0·27
10	864	616·33	616·49	616·73	617·19	−0·16	−0·40	−0·86	−0·24
11	877	614·31	613·96	614·24	614·70	+0·35	+0·07	−0·39	−0·28
12	885	612·34	612·52	612·76	613·22	−0·18	−0·42	−0·88	−0·24
13	895	610·45	610·52	610·82	611·28	−0·07	−0·37	−0·83	−0·32
14a	1002·8	590·04	589·86	590·07	590·53	+0·18	−0·03	−0·49	−0·21
14γ	1006·8	589·43	589·26	589·43	589·89	+0·17	0·00	−0·46	−0·17
15	1200·4	-----	562·70	562·97	563·39	---	---	---	−0·27
16	1207	561·99	561·80	561·98	562·40	+0·19	+0·01	−0·41	−0·18
17	1280	-----	553·19	553·27	553·68	---	---	---	−0·08
18	1324·8	-----	547·86	548·13	548·54	---	---	---	−0·27
19	1343	545·97	545·83	546·05	546·46	+0·14	−0·08	−0·49	−0·22
20	1421·6	-----	537·38	537·52	537·92	---	---	---	−0·14
21	1463	533·16	533·05	533·29	533·69	+0·11	−0·13	−0·53	−0·24
22a	1523·5	527·32	527·24	527·38	527·83	+0·08	−0·06	−0·51	−0·14
23	1569·8	523·69	523·50	523·74	524·13	+0·19	−0·15	−0·44	−0·24
24	1577·5	-----	522·96	523·09	523·49	-----	-----	-----	−0·13
25	1634	518·79	518·63	518·73	519·12	+0·16	+0·03	−0·33	−0·10
26	1648·8	517·66	517·51	517·70	518·09	+0·11	−0·04	−0·43	−0·19
27β	1655·6	517·15	517·07	517·15	517·54	+0·08	0·00	−0·39	−0·08
28	1750·4	-----	510·18	510·30	510·68	-----	-----	-----	−0·12
29	1777·4	-----	508·27	508·41	508·79	-----	-----	-----	−0·14
30	1834	-----	504·37	504·55	504·93	-----	-----	-----	−0·18
31	1961	-----	496·01	496·15	496·53	-----	-----	-----	−0·14
32	2041·4	489·50	489·38	489·54	489·90	+0·12	−0·04	−0·40	−0·16
33	2067	487·55	487·46	487·55	487·91	+0·09	0·00	−0·36	−0·09
34	2080·1	486·52	486·39	486·49	486·87	+0·12	+0·03	−0·35	−0·10
35	2309	-----	467·00	467·07	467·42	-----	-----	-----	−0·07
36a	2489·4	-----	453·75	453·73	454·09	-----	-----	-----	+0·02
37	2721·6	438·63	438·58	438·75	439·08	+0·05	−0·12	−0·45	−0·17
38	2797	434·28	434·28	434·34	424·66	0·00	−0·06	−0·38	−0·06
39	2822·8	432·78	432·74	432·82	433·14	+0·04	−0·04	−0·36	−0·08
40	2854·7	431·03	431·12	431·35	431·70	−0·09	−0·32	−0·67	−0·23
H	-----	397·16	397·13	397·10	397·42	+0·03	+0·06	−0·26	+0·03
H_1	-----	393·59	393·76	393·74	394·05	−0·17	−0·15	−0·46	+0·02

lines on the ruled glass surface employed was determined. The other columns exhibit the differences between the first and second, first and third, first and fourth, and second and third series of measurements.

In table VII, I have given the measurements of the wave lengths of the eleven principal lines of Fraunhofer made by different observers. The differences, as will be remarked, are rather large, but it must be borne in mind that the breadths of the lines themselves is probably one source of error,—different observers measuring different parts of the same line.

In my second discussion of Kirchhoff's scale, I have divided the observations into twelve groups, giving weights to all the measurements of wave lengths, and assigning the probable error in the case of each group as well as the values of the con-

Table VII.

	Fraunhofer.	Ditscheiner 1.	Ditscheiner 2.	Angstrom.	v. d. Willigen.	Mascart.	Bernard.	Stefan.
A	-----	-----	-----	761·20	763·36	-----	760·6	759·8
B	687·85	687·81	688·33	687·49	687·48	686·66	686·9	687·2
C	655·63	656·66	657·11	656·77	656·56	656·07	656·1	655·8
D	-----	590·07	590·53	590·04	589·86	589·43	----	589·4
D	588·77	-----	589·89	589·43	589·26	588·82	588·8	----
E	526·51	527·42	527·83	527·38	527·24	526·79	526·8	525·3
b	-----	517·70	518·09	517·66	517·51	517·06	----	518·7
F	485·63	486·49	486·87	486·52	486·39	485·98	485·9	484·3
G	429·60	431·35	431·70	431·03	431·12	430·76	430·6	430·2
H	396·30	397·10	397·42	397·16	397·13	396·72	396·8	----
H_1	-----	393·74	394·05	393·59	393·76	-----	----	----

stants deduced for each portion of the scale. Unfortunately, several errors have crept into the table of constants; these I will correct in this place, giving the proper values in table VIII.

Table VIII.

	1	2	*a*	*b*	*c*	*d*	*e*
1	694·1	877	656·65	−26·325	+1·0490	+0·3618	------
2	877	1135·1	614·19	−17·670	−5·5610	+4·6196	−0·8913
3	1135·1	1303·5	571·47	−14·008	+1·5466	−0·3707	------
4	1303·5	1421·5	550·82	−12·842	+2·1860	−0·7367	------
5	1421·5	1577·6	537·49	−10·063	−0·6920	+0·7960	------
6	1577·6	1750·4	523·09	− 8·125	+0·8911	−0·2463	------
7	1750·4	1920·2	510·30	− 6·792	−0·1661	+0·1180	------
8	1920·2	2067·1	498·76	− 5·029	−4·2235	+1·6624	------
9	2067·1	2250·0	487·56	− 8·7225	------	------	------
10	2250·0	2547·2	471·24	− 7·048	------	------	------
11	2547·2	2721·6	449·67	− 6·154	−0·4536	+0·2028	------
12	2721·6	2869·7	438·68	− 7·028	+2·1490	+1·5740	−1·6350

By means of these constants the wave lengths of all the metallic lines upon Kirchhoff's chart have been computed. These are given in table IX.*

Table IX.

Calcium.

K	λ
717·8	650·47 (2b)
720·1	649·88 (2e)
729	647·60 (2b)
731·7	646·93 (5b)
736·9	645·60 (3b)
740·9	644·60 (5b)
840	621·81
844·5	620·90
848	619·95
849·5	619·54
855	618·54
857·5	618·03
860·2	617·50 (3d)
863·9	616·78 (5b)
884·9	612·94 (4b)
891·9	610·83
941·5	602·56
943	602·26
1029·3	585·87 (3c)
1217·8	560·69 (5d)
1219·2	560·53 (3c)
1221·6	560·24 (5d)
1224·9	559·87 (5d)
1228·3	559·42 (2d)
1229·6	559·27 (4c)
1235	558·62 (3d)
1443·5	535·25 (2b)
1522·7	527·40 (6c)
1528·7	526·88 (5c)
1530·2	526·75 (4c)
1532·5	526·55 (4b)
1533·1	526·49 (4b)
1627·2	519·24 (5b)
1832·8	504·66 (2a)
2058	488·40 (6c)
2606·5	445·93 (5c)
2638·5	443·89 (4e)
2653	442·96 (1d)
2834·2	433·01 (5e)
2855·2	431·56 (4)
2864·7	430·79 (4b)
2869·7	430·21 (5c)

Barium.

K	λ
718·7	650·24 (2)
874·3	614·78 (4b)
890·2	611·75 (1b)
903·5	609·17
934	603·08
964·5	597·05
1031·8	585·51 (2a)
1050	582·88
1083	578·51 (2a)
1274·2	553·96 (3b)
1287·5	552·40 (1c)
1371·4	542·87 (1b)
1989·5	493·78 (6c)
2031·1	490·20 (2c)
2461·2	456·36 (6b)
2502·3	453·46 (4c)
1269·5	554·52
1274·7	553·90 (3a)
1286	552·57
1301	550·83
1317	549·11
1320·6	548·68 (4c)
1539	525·98
1562	524·18
1575	523·24
1578·5	522·97
1582	522·71
2385·5	461·69
2386·6	461·62 (2a)
2858·5	431·38 (4a)
2860	431·18 (1)

Strontium.

K	λ
717·5	650·68
753·8	641·38 (3b)

Magnesium.

K	λ
1634·1	518·73 (6g)
1649	517·64 (6f)
1655·9	517·17 (4d)

* In this table the intensities of the lines are denoted on an increasing scale by the numbers 1, 2, 3, 4, 5, 6, and the breadths by the letters a, b, c, d, e, f, g, so that a denotes the least and g the greatest breadth.

TABLE IX (continued).

K	λ
2414·8	459 62 (2b)
2565	448·57 (6c)
2568	448·39 (3b)

SILVER.

K	λ
1330	547 55
1335	546·96
1600	521 32

GOLD.

K	λ
809·5	628·23 (3b)
967	596·57
969 6	595·08 (3a)
1042·5	583·95
1572	523·46
2157	479·76 (3a)

ANTIMONY.

K	λ
798·1	630·84 (3a)
824·5	624·92
881	613 50 (1a)
906·1	608·69 (2c)
919	606·10
942	601·46
957·2	598 41
994	591·61 (1b)
997·1	591·09 (2b)
1002·7	590·22
1004·8	589·76
1187·1	564·54 (2a)
1189	564·28'
1245	557·42
1247	557·19
1334	547·08 (4b)
1338	546 61
2252	471·10
2254	470·86

ARSENIC.

K	λ
860	617·54
890·5	611·69
932·5	603·38 (4b)
1179·4	578·95 (1a)
1254·5	558·29
1304·5	550·42
1458	538·75
1460 5	533·55
1600	521·32

CHROMIUM.

K	λ
1385·7	541·35 (5b)
1601·4	521·20 (6b)
1604·4	520·98 (5b)
1606·4	520·83 (5b)

COPPER.

K	λ
1081·8	578·67 (2b)
1497·3	529·30 (1a)
1588·3	522·24 (1g)
2329·5	465·64

CADMIUM.

K	λ
730·5	647·22
740·9	644·59 (5b)
1453·7	531 27
1769 5	509·00
2148	480·56
2294·5	468·10 (2b)
2667 7	441·94

MERCURY.

K	λ
869·2	537·67 (2b)
1090·5	577·54
1092·5	577·28
1157	568 47
1161	567·94 (2a)
1338	546·61
1340·5	546·33
1369	543·13
1372	542·80
{ 2777	{ 535·45 } (2)
{ 2779	{ 535·36 } (2)

LEAD.

K	λ
735	646·09
923	605·29 (2b)
924	605·09
944	601·00
1213	561·29
1263	555·29
1266	554·93
1419·5	537·71
1826	505·13
1832	504·71
2714	439·18
2716	439·07

TIN.

K	λ
736	645·83
1070	615·59
1250	556·83
1252	556·59
1748	510·55
2417	459·47
2503	453·41

IRON.

K	λ
721·1	649·62 (2b)
756·9	640·60 (5b)
798·5	630·53 (4a)
831	623·52 (4c)
849·7	619·60 (3c)
877	614·27 (4c)
912·1	607·48 (3b)
931·3	603·62 (4b)
991·9	591·95 (3b)
1096·1	576·79 (3c)
1200·6	562·83 (4b)
1207·3	561·99 (5g)
1217·8	560·71 (5d)
1231·3	559·06 (5d)
1239·9	558·01 (4a)
1242·6	557·71 (6c)
1245·6	557·35 (4d)
1337	546·72 (4d)
1343·5	545·98 (6c)
1351·1	545·12 (5d)
1352·7	544·93 (5b)
1362·9	543·80 (5b)
1367	543·34 (6d)
1372·6	542·74 (5b)
1380·5	541·89 (4c)
1384·7	541·44 (4c)
1389·4	540·93 (6c)
1390·9	540·78 (5d)
1397·5	540·07 (5c)
1401·6	539·63 (4c)
1410·5	538·68 (4c)
1421·5	537 51 (6c)
1423	537·32 (5b)
1425·4	537·11 (5b)
1428·2	536·81 (5b)
1450·8	534·50 (5c)
1451·8	534·40 (5b)
1462·8	533·26 (5c)
1463·3	533 21 (5c)
1466·8	532 96 (5c)
1473·9	532·13 (5b)
1487·7	530·75 (5b)
1506·3	528·93 (5c)
1508·6	528·76 (5b)
1522 7	527·41 (6c)
1523 7	527·32 (6c)
1527·7	526·98 (5c)
1569 6	523·63 (5c)
1577·2	523·07 (5c)
1622·3	519·62 (5c)
1623 4	519·53 (5b)
1650·3	517 55 (6b)
1653·7	517·31 (6b)
1655 6	517·16 (6c)
1662·8	516·67 (5b)
1693·8	514·47 (6e)
1701·8	513·87 (5c)
1867·1	504·33 (5d)
1961·2	496·11 (6b)
2001·6	492·76 (5c)
2005·2	492·44 (6d)
2007·2	492·27 (6c)
2041·3	489·42 (6c)
2042 2	489·35 (6b)
2058	488·14 (6c)
2066·2	487·55 (5c)
2067·1	487 49 (5c)
2082	486·42 (6a)
2670	441·88 (6e)
2686·4	440·85 (6f)
2721	438·75 (6)
2822·3	438 61 (6)
2854·7	431·61 (6)
{ 2013·5	492·73
} 2018	491·34
2136	489·88 (5a)
2240	472·25 (3b)
2291·8	468·29 (2g)

ZINC.

K	λ
771·8	636·99 (1a)
893	611·21
896	610 64
930	603·88
933	603·27
1004	589·90
1996	493·23
2000·5	492·85

COBALT.

873	615·02
884·9	612·75 (4b)
942	601·46
1319	550·11 (3c)
1346	545·70
1354·5	544·74
1424	537·25
1429·8	536·65
1433	536·32
1438·9	535·72 (4c)
1440·2	535·59 (1b)
1448·7	534·71 (2a)
1449·4	534·54 (1a)
1510·3	528·55 (2c)
1525	527·20 (1b)
1527·7	526·96 (5c)
1566·5	523·86 (2b)
1572	523·46
1596	521·63
2071·3	487·19 (1b)
2103·3	484·66 (4b)
2132·3	482·00 (2a)
2157	479·76 (3a)
2171·5	478·32 (3b)
2206 4	475·59 (1a)
2422·3	459·10 (6d)
2493·6	454·07 (5a)

PLATINUM.

K	λ
709·3	652·67
949	600·07
952·5	599·38
953·2	599·34
954·5	599·00
956·4	598·57
958·5	598·22
965·5	596·86
970·5	595·90 (1b)
972	595·62
1325·7	548·07 (1)
{ 1488·2	530·70 } (3)
{ 1489	530·62 } (3)
1576·8	523·10 (1)
{ 1806·1	506·49 } (2)
{ 1806·9	506·43 } (2)
2057	488·22 (1)
2463·4	456·19 (4b)
2540·5	450·77 (2g)
2626·3	445·65 (2a)

RUTHENIUM AND IRIDIUM.

K	λ
778·3	655·45
1348·3	545·44 (1b)
1489·9	530·52 (1a)

TABLE IX (continued).

NICKEL.

K	λ	
856·8	618·19	(2a)
857	618·10	(2a)
887·7	612·23	(2a)
891·7	611·47	(2a)
1005	589·73	(2b)
1029·3	585·87	(3c)
1324·8	548·17	(4d)
1643	518·08	(1b)
1653·7	517·30	(6b)
1672·2	515·97	(4a)
1684	515·28	(4a)
1690	514·71	(5b)
1697	514·21	(5c)
1727·3	512·05	(3b)
1748·9	510·48	(3c)
1749·6	510·43	(2d)
1775·8	508·57	(3b)
1776·5	508·52	(3c)
1842·2	504·02	(4b)
1868·4	502·25	(5b)
1920·2	498·76	(4b)
1925·8	498·50	(4b)
1987·5	493·87	(3a)
2008·1	492·19	(1b)
2025·7	490·59	(4a)
2064·7	487·66	(2c)
2073·5	487·03	(3b)
2086·9	485·97	(3b)
2112·7	483·82	(3b)
2115	483·56	(3a)
2164	479·16	(4a)
2199·2	476·21	(3a)
2249·7	471·26	(6a)
2334·1	405·31	(2d)

CERIUM.

K	λ	
1190·1	564·15	1
1249·9	556·84	1
1256·7	556·03	1
1329·1	547·65	2
1332·4	547·27	2
1336·2	546·82	1
1385·0	541·40	2
1401·7	539·62	2
1438·9	535·72	3
1460·9	533·46	1
1517·9	527·84	3
{1571·0	523·54}	1
{1572·4	523·43}	

K	λ	
1573·0	523·39	2
1623·1	519·56	1
{1629·2	519·10}	2
{1630·4	519·01}	
1683·1	515·20	1
1725·5	512·18	1
1777·5	508·45	2
{1782·4	508·12}	1
{1784·5	507·97}	
1938·8	497·68	2
2052·3	488·56	1
2221·5	474·22	1

DIDYMIUM.

K	λ	
1225	559·83	2
1230	559·22	1
{1364·5	544·62}	1
{1365·2	544·55}	
1431·9	536·44	1
1471·1	532·42	1
{1518·6	527·78}	1
{1519·4	527·71}	
1536	526·24	1
1541·4	525·79	1
1548·9	525·18	2
1567·5	523·79	1
1709·2	513·35	2

LANTHANUM.

K	λ	
{1411·6	538·56}	2
{1412·8	538·43}	
1416·8	538·00	2
1451	534·48	1
1606·8	520·80	2
1627·9	519·20	2
1634·8	518·69	2
2136·8	481·59	1

LANTHANUM AND DIDYMIUM.

K	λ	
1025	586·52	1
1064·5	580·97	1
1066·1	580·72	1
1071·1	580·06	1
1075·6	579·47	1
1077	579·28	1
1092·1	577·33	2

K	λ	
1302	550·74	1
1303·4	550·60	2
1317·6	549·05	1
1345·4	544·62	1
{1486·8	530·84}	2
{1489·2	530·60}	
1622·3	519·62	1
1623·3	519·55	1
1716·6	512·82	2
1728·8	511·95	2
{1894·5	500·52}	2
{1895·2	500·48}	
1903	499·97	1
1940·2	497·60	1
{1988·6	493·86}	1
{1989·5	493·78}	
2003·8	492·56	2
2004·7	492·49	2
2031	490·25	2
2081	486·49	2
2121·4	483·02	1
2208·2	475·44	2
2214·5	474·87	2
2217·8	474·57	2
2646·2	443·39	

ATMOSPHERIC LINES.

K	λ
711·4	652·13
948	600·27
949·4	599·99
949·8	599·91
951·7	599·54
954·2	599·05
958·8	598·15
959·6	598·00
961·9	597·56
963·7	597·20
964·4	597·06
965·7	596·81
968·7	596·22
969	596·18
969·6	596·06
970·5	595·90
972·1	595·60
974·3	595·18
975	595·05
975·7	594·93
976·1	594·85
977·4	594·60
977·7	594·54
982·0	593·75
982·3	593·70
988·9	592·51
989·2	592·45
989·6	592·38
993·1	592·77
993·4	592·73
998·1	590·90
999·2	590·70
1000	590·57
1001·4	590·33
1005·8	589·60
1008·3	589·17
1009·2	589·02
1010·5	588·81
1013·9	588·26
1015·1	588·07
1016·4	587·86
1017·7	587·65
1018·2	587·57

PALLADIUM.

K	λ	
1114·7	574·36	1
1146·2	569·94	2
1164·9	567·42	2
1185·6	564·66	1
1264·6	555·10	2
1269	554·58	2
1279·1	553·39	1
{1400	539·80}	2
{1407	539·06}	
1430·1	536·62	1
1447·0	534·89	1
1477·0	531·82	1
1495·2	530·01	3
1540	525·90	1
{1566·5	523·86}	2
{1567·1	523·82}	
1601·4	521·21	1
{1660	516·85}	3
{1660·7	516·80}	
1732·9	511·65	2
1801·9	506·78	1
2062	487·76	2
2123·6	482·81	2
2162	479·33	2

As it is important to be able to judge of the probable error in the determination of each of these lines, I reprint, for the sake of convenience, the table of data employed and differences obtained given in my paper on the determination of wave lengths by the method of comparison. In this reprint, two or three trifling errors have been corrected.

I have already called attention to the very large probable error (±0·66) in the tenth group of observations discussed. That it may not be supposed that I have too hastily adopted a straight line as most nearly representing the data assumed, I will here give the successive orders of differences corresponding to parabolas of the 2d, 3d, 4th and 5th degrees, which I have endeavored to draw through the same points.

Table X.*

N	W	K	λ	Δ	ε
1	9	694·1	656·65	0·00	
2	2	711·4	652·07	−0·06	
3	2	719·6	650·03	+0·02	
4	1	783·8	634·23	+0·08	
5	4	831·0	623·59	+0·08	
6	4	849·7	619·60	0·00	
7	7	860·2	617·48	0·00	
8	8	863·9	616·71	−0·04	
9	6	874·3	614·72	0·00	
10	9	877·0	614·23	0·00	+0·04
10	9	877·0	614·23	+0·05	
11	7	884·9	612·75	0·00	
12	7	894·9	610·80	−0·06	
13	2	958·8	598·12	−0·02	
14	11	1002·8	590·07	−0·05	
15	18	1006·8	589·43	−0·02	
16	4	1019·3	586·25	+0·35	
17	3	1096·1	576·71	−0·13	
18	3	1102·9	575·77	−0·18	
19	8	1135·1	571·49	+0·06	+0·13
19	8	1135·1	571·49	0·00	
20	2	1155·7	568·69	+0·02	
21	10	1174·2	566·32	+0·09	
22	5	1200·6	562·95	+0·07	
23	16	1207·3	561·96	−0·08	
24	9	1217·8	560·75	0·00	
25	8	123·13	559·12	0·00	
26	7	1242·6	557·76	0·00	
27	10	1280·0	553·25	−0·06	
28	1	1303·5	551·11	+0·59	±0·16
28	1	1303·5	551·11	+0·28	
29	2	1306·7	550·68	+0·26	
30	12	1324·8	548·11	−0·07	
31	3	1337·0	546·76	+0·02	
32	8	1343·5	546·03	+0·04	
33	5	1351·1	545·06	−0·07	
34	10	1367·0	543·40	+0·04	
35	11	1389·4	540·90	−0·04	
36	1	1410·5	538·77	+0·09	
37	9	1421·5	537·50	0·00	±0·12
37	9	1421·5	537·50	0·00	
38	9	1450·8	534·49	−0·02	
39	18	1463·3	533·27	+0·04	
40	6	1492·4	530·21	−0·09	
41	6	1506·3	528·77	−0·18	
42	7	1515·5	527·99	+0·10	
43	9	1523·7	527·42	+0·07	
44	2	1541·9	525·97	+0·20	
45	8	1569·6	523·72	+0·05	
46	8	1577·6	523·08	−0·05	±0·08
46	8	1577·6	523·08	−0·01	
47	1	1589·1	521·96	−0·21	
48	6	1601·7	521·31	+0·12	
49	4	1622·3	519·65	+0·03	
50	18	1634·1	518·71	−0·03	
51	14	1648·8	517·68	+0·01	
52	13	1655·6	517·13	−0·05	
53	6	1693·8	514·63	+0·16	
54	4	1737·7	511·41	+0·05	
55	4	1750·4	510·28	−0·16	±0·10
55	4	1750·4	510·28	−0·02	
56	6	1777·5	508·39	−0·02	
57	5	1799·0	506·89	−0·04	
58	13	1834·3	504·54	+0·03	
59	5	1854·9	503·24	+0·12	
60	5	1867·1	502·25	−0·05	
61	2	1873·4	501·66	−0·23	
62	7	1885·8	501·05	−0·01	
63	4	1908·5	499·70	+0·12	
64	6	1920·2	498·76	−0·07	±0·08
64	6	1920·2	498·76	0·00	
65	17	1960·8	496·14	+0·01	
66	2	1975·7	495·03	+0·08	
67	7	1983·3	494·33	0·00	
68	6	1989·5	493·75	−0·05	
69	7	2005·2	492·31	−0·15	
70	3	2018·5	491·39	+0·07	
71	9	2041·3	489·52	+0·09	
72	2	2058·0	488·16	0·00	
73	9	2067·1	487·53	0·00	±0·06
73	9	2067·1	487·53	−0·03	
74	21	2080·0	486·49	+0·06	
75	1	2103·3	484·62	+0·32	
76	8	2119·8	482·80	−0·16	
77	1	2148·9	480·54	+0·23	
78	1	2157·4	479·53	−0·03	
79	3	2160·6	479·21	−0·19	
80	6	2187·1	476·92	−0·18	
81	3	2201·9	475·89	+0·09	
82	2	2221·7	474·33	+0·22	
83	5	2233·5	473·35	+0·30	
84	2	2250·0	471·24	−0·38	±0·14
84	2	2250·0	471·24	+0·02	
85	4	2264·3	470·69	+0·05	
86	12	2309·0	467·06	−0·02	
87	5	2416·0	460·61	+1·06	
88	5	2436·5	458·65	+0·34	
89	3	2457·5	456·80	+0·18	
90	6	2467·6	455·70	−0·20	
91	8	2489·4	453·71	−0·66	
92	2	2537·1	450·54	−0·46	
93	1	2547·2	450·12	−0·17	±0·66
93	1	2547·2	450·12	+0·44	
94	9	2566·3	448·45	−0·05	
95	7	2606·6	445·99	+0·05	
96	4	2627·0	444·63	0·00	
97	7	2638·6	443·83	−0·05	
98	8	2670·0	441·87	−0·01	
99	7	2686·6	440·86	+0·01	
100	12	2721·6	438·74	0·00	±0·15
100	12	2721·6	438·74	+0·15	
101	7	2734·9	437·79	+0·09	
102	8	2775·7	435·66	+0·14	
103	11	2797·0	434·32	−0·33	
104	11	2822·8	433·80	+0·23	
105	17	2854·7	431·34	−0·25	
106	7	2869·7	430·36	+0·26	±0·24

* In this table the column W gives the weights for the different wave lengths.

$\Delta^2\lambda$	$\Delta^3\lambda$	$\Delta^4\lambda$	$\Delta^5\lambda$
+0·02	+0·48	−0·46	−1·04
+0·05	+0·34	−0·13	−0·24
−0·02	−0·19	−0·12	+0·25
+1·06	+0·69	+0·57	+0·19
+0·34	+0·04	−0·17	−0·39
+0·18	+0·04	−0·21	−0·13
−0·20	−0·25	−0·47	−0·27
−0·66	−0·48	−0·51	−0·12
−0·46	+0·38	+1·58	+1·27
−0·17	−0·85	+2·41	+1·46

These differences are sufficient to show that a straight line gives, upon the whole, the best representation of the observations, but that in all probability the form of the function to be assumed for the purpose of interpolation is not parabolic. The same remark applies, though in a much less degree, to the ninth group. In this case the successive differences are as follows:—

$\Delta^2\lambda$	$\Delta^3\lambda$	$\Delta^4\lambda$	$\Delta^5\lambda$
−0·03	+0·04	+0·13	+0·25
+0·06	+0·08	+0·07	0·00
+0·32	+0·25	+0·17	+0·12
−0·16	−0·27	−0·36	−0·29
+0·23	+0·09	+0·08	+0·25
−0·03	−0·16	−0·15	−0·01
−0·19	−0·32	−0·30	−0·17
−0·18	−0·27	−0·19	−0·27
+0·09	+0·06	+0·14	−0·09
+0·22	+0·28	+0·30	+0·10
+0·30	+0·43	+0·37	+0·31
−0·38	−0·15	−0·38	+0·20

In computing the wave lengths of the lines on Kirchhoff's scale between the scale numbers 2067·1 and 2250·0, a parabola of the 5th order was employed, because this distributes the errors rather more evenly than the straight line the constants for which are given for group 9 in table VIII. The probable error is about the same in both cases.

To complete my work, it remains for me to show to what extent the wave lengths calculated by the two wholly independent processes of interpolation, with different data, agree with each other. Table XI contains all the lines for which the wave lengths can be determined for both scales.

In this table columns K and H give the scale numbers upon Kirchhoff's and Huggins's scales, respectively; columns Kλ and Hλ the corresponding wave lengths, and column Δ, their differences. An impartial examination of this table—which of course includes table V—will show, I think, as close an agreement as can be reasonably expected. The differences between the wave

Table XI.

Calcium.

K	H	Kλ	Hλ	Δ
717·8	622	650·47	650·34	+0·13
720·1	625	649·88	649·77	+0·11
729	637	647·60	647·47	+0·13
731·7	642	946·93	646·51	+0·42
736·9	649	645·60	645·17	+0·43
740·9	655	644·60	944·03	+0·57
860·2	813	617·50	617·51	−0·01
863·9	818	616·78	616·67	+0·11
884·9	843	612·94	612·78	+0·16
894·9	859	610·83	611·06	−0·23
1029·3	1031	585·87	586·05	−0·18
1219·2	1247	560·53	560·65	−0·12
1221·6	1249	560·24	560·39	−0·15
1228·3	1258·5	559·42	559·42	0·00
1229·5	1260	559·27	559·27	0·00
1235	1265	558·62	558·59	−0·03
1443·5	1506	535·25	535·45	−0·20
1522·7	1599·5	527·40	527·36	+0·04
1528·7	1605	526·88	526·93	−0·05
1532·5	1609	526·55	526·61	−0·06
1627·2	1702	519·24	519·24	0·00
1832·8	1907	504·66	504·40	+0·26
2606·5	3124 ?	445·93	455·49	+0·44
2638·5	3181	443·89	443·64	+0·25
2653	3212	442·96	442·66	+0·30
2855·2	3561 ?	431·56	431·94	−0·38
2864·7	3602·5	430·79	430·79	0·00
2869·7	3617	430·21	430·36	−0·15

Barium.

K	H	Kλ	Hλ	Δ
718·7	621·5	650·24	650·44	−0·20
890·2	847	611·75	612·15	−0·40
934	908	603·08	602·70	+0·38
964·5	943	597·05	597·58	+0·53
1031·8	1034	585·51	585·67	−0·16
1050	1067	582·88	582·77	+0·11
1083	1096	578·51	578·00	+0·51
1274·2	1308	553·95	534·06	−0·11
1287·5	1327	552·40	532·06	+0·34
1989·5	2075	493·78	493·57	+0·21
2031·1	2133	490·20	490·23	−0·03

Mercury.

K	H	Kλ	Hλ	Δ
1157	1177	568·47	568·55	−0·08
1340·5	1385	546·33	546·13	+0·20
1372	1421	542·80	542·80	0·00

Strontium.

K	H	Kλ	Hλ	Δ
763·8	669	641·38	641·39	−0·01
1274·7	1311	553·90	553·74	+0·16
1286	1324	552·57	552·38	+0·19
1301	1341	550·83	550·61	+0·22
1317	1349 ?	549·11	549·78	−0·67
1320·6	1359	548·68	548·75	−0·07

K	H	Kλ	Hλ	Δ
1539	1617	525·98	525·95	+0·03
1562	1638	524·18	524·26	−0·08
1575	1651	523·24	523·23	+0·01
1578·5	1656	522·97	522·83	+0·14
1582	1659	522·71	522·60	+0·11

Silver.

K	H	Kλ	Hλ	Δ
1330	1372	547·55	547·44	+0·11
1335	1380	546·96	546·63	+0·33
1600	1675	521·32	521·34	−0·02

Gold.

K	H	Kλ	Hλ	Δ
967	951	596·57	596·45	+0·12
969·5	956	596·08	595·75	+0·33
1042·5	1045	583·95	583·38	+0·57
1572	1647	523·46	523·56	−0·10
2151	2326	479·76	479·64	+0·12

Antimony.

K	H	Kλ	Hλ	Δ
798	729	630·84	630·49	+0·35
881	837	613·50	613·73	−0·23
957·5	937·5	598·41	598·72	−0·31
994	988·5	591·61	591·45	+0·16
1004·8	1005	589·76	589·76	0·00
1187	1214	564·54	564·41	+0·13
1247	1279	557·19	557·18	+0·01
1338	1383	546·61	546·33	+0·28
2252	2488	471·10	471·03	+0·07

Arsenic.

K	H	Kλ	Hλ	Δ
860	812	617·54	617·67	−0·13
890·5	850	611·69	611·67	+0·02
1179·5	1090	578·95	578·73	+0·22
1460·5	1529	533·55	533·41	+0·14

Cadmium.

K	H	Kλ	Hλ	Δ
730·5	639	647·22	647·08	+0·14
1453	1517	534·27	534·47	−0·20
1769·5	1843	509·00	508·83	+0·17
2148	2315	480·56	480·27	+0·29
2667·7	3239	441·94	441·81	+0·13

Tin.

K	H	Kλ	Hλ	Δ
736	648	645·83	645·27	+0·56
1748	1821	510·55	510·40	+0·15

Iron.

K	H	Kλ	Hλ	Δ
756·9	673	640·60	640·64	−0·04
831	772	623·52	623·38	+0·14
849·7	795	619·60	619·92	−0·32
1207·3	1236	561·99	561·93	+0·06
1217·8	1247	560·71	560·69	+0·02

TABLE XI (continued).

K	H	Kλ	Hλ	Δ
1231·3	1261	559·06	559·14	−0·08
1239·9	1276?	558·01	557·49	−0·48
1352·7	1400	544·93	544·64	+0·29
1367	1413	543·34	543·37	−0·03
1372·6	1421·5	542·74	542·78	−0·04
1380·5	1434	541·89	541·73	+0·16
1384·7	1438	541·44	541·39	+0·05
1390·9	1445	540·78	540·80	−0·02
1397·5	1456	540·07	539·86	+0·21
1401·6	1459·5	539·63	539·55	+0·08
1421·5	1481	537·51	537·67	−0·16
1423	1485·3	537·32	537·28	+0·04
1425·4	1486·5	537·11	537·18	−0·07
1527·7	1603	526·98	527·09	−0·11
1569·6	1645	523·63	523·70	−0·07
1577·2	1653	523·07	523·09	−0·02
1622·3	1696	519·62	519·70	−0·08
1623·4	1698	519·53	519·55	−0·02
1653·7	1728	517·31	517·25	+0·06
1655·6	1731	517·16	517·03	+0·13
1693·3	1767	514·47	514·33	+0·14
1701·8	1775	513·87	513·74	+0·13
1961	2036	496·11	496·05	+0·06
2001·6	2092	492·76	492·65	+0·11
2007·2	2098	492·27	492·30	−0·03
2041·3	2147	489·42	489·47	−0·05
2886·4	3272	440·85	440·81	+0·04
2822·3	3341	438·61	438·55	+0·06

COBALT.

K	H	Kλ	Hλ	Δ
889·9	844	612·75	612·62	+0·13
1354·5	1401	544·74	544·54	+0·20
1424	1483	537·25	537·50	−0·25
1429·8	1491	536·65	536·79	−0·14
1433	1496	536·32	536·34	−0·02
1438·9	1500·5	535·72	535·75	−0·03
1440·2	1501·5	535·59	535·66	−0·07
1448·7	1514	534·71	534·74	−0·03
1510·3	1584	528·55	528·60	−0·05
1525	1602	527·20	527·17	0·03
1527·7	1604	526·96	527·01	−0·05
1566·5	1642	523·86	523·94	−0·08
1572	1650	523·46	523·31	+0·15
1596	1670	521·63	521·73	−0·10
2071·3	2186	487·19	487·29	−0·10
2103	2236	484·66	484·48	+0·18
2132·3	2286	482·00	481·80	+0·20
2157	2325	479·76	479·70	+0·06

ZINC.

K	H	Kλ	Hλ	Δ
771·8	696	636·99	637·37	−0·38
896	855	610·64	610·89	−0·25
1004	1001	589·90	589·90	0·00
2240	2469	472·25	471·98	+0·27

LEAD.

K	H	Kλ	Hλ	Δ
1213	1240	561·29	561·46	−0·17
1419·5	1479	537·71	537·85	−0·14
2716	329	439·07	438·93	+0·14

lengths on the two scales are due to several causes, which it may be well to analyze. In the first place, the errors for any one line may be in opposite directions, so that the difference Δ, gives their sum. For the same reason, a close coincidence in the values of the wave lengths of the same line may sometimes be due to errors which are in the same direction and which nearly balance each other. Too much importance, therefore, must not be attached either to absolute coincidence in value, or to large differences. Secondly, the measurements will, other things being equal, be most accurate in the case of the lines which are narrowest and most sharply defined, while those which are broad, or nebulous, will give comparatively large errors from the difficulty of determining their true positions upon the scale. Finally, it is to be remarked that a large number of the lines, the wave lengths of which have been determined by interpolation from Kirchhoff's scale, have been, from necessity, taken from the chart, as they are not entered in the accompanying tables. A new and by no means insignificant source of error is thus introduced. In estimating the degree of reliance to be placed upon wave lengths determined by interpolation, it must be borne in mind that each wave length is calculated by means of a formula, the constants of which are determined from a number of measured wave lengths, which is of course a material advantage. Part of the irregularity of certain portions of Kirchhoff's scale is undoubtedly due to variations in the positions of the prisms. In addition, however, it is possible that the lines of which the wave lengths were measured by Ditscheiner were not always correctly identified with lines upon Kirchhoff's scale. After the most careful study of the whole subject, I am disposed to give the preference to the values of the wave lengths as determined by my discussion of Mr. Huggins's measurements in connection with those of Angström. With the materials obtained as above I have endeavored to determine whether the distribution of the lines corresponding to any one element is subject to any definite law. The solution of this problem was first attempted by Mr. Hinrichs,* who, from data in my judgment far too limited in number, drew the conclusion that the spectral lines in the case of any element are distributed in groups, the lines belonging to any one group being equidistant. This would probably bring the whole subject within the reach of Wrede's theory of absorption,† which is a special application of the principle of interferences. My own study of the subject does not justify the conclusion to which Mr. Hinrichs arrived. Even a cursory examination of the tables of wave lengths which I have given above will serve to show that, in very numerous instances, the distances between two successive spectral lines of the same element is less than the probable error in measuring the wave lengths. As the differences in wave length measure the distances between the lines, the element of uncertainty becomes much too large to permit us to reason with safety upon the data now at our disposal. In the mean time it cannot, I think, be denied that the success with which Wrede's theory explains the absorption bands in nitrous acid and other vapors, gives a certain probability to Mr. Hinrichs's views.

My grateful acknowledgments are again due to Mr. S. P. Sharples, by whom I have been assisted in the whole of my laborious work, with a zeal and skill which demand the fullest recognition.

Cambridge, Dec. 23d, 1868.

P. S. In a memoir presented to the Royal Society, March 2d, 1867, Mr. Airy has given a complete reduction of the numbers upon Kirchhoff's scale to the corresponding values in wave lengths. The high and well deserved reputation of the Astron-

* This Journal, II, vol. xlii, p. 350. † Pogg. Annalen, xxxiii.

omer Royal, lends to whatever comes from his pen a more than ordinary interest and value. Those therefore who like myself have felt convinced that an explanation of the physical cause of the spectral lines of the elements could only be sought in an accurate knowledge of the wave lengths of the lines themselves, will eagerly greet the appearance of Mr. Airy's paper. Under these circumstances it becomes desirable to submit the memoir in question to a critical examination and to determine to what extent it furnishes materials for further investigation.

Mr. Airy's method is simple. Taking the original measures of Fraunhofer for the lines C, D, E, F and G, as the basis of computation, and assuming that the relation between the wave lengths and the numbers upon Kirchhoff's scale can be expressed for the whole scale from C to G by a single function of the form*

$$f_1 = a + bk + ck^2 + dk^3 + ek^4$$

he forms five equations, the solution of which by the method of least squares gives the numerical values of the constants a, b, c, d and e. By means of these constants Mr. Airy constructs a table giving the values of the scale-numbers for every ten units of Kirchhoff's scale, from A to G. The portion of this table between A and C is constructed by extrapolation, the data for A and B not being introduced. I shall not dispute the legitimacy of this process with so great an authority as Mr. Airy; it seems to me unsound in principle and therefore unsafe in practice. After the completion of this part of his work, Mr. Airy became acquainted with Angström's memoir, and with the two papers of Ditscheiner on the wave lengths of the spectral lines upon Kirchhoff's chart. He then decided to base his reduction upon the second series of wave lengths as given by Ditscheiner. In place however of beginning *ab initio* with the new data, Mr. Airy preferred to introduce corrections into the results already obtained, adding also a value for the wave length of Fraunhofer's line B. For the method of making these corrections I must refer to the original paper. They are applied directly and only to the lines B, C, D, E, F and G. The corrected values of the wave lengths of these six lines being obtained, the wave lengths in the table above referred to were then corrected. With these corrections a curve was drawn and the corrections for every 0·01 of k were obtained graphically. Finally, the corrections for all the individual lines were interpolated numerically and applied separately to each computed wave length.

From this it appears that out of the 107 absolute measurements of wave lengths made by Ditscheiner, and by him refer-

* $k = \frac{1}{1000}$Kl and $f_1 = 100000000 \times$Frl.

red to lines upon Kirchhoff's scale, Mr. Airy, in his interpolation, employed only six, the intervals between these amounting respectively to 101·4, 308·7, 520·9, 556·3, 774·7, scale divisions. Through these six points Mr. Airy endeavors to draw a parabola of the 5th order. He then employs the 101 other lines measured by Ditscheiner only to compare with the numerical results given by his single formula.

As Mr. Airy has quietly ignored the existence of my reduction of Kirchhoff's scale read before the National Academy of Sciences in August, 1866, and published in abstract in this Journal for January, 1867, I naturally inferred that his work would prove so much more perfect than my own as wholly and justly to supersede it. The following comparative tables of differences will show, I think, that this is not the case. For the sake of completeness I add a column of differences obtained in my second reduction of Kirchhoff's scale, in which Cauchy's method of interpolation was used, and weights were given to the observations.

TABLE XII.

Airy.	Gibbs 1.	Gibbs 2.	Airy.	Gibbs 1.	
+0·25	+0·03	−0·06	+2·01	+0·01	−0·05
+0·43	+0·24	+0·02	+1·87	−0·18	−0·23
+0·69	−0·02	+0·08	+2·12	+0·02	−0·01
+0·90	−0·08	+0·08	+2·32	+0·07	+0·12
+0·94	−0·15	0·00	+2·13	−0·11	−0·07
+1·03	+0·06	0·00	+2·21	+0·11	+0·01
+1·02	−0·02	−0·04	+2·04	+0·04	+0·08
+1·15	−0·06	0·00	+1·82	−0·14	0·00
+1·22	−0·04	+0·05	+1·62	+0·04	−0·05
+1·31	+0·04	0·00	+1·00	−0·59	−0·15
+1·38	+0·09	−0·06	+1·10	+0·04	+0·07
+0·58	−0·26	−0·02	+0·67	+0·04	+0·09
0·00	+0·05	−0·05	+0·34	+0·02	0·00
+0·01	+0·10	−0·02	+0·27	+0·03	−0·03
+0·44	+0·09	+0·35	−0·02	+0·06	+0·06
+1·00	−0·06	−0·13	−0·53	+0·05	+0·32
+1·04	−0·04	−0·18	−1·39	+0·01	−0·16
+1·25	+0·20	+0·06	−1·90	−0·04	+0·23
+1·23	+0·17	+0·02	−2·46	0·00	−0·03
+1·26	+0·06	+0·09	−2·60	0·00	−0·19
+1·29	+0·15	+0·07	−3·52	+0·49	−0·18
+1·11	−0·01	−0·08	−3·57	+0·12	+0·09
+1·22	+0·10	0·00	−4·01	−0·03	+0·22
+1·22	+0·32	0·00	−4·32	−0·01	+0·30
+1·23	+0·03	0·00	−5·51	−0·26	−0·38
+1·09	−0·03	−0·06	−5·32	−0·08	+0·05
+1·66	+0·25	+0·59	−6·43	−0·20	−0·02
+1·53	+0·09	+0·26	−6·83	+0·01	+1·06
+0·97	−0·05	−0·07	−7·59	−0·85	+0·34
+0·93	+0·05	+0·02	−8·23	+0·10	+0·18
+0·90	+0·09	+0·04	−8·66	−0·14	−0·20
+0·72	+0·10	−0·07	−9·30	+0·38	−0·66
+0·73	0·00	+0·04	−9·53	−0·04	−0·46
+0·56	+0·02	−0·04	−9·30	+0·20	−0·17
+0·57	+0·01	+0·09	−9·65	−0·18	−0·05

TABLE XII (continued).

Airy.	Gibbs 1.	Gibbs 2.	Airy.	Gibbs 1.	Gibbs 2.
+0·43	−0·05	0·00	−9·22	+0·03	+0·05
+0·28	−0·07	−0·02	−9 10	+0·01	0·00
+0·24	0 00	+0·04	−9·01	0·00	−0 05
−0·08	−0·06	−0·09	−8 52	+0·54	−0·01
−0·22	−0·16	−0·18	−8·12	+0·12	+0·01
−0 14	−0·06	+0·10	−7 14	+0·04	0·00
+0·01	+0·08	+0·07	−6·82	−0·22	+0·09
+0 20	+0·18	+0·20	−4·77	+0·11	+0·14
+0·38	+0·26	+0·05	−3·91	−0·04	−0·33
+0·41	+0·21	−0·05	−2·50	−0·04	+0·23
+0·28	−0·05	−0·21	+0·10	−0·57	−0 25
+0·70	+0·25	+0·12	+1·06	+0 21	+0·26
+0·81	+0·09	+0·03			
+0·80	−0 03	−0 03			
+1·04	−0 03	+0·01			
+0 07	−0·11	−0·05			
+1·60	+0·16	+0·16			
+1·78	+0 15	+0·05			
+1·62	−0·12	−0·02			
+1·50	−0·01	−0·03			
+1·83	+0·04	−0·04			
+1·98	+0·05	+0·03			
+2·14	+0·14	+0·12			

In computing the differences between observation and calculation given in this table, it must be borne in mind that Mr. Airy's results are compared with Ditscheiner's second series of wave lengths, while my own are compared with the first, as stated in my paper above referred to.* The greatest difference given by Mr. Airy's formula amounts to not less than 9·65 units, his unit being made to correspond with mine, by placing the decimal point six figures to the right. Now, in the part of Kirchhoff's scale in which this difference occurs, the uncertainty in the position on the scale of the line in question becomes, for a difference of 9·65, not less than 150 scale divisions. One other remark will be sufficient. In all processes of interpolation which are equivalent to drawing curves through points, a certain uniformity of distribution of the + and − signs, or in other words of the excesses and deficiencies given by the formula, is necessary. Mr. Airy's formula, as will be seen by a single glance at the signs in the column of differences, violates even this elementary principle.

It is not an agreeable task to point out defects in the work of another, but in the interest of science I am obliged to declare, that for all the purposes of physical investigation, Mr. Airy's paper, in its present form, has no value whatever.

In the proceedings of the Royal Society, vol. xvii, p. 1, Mr. G. J. Stoney has given, in connection with a very interesting and suggestive paper on the physical constitution of the sun and stars, a table of wave lengths for certain lines upon Kirchhoff's scale. This table is so arranged as to give the scale

* See Table VI.

numbers corresponding to equal intervals expressed in wave lengths, and is based upon Angström's measurements alone. For the sake of completeness, I give this table in full, together with the wave lengths for the same lines, as found by myself by Cauchy's method of interpolation, and the differences. For the sake of comparison I have changed the position of the decimal point in the wave lengths determined by Mr. Stoney, so as to correspond with my own results.

TABLE XIII.

K	Sλ	Gλ	Δ	K	Sλ	Gλ	Δ
719·1 ?	650	650·14	—·14	1527·9	527	526·94	+·06
758·5 ?	640	640·22	—·22	1540·2	526	525·88	+·12
800 ?	630	630·39	—·39	1552·8	525	524·87	+·13
845·5	620	620·71	—·71	1565·5	524	523·96	+·04
855	619	618·54	+·46	1578·3	523	522·97	+·03
860·6	617	617·42	—·42	1591·2	522	522·01	—·01
865·7	616	616·50	—·50	1604·3	521	520·99	+·01
870·9	615	615·40	—·40	1617·5	520	519·98	+·02
876·1	614	614·44	—·44	1630·8	519	518·99	+·01
881·3	613	613·46	—·46	1644·3	518	517·99	+·01
886·6	612	612·44	—·44	1658·3	517	516·97	+·03
891·9	611	611·43	—·43	1673	516	515·92	+·08
897·2	610	609·61	+·39	1762 ?	510	509·52	+·48
948 ?	600	600·26	—·26	1913 ?	500	499·30	+·70
1003	590	590·07	—·07	1950·8	497	496·87	+·13
1009·7	589	589·00	·00	1962·5	496	496·00	·00
1070 ?	580	580·20	—·20	1974·3	495	495·05	—·05
1144 ?	570	570·25	—·25	1986·1	494	494·07	—·07
1199·3	563	562·98	+·02	1998	493	493·06	—·06
1207·4	562	561·98	+·02	2010·2	492	492·01	—·01
1215·6	561	560·97	+·03	2022·6	491	490·95	+·05
1223·8	560	559·97	+·03	2035·2	490	489·91	+·09
1232	559	558·98	+·02	2047·9	489	488·89	+·11
1240·2	558	558·00	·00	2060·8	488	487·95	+·05
1248·4	557	557·01	—·01	2073·7	487	487·03	—·03
1309	550	550·13	—·13	2086·7	486	487·15	—·15
1334·7	547	547	·00	2099·8	485	485·04	—·04
1343·3	546	546	·00	2164 ?	480	479·34	+·66
1352·1	545	545·01	—·01	2292·5 ?	470	468·25	+1·75
1361·2	544	544	·00	2422 ?	460	459·12	+·88
1370·5	543	542·97	+·03	2553·2 ?	450	449·72 ?	+·28
1379·7	542	541·97	+·03	2651·5	443	443·06	—·06
1389	541	540·98	+·02	2667·6	442	442·03	—·03
1398·3	540	539·98	+·02	2683·7	441	441·02	—·02
1407·7	539	538·98	+·02	2699·6	440	440	·00
1417·2	538	537·96	+·04	2715·7	439	439·07	—·07
1426·6	537	536·98	+·02	2732	438	437·95	+·05
1436·1	536	536·01	—·01	2748·8	437	436·89	+·11
1445·6	535	535·14	—·14	2766	436	435·95	+·05
1454·4	534	534·13	—·13	2783·4	435	435·17	—·17
1464	533	533·14	—·14	2801·1	434	434·46	—·46
1473·7	532	532·15	—·15	2819	433	433·77	—·77
1483·8	531	531·13	—·13	2837	432	432·90	—·90
1494·1	530	530·12	—·12	2855	431	431·59	—·59
1504·8	529	529·12	—·12	2873·1	430	429·87	+·13
1516·1	528	528·02	—·02				

Mr. Stoney does not state what method he employed in interpolating; the correspondence between his values and my own is in general very close, though I employed the data obtained by Ditscheiner, in place of those of Angström.*

Cambridge, Jan. 1st, 1869.

ART. XVI.—*On the condition of our knowledge of the Processes in Luminous Flames;* by EUGENE W. HILGARD.

FOR more than half a century after Sir Humphrey Davy's important investigations of the subject of flames, the experiments and explanations of that eminent philosopher have passed, unchallenged and almost unchanged, into text-books and lectures. As regards luminous flames especially, he established the necessity of the presence of a solid incandescent body to produce useful luminosity; and in reference to the flames of hydrocarbons in particular, he suggested that the liberation of carbon in them was owing to the combustion of the hydrogen of the compound in advance of its carbon, the latter being heated to incandescence by an oxhydrogen flame, as it were, and failing to be consumed until all the hydrogen was first oxydized.

It is remarkable that an explanation so directly at variance with the daily experience of blacksmiths, and with a lecture experiment performed even in the most elementary course of chemistry, could so long have passed current; for the decomposition of steam by ignited charcoal into hydrogen and carbonic oxyd gases is an old observation. Nay, most of us who have performed this experiment on the lecture table, have been in the habit of passing the gas through lime water or potash lye, to free it from *carbonic acid* before showing its properties. And yet we were taught, and have ourselves taught, that in the flame, hydrogen burnt first and carbon afterward, and any doubt on the subject was quieted by a reflection about the influence of "chemical mass" on affinity.

When, in 1852, I was a student in the laboratory at Heidelberg, to which Bunsen had then but just been called, the latter suggested to me as an interesting subject of investigation, the composition of the gases in the various portions of the flame; stating at the same time that the prevalent opinion concerning

* It is proper to state here, that since this paper was read before the National Academy of Sciences, all the calculations have been revised and many corrections and alterations have been introduced. The original title was, "On the determination of wave lengths by the method of comparison," and a portion of the memoir has already appeared with this title. This Journal for May, 1868.

the respective affinities of hydrogen and carbon for oxygen were certainly erroneous—that, when mixtures of carbonic oxyd and hydrogen were exploded with oxygen in the eudiometer, the distribution of the latter took place according to some law dependent upon the relative proportions between the combustible gases, and also the amount of oxygen present, but discriminating widely in favor of carbon. The experiments upon which this opinion was based, are probably those given in his "Gasometric Methods" at the conclusion of the chapter on the combustion of gases.

In order to study the processes taking place in the flame, it became necessary to investigate first of all, the composition of the gases in the interior cone, from which the flame derives its substance. I constructed a lamp adapted to the introduction of a suction tube into the flame from below, and made a series of fifteen analyses of the gases so collected at various points of the interior cone of the flames of beef tallow, and of wax. The low temperature known to prevail in this portion of the flame rendered it least adapted to elucidating the mooted question of affinities; but being the generator from which the other portions of the flame are supplied, a knowledge of its component gases was indispensable. My analysis (published in Liebig's Annalen der Chemie und Pharmacie, vol. xcii, p. 129, 1854) showed, however, the existence even in the highest portion of the cone, of free hydrogen with a large excess of carbonic oxyd and carbonic acid; the amount of hydrogen varying but little from base to point, while the carbonic acid increased in about the same ratio as nitrogen, i. e., in proportion to the oxygen entering the flame. Bunsen as well as myself failed, however, to draw the legitimate conclusion from these facts, at the time; the more as, with the materials I used, it was impossible to follow the formation of water by progressive oxydation.

The latter difficulty was avoided by Landolt, who, two years later took up the same subject, the failure of my health having rendered doubtful the prospect of my ever being able to resume it so as to carry out the proposed investigation of the other parts of the flame.

Landolt* used illuminating gas of known composition, and was therefore enabled to determine the deficient factor in my analysis, viz., water. So far as comparable his results in general confirmed mine. He felt compelled by the increase of free hydrogen in the higher parts of the flame examined by him, to assume the occurrence of a reaction between free carbon and (preformed) water; but he also failed to draw the inevitable conclusion as to what *must* happen in the luminous cone.

* Pogg. Ann., vol. xcix, p. 389.

A later research of Lunge* on the composition of the gas contained in the interior cone of the flame of a Bunsen's burner, must have led to the truth of the matter, by showing how little oxygen sufficed to render a flame non-luminous when previously mingled with the gas. I have not seen Lunge's memoir; but he likewise seems to have failed to draw the important conclusion of which his analysis must have contained the elements.

Next, in June, 1860, comes a memoir of Erdmann,† who in discussing the principles upon which his gas-tester (a modification of Bunsen's burner) is based, first distinctly enunciates that according to his experiments, the carbon in a flame is oxydized *before* the hydrogen, and that the separation of carbon upon which luminosity depends, is due to heat alone, as would be the case were the gas passed through a red hot tube.

Finally, eighteen months later, we have an elaborate and elegant research by Kersten,‡ wherein he proves by eudiometric experiments that (at least within the limits of the proportions employed by him) whenever a hydrocarbon is exploded with oxygen insufficient to burn more than the carbon to carbonic oxyd no hydrogen at all is oxydized; but that as between carbonic oxyd and hydrogen, the formation of carbonic acid on one hand and of water on the other depend upon "chemical mass," as Bunsen had already shown.

This question has therefore been peremptorily settled by decisive experiments, as much as eight years since. Yet the latest editions of text-books published in this country, and even those which, like the excellent work of Messrs. Eliot and Storer, claim to represent the latest state of the science, still retain the old error regarding the succession of oxydation.

There is another point which, though I took special pains to demonstrate the facts fourteen years ago, is still incorrectly stated in almost all text-books as well as books of reference. I allude to the definition of the several essentially distinct parts of the flame. *Three* only are usually mentioned, viz., the inner cone, the luminous portion, and the outer, faintly luminous envelop or veil. Yet Berzelius already distinguished the very important fourth part, viz: the blue cup-shaped zone surrounding the base of the flame, which is as sharply defined from the inner cone, as from the outer veil with which it is usually confounded.

That this blue cup is identical with the blue cone of the blowpipe oxydation-flame, is stated by Plattner in the first edition of his work on the blowpipe. Strangely inconsistent with his

* Annalen der Chemie und Pharmacie, vol. cxii, p. 205.
† J. pr. Chem., June, 1860, p. 241. ‡ J. pr. Chem., Dec. 1861, p. 290.

own definition, he nevertheless teaches that the blue cup is formed by the combustion chiefly of carbonic oxyd, produced by the "first and weakest effect of the heat on the fuel"—an assumption as little justified by experiments regarding such action, as was that of the combustion of hydrogen previous to carbon. It is palpably inadmissible in reference to the blue oxydation cone, which is of course identical with the flame of a Bunsen's gas burner—the supply of oxygen being, in both cases, sufficiently great, and so mingled with the entire gas as to suppress the separation of carbon.

The part performed by the blue cup, viz: that of a self-heating retort with walls impervious to oxygen, in which dry distillation is accomplished; its theoretical import, as the counterpart of the luminous portion, where the same gases are burnt with evolution of light, render the neglect with which it has been treated doubly surprising. That it is totally distinct from the outer veil is readily perceived when the eye is protected from being dazzled by means of a screen of the shape and size of the luminous hollow cone. The veil is then seen surrounding the blue cup as well as the higher portions of the flame, and is thus proved to be nothing more than a zone of glowing gas; which of course, however, cannot be strictly defined from the luminous envelop, the oxydation being a gradually progressive one, from the highly luminous central portion to that brownish, semi-transparent zone of transition, where the carbonic oxyd, burning simultaneously with hydrogen, fails to produce its characteristic blue tint because of the excessive temperature existing there. The same is the case when it is burnt by itself from a jet kept at a white heat.*

The inner cone, too, is still incorrectly defined as "the space containing the combustible vapors and gases generated from the wick." This would lead any one to suppose, that the external atmosphere had only the effect of burning off the outside of this gaseous mass, and some text-books have gone far in the graphic delineation of this process. My analyses first proved fourteen years since, that even in a tallow flame, characterized by an excess of fuel over the available oxygen, the percentage of nitrogen gas does not fall below 59 per cent in the lowest part of the flame, and increases to 76 per cent at the point of the inner cone, with from 10 to 15 per cent of carbonic acid. The products and educts of combustion therefore greatly exceed the combustible gases and essentially modify the processes thereafter occurring, for it is clear that in the luminous portion, the water and carbonic acid must in part at least again yield up their oxygen, before final combustion.

* See Gmelin's Handbook, art. Carbonic Oxide.

On the latter point, I have never been able to agree with Bunsen, who maintained that the combustion of the carbon in the luminous envelop must be effected by free oxygen penetrating into it; because its combustion at the expense of the reduction of carbonic acid and water, could not be the cause of such intense production of light and heat; and upon his authority, I reluctantly adhered to this view in my paper, above referred to. I believe with Kersten (l. c., p. 314), that the burning surface of the luminous cone, where carbonic oxyd and free hydrogen meet the oxygen of the air, is the sole source of the heat which causes the intense ignition of the interior, and that the oxydation of the free carbon is effected only through the medium of the intensely heated carbonic acid and steam which penetrate from the outside. I habitually compare the process taking place in the luminous envelop, to what would happen if illuminating gas were passed through an ignited tube into which steam and carbonic acid are injected through lines of lateral orifices. The ignited carbon set free by heat would render the interior atmosphere intensely luminous at the near end, but the luminosity would rapidly decrease toward the far end, where we would have such a mixture of carbonic oxyd and hydrogen as that which, I conceive, is burning on the surface of the luminous cone, and which *there*, above the point of the latter, produces the maximum temperature.

Unfortunately, a direct solution of the question by an investigation of the gases contained in the luminous envelop, is exceedingly difficult. Much remains to be done in the study of the details of the differentially variable processes by which the familiar phenomenon of a luminous flame is produced; but what *is* known is at least worthy of a place in the text-books.

Art. XVII.—*A Notice of some Manuscripts in Central American Languages*; by Daniel G. Brinton, A.M., M.D.

The natives of Yucatan and most of those who formerly inhabited the provinces of Vera Paz, Chiapas, Guatemala, and Tabasco, spoke closely related languages, the most prominent of which was the Maya, current on the peninsula. Its name has been applied generically to them all, and may thus be understood to include the Maya proper or Yucateca, the Cakchiquel or Guatemalteca, the Quiche or Utlateca, the Tzutuhil or Atiteca, the Zahlopakap, the Pokome, the Tzotzil, the Mam, the Tzendal supposed to be, or most nearly to resemble, the parent stem, and the Huasteca of Tamaulipas which was shown by the authors of the *Mithridates** to be an offshoot of the Maya. These various dialects resemble each other, both in vocabularies and grammatical forms as closely as the various Romanic tongues of modern Europe.

This linguistic family is of great interest for several reasons. It included the most highly civilized portions of the red race; their ruined cities are among the wonders of the New World; they had elaborated a phonetic alphabet far superior to the picture writing of the Aztecs; they had a body of mythology and poetry of which some very respectable relics still exist; and what of civilization was found in ancient Anahuac is supposed by many to have been inspired by them; moreover there is some philological ground to believe that the Natchez of Louisiana, the most cultivated aboriginal nation north of Mexico, had a large infusion of their blood.

They have deservedly therefore attracted the especial attention of those given to the study of native American languages. Mr. E. G. Squier has published a "Monograph of authors who have written on the languages of Central America, and collected vocabularies or composed works in the native dialects of that country" (New York, 1861); the Abbé E. C. Brasseur de Bourbourg has emphasized their importance and in his "*Collection de documents dans les langues indigènes*" (Paris, 1862–64,) has laid before the world that most interesting Quiche document, the Popol Vuh; Count Francisco Pimentel has treated of them at considerable length in his work on the languages of Mexico; M. H. de Charencey of Caen has inserted several excellent essays upon them in various scientific serials; while Dr. H. Berendt of Tabasco has collected a vast amount of material in the different dialects, which he expects to send to press on his return from the explorations in Central America in which he is now engaged.

In addition to the materials here indicated for a comparative study of this group there are in the library of the American Philosophical Society at Philadelphia, some manuscripts presented by Mariano Galvez, Governor of Guatemala, in 1836. They seem to have escaped the notice of scholars, their very existence there having been entirely unknown even to Mr. Squier, of New York City, although he tells us in the introduction to the above mentioned monograph that he had "given ten years of devotion to Central American subjects;" while not one of them is included in the more recent list of works given by Pimentel,† nor in Ludewig's "Literature of

* *Mithridates oder Allgemeine Sprachenkunde*, Th. III, Abth. III, S. 15, Berlin, 1813.

† *Cuadro Descriptivo de las Lenguas Indígenas de Mexico*, Tom. II, p. 124, Mexico, 1865.

American Aboriginal Languages." Some notice of them therefore will doubtless be welcome to "Americanistes."

The first I shall describe is a work on the Cholti dialect of the Maya by Francisco Moran. It is a small quarto of 92 leaves. The first three pages contain a narrative in Spanish, difficult to decipher, by Thomas Murillo, a layman, touching the missions in 1689–92. Then comes one leaf not numbered, with notes on the verso in Cholti, nearly illegible. On the recto of the fourth leaf,—

> Arte ‖ en lengua cholti que qui ‖ ere decir lengua de mil ‖ peros.

32 pages in a clear hand, ornamented with scroll work and pen sketches of birds and grotesque animals. On page 35,—

> Libro de lengua cholti que quiere ‖ decir lengua de milperos.

24 pages in a cramped but legible hand. At the end the colophon,—

> Fin del arte q^e^ trae no. M. R.^do^ P.^e^ Frai Fran^co^ moran en un libro de quartilla grande alto, que enquaderno i Recogio de nuestros Religiosos i barias cosas [añadió], el R.^do^ P.^e^ Frai Alonzo de Triana; Requiescant in pace todos. Amen Jesus, Maria Joseph.

A few notes on elegant phrases are added "que mi dio el P. Angel."

This is a duplicate of the preceding *Arte*, differing from it, however, in several particulars, being more full and accurate. They both seem to be copies of the original of Moran, not the one of the other.

After the *Libro* follow eight leaves of questions and answers at the confessional, etc., in Cholti. On p. 77 commences,—

> Confessionario en lengua ‖ cholti, escrito en el pue ‖ blo de san lucar salac de ‖ el chol, año de 1685:

three leaves ending with a catchword indicating that it is but a fragment.

The remaining leaves are occupied by a vocabulary, Spanish and Cholti, chiefly on the rectos only. At the commencement is the following marginal note,—

> Todo el Vocabulario grande de no. M. R. P.^e^ fr. fran^co^ moran esta tra Dusido en este libro, Por el ABesedario, i algunos bocablos mas.

The colophon is,—

> En este pueblo de lacandones llamado de Nta Señora de los dolores en 24 de Junio dia de S.^n^ Juan de 1695 años.

We have here therefore two copies of the grammar and one of the vocabulary of the Dominican missionary Francisco Moran, referred to by Father Francisco Vasquez in his *Cronica* (1714) as written in the characters invented by the Franciscan friar Francisco de la Parra (about 1550) to express the five peculiar consonants of the Maya group of languages. These are modifications of *k, p, ch, t,* and *tz.** Both these copyists have, however, adopted Roman letters. Neither the original nor any other copies are known to exist, nor any other work in the Cholti dialect, though a certain Father Córdoba also wrote a grammar of it.† It has even been uncertain whether the Cholti was an independent dialect. It is not mentioned at all in Ludewig's "Literature of American Aboriginal languages," and Mr. Squier gives the title of Moran's work from Vasquez thus,—

"Arte de la Lengua Cholti (Chorti ?)."‡

The Chorti, however, was spoken in Chiquimula and vicinity, while the Cholti, Chol, or Putum, was the dialect of the village of Belen in Vera Paz, of parts of Chiapas, and generally of the eastern Lacandones among the mountains between the former province and Guatemala. The name *chol* means cornfield, in Mexican Spanish *milpa,* and *ahcholob* or *cholti* owners or cultivators of cornfields, *milperos.* From the short vocabulary of Chorti collected by Mr. Stevens at Zacapa it appears to be farther than the Cholti from pure Maya.

The grammar of Moran is succinct, clear, and comprehensive, and eminently deserves publication, together with selections from the vocabulary. I have made a careful copy of it for my own use and have found it of great service as illustrating certain points of growth in these idioms, for instance with reference to the development of the personal pronouns, recently discussed in a scholarly essay by M. de Charencey;§ and affording some additional illustration of the "vowel echo," *l'echo vocalique* of the Maya dialects, to which the same writer has called attention as analogous to the law of the harmonic sequence of vowels common in Scythian languages.‖

The remaining manuscripts are in the Cakchiquel dialect, at one time and even yet much spoken and studied in Guatemala, and hence called Guatemalteca.

* I have also noticed the occasional use in these manuscripts of a peculiar vowel sound represented by an *i* with a diacritical mark beneath it.

† Pimentel. *Cuadro Descriptivo de las Lenguas Indigenas de México,* T. II, p. 234. Córdoba is not mentioned by Mr. Squier.

‡ *Monograph of Authors,* etc., p. 38.

§ *Le pronom personnel dans les idiomes de la famille Tapachulane-Huastèque:* Caen, 1868.

‖ *Etude comparative sur les langues de la famille Maya-Quiché, Revue Americaine,* Tom. I.

Calepino ‖ en lengua cakchi ‖ quel por Fray Francis ‖ co de Varea hijo de ‖ esta S. Provincia del ‖ SS. nombre de Jesvs ‖ de Religiosos de ‖ N. P. S. Francisco de Goatema ‖ la.

A small 4to, one unnumbered leaf, 227 leaves paged, 11 unnumbered leaves of additions. Colophon at foot of page 453,—

Acabase de escrevir y trasladar este bocabulario yo fray franco ceron, siendo guardian aunque sine meritis deste convento de S. Pedro de la laguna, oy dia catorse de enero del año del Señor de mil seyscientos y noventa y nuebe, dia del Dulcissimo nombre de Jesus Patron de nuestra S. Prova de Gua.tta y en el tercer año del Provincialato de N. M. R. P. fr. Juan Bautista.

The title is on the recto of the second leaf. On the recto of the first leaf is the form of absolution in Latin and Cakchiquel ; on the verso a note dated 1732 to the effect that the owner, a priest, received this volume in payment for masses for the soul of its previous possessor, a certain Señor Achutegui.

The dictionary is Cakchiquel and Spanish, written closely but legibly, with 35 lines to a page and averaging about four lines of examples to each word. An abundance of phrases and forms are given, but the alphabetical order is not strictly preserved. The characters of Parra are used throughout.

No author of the name of Varea is mentioned by Mr. Squier. But Francisco *Varela* is said to have gone to Guatemala in 1596 and to have composed a "Calepino" in 400 pages folio.* No doubt this is the same person, and unless the original still exists in the convent of San Francisco de Guatemala, this is probably the only monument of his labor extant.

The next manuscript is a large folio, bound like the preceding in parchment, of 476 leaves numbered on the recto. The title is,—

Vocabulario ‖ De la Lengua cakchiquel, v, Guatimalteca ‖ Nueuamente hecho y recopilado con summo estudio ‖ trauajo y erudicion por el P.e F. Thomas Coto. Pre ‖ dicador y Padre de esta Prouj.a de el S.S.mo Nõbre ‖ de Jesus de Guatimala. En que se contienen ‖ todos los modos y frases elegantes conque los ‖ Naturales la hablan y d. q. se pueden valer ‖ los Ministros estudiossos para su mejor ‖ educacion y enseñanza.

This dictionary is a splendid testimonial to the zeal and scholarship of the Franciscan missionaries. The pages are large, with double columns, 37 lines to a page, written quite distinctly though here and there the ink has faded so that it is difficult to read. The first 15 pages are handsomely written

* Monograph, etc., p. 47.

in imitation of printed letters. The characters of Parra are adopted for the five peculiar sounds. Unfortunately the copy is incomplete, ending with the word *vendible.* As it is exclusively Spanish and Cakchiquel, it complements the Cakchiquel and Spanish *Calepino* of Varea.

It should be observed that the letter C is wrongly bound so that the latter part of it comes first, and several other letters do not seem to have been finished. This copy appears to date from early in the last century and is unique so far as I know. Coto was a native of Guatemala and lived in the latter part of the 17th century. Mr. Squier gives under his name only one title; "Thesaurus Verborum; ó Frases y Elegancias de la Lengua de Guatemala;" which probably is the same work as the above. It is peculiarly valuable not only for the linguistic material it contains, but for the light it throws on numerous customs of the natives, on the botany and zoology of the country, and for its quotations of manuscript works in Cakchiquel. Coto's principal authorities are Father Francisco Maldonado's sermons in that tongue, those of Father Antonio Saz (*de san Joachim, de la visitacion, de la asuncion, de la concepcion, manual en la lengua* and others, none of them mentioned by Mr. Squier or Pimentel,) Father Domingo Vico, bishop of Chiapas, and the "*calepino*" of Varea.

Under many words quite a description is given of this or that usage. For instance, under the word *baile*, native dance, which I choose having in mind the remarks on it made by the Abbé Brasseur de Bourbourg in his introduction to the Quiche drama of Rabinal Achi,* he remarks that they are of many kinds; that for instance which represents Noah coming out of the ark is called *avatal*; that in which they whirl a stick with their feet, *vugh*; that only engaged in by lovers *xgul*; that in which they played on their flutes of hollow reeds, *lotz tun*; this latter, he adds, was prohibited by Bishop Uguarte on the representations of Antonio Prieto de Villegas, commissary of the holy office, a learned man, thoroughly versed in Quiché, and for twenty years incumbent of the benefice of Matzatenango; it was also prohibited by the diocesan constitution in 1690; several other *bailes* are also described.

Under the word *luna* he mentions that these Central American nations partook of a singular belief which we find very widely spread on the American continent. It was that an eclipse was caused by some animal eating the moon, and to drive it away accordingly they broke their jars, shouted, whipped their dogs, and made all the noise possible. They likewise attributed to this orb a malignant influence and supposed

* *Collection de Documents dans les Langues Indigénes*, vol. II.

her to be the cause of disease, a belief extremely common among the illiterate everywhere.

The fourth manuscript is a large folio of 77 leaves not numbered, written in Cakchiquel in ordinary characters. On the recto of the second leaf is the following title,—

Arte pronunciacion y ortographia de la lengua en el mismo ydioma ‖ Cakchiquel.

On the fourth line of the verso of the same leaf,—

RAMILLETE, Manual para los Yndios sobre ‖ la Doctrina Christiana ‖ por fray francisco Maldonado minorita, ‖ Sub Censura sante Romane eclesie Dialogo primo.

This "nosegay" or anthology consists of twelve dialogues on the confession, creed, sacraments, good works, etc., between a priest and his catechumen. After the twelfth dialogue there is an addition of nine leaves in Cakchiquel with the following title,—

Esta explica ‖ cion de la Doctrina Christiana ‖ va con el mesmo testo de la cartilla inpresa el Año ‖ de mill y quinientas y çinquenta y seys por explicar los ‖ terminos que los Yndios Saben mal entendidos, por tuvien ‖ do el mismo authorre fformado la dicha Cartilla por man ‖ dado de ill.mo Señor Don fray juan çapata y Sanctoval ‖ obispo de guatemala, se puso aqui en la misma for ma ‖ que la Conrregie para que sirva de brevi.ssa exposicion A ‖ la antigua sub cenSura Sanct ‖ te Romane ecele ‖ ssie.

At the close is a table of contents followed by this colophon in Cakchiquel:

Chupam 6 de Julio huna 1748 año mixgizvi vugibaxic vae vutz libro Ramillete Manual tihobal quichin Yndios chupam vutzilz Dios Doctrina Christiana yn Seuastian lopez tzarin vae ueva voch-Sancta Maria Asumpçion tecpanatitan de tzolala.

Then follow two leaves in Cakchiquel headed:

A la emperatrix a la vergen Maria Señora Nra su humilde esclavo.

From this evidence we learn that this is a copy made in 1748 by Sebastian Lopez at Solola on Lake Atitan of two works, the older printed in 1556, author not given, the other by Francisco Maldonado. The former must be the "Doctrina Cristiana en Lengua Utlateca" or Quiche, published at Mexico in that year, whose author, Fray Francisco Marroquin died in 1563. It is true that this was said to be in Quiche,* and that Zapata y Sandoval was not a bishop until 1613.† But as I have

* Fr. Pedro de Betanzos who died in 1570, published a "Doctrina en Lengua de Guatemala" also at Mexico, year unknown. If this should prove to have appeared in 1563 also, one of the difficulties would be surmounted.

† Squier, Monograph, p. 52.

never seen a copy of Marroquin's *Doctrina,* I am unable to reconcile these discrepancies.

The *Dialogos* is a work hitherto unknown of Maldonado, one of the most learned of the Franciscan missionaries. He lived in the latter half of the 17th century. The only one of his productions given by Mr. Squier is "Sermones y Panegiricos en Lengua Cakchiquelche," which is that also chiefly referred to by Father Coto in his dictionary.

The next work is a small quarto of 109 leaves. Unfortunately the first leaf, with the general title, is missing. The top of the second leaf commences in the midst of a sentence in a *Doctrina Christiana* in Cakchiquel. This covers ten leaves, and is followed by two leaves of "Preguntas de la Doctrina," all in Cakchiquel. Next comes a "Confessionario breve en lengua Cakchiquel." The Spanish translation of each question and answer is also given.

After the Confessionario are three leaves, unnumbered and blank, except that on the recto of the second is a Latin Prayer to the Virgin, difficult to decipher.

On the recto of the next leaf is the following,—

Arte ‖ de la lengua cak ‖ chiquel.

It is written in a clear small hand, covers fifty-four pages with 30 lines on an average to the page, sometimes with one column, sometimes with two, and closes with this colophon,—

Martes ā 24 de Junio de 1692 años dia del Nacimiento de S. Juan Baptista se acavo el traslado de oraçiones y Arte en Kakchiquel.

From the close of this to the 96th leaf there is another series of doctrinal questions headed,—

Vae *K*utubal *K*habal ti ‖ *K*ut ubex richin Christianos ‖ cakchiquel *K*habal ri ‖ chin cakchiquel vinak.

(I designate the peculiar modification of the consonants by italics.)

Another "Confessionario breve en lengua castellana y cakchiquel" then follows, twelve pages in length, differing considerably from the previous one. The rest of the volume is taken up with "Platicas," short discourses on religious subjects. One of them is an incident from the life of Saint Vincent Ferrer, related for the purpose of "terrifying the natives, and dispelling the shame they usually have about confessing." There is an index to the book, and on the verso of the last leaf this note in regard to the binding,—"Este quaderno es de Fr. Alberto Miguez;" said "quaderno" being in dark calf, without boards, and with strings. The characters of Parra are employed in all the divisions of the work, and the writing is mostly quite legible.

There is no hint throughout where this was written, nor by whom. The colophon above quoted seems to show that it is the original, at least of the *Arte* and the prayers. From the mention of Saint Vincent Ferrer, a Dominican, and from the known rivalry of the two orders at that time in Central America, I am inclined to attribute it to a Dominican rather than a Franciscan. None of the bibliographical authorities already quoted mention any writer of either order who prepared works of this kind in Cakchiquel at or very near 1692. The manuscript proceedings of the Philosophical Society for Sept. 1836, when the books were received, throw no light on the matter.

The linguistic value of the *Arte* is considerable. Only two grammatical notices of the language seem to have been published, one about 1560 in Mexico, another in 1753, in Guatemala. Both of them are excessively rare, and indeed it is doubtful if any copy of the first is in existence. The Cakchiquel is peculiarly important in the comparative study of this group of languages, and with the rich materials here at hand to illustrate all its constructions, a publication of this short manuscript with notes would be most welcome to American linguists.

In concluding this brief notice of these interesting documents I wish to express my acknowledgments to Prof. J. P. Lesley, librarian, and Mr. Eli K. Price, member of the Philosophical Society, for facilities afforded me in examining its library.

Art. XVIII.—*Notices of New Meteoric Irons in the United States;* by Charles Upham Shepard.

1. *Meteoric Iron from Auburn, Macon county, Alabama.*

In October last, Prof. J. Darby, of the East Alabama College, forwarded to me by mail at Amherst, about twenty grains of what he suspected to be meteoric iron, with the desire that I would subject it to examination. He stated in his letter that the fragments had been detached from a mass weighing about eight pounds that had been ploughed up in his neighborhood many years since, but which until lately had been overlooked in his collection. He described the mass as consisting of agglutinated irregularly shaped concretions, presenting the appearance of having from a previous state of separation, been suddenly compressed together into a single rounded mass.

A partial examination induced me to believe the fragments sent to be of meteoric origin; and led to a correspondence with Prof. Darby, which resulted in his placing one half the mass, weighing four pounds, at my disposal, accompanied with the following more complete history of the discovery:

"The man who brought the mass to me (and who was its discoverer) had taken it, without my knowledge, to a blacksmith's shop, where in the cold state it was broken in two upon an anvil, by means of a sledge hammer. Originally, it must have been nearly globular in form. The surfaces produced by the separation had, when I received the specimen, a metallic luster. The finder made known to me the exact spot where he had ploughed it up. It was on what is known as the Daniel plantation, about three-quarters of a mile west of our college building, and near the eastern edge of a field, just across a branch (small stream). I have searched in the region for other specimens without effect, but have instructed the negroes to bring to me any thing unusual they may hereafter discover. The name of the man who found the mass and his present residence are unknown."

On receiving the half of the mass I was at once struck with Prof. Darby's first description of its appearance. The surface of fracture, or separation, is coarsely granular, exhibiting large irregularly shaped concretions, which show only obscurely, traces of octahedral cleavage. The former metallic luster is now replaced by a rusty brown film; while numerous cracks or chinks are observable, not merely separating the concretions but often traversing the mass of each individual. Indeed the entire specimen is thus cracked up and subdivided by these open veins, as if it had been shattered by striking when in a semi-fused state, against a rock, at the time of its fall. So imperfect is the cohesion at present that it would not be very difficult to break it into pieces, (from the size of a large pea up to that of an almond) by vigorous blows from a sledge-hammer. Some of these concretions are partially stalactitic, tuberose or sub-mammillary, as if a secondary softening or fusion of the iron had taken place at the time of its descent. This structure is altogether peculiar for a meteoric iron, though not unknown in native copper and silver.

The larger concretions have a slight tendency to separate into smaller ones of the size of peas, whose figure, however, is that of the granular individuals of magnetite and pyrites, except perhaps in having a tendency to an elongation in the concretion which occasionally passes into the sub-columnar structure.

One single globule of troilite (sulphid of iron), half an inch in diameter is visible upon the fractured surface of the mass. It is compact in structure, and yellowish-brown in color.

As yet I have not been able to have any portion of the mass regularly slit and polished; but a surface of one of the larger compound concretions, on being ground upon its broader side and polished, afforded me an area of half an inch square. This face, on being subjected to the action of dilute nitric acid, gave me a series of markings altogether new. They are extremely fine and delicate in their dimensions, and require a strong light with the aid of a microscope, to be seen with distinctness. The first character that displays itself is somewhat that of a mesh or net-work, and arises from the polygonal boundaries of the granular concretions. The areas within these lines or edges (which are exceedingly thin) have a glittering luster when held at a fixed angle to the light, though this angle often varies for different concretions, as in the case of a polished surface of coarse grained calcite or fluor. The second character that arrests attention in the examination, is the finely striated surface of each concretion,—one set of lines being perfectly straight and equidistant, as in calcite and labradorite, while a second set, but less distinct, cross these at right angles. The final peculiarity of the markings consists in this,—that these fine striæ are wholly made up of dots or beads, which are arranged in almost absolute contact, and are therefore to be regarded as consisting wholly of sections of rhabdite needles, while on the other hand, the mesh-like markings first noticed, are composed of plates of schreibersite.

From the foregoing it is apparent that the Auburn iron, when etched, is unlike any other hitherto described. But before a complete account can be given of its internal structure, an opportunity must be had of making further observations upon slices properly prepared from different portions of the mass.

The specific gravity, as obtained from four fragments, varied from 7·0—7·17, and gave 7·05 as the mean.

The iron is apparently free from chlorine. It dissolves in hydrochloric acid without extrication of sulphuretted hydrogen; and leaves behind a slight residue of thin brilliant, blackish scales and needles. The following result was obtained on about two grams weight of the iron:

Iron,	94·580
Nickel,	3·015
Phosphorus,	0·129
Insoluble,	0·523
Chromium, Magnesium, Calcium, Silicon, (?) } and loss	1·753
	100·000

Inasmuch as the iron analyzed was slightly enfilmed with oxyds, that, in the results obtained, had to be calculated back to the metallic state in common with the oxyds coming from the pure metals, a trifling part of the loss in the analysis should go to increase the iron and nickel above given.

The composition of the troilite, schreibersite and rhabdite in the mass is too well known to render necessary any attempt to analyze them in the present case. Neither cobalt, tin, nor copper was detected in this iron.

2. *Meteoric Iron from Southeastern Missouri.*

For my knowledge of this iron I am indebted to Prof. B. F. Shumard, of Saint Louis, who sent me a specimen of it several years ago, but through an accident it failed to reach me until lately. He wrote me under date of Nov. 4, 1868, as follows: "The specimen is from one in the collection of the St. Louis Academy of Sciences which I found among minerals that belonged to the old Western Academy of Sciences, of St. Louis. The label with it gives only '*S. E. Missouri*' as the locality. Its meteoric character was not known until I examined it."

In reply to my request for further information, Prof. Shumard has favored me (Dec. 18) with the following additional particulars: "The specimen in the Academy's collection is irregularly lozenge-shaped, $3\frac{3}{10}$ inches long, $1\frac{1}{2}$ wide, and $1\frac{1}{10}$ thick. The extremities and upper face are rough and irregular, one lateral piece is smooth with a wavy surface, the other has been cut to supply specimens to Prof. Silliman, Haidinger and yourself. The under side is rough near one end, while the remainder of it has been smoothed by hammering. The specimen bears the appearance of having been heated. Its present weight is nine ounces; and if you will add to this what has been taken to furnish the specimens referred to above you will probably not be far from the truth, in calling the original weight twelve ounces. Nothing has been published concerning the specimen. I discovered it in the museum of the Academy during the year 1863."

In respect to the figures developed by etching, it belongs to my order of megagrammic irons; and most resembles those of Arva and Cocke county. It is rich in schreibersite, insomuch that when long acted upon by acid, this mineral projects in thick laminæ above the surface, resembling mica on certain weathered coarse grained granites. The bars and spaces which are intermediate, however, are not traversed by those delicate lines of the same substance, so generally occurring in other irons.

Specific gravity 7·015—7·112. It appears to contain no chlorine. It gave:

Iron,	92·096
Nickel,	2·604
Schreibersite,	5·000
Chromium, cobalt, magnesium, phosphorus,	traces.

A fine residue of Fe Si and C. Neither copper nor tin was found.

3. *Composition of Meteoric Iron from Losttown, Cherokee Co., Georgia.*

My general description of this iron was contained in vol. xlvi, p. 257 of this Journal. Its analysis has afforded me—

Iron,	95·759
Nickel,	3.660
Insoluble (schreibersite and rhabdite,)	0·580
Chromium, cobalt, tin (?), magnesium,	traces.

Charleston, S. C. Dec. 29, 1868.

Art. XIX.—*On Nitrification;** by S. W. Johnson.

Formation of nitrogen compounds in combustion.—Saussure first observed (Ann. de Chimie, lxxi, 282), that in the burning of a mixture of oxygen and hydrogen gases in the air, the resulting water contains ammonia. He had previously noticed that nitric acid and nitrous acid are formed in the same process.

Kolbe (Ann. Chem. u. Pharm., cxix, 176) found that when a jet of burning hydrogen was passed into the neck of an open bottle containing oxygen, reddish-yellow vapors of nitrous acid or nitric peroxyd, were copiously produced on atmospheric air becoming mingled with the burning gases.

Bence Jones (Phil. Trans., 1851, ii, 399), discovered nitric (nitrous ?) acid in the water resulting from the burning of alcohol, hydrogen, coal, wax, and purified coal-gas.

By the use of the iodid of potassium starch-test (Price's test), Boettger (Jour. für Prakt. Chem., lxxxv, 396,) and Schönbein (ibid., lxxxiv, 215,) have more recently confirmed the result of Jones, but because they could detect neither free acid nor free alkali by the ordinary test-papers, they concluded that nitrous acid and ammonia are simultaneously formed—that, in fact, *nitrite of ammonia* is generated in all cases of rapid combustion.†

* The substance of this paper was orally presented to the National Academy of Sciences, in August, 1868.

† Nitrous acid does not appear when the combustible contains sulphur, since it is decomposed at high temperatures by sulphurous acid.

Meissner (Untersuchungen über den Sauerstoff, 1863, p. 283,) was unable to satisfy himself that either nitrous acid or ammonia is generated in combustion.

Finally, Zabelin (Ann. Chem. u. Ph., cxxx, 54), in a series of careful experiments, found that when alcohol, illuminating gas and hydrogen burn in the air, nitrous acid and ammonia are very frequently, but not always formed. When the combustion is so perfect that the resulting water is colorless and pure, only nitrous acid is formed; when, on the other hand, a trace of organic matters escapes oxydation, less or no nitrous acid, but in its place ammonia appears in the water, and this under circumstances that preclude its absorption from the atmosphere.

Zabelin gives no proof that the combustibles he employed were absolutely free from compounds of nitrogen, but otherwise, his experiments are not open to criticism.

Meissner's observations were indeed made under somewhat different conditions; but his negative results were not improbably arrived at simply because he employed a much less delicate test for nitrous acid than was used by Schönbein, Boettger, Jones and Zabelin.*

We must conclude then, that nitrous acid and ammonia are usually formed from atmospheric nitrogen during rapid combustion of hydrogen and compounds of hydrogen and carbon. The quantity of these bodies thus generated is, however, in general so extremely small as to require the most sensitive reagents for their detection.

Formation of nitrogen compounds at low temperatures.—Schönbein was the first to observe that nitric acid may be formed at moderately elevated or even ordinary temperatures. As is well known he obtained several grams of nitrate of potash by adding carbonate of potash to the liquid resulting from the slow oxydation of phosphorus in the preparation of ozone.

More recently he believed to have discovered that nitrogen compounds are formed by the simple evaporation of water. He heated a vessel (which was indifferently of glass, porcelain, silver, &c.), so that water would evaporate rapidly from its surface. The purest water was then dropped into the warm dish in small quantities at a time, each portion being allowed

* Meissner rejected Price's test in the belief that it cannot serve to distinguish nitrous acid from hydric peroxyd. He therefore made the liquid to be examined alkaline with a slight excess of potash, concentrated to small bulk and tested with dilute sulphuric acid and ferrous sulphate, (Unters. ü. d. Sauerstoff. p. 233). Schönbein had found that iodid of potassium is decomposed after a little time by *concentrated* solutions of hydric peroxyd, but is unaffected by this body when dilute, (Jour. für prakt. Chem. lxxxvi, p. 90.) Zabelin agrees with Schönbein that Price's test is decisive between hydric peroxyd and nitrous acid. (Ann. Chem. u. Ph., cxxx, p. 58.

to evaporate away before the next was added. Over the vapor thus generated was held the mouth of a cold bottle until a portion of the vapor was condensed in the latter.

The water thus collected gave the reactions for nitrous acid and ammonia, sometimes quite intensely, again faintly, and sometimes not at all.

By simply exposing a piece of filter paper for a sufficient time to the vapors arising from pure water heated to boiling and pouring a few drops of acidified iodid of potassium-starch-paste upon it, the reaction of nitrous acid was obtained. When paper which had been impregnated with dilute solution of pure potash was hung in the vapors that arose from water heated in an open dish to 100° F. it shortly acquired so much nitrite of potash as to react with the above named test.

Lastly, nitrous acid and ammonia appeared when a sheet of filter paper, or a piece of linen cloth, which had been moistened with the purest water, was allowed to dry at ordinary temperatures, in the open air or in a closed vessel. (Jour. für Prakt. Chem., lxvi, 131). These experiments of Schönbein are open to criticism and do not furnish perfectly satisfactory evidence that nitrous acid and ammonia are generated under the circumstances mentioned. Bohlig has objected that these bodies might be gathered from the atmosphere where they certainly exist, though in extremely minute quantity.

Zabelin, in the paper before referred to (Ann. Ch. Ph., cxxx, p. 76), communicates some experimental results which, in the writer's opinion, serve to clear up the matter satisfactorily.

Zabelin ascertained in the first place that the atmospheric air contained too little ammonia to influence Nessler's test, which is of extreme delicacy and which he constantly employed in his investigations.

Zabelin operated in closed vessels. The apparatus he used consisted of two glass flasks, a larger and a smaller one, which were closed by corks and fitted with glass tubes, so that a stream of air entering the larger vessel should bubble through water covering its bottom and thence passing into the smaller flask should stream through Nessler's test. Nextly, he found that no ammonia and (by Price's test) but doubtful traces of nitrous acid could be detected in the purest water when distilled alone in this apparatus.

Zabelin likewise showed that cellulose (clippings of filter paper or shreds of linen) yielded no ammonia to Nessler's test when heated in a current of air at temperatures of 120° to 160° F.

Lastly, he found that when cellulose and pure water together were exposed to a current of air at the temperatures just named, ammonia was at once indicated by Nessler's test. Nitrous acid, however, could be detected, if at all, in the minutest traces only.

The reader should observe that Boettger and Schönbein, finding in the first instance by the exceedingly sensitive test with KI and starch-paste, that nitrous acid was formed, when hydrogen burned in the air, while the water thus generated was neutral in its reaction with the *vastly less sensitive* litmus test-paper, concluded that the nitrous acid was united with some base in the form of a neutral salt. Afterward, the detection of ammonia appeared to demonstrate the formation of nitrite of ammonia. Schönbein's explanation of the mode in which this salt may be generated, viz., by the direct union of water and nitrogen, seems to have perfectly satisfied the chemical critics.*

This theory has, however, nothing to warrant it, even in the way of probability. If traces of nitrite of ammonia can be produced by the immediate combination of these exceptionally abundant and universally diffused bodies at common temperatures, or at the boiling point of water, or lastly in close proximity to the flames of burning gases, then it is simply inconceivable that a good share of the atmosphere should not speedily dissolve in the ocean, for the conditions of Schönbein's experiments prevail at all times and at all places so far as these substances are concerned.

The discovery of Zabelin that ammonia and nitrous acid do not always appear in equivalent quantities or even simultaneously, in no wise conflicts with any of Schönbein's facts. A quantity of free nitrous acid that admits of recognition by help of Price's test would not necessarily have any effect on litmus or other test for free acids. There remains then no necessity of assuming the generation of nitrite of ammonia, and the fact of the separate appearance of the elements of this salt demands another explanation.

The writer is not able perhaps to offer a fully satisfactory explanation of the facts above adduced. He submits, however, some speculations which appear to him entirely warranted by the present aspects of the case, in the hope that some one with the time at command for experimental study, will establish or disprove them by suitable investigations.

He believes that in no case can free nitrogen unite directly

* Zabelin was inclined to believe that his failure to detect nitrous acid in some of his experiments where organic matters intervened, was due to a power possessed by these organic matters to mask or impair the delicacy of Price's test, as first noticed by Pettenkofer and since demonstrated by Schönbein in case of urine.

with water, but in the conditions of all the foregoing experiments, it enters combination by the action of *ozone*, as Schönbein formerly maintained and was the first to suggest. Ozone is formed not only in all rapid combustions but also in the slow oxydation of organic matters (paper and linen.)

Pincus has lately published some observations which any chemist may readily verify, that demonstrate the formation of ozone in combustion at high temperatures. It is only necessary to hold a cold, clean and dry beaker glass for a few seconds over a jet of pure hydrogen burning from a metal tube with a flame $\frac{1}{8}$ of an inch high, and then to smell of its contents. The ozone odor is most plainly perceived. The same result may be obtained from a small flame of alcohol or of a stearine candle.—(Die Landwirthschaftlichen Versuchs-stationen, ix, 473.)

The formation of ozone in all cases of slow oxydation at common temperatures is in analogy with its appearance during the eremacausis of phosphorus, and if antozone really exists and is the genetic complement of ozone as is indicated so strongly by the researches of Schönbein and Meissner, we must consider the case pretty well made out in theory, for hydric peroxyd which is held to be the result of the union of antozone with water, is formed in so many instances of slow oxydation of metals and organic bodies that Schönbein felt justified in assuming its production, or that of a corresponding organic antozonid in them all.—(Jour. für. prakt. Chem., xcviii and xcix.)

In Schönbein's experiments before mentioned, where paper or linen were not employed, the dust of the atmosphere probably supplied the organic matters.

The first result of the oxydation of nitrogen is nitrous acid alone (at least Schönbein and Bohlig detected no nitric acid), when the combustion is complete as in case of hydrogen, or when organic matters are excluded from the experiment. Nitric acid is a product of the subsequent oxydation of nitrous acid. When organic matters exist in the product of combustion, as when alcohol burns in a heated apparatus yielding water having a yellowish color, it is probable that nitrous acid is formed, but is afterward *reduced* to ammonia.

The reduction of nitrates to nitrites by various organic matters was announced by Schönbein several years ago. He found that all the vegetable and animal albuminoids, gelatine, and most of the carbohydrates, especially starch, glucose and milk-sugar (but not cane sugar) had this effect.—(Jour. für. prakt. Chem., lxxxiv, p. 207.) Zabelin in the article before cited, refers to Schönbein as authority for the fact that these organic bodies reduce nitrites to *ammonia* and ultimately to nitrogen and although we have not been able to find such a statement in those of Schönbein's papers to which we have had access, it is entirely credible and in accordance with numerous analogies.

Pelouze could find no nitrates (nor nitrites?) in the drainings of dung-heaps. Lawes, Gilbert, and Pugh, detected no trace of nitrites in moist mixtures of ignited earth, with bean-meal, starch and saw-dust, over which ozone (&c., by phosphorus,) was passed daily for several months. (Trans Roy. Soc., 1861, p. 496.) Boussingault states that mixing quick-lime or carbonate of potash with garden soil prevents nitrification and causes the development of ammonia. (Agronomie, etc., iii, p. 200.) In these cases the readily oxydable matters were in excess of active oxygen ; the process was therefore a *putrefactive and reducing one*, and although nitrites may have been and doubtless were formed on the surface, where oxygen was in excess, they were deoxydized in the interior of the mass.

Zabelin alludes to this reduction of nitrites by organic bodies not with the purpose of accounting for the formation of ammonia from nitrous acid, but to explain why he failed to detect nitrous acid in some of his experiments. Apparently firm in the conviction that nitrite of ammonia was in all cases formed, he says, "I believe that at the high temperature of our experiments the nitrite of ammonia produced was decomposed into ammonia that was carried off and into nitrous acid which mostly remained in the water of the flask, where it was probably further decomposed, under oxydation of the linen or the paper. Therefore it was found only in traces while ammonia was very easy to identify." He says further, "By our experiments in the burning of illuminating gas and alcohol we have recognized the same action of organic matters on nitrites; nay, nitrous acid gradually decomposed in their presence at ordinary temperatures; the water at the same time acquired the power of decolorizing iodized starch." (Ann. Chem. Pharm., cxxx, 85.) Now ammonia speedily bleaches starch made blue by iodine, and thus Zabelin's observations sustain our hypothesis.

If, as thus appears extremely probable, ozone is developed in all cases of oxydation both rapid and slow, then every flame and fire, every decaying plant and animal, the organic matters that exhale from the skin and lungs of living animals or from the foliage and flowers of plants, especially, perhaps, the volatile oils of cone-bearing trees, are indirectly, means of converting a portion of free nitrogen into nitrous and nitric acids, or ammonia.

In the interesting experiment of Mr. Loew, where pure water evaporating from the surface of paper which has been impregnated with alloxan, gives rise to the formation of ammonia as is indicated by the red color of murexyd supervening, (this Journal, xlv, p. 29,) the explanation we have offered, accounts for the facts observed as well as the hypothesis of Schönbein. We assume eremacausis of the moist paper, perhaps of the alloxan itself, to take place, ozone and by it nitrous acid to be formed, the latter being subsequently reduced to ammonia.

Our explanation is further sustained by the facts which we possess concerning the part which the organic matters perform in nitrification as it goes on within the soil.

Organic matters are universally recognized as essential in the composts which are employed in saltpeter plantations. Those earths in which nitrification proceeds spontaneously, are in general, characterized by a considerable content of humus. We have precise observations by Boussingault which demonstrate that organic matters are indispensable to nitrification. This investigator in the prosecution of his researches on vegetable nutrition, found in a large series of experiments, that when plants vegetate in calcined earth, neither the plant nor the earth gathers appreciable quantities of nitrogen from the atmosphere, during a period of several months, but when soil containing humus is employed under similar circumstances, either the soil or the plant (in most cases both) assimilate nitrogen to a decided degree.

For example, a lupin vegetating for ninety-seven days in 130 grams of rich garden soil (mixed with pure sand to favor access of air) the whole being confined in a glass case, assimilated 0·0217 grm. of nitrogen, and the soil far from exhibiting loss, during the same period, fixed 0·0454 grm. of this element. (Agronomie, etc., t. 1, p. 339). Of the nitrogen thus gathered from the atmosphere and fixed in a solid form by the soil, only one-ninth existed in the state of nitric acid and ammonia, when the experiment was concluded. We infer that this nitrogen for the most became a constituent of the soil by nitrification induced by the eremacausis of its humus, with the formation of ozone.

The passage of nine-tenths of this fixed nitrogen into a constituent of humus, where it doubtless exists in what we may call organic combination, probably took place by reduction of nitrogen oxyds to ammonia, and subsequent union of the latter with products of the alteration of cellulose in the same way that amid-like bodies have been obtained by Dusart, Schützenberger and P. Thenard, in heating together ammonia with dextrin, starch and glucose, in partial realization of Prof.

T. S. Hunt's idea of the synthesis of gelatine, from a carbohydrate and ammonia.—(Kekulé's Lehrbuch, ii, 356.)

Certain experiments executed by Mulder, more than twenty years ago (Chemistry of Animal and Vegetable Physiology, p. 673,) confirm the view we have taken. Two of these were "made with beans which had germinated in an atmosphere void of ammonia, and grown in one case in ulmic acid prepared from sugar, and also free from ammonia; and, in the other case, in charcoal, both being moistened with distilled water, free from ammonia. The ulmic acid and the charcoal were severally mixed up with one per cent. of wood ashes, to supply the plants with ash-ingredients. I determined the proportion of nitrogen in three beans and also in the plants that were produced by three other beans. The results are as follows :—

	White beans in ulmic acid.		Brown beans in charcoal.	
	Weight.	Nitrogen.	Weight.	Nitrogen.
Beans,	1·465 grm.	50 cub. cent.	1·277	27 cub. cent.
Plants,	4·167 "	160 " "	1·772	54 " "

The white beans, therefore, whilst growing into plants in substances and an atmosphere, both of which were free of ammonia, had obtained more than thrice the quantity of nitrogen that originally existed in the beans; in the brown beans the original quantity was doubled." Mulder believed this experiment to furnish evidence that ammonia is produced by the union of atmospheric nitrogen with hydrogen set free in the decay of organic matters. But the researches of Will have fairly established the impossibility of nascent hydrogen uniting to free nitrogen. The results of the experiments are fully explained by assuming that nitrogen was oxydized in nitrification and no other explanation yet proposed, accords with existing facts.

To sum up, the writer believes that in nature, free nitrogen enters into combination, in all cases, by oxydation, that the agent of oxydation is ozone, that in the soil this ozone originates, for the most part, in the slow oxydation of organic matters, and that ammonia and the organic nitrogen of humus, peat and coal are the result of the reduction of oxyds of nitrogen either in the living organism in the acts of nutrition, or by the organic matters of the dead plant or animal. The union of atmospheric nitrogen and oxygen under the influence of electrical tension has been shown by Meissner to be preceded by the production of ozone. By a long series of critically conducted observations, Daubeny (Jour. Chemical Soc., 1867,) has made probable that ozone appears in the vicinity of active foliage exposed to sunlight, and concludes that the oxygen set free from combination in the plant, is partly ozonized, as is true of that which separates in the decomposition of permanganates and chromates by oil of vitriol.* The plant, then, appears to be an agent of nitrification when living as well as when dead, and ozone is the result of a molecular change which accompanies the decomposition as well as the formation of oxygen compounds.

Sheffield Laboratory, Dec., 1868.

Art. XX.—*Geological Nòtes on the Andes of Ecuador*; by James Orton.

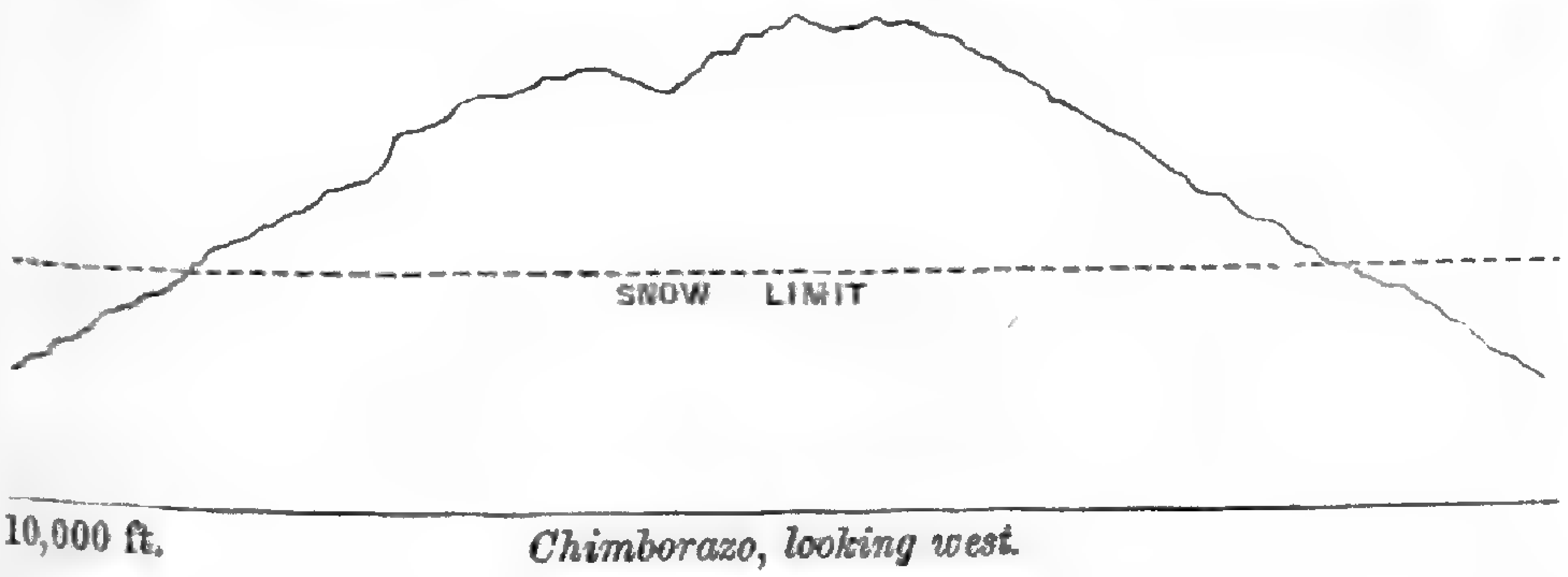
The Andes traverse the Republic of Ecuador in two Cordilleras, which are nearly parallel to each other and on the average, forty miles apart. The great valley which they enclose is about 300 miles in length, and has a general direction of 58° W. It is divided by two transversal dikes—the knots of Tiupullo and Assuay into three basins; Quito, Ambato, and Cuenca, having the respective altitudes of 9,500, 8,000 and 7,800 feet above the Pacific. There are some subordinate knots, and some longitudinal ridges; but all the basins lie parallel to the axes of the Cordilleras—a characteristic feature of the Andes. There are deep valleys on the outside flanks, which are evidently valleys of erosion; but the basins enclosed by the Cordilleras were created with them.

We b·lieve with Darwin, that the Andes did not suddenly reach their present gigantic proportions. Wilson counted six terraces in going up from the sea, through the province of Esmeraldas toward Quito. Moreover, such an assemblage of great volcanoes, among them Cotopaxi, the highest open vent on the earth's surface, and Sangai, the most active in its eruptions, shows that the energy which heaved the Andes, is of deep seated origin, and that it is not yet expended. We are also reminded of the law that "volcanoes in a state of action, concur with proofs of recent elevation." Above the terraces are the *cerros*, or outlying spurs; and still higher are the *paramos* or bleak, grassy highlands, out of which rise the icy peaks. The west slope of the Ecuadorian Andes, is about 275 feet per mile; on the east, it is 125.

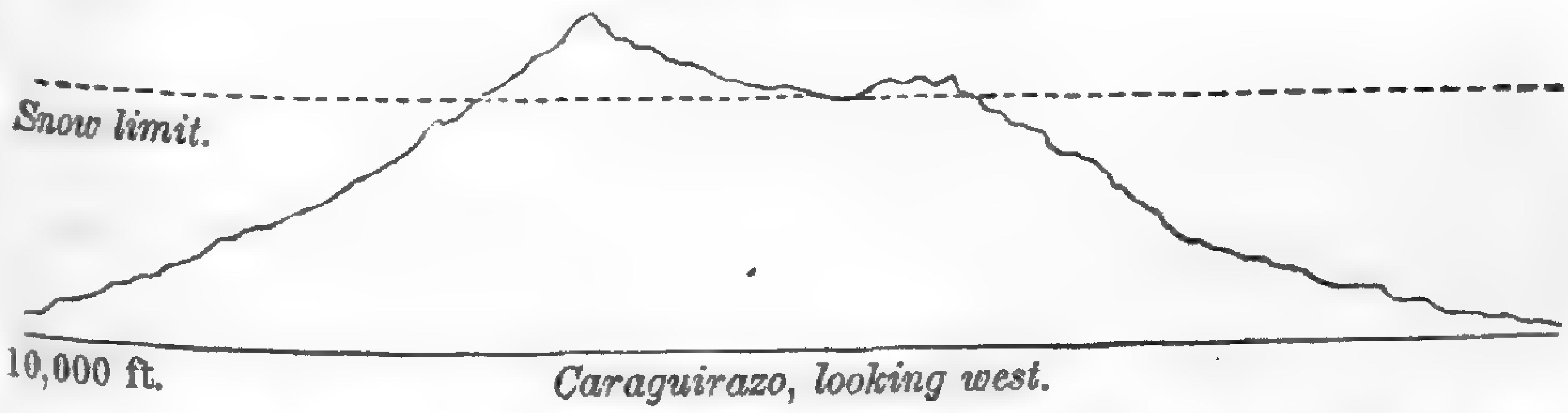
The mountain chain is built up of granitic, gneissoid and schistose rocks, often in vertical position, and capped with

* May not the liberation of chlorine in preparing oxygen from a mixture of chlorate of potash and oxyd of manganese, be due to the action of ozone? Chlorine appears when the decomposition is effected in a low heat, but not when a high temperature is used. In the latter case ozone cannot exist.

trachyte and porphyry. Large masses of *solid* rock are rarely seen; everything is cracked, calcined or triturated. While in Bolivia, the eastern Cordillera shows a succession of sharp, ragged peaks, in contrast with the conical summits of the Cordillera of the coast; there is no such distinction in the Andes of the equator. The eastern Cordillera, has a greater mean height, and it displays more volcanic activity. Twenty volcanic mountains surround the great valley, of which twelve are in the oriental chain. Three of the twenty (Cotopaxi, Sangai and Pichincha,) are now active; and five (Chiles, Imbabura, Guamani, Tunguragua and Quirotoa,) are known to have been active since the conquest. The truncated cone of Cotopaxi, the jagged, Alpine crest of ruined Caraguirazo, and



Chimborazo, looking west.



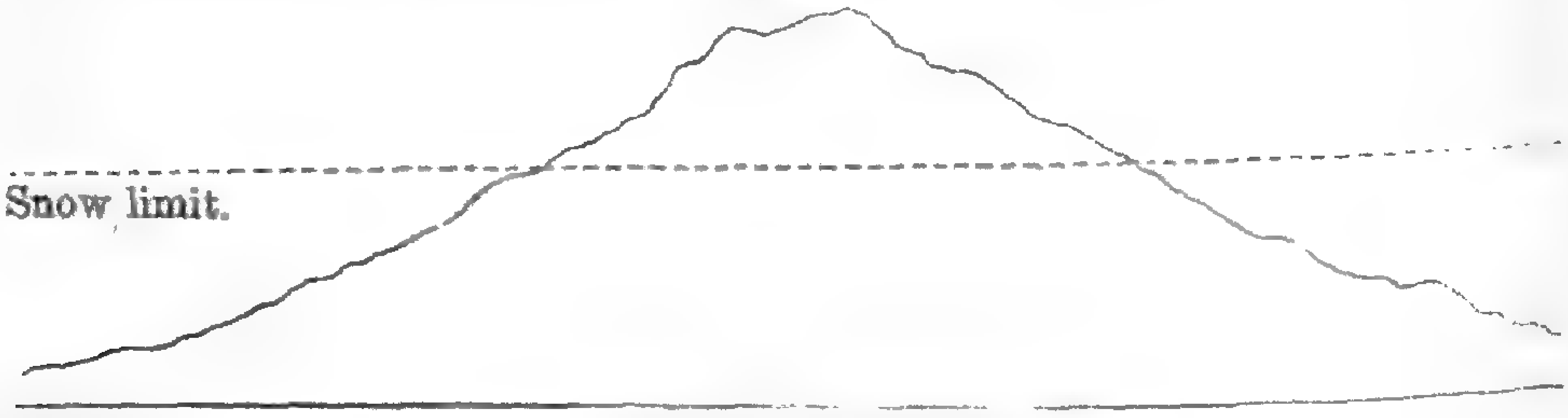
Caraguirazo, looking west.

the dome of Chimborazo, are the representative forms of the volcanic summits. Antisana and Cayambi, are fashioned after Chimborazo, though the latter is table topped rather than convex; Altar, Quirotoa, Iliniza, Sincholagua, Rumiñagui and Corazon, resemble Caraguirazo; Tunguragua, Sangai, Llanganati, Cotocachi, Chiles and Imbabura, imitate Cotopaxi; Pichincha, Atacatzo and Guamani are irregular. The extinct volcanoes usually have double domes or peaks. This twin feature is prominent in other Andean volcanoes; e. g., Illimani.

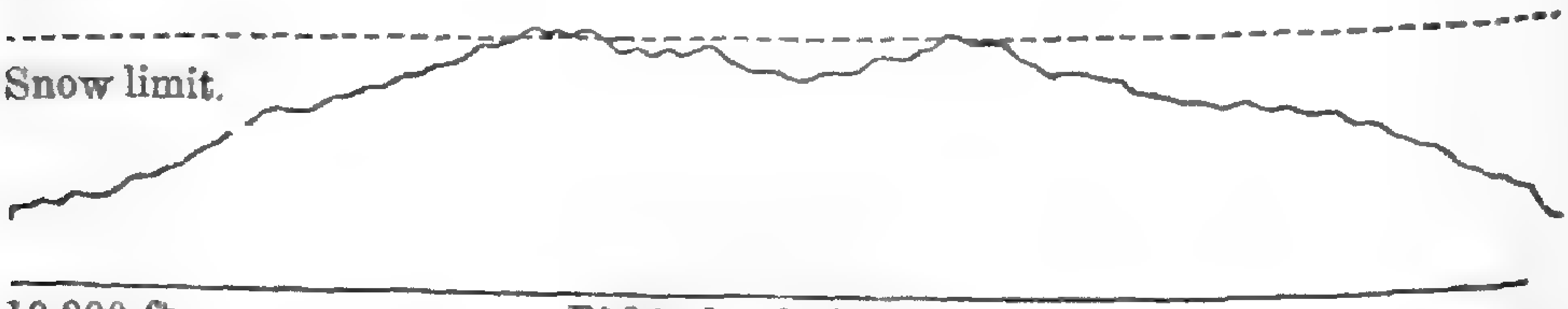
The growth of the cones since they began to erupt, is plainly exogenous. They rarely eject liquid lava, but chiefly water,* mud, ashes and fragments of trachyte and porphyry. From the deluges of water, result deep furrows in the sides;

* Much of the water sent down from Cotopaxi, may be due to the melting of snow; but this cannot be true of Pichincha or Imbabura.

and from the prevalence of the east wind, which is always met by the traveller on the crest of either Cordillera, there is a greater accumulation of ashes, and less snow on the west slope. In some of the craters, particularly of Pichincha, Altar and Rumiñagui, the western wall is lower than the eastern. Cotopaxi is exceptional, being lowest on the east side.* As there is no synchronism in the eruptions of Etna and Vesuvius, so there is no simultaneous activity of Cotopaxi and Pichincha. These volcanoes must have independent reservoirs, for Cotopaxi is 3,000 feet higher than Pichincha, and only



Cotopaxi, looking south.



Pichincha, looking west.

thirty miles distant. It is generally believed by the natives, that Cotopaxi and Tunguragua, are sympathetic. The volcanoes of Ecuador, (excepting Sangai,) belong to Scrope's third order; "that in which eruptive paroxysms of intense energy alternate with lengthened periods of complete external inertness,—phase of prolonged intermittences."

Taking Cotopaxi as the center of the system of Ecuadorian mountains, we may arrange the lower peaks, on concentric orbits. On the first are Ruminagui and Sincholagua, 10 miles distant; on the second, Iliniza, Corazon, Atacatzo and Antisana, 25 miles; on the third, Quirotoa, Pichincha and Guamani, 30 miles; on the fourth, Langanati, 40 miles; on the fifth, Tunguragua, Caraguirazo and Cayambi, 50 miles; on the sixth, Chimborazo, Imbabura, and Cotocachi, 60 miles; on the seventh, Altar, 65 miles; on the eighth, Sangai, 75 miles; on the ninth, Chiles and Assuay, 100 miles.

The chief dome of Chimborazo, presents from Guaranda a magnificent paraboloidal mass; while from a spot near Riobamba, the profile of the whole mountain has the figure of a lion in repose. One fourth of the entire altitude is perpetu-

* In the volcanoes of the Galapagos Islands, the south wall is lowest; while the Mexican craters, are lowest on the northeast.

ally covered with snow. "Chimborazo (said Humboldt,) is formed of one simultaneously uplifted mass of trachyte which has pierced, and reposes upon, secondary strata," and Daubeny adds that "the trachyte dome was raised, and then the volcanic fire was shifted to another quarter." But to us the upper third of the mountain appeared like a vast accumulation of volcanic matter ejected around and over a fissure in the upturned metamorphic rocks of the Cordillera. The rounded contour of the trachytic beds, as well as their porous texture, as Scrope contends, indicates their protrusion in an imperfect state of liquidity. We observed the following rocks on Chimborazo :*

Fine-grained trachyte, gray, brown and dark colored	(common.)
" " soft, reddish,	"
Coarse, porphyroid trachyte, gray,	"
Cellular trachyte, iron stained,	"
Compact trachyte, gray,	(rare.)
Fine-grained trachyte with seams of flint,	"
Obsidian,	"

In the College of Riobamba are specimens of brown trachyte in quite regular trihedral prisms.

The crown of Pichincha presents three groups of rocky peaks. The most westerly one, called Rucu-Pichincha, alone manifests activity. It is the only volcano in Ecuador which has not a true cone crater. Some violent eruption beyond the reach of history, or tradition, has formed an enormous funnel-shaped basin 2,500 feet deep, 1,500 feet in diameter at the bottom, and expanding upward to a width of more than half a mile.† The abyss is girt with a ragged wall of dark trachyte which rises on the inside, either vertically or at the angle of 50°. The outside of the cone (so-called) is inclined 30°, and like the inside is covered with fine volcanic debris, chiefly pumice. The height of the cone as compared with the whole height of the mountain is as 1 to 10; that of Vesuvius being 1 to 3, and Teneriffe, 1 to 22. Bouguer and LaCondamine in 1742, were the first to reach the brink of this crater; Visse and Moreno in 1844, were the first to enter it. The descent is extremely perilous, but the traveller is rewarded with one of the most imposing sights in nature.‡ The bot-

* Complete series of the Rocks mentioned in this paper, are preserved in the Cabinets of the University of Rochester, and of Ingham University, LeRoy, N. Y. They were kindly identified by Dr. T. Sterry Hunt, F.R.S., the distinguished mineralogist of the Canada Survey.

† Orizaba in Mexico, 2,000 feet higher than Pichincha, has a crater 6,000 feet wide, but only 500 feet deep. That of Kilauea is from 600 to 1,000 feet deep.

‡ The author made two attempts to reach the bottom of this crater, the second of which was successful, Oct. 22, 1867. We were two hours in descending, and three hours in our exit. Mr. Farrand, a photographer, was the only American who had preceded us.

tom of the crater is covered with huge blocks of porphyry and trachyte scattered about in wild confusion. West of the center rises the real cone of eruption, an irregular heap of stones about 250 feet high and containing numerous fumeroles. As Moreno rightly says, all the vents are situated in this little cone. The present products are sulphur and plumose alum lining the fissures, and aqueous vapor, with a small percentage of carbonic and sulphurous gases. The temperature of the vapor just within the fumerole we found to be 184°, water boiling beside it at 189°·2. The gigantic wall which girdles this fiery mount, is not only lower on the west side, but a deep cleft leads down into the wilds of Esmeraldas. A year ago the column of smoke did not rise above the top of the crater, but the volcano has lately been showing signs of activity such as it has not exhibited since the last grand eruption of 1660. On the 19th of March, 1868, detonations were audible at Quito, five miles distant in a straight line; and three days after there were more thunderings, with a great column of vapor visible from Chillo, twelve miles to the east. These phenomena were accompanied by an unusual fall of rain. On the 16th of August, occurred the great earthquake, since which event Pichincha has not made any extraordinary display.* The solid products of Pichincha since the Spanish invasion have been chiefly pumice and ashes. The roads leading to Quito cut through hills of pumice. On the plain of Iñaquito and in the valley of Esmeraldas are vast erratic blocks of trachyte, some containing twenty-five cubic yards, and having sharp angles, and in some cases a polished unstriated surface. M. Visse does not consider them to have been thrown out of Pichincha, as LaCondamine and tradition have judged. We dislike to disagree with this *habile observateur.* It is true, as he says, that they could not have come out of the present cone at a less angle than 45°, for they would have hit the sides of the high escarpment and rolled back again, while at a higher angle they would not have reached their present location. But they could be the fragments of the upper portion of the original trachytic cone blown into the air, at the great eruption which cleared out the enormous crater. The following rocks we observed within and around Pichincha:

Pumice, (lapilli,)	(common.)
Coarse-grained trachyte with augite crystals,	"
Granular trachyte, grayish,	"
Fine granular trachyte, reddish,†	"

* The natives ascribe the earthquake of 1859 to Pichincha.
† Used as building material in Quito.

Porous trachyte, with augite crystals,	(common.)
Porphyroid trachyte, dark base,	"
" " reddish,	"
" " gray,	"
Very compact trachyte, gray,	"
Sulphur, seen only in the crater, where it is abundant either pure or forming a conglomerate with cinders,	"
Fine grained trachyte, iron-stained,	(rare.)
Porous, granular trachyte, iron-stained,	"
Soft, friable trachyte, yellowish,	"
Fine-grained, decaying trachyte, with seams of flint,	"
Porphyroid trachyte, disintegrating,	"
Granular trachyte, "	"

Thirty-five miles S.E. of Pichincha is the extinct volcano of Antisana, loftier than Cotopaxi, and overtopped only by Chimborazo and Cayambi. Humboldt mentions three lava streams ; we discovered a fourth on the north side, reaching down to Papallacta. The great stream called *Volcan d'Ansango,* can be traced for ten miles, and its thickness as determined by our barometer, is 500 feet, with an average slope of 15°. It consists mainly of a dark, tough, porphyroid trachyte in angular fragments. At the foot of the mountain near Padregal, is a plain containing innumerable rounded, symmetrical hills of volcanic earth. Half way between Antisana and Pichincha the steps of a horse give a hollow sound, showing that the rock is porous and perhaps cavernous. In the same plain is a hot spring (118°) exhaling sulphuretted hydrogen. We ascended Antisana, to the altitude of 16,000 feet, collecting in this ascent the following representative rocks :

Cellular vitreous trachyte, black with few crystals,	(common.)
Fine grained, porphyroid trachyte, dark,*	"
Coarse " " reddish,	"
Compact trachyte, gray, found at snow limit,	"
Porphyroid " iron-stained, " "	"
Fine-grained " "	(rare).
Porous " dark,	"
Fine-grained " with augite crystals,	"
Pumice,	"

Fifteen miles S.W. of Antisana rises Cotopaxi, the loftiest and most symmetrical active volcano on the globe. Every representation, excepting the photograph taken by Farrand, is erroneous. Humboldt made the south slope 52°, and the north 50°. Guzman made the slope 69° 30'! Villavicencio in his *Geografia* makes it 40° ; and in a view drawn by Salas, the first artist in Quito, the slope is 35°. Spruce makes it 29°

* This and the preceding are characteristic of the "lava streams."

30′. The true slope of the south side is 30° 45′; of the north, 26° 45′; and of the west and east a little over 30°. Spruce gives 121° as the apical angle of the cone; more correctly it is 122° 30′. On the summit is a circular parapet of scoriæ as on the Peak of Teneriffe. On the east side are signs of an ancient lateral eruption. Cotopaxi is emphatically the pumice-producing volcano. The ash and cinder, sand and pumice accumulations in the vicinity are immense. In one place (Quincherar) they are 600 feet deep. The new road to the capitol which crosses the Chisinchi ridge about half way between Cotopaxi and Iliniza, presents the following section showing the character and relative amount of material successively erupted:

Soil,	1 ft.
Fine yellow pumice,	5 "
Compact black ashes with seams of pumice,	10 "
Fine yellow pumice,	1 " 6 in.
Compact black ashes,	12 "
Fine yellow pumice,	2 "
Compact black ashes with seams of pumice.	

Near Tacunga is the following section:

Soil,	0 ft. 0 in.
Stones and cinders,	5 "
Fine pumice,	0 " 6 "
Stones and cinders,	10 "
Compact black ashes.	

Compare a section at Pompeii:

Soil,	3 ft. 0 in.
Brown incoherent tuff,	1 " 6 "
Small scoriæ and white lapilli,	0 " 3 "
Brown earthy tuff,	4 " 9 "
Whitish lapilli,	0 " 1 "
Gray solid tuff,	0 " 3 "
Pumice and white lapilli,	0 " 3 "

The mud deposited at the foot of Cotopaxi, von Cotta would call a kind of volcanic tufa. The plain of Mulalo is strown with huge blocks of dark trachyte, some of them thirty feet square. They lie in rows; and Visse contends that they are not the product of volcanic eruption. The following are the rocks seen about Cotopaxi:

Pumice, with dark specks of augite,*	(common.)
" pure white,	"
Porous trachyte,	"

* The pumice seen by Castelnau and Bates floating on the Amazons was doubtless brought down from Cotopaxi by the Pastassa.

Very compact trachyte, dark gray,	(common.)
Granular trachyte, whitish with hematite scales,	"
Fine-grained trachyte, reddish, mottled,	"
" " gray,	"
" " dark, very tough,	"
Porous trachyte, iron-stained,	"
" " black,	"
Porphyroid trachyte, black vitreous base,	"
" " flesh-colored,	"
" " fine grained, dark,	"
Obsidian,	(rare.)
Fine-grained trachyte, dark, augitic,	"
" " cellular,	"
Soft, disintegrating trachyte, hardens under water,	"
Feldspatho-augitic rock, reddish,	"
Coarse porphyroid trachyte, reddish,	"
" " " light gray,	"

Tunguragua is a beautiful cone rivaling Cotopaxi; but it is 2,000 feet lower. Spruce calls one of the slopes 43° 1[illegible]′, and the apical angle 92° 30′. We found the west, east and south slopes 38°, making an angle at the apex of 104°. The west side is covered with fine black sand; on the north is an immense stream of black porphyroid fragments, much resembling the Antisana currents.* At the base of the mountain there is a fine grained ferruginous sandstone; and at Guanandu there is a cliff presenting columns of dark trachyte having a distinct prismatic form. At Baños is a hot ferruginous spring (130°). The last eruption lasted from 1773 to 1780; but Spruce asserts that he saw smoke issuing from the western edge of the truncated apex in 1857. The lithology of Tunguragua is illustrated by the following specimens:

Vitreous trachyte, black with occasional feldspar crystals,	(common.)
" " " vesicular,	"
Fine-grained trachyte, gray with augite crystals,	"
Porphyroid trachyte, banded black and gray,	"
" " dark base, numerous crystals,	"
" " reddish,	"
" " gray,	(rare.)

Altar is the most alpine of the Ecuadorian mountains. From the west it appears to be what it undoubtedly is, a broken down volcano, presenting eight snowy needle peaks surrounding an immense crater. It has not been active since the days of the Incas; but there is a tradition (still living in its Indian name, *capac-urcu*, the chief,) that originally it over-

* Humboldt says that Antisana was the only Quito volcano where he saw anything like a lava-current.

topped Chimborazo, and that after a violent eruption, which lasted eight years, the walls fell in. The plain of Riobamba is covered with fragments of trachyte, the debris of the last cone.

Twenty-five miles S. E. of Riobamba, is the ever active Sangai. Little is known of this busy volcano, as the marshes surrounding it and the great depth of fine ashes on the sides, render it unapproachable. It is constantly ejecting ashes and incandescent stones (LaCondamine adds "sulphur and bitumen,") accompanied by hourly explosions sometimes heard at Guayaquil. It is said to be more active in winter than in summer.

From these examples it will be seen that the products of the Ecuadorian volcanoes are mainly feldspathic. Trachyte abounds throughout the Andes. All the summits which rise above the limit of perpetual snow are trachytic. The trachytes of Bolivia, says D'Orbigny, are always micaceous; those of the equator are cellular, porous, granitoid, granular or compact. Cotopaxi alone produces foam-like pumice.* A most perfectly glassy translucent obsidian is occasionally found on Cotopaxi and Chimborazo. Its sp. gr. is 2·35 to 2·40. Humboldt mentions pitchstone as occurring on Antisana. There are numerous rocks on that volcano, and especially on Tunguragua, which exhibit a black vitreous base resembling pitchstone, but it is anhydrous, (losing only $\frac{3}{1000}$ of its weight by ignition,) and is therefore a soft trachytic glass. A coarse variety, approaching "ribboned obsidian," we found on Antisana. Some of the porphyritic rocks are conglomerate; but the great majority are true porphyries having a homogeneous base. Dr. Hunt calls them "porphyroid trachytes." Many of the specimens would be labeled Andesite by Darwin; but we demand a uniform and more definite definition of the term before using it. They have a black, rarely reddish, vitreous or impalpable base approaching obsidian, with a sp. gr. of 2·59 in pure specimens, and holding crystals of glassy feldspar and sometimes of pyroxene and hematite. They differ from the Old World porphyries in containing no quartz and seldom mica. The absence of basalt as well as the paucity of quartz is noticeable. Granitic rocks, also, so abundant in eastern South America, are rarely visible on the Pacific side, in the neighborhood of volcanoes. But they are only covered by

* James S. Wilson, C. E., in the employ of Ecuador, claims the discovery that "pumice is not a rock produced by volcanic fusion, but simply a rock altered by the solution and removal of one or more of its ingredients by humid heat, probably assisted by gases and acids. Thus the pumice of Pichincha is granitic, and specimens are found showing gradations of change from compact stone to porous pumice." It is remarkable that true trachyte, pumice and obsidian are wanting in the Galapagos only 700 miles from Pichincha. According to Pentland, trachytic pechsteins, obsidians and other vitrified products are very rare in Peru and Chili.

volcanic accumulations. At Cuenca, south of Chimborazo, metamorphic rocks abound. On the east slope of the eastern Cordillera mica-slate is the prevailing rock. The "prodigious beds of gypsum" seen in Chili are wanting in Ecuador. A dolomitic marble occurs near Cayambi.

The valley of Quito is doubtless filled up, like the Bolivian plateaus, with the debris of granitic and ancient sedimentary rocks, but it is now covered by a vast layer of pumice and scoriæ which have been falling from the early tertiary to the present time. Nearly the only fossils brought to light are some pleistocene mammals. At Alangasí, six miles from Quito, teeth of the Mastodon (Andium ?) have been found; and in the ravine of Chalan seven miles south of Riobamba, we discovered the femur and patellæ of a Mastodon, the skull of a Horse, and numerous leg-bones not yet identified. They were imbedded in the middle of a cliff (200 ft. high) of the compact tenacious clay resulting from the union of trachytic ashes with water, and were associated with terrestrial shells, identical with living species in the vicinity. The bones were drifted to this spot and deposited (many of them in a broken state) in horizontal lines. It is interesting to speculate upon the probable climate and the character of the vegetation in this high valley, when these extinct mammifers lived.*

Rochester, N. Y., Nov. 10th, 1868.

Art. XXI.—*Observations upon Autumnal Foliage*; by Joseph Wharton.

If chlorophyl, the green coloring matter of leaves, should be, like many other greens, a compound color, it must have for one of its elements a vegetable blue, capable of being reddened by acids.

If the juices of leaves, kept in a neutral condition by the vital force, or by alkaline matter brought in the sap from the earth, should, when circulation ceases, become acidified by the atmospheric oxygen, those juices would then be capable of reddening the vegetable blue of the chlorophyl.

If, however, that vegetable blue should be thus reddened, it ought to become blue again, when exposed to an alkali; or, in other words, if green leaves should be reddened in the autumn in the manner here suggested, by the unresisted action of the oxydizing atmosphere, they ought to return from red to green, if immersed in an alkaline atmosphere.

* See further, on this subject, an article on the heights of the South American Andes of Ecuador, etc., by Dr. Moritz Wagner, in the Berlin Zeitschrift für Allgemeine Erdkunde, xvi, 232, 1864.

Reasoning thus one autumn day about ten years ago, I arranged a wire staging, to stand under a glass receiver, which dipped into a dish of water, and under which was also placed a capsule containing ammonia. Upon this staging I placed in succession a variety of autumnal red leaves, and had the gratification to perceive that in most cases the green color was restored.

The rapidity and completeness of this restoration varied greatly in different leaves; while those which were covered by a thin and porous cuticle passed visibly from red to green before the eyes, (for instance Sassafras, Blackberry, Maple, etc.), others, whose cuticles were comparable to an impervious varnish, (for instance some of the Oaks,) changed gradually into brown, without showing any trace of green, except sometimes in a few spots where an imperfection in the leaf existed.

In order to determine fully whether the behavior of this latter class of leaves was really owing to the protection afforded to the pulp or chlorophyl by the cuticle, I wounded several such oak leaves in divers spots, and found that although these leaves, when exposed to ammoniacal vapor, became generally brown, each wound became the center of an irregular patch of green.

Of course the final result of exposure of any leaf to the vapor of ammonia, is destruction of all delicacy of tint, and production of a general decaying brown color; but the restoration of green is perfectly distinct, and this green color endures for some minutes, or even hours, if the leaves are soon enough removed from the vapor.

This simple experiment had more significance for me, when I read, a few years afterward, that the distinguished French chemist Frémy, had actually separated chlorophyl into two distinct substances, one blue, the other yellow.

Frost probably plays no other part in causing the autumnal tints, than merely to arrest the circulation by killing the leaves. When a sharp frost occurs early in the Fall, while the pulp of the leaves is still full and plump, the red colors come out brilliantly, because there is plenty of the blue substance to be acted upon by the juices, then also abundant.

When on the other hand the leaves die slowly and are at the same time slowly dessicated in a late and dry autumn, the pulp becomes so meager, and the skin so dry and hard, that an abundant production of fine red tints is impossible, and brown, the color of decay, predominates.

The connection between arrest or feebleness of circulation and reddened foliage, is well illustrated by the Sassafras, the leaves upon the extremities of whose branches are usually well nourished and succulent, while the inner leaves—those nearer the trunk—are smaller and weaker. It is not uncommon for a Sassafras tree, which stands alone, to present in the autumn the appearance of a green mass of foliage, illuminated from within by red and yellow lights: this results from the complete change of those feeble leaves in the interior, before the more vigorous outward leaves are in a condition to yield.

An Oak tree, observed last Fall, afforded a similar illustration. Twigs had sprouted from the trunk, and the leaves upon those twigs all reddened long in advance of the leaves upon other parts of the tree. When, shortly after, and long before the general stripping of the tree, those twigs fell, they appeared upon examination to have been detached from the trunk thus early by a process similar to that which separates the single leaf from the plant, and whose first effect had of course been an untimely stoppage of the circulation.*

Leaves which die in the Summer are usually dried out directly, or, if the weather is moist, are quickly rotted; still, it is common enough to see single dead leaves of Gum, Sumac, or Sassafras, splendidly reddened many weeks before a frost.

In June, 1868, I observed leaves of Mahonia upon a sickly twig thus colored, and on July 19th, I found thoroughly reddened leaves of Sassafras and Gum, and leaves of American Poplar and Chestnut of perfect autumnal yellow; all being of course such as by feebleness of, or injury to, the sustaining twigs, had been deprived of their circulation.

It is somewhat remarkable, however, that of the millions of dead leaves which I saw last Summer upon twigs wounded by the seventeen year locusts, not one showed autumnal tints; though I observed upon a number of trees, bearing the brown leaves of the locust-wounded twigs, an occasional leaf of a full autumnal red upon a twig not so wounded.

For the convenience of those who may incline to pursue this subject, I add a compend of some of the principal recent investigations concerning chlorophyl and related matters.

Comptes Rendus, l, 405. Frémy separates chlorophyl, when

* The Medical Press and Circular, of Paris, states that M. Trecul and others have lately been engaged in investigating the cause of the autumnal stripping of trees, and their researches would seem to point to the conclusion that in many plants a phenomenon occurs just before the fall of the leaf, which is not unlike the process which accompanies the shedding of horns in animals. It consists in the obstruction of the proper vessels at the base of the petiole, or leaf stalk. The obstruction, according to an American writer, is caused by the multiplication of cells, which first occurs in the parietes of the vessels. The cells increase and multiply, till at last the vessels are completely choked up in the neighborhood of the insertion of the leaf, and thus a differential plane is formed, across which the leaf stalk breaks, and the leaf accordingly falls.

dissolved in alcohol, into two coloring matters, by submitting it to a mixture of ether and chlorhydric acid; the former takes up the yellow matter, (phylloxanthin,) the latter the blue matter (phyllocyanin,) each liquid having distinctly the yellow and the blue color respectively, which being mixed by shaking together, form a leaf green. The yellow coloring matter of new sprouts, and of etiolated leaves, contains phylloxanthin, capable of being developed into chlorophyl; in autumnal yellow leaves, the phyllocyanin has been destroyed. The yellow matter, Frémy supposes to be more universal and more stable than the blue.

Comptes Rendus, lvii, 39. Châtin and Filhol state that the surface of young leaves is covered with a fatty substance, protecting the tissues from the air, which varnish diminishes toward Fall. This being removed, the leaf becomes dead colored. Deoxydizers (e. g., SO^2) restore red leaves to yellow. Red leaves contain yellow matter deeper in, below the red. Leaves remaining yellow, are so because the oxydation which turns green leaves first yellow and then red, has been arrested in them at the yellow stage. (This appears to me by no means applicable to such yellowed leaves as the American Hickory and tulip Poplar.—J. W.)

Comptes Rendus, lxi, 188. Frémy reports that he obtains pure chlorophyl as follows. By agitating hydrate of alumina with the ordinary alcoholic solution of chlorophyl, a green paste or lac is obtained, a fatty substance which accompanies chlorophyl being left in the alcohol. This green paste being afterward boiled in alcohol, the latter takes up from the alumina pure chlorophyl, which is deposited when the alcohol is afterward sufficiently diluted with water. Chlorophyl thus purified, being boiled long enough with hydrate of baryta, is decomposed into phylloxanthin, (a neutral body, analogous to glycerin,) and phyllocyanate of baryta. The mass of precipitate containing these two substances, being treated with alcohol, the phylloxanthin is dissolved, and is obtained by evaporation in crystals, yellow plates, or reddish prisms. Phyllocyanate of baryta, treated with SO^3, yields its acid, which is insoluble in water, but forms an olive colored solution in alcohol or ether; dissolved in SO^3, or HCl, it gives liquids, which according to their concentration are green, reddish, violet or beautiful blue. Frémy does not consider chlorophyl a simple mixture of its constituent substances.

Comptes Rendus, lxi, 371. M. E. Filhol shows that the treatment of chlorophyl by acids decomposes it, and produces substances not preëxisting. No substance, therefore, got from chlorophyl, which has been even in a feeble degree subjected to acids, can be considered as a constituent of chlorophyl.

Comptes Rendus, lxi, 436. M. A. Trécul observes what he thinks to be naturally crystallized chlorophyl, in cells in the bark of Lactuca Altissima.

Liebig & Will's Jahresbericht, 1863, p. 561. Stein remarks that the red spots in the flowers of Aesculus Hippocastanum, also the flowers of Aesculus Pavia, which are first yellow then red, are turned green by alcoholic solution of soda. In other red flowers the red is turned green by alkalies, but is turned blue by the acetates of alumina, magnesia, or protoxyd of manganese. This red coloring matter appears to be the same as that of such blue flowers as Hyacinthus botryoides and Centaurea cyanus, which is reddened by alcoholic hydrochloric acid, and then again turned green by alkalies, and blue by the above named acetates. Stein supposes the blue coloring matter to be formed by combination of the red coloring matter with a base, namely lime, since the ashes of cockle contain much lime. (Centaurea cyanus, immersed directly in ammonia, becomes green, apparently by optical combination of yellow produced by the alkali with the blue remaining unchanged.—J. W.)

Jahresbericht, 1865, p. 628. F. V. Jodin finds that green leaves killed by alcohol, or by heating in closed vessels to 100° C., are rapidly bleached when exposed to light and air, but retain their color if kept in the dark.

Journal für praktische Chemie, xcv, 219. Vohl took, in 1856 horse chestnut leaves, which had been killed by a strong night frost, and laid them away in a close stoneware jar with water, so as to allow them to rot in the dark. In 1865, nine years later, he took out those leaves, and by treatment, first with ether, then with alcoholic ether, extracted vegetable wax and chlorophyl, thus proving chlorophyl to be a tolerably permanent body, when not exposed to light or air.

Art. XXII.—*On a Modified Form of the Nitrate of Silver Test for Arsenic Acid;* by Charles E. Avery, Student in the Massachusetts Institute of Technology.

Since arsenate of silver is slightly soluble in an aqueous solution of nitrate of ammonium, and readily soluble both in ammonia and dilute nitric acid, it is not easy to detect small quantities of arsenic by means of nitrate of silver, as usually employed, unless the test be applied with extreme care. It is evident, however, that if the liquid to be tested for arsenic acid,—as for instance the nitric acid solution of a spot or mirror of metallic arsenic,—could be charged with a salt, or acidulated with an acid incapable of dissolving arsenate of silver, it would be possible to test for arsenic without special precautions. At the suggestion of Prof. F. H. Storer, I have tested this idea by experiment.

All acids which I have tested in this regard exert some solvent power; but those having very little solvent action may be used almost as though they had none. I find, in fact, by experiment, that the addition either of acetate of sodium, acetate of ammonium, or Rochelle salt, to a mixed solution of arsenic and nitric acids, is sufficient to ensure the immediate precipitation of arsenate of silver, when ammonio-nitrate of silver is introduced.

By placing a small quantity of a nitric acid solution of arsenic acid upon a watch glass, stirring into it a few drops of a strong solution of either of the alkaline acetates or of Rochelle salt, and then adding a drop or two of ammonio-nitrate of silver, the characteristic brown-red precipitate of arsenate of silver is at once thrown down, even when the solution under examination contains comparatively little arsenic.

The acetates are to be preferred to the double tartrate; for, unless there be nitric acid enough in the liquor tested to set free all the tartaric acid, white bitartrate of potassium separates on agitation, and obscures the reaction.

Instead of the acetates or tartrate, recently precipitated carbonate of silver may be employed to neutralize free nitric acid in testing for arsenic acid. If the nitric acid solution be poured upon an excess of freshly precipitated carbonate of silver, the red arsenate, instantly formed, shows clearly on the ground of snow-white carbonate; this is a striking reaction, and therefore a delicate test. Oxyd of silver, when tried in a like manner, gave no results of value.

Experiments made with sulphate, succinate, and nitrate of ammonium, served merely to establish the superiority of the acetates and carbonate, as above described; and to show that arsenate of silver is less readily precipitated in the presence of sulphate, and especially nitrate of ammonium, than from solutions of either of the other salts employed in my experiments.

When present in relatively large quantity, arsenic acid readily precipitates silver from a solution of nitrate of ammonium and ammonio-nitrate of silver, but the color is uncertain; the same objection applies to the succinate of ammonium.

The experiments were varied by changing the proportions of the reagents so that the several results stated above are deduced from many trials. The same tests being tried, with phosphoric in place of arsenic acid, the alkaline acetates gave the best results.

The following quantitative experiments were then tried, to determine the relative solubility of arsenate of silver in solutions of acetates of sodium, Rochelle salt, and nitrate of ammonium. In experiment No. 1, 20 grms. of crystallized acetate of sodium were dissolved in a mixture of 100 c. c. of water and 60 drops of a saturated solution of arsenic acid. In No. 2, 20 grms. of Rochelle salt and in No. 3 a like quantity of nitrate of ammonium, were dissolved in similar mixtures.

Each of the solutions was nearly but not quite neutralized by adding carbonate of sodium; and ammonio-nitrate of silver was then added to them from a dropping flask. A slight milkiness, due probably to the presence of chlorine in the carbonate of sodium, was produced by the first drop in each of the three solutions; but this cloudiness had a distinct red tinge in the tartrate solution, (No. 2,) while it was bluish white in the acetate (No. 1) and the nitrate (No. 3). When twelve drops of the ammonio-nitrate had been added, a distinct red precipitate had formed in No 1, (acetate,) and a very strong one in No. 2, (tartrate); but no change appeared in No. 3, (nitrate). The precipitate in No. 2, showed white streaks, or layers, at the moment of its formation (probably of bitartrate of potassium), which soon disappeared.

Adding more of the ammonio-nitrate to No. 3, with occasional drops of arsenic acid solution, to keep the mixture acid, a purple cloudiness appeared, which increased up to the sixty-fifth drop, but no precipitate fell.

In a repetition of No. 3, (nitrate,) no precipitate fell until 80 drops of the ammonio-nitrate of silver had been added; when it reached 85 drops, the precipitate was exceedingly voluminous. In three other experiments, the liquids were acidulated with tartaric acid. In experiment A, 75 drops of a strong solution of arsenic acid, 125 c. c. of water, and 25 grms. of crystallized acetate of sodium, were mixed, neutralized with carbonate of sodium, and tartaric acid added. In B and C, 25 grms. of Rochelle salt and of nitrate of ammonium were respectively substituted for acetate of sodium.

Seven drops of ammonio-nitrate were then added to each of the solutions. In A and B, the red-brown arsenate precipitate appeared; but in C, no precipitate was formed except the light cloudiness due to impurities.

Boston, Dec. 1, 1868.

ART. XXIII.—*Notices of papers in Physiological Chemistry*—No. II; by GEORGE F. BARKER, M. D.

5. *On the formation of Sugar in the liver.**

[Continued from page 32.]

(43.)† In a paper on the origin of the sugar of the chyle, published June 28, 1858,‡ COLIN calls attention to the large amount of saccharine matter produced in the intestines. A horse, for example, consuming daily 5 kilograms of hay, as much straw, and 3·6 kilos. of oats, obtains from this food, according to Boussingault, 6196 grams of sugar, of starch and of other analogous principles, capable in great measure, of being converted into glucose and dextrin. A fraction of this mass enters the portal vein, goes to the liver, and thus reaches the general circulation; another fraction, mixed with the chyle is absorbed by the lacteals, and poured into the blood; so that this fluid finally receives the whole of the absorbed products. Is it a matter of surprise, therefore, that sugar should be found in the chyle? and can its intestinal origin be doubted? Poisseuille and Lefort have asserted that the sugar found in herbivorous chyle is brought by the lymphatics and the arteries from the liver; though they have given no proof of this singular assertion. To account for the presence of sugar in the chyle they contend, 1st, that it is carried there by the blood and lymph; 2d, that the lacteals cannot absorb it, even when ready formed; 3d, that the chyle is simply an intestinal lymph, to which fatty matters have been added; and 4th, that the glucose found there is in small quantity, much inferior to that existing in other lymphatic vessels. As to the first point, the liquid taken from the great chyliferous trunks passing to the *receptaculum chyli* of a carnivore, as also the fluid drawn from the large lacteal vessels which accompany the mesenteric arteries of ruminants fed on meat, is evidently pure chyle unmixed with lymph; and, as it contains sugar, this sugar must have come from the intestine. To controvert the second assertion, nothing is easier than to show that the lacteals absorb saccharine substances with great facility; since the liquid

* From this point, the range of the discussion widens to include the amyloid function not only of liver tissue, but also of other tissues. The obvious bearing of these facts on the glycogenic theory, is a sufficient reason for noticing them in this connection.

† Through an oversight, paragraphs (43) and (44) in the January number, were prematurely inserted. They should be numbered (55) and (56), and placed in their proper order.

‡ C. R., xlvi, 1264.

taken from a thoracic fistula after a saccharine food, shows a gradually increasing proportion of sugar as the absorption goes on. In the third place, physiologists generally agree in considering the chyle as the product of an absorption effected by the intestinal villæ; they think with reason that the chyle comes from the food because of its fibrin and albumin, its fatty matters and salts. Why does it not derive its sugar also from this source? But lastly, it is not true that chyle contains less glucose than lymph. The authors quoted have compared the one liquid taken from a herbivore, with the other from a carnivore; the chyle from a mutilated and dying cow with the lymph taken from a dog. Colin, on the other hand, as a result of experiments on more than 30 animals, cows, bulls, rams, pigs, and dogs—the two fluids being collected at the same time—finds that the amount of sugar is sometimes equal in both, sometimes unequal; but that in the latter case, the difference is always in favor of the chyle. The absolute quantity of glucose in the chyle is somewhat variable; it is less with herbivora than with flesh-eaters, since in the former the chyle is largely diluted. In the solipeds and in ruminants fed on hay and straw, it oscillates from 130 to 160 milligrams to the 100 grams of liquid; in the carnivora fed exclusively on meat, from 120 to 140. It increases rapidly when the food is rich in sugar; with a dog previously fed on meat, it rose from 137 to 205 milligrams within two hours after the ingestion of a liter of milk containing 40 grams of glucose, and then returned to the normal quantity. In the case of a horse whose chyle, when fed on hay and straw, contained 150 milligrams of sugar in 100 grams, it rose to 214 milligrams in one hour after giving the animals 200 grams glucose in several liters of water; and to 259 milligrams two hours later. Moreover, the activity of digestion affects the amount of sugar. The chyle of a bull, having a thoracic fistula from the mesenteric lacteals, and another from the lymphatics of the neck, showed at first 104 to 110 milligrams of sugar; as the animal became feeble it fell to 84, then to 66, and at death only traces could be detected. Lastly, compared with the lymph, the quantity of sugar varies very little; in a bull. 100 grams of chyle contained 106, of lymph 102 milligrams of sugar; horse A, chyle 149, lymph 123 milligrams; horse B, chyle 141, lymph 112; mare, both 158 milligrams; a dog, chyle 128, lymph 152; a second dog, both 135.

(44.) Poisseuille and Lefort replied to Colin, July 19,* stating that he had evidently confounded the results of separate experiments made by them. His statements refer solely to

* C. R., xlvii, 112.

the experiments on fasting carnivora (Exp. A) where it is stated that "the sugar found in the lacteals comes from the lymphatics of the liver;" nowhere is this fact stated of the herbivora in full digestion. In these animals glucose was recognized (Exp. D) not only in the chyle but also in the blood of the inferior and superior venæ cavæ, in the mucus of the small intestine, in the intestinal walls, in portal blood, etc. It was distinctly stated that the sugar in the intestinal walls might have come from the intestinal contents. As to Colin's results, by which he proves that the chyle contains more sugar than the lymph, in opposition to the opinion they expressed, Poisseuille and Lefort leave the matter to the Commission having the papers to report upon.

(45.) On the second of August, BERTHELOT communicated a paper to the Academy* on the transformation into sugar of various proximate principles contained in the tissues of invertebrate animals. The animal matter which enters into the composition of the vertebrate skeleton is markedly different from that contained in the invertebrate skeleton. In the former it is insoluble in cold water, soluble in alkalies and much resembles albumin; in the latter, two distinct varieties exist, one of which resembles horn very closely, while the other has a remarkable identity with cellulose, the chief constituent of vegetable tissues. Chitin is an example of the former; the substance forming the mantle of tunicate molluscs, of the latter. Both are insoluble in hot and cold water, in alcohol, acetic acid, etc.; are not attacked by concentrated boiling potassic hydrate nor by dilute mineral acids. Neither of them presents any resemblance to albumin. When the tunicate envelop is obtained free from nitrogen, it has the composition of cellulose; chitin however, contains, even when purified, $\frac{1}{15}$ of its weight of nitrogen. The one is an isomer of cellulose; the other has only a certain analogy with it. Thus far, no more intimate bond of union between the chemical functions of these bodies and those of vegetable cellulose had been obtained. Berthelot sought such a connection by boiling for several hours with concentrated hydrochloric acid, the envelop of an ascidian (*Cynthia papillata* Say) then boiling again with a potash solution of 32° B., washing thoroughly, drying and analyzing it. Numbers were obtained identical with those given by cellulose; but since this material is evidently distinct from that substance, not being altered by boiling with dilute mineral acids for many weeks even, he proposes for it the name *tunicin*. When treated in the cold with concentrated sulphuric acid, however, it slowly

* C. R., xlvii, 227.

dissolves; and on dropping the solution into 100 times its weight of boiling water, saturating by chalk, filtering and evaporating, a syrupy liquid, browned on heating with potassic hydrate, reducing the copper-tests, and fermenting with yeast, is obtained. Chitin when treated by the same process, also yields glucose.

(46.) Some experiments on the accurate determination of sugar by the fermentation process in an improved apparatus, were communicated by POISSEUILLE to the Academy on the 6th of December.* The copper-test often fails to give a precipitate in liquids containing sugar; and the saccharimeter is useless when the liquid to be tested is colored. The fermentation test then is the only one left; and this is accurate only when certain precautions are employed. The special apparatus devised by Poisseuille consists of a graduated tube open at both ends, and having a capacity of from 18 to 100 c. c. The upper end is closed with a cork, through which passes a funnel tube with a lateral opening near the bottom; the lower end is enclosed in an india rubber tube, closed below, in which it slides freely, the object being to increase in this way the capacity of the apparatus. This caoutchouc tube is 17 to 18 centimeters long. The amount of sugar in the liquid to be tested having been approximately determined by a copper-test, a suitable graduated tube is selected, a known volume of the decoction is placed in it, some yeast diffused through distilled water is added, the whole is corked and placed in a vessel of water of the desired temperature. As the fermentation goes on, carbonic acid gas is disengaged and forces the liquid up the central tube; it falls again, however, on lowering the rubber tube. When the experiment is concluded, the gas is easily measured, the necessary corrections made, and the amount of sugar calculated. If the contained gas be agitated with a solution of potash introduced through the funnel tube, the amount of carbonic acid evolved may be more accurately estimated by absorption.

(47.) In a subsequent note read Dec. 27,† POISSEUILLE mentions that the liquids used in his apparatus should be previously saturated with carbonic acid gas at a temperature of 32° C. After the fermentation, the apparatus is placed in water at this temperature, and the gas measured. To free the liquids used from contained carbonates, they are slightly acidulated with tartaric acid; after gas ceases to escape, they are placed in the apparatus.

* C. R., xlvii, 906.

† C. R., xlvii, 1058.

(48.) On the 10th of January, 1859, BERNARD read a memoir before the Academy,* on a new function of the placenta. "The object of my communication" he says, "is to establish anatomically and physiologically the fact that beside the various uses without doubt subserved by the placenta, it is designed to accomplish the glycogenic function of the liver during the early stages of fœtal life, before this organ has acquired the structure which fits it for doing this work." He had in 1854 recognized the fact that the glycogenic function did not commence until a comparatively late period of intra-uterine life, though from the commencement of organization, the fœtal tissues contain either sugar or glycogenic matter, apparently as a necessary condition of their development. Now it has been demonstrated experimentally that in mammals this glycogenic matter does not come from the mother ; a fact obvious enough in birds, where the fœtus is separately developed. Hence this glycogenic function, performed later by the liver, must, in the earlier stages of intra-uterine life, be either diffused through various tissues, or located temporarily in unknown embryonic organs which cease to act when the liver becomes active. Experiment has shown the latter to be the true hypothesis ; there exists, even before the liver can act, a true hepatic placental organ, which produces glycogenic matter; and this provisional function disappears at the precise period of intra-uterine life when the liver becomes capable of performing it. The multiple placentas of ruminants were first examined, since they were readily procurable in the *abattoirs* of Paris. But though very numerous experiments were made with calves and lambs at all stages of intra-uterine life, no traces of glycogenic matter could be detected in any part of the placenta. Rabbits and guinea pigs were then used for the experiment; and in the placentas of these animals was a whitish substance formed by the agglomerated epithelial or glandular cellules, which cellules like those of the adult liver, were filled with glycogenic matter. The mass of these cellules seems to be situated between the maternal and the fœtal portions of the placenta; after attaining full development, it becomes atrophied, and disappears as the fœtus approaches birth. The placentas of these animals have therefore two functions: the one vascular and permanent until birth, the other glandular, with a more limited duration. But why the failure with ruminants? Believing that the cause was to be found not in the absence of this function, but in a variation of it, Bernard undertook a re-examination of the question ; and this with complete success. A remarkable and

* C. R., xlviii, 77.

unexpected result was obtained; he found that among the ruminants, while the vascular portion of the placenta—represented by the multiple cotyledons—accompanies the allantois and spreads out upon its external surface, the glandular portion of this organ is separate from it, being developed upon the internal surface of the amnion. In rodents therefore, and in other animals having a simple placenta, the glandular and vascular portions of this organ are intermixed; while in the ruminants, on the contrary, these two portions of the placenta are developed separately upon distinct membranes, and can therefore be separately studied during their evolution. By this anatomical disposition, it may be clearly proved that the vascular portion continues to grow until birth; while the glycogenic portion, attached to the amnion, is largest during the earlier periods of gestation, reaching its highest development during the third or fourth month of intra-uterine life; and then, that it gradually atrophies and disappears, so that at birth no trace of this temporary hepatic portion of the placenta exists. During the whole period of growth of this organ, the fœtal liver has neither structure nor function; as soon however as the liver is developed and its cellules begin to secrete glycogenic matter, the hepatic organ of the amnion disappears. These hepatic plates of the amnion appear among ruminants, very early in fœtal life; they are developed on the internal surface of this membrane, beginning near the line joining it to the skin and extending over its entire surface. The presence of glycogenic matter in them may be easily shown at all periods of their development by means of iodine, which colors the contents wine-red and thus defines the cellules. To prepare the glycogenic matter, the amnion is placed in hot water, and the plates are then removed by trituration in a mortar. On boiling the mass in water and precipitation by alcohol, this substance is obtained, exactly as in the case of liver-tissue. The glycogenic matter thus prepared from the amnion has the most perfect identity with that made from the liver; it dissolves in water, giving a milky solution, from which it is thrown down by alcohol and by glacial acetic acid; iodine colors it an intense wine-red,—the color disappearing by heat and reappearing when cool—and it changes into dextrin and fermentable sugar by the action of diastasic ferments, and of acids. Bernard then gives an elaborate account of the histological development of these plates and their subsequent fatty degeneration. The following are the conclusions reached by his researches: 1st, there exists in the placentas of mammalia a function now first announced, which appears to supplement the glycogenic function of the liver during the early stages of intra-uterine life. This function is localized in certain glandular or epithelial elements of the placenta, which elements in some animals are mixed with the vascular part of this organ, but are separated from it in the ruminantia, forming on the amnion plates which are epithelial in appearance. 2nd, this hepatic placental organ, by permitting the direct study of the production of glycogenic matter in an isolated anatomical element, confirms and completes the statement long ago made, that the formation of starchy matter is a function common to both kingdoms of nature. We see here this matter accumulating round the animal embryo, precisely as in the seeds of plants, it surrounds the vegetable germ. 3d, the glycogenic function in animals, begins at the outset of embryo life, and before the organ in which it is located in adult life is developed: it is then located in a temporary organ belonging to the appendages of the fœtus. 4th, the question arises, is the biliary function which is possessed by the adult liver, also performed by the placental hepatic organ now described? And again, do the same glandular elements of the liver perform both functions, or is there a distinct kind of cellule for each? Bernard thinks that this question may be settled by researches on the embryonic development of these functions now in progress.

(49.) SERRES followed Bernard with some remarks on the glycogenic bodies found in the umbilical membranes of birds.* In the course of the second or third day of incubation, as is well known, a vascular membrane covering the entire surface of the vitellus, is developed. These capillary vessels originate from separate centers called the sanguineous points of Wolff; they gradually extend their ramifications and thus form patches of radiating vessels, which anastomose about the thirtieth or fortieth hour forming a beautiful vascular network. Between these sanguineous patches there are small glandular bodies covering the entire surface of the umbilical membrane. Under the microscope they may be detected as early as the twen y-fifth hour of incubation, being distinguished by their white color from the vascular patches; they are more distinct about the 35th hour. More than five hundred of these bodies have been counted; they continue to increase up to the sixth day when they are hidden by the vascular network. The nature of these bodies as well as the function which they perform had thus far not been made out. After listening to the remarkable paper of Bernard, however, Serres could not doubt that these bodies were completely analogous to the glycogenic

* C. R., xlviii, 86.

glands of the placenta; birds being a class of animals in which the placenta is represented in part by the umbilical membrane, in part by the allantois. May it not be asserted therefore, that like mammals, birds also have a transitory liver; especially since the inverse relation of this organ to the actual liver is also true of them? The liver in birds does not act till the 11th or 12th day of incubation; and precisely at this time the umbilical membrane is replaced by the allantois which contains no glycogenic glands.

(50.) A second paper by BERNARD was presented to the Academy on the 14th of April,* on the relation of the glycogenic matter to the development of certain fœtal tissues before the appearance of the liver function. The object of his researches was to ascertain what particular histological elements are accompanied in their development by glycogenic matter. For this purpose the organs examined were divided into two classes: 1st, the exterior or limiting organs, comprising the cutaneous and mucous tissues; and 2d, the interior or contained organs, including the osseous, muscular, nervous, and glandular tissues. On examination, Bernard found that the first class of tissues, i. e., all exterior epithelial membranes, whether constituting a mucous or a cutaneous surface—contain glycogenic matter at certain periods of fœtal life. In the cutaneous surface, this substance is found infiltered into the tissues of the skin, as well as collected in the cellules of the epithelium which covers it. Certain animals, as the pig, show this very clearly; while with the rabbit, the cat, and the calf it is more difficult to recognize. The existence of the glycogenic matter in the skin of a fœtus may be demonstrated by scraping it with the blade of a sharp knife, and examining the matters removed under the microscope. Among the histological debris are seen the cellules both containing and surrounded by granular matter which is turned wine-red in color by an acidulated solution of iodine. In this way, the disposition of the glycogenic matter in the skin at all periods of development, may readily be studied.† This microscopic evidence has always been confirmed chemically, by making a decoction of the tissue; an opaline solution is thus obtained, which is colored wine-red by iodine, and is precipitated by alcohol and by glacial acetic acid in ex-

* C. R., xlviii, 673.

† To show to the eye the distribution of the glycogenic matter, Bernard places the entire fœtus in an acidulated alcoholic solution of iodine. The horny extremities, the cutaneous orifices—as the anus and the nasal openings—the eyelids, the ears, and the cornea are most colored. The glycogenic matter in the placenta may also be shown in this way.

cess; it is also changed into sugar very readily by the action of acids and ferments, and in a word, possesses all the characters of that obtained from the liver or the placenta. The horny appendages of the skin, too, contain glycogenic cellules, which disappear as development progresses. In the skin itself, the glycogenic matter disappears rapidly; being absent toward the third or fourth month of intra-uterine life, in calves 25 to 30 centimeters long, in which well defined epithelium is present. Even when absent from the cellules, it is found infiltrated into the skin. Mucous surfaces also, at certain stages of development, show glycogenic cellules. With embryo pigs, calves, or lambs 3 to 6 centimeters long, they may be observed in the mouth, tongue, pharynx, stomach, large and small intestine, occurring in the epithelium surrounding the villosities. The mucous surfaces of the air passages, and also of the genito-urinary passages show the same fact. It thus appears that all the exterior limiting surfaces possess during fœtal life, before the epithelium is fully developed, a glycogenic evolution. Of serous membranes however, the same cannot be said; no glycogenic matter having been found in the epithelium lining the pleura, the peritoneum or the arachnoid. With regard to the internal or contained tissues, they form a separate group, being with few exceptions, unaccompanied by glycogenic matter during their development. The osseous and nervous systems contain no glycogenic matter at any stage; the brain, the spinal cord, the bones—freed from periosteum—and the cartilages, not only in the human fœtus, but in that of the calf, sheep and rabbit, though examined in many ways and at all periods of growth, gave no result. Muscular tissue however, appears to form an exception, since it contains glycogenic matter; but the arrangement of this matter is different from that just noticed. In the very young embryo of the calf, for example, 2 to 4 centimeters long, embryonic cellules not colorable by iodine are found in the positions soon to be occupied by muscular tissue; a little later, when the fœtus is from 15 to 20 centimeters long, and when the histological elements of the muscle appear, the muscular fiber is seen as a tube containing nuclei mixed with a granular matter colored red by iodine; this is best seen in the fœtus of the cat. As the fiber develops it soon becomes striated; but the glycogenic matter though lessened in amount is not absent, but seems infiltrated into the tissue. In no case did this glycogenic matter appear organized, nor was it contained in cellules. This substance is also present during the development of the smooth muscles of the heart and of the intestines; in the fiber itself it is sometimes difficult to demonstrate this, but with a decoction of the tissues the reaction is easily obtained. This glycogenic matter remains in the muscular tissue during the entire period of intra-uterine life, and disappears very rapidly after birth by muscular and respiratory action. The glandular system too, contains during fœtal life no glycogenic matter ; the salivary glands, the pancreas, the glands of Lieberkühn, the spleen, and the lymphatic glands having been examined. One glandular organ, the organ which in after life performs the glycogenic function, must be excepted. Though in its evolution, the liver is not accompanied by glycogenic matter, yet toward the middle of fœtal life it becomes histologically developed and acts as a biliary and glycogenic organ. It seems designed therefore, to continue in the adult a fœtal function previously performed by other and temporary organs. Glycogenic matter then, plays as important a part in the organic development of the fœtus as it performs in the nutritive function in adults. "We know in fact that it ceases to be produced in the liver whenever any morbid condition arrests the nutritive function. The substance then which effects the evolution of the fœtal organs continues to be concerned in their nutrition in the adult. This fact establishes a direct connection between organic development and those nutritive phenomena which under various aspects, are only a continuation of it." The phenomena of nutrition in the adult may therefore be elucidated by studying the phenomena of fœtal evolution.

(51.) A paper by Rouget was presented to the Academy April 18th,* calling attention to previous memoirs communicated to the *Société de Biologie*, in which he had shown that the amylaceous substance in the amnion and placenta is not the product of a peculiar organ and is not contained in special glycogenic cellules, but occurs in the more or less modified epithelial cells of these membranes. Farther researches upon other epitheliums had detected this substance in the epidermic cells of the skin, the soft palate, the tongue, the stomach, and in all the cylindrical cells of the epithelium covering the surface of the large, and the villi of the small intestine. With certain animals, all the intestinal epithelium is filled with amylaceous matter three or four days before birth ; though at this time the liver, completely developed for a long period, furnishes this matter abundantly. Finally, finding epithelial cells containing glycogenic matter on the lingual and vaginal mucous membranes even after birth, Rouget concludes that the presence of amylaceous matter in any tissue does not argue a special

* C. R., xlviii, 792.

function, but is simply due to the peculiar constitution of the tissue itself. From these facts it is clear that to the proteic and fatty substances of food, now regarded as the only ones which form animal tissues, must be added amylaceous matters, which form tissues as truly in the animal as in the vegetable kingdom.

(52.) In a letter from Schiff, read to the Academy May 2d,* while he agrees with Bernard that a portion of the granulations in the hepatic cellules is animal starch, he claims to have discovered this fact himself in 1856, and to have published it in the Archives de Tübingue for 1857, in a communication dated the 18th of March. Two sorts of granulations surround the nucleus in the hepatic cellules. The first sort are large, have well marked outlines, are almost black in color, soluble in alcohol and ether and become diffluent in acids and alkalies. From 8 to 20 of these are contained in a single cellule. They seem to be fat globules. The second sort are very much smaller, are perfectly rounded, the contour being distinct though pale; they are insoluble in alcohol and ether, take a brownish-yellow color with an acidulated tincture of iodine, and resemble in general the starch of the compositæ, as the dahlia and arnica. They are much more numerous than the others, and were regarded in 1857 as animal starch, for the following reasons: (1) when the liver continues to produce the glycogenic matter, but the ferment fails in the blood so that it is not converted into sugar, the hepatic cellules become enormously distended with this substance; (2) when placed in a liquid containing a ferment, these cellules gradually diminish, while the liquid becomes charged with sugar; the action ceases when the cellules are empty; (3) with batrachians normally, and with mammals and birds, whose liver is saccharine at death, the globules are very numerous; the number however, is less than with batrachians in winter; (4) the number of globules at death is always related to the amount of sugar which the liver can furnish; (5) in passing into sugar, these globules become yellowish drops, soluble in water, insoluble in alcohol; evidently animal dextrin; (6) when in the spring the ferment reappears in the blood of frogs, the globules diminish and the hepatic cellules are filled with these drops, and the liver changes color, passing from dark brown to yellowish or reddish; (7) this change of color takes place at different periods with different species; with *Rana esculenta* it is very late, the liver not becoming saccharine in Switzerland till the end of May; adults pass through it sooner than young animals, females earlier than males; (8)

* C. R., xlviii, 880.

elevation of temperature alone will not produce the ferment and transform the starch; a large number of frogs were kept for more than six months and no sugar was formed though the hepatic cellules were full of starch; (9) in some cases these globules may be absorbed without producing dextrin or sugar; they appear to suffer a change which gives rise to oxalic acid; (10) the diseased livers of mammals, of birds and of frogs which contain no starch, are much darker than those in which the glycogenic transformation is carried on; (11) during hibernation, both the ferment and the hepatic starch are present, though the latter is diminished in amount; (12) nitrogenous tissues treated with cane sugar and sulphuric acid become yellow and then reddish-purple; since therefore the walls of the hepatic cellules and not their contained globules, are colored by this treatment, the latter do not contain nitrogen. Bernard's glycogenic matter according to Schiff, is not a pre-existant anatomical element, but a product derived from this by the processes employed.

(53.) BERNARD followed this paper by a recapitulation of the dates of his own discoveries.* First, in 1854, he announced the existence of glycogenic matter, under the qualified name "a sort of animal starch," in various fœtal mammalian tissues, though the substance itself was not then isolated. At the same time, he compared at length, the part which saccharine and amylaceous matters play in the organic evolution of animal and vegetable tissues, and concluded with the opinion that these substances seemed to enter as an essential condition of development of the tissues of both kingdoms of nature. Second, in 1855, he published an experiment fundamental to the glycogenic theory; showing that when the sugar of a healthy liver was removed by washing, the saccharine substance was renewed; thus proving that the sugar is not formed by the splitting up of certain blood elements, but by a fermentation analogous to that taking place in vegetables. Third, in 1857, the isolation of the glycogenic matter was communicated to the Academy on the 23d of March; and, since these experiments, there for the first time described, are referred to in Schiff's paper of the 18th of March above mentioned, it is evident that his paper was antedated and is actually posterior in date to this. The glycogenic matter was here called *amidon animal*; and it was shown to be an isomer of vegetable starch, having all its physical, chemical, and physiological properties, changing to dextrin and sugar by acids and ferments, rotating the polarized ray, etc. The existence of the ferment both in the liver-tissue

* C. R., xlviii, 884.

and in the blood was there shown. The mechanism of the function had been announced before Schiff's experiments appeared. "M. Schiff says himself that he agrees with me; but he thinks that he has more accurately distinguished and located microscopically the hepatic starch, and so has proved my views better than I have myself. If this be so, I cannot but be satisfied; but I see not how this gives him any claim to priority."

(54.) On the 30th of May, ROUGET communicated another paper to the Academy,* on the amorphous amylaceous matter contained in the vertebrate and invertebrate fœtal tissues. This substance—for which he proposes the name *Zoamylin*—is not granular, but consists of a plasma holding in suspension fatty or nitrogenous granulations. Moreover, ossifiable cartilages must be added to the embryonic tissues containing amylaceous matter; though this is not found in the substance of the cartilage, but only in the cells, which in a fœtal lamb 1½ to 2 months old, are colored rose-violet with iodine. With this amylaceous plasma too, the epithelial cells of the digestive, respiratory and genito-urinary passages, of the interior of the eyelids and even of the cornea, are filled. With young ruminant embryos, the cartilaginous, muscular, and epithelial elements of which contain zoamylin, no trace of glycogenic cellules appears on the surface of the amnion. When these are present their mode of development, their form, constitution and general appearance are precisely like those of the cellules of the horny layer of the epidermis; and hence prove the amnion to be an extension simply of the skin. The presence of amylaceous matter in the amnion and placenta, therefore, is not a special case, since all fœtal tissues contained it. No new hepatic organ exists, nor has the placenta any new function. The presence of this substance is the evidence, not of a new function in an organ, but of a new property of tissues. The production of sugar is not the object but the consequence of the existence of zoamylin in the organism. The sugar accumulated by the urinary secretion in the allantoic and amniotic fluids, is a result of the destruction of the fœtal zoamylin, precisely as urea is produced by the metamorphosis of proteic bodies.

(55, 56.) Already given as paragraphs (43) and (44).

* C. R., xlviii, 1018.

(*To be concluded.*)

ART. XXIV.—*A New Meteoric Iron—"The Wisconsin Meteorites"—with some remarks on the Widmannstätian Figures;* by J. LAWRENCE SMITH, Louisville, Ky.

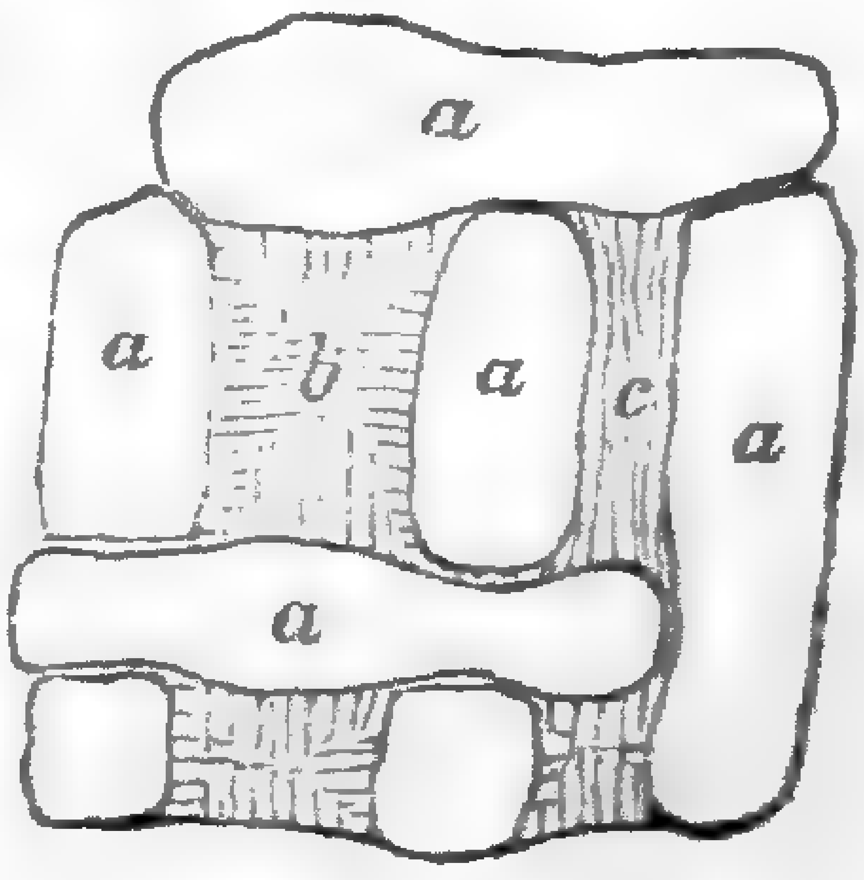
THESE meteorites were first brought to my notice by Mr. I. A. Lapham, of Wisconsin, and his attention was called to them by Mr. C. Daflinger, Secretary of the German Nat. Hist. Society of Wisconsin. They were discovered in the town of Trenton, Washington county, Wisconsin, and I have called them the "Wisconsin meteorites." Up to the present time, fragments have been found, indicating that these meteorites were of the same fall and separated at no great elevation. They were found within a space of ten or twelve square yards, very near the north line of the 40 acre lot of Louis Korb, in latitude 43° 22′ N., and longitude 88° 8′ west from Greenwich, and about 30 miles northwest of Milwaukee.

They were so near the surface as to be turned up with the plough; they weigh 60, 16, 10, and 8 pounds respectively, and present the usual pitted and irregular surfaces.

The largest of the meteorites in its extreme dimensions, is 14 inches long, 8 inches wide, and 4 inches thick, weighing 62 pounds. Its specific gravity is 7·82, and composition,

Iron	91·03
Nickel	7·20
Cobalt	0·53
Phosphorus	0·14
Copper	minute quantity
Insoluble residue	0·45

A polished surface when etched gives well marked Widmannstättian figures. There is something, however, peculiar about the markings on this iron, which is doubtless common to other irons, but which has heretofore escaped my observation, and I cannot discover, in a hasty investigation, that it has been noticed by others. My attention was called to this peculiarity by Mr. Lapham, on a slice of the meteorite I sent him etched; should these markings be entitled to a separate notice, I propose calling them *Laphamite markings.* The little drawing accompanying this, which is on a somewhat exaggerated scale, will show what they are.



The Widmannstättian figures are *a*, bright metallic, with convex ends and sides; *b c*, of a darker color, are the other markings, usually smaller and with the sides and ends concave. The material of which these dark figures are composed seems to have enveloped the lighter colored portion, which serves to make the dark lines so beautifully conspicuous. A good pocket glass will show that the darker figures are striated, with lines at right angles to the bounding surfaces. When the figure is nearly square, the lines extend from each of the four sides, but when much elongated, as at *c*, they are parallel with the longer sides. Often these lines do not reach the middle of the figure, where only a confused crystallization can be detected. In the interior of the elongated figures, the lines are quite irregular, often running together, and showing a striking resemblance to woody fibre. The nature of these markings may be easily understood. They indicate the axes of minute columnar crystals, which tend to assume a position at right angles to the surface of cooling.

These markings may have been observed by others; and as soon as the subject can be examined on other irons, a better conclusion can be formed.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the wave lengths of the metallic rays.*—THALÉN has published an extended memoir on the wave lengths of the spectral lines of the elements. The author's work does not present any new measurements, but is based upon those made by Angström, which had already been employed for the purpose of interpolation by the writer of these notices. The method of proceeding was however new. Each luminous ray, the wave length of which was to be measured, was in the first place entered either upon Kirchhoff's chart, which extends from A to G, or upon a new chart by Angström and Thalén, extending from G to H. These rays were then transferred to the normal plates of the spectrum furnished by Angström, and finally were entered upon the charts published with Thalén's memoir, each being placed according to its wave length. In some cases the graphical method was employed. The description of the process employed in determining the wave lengths is by no means clear. The spectroscope used was provided with large telescopes, and with a prism of bisulphid of carbon with a refracting angle of 60°. The number of elements examined amounted to 45; of these 23 were in the metallic state, the others being in the form of chlorids. One important result obtained by the author is the proof that the sun's

atmosphere contains titanium. The following elements had, so far as the writer knows, not before been examined with the spectroscope; glucinum, zirconium, erbium, yttrium, thorium, uranium, titanium, tungsten, molybdenum, and vanadium. Appended to Thalén's memoir is a chart upon which the spectra of the different elements are entered upon the plan first employed by Mr. Huggins, so that all the spectra are upon one sheet with the normal spectrum at the top. It must be borne in mind, however, that the lines upon Thalén's map are entered according to their wave lengths and not upon an arbitrary scale. The memoir contains, also, a complete table of the wave lengths of all the lines of the elements examined. With respect to the numerical values of these wave lengths the writer will only remark that they differ widely from those determined by himself in the discussion of Mr. Huggins's scale, in the present number of this Journal.—*Nova Acta Reg. Soc. Scient. Upsaliensis. Series tertia*, vol. vi, fasc. II, 1868. W. G.

2. *On spectral analysis.*—In another memoir on spectral analysis, published in 1866, but which has just reached us, Thalén has given what amounts to a complete history of, and treatise on, the subject. The memoir contains what has long been wanted,—maps of the spectral lines of the elements as seen with a single prism of bisulphid of carbon with a refracting angle of 60°. These maps give the principal lines for each element. They are all upon a single sheet, upon Mr. Huggins's plan, with the solar spectrum at the top for comparison, and with a scale of millimeters. A prism of the bisulphid of 60° has become a sort of normal or standard, easily and cheaply obtained, and extremely convenient for common use and reference. Thalén's chart is therefore a most acceptable addition to our resources, and will, we hope, be published separately.—*Upsala Universitets. Arsskrift.* W. G.

3. *On a normal spectrum of the Sun.*—Fizeau has presented to the Academy of Sciences, on behalf of the author, A. J. Angström, a new atlas of the solar spectrum. This atlas contains six detailed charts, representing different parts of the solar spectrum, drawn from new observations upon which their author has spent five years. The map contains also a description of the violet portion of the spectrum between G and H, which was wanting in the chart of Kirchhoff and Hofmann. Upon these charts the lines are entered simply according to their wave lengths, a method which Fizeau attributes to Angström. That the new charts will prove of the greatest value and interest cannot be doubted. With respect to the originality claimed for their construction, it may be remarked that the method was first proposed by Billet, (Traité d'Optique, vol. i, p. 47,) and that the first extended chart constructed in accordance with it, was presented by the writer of these notices to the National Academy of Sciences, Aug. 7th, 1866. This chart contained 87 lines.—(*This Journal*, vol. xliii, Jan. 1867.) *Comptes Rendus*, lxvii, 946. W. G.

4. *On the red protuberances from the surface of the sun.*—In verifying the observations of Lockyer and Janssen upon the light

of the solar protuberances, Secchi has made some new observations of interest. One protuberance was observed perfectly detached from the sun's border, the ray C being prolonged at both ends by the continuation of the dark ray. At another point the ray C appeared to touch the edge of the sun's disc. Another protuberance appeared and disappeared at intervals. One fact, showing the abundant presence of hydrogen, is that the ray C disappears almost all round the sun, at the same time that the ray F becomes much feebler.

Lockyer finds that the protuberances are simply local accumulations of a gaseous envelope, which completely surrounds the sun. The thickness of this envelope is about 5000 miles, (8000 kilometers,) and it is wonderfully regular in its entire contour, its distance from the sun being sensibly equal at the pole and at the equator.

Secchi gives the thickness of the envelope as 15 seconds in its thinnest part, and thinks that it may be at least 30 seconds on the average. The yellow line near D, as remarked by other observers, is not a hydrogen line, and does not correspond to any dark line in the spectrum. On applying the spectroscope to the examination of Jupiter, Secchi found that the dark band in the red does not coincide with the band C, which we see in our atmosphere. The same astronomer has had opportunities of studying the spectra of several meteors. One of them exhibited a discontinuous spectrum, the principal luminous rays being red, yellow, green and blue, and very brilliant. In the cases of two other very brilliant meteors, the magnesium ray (b?) was superb.—*Secchi in Comptes Rendus*, lxvii, 1018, 937, 1123. *Lockyer in the same*, lxvii, 949.

W. G.

5. *Further observations on the spectra of some of the stars and nebulæ, with an attempt to determine therefrom whether these bodies are moving toward or from the earth, also observations on the spectra of the sun and of comet II*, 1868.—Under the above title Mr. Huggins has brought together a number of interesting and important contributions to astronomical physics and chemistry. In an introduction to the memoir the author discusses the proposition first stated by Doppler in 1841, that the color and intensity of an impression of light, and the pitch and strength of a sound will be altered by a motion of the source of light or sound, or by a motion of the observer, toward or from each other. The introduction contains a concise and clear statement of the theory of the change in refrangibility of light produced by the motion of the observer, or of the luminous body, contributed by Mr. S. Clerk Maxwell. The apparatus employed by Mr. Huggins consisted of an equatorial by Alvan Clark, with an eight inch objective, and a peculiarly constructed spectroscope, the dispersive power of which was equivalent to that of about six flint prisms of 60° and one of 45°. It is only by means of an instrument having a very high dispersive power, that the small difference in refrangibility produced by a small change in the wave length can be measured. With this spectroscope observations were made of the great neb-

ula in Orion and of Sirius. In the case of the nebula, it was found that the brightest of the three lines visible, still exactly corresponded with the brightest of the nitrogen lines seen in the spectrum of the light of the induction spark, taken in pure nitrogen at a tension a little less than that of the atmosphere. The line in the nebula was single, while the nitrogen line is double. When, however, the induction spark was made to pass in front of the object glass of the telescope, the nitrogen line appeared single like that in the nebula. Mr. Huggins calculates that if the line in question be due to nitrogen the nebula is not receding from us with a velocity greater than ten miles per second, as this motion added to that of the earth's orbital velocity would have caused a want of coincidence that could be observed. But if the nebula be approaching our system, its velocity may be as much as twenty or twenty-five miles per second, as part of its motion would be masked by the motion of the earth in the opposite direction. The coincidence of a second line in the spectrum of the great nebula with Fraunhofer's line E was also fully confirmed by the new spectroscope, which increases the probability that this line is due to hydrogen. The author could find no terrestrial line corresponding to the third line of the nebula.

The great intensity of the light of Sirius and the four strong lines in its spectrum, render it well suited for examination. The author selected the line F, and found in numerous comparisons the refrangibility of this line slightly lower than that of hydrogen. From the amount of this difference in refrangibility, taking into account the facts that at the time of observation, the earth was moving from the star with a velocity of about 12 miles per second, Mr. Huggins finds that there is a probable motion of recession of Sirius from the earth amounting to 29·4 miles per second. On examining the spectrum of a solar spot, the author found as he had previously done, that there was a distinct increase in the thickness of Fraunhofer's lines. There was no absorption line in the spectrum of the umbra, which was not present in that of the sun's normal surface, nor was any ordinary solar line wanting.

The spectrum of the comet discovered by Winnecke and by Becquet on June 13th, 1868, was also examined. With two prisms of 60° the spectrum consisted of three very broad bright bands. In two of these the light was brightest at the less refrangible end; the third was of nearly uniform brightness, perhaps somewhat brighter in the center. The bands could not be resolved into lines. The spectrum of Brorsen's comet consisted of three bright bands and a continuous spectrum. By careful comparison with the spectrum of carbon under different circumstances, Mr. Huggins believes that he has shown that the light of the comet II of 1868, is identical with that emitted by highly heated vapor of carbon.—*Phil. Transactions for* 1868, p. 529.

W. G.

II. MINERALOGY AND GEOLOGY.

1. *Recherches Anatomiques et Paléontológiques pour servir à l'histoire des Oiseaux fossiles de la France;* par A. MILNE EDWARDS. Tome I. Paris, 1867. Avec un Atlas en 40 Planches, 1-96.—In 1866 the French Academy of Sciences awarded to this work the great prize of physical sciences for 1865; and from the character of the first volume, which is before us, it was a well deserved reward. The difficulties of the subject are considerable; the want of precise osteological characters applying to the class of Birds as a whole, compelled the author to investigate this subject anew. For this purpose he brought together the essential parts of the skeletons of no less than 800 species of living Birds, representing the principal ones and their types. The sternum, skull and bill, which have been mainly employed in framing present classifications are only rarely found fossil, and when found, are usually so fragmentary as to be almost valueless. The author was therefore compelled to search among the bones of the limbs, (these are often found in an admirable state of preservation,) but presenting such apparent uniformity of character, for structural details, which had escaped his predecessors. This task he has accomplished most successfully, and we now have for the class of Birds, a work which fills what had been a great gap in our paleontological literature. Milne Edwards, Jr., calls special attention to the value of the tarso-metatarsal bones, from which he has attempted in many cases a reconstruction of the bird, based upon the important part which the foot plays in our classifications of Birds; he even goes so far as to assign to this part of the skeleton a value as great for Birds as the dental system is for mammals.

The material originally at his command was not extensive, for collections of fossil birds are not found in the great museums of Europe; what there was on hand in these and several private collections in France were all carefully examined. In addition, he undertook himself extensive explorations mainly at Sansan and in the Department of l'Allier, and succeeded in the course of four years in bringing together a more extensive collection than that of any public museum, composed of over four thousand specimens, from the principal quaternary and tertiary deposits of France. It was however mainly in Auvergne, on the shores of the ancient lakes of that district, then inhabited by an immense number of birds, that the greater part of the remains were found.

The principal modifications of the representatives of the class of Birds which our author notices, he considers as plainly showing that we cannot account for the disappearance of the species living at the time of the quaternary period, by any other causes than those which have extirpated the Dodo from Mauritius, the Pezophaps from Rodriguez, the Dinornis from New Zealand, and the Alca from our own coasts. He finds in the caverns of the quaternary period no generic type not still represented in the fauna of our day, most of the species even being identical with those now living, though many of the species inhabiting France at that period are only found now in the Arctic regions. This he considers a strong argument in favor of the prevalence of a colder climate than at present during the quaternary period.

Among the tertiary Birds no species is identical with those of the present period; several special generic types occur, all of which belong to families now existing; many of the specific types of that time, forming groups which are scantily represented in our day, play a more important part, such as the small family of Flamingoes. Pelicans, Ibis, Flamingoes and gallinaceous birds of large size, analogous to the East India species, lived during the breeding season along the shores of the ancient lakes of Auvergne, as is proved by their eggs, which are found well preserved side by side with the bones of newly hatched young. This and the general character of the avifauna, furnish fresh arguments in favor of the opinion of those who consider the climate of Europe to have been much warmer during the tertiary period than at the present time.

The present volume contains the osteological studies on living birds, which the author proposes to apply in turn to each of the great families of which he has found fossil representatives; he has thus far treated of the "Palmipedes, Colymbides, Longipennes, Totanides and Ceronides; with these the first volume closes. The plates, ninety-six in number, are remarkably well drawn and printed; the iconographic part of the work will prove of great value to Paleontologists, furnishing a starting point for the determination of osteological characters, with an abundance of detail, which cannot fail to be of the greatest service for future discoverers, giving us a complete work on ornithological paleontology, to which the earlier memoirs of Brandt and Owen make such a fitting introduction. A. A.

2. *Report on recent Explorations in the Gibraltar Caves;* by Capt. Fred. Brome.—The explorations recorded in this communication were conducted principally in "Martin's" and St. Michael's Caverns.

Martin's Cave opens on the eastern face of the rock, below O'-Hara's Tower. It is an ancient sea-cave, though now upwards of 700 feet above the level of the Mediterranean. The excavations in this cavern were commenced on the 23d of June, and continued till the 22d of July. There were no traces whatever of any previous attempts at exploration. The first operation was to excavate the dark earth all along, close to the south side, which is from six to three feet in depth. At this depth the diggers came upon a stalagmite floor of varying thickness. Here, after a few hours' work, were found deposited two portions of a lower jaw, supposed to be human; about two bushels of bones of ox, goat, sheep, rabbit, &c.; several bones of birds and fish; two bushels of broken pottery of the rudest or unmarked kind, 57 pieces ornamented; 61 handles and pots; 6 stone axes and 70 flint knives; 1 excellent flint core; 20 lbs. of flint chips; 12 pieces of worked bone; a portion of an armlet and anklet; 10 lbs. of sea shells, and a few land shells, together with three rounded pebbles. On the north side the same class of objects were met with, and in a small chamber on this side, under five or six feet of earth, Captain Brome's son came upon a small chamber containing two ancient swords, one partly imbedded in stalagmite, and both much injured; and on a subsequent occasion, a small enamelled copper plate was found, which appears to have had a design upon it of a bird with its bill open, in the coils of a serpent. The colors are bright, and the object is beautifully made. These interesting relics have been referred by Mr. Franks to the eleventh or twelfth centuries. Excavations were then made in a cavern, situated on the same face of the rock, but a little to the south, and at a higher level than "Martin's" Cave, and named by Capt. Brome the "Fig-Tree Cave," in which similar rude works of art, consisting of fragments of pottery, flint and stone implements, &c., were found.

Captain Brome's greatest interest, however, was centered in "St. Michael's Cave," in which, day after day, numerous human remains were found, some imbedded in the stalagmite, others loose, associated with stone axes, flint chips, and flint knives of the smallest size hitherto met with in the Gibraltar caverns.

On the north side of the upper chamber in St. Michael's Cavern, on breaking up a thick stalagmite floor, a small aperture was discovered. When this had been enlarged sufficiently to admit of Captain Brome's entrance, he found a series of passages and caverns, the extreme traveling distance of which from the entrance was exactly 200 feet. There were no means of access to it, excepting by the aperture by which Captain Brome entered. The walls were snow-white, and the pillars and stalactites of the most variable and fantastic forms. Some of the latter, with the thickness only of a goose-quill, were five feet long! The bearings of the cavern generally run N.W. At the south end of this cavern a perpendicular fissure was discovered, through which came a strong wind. The fissure was about nine inches wide, but one of the men (military prisoners) employed was found small enough to creep through it. He returned with a wonderful story of what he had seen. On the next day, accordingly, Captain Brome sent in one of his own sons, about twelve years old, who entirely corroborated the statements previously given, viz., that there were three caves, the first very small, and about twelve feet from the narrow entrance. At some distance further there was another, about twenty feet square, and still further, a cave as large as the upper St. Michael's first chamber. The distance traveled is 250 feet from the entrance, which, added to the distance (200 feet) traveled in the first discovered cabin, make a total of 450 feet of hitherto wholly unknown caverns in so familiar a locality as the often visited cave of San Migael.

At the date of his last advices, Captain Brome was continuing the exploration of St. Michael's Cavern, with every prospect of further interesting discoveries. But, as he says, "his surmises, that the unexplored caves would yield the same relics as the Genista Cavern, have been verified, and the fact is nearly, if not quite established, that at a former period all the Gibraltar caverns were tenanted by a race having uniform habits of living."—*Brit. Assoc. Rep. for* 1867, 56, 1868.

3. *On Calamiteæ and Fossil Equisetaceæ*; by WILLIAM CARRUTHERS, F.L.S., F.G.S.—After describing the structure of the recent *Equisetaceæ*, the author gave an account of the internal structure of the various fossil stems which had been referred to this family. True *Equisetaceæ* were rare as fossils, and the stems of *Calamites* were very unlike anything known among living acotyledonous plants. The most important characters were obtained by botanists from the fructification. The author had obtained, through the kindness of Dr. Hooker, sections of vegetable structures prepared by Mr. Binney, whose extensive acquaintance with coal-plants was well known. In some of these he had discovered fruits which belonged to *Calamites* so beautifully preserved that the most minute details could be determined, and with the help of his diagrams he described their structure, and illustrated the various points in which they agreed with, and differed from, the fruits of *Equisetaceæ*. He then described the foliage which had been found connected with *Calamites*, and which had been named *Asterophyllites*; and he showed that as similar fruits had been found associated with *Annularia* and *Sphenophyllum*, which differed from *Asterophyllites* only in the amount of cellular tissue spread out on the veins, there could be no doubt that these also were the foliage of members of this large genus or tribe of plants.—*Ibid.*

4. *Geological Map of New Jersey;* by GEORGE H. COOK, State Geologist, and JOHN C. SMOCK, Assistant Geologist.—This new Geological Map of the State of New Jersey contains the results of the recent Geological survey of the State, under the direction of Prof. Cook. It appears to have been made with care, and adds much to our previous knowledge of the distribution of the rocks of the State. Besides the general map, there are sections of the strata, and those of the associated Azoic and Paleozoic rocks are particularly interesting.

5. *New Geological Map of Wisconsin;* by I. A. LAPHAM. 1869. Milwaukee.—This Map, which has just been issued, will prove of much service to all interested in the Geology of the region of which it treats. It is on a scale of 15 miles to an inch, and gives in detail the geological features of the State, as they have been carefully worked out by Hall and Whitney, the author, and other explorers.

6. *Reliquiæ Aquitanicæ*, being contributions to the Archæology and Palæontology of Périgord and the adjoining provinces of Southern France; by E. LARTET and H. CHRISTY, edited by THOMAS RUPERT JONES, Prof. of Geology, etc., Royal Military College, Sandhurst. (London. H. Ballière.)—Parts VI and VII of this important work have been issued.

7. *Gold in Scotland.*—Gold diggings in the north of Scotland will be a surprise to many persons; but there they are, in the shire of Sutherland, and with a number of diggers who are collecting alluvium from the borders of the Holmsdale river, and washing it in the stream. The quantity hitherto collected is not great, perhaps 200*l.* worth, but the quality is described as good, and the color bright: and diggers who have worked in Australia are of opinion that when proper means are taken the yield will be something considerable.—*Athenæum, Feb.* 6.

III. BOTANY AND ZOOLOGY.

1. *Field, Forest, and Garden Botany, a simple introduction to the common plants of the United States east of the Mississippi, both w[illegible] and cultivated;* by Asa Gray. Small 8vo, pp. 386. New York: 1868. (Ivison, Phinney, Blakeman & Co.)—This work, which for the present is offered only in the same cover with Gray's First Lessons, the two together being styled "*Gray's School and Field Book of Botany*," was promised in the preface of the fifth edition of the Manual, and its preparation was completed only a few days before the departure of the author, in September last, for Europe. It is designed to be a simple and easy introduction to Systematic Botany, as represented by the common plants, wild and cultivated, of the States east of the great river, and therefore most of the very rare or difficult species are omitted, thus gaining space for the admission of several hundred cultivated plants. The analytical keys are made very easy, and in the description of the plants technical expressions are as far as possible avoided, so that a person of ordinary intelligence, who has no claim to botanical acquirements, could with little effort trace out a flower to its proper place in the system, and would there learn something of its nature, and native country, and would perhaps find a hint or two about its mode of cultivation, as well as discover the "long name" which Linnæus, or some disciple of his, has seen fit to impose upon it.

This book will necessarily commend itself to all *amateurs* of horticulture, whether for the kitchen, flower-garden or conservatory, for there is no other work in the English language which gives so plain an account of the plants commonly (or even rarely) met with in cultivation. While the Manual will be the most useful book an American botanist can have on his table, he will always keep the Field, Forest, and Garden Botany near at hand, and the beginner in these studies will thank Dr. Gray for this little book, which fairly puts the science within the reach of the youthful, and makes Botany more a matter of every-day life than it has ever been before.

D. C. E.

2. *Are Unios sensitive to light? Note by* C. A. White. (Communicated for this Journal.)—Those who have studied the habits of Unios in their native element, are of course well aware of their habit of burying themselves in the mud or sand, leaving only the

posterior portion projecting, for the purpose of giving ingress and exit to the respiratory currents of water. The sensitiveness of the margins of the incurrent and excurrent orifices to the slightest touch, is also well known; but during the past summer, while collecting mollusks in one of the rivers of Central Iowa, I became convinced that those, or adjacent parts, were also keenly sensitive to light.

Unios were found numerously occupying the position referred to, plying their currents industriously through their distended orifices, but whenever my shadow in the bright sunlight came suddenly upon them, they invariably closed their orifices quickly and completely. This was repeated a great many times, and upon the same individuals, to assure myself that it was not caused by any agitation of the water, or movement of impurities in it, that might produce irritation of the parts. It was evidently the interception of the sun's rays alone that caused them so suddenly to close their orifices, yet it is worthy of remark that they did not quickly close them, if sunlight was suddenly admitted to them while respiring in the shade.

The question then arose in my mind as to the possibility that the parts were sensitive alone to the rays of heat from the sun, and not to those of light. Above the Unios was from one to two feet in depth of clear running water, rendering every thing upon the bottom distinctly visible.

Believing that the sun's rays coming directly toward any object so far beneath the surface of the water, would have its heat-rays mostly if not entirely separated from the light-rays, at or near the surface, through the absorption of these and their removal downward by the current, while nearly all the rays of light would pass on to the object with only slight refraction, I sought a place where rays of heat from sunlight, striking the surface further up the stream, would not reach the Unio to be experimented upon. This was furnished by a dense growth of trees, shading the stream completely for a considerable distance. Then placing a Unio just at the lower margin of the shade, but quite within the bright sunlight, and awaited the opening of the orifices, then quickly intercepting the sun's rays that came freely to it, by passing a screen from above downward, and again from below upward, it responded by closing its orifices as quickly as its fellows had done when my shadow passed over them in the broad open space of sunlight.

Upon the supposition that the light and heat-rays are divided at the surface of the water, as before suggested, the heat-rays must all, or very nearly all, have passed down below the Unio, by the action of the current, while the light-rays alone reached it, and their sudden interception caused it to close its orifices. Thus in this position the Unio was receiving direct rays of light from the sun, but the rays of heat that might have reached it more or less obliquely, by absorption and the action of the current, if in an open space of sunlight, were here cut off by the long shadow of the trees. Therefore no doubt is entertained that the posterior portion of these mollusks is keenly sensitive to light, but exactly what organs are thus sensitive has not been ascertained.

Iowa City, Iowa, Dec. 21, 1868.

3. *Critical Remarks on Halcyonoid Polyps. No.* 3, by A. E. VERRILL.—In the February number of the Annals and Magazine of Natural History, p. 117, Dr. J. E. Gray has published "Notes on the Fleshy Alcyonoid Corals," in which he has made a new arrangement of the group, which he divides into 12 families and 31 genera! Most of the new genera and many of the families are founded mainly on variations in the form, mode of branching, prominence of the verrucæ, and arrangement of the cells,—characters that are notoriously unreliable among polyps, and especially liable to mislead when, as in this case, we have to depend mainly on imperfect figures and descriptions, or specimens contracted in alcohol. On this account we believe that many of the groups founded on such characters cannot be maintained, when the specimens are themselves carefully studied with reference to their internal structure.

At the present time, however, we propose merely to point out some errors of the author, mainly in reference to species described in this country.

Massarella is proposed for a group having as its type, *Sympodium coralloides* Ehr., the same species taken by Kölliker as the type of Sympodium.

Sympodium verum D. and M., which he refers to it, does not agree with the generic characters given, for in the original description we find "*calycibus semper prominulis.*" It may belong to our *Callipodium* and if so would bear the name *C. verum*, which, at present, is not very appropriate.

Sympodium Pacificum V., (erroneously quoted as "S. poriferum V.") is the type of our genus *Callipodium* (Trans. Conn. Acad., Jan., 1868).

Ojeda, adopted from D. and M., is, as we are informed by Dr. Kölliker, a *sponge*.

Lobularia is adopted from Lamarck for the most numerous group of species usually referred to Alcyonium, with *L. digitata* as type. *Lobularidæ* are characterized as having a "hard, crustaceous, smooth, external coat," and division B, including the family, as being entirely covered by polyps "to the edge of the crust or the base of the stem." But *Alcyonium carneum* Ag. and *A. rubiforme* Dana, both of which he refers to *Lobularia*, have neither of these characters. Both have a barren stalk or trunk, and (in full expansion) barren bases to the branches, the polyps being clustered at the ends, as is beautifully shown in a series of photographs, which I made last season from living and fully expanded specimens. These species agree in some states of expansion with Gray's genus *Alcyonium*, as restricted; in other cases with *Danella;* and frequently with *Amocella.* All these and many other forms of growth and modes of branching were noticed by me in *A. carneum*, while studying several hundred living specimens last summer. There may be other characters, at present unknown, sufficient to separate *Danella* from *Alcyonium*, but the mode of growth certainly is not.

Lobularia Verrillii Gray, named from a specimen that I regarded as too imperfect to describe, and of which only the most obvious characters were mentioned incidentally, is certainly not a *Lobularia.*

Areocella is proposed as a genus including only *Alcyonium latum* Dana. This species is, as we have before stated, a true *Sarcophyton*, and in this case there is not even a difference in mode of growth, if specimens of *S. latum* be compared with those of the same size of *S. glaucum*, but the former grows to a larger size than observed specimens of the latter, and then becomes more irregular. The differences mentioned in the character of the surface and arrangement of the cells are wholly imaginary, as I have ascertained by an examination of Dana's original specimens. In each the polyps are dimorphous, like those of all other species of the genus, as stated by Kölliker. And in each the larger cells are distantly scattered over the upper surface, and surrounded by a circle of the smaller polyps, which are figured by Dana in *S. latum*, and thus is produced the appearance described by Gray in *Areocella*, "upper surface areolated, areolæ hexagonal, each surrounded by a series of small tubercles. Polypes in the centre of each areola." In *S. glaucum* and *S. agaricum* the same character exists, only the secondary polyps are smaller and less conspicuous. In both *S. latum* and *S. glaucum* the corallum consists internally of closely arranged parallel tubes of two sizes, corresponding to the cells, separated by narrow septa filled with large, roughly warted, spindle-shaped spicula, which are similar in the two species, but blunter, shorter, and stouter in the former; in *S. glaucum* ·250^{mm} to ·325 long, by ·062 to ·087 broad; in *S. latum* ·225 to ·250 long, by ·100 to ·125 broad. The surface is granulous and covered with very small, short, roughly-warted, irregular spicula, much alike in the two species.

Bellonella ? capitata Gray. For this he quotes "*Lobularia capitata* D. and M., Corall. Antilles, 21." We find no such name in the work referred to, and the reference seems inexplicable unless an error for *Xenia capitata* D. and M., p. 16, which is also quoted on p. 122 and referred to *Lobularia capitata* Gray, but here the reference is to "t. 1, fig. 22" instead of Pl. 1, fig. 1 and 2.

Loridella Gray. This name is proposed for certain species separated from *Xenia* (type, *X. umbellata*). The principal character appears to be "surface coriaceous, with imbedded fusiform, spined spicula." To this genus he refers *X. subviridis*, *X. florida*, *X. elongata*, and *X. rosea*, the last a name for Dana's species, figured as *X. cœrulea*. The *X. florida* Dana, also appears to be distinct from Lesson's species, and may take the name, *X. Danæ* V. Having examined Dana's types I cannot find any characters upon which to separate them generically from *X. umbellata*. So far as these species are concerned the characters do not agree with those given to *Loridella* by Gray. But if they could be separated as a genus from *Xenia*, a new name is not required, since *X. florida* is the type of *Actinantha* Lesson, which should be retained in preference to a new name, and must at least include *A. florida, Danæ, elongata,* and *rosea.*

Spoggodia is separated from *Spoggodes* on account of the cells "prominent from the sides, or forming the tips of the branchlets." But *S. gigantea* V., from Hong Kong, a species not quoted by Gray, has part of the polyps clustered as in *Spoggodes*, and part placed singly on the branchlets, as in *Spoggodia.*

Lemnalia nitida Gray (*Ammothea nitida* V.). The "family," Lemnaliadæ, is said to have the "outer surface smooth, without spicules." It contains *Lemnalia* and *Verrilliana* Gray. The surface in *A. nitida* appears smoothish to the naked eye, but under the microscope it is seen to be filled with an abundance of slender, closely interlaced spicula. The interior is composed chiefly of still longer and very slender, slightly spinulose, acicula spicula, ·250^{mm} to ·500 long, by ·020 to ·025 thick The verrucæ are covered with similar spicula, mingled with few, small, stout, warty spindles, ·075 to ·125 long, by ·050 to ·075 thick. Therefore it cannot belong to this group, as defined by Gray, but agrees better with *Ammothea*, as restricted by him. Savigny's enlarged figures of the cells of *A. virescens* agree so closely with this species that, were not the verrucæ more loosely arranged, it might be thought identical. The form and mode of branching is nearly the same in each. In mode of growth it resembles *Verrilliana*, rather than *Lemnalia.* The latter seems to be near *Cœlogorgia* Val.

Verrilliana thyrsoides Gray (*Ammothea thyrsoides* Ehr.). To this species, described by Ehrenberg as destitute of external spicula, Gray unaccountably refers *Nephthya thyrsoidea* V., which was accurately described and figured as having external spicula on the prominent verrucæ. These spicula are mostly long, thorny, club-shaped spicula, ·600^{mm} to 1·00 long, by ·100 to ·200 thick; and stouter very thorny clubs ·300^{mm} to ·500 long, by ·125 to ·250 broad; and rough, sharp, three-pronged spicula ·275^{mm} to ·325 long by ·150 to ·250 broad. The thorny ends project from the surface, especially toward the summits, and give it a very rough appearance. It appears, therefore, to be closely allied to *Nephthya*, but in the latter the larger spicula are long warty, quite regular spindles, the ends not projecting.

This species may, therefore, be regarded as the type of a new genus, which we propose to call *Eunephthya*, including *E. thyrsoides* V. and *E. glomerata* V. (Lutken sp.,) from Greenland. The latter forms an upright corallum, with a stout trunk, from all sides and to near the base of which arise short sub-conical branches, naked at their bases, like the trunk, but mostly covered with close clusters of 3 to 12, roundish, verruciform polyp-cells, which are rough exteriorly and covered with numerous very rough, thorny, club-shaped spicula, ·200^{mm} to ·350 long, by ·075 to ·125 thick.

GORGONACEA.

Isidella elongata Gray=*Isis elongata* Esper.—The locality of this species is given variously by authors, as East Indies, West Indies, Mediterranean. The Museum of Yale College possesses several good specimens, agreeing perfectly with Esper's figures, which were obtained by Prof. Dana from near Naples. The locality of Esper's specimens was unknown.

Echinomuricea V., gen. nov. Type, *E. coccinea* V. (*Acanthogorgia coccinea* V., Proc. Essex Inst., iv, p. 188, Pl. 6, fig. 7). This species appears to be generically distinct from the type of *Acanthogorgia* in the structure and form of the verrucæ, and especially in the very large size and peculiar character of the spicula, which are more or less imbricated, not in whorls, branched at base, the ends much projecting, very rough, and sharp.

4. *On the Distribution of Fresh-water Fishes in the Allegheny Region of South Western Virginia*, by E. D. COPE. From the Journal of the Academy of Natural Sciences, Philadelphia, 1868, 4to. with 3 plates.—In this memoir the author gives a very interesting and complete review of the fish-faunæ of the head-waters of four distinct river basins: the James, the Roanoke, the Kanawha, and the Tennessee.

He enumerates 56 species, quite a number of which are new. Several genera are also revised, and new ones established.

Of the whole number of species observed, 14 occur in the Roanoke; 19 in the James; 27 in the Kanawha; 34 in the Holston.

Common to the	four rivers, - - - -	5	species.
"	" Roanoke and James only,	4	"
"	" James and Roanoke only,	4	"
"	" Kanawha and Holston only,	2	"
"	" James, Roanoke, and Kanawha only,	1	"

The probable origin of the resemblances and diversities among these faunæ is also discussed.

The author has entered upon a field of research which has hitherto been very little explored in America, but which will doubtless soon become of great interest to all students of Geographical Zoölogy and the origin of species. V.

5. *Catalogue of the Reptiles and Batrachians found in the vicinity of Springfield, Mass., with Notices of all other Species known to inhabit the State*; by J. A. ALLEN.—From the Proceedings of the Boston Society of Natural History, December, 1868. This is another important contribution to American Geographical Zoölogy. The list has evidently been prepared with great care, and many important notes on habits, effects of temperature, and other matters of interest are given. The whole number of true Reptiles found in the State is 24, of which 21 have been seen near Springfield. The Batrachians are represented by 21 species in the State, of which Springfield claims 19, making 45 species of both groups in the State, with 4 or 5 others that are likely to be found hereafter. V.

6. *Notes on Radiata in the Museum of Yale College, No.* 6.—*Review of the Corals and Polyps of the West Coast of America;* by A. E. Verrill. (From the Transactions of the Connecticut Academy, Vol. I. New Haven: April, 1868, to February, 1869.)—This memoir, which has been issued and distributed in signatures, each bearing the date of publication, is now completed to the Order, *Actinaria.* The number of species of *Alcyonaria* enumerated is 62, of which all except 8 have been examined and mostly re-described by the author, while 26 species are here described as new. The descriptions are very elaborate and complete, particular attention having been given to the forms of the spicula, of which numerous microscopic measurements (sometimes amounting to 100 or 200) are given. These are regarded as of the utmost importance, since it is now known that the spicula afford the most reliable characters for distinguishing the genera and species. The *Alcyonaria* are accompanied by four plates, two illustrating the spicula.

The species are distributed among the following genera:

Renilla, 1; Ptilosarcus, 1; Leioptilum, 1; Stylatula, 2; Litigorgia, 16 (9 new); Eugorgia, 6 (4 new); Phycogorgia, 1; Psammogorgia, 3 (2 new); Muricea, 18 (9 new); Heterogorgia, 2; Primnoa, 1; Callipodium, new genus, 2 (1 new); Alcyonium, 2 (1 new); and six Gorgonidæ, not examined, of which the generic relations are doubtful. Several varieties are also described and named.

7. *Annual Report of the Trustees of the Museum of Comparative Zoology*, at Harvard College, in Cambridge, together with the Report of the Director, 1867. 22 pp. 8vo. 1868.—In this Report Professor Agassiz gives a brief statement of the aims, condition and wants of the Museum; and in connection with the latter observes that the Museum needs for its increase, care, and publication fund, a capital of $500,000. Professor Lesquereux, in a Report on the Fossil Plants of the Museum, makes the following remarks on

"*The Ancient Flora of America.*—First. That the floras of our ancient formations already had peculiar types, which separated from each other those of the different continents. This is even evident in the vegetation of the Coal measures. Therefore, the supposition of a continental union of Europe with America by Atlantides or other intermediate lands, is proved to be untenable.

Second. That the essential types of the old floras, of the Cretaceous and Tertiary formations have passed into our present vegetation, or are preserved to our time. The Cretaceous of America, for example, has already the Magnolias, which we find still more abundant in our Tertiary. This last formation has furnished a number of species of the genus Magnolia, nearly identical with that now existing in the United States, while the genus is totally absent in the corresponding floras of Europe. More than this: we find in our Tertiary the same predominating types marked on both sides of the Rocky Mountains. On the Atlantic slope, leaves of magnolias, of oaks, of elms, of maples and poplars, and not a trace of coniferous trees; while in California and Vancouver Island, the red woods or Sequoia, abound in the Cretaceous and Tertiary, as they still form the predominant vegetation of the country. These few facts are mentioned only to show the importance of collections of fossil plants from every formation of our American continent, the only part of the world where questions of general significance concerning palæontological distribution can be studied with some chances of satisfactory conclusions.

IV. ASTRONOMY.

1. *Meteors in August*, 1868.—The following observations upon the night of August 9th, were made at Durham, Conn., by Prof. C. G. Rockwood, of Bowdoin College. There were two observers.

From 12h. 15m.	to 12h. 30m.	13 meteors.	From 1h. 0m.	to 1h. 15m.	5 meteors.
" 30	to 45,	7 "	" 15	to 35,	9 "
" 45	to 1h. 0,	10 "	" 35	to 2h. 0,	12 "

In all, 56 meteors, of which all but two were conformable to the usual radiant. The sky was mostly clear, and the moon shining.

2. *Le Stelle Cadenti del Periodo di Agosto, osservate in Piemonte ed in altre Contrade d'Italia nel* 1868. 72 pp., 16mo, Turin.—These accounts of observations upon the August meteors, from seventeen Italian stations, most of them continued through three or more evenings, indicate no little zeal among Italian astronomers in the department of meteoric astronomy.

3. *Error in Stanley's Logarithmic Tables.*—Mr. Chatham Willey, of Hartford, calls attention to an error in Stanley's Logarithmic Tables. It is on page 38th, where the logarithm of 30672 should be ·4867421 instead of 4864721.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Convention of American Philologists.*—A Convention of American Philologists, will be held in Poughkeepsie, N. Y., commencing on Tuesday, July 27th, 1869, and continuing in session for several days. The call to this Convention is issued pursuant to a resolution passed at a preliminary meeting held in the New York University, on Nov. 13th, 1868.

Measures will be taken during the session to complete the organization of a permanent National Society for the Promotion of Philological Studies and Research in America. Papers upon different branches of Philology by distinguished American Linguists will be read and discussed. The time that may then remain to the Convention will be devoted to the discussion of the following, among other questions, relative to the position which the Study of Language should occupy in our educational system, to the best methods of Philological Instruction, and to the promotion of Philological Literature in America.

1. How much of the time in a Collegiate course of study should be given to the study of Language? 2. How much of this time should be devoted to the study of the Modern Languages? 3. Should the study of the French and German precede that of the

Latin and Greek Languages? 4. What position should be given to the study of the English Language in our Colleges and other high schools of learning? 5. What is the most efficient method of instruction in the Classical Languages? 6. What is the best system of pronouncing Latin and Greek? 7. Should the written accent be observed, in pronouncing Classical Greek? 8. What more efficient measures can be taken to preserve from destruction the Languages of the Aboriginal Indians of America?

It is important that all who design attending the Convention should be present at the first session, which will be held on Tuesday, July 27th, at 3 P. M.

All persons intending to be present at the Convention, and all who propose to prepare papers to be read, are requested to communicate notice thereof, not later than by July 1st, 1869, to the *Chairman of the Committee on Organization*, Prof. GEO. F. COMFORT, (care of Harper & Bros.,) Franklin Square, New York.

The call to the Convention is signed by the leading men of the country in the departments of Philology, and the Ancient and Modern Languages.

2. *Gynæcological Society, of Boston.*—A Gynæcological Society has recently been instituted at Boston, having for its objects the advancement of Gynæcic Science and Art, and their due recognition both in Boston and throughout the country. The officers of the Society for the year 1869–70, are Winslow Lewis, President; Horatio R. Storer, Secretary; George H. Bixby, Treasurer.

OBITUARY.

CARL FRIEDRICH PHILIPP VON MARTIUS.—This eminent botanist died at his home in Munich, December 13th, 1868, in the 75th year of his age. He was born at Erlangen in Bavaria, April 17th, 1794.

His father, Dr. Ernest Wilhelm Martius, was distinguished in his day as the author of several botanical and other scientific works, and was one of the founders of the Ratisbon Botanical Society. He died as recently as 1849 in his 94th year; his last work "Memories of a Ninety Years Life," contains many genial pictures of social life in Germany during that eventful period. But the ancestors of von Martius were eminent in science much earlier than this. He used to show his friends the busts of two men, both famous in their day for their knowledge and from whom he traced his descent, one by his father's, the other by his mother's side. The one was Galeotus Martius, a celebrated philosopher, philologist and astrologer of four centuries ago. The other was the scarcely less famous astrologer, Seni.

Carl Martius pursued his studies at the Gymnasium and University at Erlangen, under the direction of his father and his father's friends. He was the botanical pupil of Schreber, and to him he was doubtless indebted for a careful training in the rudiments of the science he was destined to so greatly advance. But his studies were of a wider range, and he prosecuted them all with enthusiastic devotion. He studied zoology under Goldfuss, chemistry under Hildebrand, philology under Harless, and philosophy under

Mehmes and Vogel. He had as a fellow pupil Nees von Esenbeck, and later they prosecuted together their studies at the house of the elder von Esenbeck, at Wurzburg. He studied medicine at the University at Erlangen, graduated Doctor of Medicine, and commenced the practice of that profession in his native town. In his brief practice he was eminently successful, but botanical pursuits were more congenial to his tastes, and he published his first botanical work in 1814, when but twenty years old. He was already collecting the materials for a more important one on the Cryptogamic Flora of Erlangen, which appeared three years later, 1817.

Upon Schreber's death, his herbarium was purchased by the Bavarian Academy, at Munich, and when Professor Schrank of that city came to Erlangen to remove it, he made the acquaintance of young Martius and induced him to come to Munich, where he became a pupil of the Academy, and in 1816 he was appointed to an office in the Botanic Garden, acting indeed as director for Schrank whose age interfered with those duties.

Maximilian, King of Bavaria, was interested in Botany and in his visits to the Botanic Garden made the acquaintance of Martius, and the impression was so favorable that the King selected him, with Dr. Spix the zoologist, to join as *savants* the embassy that was to accompany the young Bavarian princess destined to be the Empress of Brazil. This decided his fortune and linked his name imperishably with that great empire. The plan of the expedition was prepared by the Bavarian Academy, and they sailed in an Austrian frigate from Trieste, April 10th, 1817.

Our space will not permit of any extended notice of this expedition, nor even an enumeration of its fertile results. They penetrated into Brazil, and traversed the valley of the Amazon to Peru, a distance of over 4,000 miles, much of it through an untrodden region. They were spared fatal accident, but their rich results were only won through the fatigues, hardships, and dangers incident to such exploration. In a little church at Santarem, at the junction of the Tapajoz with the Amazon, there is a full length crucifix presented by von Martius, and beneath it a simple inscription telling that his life had been mercifully preserved when shipwrecked near there in 1819. They were prostrated by malarial fevers, annoyed by wild beasts, and in danger from treacherous natives.

They returned in 1820 with collections amounting to 3,500 species of animals, 6,500 species of plants, and a vast amount of material relating to the ethnology, philology and history of the region. The exposures and fatigues of this expedition so undermined the health of Dr. Spix that he survived his return but a few years, leaving the burden of publishing the vast amount of material they had collected upon von Martius. The narrative was published in three quarto volumes with folio atlas, entitled, "Reise in Brasilien," Munich, 1824–31.

The first department of the collection worked up was the Palms, and the first part of his "Genera et Species Plantarum" was published in 1823. He commenced with the palms of Brazil, but ultimately extended his investigations to the whole order. This labor continued through twenty-seven years, and the magnificent work was completed in 1850, in three large folio volumes, containing 245 plates, all finely executed and some of them exquisitely finished landscapes, exhibiting the habits of the species in their native localities. He published several smaller works on the palms, but his great work will ever remain classical. As an illustration of its scope, Humboldt knew less than a hundred species of this order; the work of von Martius embraces 582 species.

On a similarly magnificent scale was his "Nova Genera et Species Plantarum Brasiliensium," 1824 to 1832, folio, with 300 colored plates, and his "Icones Plantarum Cryptogamicarum quas per Brasiliam collegit," Munich, 1828–34, folio, with 76 colored plates.

In 1829 he began the publication of his great work, "Flora Brasiliensis," in folio, and for forty years, up to the time of his death, he labored upon this, and left it unfinished. Some portions of this are entirely his own work, in others he acted as editor, calling to his assistance some of the most distinguished botanists of the day. It will probably be completed in a series of monographs of the remaining orders, by other hands. In this work are already described nearly 8,000 species, illustrated with about a thousand folio plates.

He extensively investigated the medicinal uses of plants, and published several works on this department of botany, and also, to a less extent the bearings of his favorite science on Agriculture. Among his works is an essay on the Potato Rot, (Munich, 1843).

We will not attempt an enumeration of his other contributions to botanical literature, which extend to a very long list, and reach nearly every branch of the science.

He was Director of the Botanic Garden at Munich for about forty years, and under his direction it had perhaps the richest collection of species of living palms in Europe.

Von Martius was not only a student and traveller, but also eminently a collector. His private herbarium amounted to over 50,000 species; in the department of palms it was by far the richest extant. He was long anxious that this herbarium should ultimately come to America. He often expressed his wish to the writer, that if it was not purchased by Brazil, he would prefer to have it come to the United States. His library was large and in pre-Linnean works he considered it absolutely complete. It was also especially rich in botanical manuscripts and original drawings of plants.

From his youth, von Martius had a great fondness for linguistic studies, and he had a wonderful command of language. The Latin used in the systematic descriptions in modern botanical works, can hardly be called classical, yet his command of this language is exhibited by the elegant and eloquent dissertations which may be found in various of his works. He often used this language in his correspondence, and many of his letters, especially to his intimate friends, are classical in their purity and style. This fondness for linguistic study showed itself throughout his life. In Brazil he paid much attention to the language of the country and he published several works upon the subject, the last of which, "Beiträge zur Ethnographie und Sprachenkunde Amerika's zumal Braziliens," (Munich, 1867, 2 vols. 8vo,) is extensive and is said to indicate great learning in this branch of knowledge. He took a deep interest in the philology and ethnology of the Indian tribes of North America, and during his long life he accumulated an immense mass of facts relating to these subjects. In his last letter to the writer, he asked for "vocabularies" and "skulls" of our western Indians.

He was for many years Secretary to the Mathematico-physical section of the Munich Academy, and in this office delivered many orations on the decease of illustrious members of the Academy. These were collected into a volume which contains many eloquent tributes to the memory of his eminent friends. In connection with this office he carried on a voluminous correspondence in various languages, a position in which his extensive linguistic knowledge was practically available.

Any notice, however short, of this good man would be incomplete if it did not allude to his personal worth and social qualities. In form, he was a small man, of slight figure, but indicative of great powers of endurance. He had a peculiarly animated countenance and bright eye. He delighted in the society of his friends, was fond of amusements, and his childlike simplicity was truly charming. His pleasant home in Karlstrasse was the resort of the cultivated, who found there the old style hospitality, and his receptions retained the genial flavor of German social life in spite of modern innovations. The annual festival commemorating his return from Brazil, the equally genial birthday celebrations were never forgotten, and he entered into their pleasures with the zest of a child.

Perhaps the recreations derived from such home pleasures preserved his strength so well, for his intellectual labors ceased only with his life. He was passionately fond of music and played the violin with rare skill and taste, and he indulged in this even to his later years. His characteristic love for collecting showed itself even here, for he possessed a valuable collection of rare old violins.

We cannot here attempt to enumerate the various titles he held, a mere list would occupy several pages of this Journal; and the "decorations" bestowed upon him were also numerous.

He died quite suddenly from an attack of lung fever. He was buried with honors, a procession of torch bearers each with a palm branch followed him to the cemetery, where many distinguished men assembled to do honor to his memory; the hymns at the grave, were sung by the students of the University where he had long been an honored and beloved officer. W. H. B.

John Cassin.—This eminent ornithologist died in Philadelphia, on Sunday, January 10th, in the fifty-seventh year of his age. We cite the following from a tribute to his memory by Dr. T. M. Brewer, in the Proceedings of the Boston Society of Natural History, 1869, p. 245.

John Cassin was born of Quaker parentage, in Chester, Pa., Sept. 6th, 1813. In 1834, at the age of twenty-one, he took up his abode in Philadelphia, where he has ever since resided. In his earlier life he engaged in mercantile pursuits, and afterwards, for several years, held important positions under the national government. At the death of Mr. Bowen, the principal engraver of Philadelphia, Mr. Cassin assumed the management of the establishment, which he continued until his death. All the reports of explorations and surveys issued by our government have been largely indebted to him for the excellence of their illustrations.

For more than thirty years Mr. Cassin has devoted all the leisure hours he could take from the requirements of his business to the study of ornithology. Privileged to reside in Philadelphia among the kindred spirits that compose its Academy, yet more privileged in having access to its wealth of ornithological specimens,—probably the greatest in the world,—and to its even greater wealth in scientific works, where is to be found, procured by the munificence of his friend, the late Dr. Wilson, every known publication of any value on the subject of ornithology—with all these privileges no one could well have enjoyed greater advantages for pursuing his favorite study, and certainly no one could have better improved such rare opportunities.

With a full appreciation of all that I aver, I claim for my lamented friend, that as a general ornithologist, especially in regard to his knowledge of the forms of the Old World, Mr. Cassin had no superior either in this country or elsewhere—it may even be doubted if he had an equal. By long and diligent study, by the most thorough investigations, and by the most careful researches into the authorities, with a patient perseverance that nothing could discourage, he rendered himself a complete master of the science. So perfectly familiar was he with the forms of the Old World, that he investigated their classification, established new genera and described new species as readily in Africa as in America, and the savans of Europe have accepted with deference his decisions.

Mr. Cassin has been for many years an active member of the Philadelphia Academy of Natural Sciences. His valuable contributions have enriched the pages of its Journal, and added a world-wide reputation to its Proceedings. His activity and zeal in the cause of science have aided to draw around that institution munificent patrons, as well as distinguished co-laborers, under whose influence, and by whose means, the Academy has risen to the highest rank as a well endowed and honorable school of the natural sciences. Time would hardly serve me to read even the titles of the fifty-six separate and distinct papers, descriptive, analytic and synoptical, given in the Catalogue of the Royal Society of London, as contributed from time to time by Mr. Cassin to the Proceedings of the Academy, and which constitute only a portion of his valuable contributions to ornithological science. His more elaborate publications have been his Birds of California and Texas, an octavo volume, giving descriptions and colored engravings of fifty species of birds not enumerated by Audubon; the Ornithology of the

United States Exploring Expedition under Lieut. Wilkes; the Ornithology of the Japan Expedition; the Ornithology of Gilliss's Astronomical Expedition to Chili; and the portions of the Ornithology of the Pacific Railroad Explorations and Surveys relating to the Rapacious and the Wading Birds.

His communications and all his contributions to science are distinguished by their careful research, their thoroughness, and their unfailing accuracy—an accuracy that was ever above reproach, and as it seems even beyond criticism.

Nor was it alone as a closet naturalist that Mr. Cassin was distinguished. He was also an ardent lover of nature, and a close observer of living birds, both in their wild wood haunts, and under the open sky. I am indebted to him for much valuable information, derived from his observations upon the habits of various birds; and it was to aid from his unequalled knowledge that we have looked forward for the correct classification of the collection of our Society. But alas, that once ever open volume, so abounding in its wealth of knowledge, is now forever closed to us on earth, and with his fleeting spirit has passed from us that seemingly exhaustless treasury of science to which we never appealed in vain.

As a man, our departed friend was of unswerving uprightness, warm-hearted, cordial and sincere, firm and abiding in his friendship, and only a foe to whatever was wrong, ungenerous or illiberal—possessed of strong, fervent and generous impulses, and frank and outspoken in the expression of his opinions. Decided in his own views, he was still ever tolerant and liberal towards those who differed from him.

In a word, whether we regard Mr. Cassin as the naturalist, whose scientific achievements had placed him in the front ranks of the votaries of science; as the man of business, and the honored head of a house which was devoting all its energies and the highest artistic skill to the illustration of science; or as the ever sympathizing and congenial friend,—his death in the full prime and vigor of life, and in the very midst of his transcendent usefulness, can only be received as one of those inscrutable deprivations which, while we must accept them in humble faith, we cannot but deplore.

PROF. THEODORE STRONG.—The able mathematician, Theodore Strong, died at his residence at New Brunswick, N. J., Feb. 1st, at the age of 79. Having graduated at Yale College in 1812, he immediately after entered on the duties of tutor, and subsequently of Professor of Mathematics at Hamilton College, Clinton, N. Y., where he remained until 1827. He then became connected with Rutgers College, at New Brunswick, New Jersey, and continued in his Professorship there until 1862. He was a man of great excellence of character, positive in his opinions, frank and communicative, and enthusiastic in his mathematical studies. He published various mathematical papers in the first series of this Journal, and an Algebra of high order in 1860; and a Treatise by him on the Differential and Integral Calculus was in press when he died.

James David Forbes.—The Scotch physicist, James D. Forbes, died at Clifton on the 31st of December, in the 60th year of his age. Ill-health had led him to retire, a few months before his death, from the Principalship of the United Colleges of St. Salvador and St. Leonard, at St. Andrews. He was educated in the University of Edinburgh, and there he became Professor of Natural Philosophy in 1833. His memoirs on physical subjects, and especially his works on Glaciers, have given his name a worldwide reputation.

Dr. Hörnes.—Dr. Moriz Hörnes, director of the Austrian Imperial Mineralogical Cabinet, died at Vienna, Nov. 4th, 1868, aged 54 years. He is well known in scientific literature, by his researches on the mollusca of the Vienna tertiary, and various papers on mineralogy and paleontology. As director of the finest mineralogical collection of the world, he exercised the greatest liberality in offering facilities to others pursuing investigations in mineralogy; and so devoted was he to the interests of the cabinet, that during the late financial embarrassments of Austria, when the income of the museum was cut down, he supplied from his own narrow means what was needed to promote the increase of the collection. He was a man of most genial manners, and noble, earnest scientific spirit; his sudden death in the prime of life, is a great loss to science.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Handbook of Chemistry, for School and Home use;* by W. J. Rolfe and J. A. Gillet, Teachers in the High School, Cambridge, Mass. 12mo, pp. viii, 205. Boston, 1869. (Woolworth, Ainsworth & Co.).—We have already noticed the volume of the "Cambridge course of Elementary Physics" which treats of chemistry (this Journal, Jan. 1868). The present "Handbook" is designed for those teachers who desire "something easier (!) and briefer" than that. After a few general experimental demonstrations, given to familiarize the student with chemical changes, the more important elements are taken up *seriatim* and quite satisfactorily treated. Three practical sections, on the "Chemistry of the Atmosphere," on "Destructive distillation and its products," and on "Fermentation," are then introduced, and the volume closes with an Appendix on Chemical Philosophy—largely taken, as the author states, from Cooke's First Principles of Chemical Philosophy, edition of 1868—and Questions for Review and Examination. We notice that the old fiction of "Affinity" is retained, and that it is rather loosely stated (p. 45) that "Affinity changes the properties of the substances which it combines." Olefiant gas $H_4Є_2$ is written twice (pp. 134 and 149) $H_2Є$, and aerated bread is said (p. 159) to be made by kneading the dough with water charged with carbonic acid gas. The old and now entirely false dogma that "as yet, we are ignorant of the mode of arrangement" of the atoms in the molecule (p. 162) should, since Kekulé's masterly researches on the Aromatic Series, be expunged from our chemistries

On the whole, the book is a valuable addition to our meagre collection of text-books on the new system, and we commend it to the notice of teachers.

2. *Van Nostrand's Eclectic Engineering Magazine.* 80 to 96 pp. large 8vo. Monthly: vol. I, No. 1. Jan. 1, 1869.—This periodical meets a want which must have been generally felt by American Engineers, of a magazine that should furnish abstracts of important investigations, and of the expositions of new projects, valuable experimental results, and signal achievements in works of construction, that may appear in the Engineering serial publications of Europe and America. It is designed to provide the American Engineer with the means of readily informing himself with regard to the "current fact and opinion" in his profession both at home and abroad; and is calculated to prove a valuable aid in this way, and by furnishing an index to important discussions, even to those who may have the leisure to consult the original publications.

The work of selection and condensation from other Journals, and of editing, generally, this new Magazine, is under the able direction of Mr. A. L. Holley, the well known author of valuable Engineering works, and practical Engineer. Two numbers have been issued, and they must be pronounced a decided success. They afford good indication that the design set forth in the Publisher's Prospectus of presenting "within limits of space and cost that all can afford," the cream of all the principal Journals of Practical Science, will be satisfactorily realized.

3. *Annual Report of the Board of Regents of the Smithsonian Institution*, showing the operations, expenditures and condition of the Institution for the year 1867. 506 pp. 8vo. Prof. Henry adds very greatly to the scientific value of his reports, by the various memoirs which are appended. In the present volume there are biographical sketches of Legendre, Peltier, Faraday and the Jussieus; a paper on the natural history of organized bodies, by M. Marey; on the electrical currents of the earth, by C. Matteucci; on Man as the cotemporary of the Mammoth and the Reindeer in Middle Europe, from the German "Aus der Natur;" Photo-chemistry, by Jamin; on Traces of the early mental condition of man, by E. B. Taylor; besides various other articles in Ethnology, Geography, Zoology, Meteorology, Physics, etc.

4. *The Manufacturer and Builder:* a Practical Journal of Industrial Progress. Western & Company, Publishers, 37 Park Row, New York. January, March, 1869. This new serial commenced with the current year, appears monthly, at the very low price of $1.50 for the year. It is beautifully printed in a large quarto form, of three columns, on excellent paper, and will prove to be an important addition to the resources of the large class of practical men, to whose interests it is exclusively devoted. Its success is already assured by a very large circulation.

5. *Blank Maps for marking the distribution of Plants and Animals.*—The Boston Society of Natural History has issued blank maps, designed for marking, by pencil or water-colors, the limits

of known distribution of given species of animals or plants over the area delineated. Since, for such a purpose, the student needs a larger or smaller number of charts, according as his field of research is wider or narrower, they have been prepared, a circular states, as cheaply as possible, consistent with entire accuracy. They have been purposely printed on thin paper, which will take water-colors, and will be furnished in any number at the cost price of a very large edition, viz: $6.70 per hundred. Copies are ready for delivery, and can be obtained by forwarding the necessary sum to Samuel H. Scudder, the Librarian of the Society.

6. *Le Naturaliste Canadien.*—A popular scientific monthly in French, of the above title, has been commenced at Quebec under the editorial charge of M. L'Abbe Provancher, curé de Portneuf. Each number contains 24 pages 8vo, and the first was published in December last.

Report of the British Association for the Advancement of Science for 1867. 522 and 196 pp. 8vo. London, 1868. (John Murray).

Contributions to the Fauna of the Gulf Stream at great depths; by L. F. de Pourtales, 1st series, 1867, 2d series, 1868. Bulletin of the Museum of Comparative Zoology, Cambridge, Mass.

Catalogue of the Orthoptera of North America described previous to 1867. By Samuel H. Scudder. 89 pp. octavo, 1868.—Smithsonian Miscellaneous Collections.

On the Origin of Genera. By Edward D. Cope, A. M. 80 pp. octavo, 1869. From the Proceedings of the Acad. Nat. Sci. Philadelphia.

Proceedings of the Essex Institute, Salem, Mass. Vol. v, No. 8.—p. 188, Progress of Sacred Music in New England; *A. C Goodell, Jr.*—p. 191, American Lichenography; *H. Willey.*—Communications.—p. 273, List of the Birds of New England; *E Coues.*—p. 315, Synopsis of the Polyps and Corals of the North Pacific Exploring Expedition, Part 4, Actinaria; *A. E. Verrill.*

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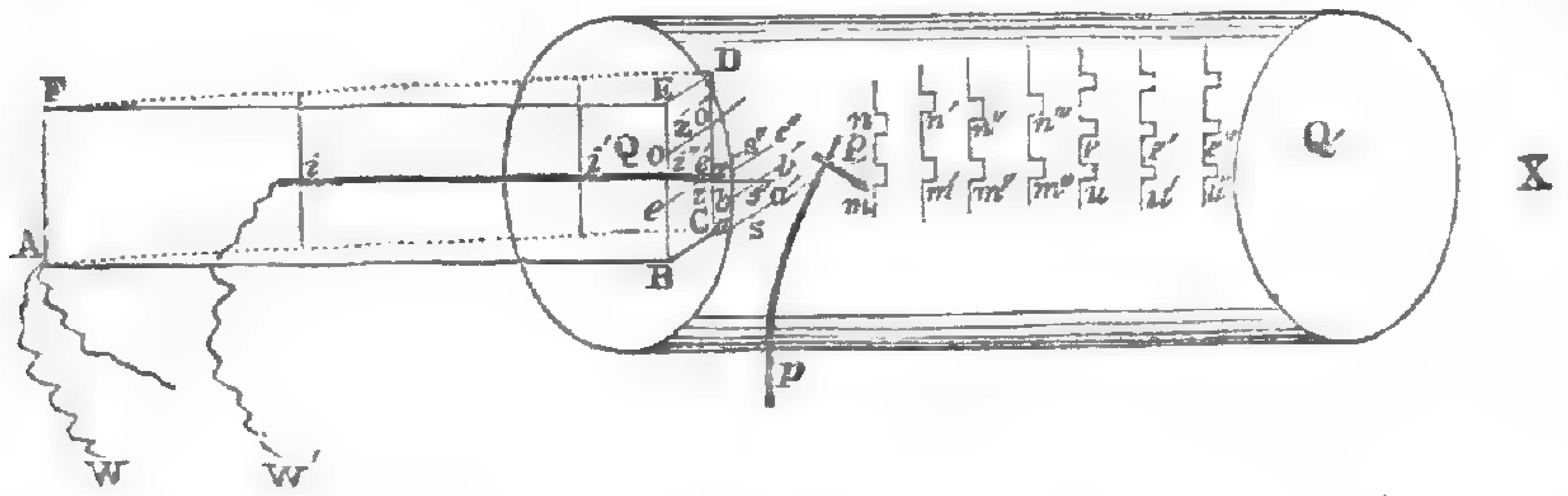
ART. XXV.—*On the Variability of Personal Equation in Transit Observations;* by WILLIAM A. ROGERS, Director of Alfred University Observatory.

THE personal equation of an observer is the interval of time which habitually intervenes between the actual and the observed transit of a star over a given thread of the transit instrument. This may be termed the absolute personal equation.

The relative personal equation of two observers is the interval of time by which one habitually observes a transit either earlier or later than the other; or, it is the difference between their absolute equations.

It is well known that personal equation is one of the most uncertain elements in chronographic determinations of longitude. It has been the custom, at least in this country, in observations for longitude, to assume that the value of this function of the time remains unchanged during the entire series, even though the comparisons for the determination of this element were delayed for several weeks. Now if the personal equation of the observers is not a constant quantity, if it can be shown that from any cause it varies from week to week, from day to day, and even from hour to hour, between limits nearly as large as the usual value of the function itself, it will be evident that some uncertainty yet remains in the accepted values of longitude, especially since in almost every instance, the observations for longitude and personal equation have been separated by a considerable interval of time. The investigation of the variability of personal equation is, therefore, not an idle inquiry.

The results on which this discussion is founded depend upon about 8,000 observations of artificial stars. In the present instance, the artificial stars were made of paper and centered upon fine steel wires placed in a vertical position. Now it is evident that if the wire to which the star is attached, could be made to pass a stationary vertical wire with a uniform motion, and if the exact time of opposition could be automatically recorded, we should have a standard with which the observed time of passage could be compared. The following device was employed to accomplish this object.



Let Q Q′ in the figure represent the cylinder of a Bond chronograph. At one end of the cylinder and attached to the table upon which the chronograph rests, is placed a wire frame work ABCDEF, in such a position that the vertical face BCDE shall be parallel with the plane of the end of the cylinder and in close proximity to it. *ee′*, *oo′* are two vertical threads of fine wire. *aa′*, *bb′*, *cc′*, are fine steel wires placed vertically at the circumference of the base Q. Upon these wires are centered the stars *s s′ s″*. *i i′* is a wire insulated with respect to the frame work at *i* and *i′* but in contact with it at *z*. To the end of this wire is attached a fine needle *i″ i‴* projecting beyond the plane of the wires on the cylinder just far enough to come in contact with them as they revolve. The wires *w w′* run to a battery after passing through a coil (not represented in the figure) with which the pen *p p′* is connected.

As the cylinder revolves, the instant *b b′* arrives opposite *e e′* it breaks the connection at *z* and the circuit continues broken till the needle springs back upon *ee′*. During this interval a spring attached to *p p′* moves it horizontally, making a break at *m*. The beginning of the break is the instant of opposition. At another revolution another break is made at *m′*, and we thus have the instant of opposition automatically recorded upon the line *m m‴*. Suppose an observer standing at X with a break-circuit key, to *observe* through a small telescope, the instant of conjunction between the fixed wire *o o′* and the movable one *b b′*. By breaking the circuit at that instant, a break is made at *n*, depending only on the judgment of the observer. *Now, if the measured interval between the two fixed wires e e′ and o o′ is* *equal to the interval between m and n, expressed in the same unit, there is no absolute personal equation. If m n is less than z z′ the observation is too early, if greater, it is too late, in either case by the difference between z z′ and mn.*

In order to find the common unit of measurement, the galvanic connection is made through the clock; then every alternate swing of the pendulum gives the constant spaces $u\,r$, $u'\,r'$, $u''\,r''$, each equal to two seconds of time. In the present instance this space was divided upon mica into twenty equal parts by the aid of a filar micrometer screw. By placing the scale upon $m\,n$, the distance from m to n is measurable to ·1^s and by estimation to ·01^s. The value of $z\,z'$ adopted in this discussion after numerous and careful measurements upon different parts of the scale, is ·77^s+·015^s. Hence whenever the interval between the beginning of the automatic break and that made by the observer is less than ·77^s the observation is too early, and when greater, it is too late. The difference, either way, is the absolute personal equation. If there are two observers, their relative personal equation is easily found, from the absolute equation of each.

In entering upon this discussion the first and the main question is:

Does the personal equation vary from any cause?

As the basis of investigation, I give below the mean of the values found for each date, with explanatory remarks concerning the conditions under which the observations were made. By an abnormal position of the body, I mean as painful a position as I could assume. The wires were illuminated by placing a light nearly in front of them. Faint illumination was produced by reducing the volume of the flame.

The observations were made by Prof. Edward M. Tomlinson, Mr. Herbert E. Babcock, and myself.

Absolute Personal Equation.

ROGERS.

Date. 1867.	Equation.	Remarks.
Nov.		
19	= −·118s	
20	·093	
21	·057	—Read the record for Nov. 19 and 20. First knowledge of the value of my personal equation. Observations from Nov. 19 to Nov. 29 were made under a normal condition of the body.
22	·022	
23	·088	
24	−·047	
25	+·025	
26	·019	
27	·034	
28	·019	
29	+·017	—Read up all the preceding records.
Dec.		
5	−·067	Normal position.
5	·032	Abnormal position.
6	·038	Normal.
6	·037	Abnormal.
7	·042	Normal.
7	·040	Abuormal.
8	·042	Normal.

Date. 1867.	Equation.	Remarks.
Nov.		
8	—·040s	Abnormal.
12	·047	42°=read'g of thermometer.
Dec.		
12=	·083s—4°.	Hands protected with cotton gloves.
12	·030—4°.	Hands unprotected.
12	·080—3°.	Hands protected.
12	·072—3°.	After warming; hands unprotected.
12	·061	42°. Regular beat of chronograph pendulum.
16	·024	34° " " "
16	·040	35°. Irregular beat.
16	·061	35°. Regular beat.
16	·054	9°. Normal.
16	·039	38°. Normal.
16	·036	38°. Abnormal.
22	·034	Bright wires.
22	·061	Very faint wires.
24	·032	Bright wires.
24	·053	Moderately faint wires.
24	·023	Bright wires.
28	·040	Bright wires.
28	·039	Faint wires.
28	·023	Bright wires.
1868.		
Jan.		
13	—·057	Bright wires.
13	·118	Very faint wires.
13	·061	Bright wires. Abnormal.
13	·065	" " Normal.
13	·078	Dark field.
13	·087	Normal.
Feb.		
10	·075	Thermometer =38°.
10	·067	" — 3°.
22	·069	" 34°.
22	·039	" — 8°.
22	·035	" 34°.
April.		
27–8—	·040	Observed from 8 P. M., Apr. 27, till 5h A. M. Apr. 28, at irregular intervals; the intervening time being occupied with observations in the prime vertical for latitude.
27–8	·045	
27–8	·077	
27–8	·069	
27–8	·049	
27–8	·041	Very tired and sleepy.
27–8	·029	Time of observation, 8h A.M. Slept from 5h till 8h A. M.

Date. 1868.	Equation.	Remarks.
April.		
27–8—	·033s	Time, 9h A. M.
27–8	·051	Time, 9½h A. M.
27–8	·011	Time, 2h P. M. Slept from 12h till 2h.
27–8	·051	Time, 7h P. M. Engaged in severe exercise from 4h till 7h.
May.		
26	—·042	Observed by day-light.
26	·041	" " lamp-light.
26	·050	} Severe physical exercise between these observations.
26	·032	} Very tired.
31	·051	Day-light.
31	·040	Lamp-light.
31	·019	After observation with the equatorial for relative equation. Tired.
June.		
2	—·045	Day-light.
2	·019	Lamp-light.
2	·034	Day-light.
2	·045	Lamp-light.
4	·033	} Observed with the equatorial from 8h P.M. till 11h. Slept from 11h P. M. till 1h 30m. A. M. Watched with the sick till 5 A. M. Time of observation, 5h 30m A. M.
4	·040	}
5	·027	}
9	·033	Normal.
15	·024	} Slept very little June 14. Slept from 1h P. M. till 3h.
15	·018	} Time of observation 3h P. M.
17	·055	After severe exercise.
22	·053	Normal.
26	·045	Normal.
26	·046	Normal.
26	·019	} *Assumed* that I observed too late. No knowledge of the value of my equation.
27	·008	}
27	·028	Normal.
28	·040	} Normal.
28	·013	} Hungry.
28	·010	}
28	·018	}
28	·007	} Ate nothing for 30 hours.
28	·007	}
28	.006	}
28	·007	}
28	—·001	} *Very* hungry.

TOMLINSON.

Nov. 19=	+·096s
20	·116
21	·116
22	·139
23	·167
29	·213
Nov. 24=	+·149s
25	·180
26	·204
27	·179
28	·163
June 15	·027

BABCOCK.

Nov. 21=	+·035s
23	+·013
24	—·007
25	·000
26	+·011
27	+·018

Feb. 10	=+·036^{s}	June 15	+·047^{s}	Nov. 29	—·030^{s}
10	—·037	17	·055	Feb. 10	·035
Apr. 27	+·047	22	·032	10	·143
May 31	·062	26	·036	Apr. 27	·071
June 2	·034	27	+·047	27	—·137
4	·054				
9	+·070				

Relative Personal Equation.

Rogers minus Tomlinson= R—T.				Rogers minus Babcock= R—B.	
Nov. 19	=—·214^{s}	Apr. 27–8	=—·098^{s}	Nov. 22	=—·112^{s}
20	·209	May 31	·113	23	·101
21	·163	31	·102	24	—·040
22	·161	31	·081	25	+·025
23	·255	June 2	·079	26	+·008
24	·196	2	·053	27	·016
25	·155	2	·068	29	+·047
26	·185	2	·079	Feb. 10	—·040
27	·145	4	·087	10	+·076
28	·144	4	·094	Apr. 27–8	·031
29	·196	4	·081	27–8	+·026
Feb. 10	·111	9	·103	27–8	—·006
10	·030	15	·061		
Apr. 27–8	·087	15	·065		
27–8	·092	17	·110		
27–8	·124	22	·085		
27–8	·116	26	·081		
27–8	·096	26	·082		
27–8	·088	26	·055		
27–8	·076	27–8	·055		
27–8	·080	27–8	·075		
27–8	·098	27–8	—·087		
27–8	—·058				

It will be evident from an examination of the values given above, that personal equation is a varying quantity, if it can be shown that the variation exceeds the probable error of observation. Without going into details, I give below the value of the probable error from each source, depending upon a sufficiently large number of observations :

I. Probable error of observation for each star—ten revolutions,	R=±·013^{s}
	T=±·017
	B=±·016
II. Probable error of each reading on the scale	=±·02
III. Total error derived from a change in the common unit of measurement as affected by a variable beat of the chronograph pendulum, (estimated),	=±·02

IV. Error of centering a single star, $=\pm\cdot02$ (estimated).

V. Error arising from the condition that the revolving pins may not have been in a vertical plane with the stationary ones at the instant of conjunction, $=\pm\cdot005$ (estimated).

VI. Parallactic error, arising from the parallax of the two wires at the instant of observation. } Inappreciable, the revolving wires almost touching the fixed one at their transit.

With regard to these errors it is to be observed:

(*a.*) Since there were an average of 8 artificial stars attached to the cylinder, and the observations were on the average continued through 10 revolutions, each result depends on about 80 observations. The probable error of observation and reading for the final result must therefore certainly be less than $\pm\cdot01^s$. No allowance has been made for the third source of error. While it was not difficult to detect the error itself by a change in the measured length of the comparison unit, it was so variable in amount between the extreme limits $+\cdot02^s$ and $-\cdot02^s$, that I did not find it possible to deduce a definitive mean value. I am confident, however, that the final result for any date cannot be affected with so large an error as $\pm\cdot005^s$.

(*b.*) Errors IV and V affect the absolute equation; but in considering the *variability* of this function, they are to be disregarded, since the wire frame work, the wires and the stars, remained absolutely in the same position from day to day, unless purposely disturbed. So also, these errors are eliminated, if the observations are for relative personal equation.

It is therefore obvious that the change manifest in the value of the personal equation, whether absolute or relative, cannot be accounted for either as instrumental errors, or as errors of observation, but must be due to the external conditions under which the observations were made. Having determined the fact of the general variability of the personal equation, let us now consider the variations due to certain given physical conditions.

I. *Does the personal equation vary between a normal and an abnormal position of the body during observation?*

Normal position minus Abnormal.

R.—Dec.	6	$=-\cdot031^s$	
	7	$-\cdot002$	
	8	$-\cdot002$	
	16	$-\cdot003$	
Jan.	13	$-\cdot004$	
	13	$-\cdot026$	**Mean $=-\cdot011^s$**

Thus, while the change is not large, every series of observations gives the same sign. It will not answer, however, to assume, either that the mean value remains constant, or that another observer would find the same value, since several conditions contribute to the result found. In reading up the records, the rather curious fact was noticed, that the probable error of observation was less for an abnormal than for a normal position of the body.

II. *Does a change of temperature affect the personal equation?*

Absolute.

	R.	
Dec.	12	42° − (−4°) = + ·036ˢ
	12	42° − (−4°) = − ·014
	12	42° − (−3°) = + ·019
	12	42° − (−3°) = − ·011
Jan.	13	35° − (−9°) = − ·007
	13	38° − (−9°) = + ·015
Feb.	10	38° − (−3°) = − ·008
	22	34° − (−8°) = − ·030
	T.	
Feb.	10	38° − (−3°) = + ·073
	B.	
Feb.	10	38° − (−3°) = + ·107

Relative.

Feb. 10.

			Change.
B.—T.	38°	= + ·071ˢ	
	−2°	+ ·105	·034ˢ
R.—T.	38°	= + ·108ˢ	
	−2°	= + ·028	·080
R.—B.	38°	= + ·037ˢ	
	−2°	= − ·077	·114

The observations for ordinary temperature were made in the clock-room. For the low temperature observations, an aperture about 1·5 inch in diameter was made through a pane of window glass and the theodolite was placed on the outside. The first and third set of observations for Dec. 12 were made with gloved hands, the second and third with hands unprotected. It will be seen that in my own case, the change is slight, while with B. and T. it is large. The values depend upon very careful observations continued through 20 revolutions of the cylinder. The probable error for high and low temperature did not sensibly vary with myself and B., but was about ±·005ˢ larger for low temperature in the case of T.

III. *Does an exhausted state of the system produce a variation of the personal equation?*

It will be seen from the observations of Apr. 27–8, May 26–31 and June 4–5–15–17, that no decisive change resulted from extreme weariness. The mean effect was a slight tendency to diminish the equation by an amount hardly measurable with certainty. This result was contrary to my anticipations, and if confirmed by other observers, establishes a fact of much importance, inasmuch as astronomical observations are usually carried far into the night.

In every instance, the equation was quite largely diminished when the observation was made directly after waking from a sleep preceded by extreme exhaustion. Here, as before, I found that an abnormal condition rather improved the probable error of observation.

IV. *Does hunger affect the value of the personal equation?*

Normal state minus a state of hunger.

June 27–8	—·027^s
	·030
	·022
	·032
	·034
	·034
	·034
	·034
	·033
	—·039

There is thus a decided and quite regular change, the mean being —·032^s.

V. *Does the mental state of the observer have any influence on the personal equation?*

Normal state minus a state in which the observation is *assumed* too late.

Nov. 20–1	—·036
June 26	—·026
27	—·038

I have already remarked that I obtained the first knowledge of the value of my personal equation, Nov. 21, 1867, and that after Nov. 29 I had no farther knowledge of its value, till after all the observations were completed, not in fact till I read up the records in July, 1868. As the value for Nov. 29 was positive, I *arbitrarily assumed* it negative in order to ascertain the effect of this assumption upon it. The result confirms the suspicions which I have for some time entertained, that the simple knowledge of the value of one's personal equation induces a tendency to reduce its value. Since constancy in value is more desirable than this reduction, which is uncertain and variable, it follows that it may not be best for an observer to have a knowledge of his personal equation. I ought to remark, however, that the *expectation* of having my suspicions on this point confirmed, *may* have had something to do with the results found, though I endeavored to free my mind from every bias except the single one assumed.

Let us now consider certain variations of the personal equation in which the condition of the observer is not taken into account.

I[1]. *Does a change in the character of the illumination of the wires affect the personal equation?*

Bright wires minus faint wires.

Dec. 22=	$+\cdot027^s$	; very faint.
24	$+\cdot024$	; faint.
28	$\{-\cdot001$, $+\cdot016\}$	; moderately faint.
Jan. 13	$+\cdot060$	; extremely faint.

II[2]. *Does the personal equation vary between a natural and an artificial illumination of the wires?*

Natural minus artificial.

May 26=	$-\cdot002^s$
31	$-\cdot011$
June 2	$-\cdot026$
2	$-\cdot011$
2	$-\cdot000$

These observations were made during the day time. The artificial illumination was produced in the way already described, the clock-room being darkened.

III[2]. *Does the size of the stars observed affect the personal equation?*

From Nov. 20, till Nov. 29, only five stars were employed, the first one being larger than the others.

R.—T. from Nov. 20 till Nov. 29.

Large star.	Small stars.
$-\cdot233^s$	$-\cdot144^s-\cdot167^s-153^s-166^s$

Large star minus mean of small stars.

$$-\cdot233^s-(-\cdot152^s)=-\cdot081^s.$$

First wire minus each of the following wires from Nov. 20 till Nov. 29.

T. R.

$$\frac{+\cdot052^s+\cdot049^s+\cdot034^s+\cdot053^s}{4}=+0\cdot47^s \quad \frac{-\cdot040^s-\cdot022^s-\cdot049^s-\cdot033^s}{4}=-\cdot036^s$$

$$\text{Difference} = R-T = -\cdot083^s.$$

I must remark, however, that I did *not* find a well-defined corresponding difference between bright and faint stars in the observations for relative personal equation with the equatorial.

IV[2].—*Does a variation of the interval between the wires affect the personal equation?*

From May 26 till June 27, ten stars were attached to the cylinder, the first three being separated by an interval of from ten to fifteen seconds, and the last seven by an interval varying between two and three seconds.

R—T.

	Mean of the first three wires.	Mean of the last seven wires.
May 26	=—·201[s]	—·068[s]
June 2	·080	·075
4	·086	·088
9	·187	·059
15	·116	·052
17	·155	·091
22	·152	·051
26	·122	·064

	Differences.	
May 26	=—·133[s]	
June 2	·005	
4	·000	
9	·128	Mean =—·069[s]
15	·064	
17	·064	
22	·101	
26	·053	

While these differences do not agree well with each other they all have the same sign. The disagreement is without doubt partly due to certain local disturbances on some of the wires, which will be noticed under the next head.

V.—*Does the shape of the stars observed affect the personal equation?*

By comparing the values R.—T. for each star, I found in several instances that two and sometimes three stars gave results widely differing from the rest, for two or three days at a time, after which the difference would disappear. As these differences did not occur in the values R.—B., it is evident that the variations were mainly due to the observations of T. I can attribute this to no other cause than the influence which the shape of the stars had upon the judgment of the observer in determining the time of transit. In actual observations of bright stars there is no doubt but that projecting wisps of light affect the personal equation. Something analogous to this may have occurred in the present instance. I can assign no reason for the recurrence of the disturbances.

I close this investigation with the following inquiry:

Does the relative equation derived from artificial stars agree in value with that derived from actual observations with the transit instrument?

This inquiry is an important one; for if this agreement is found to exist, it will then be easy to free longitude from the error arising from the variability of personal equation, by determining the value of this function by means of artificial stars, the observations for this purpose being nearly coincident with those of longitude.

The observations of actual stars were made with the equatorial, four, of four wires, constituting a complete set. Thus:

R observed the first four wires.
T " " last " "
R " " " " "
T " " first " "

Reducing each observation by the interval from each wire to the mean of the wires, and taking the arithmetical mean of the results, the relative equation was found free from the error of wire intervals.

R.—T. from the equatorial.		R.—T. from artificial stars.
May 31	=—·08^s	—·113^s
June 2	·11	·079
4	·15	·087
9	·16	·103
15	·09	·071
17	·06	·110
22	·16	·085
Mean from 880 comparisons,	=—·116^s.	Mean from 1,000 comparisons, =—·093^s

I do not positively assert that this agreement will exist in every case, because—

(*a*.) The result might have been different had the observations been made with the transit instrument.

(*b*.) The same agreement might not exist with other observers.

I do not consider that the results which I have found, settle definitively any point except the general variability of the personal equation. The conditions of the problem are so complex that it is impossible to assume one condition and reject the consideration of all others. The last inquiry is worthy of further investigation, as affording the means of obtaining the most probable value of the relative equation.

ART. XXVI.—*Upon the Atomic Volumes of the Elements;* by FRANK WIGGLESWORTH CLARKE, S.B.

IN a recent paper in this Journal I called attention to some remarkable relations which seem to exist between the atomic volumes of certain elements in the liquid condition. Since that time I have found similar relations between the same elements in the solid state, and also between the atomic volumes of some other solid elements of which no liquid compounds are known.

A few relations between the atomic volumes of different metals have previously been observed. Chief of these we find the equality of the atomic volumes of the metals of the iron group, the similar equality connecting the platinum metals, the equality between silver and gold, and that between molybdenum and tungsten. But, as far as I have been able to learn, nothing seems to have been done toward studying the more remarkable multiple relations which connect the members of certain natural groups with each other.

None of the specific gravities cited in this paper are due to my own determinations. I have simply collected the most reliable data which I have been able to find upon specific gravity, and thence calculated the atomic volumes of the elements. But in every case I shall try to give the authority to whom the experimental observations are due.

The alkaline metals most appropriately come first in order. For the first four only have we any data, since cæsium has not been isolated. The specific gravities are as follows. Lithium 0·589, Bunsen; sodium 0·972, and potassium, 0·865, Gay Lussac and Thenard; rubidium 1·52, Bunsen. Upon dividing the atomic weights of the four metals by these specific gravities we get as their atomic volumes respectively the numbers 11·9, 23·7, 45·1, and 56·2. It will at once be seen that the three highest of these values are very nearly multiples by whole numbers of the lowest. If we assume that in reality an exact multiple relation connects the members of this group, and then, adding together the atomic volumes actually found for them, we take an average in order to get the multiples with the least possible alteration of our data, we obtain for these four metals the values 11·4, 22·8, 45·6, and 57·0. These stand to each other as 1 : 2 : 4 : 5, and vary but very slightly from the numbers actually found, and, moreover, correspond to these trifling alterations in the specific gravities, respectively, ·025, ·037, ·010, and ·022. Is it not safe to assume that under strictly comparable circumstances this apparent multiple relation would prove to be exact?

The oxygen group, oxygen, sulphur, selenium, and tellurium, affords a still more remarkable series of values. The atomic volume of oxygen in its solid compounds, varies according to the nature of the combination. For this element in the solid state Kopp has deduced three values,—2·6, 5·2, and 10·4. Kopp himself has called attention to the fact that these stand to each other as 1 : 2 : 4.* For this element in liquid compounds the same observer deduced the values 7·8 and 12·2. If we compare these with the values for solid oxygen we shall find that the four lowest numbers stand exactly in the ratio of 1 : 2 : 3 : 4, and that an alteration of 0·8 in the highest, making it 13·0 instead of 12·2, will place it also as a fifth multiple of the lowest. I cannot but think it probable that Kopp himself must have noticed this relation, although I find no mention of it in any paper of his that I have seen. The atomic volume of sulphur varies, both with its crystalline form, and with the state of combination. The specific gravity of the octahedral modification, according to Marchand and Scheerer, is 2·045. This gives us for its atomic volume the number 15·6, precisely three times the second value for oxygen. In the liquid condition also, it will be remembered that the lower value for sulphur, 23·4, is three times the lower value for oxygen.

Many metallic sulphids possess atomic volumes very nearly the sums of those of the free sulphur and the metal. The native sulphids, syepoorite, CoS, millerite, NiS, and troilite, FeS, however, seem to vary from this rule. Their atomic volumes, deduced from the specific gravities given in "Dana's Mineralogy," are respectively 16·9, 17·6, and 18·4. But here it must be borne in mind that the native minerals are not absolutely pure, and that the specific gravity of each mineral often varies considerably. Since the metals in these three sulphids have equal atomic volumes, and since their corresponding compounds in most cases have equal values also, it seems safe to assume that these minerals in an absolutely pure condition, would possess also equal atomic volumes. The mean of the three numbers above given is 17·6. If we subtract from this the value of the metal, 6·9, we get 10·7, as the atomic volume of the sulphur, a number varying but 0·3 from twice the second number for oxygen. As we pass on we shall meet farther confirmations of this apparent multiple relation. In the liquid state sulphur has three atomic volumes, two of which,

* Buff, Kopp and Zamminer. "Lehrbuch der Physikalischen und Theoretischen Chemie."

23·4 and 28·6, are cited in my last paper.* The first of these is three times the lower value for liquid oxygen, and of course is also a multiple of 2·6, which seems to be the starting point for this series. The second is also an exact multiple of 2·6. In two observations Buff obtained for the atomic volume of sulphuric anhydrid at its boiling point, the numbers 44·18 and 44·19. Regarding this compound as $(SO_2)O$, and taking Kopp's determination of the higher value for liquid oxygen, he deduced from these numbers as the atomic volume of hexvalent sulphur in its liquid compounds, the value 12·0. If, however, we take as the higher value of oxygen the altered number 13·0, we shall get as the value of sulphur in $(SO_2)O$ the number 10·39, which agrees closely with the 10·4 above suggested for sulphur in the solid sulphids of iron, cobalt and nickel.

The approximate equality between the atomic volumes of solid sulphur and selenium has often been noticed, and in my last paper I showed that a similar equality probably existed in the liquid condition. The specific gravity of crystalline selenium, according to Hittorf, is 4·808. This gives us as its atomic volume, 16·5, which exceeds by 0·9 the value for octahedral sulphur. An exact equality between sulphur and selenium, however, seems to me doubtful, since I have repeatedly noticed that, *as a rule*, the atomic volumes of selenids slightly exceed those of the corresponding sulphids, and also that the selenates have values slightly in excess of the sulphates. Yet there are striking exceptions to this, and the point is by no means decided.

Like the sulphids, many selenids have atomic volumes equal to the sum of those of the selenium and the metal. For the proto-selenid of iron we have no data, but the artificial selenids of cobalt and nickel, CoSe and NiSe according to Little, have the specific gravities 7·65 and 8·46, whence we obtain for their atomic volumes the numbers 18·2 and 16·9, or a mean of 17·5. This is very near the mean obtained for the sulphids, and gives us as another value for selenium, the number 10·6, only 0·2 greater than the double multiple of oxygen.

The sp. gr. of tellurium, according to Löwe, is 6·18, and hence we get as the atomic volume of this element, the value 20·7. 20·8 is exactly four times the second value for oxygen.

We now see that in the solid condition the atomic volumes of these four elements are to each other very nearly in the relation of 1 : 3 : 3 : 4, and since the first three preserve the

* My paper in the March No. of this Journal was sent to press before I had seen Buff's calculation of the atomic volume of hexvalent sulphur. See Buff's "Grundlehren der Theoretischen Chemie"

same relation in the liquid state, we must naturally expect that the fourth, (for which we have no data in its liquid compounds) will follow the same rule also. But this, although so extremely probable, remains to be proved. Other theoretical reasons might be adduced in support of the idea, but I prefer leaving the question for experiment to settle. In order to show more clearly the remarkable relations connecting the different atomic volumes of this group, I have arranged the values for these elements, both in the solid and liquid state, in the following tabular form. The independent numbers are those determined by theory, those in parentheses being the values actually determined.* This is in order to show the extent of alteration when change has been necessary.

Oxygen,	2·6	5·2	7·8	10·4	13·0 (12·2)	---	---	---	---
Sulphur,	--	--	--	10·4 (10·7)		15·6	---	23·4 (22·6)	28·6
Selenium,	--	--	--	10·4 (10·6)		15·6 (16·5)		23·4 (23·2)	---
Tellurium,	--	--	--	---	---	---	20·8 (20·7)		---

Of these thirteen numbers six need no alteration, and the amount of change in the other seven, varies from a minimum of 0·1 to a maximum of 0·9. Moreover, from 2·6 up to 28·6, but two multiples of the lowest are wanting, and if we place, from theoretical reasons given above, the atomic volume of liquid tellurium at 31·2, this relation becomes still more noteworthy.

It is worth while in this connection to observe the remarkable multiple relation connecting the atomic weights of this group.

Between the different allotropic conditions of sulphur and selenium I find no numerical relations whatever. The atomic volume of prismatic sulphur I find to be from 16·3 to 16·7, and that of amorphous selenium to be 18·6. These values find no place in the series above given.

The nitrogen group affords another remarkable series of relations. In the liquid state, it will be remembered, nitrogen has three values, 2·15, 8·6, and 17·2, all multiples of the lowest. Then came boron, phosphorus, vanadium, and arsenic, with a common value, three times the second number for nitrogen. In the solid state, however, we find a variation from this. For nitrogen itself we have few suitable data, and I have made no elaborate calculations concerning it; yet as far as I have examined, it seems to possess several values. One of these is easily found. Kopp determined the atomic volume of NO_3 in nitrates, as 28·6: and if we subtract from

* In the numbers which required no alteration, parentheses seemed unnecessary.

this three times the most common value for oxygen, or 15·6, we have left, as the atomic volume of the nitrogen, the number 13·0. The specific gravity of crystalline red phosphorus, ("metallic phosphorus") according to Hittorf is 2·34, and hence, as its atomic volume, we get 13·2. Bettendorf found the specific gravity of crystalline arsenic to be 5·727, which gives us the atomic volume 13·1. For free vanadium we have no data, but the oxyd V_2O_2 has the specific gravity 3·64, (Schafarik) and hence the atomic volume 37·1. From this, subtracting the value of the oxygen, giving the latter element its second value, we obtain as the atomic volume of vanadium the number 13·3.

It must be borne in mind that both in this case, and with the radical NO_3, it is a pure assumption to give oxygen its second value, instead of its first or third, yet the assumption seems warranted by the results. From these numbers we see that in the solid state, some at least of the atomic volumes of nitrogen, phosphorus, vanadium, and arsenic, are equal, the mean being 13·15. 12·9 (a change of 0·25) is precisely half the atomic volume of the three latter elements in the liquid state, and a multiple by 6 of the lowest value for nitrogen. In connection with the apparent equality between phosphorus and arsenic, the fact is perhaps worth recalling that the alkaline phosphates and arsenates with 12 aq. were found by Playfair and Joule to have equal atomic volumes.

Antimony was found by Dexter to have a specific gravity from 6·707 to 6·718. This gives its atomic volume as 18·1. If we make this 0·9 less, or 17·2, it becomes precisely half the value for this metal in its liquid compounds. Bismuth, according to Marchand and Scheerer, has a specific gravity of 9·799. Its atomic volume, then, is 21·43. 21·5 is exactly 10 times the lowest value of nitrogen. In the liquid state its atomic volume is a matter of uncertainty, since the specific gravity of only one volatile compound of bismuth has been taken. In my last paper I suggested that perhaps this metal in its liquid compounds might have the same value as antimony, although farther investigations might place it higher. If now, we assume that bismuth, like phosphorus, vanadium, arsenic, and antimony has in the solid state an atomic volume half that which it possesses in liquid compounds, we shall get as the atomic volume of liquid bismuth, the number 42·86, or more probably, 43·0, which is a multiple of the second value of liquid nitrogen. This seems highly probable, yet cannot be regarded as settled. Nevertheless, it will be seen that the values actually found for the metals arsenic, antimony, and bismuth, are to each other very nearly as 3 : 4 : 5.

The atomic volume of ordinary phosphorus is from 16·9 to 17·0, and that of the amorphous red variety from 13·9 to 14·5. That of amorphous arsenic is 15·9. These numbers seem to bear no distinct relations to the others.

In its liquid compounds the atomic volume of boron is equal to that of phosphorus and arsenic, but this relation does not hold true for the solid element. The specific gravity of the diamond modification is given in Graham-Otto's "Lehrbuch" as 2·68, whence we get 4·1 as the atomic volume. If we place this element in the nitrogen group, as its atomic volume in the liquid state would induce us, it is worth while to notice that its value here is very nearly twice the lowest number for nitrogen, and one-sixth (if the alteration be made) of its value in liquid compounds.

Passing now to the carbon group, we still find close multiple relations existing. The sp. gr. of the purest graphite is 2·25 (Brodie) and hence its atomic volume is found to be 5·3. If we alter this to 5·5, we have just half the value of carbon in its liquid compounds. In the "Handwörterbuch" the specific gravity of graphitoidal silicon is given as 2·49, and from this we get 11·2 as its atomic volume. This is 0·2 greater than a multiple by two of the altered value for graphite.

For titanium in the free state I have been able to find no data, but in its compounds it approximates closely to silicon. Rock crystal has the sp. gr. 2·663 (Deville), and an atomic volume of 22·5.* Octahedrite, TiO_2, has a sp. gr. of 3·82—3·95, corresponding to an atomic volume of 21·5. If we assume that the atomic volumes of silicon and titanium are equal, and double the altered value for graphite, we shall find as the calculated atomic volume of rock crystal and of octahedrite, the number 21·4, (giving oxygen its second value.) Again, the native mineral enstatite, $MgSiO_3$, has a sp. gr. of 3·13, and an atomic volume of 31·9. The artificial titanate, $MgTiO_3$, obtained by Hautefeuille, has a sp. gr. of 3·91, and hence the atomic volume 31·2. If we also take into account the seeming equality between the atomic volumes of these elements in their liquid compounds, the conclusion concerning their values in the solid condition seems almost inevitable.

The specific gravity of tin, according to Miller, is 7·177. Hence its atomic volume is 16·4, which is only 0·1 less than three times the changed value for graphite. If now we take into review these four tetrad elements we see that with very trifling variations, their atomic volumes for the solid state are

* For specific gravities of minerals quoted in this paper, see the last edition of "Dana's Mineralogy."

to each other as 1 : 2 : 2 : 3. In the liquid state these elements have values standing to each other as 1 : 3 : 3 : 4, and therefore, although in the case of carbon a very simple connection appears between the two states of aggregation, with the higher members of the group the relations become complicated.

The specific gravity of the diamond is 3·55, according to Pelouze, and from this we get as its atomic volume the number 3·4. Although this seems to stand in no direct relation to the other values found in this group, still it affords a starting point for a number of metals, as I shall show hereafter. In connection with the carbon group the tetratomic metal zirconium is worth noticing. Its sp. gr. is given by Troost as 4·15. From this we get the atomic volume as 21·7, a value 0·3 less than four times that of graphite. This, however, may be accidental.

Another noteworthy series of relations appears between the atomic volumes of certain groups of metals. In this series the iron and zinc groups are connected, and possibly also the platinum group is involved. The equality between the atomic volumes of the members of the iron group has often been noticed, yet, for the sake of completeness, I have thought it advisable to recalculate their values. The specific gravities are as follows: Chromium 7·3, Bunsen; manganese 8·013, John; iron 7·844, Bröling; nickel 9·118, and cobalt 8·957, Rammelsberg; uranium 18·4, Peligot; copper 8·94, a mean of five determinations by Marchand and Scheerer. Deduced from these, their atomic volumes are respectively, 7·1, 6·7, 7·1, 6·7, 6·7, 6·5, 7·1, the average being 6·84.

The sp. gr. of magnesium is 1·743 (Bunsen), that of cadmium 8·6 (Graham-Otto's "Lehrbuch") and that of frozen mercury is given by Schulze as 14·391. Hence the atomic volumes of these three metals are respectively 13·8, 13·0, and 13·9, or, very probably, equal, the average being 13·6. Zinc, which we would naturally expect to find classed with magnesium and cadmium, varies, its sp. gr. being 7·03—7·2, (Bolley) and hence its atomic volume 9–9·2. Now it has been observed that the protosulphates of magnesium, zinc, iron, nickel, and cobalt, and also the double sulphates of magnesium and copper, magnesium and zinc, and magnesium and cadmium, all of which contain 7 aq, possess equal atomic volumes.* Now, since all of these compounds are formed upon precisely the same type, and since an element may possess different atomic volumes in different compounds, we should be inclined to suspect definite relations between the values of the different metals forming these salts. These metals in the free state represent three dif-

* See "Watts' Dictionary," vol. i, Art. "Atomic Volume."

ferent values, viz: 6·84 for the iron group, 9·0–9·2 for zinc, and 13·6 for the cadmium group, yet in these sulphates all appear to possess the same atomic volume. Now it is noteworthy that 13·6 is almost exactly twice 6·84, and zinc stands between in such a manner that the three values are to each other almost exactly as 3 : 4 : 6. Whatever may be the place of zinc in this series, whether accidental or not, it is certainly very remarkable that the metals cadmium and magnesium, forming sulphates isomorphous with those of iron, cobalt, and nickel, should possess an atomic volume almost precisely double that of the latter metals. This relation must be more than a mere coincidence.

Another point to be noticed in regard to zinc and mercury is an exception to a rule which has seemed to hold true with the groups hitherto given. Between the atomic volumes possessed by these metals in the solid state, and their values in the liquid condition, no direct relation is manifest.

The platinum metals afford another remarkable instance of equality among atomic volumes, as has often been noticed. The specific gravities are as follows, according to Deville and Debray. Platinum 21·15, iridium 21·15, osmium 21·3–21·4, rhodium 12·1, ruthenium 11—11·4, palladium 11·4. Their atomic volumes are respectively 9·3, 9·3, 9·2–9·3, 8·5, 9·1–9·4, and 9·3. The average is 9·15, almost exactly the value for zinc, although this is probably a mere coincidence. A revision of the sp. gr. of rhodium will probably place it lower, and increase the atomic volume, so as to make the value for this group about 9·3.

Molybdenum and tungsten afford another well known examample of equality. The sp. gr. of the first is 8·64, (Bucholz,) and that of the second from 16·54—18·447, according to the state of aggregation and mode in which it was prepared, according to the observations (independent of each other) of Van Uslar and Zettnow. The atomic volume of molybdenum is therefore 11·2, and that of tungsten from 9·9–11·1. If instead of 11·1–11·2, we place the value for these metals at 11·4, we find that the latter number falls into a vacant place in the iron, zinc, and cadmium series, standing in that series so as to make the four values to each other as 3 : 4 : 5 : 6. This may be accidental, however.

The equality between silver and gold is also well known, the atomic volumes of these two metals being represented by the number 10·2.

A noteworthy relation to oxygen is found in the atomic volumes of the remarkably similar elements calcium, barium, strontium, and lead. The sp. gr. of calcium is 1·55 (Liés Bodart and Jobin), that of barium is 4·0 (Clarke), that of strontium 2·5418 (Matthiessen), and that of lead is 11·37 (Reich.) Hence the atomic volumes are respectively 25·8, 34·2, 34·3, and 18·2. These numbers approximate very closely to multiples of the lowest value for oxygen, 2·6. The number for lead requires no change whatever, that for calcium but 0·2, and those of barium and strontium but from 0·4–0·5, to produce the exact multiples. Whether this relation is accidental or not, it is impossible for me to say, but the coincidence is certainly remarkable.

Another similar relation we find between the atomic volume of the diamond, and those of certain metals which are often classed as tetratomic. The sp. gr. of glucinum is 2·1 (Debray), that of aluminum 2·56–2·65 (as given in the "Handwörterbuch), and that of thorium 7·657–7·795, (Chydenius). Hence their atomic volumes are respectively 6·7, 10·1–10·6, and 30·4–30·9. The atomic volume of the diamond is 3·4, that of glucinum wants but 0·1 of twice that, three times 3·4 is within the limits between which aluminum varies, and nine times 3·4, or 30·6, is within the limits for thorium. Cerium, according to Wöhler, has the sp. gr. 5·5, corresponding to an atomic volume of 16·7, which is 0·3 less than an exact multiple of that of the diamond. Perhaps this metal might be classed with the three above mentioned.

The remaining metals, as far as we have data, seem to find places in none of the previous series. Thallium, according to Crookes, has a sp. gr. of 11·81–11·91. Its atomic volume, then, is 17·2. Winkler gives the sp. gr. of indium as 7·421, which corresponds to an atomic volume of 10·2. For niobium, tantalum, lanthanum, didymium, yttrium, and erbium, we have no data whatever.

The sp. gr. of iodine is 4·947 (Handwörterbuch). This gives as its atomic volume the number 25·6, almost exactly two thirds of its value in its liquid compounds. The atomic volumes of bromine and fluorine in their solid compounds I have not been able to calculate in any satisfactory manner from the data at my command, since the values obtained did not agree with each other. The atomic volume of chlorine in solids, has been calculated by Kopp, who obtained two values; but between these and its value in the liquid state I have found no relations.

For hydrogen, the data at hand are quite complicated, not being in any connected series of compounds, and as far as I have been able to decide, it appears to have several values. However, in some of its solid compounds, hydrogen seems to possess the same atomic volume as in the liquid state, viz., 5·5. If we calculate the specific gravities of cerotene, $C_{27}H_{54}$, and melene, $C_{30}H_{60}$, upon the basis of this value for hydrogen, giving carbon the amended atomic volume of graphite, we shall get for the first the sp. gr. 0·846, and for the second 0·848. The specific gravities actually found for these bodies, as given in Weltzien's "Systematische Zusammenstellung," are respectively 0·861, and 0·890, a variation in the first case, of only 0·015, and in the second case of 0·042, from the calculated values.

Having thus compared the atomic volumes of many elements with each other, a question which now naturally arises, is, how far are the relations which have been found, significant, and how far are they accidental.

In work of this kind, great caution is necessary, for a slight prejudice in favor of the kinship of two elements might often lead one greatly astray. Thus, gold, silver, aluminum, and indium, possess almost exactly the same atomic volume, about 10·2, and yet no direct chemical relations are known to connect them, and they seem to belong to four different metallic groups. Probably of a similar nature is the seeming relation between zinc and the platinum metals. Yet in spite of these dangers of error, some of the relations which are found between the atomic volumes of certain elements, cannot but be considered as deeply significant. The series of relations which I regard as fully made out, are the following: The multiple series of the alkaline metals; the series formed by oxygen, sulphur, selenium, and tellurium; the nitrogen series, and also that formed by carbon, silicon, tin, and titanium. The equal values for the platinum metals, and the similar case of the iron group. Of the series less thoroughly made out, or less easy to account for, I am inclined to regard that which connects the iron group with zinc and the cadmium group and possibly also with molybdenum and tungsten, as genuine. Also that series connecting the metals of the alkaline earth with oxygen. The series formed by the diamond, with glucinum and the kindred metals, and also the seeming relation between lead and oxygen, appear to me more questionable.

As a rule it will be seen that whenever an element possesses more than one atomic volume in its compounds, these different values are distinctly related to each other, and also to the value of that element in the free state. But between the different allotropic forms of an element, we find no distinct relations at all! If this be found to hold true, it will be plain that the different properties possessed by the same element in different compounds cannot be accounted for upon the supposition that in one compound the element exists in one allotropic form, and in another compound in another.

One of the most important points to which I have been led, seems to me to be the very direct and simple relations connecting the atomic volumes of an element in two different states of aggregation. In the oxygen series, the most common value for each element in the solid state is almost precisely two thirds of that which it has in the liquid condition. Phosphorus, arsenic, vanadium, antimony, and bismuth seem to have in the solid state, values just half those which they possess as liquids, and carbon seems to follow the same rule.

The exceptions found under zinc and mercury, however, must be borne in mind, showing as they do, that the law is not a general one. But how far these relations between solids and liquids holds true, seems to me a matter of great interest, as possibly affording a clue to some general law connecting the different states of aggregation.

One more point and I am done. In many cases, even when the relations between the elements have been plainest, it has been necessary to slightly alter the atomic volumes actually found, in order to bring them in accordance with theory. Some of these alterations have been wholly within the limits of experimental error, yet others are in this respect more doubtful. Now it has often been suggested that the atomic volumes of solids, like those of liquids, should be compared at similar temperatures, and under corresponding circumstances. For solids, the similar temperatures are supposed to be the melting points. Now in many of those cases in which I have altered the values actually found for elements, it would be wholly unjustifiable for me to ascribe the variations from theory to experimental error. And yet the multiple relations are often extremely plain, and by their occurrence in so many different series, they in a certain sense confirm each other. Therefore, taking all things into consideration, I am firmly convinced that whenever the atomic volumes of the elements are compared under strictly similar circumstances, then the relations between them will be found exact. In other words, the variations from theory are probably due to the fact that the specific gravities from which we calculate, were determined under dissimilar conditions. Concerning the cause of these relations between different elements, I shall hazard no conjectures. Not only are our data too incomplete, and unfit to form the basis of any theory, but it seems to me that until we know more of the nature of the elements themselves, it will be too early to attempt to frame any hypothesis whatever, concerning the cause of relations between them.

ART. XXVII.—*Contributions from the Laboratory of the Lawrence Scientific School. No. 7.—On some Minerals from Newlin Township, Chester Co., Penn., described by Dr. Isaac Lea;* by S. P. SHARPLES, S.B., Assistant in Chemistry.

A YEAR or two ago, Wm. W. Jefferis, of West Chester, Penn., gave me several minerals for examination which had been described by Dr. Lea in a paper read before the Academy of Natural Sciences of Philadelphia, April 9th, 1867.

The first of these that I examined was the Diaspore which Prof. Brush described in this Journal and of which an analysis is given on page 169, of Dana's Mineralogy. The diaspore was embedded in a mineral which Prof. Dana calls margarite (loc. cit.), and which Dr. Lea in his paper spoke of as emerylite. I found it to contain silica, alumina, potassa and water, with traces of iron, magnesia and soda in the following proportions:

	1.	2.	3.	4.	5.	Mean.	Oxygen.
SiO_2	43·46	43·85	43·57	----	----	43·56	23·21
Al_2O_3	----	38·12	38·20	----	----	38·16	17·76
H_2O	----	5·71	5·58	----	----	5·64	5·01
K_2O	----	----	----	10·79	10·82	10·81	2·18

Sp gr. 2·87.

The oxygen ratio as will be seen, is for $\dot{R}$, $\bar{\bar{R}}$, $\ddot{S}i$, $\dot{H}$, 1:8·5:11:2, which corresponds with that given for damourite.

The mineral occurs in mica-like crystals, some of which are from one to two inches across. Some of the specimens in Dr. Lea's collection have well crystallized edges. In color it varies from white to greenish or yellowish. Before the blowpipe it fuses with extreme difficulty to a white enamel.

Lesleyite.—This mineral is described by Dr. Lea as follows: "Fibrous or lamellar, sometimes inclining to massive. Color whitish passing into reddish. Hardness about three. Streak white. Before the blowpipe it parts with its water and becomes opaque white. Does not fuse with borax. Does not dissolve in muriatic acid. Under the microscope it presents no observable characteristics.

"Its gravity is greater than that of quartz. There is a disposition in the crystalline fibrous structure to diverge from a central point, to be stellate, and in one crystal before me, the radiating fibers are nearly four inches long."

Analysis of the white variety gave me silica, alumina, potassa and water with traces of iron, soda, lithia and magnesia.

	1.	2.	3.	4.	Mean.	Oxygen.
SiO_2	33·63	33·54	----	----	33·59	16·88
Al_2O_3	55·38	55·45	----	----	55·41	25·82
K_2O	----	----	7·30	7·57	7·43	1·27
H_2O	4·31	4·29	----	----	4·30	3·82

Sp. gr. 3·203

Oxygen ratio for $\dot{R}$, $\bar{R}$, $\ddot{Si}$, $\dot{H}$, 1 : 20 : 14 : 3.

The reddish gave C. W. Rœpper of Bethlehem, Penn.:

	1.	2.	3.	Mean.	Oxygen.
SiO_2	47·14	46·87	----	47·00	25·06
Al_2O_3	33·56	32·98	----	33·27	15·50
Fe_2O_3	2·75	2·93	----	2·84	·85
K_2O	----	----	9·97*	----	1·69
H_2O	6·73	6·70	----	6·71	5·97

Sp. gr. 2·87

Oxygen ratios for $\dot{R}$, $\bar{R}$, $\ddot{Si}$, $\dot{H}$, 1 : 9·7 : 14·8 : 3·5.

These minerals occur as a coating on corundum and seem to pass insensibly into each other. Prof. Cooke has quite good specimens of the brown variety that came from Sparta, N. York, where it also occurs coating the corundum.

Both varieties are evidently the products of the action of water containing alkaline silicates upon the corundum, and are more nearly related to pinite than to any other mineral species. For pinite, Dana gives the formula ($\dot{R}$, $\bar{R}$), $\ddot{Si}$, $\dot{H}$, 3 : 4 : 1. For the white variety the ratio is 7 : 5 : 1, while the reddish gives 3 : 4 : 1.

Pattersonite.—Of this mineral Dr. Lea says: "Basal cleavage imperfect, rarely if ever presenting a hexagonal prism, but disposed to present triangular plates which joining make a sub-tetrahedral mass. The laminæ are not flexible and but slightly translucent. The color is metallic bluish gray, resembling zinc. The streak is grayish. Before the blowpipe parts with its water but does not exfoliate nor intumesce. With borax gives a black bead." I found it to contain silica, iron, alumina, magnesia, potassa and water, with traces of soda and lithia. The potassa was determined by difference owing to the very small amount of the mineral at my command.

	1.	2.	Mean.	Oxygen.
SiO_2	30·20	----	30·20	16·11
Fe_2O_3	14·88	----	14·88	} 14·04
Al_2O_3	20·48	20·61	20·55	}
MgO	1·19	1·38	1·28	} 2·43
K_2O	----	----	11·35	}
H_2O	11·80	11·66	11·73	10·43

Ratio for $\dot{R}$, $\bar{R}$, $\ddot{Si}$, $\dot{H}$, 1 : 5·7 : 6·6 : 4·3 ; for ($\dot{R}$ $\bar{R}$), $\ddot{Si}$, $\dot{H}$, 3 : 3 : 2·

* Sharples.

This analysis and ratio does not seem to agree with any of the hydrous silicates in Dana's Mineralogy, and the species is therefore probably a new one. It occurs very sparingly implanted upon Lesleyite, and like it, is most probably the product of the alteration of the corundum.

I am indebted to Dr. Lea for specimens of all of these minerals and for the use of his large specimens for comparison with each other.

Cambridge, Mass., Jan. 8, 1869.

Art. XXVIII.—*On the Washing of Precipitates;* by R. Bunsen.*

A precipitate is washed either by filtration or by decantation: in the former case the portion of liquid not mechanically retained is allowed to drain from the precipitate; in the latter it is separated by simply pouring it away, the foreign substances contained in the precipitate being then removed by the repeated addition of some washing-fluid, in each successive portion of which the precipitate is, as far as possible, uniformly suspended, this process being continued until the amount of impurity becomes so minute that its presence may be entirely disregarded.

Supposing v to represent the volume of the moist precipitate remaining at the bottom of the vessel after decantation, or upon the filtrate after filtration, V the volume of wash-water employed at each successive decantation, n the number of decantations, and $\frac{1}{a}$ the fraction expressing the proportion of the original amount of impurity still remaining in the precipitate after n decantations, then

$$\left(\frac{v}{v+V}\right)^n=\frac{1}{a}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Calling W the total volume of wash-water resulting from n decantations, then

$$nV=W; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

therefore

$$\left(1+\frac{W}{nv}\right)^n=a,$$

or

$$W=nv(\sqrt[n]{a}-1). \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

If we differentiate W with respect to n and make the differ-

* From Lond., Edin. and Dublin Phil. Mag., Jan. 1869. Translated from the Ann. der Chem. und Pharm, vol. cxlviii, p. 269.

ential quotient equal to 0, then the minimum value of W becomes, when $n=\infty$,

$$W=v \text{ nat. log. } a. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Precipitates obtained in the course of chemical analysis may in all cases be assumed to be sufficiently washed when the impurity retained by them amounts to no more than the $\frac{1}{100000}$ part. Making therefore $a = 100000$ and $v = 1$, it results from equation (4) that the least quantity of fluid required in order to remove the impurity contained in a precipitate to the $\frac{1}{100000}$ part, amounts to eleven and a half times the volume occupied by the precipitate itself in the liquid in which it exists. It is evident, therefore, that the amount of water actually necessary to wash a precipitate the more nearly approaches this minimum the oftener we decant, and the smaller the quantity of washing-water we employ at each decantation.

Since some of the principal sources of error in analytical work consist in the incomplete or in the too protracted washing of precipitates, it becomes important to know how to ascertain the progress of the washing throughout the several stages of the process. By employing the same volume of water at each successive addition, and est mating its relation to that of the precipitate remaining at th bottom of the vessel or upon the filter, we can find from the following Table, calculated by

$\frac{1}{100000}$.			$\frac{1}{50000}$.			$\frac{1}{20000}$.			$\frac{1}{10000}$.		
I.	II.	III.	I.	II.	III.	I.	II.	III.	I.	II.	III.
$\frac{V}{v}$.	n.	W.	$\frac{V}{v}$.	n.	W.	$\frac{V}{v}$.	n.	W.	$\frac{V}{v}$.	n.	W.
0·5	28·4	14·2	0·5	26·7	13·3	0·5	24·4	12·2	0·5	22·7	11·4
1	16·6	16·6	1	15·6	15·6	1	14·3	14·3	1	13·3	13·3
2	10·5	21·0	2	9·8	19·7	2	9·0	18·0	2	8·4	16·8
3	8·3	24·9	3	7·8	23·4	3	7·1	21·4	3	6·6	19·9
4	7·1	28·6	4	6·7	26·9	4	6·1	24·6	4	5·7	22·9
5	6·4	32·1	5	6·0	30·2	5	5·5	27·6	5	5·1	25·7
6	5·9	35·5	6	5·6	33·4	6	5·1	30·5	6	4·7	28·4
7	5·5	38·8	7	5·2	36·4	7	4·8	33·3	7	4·4	31·0
8	5·2	42·0	8	4·9	39·4	8	4·5	36·1	8	4·2	33·5
9	5·0	45·0	9	4·7	42·3	9	4·3	38·7	9	4·0	36·0
10	4·8	48·0	10	4·5	45·1	10	4·1	41·3	10	3·8	38·4
11	4·6	51·0	11	4·4	47·9	11	4·0	43·8	11	3·7	40·8
12	4·5	53·9	12	4·2	50·6	12	3·9	46·3	12	3·6	43·1
13	4·4	56·4	13	4·1	53·3	13	3·8	48·8	13	3·5	45·4
14	4·2	59·4	14	4·0	55·8	14	3·7	51·1	14	3·4	47·5
15	4·2	62·3	15	3·9	58·5	15	3·6	53·6	15	3·3	49·8
16	4·1	65·0	16	3·8	61·1	16	3·5	56·0	16	3·3	53·0
17	4·0	67·8	17	3·7	63·6	17	3·4	58·3	17	3·2	54·2
18	3·9	70·4	18	3·7	66·1	18	3·4	60·5	18	3·1	56·3
19	3·8	74·3	19	3·6	68·6	19	3·3	62·8	19	3·1	58·4

means of the formula above given, the number of times it is necessary to decant in order to diminish the amount of impurity in the precipitate to the $\frac{1}{100000}$, $\frac{1}{50000}$, $\frac{1}{20000}$ or $\frac{1}{10000}$ part.

Column I. shows the relation between the volume of the precipitate and that of the washing-water employed for each successive decantation, column II. the number of decantations required to diminish the amount of impurity to the necessary extent, and column III. the total volume of water obtained from the several decantations.

When the washing-process is performed in a beaker, the relation between the volume of the precipitate and that of the liquid may be easily determined by holding a strip of paper along the side of the vessel and marking upon it the respective heights of the precipitate and supernatant liquid; then on folding the portion of paper lying between the two marks in such a manner that each fold corresponds to the height occupied by the precipitate, the number of folds will give the argument in column I. to find in column II. the number of decantations needed to wash to the required extent. If the washing be conducted as in the ordinary method of filtration, funnels possessing an angle of 60° must be invariably employed, and the capacities of the various-sized filters once for all determined by means of a burette. After the precipitate has been brought upon the filter and allowed to drain, it is mixed as thoroughly as possible with water from a graduated washing-flask. Call the amount of water thus necessary to fill the filter $\mathfrak{v}$, and the capacity of the empty filter $\mathfrak{V}$, then $\frac{\mathfrak{v}}{\mathfrak{V}-\mathfrak{v}}=\frac{V}{v}$ in column I; that is, the argument needed to find in column II the number of times it is necessary to refill the filter in order to wash the precipitate to the desired extent.

I by far prefer using this Table to employing the method generally followed of ascertaining the completion of the washing-process by evaporating a quantity of the filtrate on platinum-foil, since in the latter case it is only possible to obtain an infallible proof when we have to deal with a precipitate possessing an extremely high degree of insolubility; if the precipitate be soluble to any marked extent, the result is completely illusory.

In the process of filtration as hitherto conducted, the time required is so long and the quantity of wash-water needed so great that some simplification of this continually recurring operation is in the highest degree desirable. The following method, which depends, not upon the removal of the impurity by simple attenuation, but upon its displacement by forcing the wash-water through the precipitate, appears to me to combine all the requisite conditions and therefore to satisfy the need.

The rapidity with which a liquid filters, depends, *cæteris paribus*, upon the difference which exists between the pressure upon its upper and lower surfaces. Supposing the filter to consist of a solid substance, the pores of which suffer no alteration by pressure or by any other influence, then the volume of liquid filtered in the unit of time is nearly proportional to the difference in pressure: this is clearly shown by the following experiments, made with pure water and a filter consisting of a thin plate of artificial pumice-stone. The thin plate of pumice was hermetically fastened into a funnel consisting of a graduated cylindrical glass vessel, the lower end of which was connected with a large thick flask by means of a tightly fitting caoutchouc cork. The pressure in the flask was then reduced by rarefying the air by means of a method to be described upon another occasion; and for each difference of pressure p, measured by a mercury column, the number of seconds t was observed which a given quantity of water occupied in passing through the filter. The following are the results:—

I.

p. meter.	t. ″	pt.
0·179	91·7	16·4
0·190	81·0	15·4
0·282	52·9	14·9
0·472	33·0	15·6

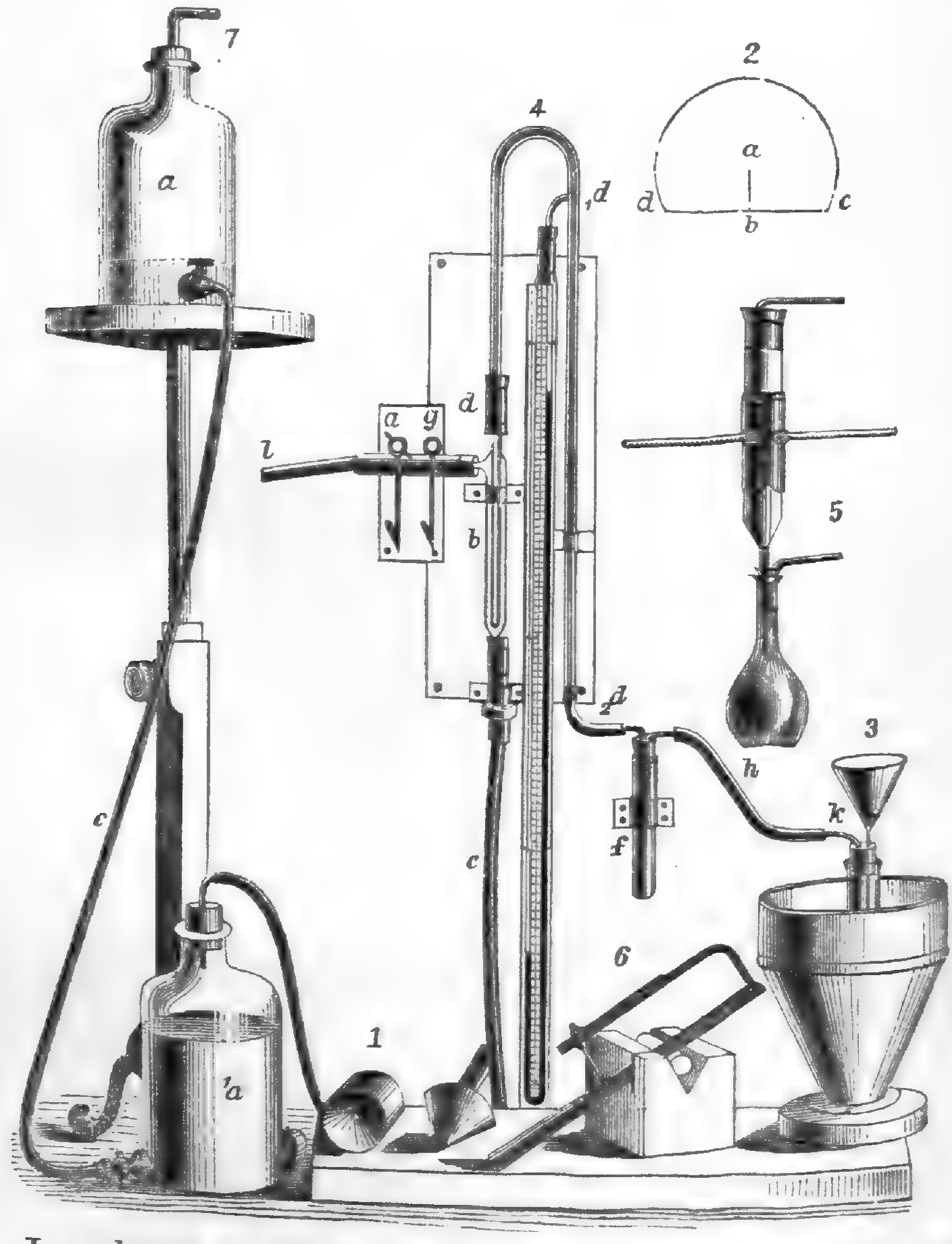
In the ordinary process of filtration, p on the average amounts to no more than 0·004 to 0·008 meter. The advantage gained, therefore, is easily perceived when we can succeed by some simple, practicable and easily attainable method in multiplying this difference in pressure one or two hundred times, or, say, to an entire atmosphere, without running any risk of breaking the filter. The solution of this problem is very easy: an ordinary glass funnel has only to be so arranged that the filter can be completely adjusted to its sides even to the very apex of the cone. For this purpose a glass funnel is chosen possessing an angle of 60°, or as nearly 60° as possible, the walls of which must be completely free from inequalities of every description; and into it is placed a second funnel made of exceedingly thin platinum-foil, and the sides of which possess exactly the same inclination as those of the glass funnel. An ordinary paper filter is then introduced into this compound funnel in the usual manner; when carefully moistened and so adjusted that no air-bubbles are visible between it and the glass, this filter, when filled with a liquid, will support the pressure of an extra atmosphere without ever breaking.

The platinum funnel is easily made from thin platinum-foil in the following manner:—In the carefully chosen glass funnel is placed a *perfectly accurately fitting* filter made of writing-paper; this is kept in position by dropping a little melted sealing-wax between its upper edge and the glass; the paper is next saturated with oil and filled with liquid plaster of Paris, and before the mixture solidifies a small wooden handle is placed in the center. After an hour or so the plaster cone with the adhering paper filter can be withdrawn by means of the handle from the funnel, to which it accurately corresponds. The paper on the outside of the cone is again covered with oil, and the whole carefully inserted into liquid plaster of Paris contained in a small crucible 4 or 5 centims. in height. After the mixture has solidified, the cone may be easily withdrawn; the adhering paper filter is then detached, and any small pieces of paper still remaining removed by gently rubbing with the finger. In this manner a solid cone is obtained accurately fitting into a hollow cone, and of which the angle of inclination perfectly corresponds with that of the glass funnel.

Fig. 1 represents the cones. By their help the small platinum funnel is made. A piece of platinum (fig. 2 shows the natural size)* is cut from foil of such a thickness that one square centimeter weighs about 0·154 grm., and from the center *a* a vertical incision is made by the scissors to the edge *c b d*. The small piece of foil is next rendered pliable by being heated to redness, and is placed upon the solid cone in such a manner that its center *a* touches the apex of the latter; the sides *a b d* are then closely pressed upon the plaster, and the remaining portion of the platinum wrapped as equally and as closely as possible around the cone. On again heating the foil to redness, pressing it once more upon the cone, and inserting the whole into the hollow cone and turning it round once or twice under a gentle pressure, the proper shape is completed. The platinum funnel, which should not allow of the transmission of light through its extreme point, even now possesses such stability that it may be immediately employed for any purpose. If desired, it may be made still stronger by soldering down the overlapping portion in one spot only to the upper edge of the foil by means of a grain or two of gold and borax; in general, however, this precaution is unnecessary. If the shape has in any degree altered during this latter process, it is simply necessary to drop the platinum funnel into the hollow cone and then to insert the solid cone, when by one or two turns of the latter the proper form may be immediately restored. The platinum funnel is placed in the bottom of the glass funnel,

* The size of this cut has been reduced about one third. The diameter of fig. 2, in the original drawing, is 2·5 millimeters.—Eds.

the dry paper filter then introduced in the ordinary manner, moistened, and freed from all adhering air-bubbles by pressure with the finger. A filter so arranged and in perfect contact with the glass, when filled with a liquid will support the pressure of an entire atmosphere without the least danger of breaking; and the interspace between the folds of the platinum-foil is perfectly sufficient to allow of the passage of a continuous stream of water.



In order to be able to produce the additional pressure of an atmosphere, the filtered liquid is received in a strong glass flask instead of in beakers.* This flask is closed by means

* These flasks must be somewhat thicker than those ordinarily used, in order to prevent the possibility of their giving way under the atmospheric pressure.

of a doubly perforated caoutchouc cork, through one of the holes of which the neck of the glass funnel is passed to a depth of *from 5 to 8 centimeters* (fig. 3); through the other is fitted a narrow tube open at both ends, the lower end of which is brought *exactly to the level of the lower surface of the cork*, to the other is adapted the caoutchouc tube connected with the apparatus (fig. 4) destined to produce the requisite difference in pressure: this apparatus will be described immediately. The flasks are placed in a metallic or porcelain vessel (fig. 3), in the conical contraction of which several strips of cloth are fastened. This method of supporting the flask has the advantage that, in one and the same vessel, flasks varying in size from 0·5 to 2·5 liters stand equally well, and that, by simply laying a cloth over the mouth of the vessel, the consequences of an explosion (which through inexperience or carelessness is possible) are rendered harmless.

It is impossible to employ any of the air-pumps at present in use to create the difference in pressure, since the filtrate not unfrequently contains chlorine, sulphurous acid, hydric sulphid, and other substances which would act injuriously upon the metallic portions of these instruments. I therefore employ a *water* air-pump constructed on the principle of Sprengel's mercury-pump, and which appears to me preferable to all other forms of air-pump for chemical purposes, since it effects a rarefaction to within 6 or 12 millimeters pressure of mercury.

Fig. 4 shows the arrangement of this pump. On opening the pinchcock *a*, water flows from the tube *l* into the enlarged glass vessel *b*, and thence down the leaden pipe *c*. This pipe has a diameter of about 8 millims., and extends downward to a depth of 30 or 40 feet, and ends in a sewer or other arrangement serving to convey the water away. The lower end of the tube *d* possesses a narrow opening; it is hermetically sealed into the wider tube *b*, and reaches nearly to the bottom of the latter. A manometer is attached to the upper continuation of this tube *d* by means of a side tube at d^1; at d^2 is attached a strong thick caoutchouc tube possessing an internal diameter of 5 millims. and an external diameter of 12 millims.; this leads to the flask which is to be rendered vacuous, and is connected with it by means of the short narrowed tube *k*. Between the air-pump and the flask is placed the small thick glass vessel *f*, in which, when one washes with hot water, the steam which may be carried over is condensed. All the caoutchouc joinings are made with very thick tubing, the internal diameter of which amounts to about 5 millims., the external diameter to about 17 millims. The entire arrangement is screwed down upon a board fastened to the wall, in such a manner that each separate piece of the apparatus is held by a single fastening only, in order to prevent the tubes being strained and broken by the possible warping of the board. On releasing the pinchcock *a*, water flows from the conduit *l* down the tube *c* to a depth of more than 30 feet, carrying with it the air which it sucks through the small opening of the tube *d* in the form of a continuous stream of bubbles. No advantage is gained by increasing the rapidity of the flow, since the friction exerted by the water upon the sides of the leaden pipe acts directly as a counter pressure, and a comparatively small increase in the rapidity of the flow is accompanied by a great increase in the amount of this friction. Accordingly at *g* is a second pinchcock, by which the stream can be once for all so regulated that, on completely opening the cock *a*, the friction, on account of the diminished rate of flow, is rendered sufficiently small to allow of the maximum degree of rarefaction. Such an apparatus, when properly regulated once for all by means of the cock *g*, exhausts in a comparatively short time the largest vessels to within a pressure of mercury equal to the tension of aqueous vapor at the temperature possessed by the stream.* The tension exerted by the water-stream in my laboratory, in which six of these pumps are used, amounts to about 7 millims. in winter and 10 millims. in summer. The filtration is made in the following manner: The flask standing in the metallic or porcelain vessel (fig. 3) is connected by means of the slightly drawn-out tube *k* with the caoutchouc tube *h* attached to the pump, the cock *a* having been previously opened and the properly fitted moistened filter filled with the liquid to be filtered. As usual, the clear supernatant fluid is first poured upon the filter; in a moment or two the filtrate runs through in a continuous stream, often so rapidly that one must hasten to keep up the supply of liquid, since it is advisable to maintain the filter as full as possible. After the precipitate has been entirely transferred, the filtrate passes through drop by drop, and the manometer not unfrequently now shows a pressure of an extra atmosphere. The filter may be filled (in fact this is to be recommended) with the precipitate to within a millimeter of its edge, since the precipitate, in consequence of the high pressure to which it is subjected, becomes squeezed into a thin layer broken up by innumerable fissures. As soon as the liquid has passed through and the first traces of this breaking up become evident, the precipitate will be found to have been so firmly pressed upon the

* The time required to obtain the above degree of exhaustion in a flask of from 1 to 3 liters capacity ranges from six to ten minutes; the quantity of water necessary amounts to about 40 or 50 liters.

paper, that on cautiously pouring water over it it remains completely undisturbed. The washing is effected by carefully pouring water down the side of the funnel to within a centimeter *above* the rim of the filter: the washing flask for this purpose is not applicable; the water must be poured from an open vessel. After the filter has in this manner been replenished four times with water and allowed to drain for a few minutes, it will be found to be already so far dried, in consequence of the high pressure to which it has been subjected, that without any further desiccation it may be withdrawn, together with the precipitate, from the funnel, and immediately ignited, with the precautions to be presently given, in the crucible.

If the porosity of a paper filter containing a precipitate were as unalterable as that of a pumice-stone filter, the experiments above described would show that the times required for filtration, according to the old method on the one hand, and the new one on the other, would be inversely proportional to the difference in pressure in each case; that is, by using the pump under the full pressure of about 740 millims., the time needed to wash a precipitate, occupying by the old process an hour, would at the utmost not amount to more than 30 seconds. In using these pumice filters (about which I will speak presently) to drain crystals from adhering mother liquors, or, say, to wash crystals of chromic acid by means of concentrated sulphuric acid and fuming nitric acid, the time occupied in the filtration is scarcely longer than that needed to pour a liquid slowly from one vessel to another. In filtering by means of paper, the precipitate gradually closes up the pores of the filter, and accordingly such an extraordinary acceleration as this can no longer be expected. But the following examples will show the saving of time and labor the method effects, even under all unfavorable conditions. For these experiments I have purposely chosen the hydrated chromium sesquioxyd, since it is one of the most difficult of precipitates to wash thoroughly. A solution of chromium chlorid was prepared by acting with fuming hydrochloric acid upon potassium dichromate; and by means of a measuring-vessel, which allowed the amount of chromium to be estimated to within 0·0001 grm., successive portions of the liquid were withdrawn, and the chromium oxyd contained in them precipitated with the usual precautions by ammonia. The volume of liquid, the quantity of ammonia employed, the time occupied in boiling and in permitting the precipitate to settle, the angle of inclination possessed by the funnel, and the size of the filter were the same in all the experiments. All the precipitates were washed with hot water, and, after burning the filter, ignited over the blowpipe for a few minutes; in weighing, the platinum crucible was tared by one of about equal weight, and the position of equilibrium of the beam determined by vibrations.

I first attempted to filter one of the precipitates in the ordinary way. $\frac{V}{v}$ amounted to 2; and consequently, from the table, 8·4 fresh additions of water were required in order to wash the precipitate to the $\frac{1}{10000}$ part. The times required were as follows:—

In transferring the precipitate from the beaker and allowing it to drain - - -	40′
For the first addition of water to run through,	48
" second " "	70
" third " "	80
Total length of time - - -	238

At this point the experiment was discontinued, as the filtrate became turbid. A second experiment failed from the same cause.

Accordingly I attempted to wash the precipitate by decantation. The volume of the precipitate amounted to about 30 cub. centims.; the quantity of water required to fill the beaker was seven times the volume of the precipitate; hence $\frac{V}{v}$ was 7, and the requisite number of decantations to reduce the amount of impurity to the $\frac{1}{50000}$ part was 5·2. The times observed were as follows:—

II.

For the first decantation to run through the filter	15′
" second " " "	12
" third " " "	18
" fourth " " "	15
" fifth " " "	18
In transferring the precipitate to the filter	30
Time required in washing	108
Weight of the precipitate	0·2458 grm.
Volume of wash water nV	1050 cub. centims.

III.

Experiment repeated. Number of decantations 7. Other circumstances the same as in the foregoing determination.

Time required in washing	140′
Weight of the precipitate	0·2452 grm.
Volume of wash-water	1200 cub. centims.

IV.

After ten decantations.

Time required in washing	180′
Weight of the precipitate	0·2443 grm.
Volume of wash-water	1750 cub. centims.

By filtration with the platinum cone and the pump the following results were obtained:—

V.

In transferring the precipitate to the filter (17 cub. centims. water)	6′
For the first addition of water (25 cub. cent.) to run through,	2
" second " " "	3
" third " " "	2
" fourth " " "	2
" fifth " " "	2
In draining the precipitate	2
Time required	19

Weight of precipitate	0·2435 grm.
Volume of wash-water	142 cub. centims.
Pressure of manometer	0·576 meter.

VI.

In transferring the precipitate and allowing the water (18 cub. centims.) to run through	8′
For the first addition of water (25 cub. cent.) to run through,	4
" second " " " "	5
" third " " " "	5
" fourth " " " "	5
In draining the precipitate	1
Time required	28

Weight of precipitate	0·2434 grm.
Volume of wash-water	118 cub. centims.
Pressure	0·600 meter.

VII.

In transferring the precipitate and allowing the water (20 cub. centims.) to run through	4′
For the first addition of water (25 cub. cent.) to run through,	3
" second " " " "	3
" third " " " "	3
In draining the precipitate	3
Time required	16

Weight of precipitate	0·2432 grm.
Volume of wash-water	95 cub. centims.
Pressure	0·584 meter.

VIII.

In transferring with 25 cub. centims. of water	8′
For the first addition of 25 cub. centims. to run through	5
For the second addition of 25 cub. centims. to run through	5
In draining the precipitate	3
Time required	21

Weight of precipitate ..... 0·2435 grm.
Volume of wash-water ..... 72 cub. centims.
Pressure ..... 0·593 meter.

IX.

In transferring with 15 cub. centims. of water and allowing it to run through	7′
For a single addition to run through	3
In draining the precipitate	2
Time required	12

Weight of precipitate ..... 0·2439 grm.
Volume of wash-water ..... 41 cub. centims.
Pressure ..... 0·572 meter.

X.

In transferring the precipitate with 13 cub. centims. of water,	5′
For a single addition of water (26 cub. cent.) to run through,	8
In draining the precipitate	1
Time required	14

Weight of precipitate ..... 0·2439 grm.
Volume of wash-water ..... 39 cub. centims.
Pressure ..... 0·530 meter.

In washing, by means of decantation, in the ordinary manner, the amounts of chromium sesquioxyd found were as follows:—

	grm.				
II.	0·2458,	after 5	decantations,	washed to the	$\frac{1}{5000}$ part.
III.	0·2452	" 7	"	"	$\frac{1}{200000}$ part.
IV.	0·2443	" 10	"	"	$\frac{1}{10000000}$ part.
	0·2451 mean.				

By the use of the pump:—

	grm.		
V.	0·2435,	after 5	additions of water.
VI.	0·2434	" 4	" "
VII.	0·2432	" 3	" "
VIII.	0·2435	" 2	" "
IX.	0·2439	" 1	addition of water.
X.	0·2439	" 1	" "
	0·2436 mean.		

Hence the probable amount of chromium sesquioxyd contained in the solution, according to the experiments with the pump, was 0·2436 grm.: according to the old method of decantation it was somewhat higher, namely 0·2451 grm. This excess of 1·5 milligram shows that the adhesion of the soluble matters to the precipitate and to the filter is, in consequence of the greater pressure, more easily overcome in the new method than in the customary process; it follows, therefore, that we can obtain a more complete washing by the new method than by the old. The old process of decantation required 108 minutes and 1050 cub. centims. of water to effect a washing to the $\frac{1}{50000}$ part; the new, on the contrary, only 12 to 14 minutes, and not more than 39 to 41 cub. centims. of wash-water. If a precipitate be heated in a platinum crucible immediately after filtration by the older process, a portion will inevitably be projected out of the crucible. Hitherto, therefore, it has been necessary to dry the filter and precipitate before ignition. Now to dry a quantity of hydrated chromium sesquioxyd containing 0·2436 grm. Cr^2O^3 in a water-bath at 100° C. requires at least five hours; and, moreover, bringing the dried precipitate into the crucible, burning the filter, and gradually igniting the mass is in the highest degree tedious and troublesome. All this expenditure of time and labor may be saved by employing the new method. By its means a precipitate is as completely dried upon the filter in from 1 to 5 minutes as if it had been exposed from 5 to 8 hours in a drying-chamber; and it can immediately, filter and all, be thrown into a platinum or porcelain crucible and ignited without the slightest fear of its spurting. By operating in the following manner the filter burns quietly without flame or smoke; this phenomenon, although remarkable, easily admits of an explanation. The portion of filter-paper free from precipitate is tightly wrapped round the remainder of the filter in such a manner that the precipitate is enveloped in from four to six folds of clean paper. The whole is then dropped into the platinum or porcelain crucible lying obliquely upon a triangle over the lamp, and pushed down against its sides with the finger. The cover is then supported against the mouth of the crucible in the ordinary way, and the ignition commenced by heating the portion of the crucible in contact with the cover. When the flame has the proper size and position, the filter carbonizes quietly without any appearance of flame or considerable amount of smoke. When the carbonization proceeds too slowly, the flame is moved a little toward the bottom of the crucible. After some time the precipitate appears to be surrounded only by an extremely thin envelop of carbon, possessing exactly the form (of course diminished in size) of the original filter; the flame is then increased, and the crucible maintained at a bright-red heat until the carbon contained in this envelop is consumed. The combustion proceeds so quietly that the resulting ash surrounding the precipitate possesses, even to the smallest fold, the exact form of the original filter. If the ash shows here and there a dark color, it is simply necessary to heat the crucible over the blowpipe for a few minutes to effect the complete removal of the trace of carbon. This method of burning a filter is extremely convenient and accurate; it is only necessary to give a little attention at first to the slow carbonization of the paper, after which the further progress of the operation may be left to itself.

Gelatinous, finely divided, granular, and crystalline precipitates, such as alumina, calcium oxalate, barium sulphate, silica, magnesium ammonium phosphate, &c., may with equal facility be treated in this manner; so that even in this particular the work, in comparison with the method generally adopted, is considerably shortened and simplified.

From the above experiments it appears that the time necessary to filter and dry a quantity of chromium sesquioxyd, hitherto requiring about 7 hours, is reduced by the new method to 13 minutes. This saving of time is, moreover, proportionately greater in the case of precipitates more easily filtered than hydrated chromium sesquioxyd. Particularly is this so in separating a finely suspended precipitate from a large volume of water. Under these circumstances the clear fluid runs through the filter in a continuous stream, so rapidly that it is scarcely possible to maintain the supply; the entire operation, in fact, requires scarcely more time than that necessary to pour a liquid from one vessel to another. Filtration, therefore, may be effected as quickly through the smallest as through the largest filter. Moreover the exceedingly small amount of water required to wash a precipitate completely renders unnecessary the tedious evaporations which by the older method are almost inevitable when the filtrate is needed for a further separation. Thus the introduction of impurities from the action of the liquid upon the dish in the course of evaporation is prevented; and also the loss due to the slight solubility of the greater number of precipitates in the wash-water is reduced to a minimum. Supposing we had to analyze an alkaline chromate in which the quantity of chromic acid is equivalent to 0·2436 grm. chromic sesquioxyd, as in the above described experiments, then to determine the proportion of alkali we should, by using the older method, require the preliminary evaporation of about 1050 cub. centims. of liquid; by the new method the evaporation of 40 cub. centims. only is necessary. Now by employing the best form of water-bath, i. e., one possessing a constant water-level, such as is used in my laboratory, it is possible, under favorable circumstances, to evaporate in a porcelain dish 1 cub. centim. of water in 27 seconds. Consequently the evaporation of the filtrate obtained by the older method would occupy about eight hours, whilst by the new 18 minutes only are required. The total length of time needed to filter the chromium sesquioxyd, wash and dry the precipitate, and evaporate the filtrate is reduced, therefore, from 14 or 15 hours to about 32 minutes.

The experience I have subsequently gained in my laboratory, where the method has been in general use for the last nine months, fully confirms the above results. It has shown that, on the average, three or four analyses can now be made in the time formerly demanded by a single one.

Another and an inestimable advantage springs from the peculiar condition of a precipitate filtered by this method. It not unfrequently happens, even in the hands of experienced manipulators, in consequence of the agitation it is necessary to give to the contents of the filter to effect their complete washing, that the surface of the filter becomes injured and torn, so that the precipitate becomes mixed with filaments of paper; this is particularly the case in using hot water. Supposing the precipitate to consist of mixed hydrates of the sesquioxyds (for example, iron and alumina), it will be found, on redissolving in an acid, that the filaments, like tartaric acid, prevent the complete separation of these substances by subsequent precipitation; thus the alumina will contain iron, and on precipitation by means of ammonium sulphid will be colored black. On the other hand, by employing the new method the precipitate coheres so firmly that the introduction of this source of error is impossible, even by using common gray filter-paper. The most gelatinous precipitates, as hydrated ferric oxyd, alumina, &c., adhere to the filter in a thin coherent layer, and may be removed, piece after piece, so completely that the paper remains perfectly clean and white. The advantage thus gained where it is necessary to transfer mixed precipitates to another vessel in order to effect their subsequent separation is evident.

The filter-pump, moreover, is exceedingly serviceable in separating precipitates or crystals from syrupy mother-liquors. Thus honey-sugar may be so completely separated from the thick viscid liquid in which it forms by a filter of coarse gray paper, that it remains only slightly colored, and by a single crystallization from alcohol may be obtained in small white shining needles. And since the bulk of the moist precipitates, particularly that of the more gelatinous, is so much diminished under the high pressure, the precipitate only occupying one-third to one-sixth of its bulk under ordinary circumstances, a filter of one-third to one-sixth of the size usually employed may be taken, and thus the amount of ash proportionately essened.

As the water air-pump suffers no injury from the presence of corrosive vapors or gases, we can equally well employ it to filter liquids containing nitrous acid, sulphurous acid, fuming nitric acid, chlorine, bromine, volatile chlorids, &c. In such cases I use a peculiar filtering arrangement, consisting of a cylindrical glass vessel, the lower end of which is drawn out before the blowpipe to the form shown in fig. 5; in this drawn-out portion a thin plate, 1 or 2 millims. in thickness, of *artificial* pumice, such as is used by polishers, is packed water-tight by means of asbestos. This apparatus is arranged for the purpose required exactly as the funnel in the method of filtration by pressure above described. In order to have a number of these filters in readiness, a pumice-stone cylinder of the required diameter is turned in a lathe, and then the thin plates sawn off by means of a small hand-saw in the small wooden support shown in fig. 6. The upper surfaces of the plates may afterwards be rendered perfectly even by a coarse file.

By the aid of these pumice-stone filters many chemical products may be made, the preparation of which has hitherto been almost impossible. For the sake of example I take the preparation of pure dry chromic anhydrid; in an hour it is easily possible to filter, wash, and dry crystals of this substance an inch in length. A solution of two parts of potassium dichromate in 20 parts of water mixed with ten parts concentrated sulphuric acid, deposits, after standing about 24 hours, numerous brilliant needles of chromic anhydrid. These may be drained from adhering mother-liquor upon the pumice filter by means of the pump, and in a few minutes completely washed by a small quantity of fuming nitric acid free from nitrous acid. A covering of tolerably strong sheet copper provided with two arms, as shown in fig. 5, is then placed around the tube; by hanging lamps upon the arms the tube may be readily heated to about 60° or 80° C.; and by connecting a chlorid-of-calcium tube with the upper end of the glass vessel, a current of dry air may be drawn through the apparatus by means of the pump, and thus in a comparatively short time large and brilliant crystals of chromic anhydrid, perfectly dry and free from all impurity, may be easily obtained.

A single pump of the above description costs, including the leaden piping, about 8 thalers (24 shillings); and experience has shown that five or six are amply sufficient for a laboratory of fifty or sixty students. The apparatus, as may readily be seen, can be applied in the operation of evaporating *in vacuo;* if, however, circumstances will not permit of its being adapted to this purpose, then a fall of 10 or 15 feet is sufficient to filter a precipitate according to the above described method, and so far to dry it that it can be immediately ignited in the crucible. It is therefore not absolutely necessary to employ an air-pump in this process of filtration; any apparatus producing a difference of pressure amounting to a quarter of an atmosphere is sufficient. The simple arrangement represented in fig. 7 is very useful, and is frequently employed in my laboratory. It consists of two equal-sized bottles, *a* and *a'*, of from 2 to 4 liters capacity, each of which is provided near the bottom with a small stopcock designed to regulate the flow of water. Suppose *a* filled with water and placed upon a shelf as high above the ground as possible and *a'* placed empty on the floor, and the two stopcocks connected by means of caoutchouc tubing *c*, then on allowing water to flow down the tube the air in the upper bottle becomes somewhat rarefied; and in order to employ the consequent difference in pressure (amounting to a column of mercury about 0·2 meter in height) for the purpose of filtration, it is only necessary to connect the mouth of the upper bottle with the tube of the filter-flask. When the water has ceased to flow, the position of the bottle is reversed, when the operation recommences. So small a pressure as 0·2 meter suffices to render the filter and its contents so far dry that they may be immediately withd awn from the funnel and ignited without any other preliminary desiccation. The following experiment, made with a portion of the same solution of chromium used in the former determinations, will serve to show the saving of time effected by this simple arrangement:—

XI.

Transferring the precipitate with 14 cub. centims. of water	14'
For a single addition of 26 cub. centims. of wash-water to run through	7
To drain the precipitate	4
Time required in washing	25
Weight of the precipitate	0·2435 grm.
Volume of wash-water	40 cub. centims.
Pressure in manometer	0·184 meter.

This amount of chromium sesquioxyd (0·2435 grm.) differs from the mean of the former experiments (0·2436 grm.) by one-tenth of a milligram only, and shows that even by a pressure of 0·184 meter the washing is as complete by the single addition of 26 cub. centims. of water. The duration of the filtering process in the former experiments ranged from 12 to 14 minutes under a difference of pressure amounting to from 0·53 to 0·572 meter; in the last experiment it required 25 minutes under a pressure of 0·184 meter, or about double the length of time. The time needed to analyze potassium chromate in the former case was reduced from 14 hours to 32 minutes; by the latter method the reduction would be from 14 hours to 44 minutes.

The employment of the second method is particularly to be recommended to beginners in qualitative analysis. The experimenter needs only a single funnel, he is obliged to work carefully and cleanly, and the great saving of time and work amply compensates for the little trouble needed to reverse from time to time the position of the bottles.

I believe that the above-described water air-pump will soon become an indispensable piece of apparatus in chemical laboratories. It not only serves as the most convenient method of producing the differences in pressure required to accelerate the process of filtration, and of obtaining the necessary vacuum for evaporation; it is equally adapted to purposes to which neither the mercury nor the ordinary pumps are in any way applicable. By its aid it is possible to calibrate a thermometer with the greatest accuracy, and to estimate the vapor-tension of such corrosive bodies as bromine, chromyl dichlorid, &c., by the simplest method possible, in which the necessary operations require scarcely more time than an ordinary determination of a boiling-point.

I purpose returning to these applications of the instrument in a future communication.

Art. XXIX.—*Note upon the origin of the Phosphatic Formation*; by C. U. Shepard, Sr.

Concerning the origin of this extensive formation, several explanations suggest themselves. Among these the best answering the purpose at present is perhaps the supposition, that the great Carolina Eocene bed of shell-marl on which it rests, formerly, and for a long period, protruded many feet above the present sea-level, giving rise to a luxuriant soil (analogous to that now existing over portions of some of the guano islands and shores) and which was then depressed beneath the sea, where it under-went those changes that have resulted in the present formation. For the superabundance of the phosphate of lime in the soil supposed, we would point to the deposition of bird-guano as it is now going on upon the Musquito coast of the Caribbean sea. A layer of such soil, clothed with an abundant vegetation of from three to five feet in thickness, and nearly submerged, and afterwards becoming more or less covered with sand, in the absence of strong tidal action and oceanic currents (and these may have been precluded by shoals and even land in a seaward direction) would give rise to enormous quantities of carbonic acid, whereby the carbonate of lime of the soil and of portions of the upper layer of the marl itself would be withdrawn, and thus permit the segregation of the phosphate of lime into nodules or even into an imperfect stratum. The alumina and oxyd of iron would be precipitated upon, and among the phosphate everywhere as we find them, mingled also with considerable quantities of siliceous matter. The sulphate of lime which occurs intimately distributed through the phosphatic masses, may be supposed to have originated by double decomposition through the meeting of sulphate of soda and chlorid of calcium. The carbonate of lime present in the nodular phosphate, would be looked for, as the residuum of that belonging to the soil and the marl, which the free carbonic acid was inadequate to dissolve. Meanwhile the deposit would be enriched from the precipitation out of the supernatant waters, of the osseous remains of fishes and other marine animals; and to some extent also by those from the land, which the rivers might bring into the estuary.

The nodular phosphates from some portions of the formation strikingly resemble in their color, hardness and specific gravity, the stone-guano (*pyroclasite*) of Mong's island in the Caribbean sea; and like it, decrepitate violently when heated. The Caribbean mineral is known to originate in sea birds, as the process of its formation is still in progress at many places on the Musquito coast. It is not unreasonable therefore, to ascribe the origin of our phosphatic formation partly to the same cause, at least under the modifications above pointed out.

A reason why the nodular layer is interrupted in its lateral extent may be, that the original surface, prior to its submersion, was not everywhere sufficiently above the sea-level to give rise to a soil abounding in organic matter. There probably existed lagoons, whose bottoms were free from the phosphatic deposits, which, in some instances, may correspond to the places where these are wanting, as well as to the ferruginous sand-layer, at Port Royal on St. Helena, Pinckney Island, and other localities, where the stratum of the nodular phosphate appears to find its equivalent in this latter formation.

It is certainly a striking fact, that no remains of plant or animal, then in process of growth, have been detected within the nodular layer. The nodules merely contain occasional casts and imprints of the fossils belonging to the Eocene bed on which it rests. The condition of the waters, either as to what they held in solution or from their temperature, appear to have been incompatible with organic life. Even the remains of fishes and sea-tortoises, whose detached bones are so frequent, would seem to have belonged to individuals accidentaly caught within the land-locked area when the changes of level along the coast occurred.

In some instances, where the nodules are small (from two to six inches in diameter), they have evidently been subjected to slight attrition, and assume the appearance of water-worn pebbles, reminding the observer strikingly of diluvial drift. A nearer inspection, however, soon satisfies him that they have merely been subjected to a partial movement, for a slight distance backward and forward, by wave action, without breaking up the general continuity of the layer, or much influencing its thickness in any one place. Nor is this appearance maintained over more than a few acres, when the usually tuberose and deeply pitted surfaces of the masses reoccur. At St. Helena, on the beach, opposite Port Royal city, at half tide level, there occurs a highly interesting patch of fluvio-litoral marl, two or three feet in thickness, containing 45 per cent carbonate of lime, and literally filled with the blanched but perfectly preserved shells of the following genera:—*Planorbis*, *Physa*, *Auricula*, *Nassa*, with a few *Columellas*. This formation appears to be more recent than that of the phosphatic nodules, which however is here wholly wanting, unless the outcrop of the ferruginous sand, occurring at a little distance to the south of the marl, at or near low water level, be regarded as the equivalent of the nodules. At any rate, it is clear that the marl has not been subjected to the corrosive or solvent agency so apparent upon the superior layer of the Eocene. The freshwater marl above mentioned has its interest hightened by containing the gigantic skeleton of a mastodon, brought to light about a year ago, by Capt. Charles Boutelle, of the Coast Survey, and by him generously contributed to my geological museum in Amherst College. A further account of its discovery may soon be looked for in this Journal.

Art. XXX.—*A Point in the Geology of Western Vermont;* by the Rev. J. B. Perry.

Proposing to touch briefly, in a series of short articles, on a few prominent features in the geological structure of Western Vermont, as my first point I send for insertion in the Journal of Science, an extract from one of a course of University Lectures, which I am now delivering in the Museum of Comparative Zoology, at Harvard College, on the Geology of the Basin of Lake Champlain.

There is a series of sedimentary rocks in Western Vermont, which were many years ago described by Dr. Emmons, and by him termed Taconic. They lie, for the most part, on the east of easily-recognized Lower Silurian formations lining the shore of the Lake, in the southern part of the Champlain Basin, and themselves constitute its border in the more northerly portions of the State. The beds composing the eastern limits of this range of rocks may be found situated, to a large extent, along the base of the Green Mountains. The width of the entire range is from eight or ten to some fifteen or twenty miles, it varying somewhat in different parts of the Basin. These rocks seem to constitute several distinct groups or divisions. The series, which is apparently the oldest, lies on the east, and consists, first, of Talcose or Talcoid Slates, accompanied by Quartzites and Conglomerates; next, of calcareous beds known as Stockbridge Limestone; and, finally, of Talcose or Talcoid Slates. The second series comprises at least two main sections. Starting from the west we have, first, the Black Slates, or the Swanton Group, which includes thin beds of sandstone and limestone. Proceeding eastward, in apparently ascending order, we encounter the Brown Slates, or the Georgia Group, which also contains thin beds of interstratified sandstone and limestone, and is capped on the east by a conglomerate. Along the western portion of this series there is a band of Red Sandstone running northerly and southerly which has come during the past few years to be regarded with certainty as of Potsdam age.

Now, these formations, according to a view which has been very widely prevalent, are made up of Lower Silurian rocks, which have been largely affected by heat, their structure having been more or less effaced, and their organic remains either obliterated, or, to a considerable extent, obscured. We are, accordingly, invited to look at the formations in question, for a few moments, that we may see how far this theory is sustained by facts.

It may be remarked, in a preliminary way, that traces of metamorphic action are by no means so often met with in these rocks, as the hypothesis implies. That some portions of these beds have undergone changes, under the agency of heat both dry and moist, in connection with pressure, galvanism, and the like, is undoubtedly the fact. But these instances of metamorphism are local, and by no means such as to obscure the character of the whole series of rocks, or utterly destroy the fossil remains.

With this preliminary remark we may proceed to examine the thickness of these formations.

Doing this, we shall be able to see how they correspond, in this respect, with the Champlain or Lower Silurian Series. Beginning with the inferior members of the Taconic we find the Talcoid Slates with Quartzites and Conglomerates to be, according to President Hitchcock, 1,200 feet in thickness. It should be added that the pebbles which go to make up the Conglomerate consist of Gneiss, of Schist, and fragments of other rocks, which, perhaps, belong to a pre-sedimentary age. The thickness of the Stockbridge Limestone may be well estimated in Danby Mountain, more recently known as Mount Eolus. These calcareous beds, varying little in that locality from a horizontal position, are overlaid by Lower Taconic Slate, and so situated as to preclude all probability of repetition. The thickness of this single mass of limestone is between 1,900 and 2,000 feet. The overlying Talcoid Slate may be better seen in a single mass, and its amount more correctly estimated, in Berkshire county, Massachusetts, than in Vermont. Graylock is made up of immense beds of this slate, overlying the Stockbridge Limestone, and having the successive layers only slightly inclined. Their entire thickness is 2,000 feet. We thus have more than 5,000 feet of rocks in the Lower Taconic Series.

Advancing to the next main division, we find the Black Slates with interstratified beds of Sandstone and Limestone exhibited in Swanton in great force. They dip from 30° to 90° toward the east, show no indications of any repetitions in the beds, and as thus lying, their upturned edges occupy about five miles in an east and west direction. I fail to see how they can be regarded as less than 10,000 to 15,000 feet in thickness. Frequent opportunities to study this series of rocks in various different localities have confirmed my convictions that this estimate is none too large. The Georgia Group, in other words, the whole series of Brown Slates with included Sandstones and Limestones, evidently succeeding the Swanton Group, and as well extending to the Lower Taconic, as overlying its western limits, has been estimated by Dr. Hitchcock, and others as having a thickness of not less than 10,000 feet. We accordingly have for the thickness of the second great division of the Primordial rocks from 20,000 to 25,000 feet, and for that of the two great divisions thus far considered from 25,000 to 30,000 feet.

Leaving the Potsdam Sandstone out of view for the present, we may notice the thickness of the succeeding beds in their typical localities. That of the Calciferous Sandrock is about 300 feet; that of the Chazy Limestone is nearly the same; while that of the Birdseye is only 30 feet, and that of the La Mott, or Black River, 15 feet. The Trenton Limestone is some 400 feet thick, the Utica Slate about 75 feet, while the Lorrain Shales may reach 700 feet.* This estimate gives us less than 2,000 feet as the aggregate thickness of all these rocks.

We have now only to place the immense force of the Taconic rocks in parallelism with the thickness of the Lower Silurian formations, to see at once the striking disparity. Would we also take them up, bed by bed, and look at them in contrast, we should discover how utterly impossible it is to bring them into coördination. But time fails me now to enter into the details of any such comparison.

We may next notice the relation of the Potsdam Sandstone of Vermont to the formations under consideration.

A band of Red Sandstone runs northerly and southerly through most of Western Vermont, and may be seen in places resting on the Black or Swanton Slates. Portions of this rock were long ago described by Dr. Emmons as Potsdam Sandstone. Most geologists, however, regarded it as of Medina age, and the underlying argillaceous beds as Utica Slate and Lorrain Shale. As it is now clearly made out, by evidence which need not be here repeated, that the sandstone in question belongs to the Potsdam period, the view entertained by many in regard to the subjacent slate falls to the ground. That there may be no doubt on this matter, and as some still persist in asserting that the Black Slates are Hudson river, the Potsdam beds having been shoved, or folded over upon them, I will refer to some evidence having a bearing on the subject, and calculated,

* These figures, as should be evident, are to be regarded only as a rough approximation, which may serve as a basis of comparison. I am, of course, well aware that a greater thickness has been ascribed to some of these beds, especially during the past few years. For instance, the Utica and Lorrain formations are represented as existing in immense force in Western Vermont. But this is done, by counting beds in the Hudson River formations, which really belong as I shall endeavor to indicate, to another age.

as I think when it is understood, to put the question to rest forever.

There are several localities at which the Potsdam Sandstone may be observed resting on the Black Slate very nearly as it was originally deposited. No single locality gives all the evidence; each that I propose to mention furnishes much that is important; while a great many outcrops need to be examined, in order that the relations of the rocks may be seen in all their bearings. I will now cite in particular the southwest shore of Shelburne Bay; also, Lone Rock Point in Burlington, and the northwest side of Snake Mountain, in Addison. In some places at these localities the upper part of the slate shows that denudation took place before the deposition of the overlying sandstone, and thus evinces a want of conformity in the order of succession. Again, there are many instances serving to prove that the slate was unconsolidated at the time the Potsdam beds were laid down, and that they (and not strata of some other period) were actually deposited upon the slate, which now lies beneath them; for the upper layers of the slate contain imbedded in themselves numerous fragments of the overlying Potsdam. Moreover, the lower portion of the arenaceous deposit is exactly fitted into all the surface cavities and depressions of the slate; so exactly fitted as to conform with them entirely, as putty conforms with the irregularities of the object upon which it is thrown down; a thing which could not have occurred had not the sandstone been laid down upon the slates very nearly as we now find them. It thus appears, in the clearest manner, that the two formations occupy to-day substantially their original position, and exhibit in the main their true relations to each other. And this leads me to remark once more, that there are places in which no break occurs between the two formations, showing that there has been no movement in such instances of the Potsdam upon the underlying slate. In other places, however, owing to the unequal pressure exerted at different points, and thus to the inequalities in the uplifting of the formations, one part being more elevated than another, there is a rupture. In some cases this may be seen a few feet below the summit of the slate, while the upper part of the slate still cleaves to the overlying arenaceous bed. There are other instances in which the break runs above the base of the sandstone, its lower portion, meanwhile continuing to adhere to the subjacent slate. In yet other localities, the rocks are slightly separated according to their natural limits. There are additional points of view, at which "slickensides" may be observed; but there are almost always associated with them correspondencies in the two rocks, showing that the lateral motion was one of small extent. From these facts, to mention no others, it is evident—(1) That the two series of beds are unconformable; (2) That the sandstone was deposited, nearly as it is now found lying, upon the slate; (3) That, in some places, there has been no horizontal movement of the rocks upon each other; and (4) That there has been, in other localities, a slight, and only a slight, sliding of the upper mass upon the lower. We thus see that the underlying black, argillaceous beds cannot, by any possibility, be Utica Slate, or Lorrain Shales.*

But let us look at another phase of the matter. Some have supposed that the sandstone, while it overlies the Swanton Slate, runs under the beds of the Georgia Group, which, in many places, occur in uplifts lying to the east of the Potsdam band. Instead of this being the fact, there are localities in Swanton, Shelburne, and other townships, which render it clear that the Potsdam formation, in some places, overlies the Brown Slates, and in others rests against their upturned edges. Meanwhile, years of search have failed to reveal a single instance of its underlying, or running beneath, the beds of the Georgia Group.

This, however, is not all the evidence. An outstanding bed of what appears to be Potsdam Sandstone occurs as an overlying mass near Franklin Centre, and not far from the eastern limits of the Brown Slate. But a still better view of these rocks, and one respecting which there seems to be no question, may be secured in the counties of Chittenden and Addison. I refer to the fact that the Potsdam Sandstone extends in these counties broadly to the east. In order to observe this extensive exhibition of the rock, one may start from a point in Charlotte, on the west of the Red Sandstone, and where it overlies the Black Slates, and thence proceed eastward or southeastward. Advancing in either of these directions for six, eight, or ten miles, he may see the sandstone from point to point, capping the uplifts, and showing itself as an overlying rock, while he may also, from time to time, observe outcroppings of the older formations until he reaches the Lower Taconic in Monkton and Starksborough. He may thus find the Potsdam strata resting upon portions of the Swanton Slates on the west, extending across the entire limits of the Georgia Group, and also overlying a portion of the Lower Taconic beds. He may, accordingly, observe reposing unconformably beneath the Potsdam Sandstone, a series of forma-

* That portions of the Utica and Lorrain formations may occur in the same neighborhood with the Potsdam Sandstone, is, of course, possible, and most freely admitted.

tions which, in some localities, are from 25,000 to 30,000 feet in thickness.

Thus, we have brought before us certain facts and relations pertaining to the Primordial formations, which each person may observe for himself. In the light of these facts and relations, we are enabled to see the utter untenableness of the position that these beds are Lower Silurian, in the proper sense of that designation. According to the view suggested by the considerations presented, it may seem strange that any mistake should have been made in regard to these rocks. If, however, we look at the matter in another light, and remember that our geologists regarded the Red Sandstone as of Medina age, we can discover some excuse for the error; for, according to that view, they would not be surprised to find the arenaceous formation, now known as Potsdam Sandstone, crowning the subjacent masses of slate.

We may next inquire briefly in regard to the fossil remains furnished by these rocks.

On this branch of the subject I can only speak in the most general terms, reserving for another occasion the detailed account of the organic forms which, from time to time, have been brought to light. It is still doubtful whether any fossils can be referred to the Lower Taconic beds of Vermont. The *Paleotrochis major*, and *P. minor*, found in great abundance by Dr. Emmons in the Quartzite of North Carolina, are not known to occur on any other horizon.* Passing to the next great division of the Taconic we find in the Swanton Group organic remains peculiar to itself. Of these the *Atops punctatus* [trilineatus] of Emmons is the best known. In the Georgia Slates there is a much larger variety of genera and species. Among these may be mentioned species of *Obolella*, *Orthis*, *Orthisina*, and *Camerella;* also, *Olenellus Thompsoni*, and *O. Vermontana*, and *Conocephalites Teucer*. No one of these occurs, so far as is known, in any more recent rocks. When we come to the Potsdam Sandstone we meet with still other species. Among the various forms found in this rock I will simply cite the *Conocephalites Adamsi*, *C. Vulcanus*, and *C. minutus*. Now, the several species belonging to this period are distinct from all those that have been discovered in the earlier rocks, and they probably died out before the beginning of the next era. But while the several forms of *Conocephalites* peculiar to the Potsdam Group are different from, they are yet closely allied to, the species of *Conocephalites* found in the

* [These supposed corals have recently been shown by Prof. O. C. Marsh to be inorganic. This Journal, vol. xlv, p. 217.—Eds.]

Georgia Slate, thus indicating just what stratigraphy seems to suggest, viz: the true order of succession in the beds. At the same time it should be added, that while the species in these successive groups are different, they still have a common Primordial type; a type which distinguishes them from the organic forms of the Calciferous Sandrock, and of the several succeeding formations of the Lower Silurian, and thus shows that the Potsdam Sandstone is strictly Primordial, and may be properly reckoned as Upper Taconic.

In order to avoid all occasion for misapprehension, a supplementary remark should be made. It is sometimes urged as an objection, and the fact has been well known to the writer for years, that beds of Lower Silurian rocks are found within the generally admitted Taconic limits. These beds occupy troughs and depressions in the older formations, as would be natural, and occasionally crown uplifts which have survived the abrading force of ages. On these overlying strata I propose to dwell more at length when I come to speak of the formations of the Champlain Basin under other relations. I now simply remark that their occurrence, under existing circumstances, no more disproves the existence of the underlying Primordial series of rocks than would the presence of Carboniferous strata in a given district, militate against the fact of subjacent Silurian, or Devonian, formations on which they rest as overlying beds.

We thus are enabled to see, on the threefold ground of thickness, of stratigraphical relations, and of organic remains, to mention no other considerations, that the Taconic, or Primordial, series of rocks cannot be properly regarded as Lower Silurian in the legitimate sense of that term. The facts presented clearly show that the Swanton Group on which the Potsdam was in places laid down, and on which it still rests, cannot be Utica Slate or Lorrain Shales. If stratigraphy be of any account, it proves this in the most unmistakable manner. And, on this relationship, as I may venture to say, the whole matter, at least from one point of view, virtually turns. If the Potsdam Sandstone rest unconformably, as it really does, on such a series of older beds, and if there have not been any inversion or overlapping of the Potsdam in the localities referred to, and there is the best evidence that there has not, then it clearly and certainly follows that these inferior rocks are not Lower Silurian; that they are more ancient than the overlying discordant Potsdam; and that there is a series of formations of vast extent which may be known as Taconic or Primordial, and of which the Potsdam Sandstone legitimately constitutes the crowning group.

The view now presented reminds me of certain statements made by Dr. T. Sterry Hunt, in a paper read last August at the meeting of the American Association for the Advancement of Science in Chicago, and printed in the September number of this Journal. In that paper he asserts that the beds which I have designated as the Black or Swanton Slates are Hudson River formations, by which I suppose he means Utica Slate and Lorrain Shales. As he advances no argument in support of his declaration, no argument is needed in reply. So far as his assertion is concerned, I would simply wish it to be looked at in the light of the facts which have been presented. On the supposition that the slate referred to is Utica or Lorrain, we have the strange anomaly of a formation, at once belonging to the summit of the Lower Silurian, and yet originally and still overlaid by Potsdam Sandstone.

While touching this matter I ought perhaps to add, that with the *Triarthrus Becki* of Green, Dr. Hunt has confounded the *Atops punctatus* (trilineatus), a species which Dr. Emmons long ago described as having marks distinguishing it from the *Triarthrus Becki;* which a committee of the American Association for the Advancement of Science pronounced to be different; and which no less a paleontologist than M. Joachim Barrande evidently regards as distinct from the Utica species.

Again, Dr. Hunt says that the *Triarthrus Becki* has been found in the underlying Hudson River Slates at Sharpshins (Lone Rock Point), and Appletree Point, near Burlington. Being very familiar with these rocks, it may not be impertinence in me to suggest, that Dr. Hunt undoubtedly refers, in part, to fragments of organic remains long ago met with at Lone Rock Point, the specific affinities of which, however, were never accurately determined ; and, in part, to a specimen of Trilobite discovered at Appletree Point by Sir William Logan, but which was, unfortunately, as I happen to know, too indistinct for exact specific identification. At the latter point I have found, at different times, several specimens of Trilobites, probably of the same species ; also, other fossils both at the same place, and at Lone Rock Point, of which I have something more to say in the sequel.

Once more, Dr. Hunt declares that, "the *Conocephalites Teucer*," which I have cited as characteristic of the Georgia Group, "is, according to Mr. Billings, common to the [Georgia] Slates, and to the associated Red Sandstone." The implications which this assertion involves rest, so far as they have truth, on a series of misapprehensions. In the first place, the Red (or Potsdam) Sandstone is not "associated," in the sense in which the term is here used, with the Georgia Slates. In the second place, the fossil just mentioned never occurs, so far as is known, in the Potsdam Sandstone. But, in the third place, while it is not met with in the *Red* Sandstone, it is found in a *sandstone*, in a slate, and, I may add, in a limestone—in each of these several beds; in other words, it belongs to the Georgia Group, which consists of brown slate, with interstratified masses of gray limestone and brown sandstone. But, if such be the case, how could any mistake arise? First, the rocks are very complicated, and cannot be mastered without much study and long continued observation. Next, nothing is easier than misunderstanding. In speaking to Mr. Billings of the Georgia Group as containing *Conocephalites Teucer*, *Olenellus*, etc., I said, among other things: These slates are interstratified with sandstone. From this he inferred, as was afterward evident from one of his articles, that I meant the Potsdam Sandstone, whereas I referred to the brown sandstone of the Georgia series. Receiving this wrong impression unconsciously, and desiring to give full credit to others, he cited me in connection with Dr. G. M. Hall as authority in the matter—as having made out, by personal inspection, the actual interstratification of the Georgia Slates and the Potsdam Sandstone—as may be seen by turning to this Journal for January, 1862, (vol. xxxii, p. 100.) In this he did what was very natural under the circumstances, since the Red Sandstone occurs in close proximity to the Trilobite locality; insomuch, also, as he was mainly intent on the fossils; while Dr. Hall and I lived in the neighborhood, and were very familiar with the "lay" of the rocks. Accordingly, from what was perhaps an inadequate statement on my part, from an accidental misunderstanding on the part of Mr. Billings, and, as I may presume, from an innocent misapprehension on the part of Dr. Hunt, the true relations of these rocks have been misconceived. Thus, as I may add, the cited proof of the specific identity of the Georgia Group and of the Potsdam Sandstone falls to the ground.

Such is a brief exposition of a single point in the Geology of Western Vermont. In due time other points may be expected.

Art. XXXI.—*On some derivatives of Trichlormethyl-sulphon-chlorid*, $(C\,Cl_3)\,SO_2Cl$; by O. Loew, Assistant in the Chemical Department of the College of the City of New York.

This chlorid, discovered by Berzelius and Marcet in 1812, was the subject of extended investigations by Kolbe. Since that time no investigations on this compound have been made, although it possesses many interesting properties. I find that the simplest way to prepare it, is the following: 300 grms. bichromate of potassium in pieces of the size of a pea, 500 grm. common hydrochloric acid, 200 grm. nitric acid of the common strength, and 30 grm. bisulphid of carbon are mixed in a flask, filling it to one-fourth of its capacity and loosely stoppered. It is kept cool in the beginning and shaken from time to time. In about eight days the process is finished. In direct sunlight only four days are required. By addition of water the chlorid and nitrate of potassium are dissolved, and there remains the insoluble trichlormethyl-sulphon-chlorid. It is filtered off, washed, and pressed between sheets of filter-paper.

When this chlorid is dissolved in absolute alcohol and treated with sulphuretted hydrogen, sulphur is deposited, and hydrochloric acid and another compound of an acid character remain in solution. By neutralization with dry carbonate of sodium, the sodium salt of this new acid remains dissolved in the alcohol, and crystallizes by evaporation in shining scales.

0·469 grm. yielded 0·146 grm. sulphate of sodium =11·33 per cent Na.

0·236 grm. yielded 0·490 grm. AgCl =51·37 per cent Cl.

0·511 grm. yielded 0·605 grm. sulphate of barium =16·24 per cent S.

The sodium salt of the trichlormethyl-sulphurous acid requires: 11·14 per cent Na; 51·95 per cent Cl; 15·56 per cent S; 15·56 per cent O; 5·81 per cent C.

To obtain the free trichlormethyl-sulphurous acid, the sodium salt is decomposed by dilute hydrochloric acid and agitated with ether. The ether yields by evaporation this new acid in radiating needles. Both the acid and its salts are not of great stability; at a moderate temperature, especially when water is present, decomposition takes place and a very offensive odor is evolved.

The formation of this acid takes place according to the following equation:

$$\underbrace{(CCl_3)\,SO_2Cl}_{\text{Trichlormethyl-sulphon-chlorid.}}{}^{*} + H_2S = \underbrace{(CCl_3)\,SO\,OH}_{\text{Trichlormethyl-sulphurous acid.}} + HCl + S.$$

The decomposition of this acid and its salts by bromine and nitric acid is very striking; the acid disappears and new insoluble compounds are formed.

The action of bromine is expressed by the following equation:

$$\underbrace{(CCl_3)\,SO\,OH}_{\text{Trichlormethyl-sulphurous acid.}} + Br_2 = \underbrace{(CCl_3)\,SO_2\,Br}_{\text{Trichlormethyl-sulphon-bromid.}} + HBr.$$

and the action of nitric acid:

$$\underbrace{(CCl_3)SO\,OH}_{\text{Trichlormethyl-sulphurous acid.}} + \underbrace{NO_3H}_{\text{Nitric acid.}} = \underbrace{(CCl_3)SO_2(NO_2)}_{\text{Trichlormethyl-sulphon-nitrid.}} + H_2O$$

The trichlormethyl-sulphon-bromid is a white crystalline body of a faint acrid smell, insoluble in water but soluble in alcohol and ether. On the application of heat a part is sublimed without change while another part is decomposed. Ammonia dissolves it with evolution of nitrogen.

0·358 grm. yielded 0·313 sulphate of barium corresponding to 12·01 per cent S.

0·492 grm. yielded 1·158 grm. AgCl+AgBr and this mixture yielded by reduction 0·808 Ag, which proportions correspond to 30·77 per cent Br, and 40·22 per cent Cl.

The theory demands 12·19 per cent S, 40·57 per cent Cl, 30·48 per cent Br.

When trichlormethyl-sulphurous acid is brought into contact with concentrated nitric acid, a violent reaction takes place and a blue oil is deposited which loses nitrous acid by contact with the air and is converted into a white crystalline body, the odor of which inflames the eyes and is very suffocating when inhaled. This is the trichlormethyl-sulphon-nitrid especially interesting in this, that a nitrid of an organic sulph-acid has been hitherto unknown. It is insoluble in water, soluble in ether and alcohol. Water added to the alcoholic solution precipitates it unchanged. It does not melt in boiling water but is volatilized with the steam. Ammonia dissolves it slowly with decomposition, alcoholic solution of caustic potassa decomposes it quickly. When it is treated with zinc and hydrochloric acid it disappears and is converted into sulphydrate of methyl and ammonia:

* C=12 S=32 O=16.

$$\underbrace{(CCl_3)SO_2(NO_2)}_{\text{Trichlormethyl-sulphon-nitrid.}} + 18H = \underbrace{CH_3\,SH}_{\text{Sulphydrate of methyl.}} + H_4NCl + 2HCl + 4H_2O$$

0·5285 grm. yielded 0·532 sulphate of barium and 0·973 AgCl, corresponding to 13·83 per cent S and 46 12 per cent Cl.
Theory demands 14·00 per cent S and 46·61 per cent Cl.

On treating trichlormethyl-sulphon-chlorid with ammonia, trichlormethyl-sulphurous acid is formed in an unexpected manner, nitrogen is evolved and the chlorid is slowly dissolved. On evaporation broad tabular crystals are obtained. In the mother-liquor there remains chlorid of ammonium, some sulphate of ammonium and a certain quantity of this new compound. The crystals must be recrystallized with great care because at a moderate temperature an acid reaction sets in and a partial decomposition begins. These crystals show with bromine as well as with nitric acid the formation of the above mentioned bromid and nitrid. The action of the nitric acid is extremely violent.

By mixing with diluted hydrochloric acid and agitating with ether the above mentioned trichlormethyl-sulphurous acid is obtained.

0·431 grm. yielded 0·911 grm. AgCl=52·24 per cent Cl.
0·668 grm. yielded 0·725 ammonio-chlorid of platinum=6·80 per cent N.
0·753 grm. yielded 0·894 grm. sulphate of barium=16·23 per cent S.
Theory demands 53·11 per cent Cl, 6·98 per cent N, 15·96 per cent S.

The crystals are therefore the ammonium salt of the trichlormethyl-sulphurous acid and their formation can be expressed by the following equation:

$$3\underbrace{((CCl_3)SO_2Cl)}_{\text{Trichlormethyl-sulphon-chlorid.}} + 8H_3N = 3\underbrace{((CCl_3)SO_2H_4N)}_{\text{Trichlormethyl-sulphite of ammonium.}} + 2N + 3H_4NCl$$

On treating trichlormethyl-sulphon-chlorid with cyanid of potassium, evolution of cyanogen and hydrocyanic acid takes place and the potassium salt of trichlormethyl-sulphurous acid is formed:

$$(CCl_3)SO_2Cl + 2KCy = (CCl_3)SO\,OK + KCl + 2Cy$$

But when the solution is hot and very concentrated, there is also another derivative of this acid formed, one atom of Cl being replaced by one atom of OH.

$$(CCl_3)SO\,OK + H_2O = (C\,Cl_2OH)\,SO\,OK + HCl,$$

which latter decomposes another part of the cyanid of potassium.

A glance at the following formulas will show the relation between these new bodies and several others already known:

Trichlormethyl-sulphon-chlorid $(C\,Cl_3)SO_2Cl$ (Kolbe).
Trichlormethyl-sulphon-bromid $(C\,Cl_3)SO_2Br$.
Trichlormethyl-sulphon-nitrid $(CCl_3)SO_2(NO_2)$.

Trichlormethyl-sulphur*ic* acid $(CCl_3)SO_2OH$ (Kolbe.)
Trichlormethyl-sulphur*ous* acid $(C\,Cl_3)SO\,OH$.
Methylsulphur*ic* acid $(CH_3)SO_2OH$ (Kolbe.)
Methylsulphur*ous* acid $(CH_3)SO\,OH$ (Hobson.)

New York, January, 1869.

ART. XXXII.—*Note on the structure of the Blastoidea;* by E. BILLINGS, F.G.S., Paleontologist Canada Geol. Survey.

THE remains of the Blastoidea have as yet proved to be extremely rare in our Canadian rocks, only five small specimens—three of *Pentremites* and two of *Codaster*—having been collected up to the present time. While studying these with a view to their description I was led to investigate the structure of the order, especially with regard to the function of the summit openings. On combining the observations of other authors, whose views I shall give in detail in another paper, I find that we have now sufficient data to establish the following points:—

1. In the genus *Nucleocrinus* Conrad, there are sixteen apertures in the summit. Of these the large lateral aperture is both mouth and vent. There is no opening in the center of the apex where the mouth has hitherto been supposed to have its position. The ten so-called "ovarial orifices" are respiratory apertures. Between each two of these one of the ambulacral grooves enters to the interior through a small pore which is a true ovarian orifice. There are thus ten spiracles, five ovarian orifices and one buccal and anal orifice—in all sixteen.

2. In *Pentremites* there are also five ovarian pores, in the same position. The mouth is not in the center, but in the larger of the five spiracles.

3. *Codaster* has no ambulacral pores in the so-called "pseudambulacral fields." The striated surfaces in the interradial areas are true Cystidean rhombs of the type of those of the genus *Pleurocystites.* These in *Pentremites, Granatocrinus* and *Nucleocrinus* are situated under the ambulacra where they constitute the tubular apparatus described by Roemer and others.

ART. XXXIII.—*Notes on the occurrence and composition of the Nodular Phosphates of South Carolina;* by CHARLES U. SHEPARD, Jr., M.D., Prof. of Chemistry in the Medical College of the State of South Carolina.

THE belt of nodular phosphates appears to extend, more or less interrupted from the Wando and Cooper rivers, some fifteen to thirty miles above Charleston, in a south-southwesterly direction, parallel to the coast line, as far as St. Helena Sound and Bluffton, near Port Royal. As yet the precise area is unknown; no accurate survey having been made, although this want is daily felt by the community. It would be erroneous to suppose that there is a well defined stratum of any such extent as this area above mentioned; on the contrary, the bed appears only in patches, some of which, however, are many miles in diameter. On the Wando and Cooper rivers the nodules are found in comparatively small beds, generally but a few inches in thickness; still, limited deposits, one to three feet thick, have been reported in some localities of this neighborhood. On the peninsula between the Cooper and Ashley rivers, the deposit assumes the form of a well defined stratum, in many places attaining a thickness of eighteen to twenty-four inches, and underlying hundreds of acres, at an average depth of about three feet from the surface. The nodules vary in size from that of a walnut, to masses weighing two hundred pounds and over; they lie compactly together with but little marl between them. This marl is composed of 30 to 60 per cent carbonate of lime, a few per cent phosphates of iron, lime, and alumina,—the balance being chiefly sand and peroxyd of iron. At other points on the peninsula, the nodules rarely exceed a few lbs. in weight, and are sparsely distributed. The favorable localities lie east of Goose creek, near the Cooper river. The Ashley beds were the first discovered, are the best known, largest in extent, and most mined. This deposit extends, at an accessible depth, over, perhaps, one thousand acres, on both sides of the river; and running back from it for several miles in some places. The beds are quite accessible, not only on account of the depth of the Ashley river and their proximity to Charleston, but because of their lying close to the surface (generally within two feet) in a light soil which separates easily from the nodules on handling or washing. The nodules are of a yellowish-gray color, of less specific gravity than those elsewhere found, their surfaces but slightly irregular, and their composition tolerably uniform. The best beds lie on the river ten to twenty miles from Charleston; farther up stream, the nodules are found in a sandy soil, and become permeated with sand to the amount of thirty per cent and over, when the phosphates do not reach fifty per cent. On some plantations the bed of phosphatic nodules is over two feet in thickness; and the amount of marketable material produced from mining an acre may exceed twelve hundred tons. On the Stono and Edisto rivers there have been found but few rich deposits,—the stratum exhibiting continuity in but occasional spots. As a rule, the nodules lie deeper on these rivers than on the Ashley. Heavy deposits have been discovered on the flats in the neighborhood of St. Helena Sound, covering vast surfaces at little depth from the surface, occasionally forming a compact floor, or huge boulder-like masses on the bottom of the creeks which intersect that neighborhood. Finally, on the Ashepoo river, at one locality in this neighborhood, the stratum has the appearance of an immense pavement, extending over hundreds of acres at a depth of three to six feet. It is with difficulty that the large masses (often several hundred weight each) can be pried apart, so closely are they wedged together,—having a smooth, glazed, upper surface, but irregular beneath. The masses moreover are often penetrated to considerable depth, sometimes perforated by round holes, which extend generally in a perpendicular direction. These cavities have a diameter of one-half to one inch. The phosphatic masses forming this floor are nine to twelve inches in thickness, and overlie a bed of nodular phosphates of smaller size, which extends down to the depth of twelve to fifteen inches below the continuous stratum. The whole deposit is imbedded in a tenacious clay, underneath which occurs a yellow-red marl. This marl is rich in shells and the bones of marine and land animals. It is composed, when air-dry, of nearly 70 per cent sand, 18 per cent carbonate of lime, and 5 to 7 per cent phosphates of lime, alumina and iron. It is reported that the nodules form in some limited localities a second layer, and as continuous as the top stratum, —underlying it at the depth of about one foot.

The phosphatic nodules and masses generally give on friction of their fresh surfaces, a peculiar naphthous odor. This property is, as a rule, the more decided, the denser the nodules; and is in direct proportion to the amount of organic matter contained in them. The impressions of numerous fossil shells of the Eocene period occur throughout the various phosphatic masses.

Composition.—The analyses given below are of hand specimens, taken at random from large quantities of the material of the different localities mentioned. They are not supposed to represent the exact composition of the formation from each neighborhood, since a material difference is often remarked in nodules from the same pit; but these analyses have been chosen from a large number made during the past year, as showing what may be considered the average. It must be stated, however, that the amount of peroxyd of iron, alumina and magnesia given under No. 3, from the Ashley, is higher by a few per cent, than is generally the case in the nodules from that neighborhood. The specimens analyzed were tolerably clean, and more or less air-dry. With regard to the determination of the phosphoric acid, there exists considerable difference of opinion among the chemists who have had the most to do with the analysis of the South Carolina nodular phosphates,—each one, it appears, preferring a different method. My experience, as far as it extends, leads me to consider the protochlorid of uranium method as the best for the quantitative determination of phosphoric acid in compounds containing oxyds of iron and considerable alumina. It is true that the results are much lower than those obtained by other methods; but as yet I have found no reason to doubt their accuracy. The organic matter amounts to about one-third to two-thirds of the percentage put down to water of combination and organic matter. Some nodules contain nitrogenous matter sufficient to yield on decomposition with soda lime over ¼ per cent of ammonia. (In one specimen 0·26 per cent ammonia was determined). The magnesia rarely exceeds one half per cent.

Analysis No. 1, from the Cooper river.
" No. 2, " near Goose creek.
" No. 3, " the Ashley river.
" No. 4, " " Stono "
" No. 5, " " Edisto "
" No. 6, " " Ashepoo "
" No. 7, " " Ashepoo "
" No. 8, " " St. Helena Sound.

	1.	2.	3.	4.	5.	6.	7.	8.
Specific gravity,	2·44	2·03	1·94	2·19	2·28	2·54	2·49	2·7
Moisture (at 100 C.),	1·8	3·04				0·8		0·87
Water of combination and organic matter, expelled at low red heat,	4·58	6·03	9·15	7·5	7·3	5·05	8·03	7·78
Carbonic acid,	4·0	3·54	2·55	4·6	3·5	5·5	6·62	3·87
Sulphuric acid,	1·8	1·5	2·4	2·3	1·9	2·6	2·57	2·15
Phosphoric acid,	25·28	26·17	24·95	27·24	30·17	29 84	27·55	28·9
Lime,			35·71					38·58
Peroxyd of iron, alumina and magnesia,	5·92		15·01	5·6	7·6	6·	8·3	17·4
Sand,	15·7	12·13	11·67	11·27	15·0	10·5	4·	

Traces of chlorine, fluorine and sodium.

Artesian water of Charleston, S. C.—The analysis of the water of the artesian well of this city may interest some readers, not only the scientific who may possibly draw important inferences from a knowledge of its composition, since it comes up from below the layer of phosphatic nodules (sixty feet from the surface), but also travellers and health-seekers, who every season drink at this agreeable well, which is also quite popular among our citizens.

The following analysis is the average of several, completed at intervals of a few weeks during last winter and spring. Slight, but unmistakable, changes were noticed in the proportions of some of the ingredients during these investigations. There are two wells, only one, however, is completed. This, the old one, is about 1250 feet deep. The temperature of the water at the spout is 87° F. (or 30·7° C.). Specific gravity (taken at 15° C.), is 1·0015. The amount of solid ingredients in the water = 0·228–0·234 per cent.

In 100 parts solid ingredients:

Bicarbonate of soda	52·749
Chlorid of sodium	47·051
Bicarbonate of lime	0·0883
Bicarbonate of magnesia	0·01375
Silica	0·00102
Phosphates of lime, iron and alumina	0·0004
Organic matter	0·0017
Sulphuric acid in traces.	

In 100 parts well water—

Bicarbonate of soda	0·1435
Chlorid of sodium	0·128
Bicarbonate of lime	0·000273
Bicarbonate of magnesia	0·0000323
Silica	0·0000238
Phosphates of lime, iron and alumina	0.0000093
Organic matter	0·0000467
Free carbonic acid	0·0018
	0·27366

In 100 imperial gallons (at 15° C.), are contained about one and a half pounds bicarbonate of soda and one and a quarter pounds common salt.

Charleston, S. C., Jan. 21, 1869.

Art. XXXIV.—*Notes on American Fossiliferous Strata*; by T. A. Conrad.

The discovery of extinct Unionidæ in a clay bank on the New Jersey side of the Delaware river, has directed my attention to the fossil Unionidæ, found by Dr. Hildreth near Marietta, Ohio, described by Dr. Morton in this Journal, in 1836. Comparing specimens from the Delaware with those of Ohio, two species were found to be common to both localities, while a third species, found in West Virginia, near the Ohio river, corresponds to another Delaware species. Again, another Unio from the latter locality, which was presented to me several years ago by Henry C. Lamborne, is found to occur in the bank of the Potomac near Alexandria.

Unio radiatoides Lea,	N. Jersey,	W. Virginia.
U. saxulum Morton,	do.	Ohio.
U. cariosoides Lea,		
U. nasutoides Lea,	do.	Alexandria, Va.
Anodon abyssina Morton,	Ohio,	do.
A. grandinoides Lea,		

Professor Cope has obtained from the New Jersey clay a portion of a horse's head, which animal undoubtedly lived in the same period with the fresh water shells above mentioned. The question of the geological age of the clay is thus narrowed to more restricted limits, and it must be included in a series from Miocene to Post Pliocene. It has long been a desideratum to discover traces of the rivers and their deposits of the Miocene period of this country. Only one species of fresh water Univalves, and an estuary shell living exclusively in brackish water had noted a trace of a river and estuary, before the fossil shells of the Delaware were found. It seems probable now, that we have evidence of the Delaware in the Miocene period a much broader river than now, and containing a group of Unionidæ wholly extinct, and very different from the existing group. This ancient bed of the Delaware must have left many traces, but as yet they are not recognized from the great scarcity of fossils except in a mere seam less than a foot in depth. The presence of Anodon in every known locality would seem to imply tide water, ponds or sluggish rivers which became gradually filled up with sediment, and the shells were buried alive, showing no trace whatever of swift flowing or agitated waters. They, however, are all lying on their sides, whereas if the bed of the present river were elevated the Unios would generally be found in a vertical position, as we find the Glycimeris in the

Tertiary of Maryland. This group of extinct freshwater bivalves seems to have been more uniform in character than those of the present day, since it is generally the same on both sides of the Appalachian range, where now the mountains separate them into two very dissimilar groups, one plain and few in number, and the other numerous, with every variety of form. It is this uniform character, together with the extinction of the shells which lead to the conclusion of their Miocene age. Of the Pliocenes we have no trace along the Atlantic slope, and therefore the geological position must be either that of the Miocene, or of the Post Pliocene, in which latter formation all the species either marine or fresh water are identical with existing forms. The Ohio deposit containing extinct species is thus described by Dr. Hildreth. They are "in a bed of fine micaceous and siliceous sand, the upper part mixed with blue clay." This clay is identical in composition and color with that of the Delaware, containing the same amount of micaceous fragments, showing that both deposits were filled up with similar sediment, and at the same period. The fossils of Ohio are mineralogically the same as those of the Delaware and Potomac, ferruginous casts with portions of the shell remaining. The former are at an elevation of about 600 feet above the latter, which may be due to a rise in the Appalachians. Many naturalists may doubt the Miocene age of this group in consequence of the presence of *Equus fraternus* Leidy—no solid-ungulate horse having been found in the European Miocene, and because this same species occurs in the vicinity of Charleston in company with the remains of domestic animals. But Dr. Pratt, having studied the geology of that place, found the latter "entombed in direct contact, and in one common burial with the bones and remains of another and older geological period." There is therefore no doubt that the Charleston *Equus fraternus* lived in the same period with the horse of the Delaware clay.*

Crosswicks Group.

The lower beds of the eastern Cretaceous are exposed in the sections of the deep cut of the Chesapeake and Delaware Canal, and at Crosswicks, N. J. The group of casts of shells in these beds consists of comparatively small species, nearly all limited to these lower strata, so far as my observation extends. Gryphæa and Exogyra, so abundant in the higher beds of New Jersey, I have not seen in this group.

* Prof. Emmons found portions of *Equus fraternus* Leidy, in a Miocene marl pit, near Elizabeth, Bladen Co., N. C., and other specimens in the Miocene of Pitt Co.

List of Shells of the Crosswicks Group.

Crassatella prora Conrad.
Inoceramus peculiaris C.
Trigonoarca passa C.
Goniosoma inflata C.
Cyprimeria spissa C.
Axinea Mortoni C.
Nuculana Slackii Gabb.
Gyrodes infracarinata Gabb.
Natica (*Lunatia ?*) *obtusevolva* Gabb.
Turbinopsis depressa Gabb.
Actæon ovoidea Gabb.
Actæon cretacea Gabb.
Baculites vetus Conrad.*
Scaphites hippocrepis Dekay.
Solenoceras annulifer (*Hamites*) Morton.

RARITAN CLAY.

In his report on the geology of New Jersey, Professor Rogers describes a series of clays on the Delaware and Raritan rivers, which he considers the base of the Cretaceous system of the Atlantic slope. By some geologists it is supposed to be the equivalent of the Wealden of Europe, but I think it is of later origin ; posterior to the lower greensand, and indicating a continent, which disappeared beneath the cretaceous ocean. These clays seem to be evidences of estuary deposits, as I ascertained by examination of the banks near the village of Washington on South river, a branch of the Raritan, Middlesex Co. These clays rest nearly horizontally and regularly on the undulated and highly inclined surface of gray quartzose sand, replete with ferruginous seams, full of carbonaceous remains of vegetations, too fragmentary to indicate the class of plants from which they have been derived. These seams follow the line of the surface and show the sand to have been disturbed by the movements of the Triassic on which they rest. The lowest stratum of clay is ash colored, in which vast numbers of upright stems of a Neuropteris are seen passing through densely crowded impressions of large leaves, doubtless parts of the same plant. There is evidence of a gentle current in the slightly diverging stems, or it may be of wind acting on the stems above water, but the sediment fell gently on the plants and buried them in clay, which can be traced more than 10 miles at nearly the same elevation above tide. This lower bed is about four feet thick near Washington, and over it is a like thickness of black clay, the upper part full of willow shaped leaves with an entire margin. Over this again is a light colored clay of 20 feet or more in thickness, replete with similar leaves and also vegetable impressions of other kinds.

Mr. Durand informs me that a fragment of one of the leaves from this clay, is that of a fern, the spots on which represent the fructification of the plant. These leaves are abundant in

* This Baculite has more distant and much less deeply lobed septa than *B. ovatus*. I have as yet but a small fragment of this species.

a portion of clay considerably above the shells. Other plants are a new species of Podozamites (*P. proximans* Conrad, described below), nearly related to *P. lanceolatus* Emmons; a Cyclopteris which is probably identical with Emmons's species, figured in the geology of North Carolina, vol. i, p. 4, fig. 10. This is extremely abundant in the lower part of the bluff, in the light colored clay which rests on plicated sand. The latter no doubt is part of the same formation, and overlies the Red Sandstone of New Brunswick, and although the point of junction is unknown, the Raritan clay and the red sandstone may be traced within two miles of each other. Impressions of the leaves of this Cyclopteris are extremely numerous, whilst the stems are also abundant, generally in a vertical position, or so inclined and disturbed as to suggest an agitation from wind or eddies placing them in a position in which the sediment preserved them.

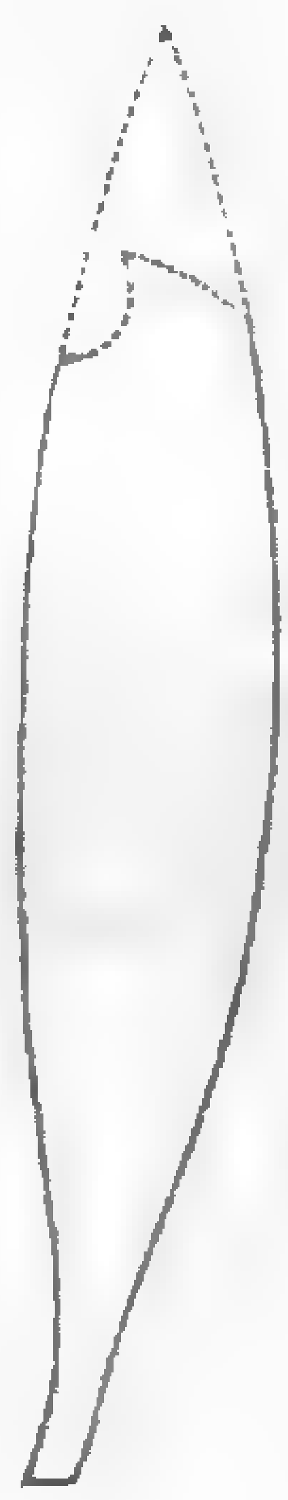
Description of a new Cycadaceous plant from the Raritan Clay.

PODOZAMITES.

Podozamites proximans Conrad. Lanceolate, gradually tapering at base.

Locality, near Washington, on South river, N. J.

This short description will serve to distinguish it from *P. lanceolatus* Emmons.



These plants go far to prove the Raritan clay to be of Triassic age, but I have further evidence in the shells to convince me that it is the equivalent of the Muschelkalk of Upper Silesia. Dunker in "Paleontographica," describes and figures some bivalves of the Muschelkalk of Silesia. which if not identical with the shells of the Raritan clay, must be nearly allied species, if external form is of any value for comparison. Dunker's shells are very perfect, and the hinge character well represented. They constitute a group in the genus Astarte, wholly unlike any that existed before or after the Triassic period, and one species at least I cannot distinguish from one in the Raritan clay. This genus Astarte of Sowerby originated in the Triassic period, and culminated in the Jurassic. In the first the species are few, plain and small. In the latter they are large, with great variety of form, and the generic character greatly developed. I am not sure whether *A. concinna* of the lower green-sand is a true Astarte, but if it should be, I believe that this formation holds the last of the genus. I find none in the chalk or its representatives. *Crassina* Lam., with which Astarte is usually confounded, originated in the Miocene period, and appears to have culminated in the North

American seas of this age, for we know eighteen species, while there is but one recent on the coasts of the Middle States, and four on those of the Eastern.

A singular cast in the Muschelkalk of Silesia is figured in "Paleontographica," vol. 1, tab. xxxii, fig. 33. Dunker remarks that it has some resemblance to a valve of a bivalve shell, but I have the best authority for pronouncing it a form of Entomostraca, though differing from any recent genus. I found a cast of the same genus in the Raritan clay, of a shorter outline than Dunker's, and with only three tubercles. The latter has five or six equal tubercles, situated like the Raritan species on the lower submargin. I propose to name this Triassic genus *Paleocypris;* the American form *P. trinodiferus* and the European *P. triassina.* The former is distinctly visible with a lens.

In a small mass of ash colored clay taken from the lowest stratum, a few fern roots are well represented in pyrites, of an upright habit, and not creeping like the rhizomes of existing ferns. The secondary roots are numerous, some of the laterals as fine as hairs, and from the base of the main root large branches run downward. This must have been a bed of ferns growing in a sandy, slightly micaceous clay, and yet in this indifferent soil grew long, stout, flexible stalks strongly striated longitudinally. The foliage is yet unknown, for the stalks are cut off as it were by a black clay above, in which the traces of vegetation are obliterated, although the dark color of the clay is derived from carbonized leaves. Above this four feet of a black stratum, there is a thick mass of ash colored clay, in the lower part of which are innumerable impressions of a Cyclopteris, the leaves of which are of a peltate form, and the stalk differs in its character from those of the lower bed. It is not longitudinally striated, but irregularly plicated. There are probably two or more species, but the imperfect specimens do not indicate the difference. F. A. Roemer has figured a fossil which he named *Cyclopteris peltata,* but the nervation is wholly unlike that of the Raritan species if his figure is correct, the nerves in which are not branched. One of our species resembles the leaf figured by Emmons, which is too imperfect in outline for comparison, but the nerve character is similar. There are two European species described with peltate leaves, and both by the same name, *C. peltata* Göppert, of the Lias, having priority over Roemer's name of *peltata.* The former also differs from the New Jersey species in a simpler or less branched form of the nerves. One of the fragments of a Cyclopteris in the Raritan clay has a deeply sinuous margin, and when entire, was over two inches in length.

In this bed, just over the black clay beneath, a patient search revealed the presence of fresh water shells, two or three species of *Pisidium*, and one apparently of *Cyrena*, which would indicate this line of the high lands on the south side of the Raritan to have been a wide estuary, although no other trace of the rivers on land of this period is known.

About a quarter of a mile east of this bluff in the suburbs of Washington, I observed similar clays at about the same level, and in the black stratum, leaves as abundant as it would be possible to press them. Lignite is plentiful, and large fragments of the trunks of trees. The clays around Baltimore are supposed, with reason, to be synchronous with the Raritan clays, and therefore it is highly probable that the fossil forest at the mouth of the Patapsco, described by Durand, may be of the same geological age.

Eocene and Miocene of Shark river, N. J.

The village of Trap on Shark river shows a good section of Eocene greensand, overlaid by about six feet of Miocene marl. This Eocene is known by the name of Squankum marl, and contains few organic remains. Prof. Cope has described two species of *Paleophis;* and a *Cœlorhynchus* also occurs in it. I have no doubt that Leidy's *Anchippodus riparius* was obtained from it, and that it was an Eocene pachyderm. Equally certain am I that the peccary tooth described by Leidy, found at Shark river, was a Miocene species. It was picked up by Dr. Kneiskern at my feet while walking with him over a bed of Miocene marl, replete with shark's teeth, and at some distance from the bluff from which it came. The color, mineralization, and degree of corrosion, all agree with the shark's teeth, and with a cast of *Volutilithes* found on the same spot, and which latter fossil is composed of a light ochreous impure limestone. Prof. Leidy would refer this specimen to his *Dicotyles nasutus* if it had been found in the same bed. This species was obtained in digging a well in Indiana, at a depth of 30 or 40 feet, and referred to the Post Pliocene period, without any evidence that the deposit belongs to that period. On the contrary, there is reason to believe that the animal lived at the same time with the fossil Unionidæ near Marietta, Ohio, both the peccary and the shells being found at a depth of about 40 feet, and that the same quadruped lived at that time in New Jersey. This will give some idea of the extent of land in the Miocene era, and the only reason why we have so few traces of the Miocene continent west of the mountains is because it is buried below the surface, and only made known to us in digging wells. On the contrary the Post Pliocene beds are often seen in the river banks and contain shells of existing species, while a more trenchant line of demarkation is nowhere seen than between these periods, as we might expect to be the case where the long interval of the Pliocenes has passed between them.

The Miocene line at Shark river is distinguished by a thin layer of calcareous earth replete with the teeth of several species of shark, of a pale ochreous color, whilst those of the Eocene are black; over the layer above mentioned is about six feet of dark Miocene marl, nearly or quite destitute of organic remains.

The upper part of the Eocene is a hard gray rock, about four feet thick, abounding in large green grains, and holding numerous fossil remains, among which *Aturia* is most characteristic of the geological age of the formation; below this of unknown depth is the loose green-sand with a few Eocene species of reptiles, mammals, fishes and plants.

Art. XXXV.—*On certain Phenomena of Transmitted and Diffused Light*; by M. Carey Lea, Philadelphia.

When a beam of sunlight is thrown upon a white screen at the distance of fifteen or twenty feet and a plate of finely ground glass is interposed in its path, the white light by passing through the colorless glass acquires a deep orange yellow coloration. A greenish or bluish tinge in the glass does not interfere with the experiment, but it is necessary that the grinding of the glass be exceedingly fine, the surface must be scarcely removed, and with the finest emery.

This experiment, which admits of some very interesting variations that will be mentioned farther on, is, I believe, new. Those upon record which approach most nearly to it, relate to certain properties of milk and of magnesia respectively, which when diffused through water produce a *reddish diffuse transmitted* light, and a bluish diffuse reflected light.

My own experiment just mentioned, differed from these in two points; first, that the red transmitted light was direct, and not diffuse; and secondly, that the blue diffuse light was wholly wanting. Nevertheless, a study of the phenomena led me to the conclusion that these several experiments as well as many other new ones, depended upon the same cause, and that consequently the explanation given for the few cases that have been up to this time observed and described, is insufficient.

Becquerel in speaking of the two cases just mentioned, viz: magnesia and milk, observes in his very valuable work on Light:

"La diffusion qui est tres-forte pour les rayons très-réfrangibles fait paraitre ces corps blancs et même bleuatres par reflexion, et jaunatres par transmission. En effet la lumière transmise comprend celle qui échappe à la diffusion par reflexion ainsi qu'à l'absorption; plus cette diffusion est grande, plus la partie est faible à égalité d'absorption, et vice versa; en outre la couleur des rayons diffusès est complémentaire de celle des rayons transmis."*

This indeed can scarcely be called an explanation, it is rather a re-statement of the facts in a more general form and simply affirms that the red rays have a greater tendency to be transmitted and the blue to be diffused. Nor does this seem to have been deduced from any observed properties of light, but appears to have been adopted from an analogy real or supposed, with properties of heat. It will be necessary therefore to glance for a moment at these properties of heat.

In 1840, Melloni showed the necessity of admitting the existence of a diffusive power in heat, from overlooking which, a large portion of the experiments recorded up to that time, were rendered inconclusive, especially those of Leslie, some of which till then, had been looked upon as fundamental in establishing the laws of heat. Up to that time, when heat fell upon any surface, it was held to undergo *specular reflection*, *transmission* and *absorption*. Melloni added to these, *diffusion*.

To enter at large into his experiments would take up too much space here. The essential point is, that, taking rays of heat, chiefly of high refrangibility, issuing from a lamp, he sifted out from these the rays of less refrangibility by causing them to pass through a lens, which at the same time, rendered them nearly parallel. These rays were then allowed to fall upon a disc of pasteboard covered with white lead. A thermoscope placed so as not to receive rays specularly reflected, was nevertheless powerfully deflected. To ascertain whether or not this deflection was produced by radiated heat, he interposed a screen of glass between the disc and the thermoscope; scarcely any diminution of the deflection followed. Had the rays been absorbed and radiated, they would have been of low refrangibility and would have been to a large extent, stopped by the glass screen. As they were not so stopped they were evidently rays of high refrangibility issuing from the lamp. The thermoscope being carried around the disc at the same distance and angle indicated an unvarying deviation. This showed that the effect observed could not arise from specular reflection.

It therefore followed that the deviation of the thermoscope was due to a function of heat different from any previously recognized, distinguished from specular reflection by the fact

* Becquerel, La Lumiere, ii, 10.

that it exerted itself in all directions equally while specular reflection took place in the plane of incidence only; distinguished from radiation by the fact that it is not like the latter, preceded by absorption.

Different substances showed great differences in the energy of their diffusion. Smoke black diffused all rays equally, but very little. Some diffused largely, and all rays equally; this was the case with the metals. Others again diffused rays of high refrangibility much more than those of lower refrangibility.

In seeking analogies between heat and light, Melloni compared this new function of heat with that species of irregular and diffused reflection in light which conveys to our senses the conception of *color*. Smoke black he observed was truly black as respects heat, as well as light, diffusing in both cases very little, but rays of all refrangibilities equally. The metals, in their relations to heat, corresponded with bodies *white* as respects light, as they diffused freely, and all rays equally. Those bodies which exercised an election to the rays, absorbing more of some, and diffusing more of others, he compared to bodies which are colored as respects light. This heat color he termed *thermochrosis*.

This analogy however, though striking, is far from perfect. For those bodies which exhibited an election, always absorbed the less and diffused the more, refrangible rays of heat. If the phenomena of light had been strictly analogous, we should on the one hand have had only black, white, and blue bodies. Or, on the other hand there should be bodies found absorbing the more, and diffusing the less refrangible rays of heat; these would have been analogous to bodies red, yellow and green, as respects light.

This defect in the analogy is not alluded to by Melloni. And of late years this function of heat has been, as appears by the quotation already made, compared to one of light distinct from the analogy of color imagined by Melloni, viz: to the property alleged to exist in milk and magnesia, of being red by transmitted and blue by diffused rays.

It will be observed how extremely unsatisfactory is this whole explanation. The appearances alleged to exist in milk and magnesia are said to arise from the tendency of blue rays to diffusion and red rays to transmission, without any reason being assigned for this tendency. Its existence is assumed to be proved by Melloni's experiments on heat. On examining Melloni's statements we find that he says nothing about the greater tendency of the less refrangible rays of heat to be transmitted, but only to be absorbed. Again, he considered the phenomena which he described to correspond with a set of light-phenomena entirely different. And farther, he attempted no explanation of the elective tendencies of these bodies as respects heat, but simply recorded the fact.

In the present paper I purpose to examine more particularly than has been heretofore done, the phenomena above spoken of, and to show that they have a very wide extension, and present very varied effects. That the appearances heretofore observed and many analogous new ones which I shall here describe, depend altogether upon *Interference,* and under that view are susceptible of a satisfactory explanation.

These phenomena present themselves in three different aspects, which may be classified as follows:

CASE FIRST.—*A strong beam of yellow, red, or reddish yellow* direct *light is produced without the complementary blue being visible.*

CASE SECOND.—*The yellow or red direct beam is visible and simultaneously the blue, the latter* diffused.

CASE THIRD.—*Reddish and bluish light,* both diffused, *are simultaneously visible.*

The simple existence of this third class disproves the explanation usually received and quoted above, for that explanation affirms that the red light tends to transmission, and the blue to diffusion. I shall endeavor presently to show that in one of the very instances usually quoted, the red and blue light undergo diffusion equally.

Case first.—Production of a yellow or red beam of direct light, in the absence of blue.

Let us take an ordinary plane silvered mirror about six inches by eight, and, allowing a large beam of sunshine to pass into the darkened room, let the beam fall upon the mirror lying horizontally or nearly so, upon a table, so as to be reflected upon the ceiling or a white wall, fifteen or twenty feet distant.

Let us now interpose in the path of the ray after its reflection from the mirror a plate of very finely ground glass. The ray which now passes through the colorless glass is no longer white, *but of a deep yellow color.* It is necessary in order that this experiment should fully succeed, that the grinding should be *extremely* fine. Ordinary ground glass will not answer at all.*

As it is extremely difficult to procure glass ground finely enough, it is fortunate that the same result can be produced in many different ways. One of the best, is the imprisonment of

* The glass must be merely "greyed," not ground, and with the finest emery.

gelatinous alumina in a film of collodion. This is accomplished as follows:

A salt of aluminum soluble in alcohol is selected, the bromid is that which I have used. A few drops of strong alcoholic solution of the aluminum compound is added to ordinary plain collodion (made by dissolving a drachm of photographic pyroxylin in six ounces of ether and as much alcohol) and to this a very few drops of strong liquid ammonia are added, and, after shaking, a little ether. After standing a day, this mixture can be extended over glass, and after it is set, the bromid of ammonium is to be washed out, and the film dried.

Such a plate placed in the path of the ray colors it bright orange yellow. If it be laid upon the mirror so that the ray passes through it twice, viz: before and after reflection, the beam will be colored orange red.

Thin strata of bromid of silver upon glass produce a similar effect.

My own views as to the nature of this coloration which will be presently explained, led me to the conclusion that a precisely similar result would be produced by any substance of a sufficiently fine state of division, irrespective of its color. Experiment confirmed this expectation and led to a remarkably striking experiment. Colorless white light was dyed deep orange red by passing through a bright blue film.

To make this experiment in a rigorous manner, both cobalt and chrome blue were rejected in consequence of their tendency under certain circumstances to transmit red rays, by reason of their two maxima of absorption. *Copper* was selected which is wholly free from this objection, and films of hydrated oxyd of copper were obtained by preparing a saturated alcoholic solution of chlorid of copper, and by treating it precisely as explained in the case of alumina.

Reflected sunlight transmitted through these plates was colored red. I scarcely know any experiment in optics that strike an intelligent observer with more surprise when first seen, than this. The film when observed by diffuse light is blue. White objects viewed through it are colored strongly blue, but rays of sunlight transmitted through it, become salmon-red.

Case second.—Yellow or red direct rays are produced, and blue diffuse light is seen simultaneously.

Finely divided sulphur in the amorphous condition exhibits this effect very beautifully. To obtain it, two or three drops of sulphuric acid are added to fifteen or twenty ounces of water, and then two or three drops of sulphydrate of ammonium. In five or ten minutes the sulphur separates but remains suspended in the liquid. If this be then placed in a glass trough with parallel sides, and a beam of sunlight be sent through it and received on a screen 15 to 20 feet distant, the image has a deep yellow color. The liquid itself presents a rich blue tint. If the liquid be transferred to a large glass vessel with cut facets placed in direct sunlight, the effect is very beautiful, flashes of deep gold colored light contrast finely with the rich blue of the liquid.

Case third.—Blue light and red are seen simultaneously and both are diffused.

When sago is made into translucent paste with hot water, it exhibits a distinct bluish color. The paste is to be largely diluted with water and placed in a black vessel, such as a flat gutta percha trough, in which it should form a layer at least an inch in depth. An oblique beam of sunshine passing through a hole of half an inch diameter in the shutter of a dark room, falls upon the surface, forming a bright oval, surrounded by a halo two or three inches in diameter. One half the halo, that farthest from the window, is yellowish red, the other half, bluish.

The proportion of the two colors is nearly equal, and both are equally diffuse. Common starch gives analogous results, not however quite so well marked. And the same effect is produced by milk diluted with about 50 times its bulk of water. With precipitated magnesia I did not perceive the blue light. It was very perceptible with alumina, though not nearly so much so as with milk, which shows the effect remarkably well.

These various effects appear to be all due to one and the same cause, interference. Let us first take the case of the ground glass and consider the effect of a very small abrasion of the surface. At the edge of this abrasion, two parallel impulsions of light pass, the one through the abrasion, the other through the original surface of the glass. At any point beyond, both have traveled the same distance, with this difference that for a very small space corresponding with the depth of the abrasion, the one will have traveled through air while the other will have traveled through glass. The latter ray will have suffered a change of phase corresponding with the retardation occasioned by the glass and proportioned to its index of refraction, and therefore these two rays will be in a condition to interfere with each other.

If we take the index of refraction of the glass at 1·5 it is evident that the ray passing through the natural surface will be retarded in the proportion of 1·5 to 1 for a distance equal to the depth of the abrasion. If this depth be extremely small, the blue rays will interfere, while the less refrangible rays do not. And for different depths of the abrasion we shall obtain a succession of interferences corresponding with the transmitted spectra of Newton's table of the colors of thin plates.

But as these scratches are unequally disposed over the surface and in close contiguity, it is evident that if various colors are produced at every point of the glass, these will re-compose white light, and the transmitted beam will be white. This takes place with common coarsely-ground glass. But if the abrasions are extremely small and very close, an excess of red light will be produced. If they are chiefly but not altogether small, the red light produced by the small ones will be diluted with white light, both that transmitted without interference and that re-composed after interference. And this exactly corresponds with the results observed.

Taking the case of two adjacent waves so striking upon the glass that one passes through the abrasion while the other passes through the glass, let us suppose that the abrasion has a depth of ·000475 millimeter, and that the glass has an index of refraction of 1·5; it is evident that a mean blue ray whose wave length in air is ·000475 mm. will have accomplished one oscillation while the adjacent wave in passing through the corresponding and equal space of glass will have accomplished exactly one and one half oscillations. Its phase will therefore be precisely the opposite of that of the first wave, and these two will be in condition to extinguish each other by interference. Abrasions therefore of the depth just mentioned will tend to the production of red light by extinguishing a certain portion of the blue.*

The effect of fine particles contained in a film is evidently quite analogous. If one impulse of light passes freely through a collodion film, while the adjacent one encounters and passes through a small particle of oxyd of copper, alumina, etc., it will be retarded or accelerated in comparison with the former according as the index of refraction of the substance is greater or less than that of the pyroyxlin, and in proportion to the relation between the two.

When the red light in place of being direct, is, as in the 3d case, diffuse, it admits of the following explanation. Direct rays of light entering a medium which holds objects of a different refractive power in suspension will be partly transmitted through and between these objects, and partly reflected by

* Although the retardation of glass of 1·5 as compared with air corresponds for a thickness of ·000475 mm. with the half wave length of the mean blue ray, it it probable that the most vivid red light would be produced by a somewhat less retardation corresponding with a somewhat less depth of groove or abrasion.

them. As the reflecting surfaces will be irregularly distributed in every direction, the light which is not transmitted will be reflected in every direction. This diffusely reflected light will in some cases pass through the particles, in other cases pass between them. If the particles have a higher index of refraction, those rays that pass through them will be retarded; if a lower index, they will be accelerated. In either case, those that pass through the edges of the particles will be in a condition to interfere with those that pass immediately adjacent. And if the particles be very small, the number of such interferences will be very great, and the preponderance of color produced will be red by the extinction of blue rays.

Nor is this the only way in which interference may take place. If we suppose the two bounding planes of the particles through which the ray passes, to be parallel or nearly so, it is evident that interference may result between the transmitted ray, and another ray twice reflected in the interior of the particle precisely as in the case of the plate of air, etc., in Newton's thin plates.

This latter mode of interference also gives us easily the key to the production of blue diffused color in the second and third cases.

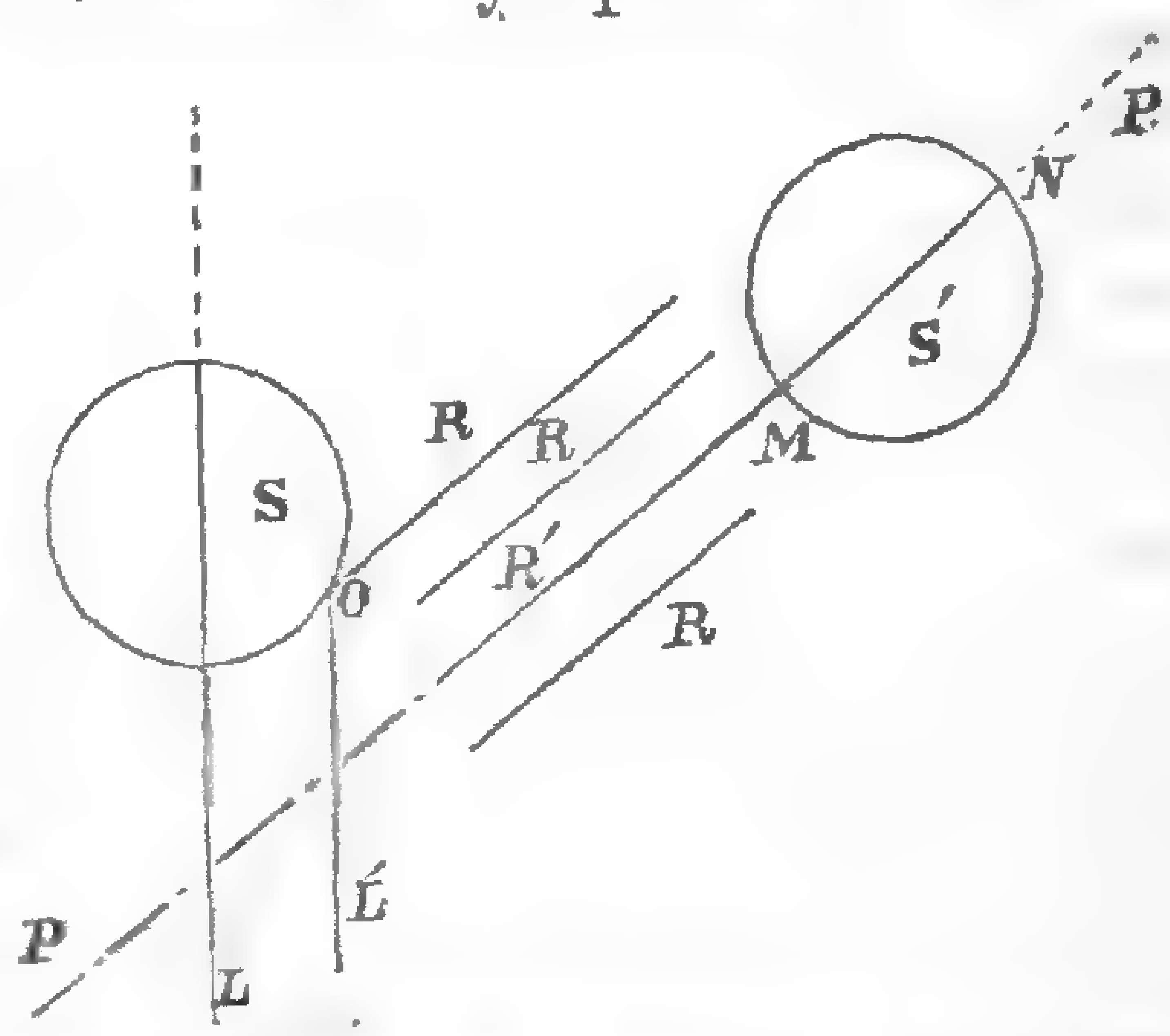
For if the bounding planes of the particle be perpendicular then the light irregularly reflected through the liquid falling in some cases with nearly perpendicular incidence upon particles, the ray reflected at the second surface will be in condition to interfere with that reflected at the first; and if the particles be sufficiently small, this will always result in the production of blue light. This interference will take place equally, and the result will be the same whether the particles have a higher or lower index of refraction than that of the medium in which they are suspended.

For, let us in the first place, suppose them to have an index less than that of the medium. The incident ray then suffers reflection at the first surface without change of phase. The transmitted ray is reflected in part at the second surface, the medium having by supposition a higher index of refraction than the particle, a change of phase equal to half an oscillation is produced, and if the particle had no thickness, the two rays would extinguish each other. If we suppose the particle to have an appreciable though small thickness, the blue rays would become concordant with a thickness smaller than that necessary for the red, and the reflected ray will be tinged with blue.

If the particles have a *greater* index of refraction than the medium, the ray reflected at the first surface will suffer a change of phase, and not that reflected at the second. Thus in either case, whether the particle have a greater or less index than the medium, there will be a difference of phase equal to half an oscillation between the rays reflected at the two surfaces, independently of the thickness of the particle, and this with minute particles will always lead to the production of blue light by the interference of the two reflected beams.

There is one condition that will be particularly favorable for the production of red light by rays *both* of which pass through the particle and of blue light by rays reflected at the front and back surface: this is when the particles have a *spherical form*, as will appear from the following considerations.

Let direct rays of light, L, L′ strike any sphere S. Those that strike nearly in the direction of a diameter will be principally transmitted. Others striking more obliquely at any point O will be largely reflected. As there are many globules, there will result a bundle of rays R R R′R, and similar parcels will be irregularly reflected in all angles and directions; for rays L′O will strike at all points of the front hemisphere of the globule S. Let us consider any one set of parallel rays R R R′R. These will strike other spheres and each sphere will receive rays like R′ following the course of a diameter. At M a portion of the light will be reflected back and a portion will pass into the sphere, part of which will be again reflected at N. These two last beams returning in the direction M R′ P will be favorably affected for interference.



In this way, diffuse rays are irregularly reflected in all directions from the surfaces of the spherical globules, and in falling upon other spheres, each direction finds a corresponding diameter bounded by tangential reflecting planes parallel to each other and perpendicular to it. Consequently a much larger proportion of the incident rays are caused to interfere in the case of spherical particles, than of those that are angular or crystallized.

If the spheres be sufficiently small, this will result in the production of blue light returned in some direction M R′. If the spheres be of irregular sizes, the blue light will be more or less diluted with white. These results will follow whether the spheres have a greater or less index of refraction than the medium, for the reason already given.

Another portion of the ray R′ passes through the sphere at N and interferes with a ray twice reflected, viz: at N and M, with production of red diffused light in some direction at N P′.

The phenomena which I have endeavored to describe and account for, are not without a certain superficial resemblance as respects their origin, to another set of interference effects produced by fine particles adhering as dust upon mirrors or thick plates of glass. They exhibit this characteristic difference, however, that the colors of thick plates are produced by the interference of two waves diffusely reflected by different particles. Whereas in the phenomena here described, this is not the case.

I have included in this brief description a few only of a very large series of experiments. The conditions may be greatly varied, and many phenomena more or less familiar are ascribable to these causes.

Thus lampblack if extended somewhat thinly over glass by smoking it, gives a film which colors luminous objects viewed through it, deep red, for lampblack in thin films is more or less transparent to light (and also to heat, as shown by Melloni). The same lampblack which thus transmits red light, is capable of diffusing blue, as may be seen, not, indeed by suspending it in water, but by diffusing a small proportion of it through a white pigment. The result is not a mere dilution, which would give gray, but the production of the well marked blue color known as lead-color.

Again, if a jet of steam be thrown out into the atmosphere, it is quickly condensed into fine particles of water. If when the sun is near the horizon, we stand nearly in a line between it and the jet of steam and at thirty or forty feet from the latter, it acquires a strong blue shade.

So when the sun shines through hazy air, its light takes a yellow tinge. The same is the case when it is viewed through a considerable thickness of water, as by a diver. The blue color of the sky, and the red of the morning and evening sky, have been explained in an analogous way. There can be little doubt that the brilliant colors of the clouds at sunset are due to interferences, but in this case the interference is between two waves both reflected, precisely as in the case of halos. An analogous effect to halos can be produced by breathing on a glass plate and observing through it the bright light of the sky as seen through a hole in a dark shutter. This experiment is not new; and the circular form of the interference rings seen depends upon the regularity of the particles. But if the regularity be destroyed by rapidly moving the plate in irregular directions while breathing upon it, irregular patches of color are produced which are the analogues of natural colored clouds. In fact the rich colors of the sunset are but broken and irregular interference halos.

Wood smoke seen in large quantities as from piles of burning brushwood, is strongly blue. As we pass nearly into a line with the sun, having the smoke between us and it, the color becomes less decided, until when the light becomes almost direct, it is yellow. A red or yellow diffuse light produced by interference caused by spherical globules, will always be weaker than the corresponding blue light, for the same reason that the transmitted spectra of thin plates are weaker than those reflected, viz: the great difference in the intensities of the interfering beams. Consequently, until we pass nearly into a line with the sun it is chiefly the blue interference light that reaches the eye.

Philadelphia, March, 1869.

ART. XXXVI.—*Process for determining the Carbon chemically combined with Iron;* by Prof. EGGERTZ, Director of School of Mines, Falun, Sweden.*

WHEN steel or pig iron containing carbon in chemical combination is dissolved in nitric acid, a soluble brown coloring matter is formed whose coloring power is quite intense, and the solution assumes a tint which is dark in proportion to the quantity of the chemically combined carbon.

Iron and graphite (or free carbon) do not influence this coloring; for the solution of nitrate of iron is colorless, or at most slightly greenish, unless extremely concentrated, and graphite is insoluble in nitric acid.

Thus in dissolving two pieces of different steels of the same weight in nitric acid, taking care to dilute the darker solution until the two liquids present exactly the same color, it is very evident that the more highly carburetted steel will furnish the larger quantity of liquid, and that the proportion of the volumes will indicate the relative proportion of color in the two steels.

If now the composition and content of carbon of one of the steels is known, the absolute per centage of carbon in the other steel may be immediately deduced.

* From the Bulletin of the Chemical Society of Paris; translated for this Journal by J. Wharton, Esq.

Suppose that 1 gram of each of two steels (a and b) have been dissolved, and that the volumes of the two solutions brought to the same degree of coloring bear the relation to each other $a : b :: 5 : 7$. Knowing that the steel (a) contains 1 per cent of carbon, you at once deduce that the steel (b) contains 1·4 per cent of carbon.

In applying this method of analysis, certain precautions must be taken, which we proceed briefly to point out.

In a cylindrical test tube dissolve gradually in the cold 10 centigrams of wrought iron, steel, or cast iron reduced to a fine powder, in $1\frac{1}{2}$ to 5 cubic centimeters of nitric acid of 1·2 specific gravity (about 25° Baumé). The use of nitric acid containing hydrochloric acid must be avoided because the solution of iron would have a yellow tint.

In proportion as the metal contains more carbon, more nitric acid must be used. After some time, when the chief part of the metal appears to be attacked, place the tube in a water-bath to the depth of about fifteen millimeters, and warm it to 80° centigrade. In this position only the lower part of the tube is in contact with the warm water; a movement takes place in the acid which favors its reaction upon the metal; a slight disengagement of carbonic acid from all the particles of carbon may be observed. The operation should always be conducted under the same circumstances as to heat and length of time.

The evolution of gas having ceased (in operating upon steel the reaction must continue two to three hours), place the tube in a large vase filled with water to bring the solution always to the same temperature. This precaution is indispensable because the same liquid is darker when warm than when cold. Afterward, pour off as exactly as possible the clear liquid into a graduated burette. Upon the black residue remaining in the tube pour some drops of nitric acid and heat carefully over a lamp. If there is no further liberation of gas, the residue consists of nothing but graphite or silica. Cool the new solution and add it to that which is already in the burette.

The liquid is then diluted with water until its color corresponds exactly with that of the normal liquid, which latter should be of such a degree of concentration that each cubic centimeter represents ·0001 gram of carbon.

If, for instance, this normal liquid is prepared from cast steel containing exactly $\frac{85}{100}$ of one per cent of carbon, one decigram of that steel must be dissolved in 8·5 cubic centimeters of nitric acid; 100 grams of steel containing 85 centigrams of carbon would thus be dissolved in 8500 cubic centimeters of the normal solution, 100 cubic centimeters of that solution would represent one centigram of carbon, and consequently one cubic centimeter of the normal solution would represent ·0001 gram of carbon.

The normal solution does not keep, and should be often renewed, since it becomes perceptibly paler even within 24 hours. For it however may be substituted a dilute alcoholic solution of sugar properly caramelised brought to exactly the same tint; this solution keeps much longer without sensible change.

As one gram of iron cannot readily be dissolved in less than 15 cubic centimeters of nitric acid, it follows that a proportion of carbon less than $\frac{15}{100}$ of one per cent,* cannot be estimated by means of the normal liquid, but this minimum is seldom found in practice.

If the proportion of carbon exceeds $\frac{5}{10}$ of one per cent, the ferruginous solution is so concentrated that it has a light greenish tint, which renders its comparison with the normal liquid difficult. In that case a normal liquid of one-third the strength is prepared by diluting the normal liquid with twice its volume of water; then each cubic centimeter of the liquid represents only one-third of the ten thousandth part of a gram of carbon. When the proportion of carbon in the specimen to be analyzed is very large (as for instance in white cast iron) only ·05 gram of the metal must be taken for analysis, and in that case half a cubic centimeter of its solution corresponds to a cubic centimeter of the normal solution. If the metal to be analyzed contains graphite, the latter must be collected on a filter before the solution is put into the burette.

This method is more exact in proportion as the percentage of carbon is smaller. With an accurate balance, and with suitable arrangements, a great number of determinations of carbon close enough for practice can be effected in a time relatively very short.

All the Bessemer steel made at Edskin in Sweden is marked after hammering (*apres l' étirage*) by figures expressing its hardness as ascertained by this color-measuring analysis of Prof. Eggertz.

It is obvious that only burettes of perfectly colorless glass must be used, or at least they must all have exactly the same tint.

* O. D. Allen of the Freedom Iron Works, Penn., has found it quite practicable by a modification of this method to distinguish between irons containing respectively $\frac{10}{100}$ and $\frac{15}{100}$ of one per cent of carbon.

Art. XXXVII.—*Geographical Notices;* by D. C. Gilman.

Arctic Researches.

1. Dr. I. I. Hayes continues to commend to public attention the importance of sending out a new American expedition for the survey of the Polar basin, entering by Smith's Sound. The following summary of his project is printed in the *New York Tribune.*

"*First:* as to design. The design of the expedition which I have proposed is to complete the exploration of the entire region northward of Baffin's bay: to trace Greenland and Grinnell Land to their termination; then ascertain if other lands lie to the northward; to explore the Open Polar Sea; and lastly to reach the North Pole, making upon the course such observations as circumstances will allow. Thus will a field be opened for the most valuable discoveries, in geography, geology, in glacier formations, magnetism, countries and currents, and in natural history. *Second:* as to plan. I would set out in May with two vessels, one a small steamer, and would make my course northward, provided with the best chart of Greenland, through the *Middle ice,* until I reached Smith's Sound, in latitude 78° 17′, where, in my old harbor of 1860–'61, I would pass the winter. Here there is abundance of game, and I would found a colony. Walrus, seals, reindeer, and foxes, could be caught in great numbers, and not only would the colony be made self-sustaining, in point of food, but a valuable cargo of furs and oil might be collected. Then I would push northward the next summer with the steamer, and would thus strike for the North Pole. In any case, I would secure a harbor, and a base of operations much to the north of the colony, and thus would the steamer and the colony become the centers from which the explorations already mentioned would be made. *Third:* As to cost. A public-spirited citizen of New York has offered to supply a suitable steamer, and there is good reason to suppose that we could obtain from the government the loan of a sailing vessel, one of the many not in use. These vessels furnished, they could be equipped and maintained in the field through two summers and two winters, at a cost of $40,000. *Fourth:* Let it be remembered that this is "the American route." The land extends there further north than in any other quarter so far as known, and Americans have thence explored to within less than eight degrees; that is to say, within 450 miles of the Pole. Independent, therefore, of the value to science of this particular line of discovery above any other in the unexplored parts of the Arctic regions, there is something of national honor involved in the pursuit of it, especially at this time when England, France, Germany, and Sweden are each aiming to reach the North Pole by various other routes ; to which end expeditions are now actually preparing. Shall we let those nations win from us the coveted honor of priority ? I do not believe there is a single person within the sound of my voice who would be indifferent to the matter, and who would not unite to see the American flag first planted at the North Pole. *Fifth :* As to the advantages of the Smith's Sound route over all the other routes, for discovery in the unexplored parts of the Arctic regions, they are but the simple enumerations which I have before made to the Society : 1. Land as a base of operations ; 2. The opportunity to colonize a party of hunters and natives as the means to a permanent support."

2. Capt. SILAS BENT, late of the U. S. Navy, who rendered important hydrographical services in the Perry expedition to Japan, and who was also attached to the *Preble* under Capt. Glynn in an earlier visit to Japan, has also published his views on the subject of the best mode of reaching the North Pole. They are found in a lecture given before the Missouri Historical Society in St. Louis, Dec. 10, 1868, which embodied two brief communications to the American Geographical Society of New York. (This is printed in a pamphlet of 30 pp. with a map, St. Louis, R. P. Studley.) Capt. Bent while on the coast of Eastern Asia made important observations on the Kuro-Sirvo or Japanese Gulf Stream, which presents some very interesting analogies to the American Gulf Stream.* These observations are contained in the second volume of the Report of the Perry expedition. In his opinion, every attempt to reach the North pole should be made by following the continuation of one or the other of these gulf streams, that is, through Behring's straits or by the Spitzbergen route, which he terms the "thermometric gateways to the pole." He is therefore decidedly opposed to the project of Dr. Hayes for an expedition through Smith's Sound. His views, though formed independently and upon his own observations and studies, coincide in many respects with those which are held by the continental geographers in Europe.

3. The very interesting observations on the coast of "Wrangell's Land," in the Arctic ocean, made in 1867 by Captain Long of an American whaling vessel, and announced in the Pacific Commercial Advertiser, have awakened a great deal of comment in the European journals; and have led Dr. Petermann

* Originally suggested by W. C. Redfield, this Journal, xxv, 131, (1834), xlv, 301, (1843).

in his Journal for January, 1869, to publish a map illustrative of all the researches which have been made in the Polar sea, entering at Behring's straits, from the days of the Russian Deshnew, in 1648, the Danish Behring in 1729, and the English Capt. Cook in 1778, to those of our countryman, Capt. Long, in the summer before last.

This historical survey concludes with some remarks, a sketch of which may interest some of our American readers. The history of discoveries in the Polar sea within Behring's straits is instructive in reference to the question, of what use will the Polar sea ever be? and why these repeated expeditions? So far as yet explored, it is a very limited portion of the ocean, far distant from Europe, known to the Russians for more than two hundred years and yet to them of scarcely any use. But the enterprising Americans have shown what can come from such a sea of ice. Capt. Roys, first of the American whalers, visited this sea in the summer of 1848; he crossed from continent to continent as far as 72° N. lat., saw no ice, but many whales, which were unusually fearless and easily captured; and during the season he had such good weather that the seamen wore light clothing. In consequence of his successful cruise, the next year not less than 154 American vessels manned by 4650 sailors went to Behring's straits and had great success in whale fishery, their captures in two years being valued at $8,442,453.

For twenty years the business has been prosecuted in that region with increasing energy, and, notwithstanding the captures which have been made, it continues to yield important results.

To these remarks the writer adds an exaggerated report of the value of American whale fisheries. We are enabled on good authority to say that whales have diminished in the Arctic seas as in all other fields, and the fleet is consequently reduced. There will not be more than forty vessels in the Arctic during the coming summer.

On the other hand, it is reported that from St. Paul's and St. George's islands, in Behring's sea, south of the straits (part of the Alaska purchase), 200,000 fur seals were captured during the year 1868, valued at about $1,000,000.

An independent treatise on the Arctic fisheries of the German ports is soon to be published as one of the supplements of Petermann's Journal.

Soundings and Temperatures in the Gulf Stream, by Commander Chimmo, R. N.

Comm. Chimmo, R. N., in H. M. S. Gannet, was ordered on a homeward voyage, last year, to define the northern limits of the Gulf Stream, and to take deep soundings and temperatures within those limits. From his communication to the Royal Geographical Society, we gather the following facts. He sailed from Halifax, July 1, 1868.

1. Lat. 43° 20′, long. 60° W., 30 miles south of Sable Island, a sounding was obtained of 2600 fathoms, or 15,600 feet, nearly 3 miles; with a weight of 232 lbs. and Brooke's apparatus, the rod brought up, after four hours patient hauling, foraminiferæ, in their various forms, chiefly globigerinæ, the interior of those fully developed being coated with fine quartzose sand.

2. West edge of the Grand Banks. A sounding of 1500 fathoms, 9000 feet, brought up quartzose sand, with globular forms of calcareous formation, and some algæ with parasitical attachments. The temperature of this mud or ooze was 56°; but at 1000 fathoms the thermometer showed 40° 3′, and at 500 fathoms only 39° 5′, the sea surface being 60°.

3. Having run north of the limit of the Gulf Stream, he stood to the southward, and at a depth of 1500 fathoms the cup brought up the usual gray impalpable mud or ooze. Temperature at surface 65°, and at 100 fathoms 15°.

4. Thirty miles south of the Grand Bank, where the deepest waters of the Atlantic were supposed to be, the same ooze was found at a depth of 1450 fathoms, disproving the idea of the deepest water being here. The same stratum of cold arctic water was here passing under the warmer waters of the Gulf Stream. The rod brought up a piece of feldspar, with particles of mica, evidently deposited by icebergs from Davis' Straits, and that very recently.

5. Lat. 42° 37′ N., and long. 41° 45′ W.; 4300 fathoms of line were run out, and no bottom reached.

6. Lat. 43° 30′ N., long. 38° 50′, where "Milne Bank" is located. From a depth of 2280 fathoms, 13,680 ft., the rod brought up ooze abounding in animal, vegetable and mineral substances. The temperatures were as follows; air 77°, sea-surface 73°, 100 fathoms below, 62°, 300 fathoms below, 55°, and at 1000 fathoms below, 42°. Going north again to the Polar waters, the surface temperature changed in 2½ hours from 72° to 58°.

7. Lat. 46°, 25 miles from the edge of the bank, a sounding of 1000 fathoms brought up large quantities of rounded quartz, of various colors. Here a section was made of the slope of the bank, showing its ascent, formation, etc., from 1000 fathoms, of colored quartzose sand; to 650, of siliceous spicules of sponges; 450, green mud; 150, quartzose sand; 60, stones; 55, stones, sand and fishbones.

8. Lat. 44° 3′ N., long. 48° 7′ W., a sounding of 1650 fath-

oms brought up foraminiferæ, spicules of sponge, and coccoliths. Temperature, 1000 fathoms, 39° 5′; 50 fathoms, 43°; surface, 61°.

9. A second sounding on "Milne Bank," still revealed no bank. In lat. 43° 40′, long. 38° 50′, the lead reached 2700 fathoms, and the rod brought up a small particle of foraminifera.

10. Lat. 43° 30′ N., long. 38° 5′ W., sounding of 2000 fathoms, bringing up foraminiferæ and a piece of stone.

11. Lat. 43° 43′ N., long. 37° 47′ W.; a sounding of 1930 fathoms brought up foraminiferæ. Temperatures, 2000 fathoms, 42°; 1000 fathoms, 43°; 400 fathoms, 49°; 100 fathoms, 59°; sea surface, 69°; air, 68°.

12. Lat. 43° 39′ N., long. 36° 46′ W. A sounding of 2600 fathoms, bringing up foraminiferæ, etc.

13. Lat. 46° N., long. 29° 40′ W. A sounding of 1650 fathoms brought up foraminiferæ and diatomaceæ surrounding six dead hyalæa shells.

14. Lat. 47° 11′ N., long. 23° 14′. A sounding reached the bottom at 2000 fathoms; temperature 42°. In this sounding were many globigerinæ.

These various soundings, varying in depth from 80 to 2700 fathoms, in an area of above 10,000 square miles, from Sable Island to the Azores, show a remarkable uniformity both of temperature and sea bottom. In all the organized forms discovered, says Lieut. Chimmo, no life was perceptible, except in two doubtful instances. "Therefore," he concludes, "these minute creatures do not live where found at the bottom of the ocean." This remark was questioned, after the reading of the paper, by Prof. Huxley, who spoke as follows:

"With regard to the deep-sea soundings which Lieutenant Chimmo had described, speaking with every respect for the zeal and high intelligence which that gentleman had displayed in his observations, and knowing practically how difficult it was to make such observations while at sea, he still might be permitted to remark that they made no substantial addition to what had already been establishsd by a considerable number of observers, with regard to the character of the Atlantic sea-bottom. In some respects he ventured to think—having been favored by the Hydrographer to the Admiralty with the particular soundings that Lieutenant Chimmo had brought home—that he had not quite clearly interpreted the facts. There could be no doubt that animal remains were contained in a very large proportion of the *Globigerinæ* shells. By proper methods of treatment, by dissolving them in acids, you may get out the soft bodies. Not only so, but Professor Frankland, to whom he had submitted portions of such soundings, had determined, by the processes of organic analysis, the existence of more than 1½ per cent of organic matter in these soundings; which 1½ per cent of organic matter could be clearly identified by the microscope in two shapes: in part as *Globigerina* shells, in part as a confused network of simple organisms, distinct from the *Globigerinæ*—one of the most remarkable of simple organisms, to which he had given the name of *Bathybius*. That simple organism—one of the simplest forms of animal life—we now know covered the whole area of the North Atlantic in all the regions that had yet been surveyed. The very admirable soundings in the Indian Ocean, which had been made by Captain Shortland, to which Captain Sherard Osborn referred, had enabled him to extend his knowledge of that organism. From the Arabian Gulf, at a depth of 2800 fathoms, along the whole of the east coast of Africa, round the Cape of Good Hope, and along the west coast until it joined the North Atlantic again, he could trace throughout the whole extent, at these prodigious depths, that that sea-bottom was covered with a network of organic matter. There could be no sort of doubt that living animals exist at the bottom of the deepest seas yet explored. How they lived there, how they acquired their store of food, was one of the most curious questions of organic chemistry; one which we could not solve at present. But it was the fact that there were two distinct constituents in this Atlantic mud: one of them like the organisms which he had described and the *Globigerinæ* living on the sea-bottom, and the other siliceous remains of organisms living near the surface, and which only reached the bottom after they died, for their skeletons had sunk down through the great depth of sea-water and mixed with the living creatures at the bottom. He looked upon those two results as now definitely acquired to science. He might remark, perhaps, in reference to something which was let fall by Captain Osborn, that, as far as he had been able to examine the deep-sea soundings from the Arabian Gulf, the character of the bottom was, in the main, very similar to that of the great Atlantic plateau. Over most parts of it the sticky, adhesive *Globigerina* mud exists in large proportion, and in certain parts *Globigerinæ* are replaced by an excessively fine and attenuated sand. But in all the specimens which had been brought up by Lieut. Chimmo, there was an entire absence of every thing but the very finest and softest calcareous or siliceous matter."

Highest Peaks of the Caucasus.

Readers of a recent article on the Caucasus, in this Journal, by Capt. v. Koschkull, may be interested in having their attention called to a capital map of that mountain range, which is given in Petermann's Mittheilungen, ii, 1869. The accompanying notes give the following as the elevations of the four most important peaks of the proper or Great Caucasus:

Elbruz,	18,572 Eng. feet,	17,426 Paris feet.
Koschan-tau,	17,123 "	16,066 "
Dych-tau,	16,928 "	15,883 "
Kasbek,	16,546 "	15,525 "

The Ararat is almost equally high with the Dych-tau, viz: 16,916 Eng. feet, or 15,872 Paris feet.

GEODETIC MEASUREMENTS IN EUROPE.

There is a good prospect that at an early day, the measurements of an arc of meridian, undertaken by the Russian government, will be extended into the Turkish dominions, and possibly into the island of Crete. If the project is carried out, an arc of 35° 35′ will have been measured, extending from 35° 5′ to 70° 40′ N. lat., the utmost possible in Euıope. A measurement of the 52d parallel, between Valentia on the Irish Coast and Orsk on the Kirgisen Steppe, has lately been completed.—*Petermann*, ii, 1869.

ART. XXXVIII.—*The Cohahuila Meteoric Irons of* 1868, *Mexico;* by J. LAWRENCE SMITH, Louisville, Ky.

THE region of Mexico bordering on Texas seems to have been most profusely furnished with these celestial visitors. In 1854 I first drew the attention of the scientific public to the meteoric irons of this region, at which time I described one brought from there by Lieut. Gouch, referring at the same time to one mentioned by Mr. Weidner near the southwestern edge of the Balsin de Mapini, on the route to the mines of Panal, weighing not less than one ton ; also to another mentioned by Dr. Berlandier, in his journal of the commission of limits, that at the Hacienda of Venagas there was (1827) a piece of iron that would make a cylinder one yard in length with a diameter of ten inches. It was said to have been from the mountains near the Hacienda (see my article on the subject, this Journal, 1854). In the description there given it was stated that the specimen examined came from 60 miles north of Santa Rosa, and therefore in one or two collections in which it is to be found it is called incorrectly *Santa Rosa meteorite.* I was allowed to cut off but a small piece of it from the original specimen, which is in the Smithsonian Institution, and consequently I was able to supply but two or three specimens. For the discovery and collection of the specimens now under consideration we are indebted to Dr. H. B. Butcher, and I will give a full detail of the discovery as communicated in letters to his father by Dr. Butcher, to whom the scientific world are certainly indebted for the labor, expense, and danger incurred in procuring them. I must not, however, fail to state that I am indebted to Dr. Feuchtwanger for first informing me of the fact of their arrival in this country, and for the exhibition of a small fragment to the members of the American Scientific Association at Chicago in 1868.

In a letter dated September 8th, 1868, Dr. Butcher writes, from information received from the son of Dr. Long who had resided many years at Santa Rosa, that in the fall of the year 1837, there appeared over the town a most brilliant meteor, having a northwest direction. He describes it as most beautiful, lighting up the whole horizon, with a trail of brilliant light following in its progress. Shortly after its disappearance among the distant mountains, they heard a rumbling sound, immediately followed by a tremendous explosion.

From the report he thought it fell and exploded as it reached the earth, somewhere between Santa Rosa and the mountains, a distance of some thirty-five miles, and the next day he started with friends to examine the route, hoping to find it. After two days severe and rough riding they abandoned the search, and returned to town. Shortly afterward, an Indian brought a piece weighing 10 or 12 lbs. into Santa Rosa, supposing it to be silver, having found it some 90 miles northwest of the town, being in the same direction in which Dr. Long and his friends had been exploring, the doctor having been deceived as to distance, he only going to the base of the mountain, instead of crossing it and then following the valley for some 40 miles farther where I think his search would have been a success.

Dr. Butcher now undertook the search, after which he writes: "I have returned fully successful, and am making preparations to send on the iron. In making my arrangements, I hired eight Mexicans and two Indians as guides, and started into the mountains in a N. W. direction, the same as taken by Dr. Long, and found the iron about 90 miles from Santa Rosa. As no vehicle could go into the mountains by the route we entered, I spent two days in exploring a new road, whereby the ox teams could bring them out, and get them to Santa Rosa. They consist of eight pieces, varying from 290 lbs., which is the smallest, to 654 lbs., which is the largest, making a total of nearly 4000 lbs. Before the explosion, the weight must have been much greater, as it is not probable that I have secured the whole, and we know some was taken away by the Indians, who thought they found large masses of silver, and carried their specimens to Santa Rosa. It appears there is on record a statement of the meteor having passed over the city in 1837, and one of my guides relates as a fact, that at that time (1837) a Lepan Indian was riding one of their small ponies through the valley, when his stirrup struck against one of the masses, causing a ringing sound like silver. He dismounted, and was confirmed in his opinion of silver, and took away a piece 10 or 12 lbs. in weight, which he carried to Santa Rosa to sell. I have received from various sources, information relative to this meteor, and all confirm me in the opinion that the autumn of 1837 is about the time of its fall. My party were in considerable danger while in the mountains, as we were encamped two miles from the regular trail, when some 300 Indians went through with a large number of their stolen horses."

Whether or not the time above specified is that of the fall of one or more of these irons, is a matter of little moment; the probabilities are, however, strongly in favor of it; nevertheless, it forms one of the most interesting groupings of meteoric irons known in any part of the world; especially, as the masses are solid and compact masses, and not fragile and half stony, as the Atacama iron, that may have been broken artificially after its fall, and the fragments scattered by Indians and explorers in search of silver. Each one of these masses merits a separate examination, which I hope to be able to give, sooner or later, to satisfy my mind on one or two points connected with their common physical structure and chemical composition. But I will not delay this paper until then.

Six of these masses have been brought to this country, weighing respectively 290, 430, 438, 550, 580 and 654 lbs. They are irregular compact masses, without any evidence of stony minerals. They belong to the softer irons, not very difficult to cut with the saw; as yet there has been but about one ounce detached from one of the masses, which has enabled me to make out the following description:

Specific gravity 7·692. It contains—

Iron,	92·95
Nickel,	6·62
Cobalt,	·48
Phosphorus,	·02
Copper,	very minute quantity.

This composition differs somewhat from the meteoric iron called Santa Rosa; but since examining that I have reason to believe that the quantity of nickel given is too small, some portion of it having remained with the iron; it being far more difficult than is usually supposed to separate accurately minute quantities of nickel from iron. Future examinations may prove that the Santa Rosa belongs to the group of irons under notice.

ART. XXXIX.—*Atomic Ratio*; by JOSIAH P. COOKE, Jr.

THE so-called *oxygen ratio*, used by mineralogists, when interpreted by the new chemical philosophy, is simply the ratio between the total quantivalences of the several classes of radicals, which enter into the composition of a mineral. Indeed its whole value as a specific character in mineralogy depends upon the fact that it expresses this fundamental relation between the different atoms, which are associated in the molecules of the compound, and we propose therefore to call it the *Atomic Ratio.*

The possible hydrates of silicon may be represented by the general formula

$$n(H_4\equiv O_4\equiv Si)-mH_2O=H_{4n-2m}O_{4n-2m}Si_nO_m,$$

and every silicate may be regarded as derived from the corresponding hydrate by replacing the hydrogen atoms to a greater or less extent. Thus the composition of garnet is proven best by*

$$\overset{ii}{R}_3, [\overset{vi}{R}_2]\,{}^{xii}O_{12}{}^{xii}\overset{iv}{Si}_3$$

in which $\overset{ii}{R}$ may be either Ca, Mg, Fe, Mn or Cr, and $[\overset{vi}{R}_2]$ either $[Al_2]$, $[Fe_2]$ or $[Cr_2]$. Garnets have been analyzed, in which these several radicals are mixed together in every conceivable way consistent with this general formula, to which they all conform. This formula expresses all that is constant so far as the composition of the mineral is concerned, and the constant element is merely a definite ratio between the quantivalences, or atomicities, of the several classes of radicals taken collectively. In the last analysis this ratio is the one specific character, which distinguishes many mineral species, and hence its importance in the science of mineralogy.

When the general formula of a mineral is given we can easily calculate the atomic ratio. We have simply to *multiply the number of atoms of each radical by its quantivalence and find the simplest ratio between these products*, and this rule holds in whatever form the symbol may be written. Thus the

* The system of notation here used is explained at length in the author's work on Chemical Philosophy recently published. The main feature of the system consists in writing the symbols in a linear form and separating by commas the several radicals, which, although united to the same central or determinant atom, are otherwise independent of each other. Figures below the symbols indicate independent atoms, except when the symbol is enclosed in brackets. These show that the atoms are united among themselves to form a compound radical and that in consequence two or more of their affinities are closed. Dashes are used to point out the directions in which the several affinities of the principal radicals are exerted, but when the number of dashes required becomes inconveniently large they are indicated by Roman numerals. The Roman numerals above the symbols indicate as usual the quantivalence of the radicals.

atomic ratio of garnet is 6 : 6 : 12 or 1 : 1 : 2. So in like manner the ratio for orthoclase is 2 : 6 : 24 or 1 : 3 : 12. On the other hand from the ratio we can as easily construct the symbol. For example the ratio in the case of anorthite is 1 : 3 : 4. By doubling this, (2 : 6 : 8) we make the first term divisible by 2, the second by 6, and the third by 4 the quantivalences of the several radicals associated in the mineral. Thus we have the skeleton as it were of the mineral $\overset{ii}{R}$, $[\overset{vi}{R_2}]$, $\overset{iv}{Si_2}$ and we now easily add the number of oxygen atoms required to "close" the molecular group, which gives us for the full symbol $\overset{ii}{R}$, $[\overset{vi}{R_2}]^{viii}O_8{}^{viii}Si_2$. In like manner from the ratio 1:3:6 we first deduce the number of atoms of the three radicals, namely $\overset{ii}{R}$, $[\overset{vi}{R_2}]$, Si_3 and then we add, besides the eight atoms of oxygen required to unite the basic radicals to the atoms of silicon, also two more to "close" the molecule.

The atomic ratio is easily deduced from the results of analysis by simply extending the usual method for finding the symbol of a body, whose molecular weight is unknown. We assume that the molecular weight is 100, and having divided in the usual way the per cent of each radical by its atomic weight we *multiply the several quotients by the quantivalence of the respective radicals.* Lastly we add together these products for each class of replacing radicals, and compare the several sums thus obtained. For example Moberg's analysis of the Bohemian pyrope gave the following results.

Si	19·30	or	SiO_2	41·35
$[Al_2]$	11·92		Al_2O_3	22·35
Fe	7·73		FeO	9·94
Mn	2·01		MnO	2·59
Mg	9·00		MgO	15·00
Ca	3·77		CaO	5·29
Cr	3·19		CrO	4·17
O	43·77			
	100·69			100·69

Dividing now each per cent by the atomic weight of the radical and multiplying by its quantivalence we obtain the following numbers :

Si	(19·30 ÷ 28) × 4 = 2·76	2·76
$[Al_2]$	(11·92 ÷ 54.8) × 6 = 1·31	1·31
Fe	(7·73 ÷ 56 × 2 = 0·27	
Mn	(2·01 ÷ 55 × 2 = 0·07	
Mg	(9·00 ÷ 24 × 2 = 0·75	
Ca	(3·77 ÷ 40 × 2 = 0·19	
Cr	(3·19 ÷ 52·2) × 2 = 0·12	1·40

whence we deduce the ratio

$$1{\cdot}40 : 1{\cdot}31 : 2{\cdot}76 \text{ or } 1 : 1 : 2 \text{ nearly.}$$

It is usual in works on mineralogy to present the results of analysis on the old dualistic plan, as if the mineral were formed by the union of various basic anhydrids with silica. Starting with such data it is not, however, necessary to calculate the per cent of each radical in the assumed anhydrids before applying the above rule, because, obviously, by dividing the per cent of each anhydrid by its molecular weight we shall obtain the same quotients as before. For example, in the analysis of garnet cited above, where the data are given on one side in the usual form, we have

$$\overset{28}{Si} : \overset{60}{SiO_2} = 19{\cdot}30 : 41{\cdot}35 \text{ or } 19{\cdot}30 \div 28 = 41{\cdot}35 \div 60$$

and so for each of the other values.

In the symbols of the silicates as formerly written on the dualistic theory the atoms of oxygen were necessarily apportioned among the different radicals in proportion to their quantivalence, although this fundamental distinction between them was itself overlooked. Thus the general symbol of garnet would be written dualistically thus :

$$3RO,\ R_2O_3,\ 3SiO_2$$

and it is evident that the number of oxygen atoms is in each case a measure of the relative quantivalences of the radicals, with which they are associated. Hence the atomic ratio might also be found by comparing together the quantities of oxygen, which the several assumed anhydrids contain, and this is the manner, in which the calculation has generally been made hitherto. Hence also the reason that the atomic ratio has been called the oxygen ratio and was long used in mineralogy before its true meaning was understood. But although the old method gives the same results as the new, it is not in harmony with our modern theories and is practically less simple. Moreover, the principle is far more general than the old method would imply and may be used with all classes of compounds as well as with those, in which the radicals are cemented together by oxygen. Furthermore it is frequently useful to compare the atomic ratios of the complex radicals which may be assumed to exist in different minerals, and interesting relations may frequently be discovered in this way, which the old method would entirely overlook. Thus for example the symbols of the more important feldspars, clays and zeolites may be written in the following form :

Feldspars.

Anorthite,	Triclinic,	$Ca, [Al_2]^{viii}O_8{}^{viii}Si_2$
Labradorite,	"	$[Na_2, Ca], [Al_2]^{viii}O_8{}^{viii}Si_3O_2$
Leucite,	Isometric,	$K_2, [Al_2]^{viii}O_8{}^{viii}Si_4O_4$
Oligoclase,	Triclinic,	$[Ca, Na_2], [Al_2]^{viii}O_8{}^{viii}Si_5O_6$
Albite,	"	$Na_2, [Al_2]^{viii}O_8{}^{viii}Si_6O_8$
Orthoclase,	Monoclinic,	$K_2, [Al_2]^{viii}O_8{}^{viii}Si_6O_8$

Clays.

Kaolinite,	Orthorhombic,	$H_2, [Al_2]^{viii}O_8{}^{viii}Si_2.\ H_2O$
Halloysite,	Massive,	$H_2, [Al_2]^{viii}O_8{}^{viii}Si_2.\ 2H_2O$
Pyrophyllite,	Orthorhombic,	$H_2, [Al_2]^{viii}O_8{}^{viii}Si_3O_2$
Agalmatolite,	Massive,	$H_2, [Al_2]^{viii}O_8{}^{viii}Si_4O_4$

Zeolites.

Thomsonite,	Orthorhombic,	$[Na_2, Ca], [Al_2]^{viii}O_8{}^{viii}Si_2.\ 2\frac{1}{2}H_2O$
Natrolite,	"	$Na_2, [Al_2]^{viii}O_8{}^{viii}Si_3O_2.\ 2H_2O$
Scolecite,	Monoclinic,	$Ca, [Al_2]^{viii}O_8{}^{viii}Si_3O_2.\ 3H_2O$
Analcime,	Isometric,	$Na_2, [Al_2]^{viii}O_8{}^{viii}Si_4O_4.\ 2H_2O$
Chabazite,	Hexagonal,	$Ca, [Al_2]^{viii}O_8{}^{viii}Si_4O_4.\ 6H_2O$
Harmatome,	Orthorhombic,	$Ba, [Al_2]^{viii}O_8{}^{viii}Si_5O_6.\ 5H_2O$
Heulandite,	Monoclinic,	$Ca, [Al_2]^{viii}O_8{}^{viii}Si_6O_8.\ 5H_2O$
Stilbite,	Orthorhombic,	$Ca, [Al_2]^{viii}O_8{}^{viii}Si_6O_8.\ 6H_2O$

And it will be seen that the atomic ratio of the basic radicals to the various complex acid radicals is the same for all these minerals, although the ratios of the basic radicals to the silicon alone are quite different. The last, however, vary according to a simple law and indicate a still further relation between the several species. These relations may be discovered by a simple inspection of the symbols, but they will be rendered much more striking by tabulating the atomic ratios deduced as above.

Lastly, the atomic ratios express the results of analysis in the simplest possible terms and in a form, in which they can be most readily compared in various combinations, and such combinations bring out unexpected results. Thus the atomic ratio of zircon $Zr \equiv O_4 \equiv Si\ (4:4)$ is the same as that of garnet, when the sum of all the basic radicals is compared with the silicon, and the two minerals although belonging to different systems have nevertheless very nearly the same crystalline form. Again, zircon is isomorphous with tin-stone and rutile. Now the total quantivalence of all the radicals both basic and acid in each of these minerals (taking the symbols as they are usually written) is a multiple of four and there are reasons for believing that the molecules of the three, last named, are more condensed than the common symbols would indicate. It is therefore possible that all four may have a similar constitution as is shown in the following table, and the important class of minerals of which spinel is the type may be included in the same scheme.

Tin-stone	SnO_2	or	$Sn_3 \overset{xii}{} O_{12} \overset{xii}{} Sn_3$
Rutile	TiO_2	or	$Ti_3 \overset{xii}{} O_{12} \overset{xii}{} Ti_3$
Zircon	$Zr \equiv O_4 \equiv Li$	or	$Zr_3 \overset{xii}{} O_{12} \overset{xii}{} Si_3$
Garnet			$R_3, [R_2] \overset{xii}{} O_{12} \overset{xii}{} Si_3$
Spinel	$R, [R_2]^{viii} O_4$	or	$R_3, [R_2]^{xii} O_{12}{}^{xii} [R_2]_2$

Such facts as these do not of course prove anything. They are however suggestive, and are adduced because they illustrate the use that may be made of this principle of atomic ratios.

ART. XL.—*Notice of some New Reptilian Remains from the Cretaceous of Brazil;* by Prof. O. C. MARSH, of Yale College.

THE only account of vertebrate fossils from the fresh-water cretaceous deposit near Bahia, Brazil, which appears to have been published hitherto, is a short notice in a paper by Mr. S. Allport, in the Journal of the Geological Society of London for 1860. In this article the author gives a description of the locality, and figures several specimens of reptilian and fish remains, but with no explanation of them except a reference to the opinions of Prof. Owen, and Sir Philip Egerton, as to their general affinities.

While engaged in a geological exploration of the coast of Brazil, in 1867, Prof. C. F. Hartt, of Cornell University, visited the same locality, and among the fossils obtained was a small collection of vertebrate remains, supposed to be mainly reptilian, which he has recently submitted to the writer for examination and description. Most of the specimens are too imperfect to admit of accurate determination, but some, however, are sufficiently well preserved to show clearly their main characters, and a number of them prove to be identical with those obtained by Mr. Allport. Several of the specimens were found on examination to be portions of large fishes, in part referable to the genus *Lepidotus*, and some of them indicating apparently a new type. These will be described, with other fossils from Brazil, in a work on the geology of that region, soon to be published by Prof. Hartt.

The most interesting of the reptilian remains collected by Prof. Hartt in the Bahia deposit is the tooth of a large Crocodilian, from the arenaceous shale near Plantaforma station, on the Bahia railroad. This specimen is in an excellent state of preservation, and indicates a species new to science. It is larger, more slender, and more pointed, than the teeth of existing crocodiles, resembling most nearly those of some extinct American species. It is conical in form, round at the base, and slightly compressed at the apex. The crown is two inches and three lines in length, along the outer side, and ten lines in diameter at the base. One edge is somewhat more convex than the other, and this is also true of one of the sides, and hence the tooth appears slightly curved in two directions. On either edge of the crown there is a sharp ridge, most prominent near the apex, over which it passes, but gradually disappearing before reaching the base, resembling in this respect the teeth of *Thoracosaurus*, from which, however, this specimen differs in being longer, and less curved than the teeth of that genus usually are. The sides of the crown are covered with fine, interrupted, undulating striæ, which appear to be different from the dental sculpture of the Crocodilia hitherto described. These striæ are most distinct near the middle of the tooth, becoming much more delicate at the base, and nearly obliterated at the apex.

In size and general appearance, this specimen resembles somewhat the teeth of *Crocodilus antiquus* Leidy, from the Miocene of Virginia, but differs from that species in being less tapering, and in having the ridge on the edges extend farther downward. It resembles still more closely the teeth of a new species of Crocodile discovered by the writer at Squankum, N. J., in the tertiary green-sand, which will soon be more fully described under the name *Thecocampsa Squankensis* Marsh. Both species have essentially the same proportions, and similar dental striæ, but the cutting ridge of the New Jersey specimens is more prominent, and extends nearly or quite to the base of the crown. The two species were apparently about the same size, both being considerably larger than existing Crocodilians.

Other parts of the skeleton of the Brazilian species would perhaps show generic characters to distinguish it from the modern procœlian Crocodiles, but in the absence of these, it may for the present be placed in the same genus. Its form, cutting edges, and especially its peculiar striæ, readily distinguish it from any species with which it is liable to be confounded, and it may appropriately be named *Crocodilus Hartti*, in honor of its discoverer, whose recent researches have thrown so much light on the geology of Brazil.

Several specimens of reptilian teeth collected by Mr. Allport at Montserrate, a locality in the same deposit about two miles southwest of Plantaforma station, evidently belong to this species, as the illustrations accompanying his paper (Plate XVI, figures 1, 2, 3, and 5) clearly indicate. The explanation of the plate refers to the specimens as, "Teeth of Crocodile with delicately wrinkled surface," but no further description is given.

In the same paper Mr. Allport has given figures of several

Crocodilian teeth from the localities at Plantaforma and Montserrate, which are quite different from those above described. These are represented in Plate xv, figure 5, and Plate xvi, figures 4, 6, 7, and 8, and are referred to on page 268 as, "Teeth of Crocodile with strong continuous striæ, and coarse riblets." These specimens, taken in connection with some imperfect remains in the collection made by Prof. Hartt, indicate the existence in this deposit of a second, and smaller species of Crocodile, probably allied to the modern gavials. The teeth are not so large as those of *Crocodilus Hartti*, and are more tapering, and more curved. They also differ widely in the striæ and lateral folds. These specimens may provisionally be referred to the genus *Thoracosaurus*, and, as the species is evidently new, it may be called *T. Bahiensis.*

An interesting fossil, found by Prof. Hartt at Plantaforma station, is a fragment of a bone, evidently reptilian, but the exact affinities of which it is difficult to determine from this specimen alone. It resembles in some respects the extremity of an ulna, but after a careful comparison the writer is inclined to consider it the proximal end of a rib. It is much flattened at the articular extremity, and tapers gradually to the broken end, which is somewhat triangular in outline. Its length is about four inches, the transverse diameter of the perfect end two and a half inches, and of the other, one and a quarter inches. The larger extremity is divided into two articular facets lying oblique to each other, the smaller one being elevated about half an inch above the other, and covering rather more than a third of the entire terminal surface. In form and general proportions this specimen is not unlike the upper end of a right dorsal rib of some of the amphicœlian Crocodiles, especially a rib in which the head and tubercle have so closely approached each other that their articular surfaces are nearly confluent. The size and other characters of the specimen, however, seem to exclude it from that order, and it probably belonged to a Dinosaurian reptile, possibly the same as a large vertebra from Montserrate, which Mr. Allport figured in his paper in Plate xvii, and which Prof. Owen suggested might prove to be allied to *Megalosaurus.*

The only other specimen in this collection that need be particularly mentioned here is a small flat bone, about two inches in length, with one articular extremity partially preserved. This appears to resemble most nearly the fibula of a Tortoise, and probably should be referred to that group of reptiles. The other vertebrate remains from Brazil obtained by Prof. Hartt are, in general, of less interest, but will be fully described in his forthcoming work.

Yale College, April 5th, 1869.

ART. XLI.—*Notices of papers in Physiological Chemistry—* No. II ; by GEORGE F. BARKER, M.D.

5. *On the formation of Sugar in the Liver.*

(Continued from page 270.)

(57.) On the 19th of December, COLIN presented a memoir on the relation of animal glycogeny to the production and destruction of fats.* His researches were extended to include the influence of this function on the composition of the chyle, the lymph and the blood, as well as the liver-tissue. With regard to the latter, he concludes as follows: (1) The sugar of the liver, at least in part, is manifestly derived from the saccharine or starchy food taken by the herbivora, or from those elements in a mixed diet, being carried to this organ by the portal vein and the hepatic artery. (2) This sugar appears to be also a product of the transformation of the fatty matters which accumulate in the hepatic cellules and in the intra-cellular spaces. (3) It appears in much larger proportion in fat animals than in lean ones; though the quantity continues to increase only up to a certain limit, and even diminishes very notably in livers which have themselves undergone fatty degeneration. (4) In the case of animals who have no fatty tissues to be absorbed, the amount of sugar diminishes from the first moments of abstinence, and soon entirely disappears. (5) When the animal is fat, on the contrary the sugar is constantly renewed and so maintained in considerable amount, even when food is withheld, if only the normal temperature of the body be preserved.

(58.) On the 2d of February, 1860, G. HARLEY communicated to the Royal Society† the results of some experiments made in connection with Dr. Sharpey at University College, London, to test the truth of Pavy's views. The first step was to ascertain the presence of sugar in the blood under favorable conditions of diet. Three-fourths of an ounce of the carotid blood of a terrier dog, killed three hours after a diet of bread, milk, and boiled liver, was added to four times as much water, to which a few drops of acetic acid had been added, and which was kept boiling in a capsule; when the albumin was completely coagulated, the liquid was filtered and tested with potassic hydrate, both with and without cupric sulphate; the reaction for sugar was obtained. A second portion of blood after standing 35 minutes gave a similar result. To obtain an animal whose condition was normal, a dog which had been running at large was taken for a second experiment, and to avoid the

* C. R., xlix, 981. † Proc. Roy. Soc., x, 289.

possibility of any change in the blood after removal, a canula with a stopcock was inserted into the carotid artery of the living animal, and the blood as drawn was allowed to flow directly into the boiling acidulated water. This blood on being treated as above, afforded sugar: as also did another specimen after standing 24 hours. For a third experiment, a good-sized dog, fed for four days solely on flesh, was given half a pound of boiled horse-flesh; three hours afterward 1½ ounces of blood were drawn from the femoral artery directly into the boiling mixture. This, as well as another portion of blood which had stood 3 hours, was found to contain sugar; and so far as the eye could judge, the amount in each case was equal. Previous experiments having shown that the sugar in arterial blood varied from 0 to 0·24 per cent, Harley considered it unnecessary to estimate it quantitatively in these experiments. The next question is whether glycogen is normally transformed into sugar. For this purpose a fourth experiment was made on a dog, fed on animal food solely for 14 days, and killed four hours after a meal of boiled horse-flesh, by section of the medulla. The abdomen was at once opened, one portion of the liver sliced off and placed in ice and salt, a second portion rinsed, macerated, and examined for sugar, with a positive result. After half an hour the frozen fragment was, without thawing, sliced directly into boiling water containing a few drops of acetic acid. On treatment, as much sugar as before, apparently, was obtained. A third piece of the liver, which had remained for 40 minutes in the abdomen, was much more strongly saccharine. Fifth, a dog fed for ten days on boiled tripe, was pithed 22 hours after eating, and in less than 20 seconds a portion of the liver was in the freezing mixture. A second portion was at once examined for sugar, together with some portal and hepatic blood collected at the same time. While the portal blood contained no sugar, the hepatic blood, and both portions of the liver yielded it; nor did portal blood furnish sugar either after standing all night, or after being mixed with saliva; though the amount in both decoctions of the liver was increased by this treatment. Sixth, a large and vigorous dog, after fasting 72 hours, was pithed, and a portion of the liver placed in ice and salt. Blood was collected from the portal vein, the liver where sliced, the right side of the heart, the aorta, and the inferior vena cava. Except that from the liver, none of these portions contained sugar. That from the right side of the heart was doubtful because most of the hepatic blood escaped from the cut surface, and also because the supply of blood to the liver was stopped by ligation of the portal vein. None of these specimens, moreover, except that from the liver, afforded any glycogen. The frozen portion of the liver, examined 3 hours afterward, yielded sugar. The seventh experiment was quantitative; a dog, previously fed on meat, received a full meal of bread and milk, and five hours afterward was pithed. A portion of the liver was at once sliced off and placed in a freezing mixture. Some portal blood collected at the same time afforded a small amount of sugar, derived evidently from the food. Two hours afterward a portion of the frozen liver, and a portion of that which had remained warm in the body of the animal, were weighed, and the sugar in each was determined volumetrically. The frozen piece of the liver contained 0·333 per cent of sugar; the other portion afforded 1·55 per cent; showing a five fold increase in 2 hours. But 0·333 per cent is no inconsiderable quantity, since in a human liver weighing 50 ounces, it would amount to seventy grains of sugar. He concludes as follows: 1st, Sugar is a normal constituent of the blood of the general circulation; 2d, Portal blood contains sugar, when the diet is mixed; 3d, On an animal diet, as also when fasting, portal blood is devoid of sugar; 4th, Whatever the diet, the liver of dogs always contains sugar; 5th, Under favorable conditions, sugar exists in the liver of animals after three days fasting; 6th, this sugar comes partly from the food, partly from the liver, when the food is mixed; 7th, The liver of animals on a meat diet, has the power of forming glycogen, which is, at least partly, transformed into sugar;—it may however be transformed into other matters also; 8th, Since sugar is found in the liver at the moment of death, it cannot be viewed as the result of a post-mortem change, but has an origin strictly physiological.

(59.) In May, R. McDonnell published* some experiments instituted to ascertain whether the liver converted the amyloid substance into sugar normally during life. For this purpose, he removed blood from the right side of the heart in the living animal, and examined it for sugar. The results were: (1.) In twelve experiments with dogs fed on meat for some weeks previous, traces of sugar were found in the blood of five; none could be detected in the blood of the remaining seven; (2.) In four rabbits, fed on boiled eggs, meat, and butter, for some days, no sugar was detected in the blood drawn from the right side of the heart; (3.) In three dogs, fed on mixed diet, and three rabbits, fed on carrots, potatoes, etc., sugar was found in the blood of the right side of the heart, and in equal quantity in blood from the carotid; (4.) In three rabbits, fed on vegetables, sugar was found in the blood removed from the right side of the heart during life; but double, and in one case more than treble the amount, was found in blood thus taken

* Dublin Hosp. Gaz., May 15, 1860; Am. J. Med. Sci., II, xliii, 214, 1862.

after the animals were killed. Hence, McDonnell deems the conclusion justifiable, that the blood of animals living on vegetable food is normally saccharine ; but that the liver does not transform its amyloid substance into sugar, and pour it out into the blood of the hepatic vessels.

(60.) On the 21st of June, PAVY presented a paper supplementary to his former one, to the Royal Society.* In it he shows that blood collected from the right side of the heart after death affords an abundant indication of sugar, while if removed by catheterism during life it contains but a trace. Hence all inferences as to the ante-mortem state, drawn from post-mortem observations, are erroneous. He proves, moreover, that the heart excised instantly after killing, contains blood as free from sugar as during life ; and demonstrates that very slight causes produce in the living animal, a considerable amount of sugar in the circulation ; simply by interfering with the breathing, a strongly marked diabetic state of the urine may be induced within an hour. To obtain, therefore, a fair specimen of blood, even by catheterism, the animal must remain perfectly tranquil during the operation. Pavy further asserts that the blood found in the right side of the heart is not more saccharine than portal blood ; that the saccharine condition of the liver, which has been hitherto regarded as a normal one, is in fact the result of a post-mortem change which takes place with an astonishing rapidity ; that the true function of the liver is to form the glycogenic substance—which the author calls *hepatine* ;—that the real function of this substance is yet an open question ; that though it may be transformed into sugar by any ferment, yet that normally, it is not so changed during life ; that abnormal states of the circulation and probably of the blood, as well as certain conditions of the nervous system, cause the production of sugar ; that after section of the spinal cord just below the phrenics, the temperature of the body falls, and the transformation of hepatine becomes so slow that the true physiological process is at once defined ; that this fact may readily be experimentally demonstrated in the livers of animals which have naturally a low temperature, as the frog, the oyster and the mussel ; that the use of starchy and saccharine food increase the amount of hepatine in the liver and hence increases the size of this organ ; that his previous experiments on dogs are confirmed by new ones made on rabbits, thus proving the conversion of sugar into hepatine ; that it is highly improbable that sugar is changed into hepatine merely to be changed back again by the same organ ; that one and a half parts of hepa-

* Proc. Roy. Soc., x, 528.

tine yield in the liver after death, one part of sugar; that hepatine and sugar differ widely in osmotic power, a fact which accounts for the retention of the former in the hepatic cells; that the liver becomes strongly saccharine on ligating the portal vein; that then the blood also gives the sugar-reaction, and in one case it was found in the urine; and that the introduction into the circulation of sodic carbonate prevents the diabetic state ordinarily induced by injury of the sympathetic system.

(61.) In a paper read August 6th,* De Luca gives the results of his examination of the liver of a person who had died from cerebral congestion, whose pancreas was partially atrophied. He found 1st, that this liver contained saccharine matter capable of reducing copper-tests and of fermenting with yeast; 2d, that when washed free from sugar and allowed to stand, a new quantity was formed, thus proving the presence of glycogenic matter; 3d, that when thus washed, glycogenic matter not capable of reducing copper tests or of fermenting, could be prepared from it; and 4th, that this same whitish substance boiled with hydrochloric acid for a few minutes reduced the tests, fermented and yielded a crystallized compound with sodic chlorid. DeLuca concludes therefore, that the glycogenic function of the liver was not at all modified by the disease of the pancreas.

(62.) On the 5th of November, Colin presented a memoir to the French Academy† on the relation of the production of sugar to the re-absorption of fats, and also to the animal heat during abstinence and hibernation. The following are his conclusions: 1st, The re-absorption and combustion of fats, the production of sugar, and the maintenance of the animal heat to its ordinary degree, are phenomena intimately connected and mutually dependent. 2d, Abstinence cannot long be supported in lean animals; it produces a very rapid lowering of the temperature which is coincident with the almost complete disappearance of sugar in the liver, the blood, the lymph, and other liquids normally saccharine. 3d, With fat animals or those moderately so, the duration of the abstinence—all the other conditions being the same—appears to be exactly proportional to the quantity of fatty matter held in reserve in the tissues; so long as this fat exists in the animal, its life is sustained, the sugar is renewed in the liver as well as in the other nutritive fluids, and the temperature of the body is not notably lowered. 4th, During hibernation, the activity of the sugar-production is proportional to the re-absorption of fat. 5th, Finally, in all animals deprived of

* C. R., li, 217. † C. R., li, 684.

food, the liver experiences some remarkable changes; it becomes partially atrophied, and its cellules lose their fat, which is replaced by sugar.

(63.) In May, 1861, GORUP BESANEZ published a method for the preparation of glycogen.* His colleague Gerlach, in washing the liver of a child two years old by injecting water into the portal vein preparatory to a permanent injection, noticed that the escaping water which at first was bloody and dark colored, grew brighter and finally became milky, though it never became transparent and clear. On standing, this milky fluid deposited a flocculent coagulum, leaving the liquid above strongly opalescent. A similar result was obtained from the liver of an adult three to six days after death. On examination Gorup Besanez found the opalescence to be due to glycogen; and he gives the following method for preparing it pure: The opalescent liquid from the liver is freed from albuminates by acidulating with acetic acid, heating rapidly to boiling, and filtering from the coagulum; it is then mixed with twice its volume of 90° alcohol; a bulky precipitate falls which, after standing some hours is collected on a filter, washed out with alcohol, dissolved in water, heated to boiling with a few drops of acetic acid—which causes the separation of a slight granular coagulum—filtered, and mixed with twice its volume of alcohol as before. A snow-white flocculent precipitate of glycogen is thrown down, impure only from the presence of a little fat; freed from this by ether, and dried in vacuo, it forms a brilliant white powder like flour, resembling very strongly starch or inulin. Water dissolves it to a strongly opalescent liquid, not cleared by boiling. The solution is colored wine-red by iodine, but is not precipitated by glacial acetic acid, becoming rather clearer by this treatment; thus agreeing with the observations of Hensen, Scheerer, and Lochner, but in opposition to those of Bernard and Lehmann. The strong opalescence of the liquid prevented any determination of its rotatory power. On analysis, his assistant Klinksieck obtained numbers agreeing perfectly with those of Kekulé, viz: carbon 44·5, hydrogen 6·35, oxygen 49·12=100; leading to the formula $Є_6H_{10}Θ_5$. Since Pelouze deduces the formula $Є_6H_{12}Θ_6$, and Lochner $Є_6H_{12}Θ_6+H_2Θ$, Gorup Besanez suggests that either the method of preparation affects the result, or that there are several carbohydrates in the liver, and hence that different methods of preparation, give different products. At all events the differences in composition and properties are too great to be ascribed to errors of analysis or observation.

* Ann. Chem. Pharm., cxviii, 227.

(*To be concluded.*)

ART. XLII.—*Meteors of November 14th*, 1868; by H. A. NEWTON. (With two Plates.)

IN continuation of the account given in the January number of this Journal (pp. 118–126), I propose to notice certain meteors observed at two or more places, with sufficient exactness to afford a parallax.

The most remarkable meteor of the night appeared at 1^h $16\frac{1}{3}^m$, New Haven mean time. It was observed at New Haven, Poughkeepsie, Palisades, Williamstown, Haverford, Wilkesbarre, and probably at Washington. The discussion of these observations will afford an occasion of explaining a method of computation of the true altitude and path from the observed tracks of a meteor.

At New Haven.—I did not see the body itself, but the bright streak remaining after its disappearance was nearly vertical, and about 2° north of Jupiter. This distance from Jupiter was carefully estimated, by Mr. Harger and myself, and we are both confident that it is within half of a degree of the truth. Mr. Harger thinks the estimate is, if anything, a trifle too great.

No note was made at the time, of the length of the bright cloud. My impression, confirmed by that of Mr. Harger, is, that Jupiter lay about the middle of the persistent streak, also that the streak was not less than five degrees long, and probably 2° or 3° longer than that. From the first it had an appreciable breadth.

At 1^h 15^m 40^s, N. Y. time, the trail had assumed the usual curved form, and I drew a rough sketch of it, and of Jupiter, as in the figure, plate I. The upper part had apparently moved northward, and the lower part southward.

At $19\frac{2}{3}$ minutes past one I drew the second sketch as in the figure. The special object I had in view was to locate its several parts with reference to Jupiter.

At $30\frac{1}{6}$ minutes after one the cloud lay horizontally just above Jupiter and extending south eight degrees from the planet, as estimated at the time. The third sketch, plate I, gives its center an altitude of one degree or more above Jupiter, and an extent northward four degrees from it.

At $44\frac{2}{3}$ minutes past one the cloud, which was growing fainter, was horizontal, and extended 12° southward from Jupiter, as noted at the time. My fourth sketch implies that it extended 3° or 4° north of Jupiter, and had its lower edge just upon the planet at this time.

At fifty-seven minutes past one the cloud had become so faint and confused with the mists of the horizon, that in order to confirm or correct my impression that I still saw it, I asked the party whether it was actually visible. Nearly if not quite all of them (about a dozen persons) asserted that it was still visible. I think that it continued in sight till the clock struck one, if not longer, although other objects prevented us from giving it special attention and recording its appearance.

At Palisades, Fern Lodge Observatory, Rockland Co., N. Y., Mr. W. S. Gilman, Jr., saw the train of the meteor, and made four sketches of its appearance at successive times, together with the following notes.

Time of appearance, $1^h\ 12^m\ 1^s \pm$ N. Y. mean time, 1st magnitude; white color; duration of flight $\frac{1}{2}$ sec. The meteor was seen by Mr. Thomas P. Gilman, who obtained a glimpse of it only. The glare of the meteor was very brilliant; length of path 50° or 60°; direction perpendicular; disappeared near and north of Jupiter; explosion not witnessed. Left train of smoke of a greenish blue color.

At $1^h\ 14^m\ 10^s$ the train was very distinct, but stars were seen through it. The appearance was as in the figure, plate I.

At $1^h\ 18^m\ 10^s$ the train had become still more curved, and the figure represents its appearance as it was seen with the naked eye. The train viewed through a four inch glass, with power 40, appeared to move sluggishly, and to dissipate slowly. It was comparable for color and texture to the appearance of the nebula in Orion in a 12 inch glass.

At $1^h\ 21^m\ 30^s$ the train is still a striking object. It is represented in the figure. Very faint at $1^h\ 24^m\ 30^s$.

At $1^h\ 28^m$ it has still more changed its form, as in the fourth sketch.

Smoke faintly visible at $1^h\ 34^m\ 30^s$. At $1^h\ 39^m$ smoky train very much diffused and very faint. Not watched subsequently.

At Vassar College, Poughkeepsie, N. Y., the meteor was seen by Miss Colby of the Senior Class, and the following report is furnished by Miss Mitchell.

"At $1^h\ 12^m\ 30^s$, a meteor started from the zenith, moved toward the west, burst south of Jupiter, like a rocket, assuming a variety of colors, and disappeared, leaving a train, which at once began to wind, taking first the form of an irregular **s** and then of a **z**, then moving southward toward two stars of Cetus, whirled around completely, broke up into two pieces, the southern portion surrounding the upper star, and both fading in the mists of the horizon. The train was seen for 44 minutes."

At Williamstown, Mass.—The following notes of observations by Messrs. B. I. Gilman, W. D. Granger and F. B. Wilder, are furnished by Mr. W. S. Gilman, Jr.

"At 1^h 15^m the largest meteor of the night fell near Jupiter. The motion was slow and we could have read very easily by its light. The path made an angle of $60° \pm$ with the horizon. The train lasted an almost incredible time, and assumed different shapes." A sketch of the path accompanied the notes.

At Haverford, Pa., at 1^h 9^m 28^s a meteor passed from near the north star in the direction of Cassiopeia vanishing near β Cassiopeia. The light was exceedingly brilliant, and the train continued visible about 44 minutes. The train was at first straight, having a direction nearly parallel to the line joining β and κ Cassiopeiæ, and about 15° in length. Almost immediately the line commenced to spread and the form changed to that of an inverted **s** (or **ƨ**). Then it assumed an elliptic form with a hollow ellipse at each of the foci, and soon afterward the **ƨ** form again. During this time it slowly drifted toward α Andromedæ, being last seen near δ Andromedæ. Then the train had an elliptic form, the transverse axis being nearly parallel to the line joining α and β Andromedæ.

The sketch furnished by Prof. Samuel J. Gummere, who himself saw the meteor, though confined to the house, shows the position of the cloud at the time it assumed the elliptic form above described.

Mr. B. V. Marsh reports that the meteor was seen by Mr. Dennis of Wilkesbarre, Pa., and that it appeared to pass near λ Orionis.

A person in Delaware Co., N. Y., describes it as west of south, and about 20° high.

Undoubtedly the meteor described by Prof. Eastman in "November Meteors of 1868, U. S. Naval Observatory," p. 8, in the following terms, is identical with the one witnessed at New Haven, Palisades, &c.

"At thirty minutes past midnight the most remarkable meteor of the shower appeared near ε Ursæ Majoris, and moved westerly across β Ursæ Minoris. The meteor and its train were colored like the one already described. [At first its color was deep orange or red, afterward changing to green, and finally to light blue. The edges of the train were colored deep orange or red, and the center was at first green, changing to light blue. The colors lasted about one second.] The meteor was much brighter than Jupiter and moved very rapidly.

"After the bright colors of the train had vanished it remained perfectly straight for about one minute when it began to assume slowly the form of a serpent in motion, gradually decreasing the distance between its extremities until it became apparently a mass of light-colored cloud about 7° in length and 4° in breadth.

"The westerly end of this condensed train at one time became divided, but after the lapse of about four minutes the extremities of the separated portions united, leaving a nearly circular aperture 30′ in diameter, 2° from the western end of the train. After this aperture closed up I could distinctly see a small star through the vapory mass. This cloud-like object remained visible, with slight changes in its outlines, for thirty minutes."

The latitudes and longitudes of the several places of observation of this meteor are as follows.

	Latitude.	Longitude.
Williamstown,	42° 45′	73° 15′
Poughkeesie,	41 40 50″	73 53 30″
New Haven,	41 18 33	72 55 25
Wilkesbarre,	41 15	75 54
Palisades,	41 0 30	73 54 7
Haverford,	40 0 36	75 18 16
Washington,	38 53 39	77 2 48

The place of Jupiter was R. A., $0^h\ 19^m\ 37^s$, and North Dec., 0° 27′ 23″. The relative positions of the places are represented in a skeleton map, upon plate II. The corresponding positions of the paths upon the heavens are presented in an adjoining diagram, upon which are also exhibited the radiant, the pole star, and Jupiter.

Several methods of computing the actual position of a track from the observed paths have been published. The most complete is by Bessel n the *Astronomische Nachrichten*, vol. xvi, p. 322. Others are by Brandes, Olbers, Quetelet, Secchi, Newcomb, &c. I have, however, found the following the most convenient method of treating observations. With a sixteen inch globe it is sufficiently accurate for any ordinary data.

First, I rectify the celestial globe for the mean latitude of the places of observation, and for the mean local time of the appearance of the meteor. Then upon the globe I paste narrow strips of paper to represent the several observed paths. In a similar manner are indicated the zeniths of the places, and those points of the celestial sphere which are at the given instant in the production of the several lines joining the places of observation. These last points will, in the case of only two observations, be in the horizon of the rectified globe, and in all cases will be very near that horizon. When the meteor belongs to a group having a well defined radiant, that point, or region, is marked in like manner.

There are certain geometrical conditions which the observations, if correct and complete, must now fulfil. Thus, through two places of observation and any point of the meteor's actual path, as the end, for example, we may pass a plane. This plane, produced, intersects the celestial sphere in a great circle. Upon this great circle should evidently be located the two observed places of extinction of the meteor, and the points where the line joining the places of observation cuts the heavens.

If then we draw a great circle through the two points last named, and through either end of the track as observed at one place, this circle should pass through the corresponding point of the other observed path. Any deviation from this circle is due to some inaccuracy of observation. It is necessary to assume that one track is to be lengthened, or the other shortened, or that one or the other is to be changed in direction, or in position, until the two observed paths satisfy these conditions.

Again, three or more observed paths if correct must, if produced backward, pass through one and the same point on the globe, to wit, that point from which the meteor was moving. This point must, moreover, be above the horizon. Again, if some of the observations by being conformable to the usual radiant, show that the meteor belongs to a group with a well marked direction, it is often safe to assume that the unconformable paths are the ones that are in error.

By recognizing these, and other necessary geometrical conditions, we may criticise and reject observations. It will often be necessary to assume that one or the other observer saw only the latter part of a path, or has mistaken a star of reference, or has erred in recording upon the chart the direction of the meteor.

I am well aware that this method of treating the observations throws special and undesirable responsibility upon the computer. But in any method of observing commonly practised, good observations have large probable errors, amounting often to several degrees. The computer must assume the duty of judging the observations, and be held responsible for its faithful discharge.

The computer is, moreover, now in a condition to use to advantage any special points in the observations which he knows to be well determined, as the passage of the meteor near, or over, a particular star. Or the peculiar position of one of the observers may far more than counterbalance any inaccuracy in the location of the path.

A chart, constructed for the purpose by means of the tables of the Coast Survey, upon which are plotted the locations of the observers, is used in measuring distances and directions whenever it gives sufficient accuracy. The figure on plate II. is a reduction of this chart.

Let us apply these principles to the meteor to which the above observations refer. The time of appearance at New Haven was only approximate, and the other observations indicate $1^h\ 16^m\ 20^s$ as the correct New Haven time. We saw it pass down nearly vertically 2° north of Jupiter.

At Haverford it appeared, as the diagram shows, above and probably to the east of Polaris, and passed a little below (say 3° or 4° below), κ and β Cassiopeiæ. The general direction of its observed paths, the character and colors of the train, and the small number of the unconformable meteors, mark this as an undoubted member of the November group, and authorize us to assume that its direction was from the radiant in Leo. This assumption, and the distance above named from β and κ Cassiopeiæ, imply that the path passed 10° or 12° above Polaris.

The differences of latitude and longitude of New Haven and Haverford are 77″·95, and 142″·83. The latter number multiplied by the cosine of the middle latitude gives the departure, which, divided by the difference of latitude, gives the tangent of the azimuth of the line joining Haverford and New Haven upon the horizon of the point midway between the two places. The secant of this angle, multiplied by the difference of latitude, gives the distance in geographical miles. This, multiplied by 1·15, gives the distance, 153·5 statute miles, with a direction S. 54° 16′ W.

Upon the diagram in plate II, I have marked by *a* and *c* the points 54° 16′ from the south and north points on the horizon. Through *a* and *c* and the point *b* of the heavens (2° north of Jupiter), let there pass a great circle cutting the Haverford track in *d*. If we imagine a triangle, whose angular points are the observers at New Haven and Haverford, and that point of the track which was directly opposite Jupiter, as seen from New Haven, and produce the sides of this triangle, then will the three arcs, *ab*, *bd*, and *dc*, severally measure the three angles of the triangle. We have, then, the angles and one side of a triangle to find the distances of this point of the meteor's path from the two observers.

The arcs of the semicircumference *ac* can be measured on the globe by a movable quadrant, or by bringing the circle *abdc* to coincide with the horizon of the globe. I have, however, found a narrow tape line, graduated for the purpose, very convenient for such measuring.

Knowing the distance of either observer from the particular point of the meteor's path, we measure on the globe the zenith distance of the corresponding observed point, *b* or *d*, and hence readily compute the actual altitude of the meteor, at that instant, above the earth's surface. An allowance for the curvature of the earth is to be added to the height above the horizon of the observer. The azimuth of *b*, or *d*, when measured, gives the means of locating the place over which this point of the meteor's path was vertical.

The arcs, *ab* and *bd*, measured upon the globe, are about 28½° and 57°, which give the distances of the track from New Haven and Haverford, 182, and 87·3 miles, respectively. But, owing to the fact that the arc, *abd*, cuts the Haverford track at a sharp angle, we seek a closer determination of the path by another method.

The position of Jupiter with reference to the track and cloud makes it worth while to compute its altitude and azimuth from New Haven at $1^h\ 16^m\ 20^s$, and $1^h\ 44^m$. These are found to be S. 75° 16′ W., and S. 80° 8′ W., for the azimuth, and 16° 48′, and 11° 43½′ for the altitudes.

Now, the observed path should be in the arc of a great circle which passes through the radiant. By bringing the radiant and the point 2° N. of Jupiter to the horizon of the celestial globe, the zenith of New Haven will be found to be 5° above the horizon. Hence, the meteor passed (though then unseen) 5° S. of our zenith.

Again, the point in the direction S. 77° 16′ W., 182 miles from New Haven on a line elevated 16° 48′, is over latitude 40° 42′, longitude 76° 9′, and 56 miles high. The elevation of the center of the radiant area above the horizon of this point was about 20°. Hence we may carry back the meteor's track with an elevation of 20° till it cuts the meridian of New Haven. We see, thus, that the meteor passed at an elevation of 120 miles and 10·5 miles south of us.

This gives us a good determination of the vertical plane in which the meteor was moving, the accuracy of the determination being due to the nearly vertical path of the meteor as seen at New Haven, and the easy reference of it to Jupiter. But the New Haven observations are not suited to the determination of the altitude of the track in the plane. This may also be said of all the observations at Williamstown, Palisades, and Poughkeepsie.

The distance from Haverford to this vertical plane, measured on the chart in the direction of Wilkesbarre, or N. 20° W., is 55½ miles. The observed altitude in the same direction is 48°, which gives an altitude of 61 miles for the meteor at that point. The distance from Wilkesbarre to the plane is 35½ miles in the direction S. 20° E. An altitude of 61 miles represents an angular altitude of 60°. The star λ Orionis happened to be in this same azimuth, and had an altitude of 57°.

This satisfies well enough the observations, for it is reasonable to assume that "near λ Orionis" means a little above it, since, if the meteor had gone below that star, the reference would have probably been to the brighter stars of the constellation. We may, then, fairly conclude that the meteor passed the vertical plane through Haverford and Wilkesbarre at an altitude of 61 miles. This is 5 miles lower than the previous determination, for the former path is 66 miles high at this point.

The beginning and end of the apparent path of the meteor was not well observed. The middle point of the cloud, in its initial position, as seen at New Haven, was nearly opposite to Jupiter. The line to this point, and the track of the meteor, make an angle of 39°, which is determined by measuring upon the globe the distance from 20° N. of Jupiter to the radiant, and taking the supplement of the result. These lines meet in long. 76° 0′, and lat. 40° 43′, at an elevation of 54 miles, and at a distance from New Haven of 175 miles.

Assuming that the apparent length was 6° at New Haven, we have an actual length of 175 *sin* 6° *cosec* 39°, or 29 miles. This represents an apparent length at Haverford of over 20°, instead of 15° as reported. If these were the only means of determining the length, the Haverford observation should be regarded as the best. But at Washington, the original length must have been appreciably more than 7°, since the train is said to have decreased in length to 7°. Each degree at Washington represents 3·2 miles on the track of the meteor. A length of only 9° for the observed track at Washington would be represented by the 29 miles of the actual path. We shall assume, therefore, that the length of the cloud was, at first, 30 miles, which implies that its eastern end was 59 miles, and its western end 49 miles high.

At what altitude the meteor was first seen, we cannot say. It passed the meridian of Haverford at a height of about 68 miles, but may have been visible before it reached that point. There is no reason to suppose that it passed beyond the western end of the cloud.

The train had an appreciable breadth at once upon the disappearance of the meteor. Probably 20′ would not be a large estimate for the breadth, as seen at New Haven, which implies a real diameter of one mile, and a volume of a dozen or a score of cubic miles. This volume was steadily and rapidly increasing through the whole time that the train remained visible.

Compare now this path with the observations at Washington, Palisades, Poughkeepsie, and Williamstown.

At Washington.—I have assumed that the meteor described by Prof. Eastman was unquestionably the same body as that observed at New Haven and other places. An error of a half hour in recording the time of appearance would make the two coincident in time. The Washington meteor was "the most remarkable meteor of the shower," its train exhibited one of the two remarkable apertures described by Prof. Gummere, its position was very near that in which the one we have described would have been seen, and its train endured for a like unexampled period. Such a meteor as that described by Prof. Eastman would have certainly attracted attention at other stations.

Prof. Eastman says that it appeared "near ε Ursæ Majoris, and moved west across β Ursæ Minoris." This path to be conformable to the radiant in Leo needs a change of direction. Looking from Washington at the path determined above from the Haverford and New Haven observations, the path would be seen crossing the line joining the two stars just mentioned. Only so much change of the path observed is needed as is necessary to make it conformable to the Leo radiant. The assigned path satisfies reasonably the Washington observations.

At Palisades.—The path of the meteor as above determined passes very nearly vertically over Palisades at an elevation of 95 miles. The lower end of the cloud was 22° high and 5° or 6° north of Jupiter. In one of his notes Mr. Gilman says that the track ended near α Piscium, an error, probably for 41 Piscium. The assigned length of path 50° or 60°, would imply a first altitude of 85, or 90 miles. This estimate, however, is not to be taken strictly.

At Poughkeepsie.—The beginning and end of the cloud as before determined would be viewed from Poughkeepsie in the direction S. 56° W., alt. 26°, and S. 59° W., alt. 19°. These are about 18° and 15° south of Jupiter, and a little higher from the horizon. The track would if produced backward pass 25° south of the zenith of Poughkeepsie. A sketch of the position of the track with reference to Jupiter, sent by Miss Mitchell, would imply that the meteor's path was nearer to the planet, than 15° or 18°. But the observations at other places forbid any essential diminution of this distance.

At Williamstown.—As seen from Williamstown the direction and altitude of the two ends of the cloud are S. 43° W., 16°, and S. 46° W., 12°, which makes the path about 30° south of Jupiter, much farther than would be implied by the sketch of the track. The meteor when due south would have been (if visible) 45° high. No essential transfer of the track toward Jupiter can be made, however, without rejecting either the New Haven or the Haverford observations altogether.

The position and direction of the cloud cannot therefore, I think, be reasonably changed from the previous determination.

Its central point may be regarded as 54 miles high, over N. lat. 40° 43′, and W. lon. 76°, and its course S. 78° W., with an angle of depression of 20° upon the horizon of the places beneath it. The heights of its eastern and western ends were 59 and 49 miles or 95 and 79 kilometers.

Motion of the train of the meteor.

The cloud left by the meteor began to coil up, and float away, shortly after the disappearance of the body. The upper part must have moved northerly, and the lower portion southerly, and the whole either westward, or downward, to satisfy the appearances at New Haven. The successive positions with respect to Jupiter show this. Jupiter moved north 10′ per minute, and downward 11′ per minute during the time of observation. The motion of the lower part must have been in the main toward Washington, as after its first shortening the cloud retained its dimensions nearly constant. The motion was too nearly in the plain of Haverford and New Haven for us to entirely separate the downward and the westward motions, especially in the absence of determinate positions at specified times. But I think there was a descent of five or ten miles in the body of the cloud. This was certainly true of the eastern or upper end, and even a greater downward motion is probable.

The upper end was borne westward so that as seen from Washington the cloud was foreshortened. The central and lower parts were seen projected upon this upper part. Hence the bifurcation and the cavity described by Prof. Eastman. The elliptic form and the oval cavities seen at Haverford are also, in part at least, the effect of perspective.

The southward motion of the lower end at Haverford, 80 miles from the cloud, was 25°, and at New Haven, 175 miles distant, was 8° or 10° during two thirds of the period of visibility. These imply a motion of at least 40 miles, or about a mile per minute.

We must therefore assume that just below an elevation of about 50 miles, there was a rapid north wind, which swept the lower portion of this cloud with it southward. The wind may have come from one or two points east of north. Above this there was a south wind, (or S. S. E.,) whose velocity may have equalled that of the lower north wind, though it may also have been much less.

By its downward motion, the eastern end of the cloud was carried from the upper current into the lower, and strewn along into the horizontal cloud, seen at New Haven, in the latter part of the period of visibility.

These three suppositions, of two currents in the air, and of a downward motion of the cloud, seem to be necessary to any explanation of the appearances of the cloud. But they do not entirely account for the whorls and changes of form described by Miss Colby and others, and so evident in Mr. Gilman's sketches.

The same were quite remarkable at New Haven also, though my sketches were not designed to show them. I only designed to show the place of the body of the cloud, relatively to Jupiter. Mr. Gilman's sketches, which reached me but a day or two after the shower, seemed a remarkably accurate expression of my recollection of the shapes of the cloud.

To account for these whorls and transformations, we may suppose between these two principal currents of air, or traversing them, various other currents. These need not necessarily be horizontal. But at that distance from the disturbing causes upon the earth's surface, it is not probable that there are so many rapid and varied currents in the narrow limits of 10 miles in vertical direction, as the various contortions of the cloud imply. We expect greater simplicity in the aerial movements in these upper regions of the atmosphere.

To attribute these motions of meteor trains to previously existing currents, implies, moreover, that complex currents extend through the whole thickness of the stratum in which the cloudy trains are developed. For in all cases of a persistent cloud, we see the same coiling motions of the line of phosphorescent matter, shortly after the flight of the meteor. Such a system of currents previously existing in the air is not probable.

An alternative seems much more reasonable, which is to assume that the motion of the meteor produced this coiling of the train. As a meteor rushed through the air, it carried before it, and with it, and dragged behind it, a mass of air of considerable dimensions. Thus a large mass of air, containing in it the cloudy line of meteor dust, was thrust down into the lower current, and then swept away to the southward. This column of air, of course, sets in motion vast eddies which cannot be easily reduced to system.

If this is a true explanation, the mass of the meteor must have been sufficient to set in rapid motion the air that fills hundreds of cubic miles of space, as well as to develop light and heat. What kind of matter it is which remains visible in the cold upper air for three-fourths of an hour, until by gradual dissipation the light faded out, I leave for others to say.

Meteor of $11^h\ 18^m$, *N. York time*,—seen by Mr. Gilman, at Fern Lodge Observatory, Palisades.—A very grand meteor, much larger than Venus, which shot nearly vertically up from the horizon, in Leo Minor. The nucleus was white, and it left a bright blue train of sparks. The figure in plate II, represents it just previous to disappearance. Length of track 15°. It was not observed elsewhere.

Meteor of $11^h\ 25^m$, *N. Y. time.*—Two meteors were seen at Palisades. One, equal in brilliancy to Venus, white, left a bright blue train, visible eight minutes. The figure plate II represents its appearance, as seen by the naked eye, at 11^h 26^m 30 . It was a greyish cloud, 2° or 3° long. In a 4 inch telescope, power 40, diameter of field 43′, it presented a double line of bluish-green luminous matter. Not observed elsewhere.

Meteor of $1^h\ 53^m$, *N. Y. time.*—A white meteor, of the first magnitude, was seen by Mr. Gilman, at the Palisades, to pass through the northern limits of Camelopardus, toward and beyond κ and β Cassiopeiæ. The length of path was 30°. It left a smoky train, of a greenish-white tinge. The train was visible about half a minute, and on turning the telescope upon it, Mr. Gilman saw the double line of smoke represented in the figure plate II. Stars as small as the eighth magnitude were seen through the train. Twists or whorls of clouds appeared as represented in the engraving. The general appearance of the smoky substance was very like that of the nebula of Orion, as seen in a large glass. The same power and field of view were employed as upon the previous train.

I find no observations at other places upon this body.

Meteor of $2^h\ 45^m$, *N. Y. time.*—Mr. Gilman, at Palisades, saw at this time a meteor disappear at ϱ Leonis ; from the direction of θ Leonis ; smoky train remained 8 minutes.

At Stamford, Mr. Fuertes recorded a curved track, of double curvature, beginning at R. A. 150°, N. dec. 7°, and ending at R. A. 149½°, N. dec. 3°. He adds the notes :—Brighter than Venus ; very curved path ; somewhat spiral and short ; left a train which placed itself at right angles with the path, and lasted 650 seconds.

The two paths belong to the same body, but are not quite consistent. The want of conformability of the Palisades track may be due to the curvature of the track. There is indicated, however, a parallax of about 6°, which represents a vertical altitude of 51 miles. The smallness of the base line and of the parallax, makes the result uncertain. If there were two principal currents in the atmosphere, one above from the South, and one below from the North, as seems to be proved by the

1^h 12^m meteor, this meteor must have reached the surface, dividing the two currents, and its train have been acted upon in the same manner as the train of the earlier meteor.

Meteor of 2^h 48^m, N. Y. time, at Fern Lodge Observatory, Palisades.—A fine meteor, half-way between Polaris and Ursæ Majoris. No description of the meteor recorded. The view of the train near the point of disappearance, by means of a 4 inch glass, a power of 40, and a field of view of 43′ in diameter, is given in plate II. The train was double, (as often observed during the evening,) and terminated in an oval cloud, at right angles to the direction of the meteor's flight.

At Stamford, Conn. (lat. 41° 2′ 45″, long. 73° 35′).—Upon the chart sent by Mr. E. A. Fuertes, who was assisted in his observations by Mr. F. G. Wheeler, is the following position for the track of this meteor :—Beginning, R. A. 136°, N. P. D. 19° ; end, R. A. 9°, N. P. D. 7°. The two tracks correspond very well with each other, though the base line is small. The parallax is about 14° for the end, which gives an elevation of 52 miles for the end of the track, and 65 for the beginning, with a length of path of 27 miles.

The distance from Palisades to the end of the path is 70 miles. The field of view at that distance had a diameter of seven-eighths of a mile. As the course of the meteor was inclined 80° to the line of vision, the lengths of the portion of the cloud included in the field of view was 0·89 of a mile.

I think the double train of this and the other meteors is due to actual duality in the meteor itself. The same double or multiple character is common among the detonating and stone-producing meteors.

Meteor of 3^h 51^m 30^s, Portland time.—Prof. Rockwood, of Bowdoin College, saw a meteor of a bright green color pass in a direction parallel to a line joining β and α Aurigæ, and about 5° north of those stars. It left a bright train, 5° or 6° in length, which gradually curled up, and floated northward as it faded.

Mr. Tuttle reports the same meteor as seen at Boston ; very bright between β and γ Ursæ Minoris ; train visible 3 minutes. The latitude and longitude of Brunswick, (place of observation,) are 43° 54′ 32″, and 69° 57′ 24″ ; and those of Boston, (State House,) are 42° 21′ 28″, and 71° 3′ 30″.

These observations give a parallax of 53° upon the base line of 121 miles. The altitude of the meteor when it crossed the line joining β and γ Ursæ Minoris, as seen from Boston, was 77 miles. This was near the beginning of the path, since the point corresponds upon the Brunswick path to a point 4° or 5° toward the radiant from opposite β Aurigæ. The cloud must have been ten miles long.

Meteor of $5^h\ 6^m$, *Portland time.*—This was a very bright green meteor, seen by Prof. Rockwood to pass directly across θ Leonis in the direction toward θ Hydræ. It disappeared after having crossed about two-thirds of the distance between these stars. It became visible about midway between η and o Leonis. It left a long train, which assumed the serpentine form before it disappeared.

Mr. Tuttle, at Boston, saw the body near η Ursæ Majoris. The altitude of this meteor at disappearance was about 48 miles. Its first appearance, if Prof. Rockwood's observation be taken literally, was at a height of nearly 150 miles. But the track was so near to the radiant, that a difference of three or four degrees in the apparent length, would reduce the first altitude below 100 miles.

Meteor of $5^h\ 6^m\ 45^s$, *New Haven time.*—This meteor passed one degree south of β Geminorum as seen by me at New Haven.

The central part of the train floated north, and the upper part south. The amount of the northward motion was three degrees in three minutes. At first the lower or western end floated south—or at least fell behind the rest, but this part appears soon to have vanished, for in my representations of the shape of the train it is only presented in the earlier diagrams.

At New York City Prof. Twining saw the same meteor, and by a careful reference to the stars in the neighborhood he locates the beginning and end of the track as in R. A., 176°, N. dec., 47°, and R. A., 191°, N. dec., 54°. The train moved a little to the east of north five degrees in three minutes, and continued visible six minutes.

The altitude of the beginning, and end of the visible path of the meteor is, from these observations, 85 and 60 miles, respectively. The motions of the train seem to indicate an upper current from the north, above that from the south, which was shown by the motions of the train of the meteor at $1^h\ 12^m$.

Meteor of $5^h\ 30^m\ 30^s$, *Boston time.*—Mr. Tuttle saw a very bright meteor at this instant which passed above Cor. Caroli, downward to a point below η Ursæ Majoris, leaving a train visible for seven minutes. But for daylight it might have been seen longer. Immediately after the meteor disappeared the train began to shorten and widen, and take a serpentine form. It finally became a cloudy mass, oblong in form, and drifted away northward under η Ursæ Majoris.

From the position of this track and the description, I feel sure that it is the same as the one described by a writer in Fairhaven, Mass., in an article signed D. (Mr. Jabez Delano), in the *New Bedford Standard* as follows.

At twenty-three minutes past five, a most superb meteor burst from the region of Ursa Major, and went toward Ursa Minor, leaving a bright trail. For two full minutes it remained unchanged. He then drove a stake in a direct line from the top of a tree, to the meteor. At this stage the trail had contracted in length and widened in breadth, presenting a serpentine aspect. It was a pale cloud, nearly circular, and larger than the full moon. It was now waning away, and soon became extinct, having been visible and stationary nearly five minutes. The two stars in Ursa Minor, known as the Guards, pointed directly toward it in a S. E. direction, about 8° distant. In the morning he took the altitude and azimuth of the line from the stake to the tree, and found the direction of the meteor to be N. 38° 30′ E., alt. 32° 40′ 51″. The latitude and longitude of the place is 41° 38′ 6″ N. lat., and 70° 53′ 51″ W. long.

One of the young ladies in Miss Mitchell's class at Vassar College gives a diagram of the path of a meteor at $5^h\ 21^m\ 15^s$. I think it is the same body, notwithstanding a slight discrepancy in the observed times. She places it between Arcturus and Corona Borealis. The reference to the stars in Ursa Minor shows the Fairhaven azimuth to have been the magnetic azimuth. The Poughkeepsie path is not drawn so as to be quite conformable. Changing it as little as is possible to make it conformable, and the three observed paths agree very well with necessary conditions, and indicate an altitude of about 59 miles for the lower part of the cloud. The northward motion of the cloud showed that it did not penetrate through the upper into the lower current, which swept away southward the lower part of the train of the meteor at $1^h\ 12^m$.

Art. XLIII.—*A Proposed Arrangement for Observing the Corona, and Searching for intra-Mercurial Planets during a Total Eclipse of the Sun;* by Simon Newcomb.

It is a fact of universal observation that, during a total eclipse of the sun, the dark body of the moon is surrounded by a brilliant crown of light, commonly likened to the "glory" represented by painters as surrounding the heads of saints. But, of the form and extent of this "corona," nothing definite seems to be certainly known. It is commonly represented as being not uniformly luminous at equal distances from the center, but as radially striated, and extending out into rays in various directions. At least, one observer has gone so far as to connect the dark and bright rays with the elevations and depressions on the limb of the moon. If these dark rays have any existence, it is difficult to account for them by any known cause. It is easy to see that they cannot be shadows of opaque masses situated anywhere in space, because the shadow of no object situated so near the line joining the observer and the sun could look long and narrow to the observer.

Do these rays and striations really exist? We know that such appearances are one of the most common optical illusions when we view a bright object on a dark ground. And, in the various descriptions of them which have come to my knowledge, I have not found a single statement that any observer took the precaution to turn his head in order to see whether these rays of millions of miles in length turned with it. Their existence cannot, therefore, be regarded as proven.

The following method is proposed as one that will immediately decide the question of the existence of these rays, and give, at least, a rough approximation to the height of the corona. Erect between the eye of the observer and the sun a series of circular screens subtending angles varying between 32′ and the greatest probable limit of the corona, say 38′. As soon as the total phase commences the observer will stand behind the larger screen and see whether he can make it hide the entire luminosity surrounding the sun, and, if not, will note very carefully the position of any object or luminosity not hidden. He will then make similar observations with the other screens in succession until one is found which shows the corona on all sides at once.

The law of diminution of light from the inside to the outside of the annulus can, perhaps, be most accurately determined by exposing a number of equally sensitive photographic plates during widely different intervals, and noting the positions of the circles of equal effect on the different plates.

It may be remarked that the height commonly attributed to the corona does not seem compatible with that of an atmosphere surrounding the sun in a state of equilibrium. Though such an atmosphere were of hydrogen, and at a temperature of 100,000° Fahrenheit, its density would be reduced one-half in every 350 miles of height. At a height of 1′ in arc, or 26,000 miles, it would be so rare as to be altogether invisible, unless we suppose the temperature to be many times 100,000°. It seems, then, that the general atmosphere of the sun, if there is one, would be altogether hidden by the moon in most total eclipses of the sun, and that its greater brilliancy on the side over which the moon least projected would be very strongly marked.

The search for intra-Mercurial planets will be facilitated by the use of similar screens of a larger size. The following proposition is laid down as one hardly admitting of doubt. If the great motion of the perihelion of Mercury deduced by Le Verrier from the observations of transits of that planet is caused by the attraction of matter in the solid or liquid state, that matter can be seen during a total eclipse if the direct light of the corona and protuberances is shut off from the eye. This is founded on the following propositions:

The masses of matter in question cannot be more than half of Mercury's distance from the sun.

Hence, taking equal surfaces of them and of Mercury, they will shine with more than four times the brilliancy of that planet.

If they are not smaller than the second magnitude they will be visible to the naked eye, properly prepared by being kept in previous darkness and shaded by the screen.

If they are smaller than the second magnitude, they must be a hundred or more in number. Some of them will then be easily detected with a telescope of large field of view unless they are below the fifth magnitude.

If they are below the fifth magnitude, they must number thousands, and they will then be visible to the naked eye as a continuous diffused light.

The screen for this search should be large enough to protect the telescope from the sun during the entire period of the total phase.

It is hoped that these arrangements and observations will be found to merit the attention of a share of the observers during the total eclipse of August next.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the refrangibility of the brilliant yellow ray of the sun's atmosphere.*—The fact that the yellow ray seen in the spectrum of the protuberances from the sun's photosphere does not correspond with the lines D of the solar spectrum was first observed by Lieut. Herschel, then by Mr. Lockyer, by Secchi, and finally by Janssen. Rayer has now determined the position of this line with accuracy, using a spectroscope of high dispersive power and a plain micrometer. Taking the distance between the two lines D as unity the author found for the distance of the yellow line from the more refrangible ray D the number 2·49, with a probable error of less than 0·03. Taking the wave lengths of the two lines D

respectively as 590·53 and 589·88, that of the yellow line is 588·27 in millionths of a millimeter; it corresponds to the division 1016·8 of Kirchhoff's scale.—*Comptes Rendus*, lxviii, 320, Feb. 1869.

W. G.

2. *On the presence of the vapor of water in the neighborhood of the solar spots and on the spectral study of certain stars.*—By observing the regions adjacent to the large solar spots with a spectroscope of high dispersive power, Secchi has frequently noticed a series of equidistant nebulous lines or bands in the red and orange near the ray 809·5 of Kirchhoff and 864 of Kirchhoff. These bands differ in intensity; they appear to consist of very fine rays enveloped in nebulosity and are seen in the penumbras and in the group of small spots recently visible, but usually disappear in the sun's full disc and are wanting in the interior of the large spots where the rays never have the form of the bands. On the 6th January the bands were seen upon the full disc of the sun, but were found to arise from a cirrhus in front of the telescope and disappeared with the cirrhus itself. Secchi remarked that under these circumstances the bands due to the neighborhood of the solar spots were sensibly increased in intensity. By studying the region near D with a spectroscope of nine prisms the author found that the yellow ray of the protuberances really exists in the sun and may be recognized far from the border. The author then sought for the yellow ray in the spectra of the stars which most resemble the sun and found it in Aldebaran, α Orionis and Pollux. Sirius presents a bright region in the corresponding place. In the red stars of the 4th type bright yellow rays may be seen like threads of gold, but it is difficult to fix their positions. Secchi concludes from his observations that the vapor of water exists in the solar atmosphere in the neighborhood of the large spots.

The author has also examined the spectrum of Sirius to determine whether there is any displacement of the hydrogen lines due to a proper movement of the star, a question already examined by Mr. Huggins. With a spectroscope of four prisms the ray F was observed to be sensibly displaced, the displacement of the center being sensibly equal to the breadth of the rays D′ D″ of sodium and being toward the less refrangible side. With a spectroscope of two prisms the displacement of the hydrogen rays α and γ with respect to the rays C and F of Sirius was also observed and in the same direction. It is now well known that the bright lines in the nebulæ are due to hydrogen and nitrogen, but as nitrogen presents spectra of two different orders it was interesting to determine to which of them the bright green line belongs. By direct comparison Secchi found that the line in question corresponds to a brilliant line in the spectrum of the second order. As the production of this line requires a considerable electric force and a higher temperature it is evident that the matter of the nebulæ is in the more advanced state of dissociation which corresponds to the second spectrum.—*Comptes Rendus*, lxviii, 358. W. G.

3. *Spectral observations of the star R in Gemini.*—SECCHI has also examined the spectrum of the variable star R Gemini (R. A. $6^h\ 59^m\ 22^s\ \delta\ 22°\ 54'$) which star obtained its maximum brightness with a magnitude of 6·5 in February last. The spectrum of this star exhibits a brilliant hydrogen ray (which of the four known to belong to hydrogen is not mentioned). It also presents other luminous bands of which the principal correspond to dark bands in the spectrum of α Orionis intermediate between b and D. From this it appears that the spectrum of this star is analogous to that of the variable star in Corona Borealis which appeared in 1866.—*Comptes Rendus*, lxviii, 361. W. G.

4. *On absorption lines produced by the passage of the solar light through chlorine.*—MORREN has found that by employing a spectroscope of five prisms of highly dispersive flint glass, absorption lines are distinctly visible in the spectrum of light which has traversed a tube filled with chlorine two meters in length. The lines begin to be visible in the part of the spectrum near b. They vary in intensity, fineness and mode of grouping and exhibit some slight free spaces. They have no regular order and extend beyond the ray F toward the ray 2110 of Kirchhoff's scale. In this last portion they are very numerous and almost equidistant. The solar spectrum proper continues visible as far as 2210, but after that the light is completely absorbed. Chlorine therefore absorbs the colored portion of the spectrum where the chemical rays are most abundant.—*Comptes Rendus*, lxviii, 376.

W. G.

5. *On hydrogen in its relations to palladium.*—GRAHAM has shown that palladium absorbs 800 or 900 times its own volume of hydrogen, its density being sensibly diminished. When a wire of palladium is charged with hydrogen by being made the negative pole of a battery, it increases in length and volume. By measuring this increase and determining the volume and weight of hydrogen absorbed, Graham found for the constituent of the alloy, palladium 95·32, hydrogen 4·68, and for the density of the hydrogen compressed by occlusion in the metal numbers varying from 1·708 to 2·055, the mean of the numbers accepted being 1·951. The author considers the hydrogen under these circumstances to be in the metallic state and calls it hydrogenium. The tenacity and electric conductibility of palladium are both diminished by the absorption of hydrogen, the former in the ratio of 100 to 81·29; the latter in the ratio of 8·10 to 5·99; on the other hand the paramagnetic character of the metal is decidedly increased by its association with hydrogen which must be classed among the magnetic elements with iron, nickel, cobalt, chromium and manganese. At a high temperature hydrogen passes with considerable facility through palladium. The greatest rate of passage observed was four liters of gas per minute through a plate of palladium one millimeter in thickness and one square meter of surface, the temperature being a little below the melting point of gold. From the above it appears that hydrogenium

is a solid white metal of density about 2, magnetic, conducting electricity and uniting with palladium in the proportion of one atom to one atom. The alloy possesses the properties of gaseous hydrogen intensified. It reduces mercury and calomel from mercuric chlorid and possesses in general a considerable degree of reducing power.—*Comptes Rendus*, lxviii, 101. W. G.

6. *Production of an artificial spectrum with a single Fraunhofer's line.*—When the discharges of a Holtz machine aided by a Leyden jar of about one foot of internal coated surface and short striking distance are made to pass through a Geissler tube placed in front of the slit of a spectroscope, the spectrum of the gas in the tube is first seen. Wüllner finds that if the striking distance be slightly increased the sodium line makes its appearance, and with an appropriate striking distance is so bright as to far exceed in intensity the lines of the gas-spectrum. When the striking distance is again slightly increased the bright lines of the calcium spectrum make their appearance with a beauty and sharpness which can scarcely be attained in any other way. By again increasing the striking distance the line of light in the tube becomes extremely brilliant and then exhibits a bright continuous spectrum in which, however, the sodium line appears perfectly dark. The explanation of this phenomenon is found in the fact that fine splinters of glass are torn from the inner side of the tube. These become intensely ignited and their light gives the continuous spectrum. The ignition of these particles takes place in an atmosphere of the vapor of sodium from the glass so that the same current produces the ignited core and the absorbing atmosphere. As the calcium lines are not seen inverted it is probable that the density of the calcium atmosphere is not sufficient to produce inversion.—*Pogg. Ann.*, cxxxv, 174. W. G.

7. *On the analysis of different varieties of carbon.*—Berthelot has communicated to the Academy of Sciences a very elaborate and detailed memoir on the different varieties of carbon, showing that the number of modifications of this element is much greater than had hitherto been supposed and, as seems to us at least, full of interest. The author in the first place recalls the three recognized varieties under the heads of diamond, amorphous carbons derived from organic matter, and graphites, natural and artificial. The method of study employed was based upon Brodie's method of oxydizing graphites by means of nitric acid and potassic chlorate. By these reagents diamond is not sensibly oxydized; the different varieties of amorphous carbon are changed into humus like substances of a yellowish brown color soluble in water and varying according to the variety of carbon analyzed, while the graphites are converted into graphitic oxyds which differ with the nature of the graphites which furnish them. Berthelot gives the name pyrographitic oxyds to the black powders which remain when the graphitic oxyds are heated. They also contain oxygen and hydrogen as well as carbon. Native plumbago, or as Berthelot terms it, ***plumbagine***, already examined by Brodie, yields a graphitic oxyd

in pale yellow micaceous scales insoluble in all reagents. Dried even at ordinary temperatures it agglomerates to brown amorphous tenacious plates in which the primitive structure has disappeared. This character is essential and invariably reappears after all the transformations of the oxyd. Iodhydric acid at 280° C. converts graphitic oxyd into a new substance which Berthelot calls hydrographitic oxyd, containing more hydrogen than its primitive, but brown, amorphous and insoluble in all solvents. The new oxyd is not decomposed with deflagration and intumescence by heat. With potassic chlorate and nitric acid it yields graphitic oxyd with all its original properties. Pyrographitic oxyd from plumbagine is a black, light flaky powder which according to Brodie has the formula $Ȼ_{22}H_2Θ_2$. Nitric acid and potassic chlorate dissolve it almost wholly like the amorphous carbons, but a small quantity of graphitic oxyd is always regenerated.

The graphitic oxyd from graphite of cast-iron presents greenish yellow scales better developed than those from plumbagine and not agglomerating during desiccation. This character which is constant readily distinguishes the oxyd from that obtained from plumbagine. The hydrographitic oxyd from this graphitic oxyd intumesces when submitted to heat, giving off a considerable quantity of iodine. By oxydation it reproduces graphitic oxyd which does not agglutinate by drying. The corresponding pyrographitic oxyd dissolves in a mixture of nitric acid and potassic chlorate in a much more complete manner than that from plumbagine. Some scales of the original graphitic oxyd are however always formed with their distinctive properties.

Graphitic oxyd from electric graphite—carbon points from a large battery—is a maroon colored powder which does not agglomerate during desiccation. This character is also constant. The hydrographitic oxyd does not intumesce when heated, and when oxydized reproduces the original oxyd. This form of graphitic oxyd is also decomposed by heat with deflagration but yields a heavy powder which is not flaky. By oxydation the pyrographitic oxyd disappears almost wholly, leaving some grains of graphitic oxyd with its original properties.

All the pyrographitic oxyds when treated with iodhydric acid in solution at 280° C. yield hydrogen containing about 6 per cent of marsh gas, leaving however a considerable quantity of a black carbonaceous residue. The author compares the graphites, amorphous carbons and their derivatives with the hydrates of carbon and ulmic matters, and believes that the varieties of amorphous carbon represent polymeric states of the true carbon which is not yet known in the free or uncondensed form. In studying the different varieties of carbon, Berthelot has arrived at the following results in addition to those related above. Coke recently calcined is entirely dissolved, giving a soluble compound of an intense color. Metallic carbon, deposited from hydrocarbon vapors heated in a porcelain tube, is dissolved with very great difficulty but completely. The same is true for gas-retort carbon and some substances called artificial graphites. Anthracite, animal charcoal and the carbonaceous matter from the Orgueil meteorite were also completely oxydized, but lamp-black left a trace of graphitic oxyd. The intense heat produced by combustion in oxygen converts a small portion of gas-retort carbon into graphite. Berthelot suggests that it is in this manner that natural graphite has been formed, the amorphous carbon, being more oxydizable at a low temperature, having been gradually dissolved. This view derives some support from the presence of a trace of graphite in lamp black. Electricity also converts amorphous carbon into graphite, the carbon carried over to the negative pole being found to contain a considerable quantity of the latter, while the positive pole contained only a trace. The actual transference of the carbon is not however necessary for the formation of graphite; carbon from sugar softened by the heat from a battery of 600 pairs being found to contain graphite in large proportion. Carbon separated from hydrocarbons by the agency of heat does not contain a trace of graphite, while that which is separated by heat from the sulphid or chlorid of carbon or by chlorine from boron contains a considerable quantity.—*Comptes Rendus*, lxviii, pp. 183, 259, 331, 392, 445.

W. G.

8. *On the direct synthesis of Cyanhydric acid.*—BERTHELOT finds that when a stream of sparks from an inductorium is passed for some time through a mixture of acetylene and nitrogen the two gases unite in equal volumes without condensation and form cyanhydric acid. To prevent the partial decomposition of the acetylene it is well to add to the mixture a quantity of hydrogen equal to about ten times the volume of the acetylene. The reaction is expressed by the equation

$$C_2H_2+N_2=2CNH.$$

The reaction in this case begins rapidly but soon slackens. A definite volume of acetylene may be made to disappear completely by putting a drop of a solution of potash into the vessel before passing the stream of sparks. The cyanhydric acid is then absorbed as fast as formed, and about five sixths of the acetylene is converted into cyanhydric acid, the other sixth being decomposed into carbonic acid and oxyd by the vapor of water present. The effect of cyanhydric acid in preventing the action in this case is due to the fact that a mixture of cyanhydric acid and hydrogen gives under the influence of the stream of sparks acetylene, or a reaction the inverse of that first described. Having observed that all hydrocarbons are decomposed by the spark with formation of acetylene, Berthelot inferred that nitrogen would give cyanhydric acid with the vapor of any hydrocarbon, and this was fully verified by experiment. In the presence of potash two or three minutes' passage of the spark suffices to give the reaction due to the formation of Prussian blue.—*Comptes Rendus*, lxvii, 1141.

W. G.

9. *On the Ammonium-amalgam.*—LANDOLT has made a series of experiments to ascertain the composition of the ammonium-amalgam. It was prepared from a solution of ammonic chlorid by

electrolysis in the ordinary way, the negative electrode being connected with mercury contained in a porous cup filled with the ammoniacal liquid, while the positive electrode dipped into mercury in an outer glass vessel, also filled with the solution of chlorid of ammonium. When the current from 6 to 10 Grove's cells was employed, the positive electrode became covered with a layer of calomel, while the mercury in contact with the negative electrode slowly increased in bulk, evolving no gas until the point of saturation was reached. Landolt first determined the ratio of the ammonia gas evolved to the hydrogen, by placing the amalgam in dilute hydrochloric acid of known strength, and measuring the hydrogen evolved. The ammonia was then calculated from the quantity of the acid which it saturated. To free the amalgam from the ammonia contained in the solution, it was washed with water; but as the decomposition continued and only the hydrogen escaped, it was evident that the ammonia thus retained must give too high a result. The first experiment gave 1:2·15 as the ratio between the hydrogen and the ammonia by volume. The second,—in which the amalgam was less quickly placed in the acid,—gave the ratio 1:2·4. These results, which entirely confirm those of Davy, establish, as Landolt believes, the conclusion that the compound NH_4 is taken up as a whole by the mercury. This is proved by the above ratio, because, were the gases separately absorbed, they would be set free again in very different proportions. In the second place, Landolt attempted a determination of the amount of the ammonium thus combined with the mercury. The amalgam was placed in a standard dilute hydrochloric acid as before; the ammonium was calculated from the quantity of acid neutralized and the mercury was determined by collecting and weighing it. The results varied from ·054 to ·090 per cent of NH_4, owing to the rapid decomposition which took place. Evidently the maximum result is nearest the truth; and if 100 parts of mercury take up 0·09 parts of NH_4, the amalgam in decomposing should yield for each volume of mercury 15·2 volumes ammonia and 7·6 volumes hydrogen; numbers which hold good for the compound prepared at ordinary temperatures. On the metallic nature of ammonium, too, Landolt made some experiments. Starting with the well known fact that potassium or sodium-amalgam will throw down most metals from solutions of their salts, he argues that the ammonium-amalgam, if analogous, should do the same. Freshly prepared ammonium-amalgam was placed in the metallic solution, the separated mercury was washed with water, dissolved in nitric acid and examined for the metal whose solution had been used. The result with cupric sulphate, argentic nitrate, and ferric chlorid solutions was entirely negative, though at least 100 grams of the amalgam was employed. While therefore NH_4 combines as such with the mercury, its metallic character is doubtful; further researches only can decide its nature. —*Ann. Chem. Pharm.*, Suppl. Bd. vi, Heft 3, p. 346, Dec. 1868.

G. F. B.

10. *On a new method for preparing Carbonylic Sulphid.*—LADENBURG has communicated to the Chemical Society of Berlin a new method for the preparation of carbonylic sulphid gas. From theoretical considerations he was led to search for a body containing the group COS, thinking the atoms might be differently arranged from that in Than's compound. He finds that Kekulé's thiacetic acid, in which this grouping of atoms occurs, is decomposed by electrolysis, by heat, or by the action of bromine. The first is the most certain in its results, but it requires a large quantity of material. The action of bromine he had not yet fully studied. The action of heat on thiacetic acid had already been studied by Kekulé and Ulrich, up to 180° C., and they observed the production of H_2S. Ladenburg employs a temperature of 300°; the gas evolved is a mixture of carbonylic sulphid with hydric sulphid, the latter constituting about $\frac{3}{4}$ of the whole volume. After the absorption of these a small quantity of a combustible gas remained, which was not accurately analyzed. In the tube a black coaly mass is left which shows that the decomposition is one not to be expressed by a chemical equation.—*Ber. d. deutsch. Chem. Ges. zu Berlin*, ii, 53, Feb. 1869. G. F. B.

11. *On Carbonylic Sulphid.*—BENDER has studied the deportment of carbonylic sulphid with alcoholic solution of potassic hydrate. This gas, prepared according to the method of Than,* and purified by passing through a U tube filled with unvulcanized caoutchouc and immersed in a freezing mixture of pounded ice and hydrochloric acid—was conducted into a cooled concentrated solution of potassic hydrate in alcohol. It was completely absorbed and after a time the fluid solidified to a crystalline mass. The crystals were placed on a filter, dried with blotting paper and recrystallized from alcohol. White needle-shaped crystals were obtained which closely resembled potassic xanthate and afforded on analysis the formula $C_3H_5KSO_2$. This salt is easily soluble in alcohol and water—though not deliquescent—and insoluble in ether. At 170° C. it decomposes into carbonylic sulphid, ethylic sulphid, and potassic carbonate, according to the equation:

$$(C_3H_5KSO_2)_2 = COS + \left.\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right\}S + \left.\begin{matrix}CO\\K_2\end{matrix}\right\}O_2.$$

In aqueous solution the decomposition is as follows:

$$(C_3H_5KSO_2)_2+(H_2O)_2=\left.\begin{matrix}H\\H\end{matrix}\right\}S+\left.\begin{matrix}CO\\K_2\end{matrix}\right\}O_2+\left(\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O\right)_2+COS.$$

The same salt apparently, was obtained by Debus by decomposing ethylic sulphocarbonate with alcoholic potash, thus:

$$CS_2O(C_2H_5)_2+HKO=CSO_2(C_2H_5)K+H(C_2H_5)S;$$

and by Chancel by passing carbonic gas into an alcoholic solution of potassium mercaptid. Its possible rational constitution may be represented in three ways, thus:

$$CS<\begin{matrix}OC_2H_5\\OK\end{matrix}\qquad CO<\begin{matrix}SC_2H_5\\OK\end{matrix}\qquad CO<\begin{matrix}OC_2H_5\\SK\end{matrix}.$$

* This Journal, II, xlv, 251, March, 1868.

Its formation from carbonic dioxyd and potassium mercaptid, as well as from carbonylic sulphid; its decomposition also, both by heat and in watery solution, prove that it contains the radical ЄΘ. But while its decomposition into ethylic sulphid by the action of heat would seem to indicate that the sulphur was united with the ethyl, its yielding potassic sulphid when heated in aqueous solution suggests that the sulphur and potassium are united. Both reactions render it improbable that the sulphur is held to the carbon by both its units of attraction. Between the two formulas $ЄΘ<^{ΘЄ_2H_5}_{SK}$ and $ЄΘ<^{SЄ_2H_5}_{ΘK}$ future researches must decide.—*Ann. Ch. Pharm.*, cxlviii, 137, Nov. 1868. G. F. B.

12. *On carbonylic chlorid, and a new compound of this substance with platinum.*—Schützenberger, in attempting the direct synthesis of carbonylic chlorid by passing a mixture of dry carbonous oxyd and chlorine gases over heated platinum sponge, noticed that beside the phosgene gas thus formed, there was produced a large quantity of a solid platinum compound which was deposited in the cooler portions of the tube, forming either a fused yellowish-brown ring—which crystallized on solidifying—near the heated part, or being carried on and deposited all along the tube as a clear yellow flocculent powder, which soon obstructs the tube. Heated to 150°, this powder melts, yielding a reddish-yellow liquid, which again solidifies on cooling to a reddish-yellow crystalline mass. By varying the conditions a second product was obtained which melted at 130°. The former substance on being heated nearly to redness, is decomposed into metallic platinum and a mixture of carbonylic chlorid and carbonous oxyd gases, though a portion is at the same time sublimed and is deposited again as a dark yellow liquid which solidifies to a nearly colorless crystalline mass. Exposed to moist air it is blackened in a few minutes. Water decomposes it with effervescence, evolving carbonic dioxyd and carbonous oxyd gases, precipitating finely divided platinum, and holding hydrochloric acid in solution. The crude product yielded on analysis from 61·57 to 62·3 per cent **Pt** 22·7 to 23·8 Cl, and 5·22 Є; and after crystallizing it from solution in carbonic tetrachlorid—dried over sodium—it gave 63·5 to 63·7 per cent **Pt**, 22·9 to 23·4 per cent Cl, 4·55 to 5·3 per cent Є, being nearly identical with the former. This analysis yields the formula $(ЄΘ)_3\mathbf{Pt}_2Cl_4$, and not $ЄΘ\mathbf{Pt}Cl_2$ which Schützenberger had at first proposed. Its chemical constitution is very easily and naturally obtained; two atoms of quadrivalent platinum are united by three links of bivalent carbonyl into a closed chain, having four free bonds, which are saturated by chlorine. It is hence a di-platino-carbonylic tetrachlorid, while $ЄΘ\mathbf{Pt}Cl_2$ would be platino-carbonylic dichlorid. Possibly on heating the former compound it may evolve ЄΘ and yield the latter, thus:—

$$(ЄΘ)_3\mathbf{Pt}_2Cl_4=(ЄΘ\mathbf{Pt}Cl_2)_2+ЄΘ.$$

Schützenberger gives also the following syntheses of phosgene;

(1.) By heating an excess of carbonic tetrachlorid with zincic oxyd to 200° in a sealed tube for several hours. $CCl_4+ZnO=COCl_2+ZnCl_2$. The product, however, is mostly destroyed by the action of the zincic oxyd thus: $COCl_2+ZnO=ZnCl_2+CO_2$.

(2.) By passing a mixture of carbonic tetrachlorid vapor and carbonous oxyd gas over pumice heated to about 400°.

$$(CCl_4)_2+CO=COCl_2+C_2Cl_4.$$

(3.) By similarly treating a mixture of carbonic tetrachlorid vapor and carbonic dioxyd. $CCl_4+CO_2=(COCl_2)_2$.

(4.) By acting on carbonic disulphid with hypochlorous oxyd:

$$CS_2+(Cl_2O)_3=(SOCl_2)_2+COCl_2.$$

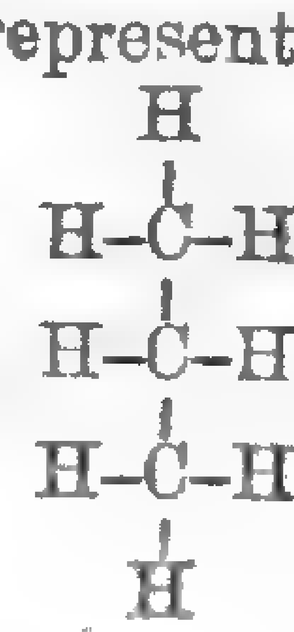
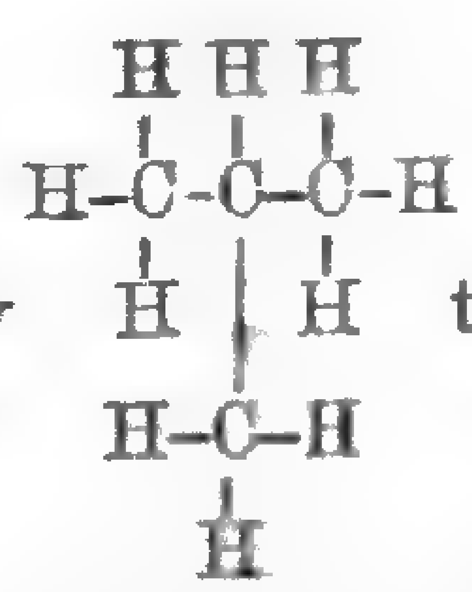
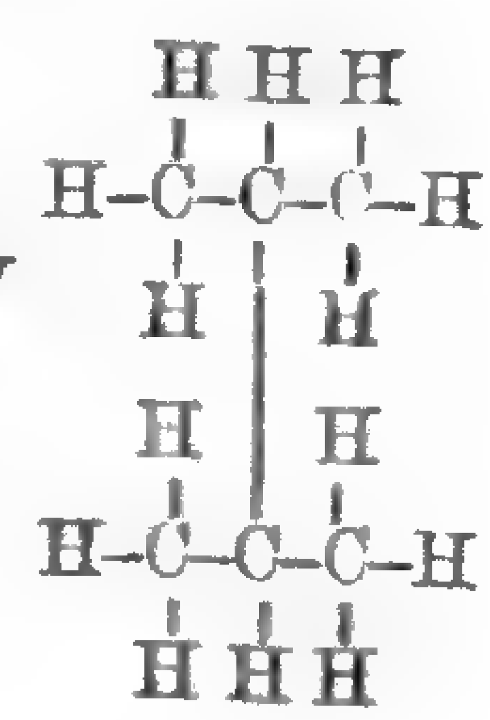
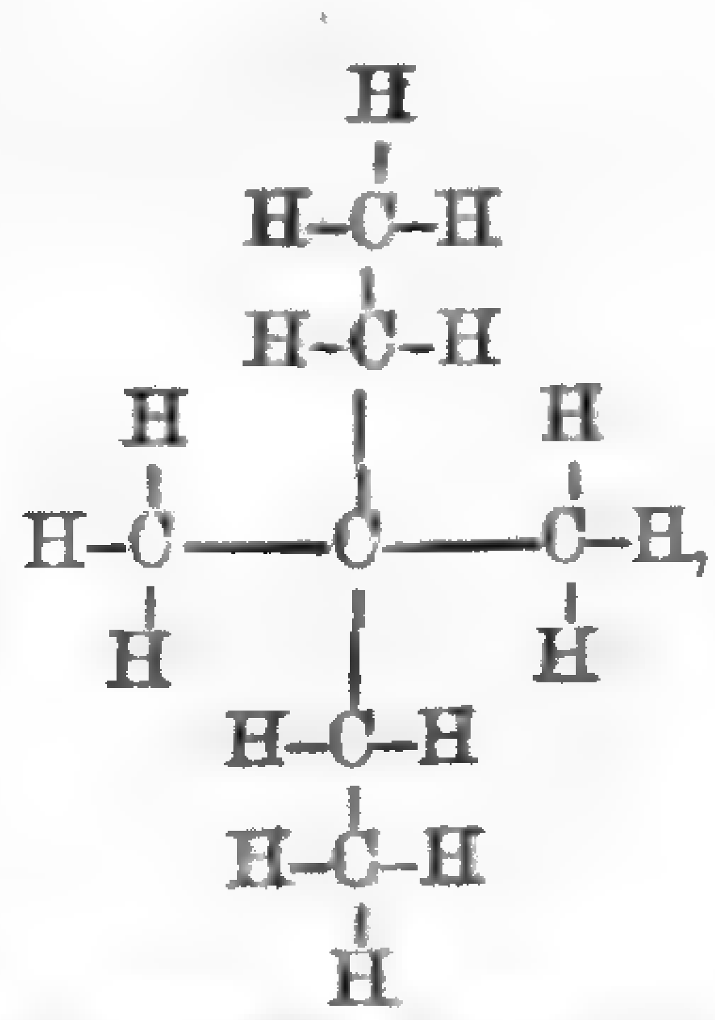
Bull. Soc. Ch., II, x, 188, Sept. 1868. G. F. B.

13. *On the relation between boiling point and constitution of hydrocarbons.*—The saturated hydrocarbons whose constitution is known with more or less certainty, SCHORLEMMER divides into four groups. The members of each group show a very regular difference of boiling-point. (1) The first group comprises hydrocarbons in which the carbon atoms form a simple chain; i. e., those in which no carbon atom is united to more than two others. Beside the first three members of the series, di-ethyl, hexylic hydrid from suberic acid, and heptylic hydrid from azelaic acid also belong in this group, since their boiling-points are higher than those of their isomers; it being a fact that compounds whose carbon atoms are thus arranged, boil at a higher temperature than isomeric bodies in which any of the carbon atoms are united to more than two others. Moreover, the formation of these bodies also places them in this group; the acids from which they are derived being produced by the splitting up of compounds richer in carbon; and as the hydrocarbons come from these acids in the same way they must possess a simple constitution. The difference of boiling point between the members of this group is 31° C. The hexylic hydrid from suberic acid, that from petroleum, and that from mannite appear identical. (2) The next group includes those which may be viewed as derived from propylic (tritylic) hydrid by annexing a simple chain to the middle carbon atom. To this group belong trimethylformene, some hydrocarbons derived from the butylic and amylic alcohols produced in fermentation, and octylic hydrid from caprylic (octylic) alcohol. The difference of boiling point here also is 31° C., amylic hydrid $\left\{\begin{matrix} CH(CH_3)_2 \\ C_2H_5 \end{matrix}\right.$ boiling at 30°, ethyl-butyl $\left\{\begin{matrix} CH(CH_3)_2 \\ C_3H_7 \end{matrix}\right.$ at 62°, ethyl-amyl $\left\{\begin{matrix} CH(CH_3)_2 \\ C_4H_9 \end{matrix}\right.$ at 91°, and octylic hydrid $\left\{\begin{matrix} CH(CH_3)_2 \\ C_5H_{11} \end{matrix}\right.$ at 124°. (3) The third group contains the isopropyl grouping twice, instead of once; i. e., there are two atoms of carbon in it, each of which is united to three others. The difference of boiling point in this group is 25° C. It includes di-isopropyl $\left\{\begin{matrix} CH(CH_3)_2 \\ CH(CH_3)_2 \end{matrix}\right.$ boiling at 58°, di-butyl-amyl-isopropyl $\left\{\begin{matrix} CH(CH_3)_2 \\ C_2H_4 \\ CH(CH_3)_2 \end{matrix}\right.$ boiling at

109°, (the second member being wanting) amyl-butyl $\left\{\begin{array}{l}CH(CH_3)_2\\ C_3H_6\\ CH(CH_3)_2\end{array}\right.$ at 132°, and di-amyl $\left\{\begin{array}{l}CH(CH_3)_2\\ C_4H_8\\ CH(CH_3)_2\end{array}\right.$ at 158°. (4) The fourth group includes hydrocarbons in which one carbon atom is united with four others. Thus far only one member of this group is known, the carbo-dimethyl-diethyl of Friedel and Ladenburg $C(CH_3)_2(C_2H_5)_2$, which boils from 86° to 87° C.*

From this classification it is easy to see that in the marsh-gas, as in the benzol series of hydrocarbons, conclusions as to the constitution of the bodies may be drawn from their boiling points.—*Ann. Ch. Pharm.*, cxlvii, 214, Aug. 1868. G. F. B.

14. *On a new source of octylic alcohol.*—Silva has examined in the laboratory of Professor Wurtz, a sample of fixed oil, derived, like castor and croton oil, from the family of the euphorbiaceæ, the plant yielding it having been described by Adanson about the middle of the last century under the name *Curcas purgans.* This plant exists in great abundance in certain parts of Africa, and especially in the Cape deVerd islands; and from the fruit, considerable quantities of the fixed oil are obtained, the physiological properties of which are analogous to those of castor oil, though it is more active. The similar origin and analogous properties of this and castor oil suggested its distillation with an alkali, as had been

* These groups may be more clearly shown by a graphic representation. Taking propylic (tritylic) hydrid as a representative of the first , the second may be represented by  trimethyl-formene, the third by  di-isopropyl, and the fourth by  carbo-dimethyl-diethyl. In the first the middle carbon atom is united to two others, in the second the upper carbon atom is connected with three others, in the third both the main carbon atoms are so united, and in the fourth all four bonds of the middle carbon atom are saturated by carbon.—G. F. B.

done with the latter by Bouis. The sample of oil—obtained from the Portuguese commissioner at the Paris Exposition—was small, but on saponification and distillation it yielded a complex liquid, which was inflammable and possessed an agreeably aromatic odor. On fractional distillation, the portion passing between 178° and 180° was a colorless, slightly oleaginous, aromatic liquid, whose analysis gave numbers corresponding to octylic alcohol. It is insoluble in water, easily soluble in alcohol and ether, and is turned yellow by light. No attempt was made to determine its constitution by oxydizing it. The "purgueira" oil—by which name the oil is known in the Portuguese colonies—contains 6·1 per cent of nitrogen, which is evolved as ammonia on distillation.—*Bull. Soc. Ch.*, II, xi, 41, Jan. 1869.

G. F. B.

15. *Tritylic (propylic) alcohol produced by fermentation.*—Some doubt existing in the minds of chemists as to the actual production of tritylic alcohol in the ordinary alcoholic fermentation of beet root, Isadore Pierre and Puchot have prepared in this way nearly a dekaliter of this substance remarkably pure. The alcohol itself has a density of 0·820 and it boils at 98° C. Propylic iodid obtained from it has a density of 1·784 and boils at 104·5°. The acetate boils between 104° and 105°.—*Bull. Soc. Ch.*, II, xi, 43, Jan. 1869.

G. F. B.

16. *On the reduction of acetic oxyd to ethylic alcohol.*—Linnemann has succeeded in effecting this reduction by acting upon acetic oxyd (anhydrid) with finely pulverized sodium-amalgam. An energetic action takes place, evolving if moisture be present, an odor of aldehyd; on adding water and continuing the action, the aldehyd odor, which at first becomes stronger, disappears entirely and in the liquid is found acetate of sodium and ethylic alcohol. The reduction takes place as follows;—

$$(1.)\ \left.\begin{matrix}\text{Є}_2\text{H}_3\Theta\\ \text{Є}_2\text{H}_3\Theta\end{matrix}\right\}\Theta-\Theta=\left.\begin{matrix}\text{Є}_2\text{H}_3\Theta\\ \text{Є}_2\text{H}_3\Theta\end{matrix}\right\}\quad(2.)\ \left.\begin{matrix}\text{Є}_2\text{H}_3\Theta\\ \text{Є}_2\text{H}_3\Theta\end{matrix}\right\}+\text{H}_2=\left(\left.\begin{matrix}\text{Є}_2\text{H}_3\Theta\\ \text{H}\end{matrix}\right\}\right)_2$$

$$(3)\ \left(\left.\begin{matrix}\text{Є}_2\text{H}_3\Theta\\ \text{H}\end{matrix}\right\}\right)_2+\text{H}_4=\left(\left.\begin{matrix}\text{Є}_2\text{H}_5\\ \text{H}\end{matrix}\right\}\Theta\right)_2$$

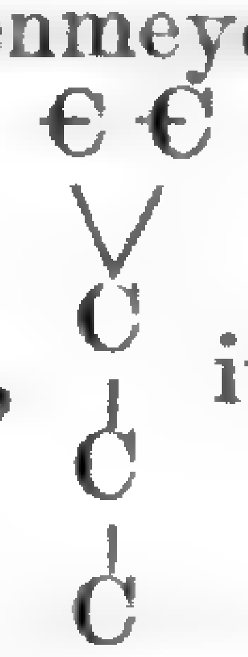
—*Ann. Ch. Pharm.*, cxlviii, 249, Nov. 1868.

G. F. B.

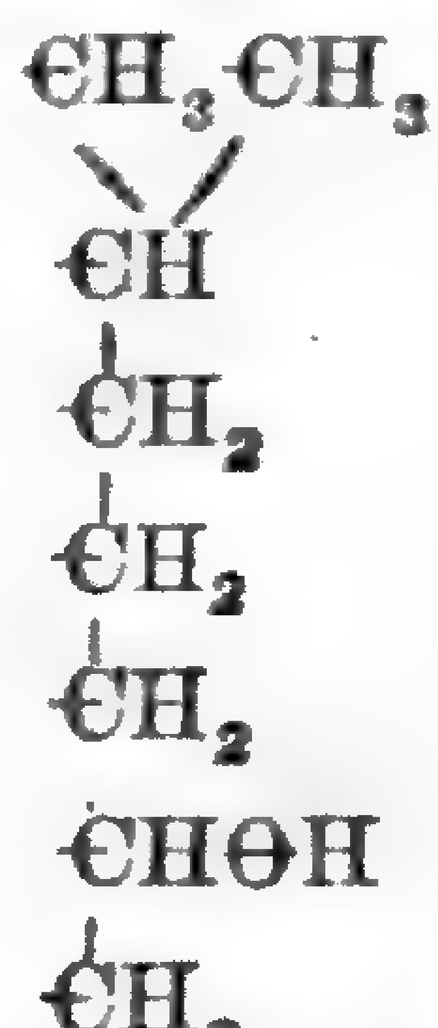
17. *On the alcohol obtained by the saponification of castor oil.* The question whether heptylic or octylic alcohol is produced by the saponification of castor oil, has been re-investigated by Schorlemmer. The product was obtained in the usual way by distilling the oil with an excess of potassic hydrate. The distillation was effected as rapidly as possible in a thin flask of copper. The distillate was rectified from fused potash so long as any action took place.* The mass of the distillate boiled between 160° and 178°, the boiling point being constant for a considerable time at 170°. It contained no methyl-œnanthyl or other ketone, sodic bisulphite being without action upon it. Upon treating it with phosphorus and iodine, the alcohol was converted into iodid, and

* Beside the alcohol itself, compounds having higher and lower boiling points were also obtained. The latter consisted of various unsaturated hydrocarbons, principally octylene, boiling at 125° C.

the fluid was separated by fractional distillation into hydrocarbons boiling below 160°, and into octylic iodid whose boiling point was 210° to 215°. No heptylic alcohol was present in the above mentioned liquid therefore, which consisted entirely of octylic alcohol mixed with hydrocarbons. To ascertain the character of this alcohol, it was oxydized with sulphuric acid and potassic dichromate. After the action ceased, the fluid was distilled and the distillate neutralized with sodic carbonate. An oily layer floated on the surface, which was neutral in its reaction, possessed the characteristic odor of methyl-œnanthyl, and united readily with sodic bisulphite to form a crystallized compound. This substance, pressed between folds of paper and dried, yielded, when distilled with sodic hydrate, the pure ketone, boiling between 170° and 172°. By continuing the action of the oxydizing agent the methyl-œnanthyl was converted into a mixture of sodic caproate and sodic acetate. The above facts prove conclusively the truth of the opinion expressed by Kolbe that the alcohol obtained from castor oil is a secondary alcohol, since it yields a ketone on oxydation, which ketone by further oxydation, falls apart into caproic and acetic acids. Its constitution is represented by the formula $\begin{cases} C_6H_{13} \\ CHOH \\ CH_3 \end{cases}$; it is methyl-hexyl iso-octylic alcohol. The constitution of the radical hexyl C_6H_{13}, is next discussed by Schorlemmer. The caproic acid prepared from fats is identical with that prepared synthetically from amylic cyanid, and both correspond precisely with that obtained above by the oxydation of the iso-octylic alcohol. And as, according to Erlenmeyer, the carbon atoms in amyl have the following arrangement,



it follows that this grouping must exist in caproic acid and in the secondary alcohol from which it was obtained. To establish this point, Schorlemmer prepared from the alcohol the hydrocarbon C_8H_{18}, by converting it into the iodid, and reducing this with zinc turnings. From its boiling-point which was 124° C., he shows that its constitution must be $\begin{cases} CH(CH_3)_2 \\ C_5H_{11} \end{cases}$. Hence, the iso-octyl alcohol from castor oil has the formula



—*Ann. Ch. Pharm.*, cxlvii, 222, Aug. 1868. G. F. B.

18. *On Sulpho-carbamid.*—REYNOLDS has succeeded in isolating the sulphur compound corresponding to urea, by heating ammonic sulphocyanate to 170° C. in an oil bath for two hours. On solution in water and crystallization, orthorhombic prisms were obtained which were not deliquescent, were very soluble in alcohol and water, sparingly so in ether, and which yielded on analysis the formula of sulphur-urea, $(\text{CS})H_4N_2$; its derivatives establish its rational constitution.—*J. Ch. Soc.*, Dec. 1868. G. F. B.

II. GEOLOGY.

1. *On a Mineral Phosphate from the island of Redonda, W.I.;* by CHARLES UPHAM SHEPARD.—This substance has lately been brought to my notice by receiving through Messrs. Willis and Chisolm of this city, a few pounds of it, sent them by Mr. Crichton of Baltimore, the proprietor of the island.

Its mineralogical characters, equally with its chemical composition, place it strongly in contrast with the pyroclasite or stone-guanos of the West Indies and of South Carolina, to which it was supposed to be related; and on the ground of these differences, I have thought it worthy of notice.

Mineralogical description.—Massive, amorphous, minutely vesicular, or sub-amygdaloidal; resembling allophane or an earthy opal. The cavities (not always present) are not much above the size of mustard seeds. They are nearly spherical, and have a white opaque lining, in consequence of which they contrast with the translucent compact mass of the stone, and appear to be foreign oolitic bodies, imbedded therein. Translucent (like flint or hornstone) to opaque, color grayish or yellowish white,—rarely milk-white in spots. H. 3·5–4, sp. gr. 1·90–2·07. Fracture even, sub-conchoidal to earthy, brittle, luster dull. The white and earthy variety adheres strongly to the tongue, like certain altered chalcedonies; but emits no argillaceous odor on being wetted.

Chemical description.—Before the blow pipe, turns white at first, and then becomes rusty, without melting. The flame becomes yellow, even after the mass is moistened with sulphuric acid, though faint green tints are occasionally visible. With solution of cobalt, the heated mass becomes deep blue.

It contains no carbonic acid and only traces of sulphuric acid and chlorine. Its solution is easily effected in the strong acids; and the solutions are turbid with flocculent silica. Ammonia throws down a copious precipitate of the phosphates of alumina and iron, and the clear supernatant liquid gives additional strong precipitates with either the chlorid of calcium or chlorid of barium; in the former case, of the phosphate of lime, and the latter, of the phosphate of baryta,—showing the mineral to be unusually rich in phosphoric acid.

On analysis the following results were obtained:

Phosphoric acid, - - - - -	43·20
Peroxyd of iron, - - - - -	14·40
Alumina, - - - - - - -	16·60
Water, - - - - - - -	24·00
Silica, - - - - - - -	1·60
Lime, - - - - - - - -	0·57
S̈, Na, Cl, Mg, - - - - - -	traces
	100·37

I detect in the mineral in a few instances, by the aid of the microscope, minute plumbago-like scales or lenticular crystals, which do not appear to be plumbago, or even specular iron.

As the phosphate is represented as existing in quantity, it suggests a valuable source of phosphoric acid or even of phosphorus itself. How it can be utilized in agriculture does not appear so obvious.

Charleston, South Carolina, Feb. 13th, 1869.

2. *Final Report on the Geology of New Jersey;* by Geo. H. Cook, State Geologist. Published by the Board of Managers, Newark, 1868.—This volume forms a handsome octavo of 900 pages, with 108 photographic engravings and wood-cuts, and six mine maps. It is accompanied by a portfolio containing four large colored maps in seven sheets, illustrative of the extent of the Azoic, Paleozoic, Triassic, Cretaceous and Tertiary and recent formations in the State, forming a complete geological map drawn to a scale of half an inch to the mile. Also four other maps in five sheets showing, (1) the group of iron mines in Morris County, printed in two sheets colored and drawn to a scale of three inches to the mile, (2) the Kingwood Iron Mine, (3) the Oxford Furnace Iron ore veins, and (4) the Sussex County Zinc Mines, the last three drawn to a scale of 8 inches to the mile. This volume is the final report of Prof. Cook of the survey authorized by the State in 1864. It also embodies in it much of the material contained in the annual reports of the survey of Dr. Kitchell made in 1854–6, but interrupted in 1857, by failure of appropriation from the State. A brief sketch of the general geography of the State forms the introduction, followed by some 300 pages devoted to the detailed and historic geology, while more than one half the book is devoted to economic geology.

Confining our observations exclusively in this notice to the "economic" part of the work, we are pleased to see a recognition of the well established fact that soil analyses are of but little benefit to farmers, although a few analyses are very properly given to show the general nature of the soils of the State. A chapter on limestones and lime gives many valuable facts and analyses, and this is followed by a discussion of the chemical characters and remarkable fertilizing properties of the greensand marl, which of late years has assumed great commercial importance as a fertilizer, no less than 134,000 tons of this material having been transported on the New Jersey railroads during the past year.

Not only "green" sand but "white" sand is very extensively "pitted" in New Jersey, nearly 20,000 tons of the latter being annually produced for glass making, and consumed by some forty glass houses in the southern part of the state.

The *clays* of New Jersey are also of great economic importance; the fire clay of Perth Amboy, South Amboy, Woodstock, and other places in Middlesex County, is shipped to all parts of the Eastern and Middle States, and thousands of tons of it are annually made into fire-brick.

We are glad to see so large a space devoted to the consideration of the Iron ores and the description of the iron mines; this chapter together with the admirable detailed map of the Morris County, Kingwood and Oxford Iron Mines, forms an exceedingly interesting feature of the book. Prof. Cook gives the product of the iron mines of the state as 300,000 tons in 1867, while in the furnaces 36,919 tons of anthracite pig-iron, 9,000 tons of charcoal pig-iron, and 5,980 tons of bar iron from bloomeries and forges were produced the same year. Of zinc-ore, Sussex County produces 25,000 tons per annum, which is manufactured into white oxyd and spelter, yielding 7,000 tons of oxyd, and 500 tons of metallic zinc. Prof. Cook estimates the total amount of zinc-oxyd produced yearly in the United States to be 10,000 tons and spelter 2,300 tons. Our space permits us to touch on but a few important points. The book is made accessible to almost every one, as it is furnished to those who desire to purchase it at the mere cost of paper and printing, all the expense of preparation and illustration being defrayed by the state.

We add here only that the scientific value of the volume is much increased by an Appendix, containing Catalogues of the Invertebrata of the Cretaceous and Eocene formations of New Jersey, by T. A. Conrad; of the Extinct Mammals and Reptiles, by Prof. E. D. Cope; of the Minerals, by Rev. E. Seymour; and of the living Vertebrata of the State, by Dr. C. C. Abbott.

III. ZOOLOGY.

1. *Are Unios sensitive to light?* by ISAAC LEA. (In a letter to the Editors of this Journal.)—In the March No. of this Journal, page 286, Mr. C. A. White heads an article with "Are *Unios sensitive to light?*" He then gives some experiments which he instituted on the subject, and he seems to be under the impression that his observations were entirely new.

If your readers will turn to the Proceedings of the Acad. Natural Sciences of Philad., for 1857, they will find a communication from me where the subject of *touch, hearing and sight in the Unionidæ* was pretty fully stated. It will be found in my paper that I experimented on various species of the family and pointed out some which gave no indication as to sensitiveness to light, while others were particularly sensitive, especially the *Unio radiatus* Lam. I there stated that the *visual organs* were placed on the fringes of the siphonal opening. That "with a good lens the terminal point of the tentacula may be observed to be rounded and furnished with at least the appearance of an eye and that it would prove to be a true eye, however imperfect, there can be little doubt." I also stated that I left the subject to Dr. Leidy, believing that "he would be able to make out the complete anatomy of the eye of the Unio."

It was mentioned also in this paper that the females were more sensitive to light than the males.

Subsequently in the introduction to my vol. 6 "Observations on the Genus Unio," &c., I mentioned the subject again, and stated that I had found that the *Unio rubiginosus* Lea, *U. Cylindricus* Say, and *An. imbecillis* Say, were all sensitive to light.

On referring to my notes made since the above mentioned publication, I find that during the years 1858, '59, '60, I found the following species "very sensitive to light," viz., *Unio subrotundus* Lea, *U. pyramidatus* Lea, *U. obscurus* Lea, *U. pustulosus* Lea, *U. Æsopus* Green. The further investigation of the subject is well worthy the attention of malacologists who are so situated as have all the conveniences of exploration, investigation and time.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On the use of sodium in explosive powders.*—Dr. H. FLECK of Dresden has made an extended series of experiments on the use of sodium as an ingredient of various explosive and inflammable mixtures, mainly with the purpose of replacing phosphorus in the match manufacture. In his earlier attempts he simply added sodium to ordinary gunpowder; but it united with the sulphur and thus destroyed its efficacy. The sulphur was then omitted from the compound, which was formed of 8½ grams saltpeter, 2 grams sodium, and 1 gram charcoal (mixture A); it was a gray mass which flashed like gunpowder on being touched with a moistened glass rod. On filling a tube with gunpowder, placing a little of this powder on the top, and throwing the whole into water, an instantaneous explosion took place. This mixture, however, burned too rapidly to ignite sulphured wood; the charcoal was therefore replaced by antimonous sulphid and a mixture made of 66 grams saltpeter, 36·5 antimonous sulphid, and 5 grams sodium (mixture B). This is preferable to the former one because it contains less sodium, the reaction being:

$$3KONO_5 + Na + SbS_3 = NaOSbO_5 + 3KOSO_3 + 3N.$$

The next point was the preservation of the sodium. This Fleck has succeeded in doing by means of paraffine, under which the sodium is melted, and then very finely divided by violently shaking the flask for five or ten minutes. A granular powder of a silvery luster is thus obtained, which when removed from the still liquid paraffine, does not tarnish, being perfectly protected by a thin coating of this hydrocarbon which it retains, and which increases its weight from 30 to 35 per cent. 6·6 grams of this sodium are substituted for the 5 grams of ordinary sodium in the above mixture B. To remove all traces of moisture from the other ingredients, they are well dried and then moistened with petroleum; the sodium is now added and the whole is well mixed together in a metallic mortar, until it becomes a uniform powder; it is then preserved in well closed vessels. As thus made, A is in the highest degree inflammable, communicating the combustion readily to the mixture B on a piece of sulphured wood. For the purposes of the match manufacture, these mixtures are made into a paste with caoutchouc dissolved in naphtha to a gelatinous mass, which then may be readily incorporated with the powder, without injuring the inflammability of the latter.

Experiments were then made on the preparation of a suitable match box, one which should act when opened. A brass box, whose cover was made to fly open with a spring, was filled in its lower part with moistened asbestus; in the upper portion, two compartments were arranged, separated by a spring slide. The upper one contained the spontaneously inflammable pellets. On opening the box the slide is automatically opened and one of these pellets falls through the opening into the second compartment, and thence through a second opening on to the moist asbestus, where it is at once inflamed. The pellets are composed first, of a doughy mass of chalk and the caoutchouc paste; this is then dipped in a mixture of 3 parts potassic chlorate and one part golden sulphid of antimony, also made pasty with the caoutchouc solution; while still moist they are shaken with the pulverized mixture B, and placed in the upper part of the match-box, which holds about 100, each being about 2 mm. in diameter. All who have seen this match-box in use have been surprised at the ease and certainty of its action; and were it possible to make the slide air-tight, it must come into general use.

Another purpose for which this mixture has been used is the preparation of primers, which are ignited when pierced by a moist needle, whose point carries an eye in which the water is lodged. It has also been employed with success in the manufacture of pulling torpedoes. And the inventor suggests its applicability for firing the charges in blasting, either under or above water. For this purpose the powder is contained in a glass tube whose mouth is closed with melted sodic hyposulphite. On immersing the tube in the vessel containing the gunpowder, and placing the whole in water, the explosion follows so soon as the hyposulphite is dissolved. Ten milligrams of the powder is sufficient to ensure the ignition of the heaviest charges.—*Dingler's Polyt. Journal*, cxc, 306, Nov. 1868. G. F. B.

2. *Decrease in the Production of Gold.*—BLAKE in his late "Report on the Precious Metals," has the following remarks on the probable future decline in the production of gold, which are worthy of notice (p. 233); as also those on placer and vein mining and the probable rise in value of gold, to be quoted hereafter.

"The statistics of the production of gold in California, Australia and other countries show very clearly the familiar fact that in all newly discovered gold regions a maximum production is soon attained and is succeeded by a gradual but certain decrease owing o the exhaustion of the placer deposits. Thus, in California the maximum product was attained in the year 1853, when the shipments were about \$55,000,000, and the production was doubtless from \$60,000,000 to \$65,000,000 in value. It is now much less than half of that amount. In Australia, in the same year, (1853,) the reported shipments from Victoria amounted to 3,150,020 troy ounces, and the production was nearly \$60,000,000 in value.* In 1867, the shipments were only 1,433,687 ounces, much less than half as much as in 1853. The apparently nearly uniform production of California for the past ten years, judging from the shipments of treasure from the port of San Francisco, is the result of the opening of other gold and silver producing regions in Nevada, Idaho, Oregon, and Arizona, which, so far as their production depends upon placers, are in their turn liable to rapid exhaustion. In British America and in Idaho and Montana the production of gold is now rapidly diminishing.

"Russia is the only country in which a nearly uniform production has been maintained through a series of years. This may perhaps be explained by the fact that the mines have not been free to all, and consequently comparatively few persons have been engaged in developing them. The climate, also, is unfavorable to rapid and continuous working, and the method of washing placer gravel by machinery in use there is necessarily slow and gives limited results, which cannot compare with those obtained by the gigantic system of sluicing practiced in California, and Australia. There has also been in Russia a constant extension eastward of the gold region by new discoveries, extending even to the Pacific coast, and there is doubtless an immense area of virgin ground from which the gold supply of Russia may be for a long time maintained at the present figures, or, possibly, greatly increased, especially if all restrictions upon mining are removed, and the country is thrown open to the skilled miners of other regions. This Siberian gold field, with the great mountain region south of it, extending into China and India, is the only extended region now known in regard to which there is any uncertainty in respect to its probable future yield of gold.

"The existence of very ancient workings in the Altai is significant, and leads us to question whether this great interior region has not already yielded up its most accessible treasures."

3. *Dana's System of Mineralogy.*—The first thousand of Dana's Mineralogy having been disposed of by the publishers, a new issue has recently been made, with some corrections, of which the following are the most important:

Page xxii, 25th line from foot, $\frac{3}{2}$ for $\frac{3}{4}$; p. 60, 21 l. from foot, 7·671 for 6·671; p. 77, 6 l. from foot, 7·43 for 47·3; p. 150, in fig. 148, $5\frac{2}{3}$ for $5\frac{3}{5}$; p. 252, in line of 297, Meionite, in 2nd formula, $\frac{1}{11}$ for $\frac{1}{4}$; p. 259, 8th l. from foot, 3·97, 3·87 for 2·97. 2·87; p. 262, 14 l. from top, *Levy*, Jahrb. Min. 1830, 71; 16 l. from foot, iv, 513 for iv, 147; p. 290, in fig. 272, $1\breve{\frac{3}{2}}r$ for $1\breve{\frac{3}{2}}l$; p. 309, in fig. 285, $\frac{1}{2}$ for 2. 2 for $\frac{1}{2}$ below, and for I; p. 320, under fig. 291, *ii* for I; p. 327, in fig. 292, below O, 2 and 3 removed to right of $\frac{1}{2}$ and 1; p. 366, in fig. 333, *i*-2 transferred to next plane to the right; p. 383, in fig. 362, *i*3 for the lower *i*2; p. 409, 14 l. from top, Ueberroth for Ueberroble; p. 448, 3 l. from foot, $\frac{1}{3}H_2$ for $\frac{2}{3}H_2$; p. 480, 3 l. from foot, 1840 for 1837; p. 495, 19 l. from top, 1841 for the first 1843; p. 530, in fig. 441, $\frac{1}{2}$, 1, 2 substituted

* Calculated at \$19 per ounce.

for e′, e, e″, and in both 441 and 442, 22 for 12; p. 551, after Bindheimite, $\ddot{\bar{S}}b, \dot{P}b, \dot{\bar{H}}$ for $\ddot{\bar{S}}, \dot{P}, \dot{\bar{H}}$; p. 593, 5 l. from foot, $+\frac{1}{8}$ aq added to end of 2nd formula; p. 721, 13 l. from foot, XLYORETINITE GROUP for RETINITE GROUP; p. 812, 806 added after Cumengite.

OBITUARY.—J. NICKLÈS.—Through a letter from Mr. Achilles Delesse, dated Paris, April 7th, we have intelligence of the death of our correspondent, Mr. Nicklès, Professor in the University at Nancy. Prof. Nicklès died after a short illness, from a severe cold aggravated by breathing an atmosphere containing some hydrofluoric acid. He had been experimenting for some time upon certain fluorids.

WILLIAM MITCHELL, a well known mathematician and astronomer, died of general debility at the Vassar College, near Poughkeepsie, on the 2nd of April, aged seventy-six years. He lived in the Observatory with his daughter, Miss Maria Mitchell, the astronomer. William Mitchell was a member of the Society of Friends, and in all relations of life a most esteemed man.

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *Handy Book of Meteorology;* by ALEXANDER BUCHAN, M.A., Secretary of the Scottish Meteorological Society. Second edition, 12mo, pp. 371. Edinburgh, 1868. (William Blackwood.)—This is, as its title declares, a very *handy* manual of meteorology, carefully prepared by a well known Scotch Meteorologist, and brought down to the times by a painstaking digest of the recent literature of the subject. It has been a prominent object with the author to present his subject in such a manner "as to engage on its behalf a wide spread interest in the general mind." The work is beautifully printed and illustrated by isobarometric and isothermal charts printed in colors in an extremely satisfactory manner. He has availed himself of the results of American investigators with fairness and good judgment for the most part. The work is on sale by Chas. Scribner & Co., 654 Broadway, New York.

2. *Annual of Scientific Discovery, &c., for* 1869. Edited by SAMUEL KNEELAND, A.M., M.D., &c. 12mo, pp. 377. Boston, 1869. (Gould & Lincoln.)—This old acquaintance is always an agreeable reminder of its predecessors of former years, as well as of numerous discoveries, facts and principles in various departments with which we are, as fellow laborers in the great field, more or less familiar, but which it is always pleasant to find marshalled in an orderly and compact manner, under their appropriate heads. Every such review of the year must of necessity be imperfect and unequal as an exposition of all that has been done, but even a cursory glance at Dr. Kneeland's Annual will show any fair-minded reader the vast variety of topics which now engage the attention of scientists and the painstaking care with which the editor has endeavored to present the most important results. The notes by the editor forming an introduction to the volume give an interesting summary of the progress of science for 1868.

A portrait (a better likeness than such efforts usually) of Prof. JAMES D. DANA is a pleasant frontispiece to the volume.

3. *First Principles of Chemical Philosophy;* by J. P. Cooke, Jr. Cambridge, 1868. 12mo.—This important manual of instruction can hardly be said to be published in the ordinary sense of that term. It has been prepared especially to present the philosophy of chemistry in such a form that it can be made with profit the subject of college recitations, and to furnish the teacher with the means of testing the student's faithfulness and ability. To Prof. Cooke more than to any American, is due the credit of having made chemistry an exact and disciplinary study in our colleges. The present work has grown out of his "Problems," prepared several years since to elucidate his methods, and is designed to inculcate these methods by the use of the unitary system of chemical notation and philosophy. It is a logical analysis and deduction of the subject which will command the careful attention of chemists whose duties require them to instruct in this difficult department.

4. *Outlines of Physiology, Human and Comparative;* by John Marshall, F.R.S., with additions by the American Editor, F. G. Smith, M.D. 1042 pages. 8vo. Philadelphia. (Henry C. Lea.) 1868.—This work treats of physiology as connected with anatomy, chemistry and physics, on which its principles so largely depend. It brings the reader to the level of the most recent investigations. It is a work of vast labor evincing, in many respects, the utmost ability and research. So far as the subject matter of the work is concerned this book is probably unsurpassed, and perhaps not equalled by any treatise on physiology in the English language.

This is more than we can say for the artistic and mechanical execution of the work. It is surprising that with a text of such excellence the publishers contented themselves with a series of illustrations inferior to those in some other works of much less merit. It is to be hoped that in the next edition they will give attention to arranging the material in conspicuous paragraphs with appropriate headings, and also introduce illustrations in keeping with the style of modern art, and more recent investigations.

5. *The Ornithological Collection of John Cassin.*—It will be seen by the advertisement that the very valuable collection of birds belonging to the late lamented ornithologist, John Cassin, is now offered for sale. This collection includes the types of many new species described by him, as well as numerous very rare and beautiful forms, seldom offered for sale, and would be a very important addition to any museum. We learn through Dr. George Smith that Mr. Cassin's birth-place was incorrectly given in our last number. He was born in Upper Providence township, Delaware county, Pa., September 6th, 1813.

6. *Catalogue of the Orthoptera of North America, described previous to* 1867*;* by S. H. Scudder. Smithsonian Miscellaneous Collections, 1868.—This work, although intended only as a compilation, cannot fail to be of great value to working entomologists. Every name under which species have been described is entered, with full references to the works in which it occurs and the locality from which it was stated to have come, but no attempt has been made to give the actual synonymy. It is accompanied by a

full list of authorities, and at the end is a tabular arrangement of the genera and families, which appears to us more natural than the systems usually adopted. V.

7. *Memoirs of the Peabody Academy of Science*, Vol. I, No. 1, royal 8vo. Press of the Essex Institute, Salem, Mass.—This new serial makes its appearance in a style that will compare favorably with the publications of any other scientific institution, whether foreign or American. The typography is beautiful and the paper of excellent quality. The size is well adapted for the purpose. The part now issued contains a very complete monographic revision of the Large, Stylated, Fossorial Crickets, by S. H. Scudder, illustrated by an excellent plate, giving details of the wings and fore legs of most of the species. This group, including the species usually designated as Mole-Crickets, the author divides into two genera, *Gryllotalpa* and *Scapteriscus.* The latter includes eight species, of which four are new; the former fifteen species, two of which are new. Nearly all the known species have been carefully redescribed, with full tables of measurements and localities. V.

8. *Bulletin of the Essex Institute*, Vol. I, Nos. 1 and 2. Salem, Mass. 8vo. Jan. and Feb., 1869.—This new monthly serial is intended to replace, in part, the "Proceedings of the Essex Institute," which will be discontinued at the end of Vol. VI. It will contain the reports of the meetings of the Society, lists of donations to the museum and library, etc., together with the shorter and less technical communications, in both the historical and scientific departments. It is furnished at the very low price of one dollar per annum. V.

Facts and Arguments for Darwin. By Fritz Müller. With Additions by the Author. Translated from the German by W. S. Dallas, F.L.S. 144 pp. octavo. London, 1869.

MEMOIRS BOST. SOC. NAT. HIST. Vol. I, Part 4.—p. 473, Weapons and Military Character of the Race of the Mounds; *C. Whittlesey.*—p. 482, Distortion of Pebbles in Conglomerates; *G. L. Vose.*—p. 488, Notes on Birds observed in Western Iowa, in July, Aug. and Sept.; also on Birds observed in Northern Illinois, in May and June, and at Richmond, Wayne Co., Indiana, between June 3d and 10th; *J. A. Allen.*—p. 527, Notes on Hesperomannia, a new genus of Hawaiian Compositæ; *W. T. Brigham.*—p. 529, Notes on Alsinidendron, Platydesma, and Brighamia, new Genera of Hawaiian Plants; with an Analysis of the Hawaiian Flora; *H. Mann.*—p. 542, Geographical Distribution of the Native Birds of the Department of Vera Cruz, with a List of the Migratory Species; *F. Sumichrast.*—p. 564. Eruption of the Hawaiian Volcanoes, 1868; *W. T. Brigham.*—p. 588, Physical Geology of Eastern Ohio; *C. Whittlesey.*—p. 599, Index to the Vol.

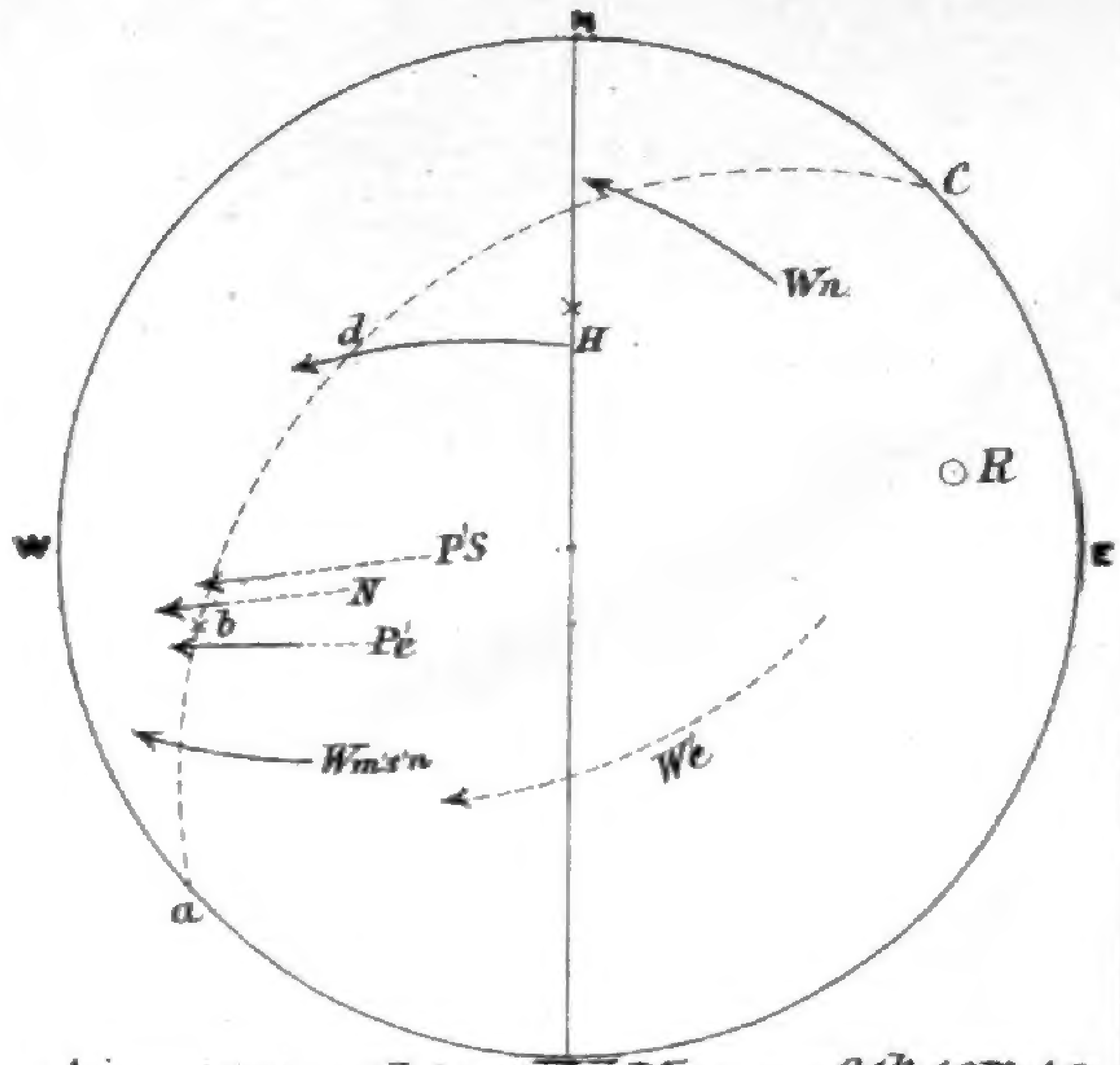
PROCEEDINGS PORTLAND SOC. NAT. HIST. Vol. I, Part 2.—p. 98, List of Fossils from Zebb's Cove, Cape Elizabeth, Me.; *W. Wood.*—p. 99, Fossil Plants at Perry, Me.; *J. W. Dawson.*—p. 104, Catalogue of Fossils from Square Lake; *E. Billings.*—p. 127, Catalogue of the Flowering Plants of Maine (continuation); *G. L. Goodale.*—p. 138. Discovery of Heath in Maine.—p. 143, Orthoptera of the State of Maine; *S. I. Smith.*—p. 152, Curious Formation of the Egg of the Common Fowl; *N. T. True.*—p. 153, The Characters of the Lepidopterous Family Noctuidæ; *A. S. Packard, Jr.*—p. 157, Fresh Water Polyzoa occurring in Maine; *A. Hyatt.*—p. 163, New Localities of Minerals in Maine; *N. T. True.*—p. 165. Mineralogy among the Aborigines of Maine; *N. T. True.*—p. 168, On the Motion of Glaciers; *J. DeLaski.*—p. 179, Decades of Maine Fungi, I and II; *M. C. Cooke.*—p. 185, Clio borealis on the coast of Maine; *W. Wood.*—p. 194, Portland Soc. Nat. Hist. from 1866 to 1869.

INDEX TO VOLUME XLVII.

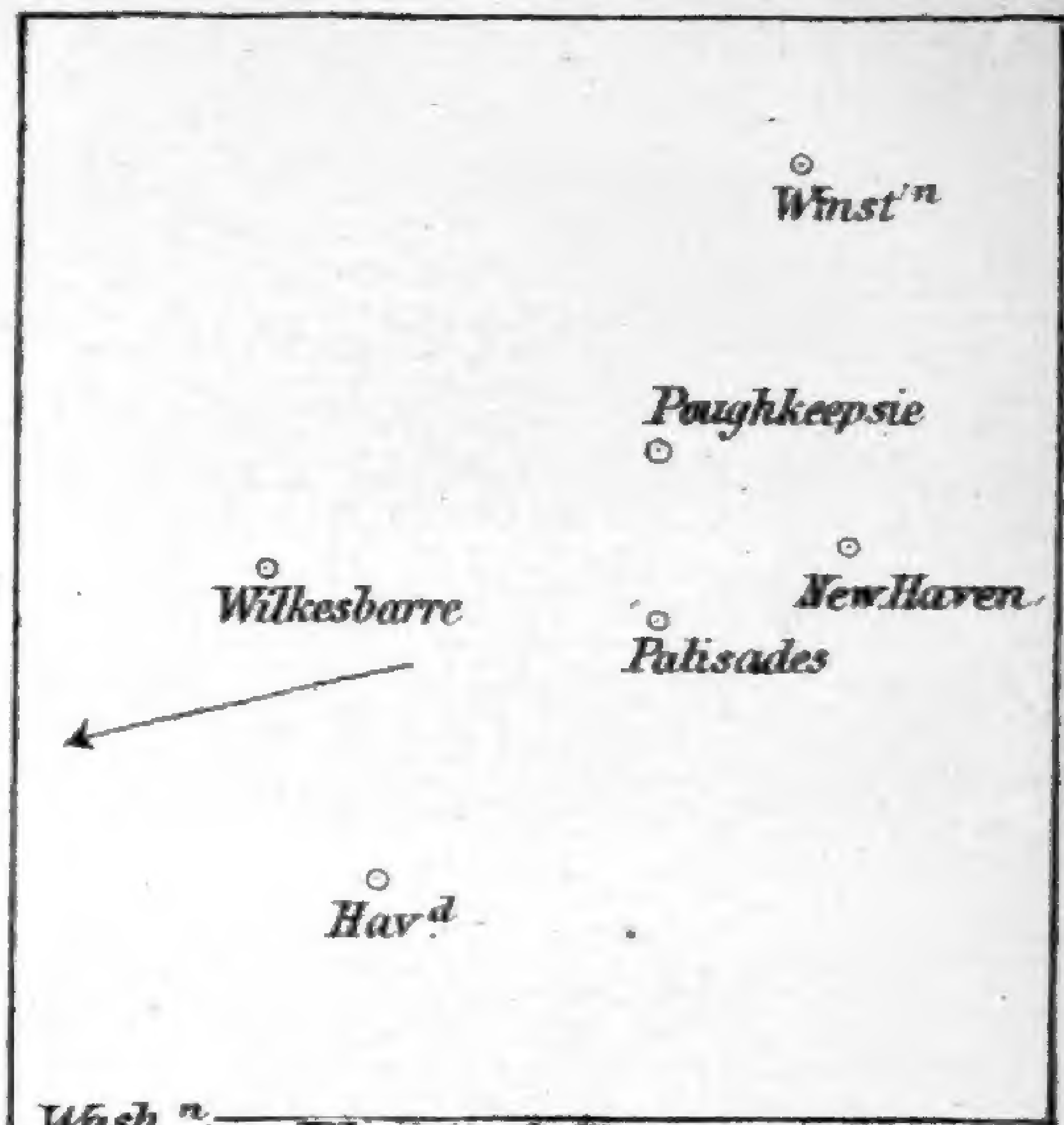
PLATE I.

METEOR OF 1h 12m 1s N.Y. M.T. AS SEEN AT
NEW HAVEN, Conn, PALISADES, N.Y., HAVERFORD, Pa., and Wmst'n. Mass.

PLATE II.



Apparent paths of Meteor of $1^h\ 12^m\ 1^s$



Places of Observation

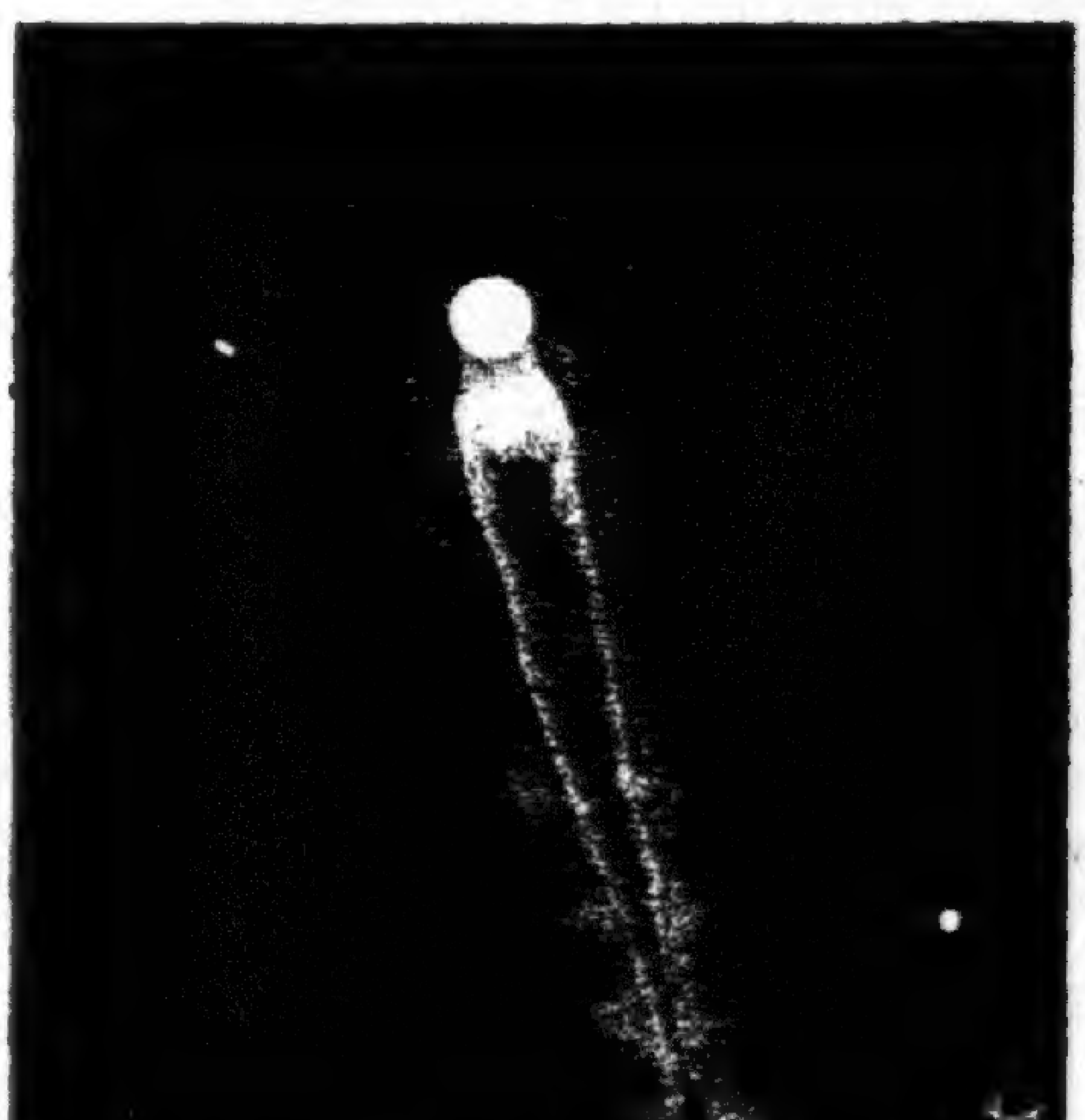
METEORS SEEN AT PALISADES, N.Y.

METEOR OF $2^h\ 48^m$

Train, and cloud at point of disappearance of meteor, as seen with a power of 40 linear in a 4 in. glass.

METEOR OF $1^h\ 53^m$

Smoky train as seen in a 4 inch glass; power of 40 linear. Field of view 43' of arc.



Meteor in Leo Minor just previous to disappearance $11^h\ 18^m$ P.M.

Train of a meteor at $11^h\ 26^m\ 30^s$ Greyish cloud 2° to 3° long.

TRACKS OF SEVERAL PROMINENT METEORS, THEIR POINTS OF OBSERVATION TELESCOPIC VIEWS OF